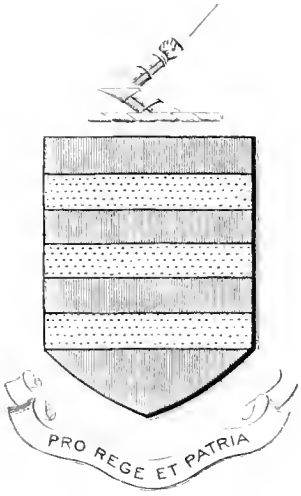

THE

JOURNAL

OF

NERVOUS AND MENTAL DISEASE,

EDITED BY

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PHILADELPHIA.

JANUARY—OCTOBER, 1878.

(WHOLE SERIES. VOL. V.)

OFFICE OF PUBLICATION—CHICAGO: 70 MONROE STREET.
BRANCH OFFICE—NEW YORK: G. P. PUTNAM'S SONS, 182 FIFTH AVENUE.

FOREIGN AGENTS:

LONDON: SMITH, ELDER & CO., WATERLOO PLACE, W. C.
PARIS: GUSTAVE MASSON, PLACE DE L'ÉCOLE DE MEDICINE.

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Vol. V. JANUARY, 1878. No 1.

Original Articles, Selections and Translations.

ART. I.—CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

BY E. C. SPITZKA, M. D., NEW YORK.

PART I.

The Raphe and its dependencies; systemic relations of the central tubular gray matter, and the participation of the tegmental ganglia in the motor projection system.

AN accurate knowledge of the nerve strands in the medulla oblongata and nodus encephali is the key to the comprehension of the remainder of the brain, and to acquire this knowledge a study of the raphe is indispensable. It is through the raphe that the important innervations of speech, and facial expression are placed under the control of the hemispheres: it is through the raphe, that the restiform column is metamorphosed from the posterior spinal columns, and finally, it is through this tract, and its dependencies, that the transformation of the pyramids, to certain areas of the cord is effected. The first trace of anything resembling the pyramidal decussation in the

ocular movements and accommodation, namely, that unlike those nuclei engaged in voluntary expression, articulation and phonation, they are not under the direct control of the hemispheres, but only mediately, that is through the corpora quadrigemina. This would be in perfect accordance with the experimental results obtained by Adamuck, which showed that these bodies were the seat of all associated movements of the eyeball.

Among those fibres, which Meynert traces as the second projection to the upper nucleus trigemini, I have found a few to originate in the substantia nigra. This relation is also suggestive, and will be discussed in Part II.

Lower down, as stated, the raphe is rendered obscure, by the crossing of the brachium conjunctivum, and it is not before the origin of the facial nerve is reached that the relation again becomes clear. At the altitude of the common abducens facial nucleus, the rectilinear fibres again become predominant, their terminus can be traced to the nucleus in question, and their origin is clearly in the deep transverse, and occasionally in the vertical fibres of the pons. That they consequently represent a cerebral source of innervation will appear clear to those who are familiar with the recent writings on the pons.

In how far these fibres place abducens-cells, or facialis-cells, under hemispheric control, it is impossible to state directly; inferentially, I should be inclined to suppose, for reasons to be stated, that if any of these fibres go to the abducens origin, they must be at a minimum. To the lower facial nucleus I have not been able to trace any raphe fibres, but I am satisfied that a direct transition of such fibres to the horse-shoe shaped facial root *takes place without the intervention of cells.* In this region, the fibres of the reticular formation begin to cross the raphe, and henceforth the latter assumes its characteristic appearance of a regularly branched tree. Here, also, it becomes locally widened by the crossing of the trapezium fibres.

It again suffers a diminution in diameter at the point where the auditory nerve nucleus reaches its greatest development, that is, in the altitude of the *striæ medullares albae*, where the facial nucleus disappears, and the hypoglossal has not be-

gun to appear. Some of the fibres in the striæ medullares can be traced to the raphe.

With the development of the inferior olivary body, and the commencing decussation of the restiform column, the raphe assumes relations which herald the approaching pyramidal decussation. It gains additional importance from its relations to the hypoglossal and accessory nerves. Here I have been able to trace the direct passage of an axis cylinder of large calibre from the anterior pyramids, to a large multipolar cell of the hypoglossal nucleus, whose peripheral process passed to the nerve roots; an incontestible proof of Meynert's projection theory.

Certain of the fibres which terminate in the hypoglossal nucleus, or the roots directly, remain on the same side of the median line whence they have originated, the majority cross to the opposite side. To what extent fibres from the reticular formation may terminate in this nucleus, or in the vago-accessory nucleus, I am unable to state precisely; I must leave this an open question. Very frequently, fibræ arcuatæ of the reticular formation, do not cross the raphe directly, but run parallel with and among its fibres for varying distances, before they emerge on the other side; that some of these may even extend as far as the nucleus is not impossible, and should such an occurrence be proven for certain of these fibres which can be traced to the hilus of the inferior olivary body, Schroeder Van der Kolk's hypothesis, regarding the relation of this body to speech coördination, would deserve renewed consideration.

There is a relative constancy in the development of the tegmental tracts, and the restiform columns in the various mammalia which I have examined, in this connection, and accordingly I have found an enlargement of the raphe, corresponding to the point where most of the fibræ arcuatæ cross, in all of them; relatively larger in the dog, as compared with man, however.

It is this area of the raphe which gradually increases in size as the decussation of the pyramids is approached. It consists at first solely of parallel bundles which cross in regular groups, entirely, and broader in front than behind, and the fasciculi emerging on either side, being separated by verti-

cal fibres, it presents one of the most beautiful objects in cerebral anatomy, and may be likened in outline to the stem of the century plant. It is one of the most favorable localities for tracing single axis cylinders for a considerable distance. Thus far these fibres do not merit the name of a sensory decussation, and Meynert's designation to that effect in his figure 260 (Stricker's Handbuch der Gewebe-lehre II., p. 303) is erroneous. These fibres do not leave the pyramids, but represent the last decussating fibres of the restiform column, and can be clearly and distinctly traced to those fibræ arcuatæ which pass through and around the internal accessory olive. Lower down, however, a transition occurs, fibres from the outer area of the anterior pyramids, join the fasciculi described, and run with them to the *funiculi cuneati* and *funiculi graciles;* this is the commencement of the proper sensory decussation, and this gradual passage of the restiform into the pyramidal decussation, has doubtless misled Meynert. The lower down we pass, the more do pyramidal fibres replace those derived from other sources, until finally the decussation is composed of pyramidal fibres solely. How the upper finely fasciculated sensory decussation is gradually replaced by the motor, has been correctly and exhaustively described by Meynert, Clarke, and others.

It is not to be held for a moment that a sharp demarcation can be made between those areas of the pyramids which take part in the motor, and those which constitute the sensory decussation; although we can say that the outer fibres, as a rule, go to the latter, and the inner to the former, yet at the transition, fasciculi from either are inextricably mingled.

Thus the raphe is the medium for the hemispheric projection of those motor innervations, which stand in the closest relation to our intellect, and this innervation, *as a rule*, is brought down from the opposite hemisphere. It is furthermore probable, that auditory impressions travel through this tract, as well as those of taste. A relatively small lesion in this raphe will consequently produce more severe symptoms than a larger lesion in the crus or pyramids, for it cuts off entirely the influence of the will over the peripheral nerves. As an illustration of this severe effect of raphe lesions, I can point to the case of a paretic, who independently of the usual

intellectual and motor disturbances characteristic of the disease from which he suffered, became, several months before his death, afflicted with complete and persistent alalia; he could not enunciate a single word, there was no agraphia or alexia, and those motions of the tongue which prevent its getting between the teeth in mastication, did not seem to be interfered with, and no trophic disturbance could be found in the tongue of any kind whatsoever. He died in an apoplectic attack, and on microscopic examination of successive sections, through the medulla and pons, an intense sclerosis of the raphe was found, occluding the whole transverse diameter, at the altitude of the hypoglossal nucleus. It occupied an approximately central position between the anterior and posterior extremities of the raphe, and beyond, that is, behind it, the vertical fibres were degenerated. The nucleus hypoglossi, as well as the outer roots, were healthy. On tracing the lesion upward, it was found to become gradually lost at the altitude of the facial origin. In this case, the sclerosis had formed around one of the long vascular twigs which run in the raphe, and, as vessels frequently form the starting point for sclerotic processes, it will be well to give that attention to this area which it deserves. I have at present a specimen of an apoplectic clot, destroying the raphe under examination. Unfortunately, no symptoms could be referred to it, as there was an immense and fatal extravasation in the corpus striatum and insula coexisting.

It may be well here to refer to the beautiful homology existing between the various fasciculi, which under the name of fibræ arcuatæ, cross the raphe throughout its whole length and depth. They all bear intimate relations to the cerebellum, and all enter into connection with ganglionic masses, which have in their turn certain structural peculiarities in common. The three fibre systems in question are the brachium conjunctivum, the trapezium, and the restiform column. The former passes through the nucleus tegmenti (Luys' *olive superieure*), the second through the superior olivary body, the last through the inferior olivary body—the olive *par excellence*. Some of these fibræ arcuatæ are derived from the tegmental tracts proper, and in them the large ganglionic cells of the reticular formation are found occupying definite positions.

This position can be only found in longitudinal sections, and is more constant in the carnivora than in man; as with the former, they also appear better developed and more numerous. Their long axis, and their axis cylinder processes (of which I have sometimes found two, one at each end, besides the numerous protoplasmic ones) run from before backwards; that is, in parallelism with the nerve roots. What their ultimate destination is, I am not able to say with regard to all of them; there is no sharp demarcation between them and the lower facial nucleus, and the lower nuclei of the accessorius nerve; in fact, from reasons founded on a comparative study of various animals, I am disposed to look upon these latter nuclei as merely being concentrations of the scattered cells in the reticular formation. The latter, in man at least, seem to be mostly subservient depots of the lateral mixed system of Meynert.

The nerve nuclei themselves, have been so accurately described by authors, that I cannot add anything to their descriptions. I would merely call attention to a difference in the shape of certain cells which are contained in, or at least situated near, the motor nuclei. These cells are round, mostly small, and resemble somewhat the bodies found in the interspinal ganglia. They form a separate and distinct accumulation just below the hypoglossal nucleus, close to the raphe, and enter into relations with the finer fibres of the hypoglossal nucleus. They are less numerous near the accessory origin, and still less at the facial. It is highly probable that these cell groups exercise some specific, perhaps trophic, function.

A somewhat similar continuous column can be traced from the upper quintus origin down through the auditory nucleus. These cells are large, and nearly all of them can be traced to the quintus-roots; they become less spherical and richer in processes the lower we descend, and in one region, well known as the substantia ferruginea, are deeply and constantly pigmented (in adult man.)

Not always do the nuclei give origin solely to those nerve roots whose name they bear; thus a beautifully marked group of cells, in the outer portion of the hypoglossal nucleus, sends

out a fasciculus, running a zig-zag course towards the origin of the spinal accessory nerve. These cells, and their processes, appear to possess rather the characters ascribed by Meynert to projection mechanisms, than to associating fasciculi. For physiological reasons, we would be inclined to suspect a close relation between the hypoglossal and part of the accessory origins; and here is an anatomical basis for the idea.

In his recent painstaking and valuable contribution to cerebral anatomy, Forel takes the ground that Meynert's statements regarding the origin and termination of the tegmental tracts, are not based upon exact observation. With regard to the posterior longitudinal fasciculus, he may be correct; but I must endorse Meynert's description for all the other fasciculi, particularly the fibres derived from the thalamus. The lower vertebrata, and also the lower mammalia, such as the rodentia, exhibit clearly on axial sections, that the vertical fibres of Meynert's motor field are connected with the thalamus above (and also with the subthalamic region), and pass mainly into the lateral column of the cord below. Single axis cylinders can be traced for a great distance in fortunate sections, and few indeed can be seen to curve out of the direct path. In man the relations are less clear on account of the great extent of the brachium conjunctivum and the nucleus tegmenti. The propriety of terming this a "motor field," may be disputed (I am inclined rather to consider it a mixed tract), but the anatomical connections are facts.

Concerning the posterior longitudinal fasciculus, I have not been able to trace it beyond the altitude of the posterior commissure in any animal; thus confirming Forel's statement. I have also found it atrophic in the mole *and bat;* and can endorse Forel's deduction, that it probably has some relation to vision, or the ocular movements, or their association with other movements. I can trace it to the deepest portion of the columns of Türck, in the cord; in man, in addition, the lower fibres scatter and contribute to the whole area of the anterior columns. I most distinctly assert, that its fibres are not lost in the reticular formation, but can in man, as well as in animals, be clearly traced as far as, or lower down even, than the pyramidal decussation. I possess two longitudinal sections,

from the pedunculer axis of the cat, which show a direct termination of a fasciculus of this bundle, in the nucleus trochlearis, after separating from the main cord, a few lines higher up; its fibres, like the majority of the fibres in the posterior longitudinal fasciculus, are of the larger variety.

The peculiar relation of the abducens nerve, to which I have adverted, when speaking of the raphe, cannot be so satisfactorily settled. On sagittal sections, from the same animal, I observe fibres of a finer variety passing *into* the abducens nucleus, in the same plane, in which the peripheral roots proper pass *out*. They unquestionably connect with the nerve cells in the nucleus, and according to Meynert's scheme, and all physiological probabilities, they must place the abducens nerve under the control of some higher ganglion. *Which*, I am not prepared to say, as the upper terminus of this "second projection tract" is unknown to me. It crosses the facial root, and seems to run under the ventricular ependyma.

In no group of animals have I found the posterior longitudinal fasciculus as well developed as in the chelonia,[1] not that it is larger, but better demarcated, and more easily traced than in other animals, examined thus far. In the *Chelydra* it is a perfectly round fasciculus, which in the absence of larger nerve nuclei, runs closer to the ependyma ventriculorum than

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*I may here remark, that the only writer who has studied the brain of the turtle microscopically—Stieda,—has committed one very grave blunder. He states that the turtle's brain possesses no conarium (pineal body.) The *Chrysemys picta*, *Nannemys guttata*, *Chelydra serpentina*, *Cistudo Carolina*, and *Calemys Muhlenbergi*, all of which species I have examined, possess a distinct pineal gland; and in the brain of a *Thalassochelys* (weighing one hundred and eighty pounds, when alive, whose head I obtained through the courtesy of Dr. E. Messemer), I found this body to fully equal the human conarium in size. Unless great care is exercised in removing the dura and arachnoidea, this body is apt to be torn away, and to lack of skill in dissecting, no doubt, are attributable the statements found in several older anatomies, that the pineal gland is occasionally absent in the human being! I have never failed to find it in fifty-one brains, in which this body was sought for; but to find it, and keep it *in situ*, it is necessary to split the velum interpositum, with whose processes this body is in a close connection. The absence of the pineal body in any group of amniote vertebrates would be so atypical, that I venture this provisory correction of Stieda's statement. I can show this body in every turtle.

in the mammalia, raising the ventricular floor so as to form a columnar elevation on each side of the median grove of the fourth ventricle. It need not be stated, that this is in no wise identical with the similarly situated eminentia teres of the mammalia, which is pushed up by the curved facial root and the motor nuclei. In the *Thalassochelys marina* the upper end of this fasciculus, approximates the crescentic shape, so characteristic of the mammalian medulla and pons. We may consider the round form to represent an ancestral type, just as the *Chelydra* represents an ancestral form of the chelonia, which is documented not only in its external configuration, but also in the simple valve-like cerebellum, which contrasts markedly with the globular and massive cerebellum of the Emydæ, and the marine species, in which that body is even rudimentarily foliated.

That the posterior longitudinal fasciculus should exist so well developed in animals whose hemispheres are as poorly developed as the turtles, seems to be conclusive evidence against Meynert's view of its origin in the ansa peduncularis, especially when we bear in mind the dictum for which we are indebted to Meynert himself, that the dimensions of a fasciculus in the transverse section, depend upon the mass of grey matter with which it is connected.

(TO BE CONTINUED.)

Art. II.—ACTION OF THE ALKALOIDS OF OPIUM.

By Isaac Ott, A. M., M. D.,

Late Demonstrator of Physiology, University of Penna.

EVERY one knows that when a full dose of opium is taken, that it produces a sleep which is not like our actual sleep, either in its accession, progress, duration or subsequent effect. All physicians are aware that in some people this drug does not produce sleep, but often hallucination, and in some cases vomiting, and rarely tetanoid effects. The composition of this drug, according to the latest researches* is as follows:

1. Morphia discovered by Sertuerner, - 1804

Its derivatives,

Oxymorphia, " " Schuetzenberger, - 1865
Desoxymorphia, " " Wright, - 1871
Apomorphia discovered by Mathiessen and Wright, 1871
Bromomorphide,
Bromotetramorphine, " " Wright - - - 1872
Chlorotetramorphine,

2. Codeine (Paverine) discovered by Robiquet, - 1832

Its derivatives,

Dicodeine,
Tricodeine,
Tetracodeine,
Desoxycodeine, " " Wright, - 1872
Bromocodide,
Chlorocodide,
Apocodeine,

3. Narcotine (opium, anarconite, trimethylnonarcotine). discovered by Derosne, - - 1873

Jahresbericht ueber die Fortschritte der Pharmacie, von Wiggers und Huseman, 1872.

Its derivatives,

Bimethylnornarcotine, Methylnonarcotine, Nornarcotine, } discovered by Matthiessen and Foster, - - 1868

Tarconine, discovered by Jörgensen, - - 1869

Cotarnine, Opianic acid, } " " Woehler, - - - 1844

4. Narceine " " Pelletier - - 1833

5. Pseudo-morphine (Phormine), discovered by Pelletier, - - 1835

6. Thebaine (Paramorphia), discovered by Pelletier, 1835

Its derivatives,

Thebenine, Thebaicine, } discovered by Hesse, - - - 1870

7. Papaverine, " " Merck, - - 1848

Its derivatives,

Nitropapaverine, Bromopapaverine, } " " Anderson, - 1855

8. Opianine, " " Hinterberger, - 1851

9. Cryptopine, " " Smiles and T. and H. Smith, - - - - - - 1864

Its derivative.

Nitrocryptopine " " Hesse, - - 1872

10. Meconidine, " " " - 1870-1871

11. Lanthopine, " " " " "

12. Codamine, " " " " "

13. Laudanine, " " " " "

14. Laudanosine, " " " " "

15. Protopine, " " " " "

16. Hydrocotarnine, " " " "

The above sixteen principal bodies are seenrely grounded as organic bases.

DOUBTFUL BASES.

1. Porphyroxine (opin) discovered by Merck, 1837, is denied by Hesse as being a separate base.

2. Metamorphine, discovered by Wittstein.

3. Rhoeadine, " " Hesse.

4. Denteropine, " " " - - 1871

NON-NITROGENOUS NEUTRAL BODY.

5. Meconine, (Opianyl), discovered by Dublanc, 1836

ORGANIC ACIDS.

6. Meconic, discovered by Sertuerner, - - 1805
7. Thebolactic, " " T. and H. Smith.

The per centage[1] in which some of the alkaloids exist is as follows:

Morphia - - -	2–15 per cent.
Narcotine, - -	4–6 " "
Papaverine, - - -	abont 1 " "
Codeine, - - -	less than 1 " "
Thebaine - - -	less than 1 " "
Narceine, - - -	.01–.02 " "

Opium[2] *en masse* causes a sleepy nearly narcotic action, quickly disappearing, whilst morphia produces a deep and prolonged sleep. Morphia disturbs the stomach more than opium and its action is more prolonged. Opium is in some cases better borne than morphia; in others the reverse happens. After either opium or morphia there is a period of excitation which in the case of morphia soon passes into sleepiness, whilst after opium this excitement lasts longer. Opium causes a more lasting tingling in the skin than morphia or codeia, and constipates more than morphia, but does not cause as much diaphoresis. Morphia increases the urinary secretion more than opium.

Dr. Eisenmann, of Würzburg, has shown that opium increases the curative properties of all the heroic remedies, and diminishes their toxic. Bernard found that a subcutaneous dose of morphia causes the amount of chloroform for purposes of anæsthesia to be smaller in quantity.

Dr. Da Costa discovered that the after-effects of morphia could be subdued or prevented by bromide of potassium.

Whilst opium in bulk is narcotic, its alkaloids are tetanic as well as soporific, these elements are powerful and in some cases antagonistic.

It is only by the separate study of them that we can elect

1. Hermann's *Toxicologie.*
2. Köhler's *Handbuch.*

the effect which we desire. In the study of poisons there are three sources which can be benefited; they are physiology, toxicology and therapeutics. As, for example, the action of atropia to paralyze the pneumogastric, to counteract an overdose of morphia, or to save cases of poisoning by *agaricus muscarius*, a variety of poisonous mushroom. Whilst the poisons of the mineral kingdom, when introduced into the system, unite to form compounds with albuminous bodies of the tissues causing a destruction of their functions; those of the vegetable, neither alter the tissues nor the blood, but seem to act upon the nervous tissue itself, probably by a chemical union with the same.* Brown and Fraser have shown that the chemical constitution of an alkaloid has a great influence on the action of a drug, as methylstrychnia acts not like strychnia, but like curarine.

All the alkaloids used by me were obtained from Merck's laboratory, Darmstadt, and were administered hypodermically. In studying the action of them, I made use of frogs and man.

As the action on the frog's heart will be studied, I will briefly give a general view of the cardiac nervous apparatus. There are situated in the heart, nerve masses, called ganglia: Remak's in the sinus venosus; Bidder's in the left auriculo-ventricular septum and Ludwig's in the auricular septum. The excito-motor are comparable to the steam of an engine, the muscles of the heart to the driving-wheels; whilst Ludwig's ganglia are similar to the fly-wheel or governor. The pneumogastrics are united with the inhibitory ganglia, and when atropine, nicotine, conia, or lobelina are administered, these nerves are paralyzed. When muscarine is administered, the heart is stopped in diastole by excitation of the inhibitory ganglia. If now, atropine is administered, the heart will commence to beat again, but if nicotine is given, no such an effect will take place.

The following experiments illustrates this:

Exp. 1. Frog at 5.8 P. M. Heart beating normally. 5.10

**Pharmakolog. Untersuch.* von Dr. M. J. Rossbach I. Band, 1874. III. *und* IV. Heft.

P. M. .001. gr. muscarine injected into the vein.—heart immediately arrested, makes slight contractions when tapped. 5.17 P. M. 1-40th of a drop nicotine by the vein. Up to 5.20 P. M., no contraction followed, when a milligramme of atropine was injected, and it began immediately to feebly beat.

To explain this, Schmiedeberg assumes the arrangement to be as follows:

Muscarine stops the heart by irritating the inhibitory ganglia, whilst nicotine like atropine paralyzes the pneumogastrics; the former paralyzes only the apparatus connecting the trunk of the pneumogastric with the inhibitory ganglia, but atropine paralyzes the inhibitory ganglia.

Although the inhibitory fibres of the pneumogastric are paralyzed by both atropine and nicotine, yet on irritating the trunks of the vagi it is found that there are accelerating fibres which run in them and are not affected, as in the following experiment:

Exp. 2. Frog was given ½ gtt. drop nicotine subcutaneously. 4.25 P. M.; heart beat 28. 4.27, P. M.; heart beat 36, vagus irritated with Dubois's coil at 130 millimetres. 4.37, P. M.; heart beat 24. 4.40 P. M.; heart beat 32, vagus irritated with Dubois's coil at 50 millimetres. 4.45 P. M.; heart beat 24, ligature placed around the pneumogastric and the nerve irritated on the side of ligature farthest from the heart. 4.46, P. M.; heart beat 24.

Hence, it is inferred that in point of fact the inhibitory fibres are not really paralyzed, but seem so because the ganglia by which they express their action are. If conia is given, there is a true paralysis of the inhibitory fibres because there are no accelerating influences exerted on the heart on irritating the vagi. It was shown that a similar alkaloid, lobelina, paralyzes the pneumogastrics. To discover if it paralyzed the inhibitory ganglia or the trunks of the vagi, or the apparatus connecting the inhibitory ganglia with the trunks of the pneumogastrics the following experiments were made:

Exp. 3. Frog at 8.18 A. M., heart beating 48 per minute, when a little chloride of muscarine was subcutaneously injected. 8.21 A. M.; the heart was arrested in diastole. 8.24 A. M., two drops of the acetate of lobelina were introduced subcutaneously.

8.51 A. M., the heart remains quiet.
8.52 A. M., .001 gramme of atropine under the skin.
8.53 A. M., a feeble contraction is seen.
8.59 A. M., eight contractions.
9 A. M., .001 gramme atropine.
9.5 A. M., eight contractions stronger.
9.10 A. M., " " "
9.12 A. M., twenty "
9.15 A. M., thirty-two contractions.
9.20 A. M., thirty-four "
9.22 A. M., forty "

As will be seen by an examination of the above experiment, sufficient time was given for the lobelina to paralyze the inhibitory ganglia, for it acts slowly, yet it was unable to start the heart as atropine did subsequently. When lobelina was first administered and then muscarine given, the slowing of the heart still took place.

Exp. 4. Frog at 9.32, received two gtt. of lobelina under the skin.

11 A. M., heart beat 52., two milligrammes of muscarine was then injected subcutaneously.

11.4 A. M., heart beat, 48.
11.5 " " " 44.
11.8 " " " 40.
11.14 " " " 32.
11.22 " " " 28.
1.36 P. M. " " 32. .001 atropine.
1.40 " " " 36.
1.42 " " " 40.

Exp. 5. Frog at 1.30 P. M., received three drops of lobelina subcutaneously. 5 P. M., heart beating twenty per minute. A few milligrammes of muscarine was injected by the vein, and the heart stopped in diastole immediately.

These experiments demonstrate that lobelina does not paralyze the inhibitory ganglia like atropine.

Dr. A. B. Meyer* has shown that when the vagi are paralyzed by nicotine, that irritation of the sinus venosus stopped

*Das Hemmungs-Nervensystem, 1869.

the heart in diastole. Now, if after giving lobelina, irritation of the sinus venosus arrests the heart, there is additional proof that lobelina does not paralyze the inhibitory ganglia.

Exp. 6. Frog at 1.21 P. M., received three drops of lobelina. 4 P. M., the sinus venosus was irritated and arrest of the heart in diastole for five seconds followed.

The next question that arises, does lobelina paralyze the accelerating fibres of the pneumogastric, or does it, like nicotine, leave them intact? With this view the next experiment was instituted.

Exp. 7. Frog at 1.20 P. M., had left vagus carefully prepared and placed on a thread, with one Grove cell eighty-one millimetres high and fifty-eight millimetres in diameter, and Dubois's secondary coil at ninety-three millimetres, the nerve was irritated and the heart stopped in diastole.

1.21 P. M., three gtt. of lobelina given subcutaneously.

1.56 " heart beat 24

1.59 " heart beat 24; vagus irritated at 70 mm. for fifteen seconds.

2.7 P. M., heart beat 26; coil at 70 mm. and pneumogastric irritated.

2.11 P. M., heart beat 24; vagus irritated with coil at 0. Helmholtz's modification of the instrument put on.

2.26 P. M., heart beat 28.

2.27 P. M. heart beat 28. vagus irritated thirty seconds with coil at 0.

2.56 P. M., heart beat 25.

2.57 P. M., heart beat 26, vagus irritated sixty seconds. Coil at 0.

As is seen, there is no increased rapidity of the heart's action such as would take place with nicotine. Now, as lobelina does not act on the inhibitory ganglia, and does paralyze the accelerating as well as the inhibitory fibres of the pneumogastric, the inference is that lobelina paralyzes the vagi by acting on the nerve trunk itself, just as it does on the motor nerves themselves.

The fact that after the administration of atropine, calabar bean is able to slow the heart, has led some to infer that it and muscarine act on different parts, but after atropine

is given, Boehm has shown that the heart was slowed some and returned to its original beat on the administration of atropine. I have also seen a similar action take place so that at present there is no very good reason to assume that muscarine and physostigmine act on different ganglia.

CRYPTOPIA.

This body was first discovered by Messrs T. and H. Smith, of Edinburgh, in 1864. It melts at 217° C., is easily soluble in chloroform, scarcely soluble in alcohol and insoluble in ether. It is basic, and forms salts with acids.

Dr. S. W. Mitchell[1] found that one-fifth of a grain of Messrs. Smith's make, introduced under the skin of a pigeon caused death on the third day, which was probably due to other causes, as he supposed.

Dr. Immanuel Munk[2] who obtained the drug from Dr. O. Hesse, of Berlin, arrived at the following conclusions:

1. Cardiac movements were slowed by moderate doses, and through large doses completely arrested; this arrest being due to paralysis of the cardiac muscle.

2. Large doses paralyzed the respiratory centers thus causing death. The spinal centers are also paralyzed and there is a loss of reflex excitability, and disturbance of movement.

3. In warm-blooded animals the cardiac contractions were reduced independently of the lessening of the respiratory frequency. In warm-blooded animals the stoppage of respiration is preceded by violent spasms and convulsions which are not due to asphyxia, for they did not appear when artificial respiration was practiced.

Sippell[3] found that at first it acted as an excitant, then as a paralysant to respiratory centres. That the reduction of the respiratory frequency produces a considerable amount of carbonic acid in the blood which causes convulsions and death. It also reduces the excitability of the spinal cord, motor nerves and muscles. It arrests the heart by paralysis of the cardiac muscle.

1 *Amer. Med. Jour.*, Jan., 1870.

2 *Versuche ueber die Wirkung des Cryptopins.* Thesis, Marburg, 1873.

3 *Fortschritte der Pharmacognosie, Pharmacie und Toxicologie* von G. Dragendorff, 1874.

Harley* found it to be only one-fourth as powerful as morphia as a hypnotic. It is also a convulsivant, and he states that there are about four ounces of this body to a ton of opium.

The cryptopia was administered in the form of the acetate, it being dissolved with a slight amount of heat.

Experiment 8. Frog at 2:7 P. M., received .02 gramme of cryptopia. 2:30 P. M., there are cramp-like movements, is partially paralyzed and sleeping. 2:39 P. M., posterior extremities only seem hyperæsthetic, the remainder of the body not. A wave of contraction runs from the body outward when it is touched, respiration slower. 2:50 P. M., legs extended. 4:35 P. M., frog is sleeping and recovering. Next morning is still greatly sensitive, and croaks when touched, a characteristic of many of the opium alkaloids. The sensibility is so great that he is elevated on the tips of his four extremities when touched.

Experiment 9. Frog at 10:40 A. M., received .005 gramme of cryptopia subcutaneously, respirations frequent. Animal sits still, but manifests a slight disposition to jump when touched. 10:42 A. M., pupil dilated. 10:45 A. M., hops about. 10:50 A. M. .005 grains of cryptopia. 10:52 A. M., shows little inclination to move, respirations frequent. 11 A. M., 005 gramme injected, is sleeping, fore extremities weak, posterior extremities strong. 11:12 A. M., on irritation the movements are feeble. 11:24 A. M., lies with extremities extended, pupils contracted, respiratory movement now and then. 11:59 A. M., .005 gramme of cryptopia, pulse 11 per quarter of a minute. 12:2 P. M., 005 gramme of cryptopia. 12:7 P. M., heart beat 4 per quarter, no respiratory movement, occasionally spontaneously struggles. 12:20 P. M., death. Motor nerves irritable with Dubois's coil at 125 millimetres one Grove cell, probe thrust down the spine excites contraction, heart beat four per minute, ventricle in complete diastole, filled with blood, when pricked, beats.

Experiment 10. Frog at 1 P. M., received .015 gramme of cryptopia, at first sits quietly, then leaps away, respiration frequent and pupils dilated. 1.57 P. M., hardly any movement on pinching, no respiratory action. Death at 4 P. M.

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* *St. Thomas Hospital Reports*, 1871, Vol. II., p. 133.

Experiment 11. Frog at 3:37 P. M. received .0025 gramme of eryptopia, some escaping, respirations frequent, pupils dilated. 3:55 P. M. loss of sensibility and motility. 4. P. M. respirations labored, pupil contracted, fore extremities weak. Death at 4:5 P. M.

The action of eryptopia is shown by the above experiments to be as follows: the frog sits still at first, then leaps away, frequency of breathing and dilatation of pupil, loss of co-ordinating power over the anterior extremities then over posterior, increase and subsequent decrease of sensibility, diminution of respiratory movement, heart slowing and finally diastolic arrest. It is filled with blood.

ACTION ON MOTOR NERVES.

In studying the action on them I employed Dubois' induction apparatus and a Grove cell. The nearer the secondary coil is approximated to the primary the less is the excitability of the nerve. By tying one of the iliae arteries, I preserved the sciatic nerve of that side from the action of the poison.

Experiment 12. Frog at 11:30 A. M. received .01 gramme of eryptopia subcutaneously, right iliac artery previously ligated. 11.45 A. M. pupils dilated, leaps away with difficulty, respirations slower. 12 M., frog is sleeping, fore extremities weak, respirations slow and superficial. 12:35 P. M., .01 gramme eryptopia, general tremors of the whole body, crawls, movements jerky, unsteady. 12:50 P. M., respirations now and then, pupil contracting, when feet are tapped there are tremulous movements. 1:26 P. M., .005 gramme injected. 1:47 P. M., .005 gramme again injected. 4 P. M., frog dead. No reflex movements upon irritation of central end of sciatic of either side. Motor nerves of poisoned side irritable at twenty-five centimetres, of sound side at 34.5 centimetres. Spine thrust causes movement, heart beat, thirty two per minute.

Experiment 13. Frog at 10:55 A. M., received .025 gramme of eryptopia subcutaneously, some escaping right iliac artery previously tied. 11 A. M., no respiratory movement visible, gasps with mouth opened widely, pupil contracted, hops about, fore extremities weak. 11:7 A. M., sleeping. 11:45 A. M., lies sprawling. 12:30 P. M., dead. Right sciatic irritable

at fifty-two centimetres: poisoned sciatic at forty-two centimetres, irritation of spinal cord produces slight twitches in both extremities, heart making a few feeble movements.

These experiments show that it requires a stronger electric current to excite the sciatics which were acted upon by the cryptopia than those which did not receive it. The conclusion necessarily follows that crytopia slightly lowers the irritability of the motor nerves.

SENSORY NERVES.

To study the action on the sensory nerves I ligated the iliac artery on one side which would prevent the poison destroying the sensory nerves in that extremity, though the motor nerves are depressed in their irritability they are yet able to carry nervous impulses, as was shown when the spine was irritated. Hence any want of reflex action must be either in the spinal cord or the sensory nerves.

Experiment 14. Frog received at 11:45 P. M., .03 gramme of cryptopia, right iliac artery previously ligated. 12 M., pupil, after a short stage of dilatation, contracted, respirations now and then. 12.15 P. M., makes slight movements, lies sprawling, pupils contracted, no respiratory movement. 1:40 P. M., dead, no movement on irritating the skin of either posterior extremity. Sciatic nerves ligatured, but no reflex action by irritating their central extremity. Heart does not beat.

Experiment 15. Frog at 12:15 P. M., received .04 gramme of cryptopia. Right iliac artery previously ligated. 12:19 P. M., pupil slightly contracted, makes a respiratory movement now and then, fore extremities weak. On pinching skin of either side no reflex action. 1 P. M., death. Trunk of right sciatic irritated and no reflex movement. These experiments prove that the sensory nerves are probably not at fault, as in the right sciatic they did not receive any poison, and consequently could call out reflex movements if the spinal cord and motor nerves and muscles were able to act. I have previously shown that the motor nerves and muscles were able to carry impulses, so the inference is that the want of reflex action is situated in the spinal cord. The study of a poison on the sensory nerves is difficult and all results on them are not as satisfactory as they might be.

REFLEX ACTION.

For considerable time it has been known that after amputation of the head, the reflex excitability of the spinal cord was increased.

Setschenow discovered that mainly in the thalami optici and corpora striata are situated nerve centres which inhibit reflex action. Now if the thalami optici or the corpora quadrigemina are irritated there is a considerable depression of spinal excitability. To estimate the amount of this reduction a frog is hung up in a vertical direction, and one of his hind feet is allowed to rest in a very dilute solution of sulphuric acid till it is withdrawn. The time that the foot is in the water expresses the amount of inhibition of reflex action, and is measured either by a metronome or clock beating seconds. The cerebrum is ablated to prevent voluntary movements. In the living frog this is done by cutting in a line with the anterior edge of the membrana tympani.

Experiment 16. Frog cerebrum ablated.

Time of reflex movement by thevery dilute acid solution	Seconds.	Heart beat per minute
2.10 P. M.	20.	60
2.14 "	.004 gr. cryptopia	
2.17 "	18	60
2.21 "	18	58
2.32 "	11	48
2.35 "	7	49
2.47 "	7	40
2.54 "	10	36
3 "	no reaction	36

Experiment 17. Frog cerebrum ablated.

12.12 P. M.	10	48
12.14 "	Cryptopia .002 gr.	
12.20 "	6	
12.24 "	9	
12.36 "	5	
12.40 "	6	
12.43 "	9	
12.46 "	12	
12.56 "	15	

1	"	21	
1.6	"	25	
1.7	"	medulla divided	
1.14	"	5	
1.19	"	9	
1.22	"	8	
1.33	"	6	40

These experiments show that cryptopia at first increases and then decreases reflex excitability. Setschenow's centres are not paralyzed, as after their removal the normal increase of reflex excitability takes place. The question that arises here is, is this effect a direct action of cryptopia on the nervous system or is it indirect by the slowing of the heart, or through the hemorrhage of the operation?

The effect on the spinal cord itself will decide on what part cryptopia acts.

Experiment 18. Frog, medulla severed.

	Seconds.	Heart Beat.
2:10 P.M.,	10	
2:11 "	.004 gr. cryptopia.	
2:14 "	9	
2:16 "	10	
2:19 "	9	
2:22 "	7	
2:25 "	6	
2:34 "	12	
2:54 "	14	
3:10 "	19	40

Here cryptopia increases, and then decreases reflex excitability, so that the action of this drug is wholly on the spinal cord itself, and not on the nerve centres of Setschenow.

Now, Weil* has shown that hemorrhage excites the inhibitory activity of Setschenow's centres, but has no action on the reflex activity of the spinal cord, where cryptopia acts. He also has shown that arrest of the heart throws this inhibitory centre into activity, but has no influence that is immediate on the spinal cord.

**Reichert's Archiv.* 1871. No. 3.

Now as the increase and decrease of reflex excitability is wholly spinal, this must be a direct action of cryptopia, and not through slowing of the heart, or hemorrhage from the operative procedures.

ACTION ON THE MUSCLE.

In studying the action on muscle I poisoned the frog with cryptopia, and after a few hours severed the medulla, destroyed the spinal cord, and placed the animal on Marey's myographic table, and attached the tendo-achillis to the myograph of Marey; the nerve or muscle then was irritated and the curve registered. I used as registering apparatus Secretan's second axis. This curve was compared with that obtained from an unpoisoned frog. I found that the height of the muscle curve was less than normal in the cryptopia series.

CIRCULATION.

		Heart Beat.
Experiment 19.	Frog at 9:53 A.M.	48
	9:56 " cryptopia	.01 gr.
	9:57	44
	10:00	40
	10:02	40
	10:36	36
	10:37, cryptopia,	.01 gr.
	10:49	32
	10:51, cryptopia,	.02 gr.
	11:18	28
	11:25	26
	12:00 M.	24
	12:30 P.M., cryptopia,	.01 gr.
	1:19	20
	2:07	20
	5:00, beating now and then, heart in diastole.	

This experiment demonstrates that cryptopia slows the heart. Now this result must be due either to an increased activity of the cardio-inhibitory apparatus, or lessened activity of the excito-motor ganglia, or the cardiac muscle itself.

Experiment 20. Frog, vagi divided.

11:12 A.M. heart beat, 36
11:15 " cryptopia, .02 gr
11:17 " 32
11:25 " 32
12:00 M. 8
12:10 P.M. 8
12:30 8
12:35 vermicular movement now and then of the ventricle, ventricle in diastole, as is seen during irritation of the pneumogastric.

12:56 P.M. Infus. digitalis, 2 cubic centimetres.
12:57 " 16 "
2:00 " heart stopped.

There is certainly no stimulation here of the central end of the central inhibitory apparatus, for such action is excluded by the section of the vagi.

Experiment 21. Frog received at 3:55 P.M., .002 gr. of atropine, and at 4:05 P.M., .005 gr. of cryptopia; 4:12 P.M., .005 gr. of cryptopia; 4:20 P.M., .01 gramme of cryptopia.

4:40 P.M. Heart beat, - - - - 48
5:25 " " " - - - - 32
6:22 " - - - - - - - 32
11:00 " Heart in diastole, and at rest.

Here cryptopia, notwithstanding the paralysis of the peripheral end of the cardio-inhibitory apparatus, still arrests the heart. Now this slowing must be seated either in the excito-motor ganglia or in the cardiac muscle itself. As the striated muscles have diminished irritability, and the cardiac muscle soon loses its excitability, it is probable that the slowing is due to weakness of cardiac muscle. The revival of the heart beat by infus. digitalis is also confirmatory evidence, as this drug is a stimulant to cardiac muscle.

ACTION ON MAN.

Experiment 22. Pulse 76; respiration 16 at 6:55 P.M.
6:56 P.M. Cryptopia, gr. ½ by the mouth.
7:20 P.M. Pulse 82; respiration 20; pupil slightly dilated;

slight tingling of the skin; feel sleepy during the evening; there is no nausea.

In accordance with previous observers, eryptopia is a pleasant hypnotic.

THEBAIN.

This alkaloid was discovered by Pelletier and Thibourmery in 1832. Magendie first announced that it was a tetanic agent. Orfila confirmed it, as did also Bernard, Baxt. Mueller,[1] Crum-Brown and Frazer, S. W. Mitchell,[2] Rabuteau and Ozanam.[3] I[4] have also studied it, and found it to be a spinal convulsivant, acting neither on the motor nor sensory nerves or muscles; that it increases the pulse and blood-pressure by an action on the vaso-motor centre and the heart itself, and that the reflex action of the depressor nerve is not interfered with.

Experiment 23. Small frog at 8.27 A.M. received .005 gr. of thebain; 8.29 A.M., pupil dilated, respirations slow; 8.30 A.M., tetanus, pupil contracted; 8.47 A.M., .0025 gr. of thebain; 9.15 A.M., pupil contracted; 10.00 A.M., animal dead; motor nerves are irritable.

As is seen, thebain is a tetanizing agent.

Experiment 24. Frog received .005 gr. of thebain by the stomach at 10.10 A.M; 10.15 A.M., tetanus followed; head severed, but tetanus still occurs occasionally.

As is the case with strychnia, thebain excites convulsions, which are spinal in origin.

MOTOR NERVES.

Experiment 25. Frog, right iliac artery and vein ligated; at 10.18 A.M., when he also received .015 gr. of thebain; 10.22 A.M., tetanus; 10.23 A.M., .005 gr. of thebain; pupil contracted; 11.05 A.M., dead; sound nerve responds at fifty centimetres; poisoned nerve at forty-five; spine thrust; causes a few twitches in femoral muscles.

The motor nerves, as well as muscles, retain their normal irritability.

1. *Das Thebain, eine Monographie.* Marburg, 1868.
2. *American Medical Journal*, January, 1870.
3. Dr. G. B. Woods' *Therapeutics.*
4. The physiological action of thebain. *Boston Med. & Surg. Jour.* Apr. 1875.

SENSORY NERVES.

Experiment 26. Frog at 10.42 A.M., received .002 gr. of thebain; right iliac artery tied; tetanus follows; death at 11.40 A.M. On irritating the skin of either side, or the central end of the sciatics, there ensues no reflex action.

Experiment 27. Frog, abdominal aorta ligated; .0025 gramme of thebain injected under the skin of the lower jaw; tetanus followed over the whole body.

Experiment 28. Frog at 9.49 A. M. had spinal cord cut below the origin of the nerves of the anterior extremities, the spinal branches of the aorta severed; .0025 gr. of thebain injected under the skin of the lower jaw; 10.00 A.M., convulsions of the anterior extremities, reflex action in posterior extremities; 10.15 A.M., convulsions of anterior extremities, none in posterior extremities, although reflex action still continues.

By the ingenious methods of Brown-Séquard[1] in experiments twenty-seven and twenty-eight, I found in the first that tetanus supervenes in the posterior extremities when they do not receive any poison; and in the second experiment, that when the spine did not receive the poison, but the sensory nerves of the posterior extremities, there was no tetanus in them. Hence the conclusion is that the tetanus is generated by a direct action on the spinal cord.

ACTION ON THE MUSCLE.

By studying the muscle-curves obtained by the method already detailed under the head of "Cryptopia," I found no change in them.

ACTION ON THE CIRCULATION.

I have shown that in warm-blooded animals the increase of pulse is due to an action on the heart, and the increase of blood-pressure to an action on the vaso-motor centre.

CODEIA.

It was thought by Kunkel[2] to be a narcotic, and that it was distinguishable from morphia by not causing, like the latter, the hyenoid posture, and that it was convulsivant.

1. *These pour le Doctorat en Medicine*, par F. W. Bonnefin.
2. Husemann. *Die Pflanzenstoffe.*

He stated that the cardiac and respiratory movements were increased in frequency. Berthè observed that it caused stupor, paralysis of the posterior extremities, weakening of the heart-beat, diminished respiration, sleep, followed by convulsions, and death. He stated that it produced the hyenoid posture, and saw no acceleration of the pulse or respiration. Bernard[1] observed that it caused a sleep not as deep as that of morphia. On frogs, according to Cogswell and Albers, it was a convulsivant, causing hyperæsthesia.

Baxt[2] observed coma, energetic contractions upon external irritation. Wachs saw in a frog a change of carriage, due to want of power over the adductors, a peculiarity I have also seen after narceine and papaverine; loss of movement, stretching cramps, clonic convulsions. Crum-Brown and Frazer, in rabbits, saw convulsions. My specimen dissolved without color in sulphuric acid, which heated to 150° and cooled, give with nitric acid a blood-red color.

Experiment 29. Frog at 11.16 A.M. received .002 gr. cod. sulph., sits still.

11.25 A.M., .008 gr. cod. sulph., sleeping.

12.00 M. Croaks when touched, hyperæsthetic.

1.10 P.M. Hyperæsthesia very great, so much so that touch throws the body into marked contraction.

Next day, 1.36 P.M., same state; .010 gr. codeia sulph. subcutaneously; 2.27 P.M., tetanus of the whole body on touch. On third day, after poisoning, tetanus, crawls with a trembling gait. During the fourth and fifth day it seemed to have recovered from the tetanus, but on the sixth day it again broke out, and death ensued.

Experiment 30. Frog at 10 A.M. received .002 gr. cod. sulph.; at 10:04 A.M., .002 gr. cod. sulph.; 11.45 A.M., tonic cramps—sleeping. On section of cord the cramps continue.

These experiments show that codeia is narcotic and convulsivant.

ACTION ON MUSCLE CURVE.

Following the usual plan already detailed, I obtained curves, which have a longer period of relaxation than takes place in

1. *Leçons sur les Anesthetiques.*
2. *Reichert's Archiv.* 1869.

normal muscle. The curve is similar to that produced by veratria.

ACTION ON CIRCULATION.

Experiment 31.	9.20 A.M.	Heart beat, 46
	9.20 "	Cod. sulph. .002 gramme.
	9.22 "	28
	9.23 "	32
	9.24 "	Cod. sulph. .002 gramme.
	9.25 "	30
	9.28 "	28
	9.49 "	26
	10.00 "	24
	11.05 "	24
Experiment 32.	Small frog, .002 gr. atropine subcutaneously.	
	10.25 A.M.	Heart beat, 36
	10.26 "	Codeiae sulph. .002 gr.
	10.27 "	36
	10.28 "	35
	10.31 "	35
	10.37 "	30
	10.59 "	27
	11.10 "	24
Experiment 33.	Frog.	Vagi cut. Heart beat.
	5.02 P.M.	28
	5.04 "	Cod. sulph. .010 gramme.
	5.05 "	24
	5.08 "	20
	5.16 "	20
	5.28 "	20
	5.42 "	20
	6.19 "	16

As is seen, the fall of heart-beats occurs after the use of atropine, and section of the vagi. Now as the striated muscle ia affected by codeia, the inference is that, the cardiac muscle may suffer the same fate.

ACTION ON MAN.

Experiment 34. At 3.20 P.M., pulse 71; respirations 19: 3.27 P.M., ½ gr. cod. sulph.; no change up to 4.07 P.M., when ½ gr. cod. sulph. was taken.

4.20 P.M. Pulse 76; respirations 22; dilatation of the pupil: feel quite sleepy; tingling through the skin.

5.12 P.M. Very sleepy; lie down; supper was taken at 5 P.M.; pulse 88; respirations 20; pupil dilated.

6.12 P.M. Pulse 80; respirations 20; pupil contracted; recovered fully by 10 P.M., although nausea and depressant action follows.

In my case codeia was a hypnotic, followed by considerable gastric disturbance.

CHLOROCODIDE.

That the paper may be more complete, I shall add to it the existing knowledge of the derivatives of the alkaloids.

Dr. Gee[1] noticed that in a cat it produced salivation; dilatation of the pupils, with restlessness, tetanus and death.

It is extremely bitter, but does not produce in man the aching fullness of quinine.

APOCODEIA.

Dr. Wickham Legg[2] states that the chloride in grain doses in dogs produces in five to ten minutes vomiting, and four to five grains, coma and death.

Its action on man is not anything at all, or feeble.

NARCEIN.

Bernard first stated that this body was an excellent narcotic. This has been refuted by Harley, Dr. S. W. Mitchell, and Dr. Da Costa. Baxt observed that in frogs it produced excitement, and then a half comatose state, without any important alteration of the breathing or heart-beat. There was also depression of irritability. It is quite evident that all the bodies used under the name of narcein, were either impure or totally different articles.

This body dissolved in hydrochloric acid and water without color, with nitric acid it gave an orange color, with sulphuric acid a yellow color passing into a greenish yellow, and finally a reddish brown.

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1. St. Bartholomew's Hospital Reports. 1869. *American Med. Journal.* 1870.

2. St. Bartholomew's Reports. 1870; pp. 97.

Experiment 35. Frog at 2 P.M. received .02 gr. of narcein dissolved in glycerine by heat, and a drop of sulphuric acid.

2.10 P.M. Pupils dilated; respirations more frequent.

2.15 P.M. Violent extension and contraction of posterior extremities; anterior extremities normal, whilst legs are extended they are in a series of vibratory movement.

2.51 P.M. .02 gr. narcein.

4.49 P.M. Same state; .08 gr. narcein given.

5.00 P.M. Anterior and posterior extremities in a tetanic state; frog sleeping.

5.20 P.M. Death; motor nerves irritable, also muscles; spine on thrust feebly irritable; heart stopped in diastole, not irritable on pricking.

This experiment demonstrates narcein to be narcotic and convulsivant.

Experiment 36. Frog; left iliac artery ligated; received .011 gr. narcein subcutaneously at 8.30 A.M; 8.45 A.M., stiffness of movement in posterior extremities; as Wachs observed with codeia, I saw difficulty in adducting the legs; no trouble with anterior extremities.

10.00 A.M. Convulsions of a tetanic character in both posterior extremities.

11.45 A.M. Recovered.

As has been first shown, the cause of the convulsive movement is not situated wholly in the muscles, as it takes place in the ligated extremity, and consequently has not its seat in irritation of the endings of the sensory nerves, or endings of motor nerves; necessarily part at least of these convulsive movements must be spinal in origin.

Now, if curare is given, the endings of the motor nerves are paralyzed, and any convulsive movement will be due irritation of the muscle itself.

Experiment 37. Frog at 8.47 A. M. received .011 gramme of narcein, tetanic convulsions in posterior extremities followed.

9.16 A. M. Leg amputated, and still spontaneous contractions in the severed leg.

12 M., curare given.

1 P. M., contraction still observed in muscle of the body on irritation.

Experiment 38. Frog at 9.40 A. M. curarized. 9.48 A. M., completely paralyzed; narcein, .055 gramme given. 10.48 A. M., .011 gr. narcein.

11.24 A. M., on tapping, femoral muscles contract and twitchings ensue.

2.40 P. M. These twitchings still continue, although frog is perfectly motionless.

As is seen, part of the convulsive movements of narcein are muscular, for they supervene after use of curare in large doses, which has the property to destroy the motor nerves, as has been proved by Drs. S. W. Mitchell and Hammond. Hence narceine is narcotic, and produces convulsions by spinal and muscular irritation.

ACTION ON THE MUSCLE-CURVE.

On studying the action on the curve of a muscle, it was seen that narcein produced a veratroid contraction of striated muscles.

ACTION ON THE CIRCULATION.

Experiment 39.	Frog at	8.40 A. M.,	heart-beat 46.
		8.47 "	narcein .011 gr.
		8.48 "	28.
		8.51 "	32.
		9.00 "	32.
		9.12 "	36.
		9.16 "	spontaneous convulsion
		9.46 "	24.
		9.48 "	24.
		11.24 "	20.

Experiment 40.	Frog: vagi severed.		Heart-beat.
		12.34 P. M.	44.
		12.35 "	narcein .011 gramme.
		12.37 "	40.
		12.47 "	36.
		1.03 "	28.
		2.40 "	24.

During the afternoon heart stopped, irritable on pricking.

Experiment 41. Frog; vagi cut.		Heart-beat.
3.36 P. M.		36.
3.37 "	narcein .011 gr.	
3.38 "		32.
3.40 "		32.
3.44 "		32.
3.50 "		30 arrest in diastole for a few seconds.
3.52 "		16.
Experiment 42. Frog.	Atropine, .002 gr.	
12.00 M.		48.
12.04 P. M.	narcein .055 gr.	
12.07 "		44.
12.11 "	narcein .055 gr.	
12.12 "		48.
12.15 "		48.
12.35 "		48.
12.49 "		42.
1.04 "		42.
2.40 "		38.

As is seen, narceine reduces the heart-beat after the section of the pneumogastrics, but after the preliminary use of atropine, the heart regains its original number of beats and remains there some time, so that there is a stimulation of the peripheral end of the pneumogastric.

ACTION ON MAN.

Experiment 43. 3.25 P. M., heart-beat 64, respiration 20.

3.26 P. M., narceine sulph. ½ gr. by the mouth.

3.54 P. M., pulse 72, respiration 20, pupil same size.

4.00 P. M., sense of constriction through the temple. No hypnotic action observed.

Experiment 44. 10.54 A. M., pulse 64, respiration 20.

10.55 A. M., narceine sulph. gr. 1., which immediately produced a constriction about the temples.

11.30 A. M., pulse 64.

2.20 P. M. No hypnotic action during the day, but headache present.

Like Drs. Mitchell and DaCosta, I never found narceine to

have the slightest hypnotic effect. It is, however, a splendid article to produce headache. The only narcotic effects observed were seen in cold-blooded animals.

PAPAVERINE.

Bernard and others think it to be a convulsivant, whilst Baxt thought it to possess hypnotic qualities, and that it reduces the heart-beat by stimulating the cardio-inhibitory apparatus. He states that it reduces the irritability by paralysis of the reflex activity.

Leidesdorf thought it to be a hypnotic in man.

PAPAVERINE.

Nitric acid colored the crystals blue.

Experiment 45. Frog at 3.20 P. M. received .02 gramme dissolved in water with a drop of hydrochloric acid.

4 P. M. Tetanus in the posterior extremities of spontaneous origin; is in a semi-comatose state.

Experiment 46. Frog at 3.16 P. M. received .02 gramme dissolved in glycerine with a drop of sulphuric acid. 3.22 P. M., hops about. 4 P. M., tetanus of posterior extremities. 5 P. M., recovering, is in a semi-comatose state.

Experiment 47. Frog poisoned with curare. When motionless, .02 gramme of papaverine given, dissolved by sulphuric acid. No muscular twitchings were seen, but on placing the muscle in connection with the myograph, the curves were veratroid.*

Experiment 48. Frog received .02 gramme papaverine. Convulsions followed. The medulla was severed and they still continued. When the leg was amputated it had twitchings.

As is seen, papaverine produces a semi-comatose state, and is a convulsivant. As the twitchings continue in the amputated leg, it is evident that the convulsions are partly by an action on either the motor-nerve ends or the muscles, quite probably the latter. It also produces a disturbance in the innervation of the adductors of the lower extremities as does codeia.

* Ott. *Action of Medicines.*

ACTION ON THE MUSCLE-CURVE.

The striated muscle-curve is veratroid.

CIRCULATION.

Experiment 49. Frog; at 11.13 A. M., heart-beat 32.
11.15 " .01 gr. papaverine.
11.18 " 24.
11.19 "diastolic arrest 16.
11.21 " 32.
11.54 " 32.
12.12 P. M. .01 gramme.
12.14 " diastolic arrest.
12.15 " 24.
12.23 " 32.
2.13 " 20.

Heart makes now and then very feeble movements; motor nerves irritable; spine feebly so; infusions of digitalis cause heart to pulsate per minute twenty-four pretty strong beats, which soon disappear.

Experiment 50. Frog at 11.45 A. M. received .002 gr. atropine
11.50 " 44.
11.52 " papaverine .01.
11.53 " 40.
11.54 " 40.
11.56 " 44.
12.08 P. M. .01 gramme papaverine
12.09 " 36.
12.12 " 36.
3.22 " 36.

After the use of atropine, papaverine has little effect on the heart, which causes the inference that it reduces the heart-beat by peripheral stimulation of the cardio-inhibitory apparatus.

ACTION ON MAN.

Experiment 51. At 2.4 P. M., pulse 68; 1 grain of papaverine taken.

3.27 P. M. Pulse 68; 1 grain of papaverine taken.

4 P. M. Feel quite sleepy, lie down: pulse 68.

5 P. M. Have some pain and tension about the forehead; not so much sleepiness; recovering; no nausea.

I agree with Leidesdorf that this substance is hypnotic.

NARCOTINE.

It is by some thought to be narcotic, by others to be worthless as such. Bernard thinks it to be convulsivant, in which statement Baxt concurs, as well as Dr. S. W. Mitchell. Baxt states that in frogs it produces a half-comatose state.

When I treated it with concentrated sulphuric acid the solution became yellow, and finally a reddish yellow. By heating the solution it became yellowish-red, then bluish-violet on the edge, and finally a reddish-violet.

NARCOTINE.

Experiment 52. Frog at 11.28 A. M., received .002 gr. of narcotine sulph. 11.45 A. M., spontaneous convulsions. 12.10, medulla severed, but twitchings and tetanus still continue.

Experiment 53. Frog received .01 gramme of narcotine sulph. at 1.42 P. M. 7.40 P. M., there are convulsions, which continue after the severance of the medulla; there are no twitchings in the amputated leg.

Experiment 54. Frog at 11.49 A. M. received .020 gr. of narcotine dissolved in water by the aid of sulphuric acid.

3 P. M. Hyperæsthesia; received .020 gr. of narcotine sulph.

3.12 P. M. High state of hyperæsthesia; on pinching croaks.

3.24 P. M. Tetanic contractions of posterior extremities.

Next day hyperæsthesia; croaks when touched; hyperæsthesia for some days; recovers fully.

As is seen, narcotine produces hyperæsthesia and convulsions, which are of spinal origin, as they persist after severance of the medulla.

CIRCULATION.

Experiment 55. Frog at 9.49 A. M. Pulse 34.
9.50 " Narcotin. sulph. .002 gr.
9.51 " 20.

9.54 " 14 systole.
quick; diastole slow.
10 " 12.
10.4 " 10.
10.7 " 14.
11.5 " 16.

Experiment 56. Very small frog. Atropine previously given.

10.27 A. M. Heart-beat 28.
10.28 " Narcotin. sulph. .002 gr.
10.30 " 16.
10.30 " 2.
Diastole 15 seconds.
Systole 1 second.
11.10 " 4.
Heart immediately responds to stimulus.

Experiment 57. Frog. Atropine .002 gr.
11.25 A. M. 36.
11.26 " Narcot. sulph. .002 gr.
11.27 " 36.
11.29 " 28.
11.31 " 24.
11.32 " 16.
11.38 " 12.
11.45 " 8.

Experiment 58. Frog. Nicotine 1-20 gt.
11.50 " 40.
11.56 " Narcotin. sulph. .002 gr.
11.57 " 40.
12 M. " 28.
12.3 P. M. 24.
12.16 " 20.
12.30 " 14.

Experiment 59. Frog. Heart-beat. Vagi cut.
5.10 P. M. 44.
5.14 " Narcot. sulph. .020 gr.
5.15 " 40.
5.24 " 16.

5.30 "	12.
5.38 "	14.
5.42 "	12.
7.32 "	8.

These experiments demonstrate that narcotine has what has hitherto escaped notice—a very marked effect on the heart of cold-blooded animals. No other alkaloid of opium, that I am aware of, is its equal in the same dose, as regards cardiac effect. This slowing of the heart is not due to stimulation of the cardio-inhibitory apparatus, but is produced by an action on the muscle, as the systole in one experiment occupied only one second, whilst the diastole occupied fifteen. Now, narcotine affects striated muscles in a similar manner, for then contraction (systole) is quick, but relaxation (diastole) is slow, as is shown in the muscle-curve.

Experiment 60. Action on man.

At 2 P. M. one grain of narcotine sulph. taken. No narcotic effect observable.

4 P. M. Another grain taken, which also produced no effect.

COTARNINE.

Buchheim and Loos[1] have shown that in frogs it produces an increasing sleepiness and lassitude, the breathing is superficial, the animal lies on its abdomen, the extremities are not drawn up and are insensible to irritation. Its action is similar to that of curare. It produces, like it, paralysis of the motor nerves. As after curare, it produces in the frog dropsy in a few days.

HYDROCOTARNINE.

Falck[2] states that as a toxic agent it is weaker than codeia or thebain, but more fatal than morphia. The color of the blood is dark. Its action reminds one of codeia, that is, narcotic and convulsivant. The tetanic form begins with an increase of respiratory frequency which can be heard, and is often whistling, excitement, licking of body and salivation, active movement of the ears, whose blood vessels at one time are strongly con-

1. Eckhard's *Beitraege*, V. Band. 2 Heft.
2. *Toxikolog.* Studien ueber das Hydrocotarnin.

gested, at other times anemic, gradual or sudden dilatation of the pupil, trembling, spasmodic contractures which go into opisthotonus, with diminished respiration, relaxation and death, when the pupils narrow. In this form of poisoning, the thermometer rises. In the narcotic form, there is increased respiration and, after mydriasis, injection of the ears, restlessness and trembling, with contraction and the head sinks down; intercurrent convulsive movements come on till death. Here the temperature sinks. Frogs go into tetanus under it, but the heart beats thirty-six to forty-eight hours after apparent death. A heart brought to a stop by muscarine is started by hydrocotarnine.

HYDROCHLORATE OF COTARNAMIC ACID.

This body is produced by the action of hydrochloric acid to cotarnin. Wickham Legg states that in the dog it produces flickering contractions and weakness, the intellect seems perfect; falls over on his side; is insensible to ordinary stimulus; respirations fall with the pulse; spasms of the muscles, lasting ten seconds, and death. In other cases, vomiting and purging ensued before the just mentioned symptoms. The temperature and respirations fell, then rose and again fell. The pulse was greatly increased, but finally fell. It requires a considerable time for it to act, in some cases days. It has little influence on the motor nerves or muscles of mammalia, but frogs soon lose their muscular irritability. The vagus is paralyzed. The slowness of the development of the symptoms was thought to be due to the slowness of absorption.

LAUDANOSINE.

Wortmann* states that in rabbits it excites more and more the centres of respiration. The excitation spreads to the brain and spinal cord in such a violent manner that tetanus ensues, which, producing asphyxia, is the cause of death. The vasomotor centre is paralyzed, or at least considerably reduced in irritability, as there was injection of the ears and increased flow of saliva. The minimum fatal dose is 67.5 millgrammes to one kilogr. of body-weight of rabbit, when administered subcutaneously.

* *Jahresbericht uber Pharmacognosie*, 1874.

LAUDANINE.

Falck compared it with strychnia and brucia in its action. Its minimum fatal dose is .025–.03 gramme to one kilogr. of body-weight, and of hydrocotarnin .16 gr. to one kilogr., body-weight, so that in toxic power thebaine is first, then laudanin, then hydrocotarnine, then morphia, and then laudanosine. After death by laudanine, the left heart contains dark-red blood. As in thebain, Falck states that there are three periods. It causes an increase of respiratory frequency, due to an action on the respiratory centre. Finally there were convulsions and trembling. In the second period of opisthotonus emprosthotonus and trismus, during which no respiration takes place, and the pupil for the most part is dilated. Then follows relaxation of the muscles and limbs, adynamia, and death from the brain outward; the heart dies last.

MORPHIA.

Small doses cause acceleration of the heart, and later, sleepiness. Large doses increase the cardiac and respiratory frequency, the skin is warm and flushed, there is a sense of tickling in the skin, and a period of excitement. In the second period the respiratory and cardiac frequency sinks, the heart-beat is weak, and sleep follows. By subcutaneous injection there is a benumbing action at the place of injection. Lichtenfels discovered that when morphia was introduced either by the mouth or subcutaneously, that it diminished the sensibility to taste, which decrease was greater on the injected side. On frogs it acts like strychnia in many respects. On dogs it causes sleep, loss of reflex action except in the cornea. After waking they creep away, have a hyenoid gait. Dr. S. W. Mitchell has shown that pigeons have an especial immunity against it. In small doses* the pulse, as in man, is first accelerated, and then retarded. If it is injected into the veins, slowing takes place first, then acceleration of the heart. This slowing is a result of both central and peripheral stimulation of the vagus, and the acceleration is due to its paralysis. If all the nerves going to the heart are sev-

*Gscheidlen. *Untersuch. a. d. Physiolog. Lab. in Wuerzburg*, II., 1.

ered, then morphia causes a small acceleration and a considerable slowing. The arterial blood-pressure is elevated then lowered, due to stimulation and subsequent paresis of the vaso-motor centre. In dogs the flow of saliva is increased. According to Nasse[1] it increases intestinal peristalsis. It retards the breathing, and section of the vagi does not prevent it. The motor-sensory nerves and reflex actions are first increased and then decreased. Meihuizen[2] saw morphia depress reflex action, then return it to normal, and finally excitation. According to him, cramps ensue which are not of a reflex nature. The alkaloid is found in the urine, liver, and blood.

MORPHIA.

Experiment 61. Frog at 11.23 A. M. receives .010 gr. of sulphate of morphia. 1.36 P. M., .050 gr. 2.27 P. M., is sleeping.

6 P. M., some hyperæsthesia.

Next day, 12 M., relaxed dozing; when touched is elevated on his extremities and is croaking.

8.35 A. M., on third day, tetanus, which continued to the seventh day, when the animal died.

Experiment 62. Frog at 10.10 A. M., received .1 gr. of sulphate of morphia. 12.30 P. M., sleeping. 2 P. M., skin of back discolored to a dirty yellow, where the solution touched subcutaneously.

Experiment 63. Frog at 10.20 A. M., received .010 gr. of morph. sulph. During the day hyperæsthesia and sleep. Next day, at 10.10 A. M., convulsions, which continued after severing the medulla.

Experiment 64. Frog: at 10.04 A. M., heart-beat 44.

10.07	"	.010 gr. morph. sulph.
10.08	"	40.
10.10	"	40.
10.42	"	40.
3.05 P. M.		36.

1. Beitraege zur Physiologie der Darmbewegungen. Leipzig: 1866.

2. *Pflueger's Archiv.*, VII., 201.

As is seen, morphia is a tetanic agent, the seat of the convulsions being spinal. It also is narcotic. Its action on the heart of cold-blooded animals is very small.

ACTION ON MUSCLE.

It also produces veratroid contraction of the muscle.

OXYMORPHIA.

This is an oxidation product of morphia.

Dr. Kreis, in Hermann's laboratory, found that in frogs and dogs it had an extremely weak action like morphia. He perceived no action when .4 gramme was taken.

APOMORPHIA.

This body is obtained by heating morphia with hydrochloric acid; and was discovered by Matthiessen and Wright. It has no narcotic properties; it is an emetic. Asphyxia has no influence on its emetic power. It hastens the pulse by an action on the accelerator. Later, like all other emetics, it slows the pulse. There is no change of blood-pressure. The breathing is accelerated during the increased rapidity of the heart's action. It excites and depresses spinal irritability. Muscular irritability is reduced.

Exp. 64 a. Frog at 10.30 A. M. received .010 gr. of apomorphia. During the day there was hyperæsthesia and twitchings; no vomiting. Like Köhler, I have never seen them vomit; although, as Drs. Mitchell and Hammond have observed, I have seen a frog put his stomach out of his mouth after a dose by the mouth of veratria. Next morning there was paralysis of voluntary movement; lies with legs extended; croaks hyperæsthetic; touching him brings out convulsions.-After section of the medulla, they continued.

Exp. 65. Frog at 6.28 P. M. received .010 gr. of apomorphia. There followed hyperæsthesia, dragging of posterior extremities, and finally loss of all reflex action. Death at 9.40 P. M.

Apomorphia, as is seen, excites and then depresses spinal irritability; the muscle curves show that muscular irritability is reduced.

Exp. 66.	Frog.	5.28 P. M.	Heart-beat 32.
		5.29 "	Apomorphia .010 gr.
		5.31 "	36.
		5.32 "	32.
		5.37 "	28.
		5.41 "	28.
		6.19 "	24.

Its action on the heart is a lowering one.

MECONINE.

According to Dr. S. W. Mitchell it simply produces in pigeons an emetic effect. Harley believes it to have a weak hypnotic action which was not observed by Dr. Mitchell.

When my specimen was added to concentrated sulphuric acid it gave a purple color which by addition of water throws down a brown precipitate, which in ammonia solution gave a reddish color, disappearing on agitation.

MECONINE.

Exp. 67. Frog at 1.15 P. M. received .020 gramme of meconine dissolved in glycerine by the aid of heat. Hyperæsthesia followed. 3.10 P. M. .020 gramme meconine in warm water. 5.45 P. M. .010 gramme in hot water, some escaping. 6.32 P. M. extremities relaxed, frog is sleepy. For the three subsequent days there was paralysis of voluntary movement, extremities relaxed and extended. When touched, croaks.

As is seen meconine causes hyperæsthesia and subsequent relaxation, with a little sleepiness.

Exp. 68.	Frog at	1.13 P. M.	Heart 32
		1.15 meconine .050 in hot solution.	
		1.16	32
		1.25	36
		1.34	32
		1.54	26
		2.30	32
		3	32

It has little or no action on the heart in small doses.

ACTION ON MAN.

Exp. 69. At 11.27 A. M., pulse 64, respirations 20.

11.32 A. M., meconine—2 grains in water by the mouth.

1.32 P. M. Pulse 64, respirations 20 during the afternoon. No hypnotic effect.

In accordance with late observers, this article has no soporific effect. The difficulty of dissolving it may be one reason.

The following expresses the conclusions to which I have come in the light of my experiments:

1. Cryptopia is narcotic; excites, and then depresses reflex action by an effect on the spinal cord, reduces power of motor nerves, abolishes sensation by an action on the spinal sensory ganglia, and lowers the heart beat by an action on its muscular structure.

2. Thebaine is a spinal convulsivant, has no action on motor or sensory nerves, or striated muscle. It reduces the heart-beat by an action on that organ, and increases blood-pressure by stimulating the cerebral vaso-motor centre.

3. Codeia is a narcotic and spinal convulsivant; produces a veratroid contraction of striated muscle, and depresses the heart-beat by an action on the cardiac muscle.

4. Chlorocodide is a tetanic agent.

5. Apocodeia produces vomiting, coma and death.

6. Narceine is soporific to cold-blooded animals, but not to man, and is a spinal convulsivant. It does not destroy the motor nerves, as they act on thrusting a probe down the spine. It produces veratroid contraction of the muscle, and reduces the heart-beat by stimulation of the peripheral end of the pneumogastric.

7. Papaverine is narcotic and convulsivant; the convulsions being partly spinal and partly peripheral, the latter it is highly probable, from an action on the muscle. It diminishes the heart's contractions by peripheral action on the cardio-inhibitory apparatus. It also causes veratroid contraction of the muscle.

8. Narcotine is non-narcotic, and a spinal convulsivant; produces veratroid contraction of striated muscle, and is a very active agent to decrease the beats of the heart by an action on cardiac muscle.

9. Cotarnine is soporific, and paralyzes like curare the motor nerves.

10. Hydrocotarnine is narcotic and convulsivant.

11. Hydrochlorate of cotarnamic acid is a convulsivant, and paralyzes the pneumogastric.

12. Laudanosine and laudanine are tetanic agents.

13. Morphia is a narcotic and spinal convulsivant. It produces veratroid contraction of muscle, and reduces the heart-beat.

14. Oxymorphia has an action like morphia, only weaker.

15. Apomorphia is an emetic, excites and reduces spinal reflex excitability, and diminishes the number of cardiac contractions.

16. Meconine is narcotic to cold-blooded animals, but not in doses of two grains by the stomach in man. It causes hyperæsthesia and paralysis of voluntary motion with general relaxation. It also produces a veratroid contraction.

As has been stated, the alkaloids of opium all have a dominant action on the nervous system, causing first increased exaggerated functions, and if the dose is large enough, a paralysis of them. In cold-blooded animals this action is mainly spinal, because all their functions are mainly spinal. But as the spinal functions are subordinated to the action of the cerebral ganglia in the animal kingdom, so there is not so much spinal action, and more excitation and depression of the cerebral actions. Morphia in cold-blooded animals brings out the spinal actions, tetanus, whilst in man it brings out the cerebral actions which usually mask the spinal action, although occasionally there is seen only a spinal action and an excitation of the cerebral action. In such cases the fault is in the nervous constitution of the individual, the spinal functions not being under the control of the cerebral. Other excitants cause in children general convulsions which in the adult are without effect, because the predominance of the spinal functions are subordinated more in the adult to the cerebral ganglia than in childhood. As to the reasons why one element of opium is active in a lower animal and useless in man, the march of experimental science will decide; for the experimental study of the action of drugs is well able to take care of itself, thanks to

the labors of Bernard in France, Von Bezold, Böhm, Köhler, Schmiedeberg, Rossbach, and others, in Germany; to Brunton, Crum Brown, and Frazer, in England; Drs. S. W. Mitchell, Amory, and H. C. Wood, Jr., in this country.

I have appended here a table, showing approximately the soporific, convulsivant, and toxic relations of the elements of opium. They are arranged in the order of their strength. Some have been studied so little that their true place in the series cannot at present be definitely laid down.

Narcotic Effect.		Convulsivant Effect.	Toxic Effect.	
MAN.	ANIMALS.	ANIMALS.	MAN.	ANIMALS.
Morphia ...	Morphia....	Thebaine.....	Morphia ...	Thebaine.....
Codeia	Codeia	Laudanine	Codeia	Laudanine .....
Cryptopia ..	Cryptopia...	Laudanosine ...	Cryptopia ..	Laudanosine ...
Papaverine..	Narceine ...	Hydrocotarnine.	Thebaine ...	Hydrocotarnine.
....	Meconine ..	Papaverine	Papaverine .	Morphia .....
.....	.....	Narcotine	Narceine ...	.....
.....	....	Codeine	Narcotine ..	.....
.....	.....	Morphine	.....	.....

ART. III.—THE COST OF CONSTRUCTING HOSPITALS FOR THE INSANE.

BY I. RAY, M. D.

(Read to the Convention of Superintendents of Hospitals for the Insane, held at St. Louis, June, 1877.)

NOTWITHSTANDING our abundant experience in the building of hospitals for the insane, the prevalent notions respecting their cost are pervaded by a degree of vagueness and uncertainty hardly to have been expected from such experience. The fact is calculated to create a very unsatisfactory state of public feeling respecting the present methods of caring for the insane. Once, every State had come to the conclusion, in the fullness of time, that it was bound to provide for their

maintenance in buildings specially adapted to the purpose by their architectural arrangements, and various appliances understood to be conducive to the end in view. Of course this provision cost something, but the cost was kept so well within the limits of what seemed to be a proper expenditure, that fault was seldom found on that score. Of late years, however, without any diminution of interest in the purpose itself, but rather with an increasing disposition to fulfill its duties to this class of unfortunates, the public has begun to complain, in no gentle tones, that the cost of these establishments has often been carried to a pitch not required for the proper care and attentions for which they are designed, and seriously onerous to the taxpayer. And this has led to the idea that the greater part of the indigent insane can be comfortably provided for in a much cheaper kind of structure than those usually built. This idea, be it observed, rests on the fallacy that, because here and there a hospital has been built improvidently, therefore we should have few or none properly deserving the name. Under its inspiration some economical devices have been resorted to in one State or another, most, if not all of which, have strikingly illustrated the belief that there is no half way institution between a poor-house and a hospital. They show the operation of the inflexible rule that what is cheap in the beginning often proves dear in the end. If there is any lesson clearly taught by the experience of the last fifty years, it is that no really cheaper structure for the care of the insane of whatever grade, can be erected, than most of the State hospitals of our country. Occasionally, improper sites and injudicious contracts have carried the ultimate cost far beyond a suitable limit, but, with these exceptions, we believe the general rule to be as we have stated. As the matter now stands, it would seem that we have but one alternative—either to provide for our insane at a cost that would be a pretty high figure at a first-class hotel; or in structures so cheaply built as to entail a large annual expense for repairs and improvements, and wanting many of the arrangements now considered necessary in order to obtain the best results of hospital management. I do not believe, however, that we are really obliged to accept such an alternative. I believe that hospitals may be built wanting in

nothing conducive to the best results, at a cost which our people will cheerfully bear. Such hospitals have been erected, and I see no reason to doubt that they may continue to be. Of course, following the general rise of prices, they will cost more than they once did, but the means of the country have risen in a corresponding degree. In order to bring the present state of public opinion on this subject into a healthier tone, the first step is to show with some degree of precision, what a hospital for the insane properly provided for the purpose, and built as the humblest man governed by reasonable ideas of economy would build for his own habitation, should necessarily cost. And with this object in view, we can have no better guide than actual examples.

In 1846, the Butler hospital in Providence, R. I., with a capacity for 145 patients, was built at the cost of about $130,000, or $900 per patient. It is mostly of two stories, with rooms on only one side of the halls, and with enough of architectural adornment to distinguish it from a cotton mill or a county jail. As compared with many other hospitals, this might be considered a rather expensive style of building. On the other hand, it must be considered that the larger expense incurred by the arrangement of the rooms was offset by associated dormitories to a greater extent than is usual. The provision for ventilating and warming proved insufficient. Had it been made then as it was ten years afterwards, a farther outlay would have been needed of about $100 per patient. Very little—less than $1,000—was required for outbuildings to supplement those already on the place, and but little more in grading the grounds, making roads, etc. The land, consisting of 100 acres, cost $6,000 only. In those days, hod-carriers got eighty cents a day, and men laid bricks for $1.50. At present prices, the establishment must have cost a little more. Leaving out of the question the land, which would have cost $50,000, (and consequently would not have been selected) we may safely put the additional cost at twenty per cent., making the average cost, in round numbers, $1,200 per patient.

Four years ago, the capacity of the Pennsylvania hospital for the insane was enlarged by the erection of a new building, called the north Fisher ward. It is of two stories, with rooms

on both sides of the hall, provided with a boiler, engine and fan, and designed for thirty patients. The walls are rough-cast, the interior neatly and substantially finished, and nothing in the whole structure that could be dispensed with or changed for something cheaper, unless it may be a few wooden columns in the halls to give a little character to a recess, and which may have cost ten or fifteen dollars apiece. The cost of this building, including the arrangements for warming and ventilating, was $39,457, or $1,314 per patient. In this case, be it observed, there was no outlay for an administration building, nor for land, roads, grading, etc. Estimating the cost of these for a hospital of 400 patients at $100,000, this, distributed pro rata, would raise the cost of this new building an additional $7,500, or $250 per patient, making the whole necessary average cost per patient, $1,564. At the present scale of prices, the cost would have been some two or three hundred less.

The hospital now building at Warren, in Pennsylvania, intended to accommodate easily 600 patients, will cost, as the builder, Mr. John Sunderland, most confidently assures me, not over $1,500 per patient. And this building is understood to be fire-proof.

The public has sometimes been misled, I apprehend, as to the proper cost of hospitals, by examples of the actual cost of buildings supplementary to establishments already in operation. There being no outlay for land, roads, or administration building, and but little for ventilation and warming, the cost of such supplementary buildings is confined almost wholly to masonry, joinery and plumbing, and here, too, the cost is greatly lessened if the patients are chiefly lodged in associated dormitories. Some four or five years ago, a new wing was added to the Government hospital at Washington, to receive 140 patients. Excepting eight single rooms, the dormitories are of the associated kind, and the stories are only ten feet high. But the building is provided with all the modern improvements, and to say that the work was directed and superintended by Dr. Nichols, is equivalent to saying that it was thoroughly done. Its cost was $425 per patient.

Conversing on this subject with Mr. Samuel Sloan, an architect of this city, who is more familiar with hospital construc-

tion, I presume, than any other man in the country, he very kindly engaged to prepare for me a schedule of the various items of expense incurred in the establishment of a hospital for the insane, two stories in height, to receive 400 patients.

Land, 100 acres, at $200 per acre	$ 20,000
Bricks laid in wall, 12,000,000 at $12 per m	144,000
Window and door heads, 900 at $3 each	2,700
Window sills, 750 at $3 each	2,250
Stone steps and door sills	4,200
Flagging for kitchen and laundry floors	1,000
Excavation, 120,000 yards at 25 cts., including grading	30,000
Plastering, 80,000 yards at 25 cts	20,000
Roofing, 75,000 feet at 12 cts	9,000
Conductors and underground drain pipe	3,800
Lumber, 900,000 feet, averaging $30 per m	27,000
Window frames, 900 at $4	3,600
Doors and frames, 650, averaging $12	7,800
Carpenter work	36,000
Painting and glazing	14,000
Hardware, including nails and locks	3,800
Iron sash and guards	6,000
Slate stairs	3,500
Iron work	4,000
Heating, laundry, machinery and fans	28,000
Fixtures for kitchen and bakery	2,500
Gas works	14,000
Gas pipes and fixtures	7,000
Stone dressings for front door of centre building	2,000
Culvert for drainage	5,000
Farm house and stabling	12,000
Roads and walks	4,000
Exercising yards (enclosing)	5,000
Entrance lodge	3,000
Water supply	15,000
Fencing 100 acres	7,000
Plumbers' work within the building	8,000
	$455,150
Add 10 per cent. for lee way	45,515
	$500,665

If connections between the wings are to be fire-proof, add to this five per cent. $25,033, making $525,698; or an average cost, per patient, $1,314.25.

If the capacity be reduced to 300, the average cost will be increased eight per cent.—$1,419.38. If it be reduced to 250,

the average cost will be increased ten per cent. =$1,445.67. If it be raised to 500, five per cent. must be deducted, making the average cost $1,248.53. If it be raised to 600, eight per cent. must be deducted, making the average cost $1,209.11. If it be raised to 700, ten and a half per cent. must be deducted, making the average cost $1,176.25. If it be raised to 800, twelve per cent. must be deducted, making the average cost $1,156.53.

Very likely, in some of the particulars the cost is placed too low to be regarded as a fair average, while in others it may be too high, the under-estimate probably balancing the over-estimate. Most of the estimates will differ somewhat with the circumstances of the place and site, notably with the facilities for reaching the ground, the opportunities for drainage and a water supply, and materials on the place that are required in construction, such as clay, lime, sand, stone. Thus, the bricks used for the hospital now building at Warren, Penn., are made under the direction of the builder from clay and sand on the spot, and at a cost, I do not doubt, of 50 per cent. less than their price in the market. In the same way were obtained the bricks used in the Government hospital at Washington. At the Butler hospital not one new road was required, and most of the materials were water-borne from the premises of the contractor to within a few rods of the place.

It will be observed that these estimates of Mr. Sloan substantially agree with the result presented by the actual examples quoted, making due allowance for difference of prices at different periods. They show, beyond question, that a hospital embracing all the modern improvements may be substantially built, with a suitable amount of land, at a cost not exceeding $1,500 per patient, its capacity being for about 300, which are as many, I believe, as any hospital should have. And they show that it cannot fall much below this, unless the site is remarkably favored in the manner just indicated. Any figure much less must lead to the suspicion of the sacrifice of some important features of construction, or of poor materials and work, or deceptive contracts.

These estimates ought to disabuse the public of another error most mischievously prevalent at the present time, viz.,

that large hospitals cost very much less than small ones. It will be observed that 500 patients can be provided for at an average cost of only $171 less than 300, and that the whole range of difference between the cost of 250 and that of 800 is but $289. I believe the public convenience is better served by more than that amount by small hospitals, in the smaller expense of transportation, not to mention just yet other considerations no less important. Not only so, but it has been found that communities avail themselves of the benefits of a hospital to an extent that is determined by their relative nearness to it. In the present disposition to build our hospitals of immense size, these facts should be duly pondered. And where the plan is adopted of having the different pieces of the building entirely separated from one another, the economical advantage is lost, and the cost of construction equals, if not exceeds, that of smaller hospitals otherwise built. That the cost of maintenance will be greatly increased, is a fact self-evident to those who have the slightest practical knowledge of hospital service.

Dr. Earle, in his last report, contrasts the cost of the Danvers hospital, now building in his State, which will reach the sum of $3,600 per patient, with that of the Fife and Kinross asylum in Scotland, which was only $688.92. The Doctor does not say that the former should have cost no more than the latter, but for anything he does say, that might be inferred. As I write with the single purpose of correcting some misconceptions in regard to so important an object as the care of the insane, a word or two on this matter may not be out of place. Without any particular knowledge of this Scotch asylum, I can only refer in a general way to some points that may account for the cheapness of its construction. In Great Britain a much larger proportion of the patients sleep, if they sleep at all, in large associated dormitories. Especially is this so in Scotland, unless they have greatly changed of late years. In the pauper department of the asylum at Morningside, near Edinburgh, with some hundreds of patients, thirty years ago, there were only about a dozen single sleeping rooms. This, of course, would make a considerable difference in the cost of construction, but surely, Dr. Earle would not advocate such an

arrangement with us. Twenty-five per cent. of our patients are full as many as can be properly placed in associated dormitories. It must also be considered that the cost of building in Great Britain is much less than it is in this country. I have good authority for saying that it is nearly, if not quite, fifty per cent. less. With such advantages as these, the Scotch asylum cannot properly serve as a standard of cost with us.

Why some of our hospitals have been built at an expense that reaches almost the incredible, is a question of the gravest import, because the answer to it may involve the weal or woe of our insane for generations to come. We may be quite sure that if it be clearly understood that hospitals are, hereafter, to cost $3,600 per patient, the public will decline to build them, while we may be equally sure, I think, that upon the estimates given above, the public will willingly provide them as fast as they are needed. The circumstances which have led to a cost excessively large vary, no doubt, in different cases, and, therefore, can only be discussed in a general way.

Probably nothing has contributed more to bring about this lamentable result than the injudicious selection of the site. For this purpose, the common practice is for the executive, or the legislature, to appoint a Board of Commissioners, not one of whom, it may be, was ever in a hospital for the insane, except as a casual visitor, or, of course, has any matured ideas of the needs and purposes of such an institution; in short, as well fitted for the duty as if it were to plan a chemical laboratory or draft an ocean steamer. Starting with the popular notion of the most desirable quality in a site for public or private purposes, they are very apt to pitch upon one which commands a wide outlook over the surrounding country, and this implies, generally, the necessity of expensive roads, to say nothing of the waste of muscle, the danger in driving, and the great increase of cost incurred by raising everything and everybody to a higher level. In such situations cheap roads are out of the question; they must be made in the most substantial manner, so as to resist the action of storms and frosts. At the asylum building in Danvers, Mass., the roads required for its various purposes have cost over $56,000. Sometimes a large expense is incurred in shifting earth, removing boulders, and preparing

the grounds,—so great, it may be, that it should have been a fatal objection to the site. At Danvers, this amounted to nearly $50,000. Not very unfrequently a site is chosen which is destitute of a sufficient water supply. The little stream which was to furnish an abundance of the needful element is found to be dry in the summer months; or the well, thought worthy of the utmost trust, proves by assiduous pumping barely adequate to the wants of a small family. To supply this deficiency has sometimes required an outlay which would once have almost built and equipped a small hospital.

All those worthy people who derive their notions of the enormous cost of hospitals from the reports of State Boards of Public Charities and equally reliable sources of information respecting hospitals for the insane, will expect to hear much in this connection of their palatial grandeur and extravagant ornamentation. That money has been sometimes spent for unwise and needless show, I would not deny. I simply say that this is not the sole source of excessive cost. Our State hospitals, in outward show and inward finish, are mostly destitute of any pretensions to grace and beauty, except what may be signified, perhaps, in a leaky cupola, or a wooden porch over the front door. Indeed, the fault has always been the other way, and it is one of no trivial importance. Not only good taste, but good sanitary results require that the architectural aspects should be in harmony with the special purposes of a hospital for disordered minds. The moral impression first produced thereby may be the first step that costs, in one direction or the other. It is exceedingly important that the patient, as he approaches the hospital, should not have those fears and suspicions so common with the insane confirmed by the sight of architectural arrangements which remind him only of places of penal confinement. To give it a distinctive character by architectural devices that need cost but little, is to promote the very purposes of the institution, and therefore, it is money well spent. For the same reason it is well to give the interior a more genial aspect than it often presents, by features as nearly as possible like those of a domestic residence. The constant sight, day after day, hour after hour, of a monotonous range of white walls, unrelieved by a

cornice, or a bit of paint, or a visible door frame, is well calculated to strengthen depression and multiply morbid fancies. Our predecessors made the mistake of supposing that such things are of no account in the management of the disordered mind, but they acted up to their light, while we, knowing their value, make the mistake of grudging the money they would cost.

Among the most fruitful sources of excessive cost in building hospitals are, I apprehend, a lack of well-conceived, well-matured plans; specifications lacking in accuracy and exactness, and contracts inconsiderately made and imperfectly executed. In this way, outlays are made not expected at the start, contingencies arise that were not anticipated, and improvements are suggested as the work goes on, which are deemed too valuable to be lost. To prevent all this, the plans should be approved, if not devised, by persons who have a practical knowledge of the requirements of such buildings; the specifications should be carefully and skillfully drawn, and the contracts controlled by an intelligent, practical builder, unhampered by "lowest bidders," and unbiased by pecuniary interests.

This inquiry warrants the conclusion that, avoiding on the one hand, a style of construction more extravagant than is needed for any rightful purpose, and on the other, a sort of false economy which leaves a large margin for future repairs and improvements, State hospitals for the insane may be established in our country, at an average cost per patient ranging from $1,000 to $1,500.

Art. IV.—SOME PECULIARITIES IN THE MYELINE OF PERIPHERAL NERVES AFTER TREATMENT BY OSMIC ACID.

By J. C. Shaw, M. D.

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SINCE the time of Schwann, it has been known that the structure of the peripheral nerves was, first, an external sheath of connective tissue—the sheath of Schwann; second, a tube of fatty phosphorized material, the white substance of Schwann, or myeline, inside of which is placed the axis cylinder. Our present knowledge of the histology of nerves dates from 1872, when Ranvier, of Paris, made a re-investigation into their structure, using osmic acid for his observations.

The osmic acid stains the myeline of a deep black color, leaving the axis cylinder unstained, and this, in turn, can be stained with carmine.

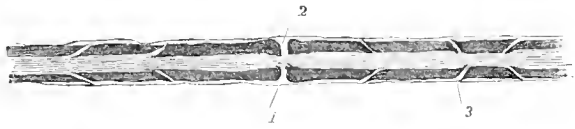
By this method, Ranvier showed that the myeline is not one continuous tube, enclosing the axis cylinder from its origin to its termination, as had been previously believed, but that at every two millimetres of the nerve there was a constriction on the myeline (see cut, Fig. 2) down to the axis cylinder. These constrictions are now known as the constrictions of Ranvier.

In nerves taken directly from living animals and examined in serum and without staining material, these constrictions of Ranvier are seen very distinctly.

Prof. J. C. Dalton, in the edition of his physiology in 1866, gives a very accurate representation of these constrictions, but

distinctly states that they are accidental breaks in the myeline, and it was reserved for Ranvier, in 1872, to announce their true character.

In December, 1876, whilst working in the Neurological Department of the School of Histology in this city, (New York) under the direction of Prof. E. C. Seguin, I observed that fresh nerves of the dog treated by osmic acid, showed some peculiar appearances in the myeline (see cut, Fig. 3).



EXPLANATION OF CUT:

Sciatic nerve of frog treated in situ.

1. Sheath of Schwann. 2. Constriction of Ranvier. 3. Fissures in the myeline as observed by me.

I have since examined the nerves of the frog, mouse and man (nerves taken immediately from an amputated lower extremity, given me by Dr. Seguin), and find that these peculiar markings are present in the myeline of each. The markings are seen best in the nerves of the frog and dog, and are to be seen very distinctly in man, less so in the mouse.

The method adopted is as follows: The nerve is taken with the greatest possible rapidity from the animal and placed in a watch glass containing 1 per cent. solution osmic acid, allowed to remain 20 minutes, washed well in water and placed in glycerine. Small portions of the nerve are then teased and mounted in glycerine.

Some of my observations have been made on nerves treated in situ in the living animal; this is doubtless painful. As the acid is very irritating, and having a great repugnance to inflicting pain on animals, I chloroformed my frogs, under the influence of which they come very rapidly. Just before they become motionless, they make a few of what appear to be efforts at vomiting.

Injection of chloral would doubtless do as well.

In nerves treated as above indicated, the markings in the myeline are to be seen with great distinctness. My reason for treating the nerve in situ was this: I found that the nerves

taken from the animal some time after death did not show the fissures, and that the nearer living the nerve is when it is submitted to the action of the acid the better the fissures show.

The fissures, however, can be seen very distinctly on nerves taken *very rapidly* from the animal and placed in the acid, and it is not necessary to treat them in situ. Care should be taken not to injure the nerves whilst removing them, so as to get as few breaks in the myeline as possible.

The regularity with which these fissures in the myeline occur is so striking that one is led to believe that he has discovered something new in the histology of nerves. Are these artificial productions, or are they normal? The following facts would incline one to believe them to be normal. The fissures are on both sides of the axis cylinder, at precisely opposite points, and the edges of the myeline in the fissures are sharply defined, as if cut across and not torn.

But on the other hand, it is to be observed that these fissures do not occur at regular intervals all through a nerve. In some parts the fissures are very near each other, and at other places much further apart. Then the fissures do not all run in the same direction (see plate). These latter reasons lead me to believe that they are artificial.

If these are artificial, how are they produced?

Ordinarily in examining nerves, it is observed that the myeline presents a serpentine appearance, which is most apparent along its outer edge. In nerves treated as above this serpentine appearance is wanting.

I believe that the osmic acid affects the myeline so rapidly that it is thoroughly saturated and stained before coagulation takes place, and that the myeline is to a certain extent rendered firm by the astringent properties of the acid, and as coagulation progresses the myeline contracts, just as all albuminoid substances contract under the action of an astringent, and thus these fissures are formed in the myeline. I speak of these as fissures, for I think it indicates better the smooth regular edges of the openings. And there are to be seen besides what we have been in the habit of speaking of as breaks in the myeline, and which are produced by force in the teasing of the preparation, while these fissures are not so produced.

ART. V.—PRESENT AND PROSPECTIVE MANAGEMENT OF THE INSANE.

BY RICHARD S. DEWEY, M. D.,

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THERE is a pronounced tendency to discussion of methods and results in the management of the insane, which show that the present system does not meet in all respects, the expectations that are entertained of it in various quarters, public and private, lay and professional.

The fourth and last biennal report of the State Board of Public Charities of Illinois contains the following language "It is our opinion, however, that the present system of caring for the insane is far from realizing the highest ideal possible of attainment." In the published "Proceedings of the Conference of Charities," held in connection with the American Social Science Association at Saratoga, in September, 1876, Dr. Nathan Allen, in a very suggestive paper, remarks: "No thoughtful person can often visit these hospitals, [for the insane] and make much observation without seeing the importance of having more careful and discriminating supervision over them, and a more thorough investigation into the whole subject of insanity. * * * until some such provision is made, prejudice and mistrust will exist more or less in reference to these institutions, causing injury not only to the officers and their management, but indirectly, far greater injury to the public."

In the annual report of the Massachusetts General Hospital for 1876, the following comment is made in connection with the McLean Asylum, one of the most elaborately equipped and carefully conducted institutions for the insane in the United States." "For the treatment of insane persons, we

could wish some practice more encouraging in its remedial effects might be devised. As now administered, asylums for such unfortunates afford little more than a place where they may be isolated from society, kindly treated, and a watchful oversight maintained to prevent them from committing injury on themselves or their attendants. This may be all their malady admits of, but we hope not, preferring to look forward to a time when asylums shall be provided in which occupation can be introduced, diversified to meet the needs of the various cases, and through such means entice, if possible, the inmates into new fields of thought, and increase the percentage of curable cases."

Numerous quotations like the preceding ones might be adduced, but their import would be the same; while in a different and less temperate strain, innumerable contributions to the press, of philanthropists, novelists, ex-inmates of asylums, and even the *brochure* of one "amateur lunatic," re-echo the same opinions. At the same time it is true that these complaints have mostly an origin remote from any practical contact with the insane, and that those directly concerned in the care of them, for the most part, express themselves satisfied with present methods, claiming only that they need amplification. On the one hand, then, we have complaints more or less founded upon theory or sentiment, but entitled to candid consideration as arising from a state of affairs confessedly unhappy, whether admitting of remedy or not. And on the other, we have those who may be supposed to know best, as being most familiar with the practical side of the question, declaring themselves in agreement with the main features of asylums as at present administered. But these latter, occupying continually one point of view, and being identified with the established order of things, may be impervious to newer, even though better ideas; while some allowance must be made, in the former instance, for the difference between actual, concrete insanity, and abstract mental disease as viewed by the general public; by reformers and all *a priori* critics, however well meaning.

It is unlikely that the right is all on either the one side or the other; and the present paper is an attempt to examine

the question, and avoid both undue sentiment or theory and prejudice or ultra-conservatism as well, being based upon much practical knowledge of the insane and earnest thought for their welfare.

The principal subjects which call for discussion may be stated as follows:

I. Means of employment and recreation for the insane.

II. Degree to which confinement and restraint are necessary.

III. The effect of asylum routine upon the individual patient.

IV. Treatment received by patients from attendants.

V. Relation of the institution for the insane (1) To the general public (2) To its own officers and employees. (3) To advancement of scientific knowledge.

Before entering upon these points it is well to remark, that the form of institution for the insane, under discussion here, is that which we find has almost universally grown up in our midst. An establishment accommodating several hundred, and merging into one—the "hospital" and "asylum." The consideration of the question of numbers proper to be received into one institution, and of the separation of chronic and acute, is aside from the present purpose, and could only adequately be treated in a separate article.

I. Taking up our first subject as proposed above; viz., "Means of employment and recreation for the insane;" it is to be remarked at the outset that suitable occupation is not only desirable, but necessary to the well-being of the insane as much as of the sane; except where there is severe bodily disease or nervous prostration. In acute cases of insanity it can seldom take the form of labor, but the large mass of the insane, wherever found, would be infinitely healthier and happier, for systematic employment, could this by any means be secured for them.

If now it be asked, what the resources of the different institutions are in the way of employment, the reply is that they are limited, and, embrace in general, only the ordinary domestic occupations and some form of agriculture, with very rarely one or two of the more common trades, to which might be added, general "choring" about the establishment.

This does not afford great latitude of choice, and, as all the various pursuits in life are represented in the asylum in about the same proportion as in the outside world, we can hardly expect a very large number to find these avocations congenial or familiar.

Now in most of our asylums it is an acknowledged fact that a "sad idleness" largely prevails. Very few of the inmates do any systematic work, aside from the care of room or person, and trifling assistance about the ward or house.

It is reasonable to suppose that with greater efforts and increased facilities, numbers of the insane might be enlisted in beneficial work who are now worse than useless. If capable, strong and kindly persons could be more generally introduced among them for the purpose of discovering some employment adapted to the capacities of each; many whom only a special effort can reach, might be most happily influenced in the direction of health and productiveness. Work-rooms should be made pleasant, cheerful and attractive, and special rooms provided for teaching the less ready or willing patients. Some would have to engage in what was new to them, many would return to labor at which they had already served an apprenticeship. Some would only engage in what was simple, others would require light or fancy labor, many would only become capable of anything useful after long and patient assistance from others, a frequent change would be necessary for a certain number, but as long as any degree of self-respect or self-control remained, there would be enough to encourage the hope of winning back the dormant energies and temporarily paralyzed faculties, by providing some pursuit for which each one was fitted.

It might seem at first that any great diversification of employments would entail increased expenditure, but a candid consideration can hardly warrant this conclusion. On the other hand, it may be asked, why would not a wise and judicious management enable the State, in time, to establish in connection with its asylums an industry that shall be self-sustaining? Let the different departments of work have a goodly intermixture of sane men and women employed precisely as elsewhere. Their services would not be worth less to

the State than to other employers, if they were properly superintended. Let them, in addition, receive careful instruction and training in adapting themselves to the encouragement and guidance of the insane, who work with them: a good mutual understanding would naturally arise, as may be seen on a small scale, even in the present industries of any institution for the insane. Many of the inmates would, of course, be useful only in the cruder processes, but a goodly percentage of others would be found skillful and reliable in a high degree. It is impossible to predict how far the productiveness of the asylum might be increased; only judicious experiment can determine this, but it is safe to anticipate the gradual building up of industries which shall engage, more or less fully, all but a comparatively small fraction.

The pursuits found feasible would vary with localities, aptitudes and abilities, both of the insane and of those associated in their care; but simple forms of manufacture, like that of plain clothing, boots and shoes, and plain furniture, blacksmithing, mason work, harness making, would be found practicable, while there is a large field in which machines would play a useful part; such are knitting machines, looms for coarse weaving, machines for manufacture of tin-ware. Again, the making of mats, brooms, and similar articles would furnish many with employment. Furthermore, useful labor in repairing could be associated with several of the branches mentioned; these, it will be perceived, are all forms of work which cost the asylum large sums, and whatever is produced in this way would be a double gain, and even though a positive saving were not effected (which is very improbable), the labor would be in itself a valuable consideration for the welfare and recovery of patients.

It is to be feared that long habits of idleness have been engendered among the insane, which a greater diversification of employments is needed to break up. Indeed, the asylum is itself indirectly the cause of an immense deal of indolence and mischief among the insane, by congregating and confining hundreds of weak, and often depraved beings, whose reciprocal influence must be bad, and who prey in turn one upon another, having no useful direction in which to expend their energies.

because all that is open to them is a choice between domestic occupations or agricultural pursuits, the latter being also at a stand-still during winter. This is a necessary evil of the asylum to be combated by every available means, and it seems probable that increased facilities for employment, with systematic and wisely directed efforts in the direction proposed, may reclaim a very considerable part of those now turbulent and mischievous, and lead them gradually forward to self-respect, contentment and usefulness.

Coming now to recreation for the insane, we find this a subject which has received much attention in almost every institution. It requires no little study to enable any one to determine what is best adapted for the diversion of the insane; so large is the element of uncertainty, so limited the field from which choice can be made, and so changed are natural and healthy preferences and feelings, but there is one universally desirable method of procedure. It is that of ascertaining carefully the state of mind to be dealt with, before attempting to act upon it. Every case of madness offers a riddle for solution, and though there are sphinxes that baffle the keenest wit, faithful study will generally obtain a clue to the mystery. It is needed, however, in every case, and in view of this fact there is danger in our large institutions that amusements will assume too wholesale a character, and that the great essential of attracting *real* attention and interest will be overlooked. The idea of compulsion, upon which the asylum is unhappily but necessarily founded, should be, as far as possible, excluded from recreations, because it is fatal to all true enjoyment. The horse may be led to the water, but we cannot make him drink, and similarly, society or the State may establish what it believes to be fountains of health and happiness for the insane, and bring them forcibly to partake, but they show themselves neither more nor less perverse than the rest of mankind in refusing what others judge to be for their happiness, but what fails to meet their own wishes.

The greatest enjoyment of the insane who retain even a glimmering of intellect, as may be witnessed in any asylum, is in that which gives them some little freedom, restores to them for the time the responsibility of their own actions, and re-

moves the sense of restraint with which they are more or less constantly hampered. It is necessary, of course, to inquire what the tastes and previous modes of life have been. As a rule, except with certain melancholy patients, something which recalls former pleasing associations and brings the atmosphere of home, reaches the heart and thus the mind. The very large majority of all insane in public institutions are from the same general station in life. The recreations of the rural population, the laboring and artizan classes, are generally simple; husking and paring and quilting "bees," singing-school, pic-nics, excursions, sleigh-rides, dancing and social parties, concerts lectures, athletic sports—most of these, with slight precautions or limitations, would be applicable to the insane, and are increasingly in vogue, but still many institutions offer little, but one, or possibly two formal gatherings during the week and chapel service on Sunday, to which all are marched with military precision.

The ordinary amusements to be found in the town near the asylum are also attractive, as well as safe for a large number of patients, and such as are able to do so would generally enjoy attending them precisely like other people, even if they could have equally good entertainment in the asylum itself.

II. The second division made of our subject is: "The degree to which confinement and restraint are necessary."

The question how far it is proper and necessary to confine the insane, affords material for much discussion. Most of the American hospitals and asylums for the insane consist of one or more buildings of colossal dimensions, with a central or "administrative" building, and wings extending to right and left, for each sex. They are built in sections, either with continuous walls or partially detached. Each wing is divided into wards from 6 to 15 in number, accommodating from 12 to 40 patients, and these wards are all guarded by locked doors and grated windows. The patient can only enter or leave the the ward with the aid of the attendant who carries the key. This is the manner in which all, with an occasional exception, are confined, and it needs no argument to prove that such confinement must be irksome; hence, it is a question worth discussion, whether it can be made less oppressive with safety

and benefit. Patients who retain any degree of mind and sensibility, must feel keenly the disgrace of being told practically every time they look out through an iron window guard, that they are unfit to enjoy the slightest and commonest liberty, and while this is necessary for a certain class, it is questionable whether the whole establishment need be regulated by the requirements of the dangerous, destructive and suicidal, who are only a fraction of the whole. It is often a source of wonder that so many patients persist in regarding themselves as prisoners, and declare that they have committed no crime deserving of incarceration. We say "are they not granted numerous privileges and surrounded with comforts? What could be less like a prison than the asylum?" But however much it may surprise us, this rebellion against rigorous confinement remains, and, perhaps, one who has never been subjected to it, can but poorly judge how deeply in our nature it has its origin.

One of the chief remedies proposed for this severe curtailment of personal liberty is the "cottage system," or the plan of providing for the insane in separate buildings constructed like an ordinary dwelling, and conducted, so far as possible, in the style of domestic life. Other changes suggested, are the removal of guards from a portion of the windows and the opening of some of the wards to free egress and ingress for the patients.

The Eighth Annual Report of the State Board of Health, of Massachusetts, contains much interesting matter as to the increased liberties allowed patients in England and Scotland. Dr. C. F. Folsom, the secretary of the Board, has elicited much information from high authorities and combines his facts and opinions in a suggestive paper entitled "Disease of the Mind." He reviews the advances made in the care and treatment of the insane in different countries, and points out a time some twenty years ago, when he considers that America led all other nations in the excellence of its institutions for the insane. To-day, however, he shows that in Great Britain and Ireland there prevails, in most asylums, an absence of restraint and a degree of personal freedom greater than is found with us. The window-guards and locked doors are removed from a

greater or less portions of many asylums. At the Fife and Kinross Asylum, all the wards occupied by male patients, and all but two of those occupied by females, are free to enter or leave at the will of the patient. The Rainhill Asylum near Prescott, has no iron guards or window sashes in any part. On the occasion of a visit to the West Riding Asylum, Dr. Folsom found all the newer parts of the building had ordinary windows without guards. In Scotland the newly built asylums are practically without window-guards, and at the Royal Edinburgh Asylum, three of the older wards accommodating 140 patients on the first, second, and third floors have been fitted with plate glass windows not guarded in any way.

This movement in favor of more free and domestic arrangments for the insane, originated perhaps chiefly with the study of the celebrated "colony" for the insane, in Gheel, Belgium, whither for centuries, those afflicted with mental disease have been accustomed to resort, and where, for the most part, they have been quartered with the peasantry and lived as inmates of the household, a degree of freedom and contentment prevailing among them elsewhere unknown. This precise plan has been imitated in Scotland, with much less satisfactory results, but the essential idea of nearer approach to home life and less restraint has taken firm hold of both popular and professional minds, and various efforts to realize it are attracting public attention,

The principal of these is perhaps the "cottage" plan of construction. It is advocated as less expensive in its administration, and more philanthropic in its results. Buildings much cheaper in proportion to the number accommodated can without doubt be constructed on the plan of the ordinary dwelling-house, and as the cost of the average insane asylum, as now constructed, amounts to $1,000 or $1,500 for each person accommodated, while in many instances the figure reaches $2,000 $3,000, and even $4,000, the saving of money will be a very important item, if the same result can be accomplished for $500 or $800 *per capita*.

The next question, of the advantages to accrue to the patient from this plan, and of the extent to which it is feasible, is a very complex one, and in the absence of any very definite

or conclusive results, it is only possible to advance opinions founded upon experience among the insane and upon some suggestive but incomplete experiments already made in that line.

It is notorious that the vast majority of the insane in our present asylums are discontented, perpetually urging their release, and so ill at ease in their surroundings, that usefulness is impaired and mental improvement hindered to a lamentable degree. The question next follows: To what extent is the cottage system a remedy for this state of affairs?

In answering this, it must be remembered,—and this sometimes seems in danger of being overlooked in these discussions,—that the fundamental idea of the asylum for the largest majority of its inmates is confinement or separation from the outside world. If the insane could follow the ordinary course of every day life, and mingle in the affairs of the world at large, there need be no asylums, for there is no merit, but rather detriment, in the mere fact of associating the insane together. The principal reason for congregating them in large institutions is the necessity of economy and efficiency in their care, and the compensating advantage is that the necessary appliances and skill can be thus commanded, and generally only thus; but so far as their reciprocal influence is concerned, the most that can be said in any case is, that it is not harmful; hence the greater the desirability of the intermixture among the insane of sound and healthy minds to as great an extent as possible.

Next, as to the origin of this discontent and suffering of the insane. Does it result from their being shut up in the asylum, or is it a necessary concomitant of disease? Each question may be answered in the affirmative for a different class. There are those who could never be made contented, or in any degree comfortable by any means which human ingenuity could devise, whose disease must pass away before any repose could come to their restless spirits, but another and much larger class is that composed of active and more or less intelligent and well disposed patients who crave liberty in a natural and healthy way and would have sufficient self-control not to abuse privileges extended to them.

The late Dr. Batty Tuke was the first to apply to the insane the maxim "the more you trust the more you may," and Dr. Tuke's brilliant success in making his asylum one of the most free in the world, with no bad results, justifies this policy. By the side of his watchword should be placed that of Dr. Browne of the West Riding Asylum,—"Treat the insane as men and women, and they will behave as such," for his remark is equally exemplified by the order, quiet, freedom and manifold industries of that famous institution. Perhaps, however, the first utterance of this kind came from an American source.

Dr. Isaac Ray wrote in 1841; "In a word, we endeavor to treat our patients as every honorable, well-bred man treats another in the common intercourse of society;" and again: "Generally speaking, the more they, [the insane] are suffered to act like other men, then more they will strive to become like them."

A somewhat different spirit has prevailed among those less familiar with the insane, and even those in contact with them do not perhaps easily divest themselves of all prejudice. It is safe to say that the most heated diatribes against the supposed wrongs perpetrated in depriving the insane of liberty and "inalienable rights" have come from those having the least practical knowledge of what the insane actually are, or how they may be dealt with. It is only necessary to observe the senseless apprehension with which personal contact with any and all insane persons is generally regarded, to see how little they are understood; and one result of this general ignorance in regard to them, is, that those undertaking their care are exposed to a constant cross-fire of opposite caprices, and liable to incur opprobrium either from a moderate and conservative course, or from any excess in the direction of freedom on the one hand, or restraint on the other; —so attractive are theories, so easy are denunciations, and so simple is destructive criticism to parties not charged with responsibility; and since a single error in judgment, or even a simple misfortune, is so liable to bring down the most bitter reproaches and undeserved censure, medical officers of asylums find themselves obliged to practice the most extreme caution, and will only gradually, and in harmony with improved gen-

eral knowledge, reach the golden mean of largest personal liberty for the insane, combined with greatest welfare and safety. It is my belief, however, that the insane may be more confided in than at present is the case with us, and that the horizon may be gradually widened for them in a way to increase both their happiness and the facility of caring for them.

In considering the "cottage" plan, if one may judge from the absence, in a tolerably large proportion of the insane, of any positive malicious or dangerous tendencies; from the beneficial effects so often observed even now of judicious increase of freedom; from the more domestic and home-like atmosphere which can be given the cottage, and from the happy effects of employment, which I believe should in most instances be made an essential of cottage life; one may reasonably expect good results from its introduction, though unable to predict how far they may reach.

The "cottage" plan as had in mind here, is a grouping of dwellings for the insane quite separated from each other, upon a tolerably extensive tract of land, and around central buildings intended for the general accommodation and for those unable to enjoy the more removed and independent life. The advantages to be derived from this plan are, a more domestic and less monotonous and restricted style of living, and less contact of those unfitted for mingling together; it is probable, too, and indeed is shown where it has been tried, that the removal of the more visible and obtrusive signs of imprisonment, will render the weight of confinement much lighter for many, and will thus greatly moderate the aversion with which they regard the asylum. Much of that destructiveness, too, which arises from the annoyance and vexation or sheer idleness of close confinement will be done away with.

One of the difficulties of the present mode of management is that it renders it harder to discriminate between the malice and mischief which are inherent necessities, and that which might be cured by a more complete change in mode of life. It will not be denied that some of the insane who seem to be under the dominion of their evil and destructive tendencies, may derive them to some extent from external circumstances; and the very element of change here introduced, is in itself im-

portant as a well known most powerful factor in the treatment of all maladies, mental even more than physical. Changes from one institution to another, or from one ward to another of the same institution are frequently found most salutary, although the two are apparently quite similar. Still more, then, with a number of differently surrounded, independent buildings, will there be opportunity for the working of change as a curative agency.

It is objected to some of the above considerations that many of the persons who would be fitted for the cottage are the very ones who complain least of the asylum as now arranged; that extra liberties and privileges may be conferred on those fitted for them as well in one building as another; that there would be greater difficulty in preventing escapes and larger opportunities for mischief, and that great additional risks of fire and suicide would be incurred.

These points must indeed modify and limit selection of patients, and render necessary great caution on the part of officers, but they cannot be held to outweigh the arguments on the other side.

A much more valid objection is that relating to proper general supervision. It must be evident that these detached buildings, covering a much greater territory, will be reached with greater difficulty, and will have only at intervals the presence of any person more responsible than the attendant. They must, therefore, tax severely the vigilance of officers in control, and will necessitate the employment of highly trustworthy attendants, and even when the best possible have been secured, there can be no certainty that abuses will not arise, and where they do, they will be much more difficult to discover and cure. The approach of physicians, supervisors and night or day watchmen to the several buildings can always be seen and prepared for; larger opportunities for lawlessness and neglect thus being afforded.

A further weighty question is, how far such a system may apply to females. There are strong objections both to leaving them day and night with no man within call, and to introducing males, even man and wife, into their household, while the additional opportunities afforded to such patients by the

"cottage," for going about unobserved and unguarded, might produce most deplorable results. Attendants also would be able to leave the house readily by night as well as by day, and persons lacking in character might in various ways bring lasting reproach upon the good name of the institution, and those responsible for it.

It is here that the greatest hesitation must be felt with regard to success, and few medical men would be willing to become responsible for widely separated communities of insane persons, so difficult to reach or control; except as they might be very gradually formed, and careful tests applied at each new step.

In reply to some of the foregoing objections, it may be urged that attendants and patients would be scarcely more out of the way in a detached cottage than they frequently are in the wards of the large buildings; the classes of patients fitted for the cottage, too, would be of the sort least liable to abuse, having intelligence enough to protect themselves by promptly reporting their grievances. Further, if both patients and attendants were kept constantly well employed as an essential of the cottage life, giving them the wholesome fatigue of honest labor; if men and their wives of reliable character were secured as attendants; if vigilant night and day supervision were exercised, and a system of lighting grounds and buildings with gas were maintained, these difficulties could be deprived of part of their formidable character.

Again, before it can be seen how such a system would work, it must be tested by experiment; all present ideas are drawn from a different system. The class of attendants may be improved, while patients entering an institution upon a more free plan might adapt themselves more readily than is anticipated, to an order of things approaching nearer their previous life.

Taking the question now in an economical point of view, an advantage in favor of the cottage is apparent, as a saving in cost of construction of from $700 to $1,000 for each person provided for is easily demonstrable; the average congregate building costing $1,500, and the cottages $500 to $800. It is of moment to inquire, however, whether this cottage is to differ in any respect from a common dwelling, and if so, in what

manner and extent? If it is practicable for considerable numbers of the patients to dwell in such houses as are proposed, it will be necessary for the welfare of a certain portion that extra appliances for security and strength be added. Brick is decidedly the preferable building material, and partitions should be of masonry as well in order to withstand fire and occasional assaults of violence, and also to shut in noise and secure quiet through the building. Certain classes of cases, too, like those of suicidal and homicidal mania and "pyromania," though otherwise fitter for a more domestic life, would need the guarded windows and "strong" rooms. Again, many of the houses would require to be built with a large proportion of single rooms, and the construction would be more expensive than an ordinary house, since many could not profitably sleep in a dormitory with others, while the noise or malice or vice of some, would necessitate single rooms for them. On the other hand, a limited use could perhaps be found for a few light wooden structures of the "pavilion" character, to be removed when contaminated or rendered unwholesome in any degree.

It can, however, scarcely be disputed that the incurably mischievous and malicious patients will be fully as comfortable and safe, and in a more satisfactory relation to those responsible for them in a securely constructed building under the immediate eye of the physician.

The provisioning and cooking will also be points of serious consequence. If meals are distributed from one central kitchen, the transportation will be a considerable expense, involving elaborate apparatus and extra help to operate it, while food will be likely to arrive cold and unpalatable, whereas if each of these buildings is to have a separate kitchen and store room, great confusion, waste and extravagance, will be risked, as well as badly prepared and improper articles of diet. Perhaps, however, these objections can be overcome by establishing a central dining hall, where the majority can meet for their meals. This is a method which works satisfactorily in many institutions, especially in the asylums of England.

The heating and lighting, the water supply, the sewerage, the bathing, the laundry arrangements, all offer difficulties.

If they are to be provided for from central sources, the expense in each instance will not be less, but greater, than that of supplying the same to one compact building; whereas if we are to depend upon ordinary arrangements such as exist in any family, grave sanitary, social and economical points, resulting from the complicating fact of insanity, at once present themselves, which the limits of this article forbid the discussion of in detail.

Another additional source of expense will be the necessity of securing a very highly trustworthy and efficient class of attendants for these detached buildings, whose reliability and skill will be found to have their definite market value.

Finally, in regard to economy, it is to be remembered, no matter how much the first cost may be reduced, that if the annual running expenses of an institution on any one plan are even very slightly in excess of those of another style, the cost will ultimately be far greater, accumulating as it will year after year.

The conclusions arrived at with regard to the cottage plan, in view of the foregoing statements, (which it has been sought to make with impartiality), are, that there is a certain goodly proportion of the insane to whom it is applicable; how large or small only a fair trial will show; that it is more practicable for males than females; that expenses of construction can be much reduced in this way, but current expenses will probably remain about the same; that for the extreme forms of insanity this plan is inadmissible. The violent require a stronger building, and, as more liable to ill-treatment or accident should be under the immediate observation of a medical officer. Recent cases need careful attendance in a well-equipped hospital. The suicidal or homicidal can only to a limited extent share the domestic life, and those in feeble health require care in the hospital asylum. Finally, the noise and violence of refractory cases should be as much circumscribed as possible, and for this reason they will be better associated in one substantial building than scattered over a wide area.

This is perhaps a proper place for a few words with regard to the "administrative" or central building. It would appear essential that there should always be a central structure, for the

use of cases requiring for any reason more special care, and for the probation and study of newly arrived patients to determine their proper place in the community of the insane. This is what obtains at Gheel, and is highly commended for its good working there.

The ordinary hospital or asylum has nearly always a main building, the most extensive portion of the structure, for the accommodation of the public, for the residence of officers, for store rooms and offices. This has frequently been constructed with an architectural display, and unnecessary elegance of internal arrangement which have called out censure and created a prejudice against it as a feature of the building, more or less superfluous and purely ornamental; but however much it may have had unessential and merely showy additions, it remains in itself indispensable in the proper character of a convenient place of reception for inmates first arriving, for offices for transaction of business, for distribution of stores, and no more suitable place can be found, for the residence of the medical officer. It should certainly be built and arranged with all regard to economy, and precisely adapted to its purposes, with no unsuitable display; but it is the only place where the person in control will be able to command that facility of prompt access to cases requiring special or immediate attention, that knowledge of what is going on in the wards, and among the scores of employees that readiness for all emergencies of fire or accident, which alone will admit of efficient discharge of duty.

With regard to the further occupancy of the central structure, many points requiring careful consideration present themselves. Here is the place for the acute cases; so often refractory and noisy, as well as for the feeble ones, or those suffering from bodily disease. Now, if this main building is greatly reduced in size by the removal of most of the intermediate patients, the incompatibility of the two will become painfully apparent, and detriment will result to both; yet a further subdivision of buildings in their interest would be extravagant, if not impracticable. Furthermore, this will be the spot for the general gatherings of patients for amusements and chapel exercises, and will be apt to be regarded by many patients and

their friends, from its general activity, life and bustle, as the most attractive and desirable location; while smaller and more distant buildings will to many have an aspect of loneliness, isolation and, perhaps, less relative importance. If this is partially obviated by having separate buildings for chapel and amusements, a certain percentage will lose the advantage of these in inclement weather, or when in poor health. These are points which argue in favor of one large building, and militate against the breaking up of the asylum into smaller parts.

In conclusion, the opinion may be hazarded that the mere separation or scattering of buildings is not so essential as pervading them, wherever situated and whatever their construction, with careful and wise supervision and a spirit of wholesome cheerfulness, while giving a more home-like character to the habitation of the insane, by dividing it up into parts as little formal and forbidding as possible in their furniture, fixtures and general plan, as well as granting every freedom compatible with necessary order, safety and industry.

The second division under the present head is the "degree of restraint necessary for the insane."

The question of restraint for the insane has led to an animated discussion between the alienists of this country and of England, in which the latter have sought to establish a position antagonistic to any and all forms of apparatus for restraining patients. American medical men in charge of the insane, on the contrary, as well as most of those controlling continental asylums, maintain that mechanical restraint is more or less required for the protection and benefit of the patients themselves. The English substitute for any and all methods of restraint is the coercion of the refractory patient by the hands of as many attendants as may be necessary for the purpose, continuing their activity as long as occasion requires, and in case of destructive tendencies, of making clothing and all other articles as strong and indestructible as possible. The writer has no prejudices with regard to either system, having employed both with advantage, and found patients quite unmanageable by one, yielding well to the other; but while not prepared to deny that restraining apparatus has ever been used to excess in the hos-

pitals of this country, one may be far from subscribing to the fanciful theory that two to six attendants are in all instances preferable as a means of restraint to some of the better mechanical contrivances. The latter will certainly often answer the desired purpose with less harm and irritation to the patient, and be more reliable in special instances of violence, or fury, or suicidal mania. Without most careful supervision and judicious control, any form of restraint is liable to abuse, and with this secured, that restraint is to be preferred which the physician can most definitely control and graduate precisely to every case.

The most unhappy feature of all restraint is the application of physical force which it necessitates, and the meeting of violence with violent and compulsory measures. This is degrading to attendants, as its use leads them little by little to lose sight of the "more excellent way" of persuasion and kindness within their reach. And whatever lowers the character of attendants reacts, of course, disastrously upon the patient; at the same time the direct effect of restraint upon the patient is degrading, as it arouses the most malignant passions, and its need frequently cannot be in any degree appreciated, while its use is often followed by humiliation and loss of self-respect.

The chief advantage of discarding mechanical apparatus of every kind, lies in the necessity it places upon the attendant of finding, just so far as may be possible, other means than those of force for controlling those under his care. If his alternative is between soothing the patient, keeping him in good humor and agreeably occupied, or holding him through a paroxysm of violence, he will be pretty sure to choose the former.

The direction in which improvement may be made in the use of mechanical restraint, is in placing it more completely in the hands of the physician. Patients are indeed much in the power of attendants, under even the most excellent management, but the employment of the means of restraint of every kind, ought to be surrounded by such safeguards as to render their use always judicious, and their abuse nearly or quite impossible. In all well-regulated asylums indeed, restraint and seclusion are only permitted upon the order or instruction of the physician,

but without the greatest watchfulness there is a constant tendency to exceed instructions. Emergencies arise, and are allowed to become precedents instead of remaining exceptions. To wait for permission a dozen times over for the same thing seems useless to attendants, and where they have once received permission in a given case to use a given form of restraint, when a case apparently the same comes up, *having the necessary appliances at hand*, they are too apt to proceed again in the same manner upon their own responsibility. Thus the system gradually degenerates, and becomes at last quite intolerable, if there are unprincipled attendants, or officers in the slightest degree lax. Furthermore, there is naturally a strong tendency on the part of the attendant to choose the shortest way out of the annoyance caused by a troublesome patient, instead of taking pains to pacify and allay irritability, and divert the thoughts into some other and better channel.

There is one means of remedying these defects which might be generally adopted with advantage. It is the placing of all the apparatus for restraint of every kind in the care of one responsible person, whose duty it should be to keep an exact record of its use, and only supply any article upon an order from the physician indicating the time, manner and continuance of the given restraint, after which the apparatus used is to be promptly returned. It would be a further step in advance to "post up," in a book provided for the purpose, full and detailed accounts of all restraint used, and scrutinize carefully the amount of restraint as a whole, and the particular instances. Attendants should be made to feel that it is their especial duty, as far as in them lies, to avoid all forms of restraint, and commended and promoted, other things being equal, in accordance with their success in doing so. When this is made for their interest, they will find it easier than they would otherwise imagine, by taking especial pains, to check with kindly and judicious management, frequent instances of violence and contention, and in the end this would be found to be the method costing themselves the least trouble and vexation. The physician often observes that patients who with one attendant have been found almost uniformly to need some form of restraint, with another will prove quite tractable, quiet and

good-humored. Aside from natural incompatibilities, this is often the simple result of the tact and gentleness of one, as compared with the ill-temper or domineering spirit of the other. Many patients constantly grow worse under restraint, their irritability and rage increasing with its continuance, and yet rendering it still more necessary, whereas in the first instance, a period of careful and unwearied kindly management would perhaps have determined a different and happier result. Notwithstanding the fact, however, that the vigilance of the physician will occasionally discover such cases as the above, there are nevertheless in all asylums many irreclaimable patients, whom no method of management yet devised can render tolerable in condition, except by more or less constant employment of restraint.

In this connection it is of interest to recall what has been accomplished in Great Britain in the direction of removing restraints. By far the largest majority of the asylums of England and Scotland employ no mechanical restraint; a reform having been begun about forty years ago by Dr. Hill, at the Lincoln asylum, and carried out with a success which gave him world-wide reputation, by Connolly, at Hanwell, from the time when he assumed charge of that asylum in '39, and to-day asylums there which employ restraint are the exception, both public and private, whether accommodating 1,400 patients, as at West Riding, or only 50 to 100, as at many private asylums. It is probable that the abolition of restraint has been carried to excess in Great Britain, as a reaction against previous barbarities in the asylums of that country, and in accordance with an exaggerated public sentiment against all possible employment of mechanical restraint aroused by novelists and sensational writers. But there is universal testimony to the fact that great improvement has been effected in the behavior and condition of the patients. And the error is, on the whole, upon the side of humanity and safety, even though in certain cases patients may have lacked for a needed salutary influence.

III. The third division of our subject is, "The effect of asylum routine upon the individual patient."

The matter of personal attention to each individual insane

man or woman, is one which cannot be too strongly brought into prominence. The public at large, and to some extent those in the care of the insane, are prone to regard the malady as a thing apart from the patient. The time has indeed gone by when demoniacal possession is accepted as an explanation of madness, but mental disease is still far from being generally understood as a result of the perversion of natural and healthy functions, which it really is. Bodily disease was once similarly regarded as an enemy for the physician to exorcise or expel from the body, almost without reference to any peculiarities of the patient himself, and insanity is still too much considered in the light of an evil external influence which has seized upon the patient, and must be driven away. Griesinger, than whom there is no weightier authority upon mental disease, as quoted by Dr. Folsom says: "Nowhere is there greater need of strictly individual treatment, than with the insane; nowhere must we more constantly bear in mind the fact that not a disease but a diseased individual, not insanity, but one who has become insane, is the object of our treatment."

In the insane man or woman with whom we have to deal, however suppressed or distorted for the time all healthy thought and feeling may be, these or their elements still remain and constitute the basis upon which all efforts at restoration must be made, and as diverse as have been the lives, the tastes, the character of these men and women, so varied should be their treatment mental, moral and physical.

The foregoing propositions may be thought trite enough. Let us now inquire how near the institution for the insane, as at present constituted meets the requirements suggested. The asylum brings together, upon a common basis of surrender of personal liberty and control by external authority, men and women from every station, rural life and town life, high life and low, trades and handicrafts, mercantile and professional pursuits are represented in about the proportions found in the outside world. Within the asylum all are conducted through substantially the same daily round; the same hours of rising and retiring, the same food with scarcely an exception, the same forms of exercise, labor and recreation. From 12 to 40 are associated together in one ward with a proportion of one

attendant to 12 or 15 patients. Each of the assistant physicians has from 150 to 300 patients to attend upon daily, and the medical superintendent makes the round of each ward once or twice a week. There are 6 to 12 wards for each sex, all constructed, arranged and ordered upon the same general plan. Now the change from former life for all who enter the institution is great indeed, and very many, when reason and power of reflection begin to return, find themselves in an abode so different from any ever previously seen or imagined, in its mode of life, its inmates, and its domestic arrangments, that they almost deem themselves transported to a different world. Disordered fancies, sharpened sensibilities, separation from friends, and the absence of anything homelike or familiar, render their condition still more pitiable; and it is plain that kind, careful and sympathetic attention is now sadly needed when reason begins feebly to reassert her sway. The patient is generally under the care of a well-meaning attendant, but a young person usually and too often engrossed of necessity in the mechanical discharge of duty; without special training and, in a majority of instances, not possessing tact and skill, even where the inclination exists, to consult the peculiarities of the person.

In an extensive institution, system, regularity and strict discipline are absolutely essential, and the making of even slight exceptions to any given rule, is a grave matter in its ultimate bearings and results. Thus, such patients as can submit easily to all requirements are measurably comfortable, but those of an eccentric, unpliant, or independent disposition, (a large proportion of the insane,) chafe continually in the harness. There is much to create a feeling that their identity is quite lost, and that they are regarded as parts of a vast machine whose daily evolutions must be performed at all hazards. That kind and careful personal attention so universally pleasing and so essential to a good understanding of any man, sick or well, is lacking, and leaves many sadly alone in their strange surroundings.

In regard to each patient individually it is desirable to determine what influences of every kind will be salutary. Absolute rest is needed for some, moderate employment for others,

recreation pure and simple, for others who if not intelligently guided would determine the question, or have it determined for them in exactly the wrong way. Some patients may be treated with implicit confidence. Others need carefully applied restraining influences. The interaction of patients and attendants is to be carefully studied. The extent to which patients may read, write or receive visits, attend chapel and entertainments, the changes from time to time in mental condition, the transfer from one ward to another, and above all in acute cases, the medical treatment; these and a hundred other questions are constantly arising which to be wisely and successfully decided, require a minute knowledge of each patient's daily life and previous history, which cannot be kept in mind with reference to hundreds of individuals, and it must be admitted that one reason why all are put through so nearly the same routine is, that it is quite impossible for the physician with the large numbers at present in charge to determine each of these questions upon its individual merits; hence all are placed too nearly upon the same level.

Now, how are the difficulties above detailed to be met? Some of these are unavoidable evils, but others, it is to be hoped, admit of a remedy. The most obvious question is with regard to the proper size of an asylum. Without pretending to assign any arbitrary limits, it may safely be asserted that the number of patients in almost every institution is greater than can be cared for with the desirable attention to details in each case, and all possible increase of physicans and attendants consistent with economy is a great desideratum. It were well if each institution could be small enough to admit of closer personal relations between physicians and patients as well as attendants and patients, but they must be taken as they at present exist, future progress in legislative appreciation of the needs of the insane being trusted for determining the foundation of institutions of more harmonious relative proportions.

Another direction in which much may be accomplished is in dividing the insane so as to bring more together those requiring similar management. Many of the chronic insane need no special medical treatment and form a class by themselves.

Again, those requiring medical treatment and special diet and nursing, would fall into one or two nearly related groups.

Further, patients of ample means and more liberal previous lives would naturally expect, and be able to command, quarters equipped on a more generous scale, and require, generally speaking, a nearly uniform management, hence they may be beneficially associated.

In the diversification of employments; in introducing more of the home element into each ward or building; in securing attendants of greater experience and character, and increasing their number, so that there may be more real *companionship* between the insane and those caring for them, I firmly believe a larger proportion than is now imagined may be made comfortable, contented and useful, as well as the per centage of recoveries increased. With regard to the physician's relations to the patients, his selection and trainings of attendants is a main responsibility and point of constant solicitude, and that is one great means of impressing the patient with the influences deemed most advantageous; but it were well if he could come closely enough in contact with each of those under his care to be known as a friend rather than esteemed as hard driven and preoccupied officer; for it is quite impossible that the medical director should know personally that each detail is managed by a hundred employes as he would have it, without constant vigilant following up of his instructions to see that they are executed to the letter, and it is only by instructive examples made of the negligent or untrustworthy that a wholesome *morale* can be maintained throughout an extensive establishment.

IV. The fourth subject proposed for discussion is, "Treatment received by patients from attendants."

Among the unhappy features of insanity is the necessity it so often involves of placing its victim under the care and control of strangers; this has its advantages, shown in the fact that many of the insane are both more reasonable and more comfortable with strangers, than with those before whom they have been accustomed to act out more completely their thoughts and impulses, or whom they have been wont to control by authority, or manage by persuasion or tact.

And here I trust a brief digression will be pardoned for the purpose of saying a few words about the management of the insane by their own friends. It is a fact familiar to those who have been in a position to know that the immediate family and friends of an insane person are not only frequently more mistaken in their kindness, but also more harsh and injudicious than attendants in any good asylum would ever be found to be. Those surrounding an insane person often conduct themselves with little judgment, and sometimes without humanity. In their most proper desire to secure necessary quiet and repose for the disordered mind, they either oppose indiscriminately every tendency to activity, good or bad, or else yield to the most fantastic and unreasonable whims in the hope of pacification, without once seeming to reflect that there is a golden mean to be observed in accordance with the dictates of common sense. The immediate friends of the insane are not only often unwise, but they are misguided and even cruel to a degree which would be scarcely credible to one who had not seen their folly or harshness, or its results. Not only are a majority of the patients brought to the asylum tricked, deceived, and kept ignorant of the step in view, but a superstitious dread of violence, which is supposed to be characteristic of all insanity, leads to the use of unnecessarily harsh and cruel methods of restraint. Patients are often brought to the hospital barbarously bound with ropes or handcuffed, when such restraint is unnecessary; even punishment and cruel beating are resorted to, to correct what is regarded as "ugliness" on the part of the patient, and often when not treated as a malefactor, he is led to suppose he is coming to a hotel, or is left only for a few hours, or days, when he will be called for again. Is it any wonder in such a case that when the patient finds himself shut up in the asylum, he loses all confidence in those he had formerly trusted, and regards with suspicion or implacable dislike, those in whose custody he has been placed by such means?

While the insane are thus regarded by the world in general as wild and dangerous animals, or as perverted mortals with whom there is no obligation of keeping faith, is it surprising that even at the asylum all superstition and ignorance cannot be done away with?

But now to return to the question of the treatment generally received from their attendants by patients in insane asylums: This is perhaps the point of greatest difficulty in the management of an institution for the insane. The selection, instruction, training and vigilant supervision of all who come in personal contact with the insane, gives the medical superintendent full scope for every energy and excellence of character that a man can possess.

It is the attendant who is in constant, familiar contact with the patient, who exercises the most potent influence upon his or her welfare.

The most obvious criticism upon attendants as found in most of our asylums, is that they are lacking in age, experience, and education. They are for the most part a class of unmarried young men and women, in perhaps a majority of cases foreigners, who seek this occupation rather than exclusive manual labor or more menial household employment. It is easily conceivable that persons of education or cultivation would seldom accept such employment, as good abilities and character are better rewarded in other fields; the wages received being generally only such as the commonest labor and service command in the outside world. Furthermore, such an increasing amount of opprobrium has of late been heaped, justly and unjustly, upon those assuming the care of the insane, that many of those who would make the best attendants, would be the last to enter the service.

The fact being then that the attendants upon the insane, nearly all come from the less skilled and educated classes, and embrace a considerable proportion of foreigners, the evils are such as may be easily inferred. Attendants are apt to have little appreciation of any duties toward the insane, except the primary ones of attending to their material wants. They maintain strict cleanliness, and such order as it is in them to secure. They are engrossed pretty constantly with the purely mechanical performance of this work, and equally mechanical obedience to the rules laid down for their observance, but are lacking in those undefinable but equally important requisites which only intelligent sympathy, tact and good breeding can supply. At the same time, there are apt to be among them a

few mere eye servants, who shirk every labor it is possible for them to escape.

The difficulty of securing persons well calculated for attendants is immense; if they possess the requisite energy and power of control, they are apt to be harsh and stern, if they are of a kind and genial disposition, they are apt to be lax in the discharge of duty, and so lacking in authority that the most injurious disorder prevails. If they are young, they are wanting in tact and judgment, if more advanced in years, notional and unpliable. Finally, when good attendants are obtained, they seldom remain long in the service, as it has little to attract, and they either have other views in life, not being able to make a permanent settlement in the asylum, or become irritable, discontented and useless under the ceaseless wear and tear of such a pursuit upon the nervous system and physical endurance; for there is a certain amount of demoralization in the continual contact with depraved and morbid minds and bodies, and the care of the worst forms of insanity has a degrading tendency. Especially is the influence mutually bad between attendants and the obstreperous, filthy, and destructive insane who require restraint, the use of which tends constantly to render both parties oblivious of the law of kindness, and substitutes physical force for moral suasion, the former gradually encroaching where only the latter need be used. But it is vastly easier in taking account of the conduct of attendants to blame them for harshness and cruelty than it is to consider what they really have to endure. They are exposed to vile abuse and risk of personal safety to which few of those who criticise them would ever think of submitting.

The considerations above adduced with regard to attendants naturally lead to the inference (correct I believe for most of our asylums), that cases arise where the helpless position of the patient and lack of knowledge or kindness of feeling on the part of the attendant lead to cruel and evil results—the cruel and evil are seldom, almost never, intentional or even conscious; such are the *morale* and discipline of all asylums, and such the vigilance exercised over all attendants; but are the offspring of selfishness or carelessness, and occasionally of excitement or anger. This, however, is not saying enough for the

charity, generosity, and practical efficiency of our age. A way should be found of securing persons of character for attendants upon the insane, of training them thoroughly and retaining their services during good behavior.

It is now pertinent to inquire what means are available for improving the quality of immediate attendance upon the insane. The first step in advance it would appear desirable to make, is to secure a higher grade of care for all acute and recent cases of insanity (which deserve most careful treatment on account of greater prospect of recovery), and provide skillful nursing for all cases of bodily sickness. A thoroughly trained nurse is a rarity among the sick either in public or private. The hospitals and asylums for insane are generally not altogether destitute of good nurses, formed on the spot by patient teaching; but as a rule, they have manifold additional duties, and can ill be spared to give all attention to single individuals. A single recent case, however, might often profitably employ the entire time and energies of a well selected attendant, perhaps by that means alone recovering health and reason; and it is desirable to bring into prominence here the fact that almost any pains taken, or expenses incurred, for curable cases, are well bestowed. One year's support at an expense of, say, $500, would be infinitely cheaper than sustaining a patient for the rest of his natural life at the public expense, if he remained insane, and he would, furthermore, if cured, return to society and the State as a producer.

What is needed is a larger corps of attendants to admit of detailing for special duty as many as may be called for by any given amount of sickness, or number of acute cases wanting special care. The proportion of attendants for recent cases should, as a whole, be greater. In the best English and Scotch asylums it is one to eight for all pauper patients, and one to three for private patients. This admits of a degree of attention to the needs of each person, and of companionship and recreation, which are highly beneficial. We might accomplish in part the same object, and not unreasonably increase expense, by bringing within more narrow limits the classes here considered, and by employing for attendants upon the great body of the chronic and mild insane, men and women of sufficient

qualifications for this less exacting work, who will labor or occupy themselves with the insane, and whose labor will be to some extent productive. Furthermore, if men and their wives are allowed to be associated in the care of the insane, a much needed element of stability will be introduced, and at the same time an additional advantage secured for the patients in the domestic atmosphere thus secured, an element which our asylums greatly need, and which their inmates sadly miss.

A higher state of organization and efficiency, a more complete equipment of institutions (the majority of which are still in their infancy with us), and an improved public sentiment, will each in their different way contribute to the securing in each institution of a more skilled and intelligent class of help, as well as a few specially qualified for companions to the insane.

V. The fifth and last topic proposed is, "The relation of the institution for the insane, (1.) To the general public, (2.) To its own officers and employes, (3.) To advancement of scientific knowledge.

It is not attempted to discuss these topics in all their bearings, but only to point out one or two more pertinent considerations with regard to each.

(1.) The relation of the asylum to the general public.

There are three causes which lead to prejudice, suspicion, and misapprehension on the part of the public toward our asylums. First. A part of every asylum is ever kept closed from the inspection of the public at large. Second. Patients often require to be totally separated from their friends, and not allowed for a considerable time to see those most interested in them. Third. There is a prevalent disposition to withhold from general view everything unpleasant or repulsive connected with the asylum, and exhibit only its agreeable and inviting features. The first two practices are founded in wisdom and well-established rules of management, but it may well be questioned whether it is not possible to avoid, a little further than is always done, the appearance of keeping from the public eye all the more disagreeable and violent features of insanity, and whether, in a way to do no injury, a truer idea might not

be given of the treatment received, and the quarters occupied by the turbulent insane. It is safe to say that by doing so, a great number of unfounded prejudices and wrong suppositions would be removed. Risk is incurred in allowing none but the attractive side of the hospital to be seen. This has in some degree the very opposite effect to that intended, giving busy imaginations and tongues all the greater room for play, and does not satisfy large numbers of visitors, many of whom, perhaps, have a senseless craving for something sensational; but others are also actuated by a perfectly legitimate desire to know how the more extreme cases are managed, as well as by sympathy with their misfortunes; for the true knowledge of the condition of the insane in our public asylums, of what is done for them, and of how they are dealt with, is a proper subject of inquiry with citizens and tax-payers.

Another evil of repression is, that where there is any suspicion or any grievance, real or imaginary, it results in an amount of smouldering and pent-up ill-will within and without the walls of the asylum, which accumulates and propagates itself continually, until an explosion is necessitated, often disastrous to all concerned. This infectious matter may be compared to waste and noisome products in the material world which are poisonous in the highest degree where allowed to collect in heaps, but scattered and exposed to the free air and sunshine, lose all their dangerous and pestilential qualities. So the admission into asylums of the utmost light and freedom, consistent with the welfare of the inmates is very desirable. There are certain limitations necessitated by propriety or regard for the feelings of friends or welfare of patients, but it is better that every detail of a given case should be fully known, and that patients should be seen in their worst as well as their best estate, than that wrong inferences should be permitted to be drawn.

2. "The relation of the asylum to its own officers and employees."

The principle for which prominence is here sought is that of the paramount importance of the interest of the insane over any and all other interests in the institutions founded for their benefit. Their confinement in the asylum deprives them of

the most jealously guarded rights of our race, and it is due to self-respect and humanity to see to it that the few privileges which still remain to them are not violated, since they are themselves without the power to demand just treatment successfully, even where they have any proper conception of their position.

In innumerable ways injury and injustice may result to the insane from their disadvantageous situation, unless all who are engaged in caring for them have frequent occasion to reflect that their positions are created *for the service of the insane* and have no other reason for existence.

In the ordinary affairs of life self-interest may be left to its natural powerful agency in regulating conduct, but in the asylum, its operation must be disastrous if it rules with unrestrained force where the power and skill are all upon one side. Great magnanimity and watchfulness and self-restraint, therefore, can alone enable either officers or subordinates to fulfill all their duties and all should be impressed with the thought that the asylum is the home of the insane and all its resources the property of the insane; held in trust for them by those in charge and that the trust is a most important and sacred one.

3. "Relation of the institution for the insane to advancement of scientific knowledge."

It is frequently remarked with regard to the numerous asylums of our country, that few of them produce any records for public or professional benefit, of researches in the wide and attractive fields of physiology and pathology of the mind and nervous system, which every asylum must offer to the enterprising explorer. Contributions to our knowledge of mind or brain are received from but few of our hospitals and asylums for the insane in spite of the large stores of rich material and opportunities for observation which are ready to the hand of any who will improve them.

Some of the reasons for this are not far to seek. Most of our institutions are as yet but imperfectly supplied with even the appointments of actual every day convenience or necessity. Still less do they possess the facilities needed for original investigation, study and research. The appliances requisite, the instruments and apparatus, the morgue, the library of stand-

ard works and periodicals, can only be obtained at considerable outlay and no money is appropriated for such purposes. We need a livelier appreciation on the part of the public and of legislative bodies, of the actual *waste* of valuable material that ought to be utilized for science which goes on continually in our institutions for the insane. As a mere matter of business the public and profession of medicine should demand some return in progress of knowledge for its outlay upon these institutions and should place them in a position to meet that demand. The additional expense would be trifling compared with the advantages gained.

Again, scientific labors and researches of the kind under consideration, to possess value, require on the part of those making them extensive preparatory study and familiarity with what has been accomplished in the same field by others, as well as close and continuous application of time and thought. But in the large majority of cases, the latter of these essentials is wanting, even where the former is present. The medical officers of our institutions have an immense number of miscellaneous duties; indeed, their medical duties, pure and simple, form but a small part of what devolves upon them—office work, business routine, and correspondence, with various administrative labors, consume a vast amount of time. In a public position, numerous demands not felt in private life, must be met. The number of patients under their care is so great that proper supervision of the whole does not admit of undivided attention to one special case or group of cases. Add to this the incessant interruptions to which they are liable, and it is easily seen why only a few of the older or more specially favored institutions yield results of consequence to science.

For the above reasons, it seems desirable to increase the medical staff of these institutions, not in order that physicians may have less to do, but that their labors may be so concentrated and specialized as to give a higher efficiency to those of each.

Every asylum should be made more and more a centre of observation and research, a source of information and instruction to the profession of medicine, and of enlightenment to the public. Our medical schools should enforce more thorough

instruction in mental disease and make a respectable knowledge of insanity an essential of graduation; not only that physicians may be better prepared to deal intelligently with madness when occurring in their practice, but that they may foresee and avoid it for their patients and aid in its eradication. There is no reason why advanced students in medicine should not act a mutually advantageous part in the economy of many hospitals for the insane by undertaking minor duties and assisting the labors of the regular medical staff.

A recapitulation of the points embraced in this paper, leads the writer to the conclusion that there is not so much any one radical reform needed in our asylums, as a higher and finer state of organization and efficiency in every department. The whole life of the asylum ought to approach more nearly the natural course of human existence. The standard should be the domestic comfort and felicity of home. True family life is the ideal for universal humanity, however imperfectly it may be attained in any given case, and with all the restrictions and limitations which insanity necessitates, it is still the safest and surest guide. Life in the asylum as at present constituted, is so unnatural, narrow, barren, and mechanical; the situation of the inmates is so artificial, that they lack opportunity to assert themselves in good directions as well as in bad. Their relation to the asylum is simply incomprehensible to a majority of the insane, and weighs upon them with a paralyzing force which it is safe to say might be relieved in numerous instances by a more natural and familiar environment.

To accomplish this object—

1. Employments need diversification.

2. The sacred right of the insane to every possible extension of liberty, and removal of restraint needs *new emphasis*.

3. Every patient needs to be treated upon his or her individual merits.

4. Attendants more highly skilled and intelligent who can, as occasion offers, be either agreeable companions or kind nurses, need to be brought into the service and increased in number.

Our institutions for the insane, now as ever, reflect the point at which average public opinion has arrived, and it would

be folly to assert that perfection had been attained or progress could not still be made. With better knowledge of insanity on the part of the people at large, quickened legislative interest, and larger facilities in the command of officers of asylums, a new atmosphere may be brought to pervade the abodes of these unfortunates; but these must all work together, and can accomplish little if at war.

A movement for the social and moral deliverance of the insane, similar to the physical emancipation effected by Pinel when he struck off the chains of the madmen in the Bicêtre, seems to be gathering force from all sides. Iron manacles were then considered appropriate for lunatics, and the great liberator was asked if he had not himself gone mad in making so wild an experiment.

To-day, perpetual enchainment in a living tomb is no longer tolerated, but fetters of the spirit are equally odious to our age, and the philanthropy of the time will not rest until it sees the asylum for the insane deprived of its many remaining imperfections.

Art. VI.—RAPIDITY OF TRANSMISSION OF NERVE-FORCE IN NORMAL AND STRETCHED NERVES—EXTRA-POLAR KATELECTROTONUS.

By Isaac Ott, M. D.

THE ability to measure the rapidity of nerve-force was thought to be impossible, but Helmholtz, by means of the galvanometer, solved the question. The galvanometer was used to measure the small interval of time. He afterwards turned his attention to the graphic method of measuring the time involved. His results on the nerves of frogs were, that between 11° and 21° C. the rapidity varied between 24.6 and 38.4 metres, the most probable mean as 26.4 metres per second. Low temperature reduces the rate of transmission. In my experiments I employed a Marey-Foucault regulator, a

glass disc on the vertical axis which revolved with great rapidity, and on the glass a piece of smoked paper was fastened. A bridge of two pieces of light strips of metal was made, and mounted on a piece of hard rubber. On one of the wings of the Foucault regulator was placed a projection-point, which, when the expansion reached a certain point, knocked over the bridge and opened the battery current in the primary spiral of Dubois-Reymonds induction apparatus. This generates an opening induction-current in the secondary spiral, which irritates the nerve causing contraction of the muscle. The excitation is sent to any part of the nerve by means of Pohl's commutator and Rosenthal's rheophoric box, which is a gutta-percha trough, having wires traversing it at intervals, and covered with a glass plate; its bottom being covered with moistened blotting paper. On these wires the sciatic nerve of a frog is laid, and kept moist. The gastrocnemius muscle is attached to the lever of a Marey's myograph, which registers the contraction on the smoked disc. Now as the current is only broken when the fan of the regulator expands to a certain point, the same rapidity of the disc is always obtained for each experiment, and the current is always broken at the same point. It is possible that there may be a slight variation of rapidity, but I think it can be overlooked. The time was obtained by allowing a tuning-fork to register one two-hundred-and-fiftieth of a second. (In Fig. 1 I give a result). The inmost circle shows where the muscle began to contract when the irritation was made near the muscle. The outer circle shows where the muscle contracted when the nerve was irritated four centimetres up the nerve from the preceding point of irritation. The zigzag curve gives the curves of the tuning-fork, each swell being 1-250th of a second. Now radii are drawn from the centre of the disc through the points of departure of the muscle-curves, and prolonged to the tuning-fork curve. Now the difference between the radii will be equal, as is seen to 1-500th of a second for four centimetres of nerve, that is twenty metres per second. These experiments were made at a temperature between 9° and 10° C. In an examination of a considerable number of experiments, I found the rate of transmission in the motor nerve of the frog was between

twenty and thirty metres per second. Care was taken to ascertain that the latent period of muscular contraction was unchanged during the experiment. When the rate of movement of nerve-force was compared with that of electricity, it was found insignificantly slow, electricity moving 464 million

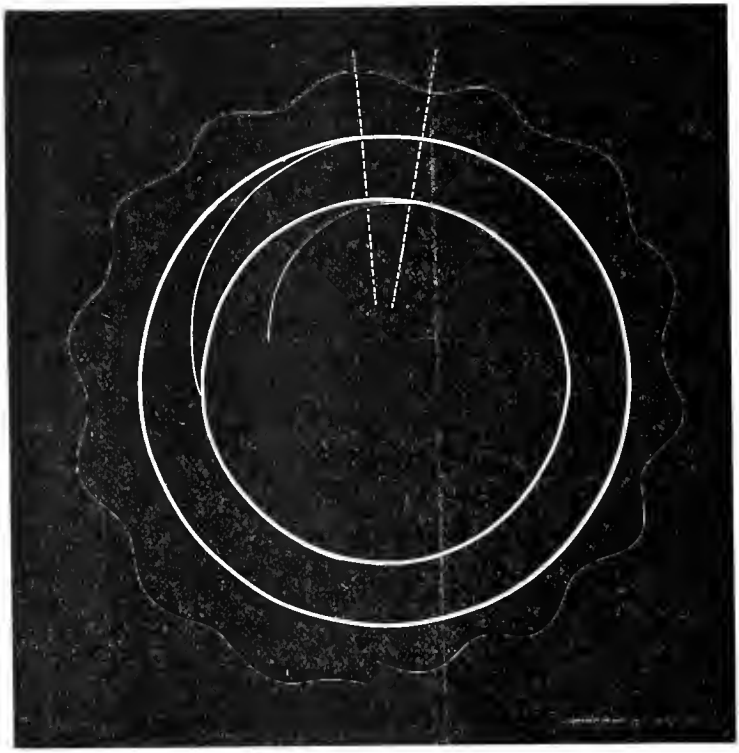

Fig. 1.

metres per second, and light 300 million metres in the same time. This slow rate of nerve-force is usually one of the great arguments against the identity of electricity and nerve-force. Whilst I do not believe in their identity, it is not a very strong argument, as there are certain agents through which electricity will not pass at all. Nerve-force must as yet be considered among one of the things called vital, due to some *Great Proximate Cause*. That electricity and nerve-force have similar properties, I shall show. Faraday unveiled that when a wire is subject to either volta-electric or magneto-electric induction

it resists the formation of an electrical current in it, whereas, in its ordinary condition, such a current would be produced. He supposed this due to a polar arrangement of the molecules of the wire, and called it electrotonus. Now Dubois found that the galvanic current, when passed through a nerve, offered a resistance to the animal current of a frog's nerve. After Faraday, DuBois called this electrotonus of the nerve, and considered it due to a polar arrangement of the nerve-molecules. Further, electrotonus offers a great impediment to the rate of conduction of nerve-force. Having studied the rate of transmission of nerve-force in normal nerves, I made some researches on the rate in stretched nerves; stretching nerves having become a recognized method of treatment in some nervous affections, such as sciatica. Here the difference between the curves of origin of muscular contraction on the same line were measured, and conjoined with that found after stretching the nerve.

Experiment 1. Nerve-muscle preparation. Temperature, 9° .2 C.

10.45 A. M. Normal distance between origin of curves 4 m. millimetres.

10.46 A. M. Nerve stretched from 3 centimetres to 4 centimetres.

10.48 A. M. Distance between origin of curves, 7 m.

Experiment 2. Nerve-muscle preparation. Temperature, 9° .2 C.

10 A. M. Normal distance between origin of curves, 4 millimetres.

10.1 A. M. Nerve stretched about 2 centimetres.

10.2 A. M. Distance between curve's origin, 5 millimetres.

10.3 A. M. Extra-polar katelectrotonus of nerve near the exciting electrodes at greatest distance from the muscle.

10.6 A. M. Distance between curves, 6 millimetres.

Experiment 3. Nerve-muscle preparation. Temperature, 10° .4 C.

11.40 A. M. Normal distance between origin of curves, 3 millimetres.

11.41 A. M. Nerve stretched from 3 centimetres to about 4.5 centimetres.

11.43 A. M. Distance between origin of curves, 4 millimetres.

11.44 A. M. Extra-polar katelectrotonus.

11.45 A. M. Distance between curve's origin, 9 millimetres.

As will be seen by an examination of the above experiments, stretching lowers the rate of transmission of nerve-force in nerves. If there was any relation between the amount of stretching and rate of conductivity it was not determined. In studying the effect of extra polar katelectrotonus, I used two Bunsen cells of considerable size, and the extent of nerve traversed was about three millimetres. When a galvanic current traverses a nerve it separates it into two zones; the part about the negative pole is called katelectrotonus, and that part about the positive pole anelectrotonus. Von Bezold found that extra polar katelectrotonus reduced the rate of movement of nerve-force. Like him, I used strong currents in the study of extra polar katelectrotonus. As to the effect of weak currents, I cannot speak. The following expresses the conclusions at which I have arrived:

1. The rapidity of transmission of nerve-force, at the temperature of about 10 C., is between twenty to thirty metres per second.

2. That stretching a nerve lowers the rate of movement of nerve-force.

3. That strong currents causing extra polar katelectrotonus, as in normal nerves, retard the movement of nerve-force.

ART. VII.—CASES OF HEMIPLEGIA, ETC.

BY S. G. WEBBER, M. D., BOSTON.

LECTURER ON DISEASES OF THE NERVOUS SYSTEM. HARVARD.

THE first of these cases illustrates a difficulty in diagnosis which is not likely to arise very frequently. Not a difficulty in regard to the nature of the lesion, but as to the cause and significance of certain symptoms. The pain and swelling of the joints occurring soon after the hemiplegic symptoms gave rise to a question as to whether they were due to the cerebral lesion, were merely the arthritic complications following cerebral softening, or were the symptoms of rheumatism.

The patient was a business man 60 years of age, who had been actively engaged in business all his life. I was asked to see him by his physician, Dr. Harriman, from whom I learned the history of the case. About two weeks before I saw him, he had an attack of pain in his left side from which he suffered only a short time, but he regained his strength only slowly. Previous to this attack, he had had pain in the left wrist which was slight and attracted but little notice. About a week after the pain in his side he awoke his wife in the early morning by trembling, shaking and sobbing in his sleep. He was awakened with difficulty and said he had had a bad dream. After this the pain in the left side returned. At that time he had no motor nor sensory paralysis. He continued under treatment on account of the pain in his side. The day before I saw him he rose and washed himself, he then lay on the lounge with an arm hanging over its back. The Dr. called to see him, and after a few minutes conversation he attempted to rise to get into bed again but could not support himself, he slipped down and was evidently weak. The left

side seemed most affected. Subsequently the left hand was found to be numb, and was used poorly. No pulse could be detected in the left radial, but could be in the left ulnar. I found him in bed with a rather florid face. The action of the two sides of the face was about equal, though when at rest the face was drawn slightly to the right. The pupils and eyes acted naturally, and the tongue was protruded straight. The arms and legs were moved freely in bed and there was no difference in the two sides, but on rising he could not manage the left leg so well as the right, and the left hand could not be so well directed with the eyes shut as the right. There was evident retardation of sensation in the left hand and left leg. The pulse in left radial was weaker than in right. Heart sounds were normal. With the ophthalmoscope both optic nerves were found rather clouded, the veins only showing well. The light troubled him, and he moved his eyes so much that it was not easy to make the examination.

During the next three or four days he improved, the pulse returned full and strong to the left radial. An examination of the urine showed no albumen and no casts. On the evening of the fourth day, he began to have pain in all the joints on left side, but in especially the hip and knee. He was sleepless and restless, the left side being moved less than right. In the morning, on moving the joints of the left side, the pain was very marked and the joints were tender to touch. When I saw him he was partially under the influence of morphia, but could be easily aroused and the pupils responded to light, when he sank off into a semi-comatose sleep the pupils were contracted. He complained especially of the right hand and right leg as painful under passive motion. The left were also sensitive, but less so than the right. Both ankles were red and swollen, the hands were slightly swollen at the joints and some of them were red. The heart sounds were much muffled, not clear and distinct as at last visit. Respiratory sounds were clear, percussion was obscured by a very tympanitic abdomen. Pulse 114 to 120. Resp. 27. Skin was hot and not very moist though he had perspired before my visit. Temperature was not taken. There was ptosis on the left side, and the facial paralysis was more marked than at the

previous visit. There was no possibility of judging as to the paralysis of the limbs on account of the pain caused by motion.

Two days later he died and at the autopsy made by the attending physician, the right middle cerebral artery was found plugged by a clot, the cerebral tissue was softened, and the arteries at the base were extensively atheromatous. The aorta near the heart had two or three patches of atheroma. The heart was healthy.

The joints may be affected as a consequence of lesion of the nervous system; it is not uncommon to find this in cases of disease of the spinal cord, it is also found in cases of cerebral lesion, though not quite so much attention has been paid to the latter lately, perhaps because the arthritis of locomotor ataxia has been described at only a comparatively recent period.

Charcot has given a description of these cases, though he was not the first to describe them, and it is from his description especially that attention has been attracted to such cases. The arthritic symptoms, according to him, are generally developed from fifteen days to a month after the apoplectic attack, about the time that the late contraction appears; the swelling, redness and articular pains are sometimes as great as is found in acute articular rheumatism. The tendinous sheaths are also often affected. The joints affected are exclusively those of the paralyzed limb, often only those of the upper extremities are affected.

The joint affection in the present case affected both sides, and the lower limbs more than the upper, began at a much earlier date than that given by Charcot, appearing the fourth or fifth day after the occurrence of the hemiplegia. Nothnagel, in Ziemssen's Cyclopedia, is less precise in stating the time when this symptom may appear; he says it may begin from a few days to four weeks after the hemorrhage. As he says nothing about his own observations, but refers to Charcot and others, it would probably be proper to consider Charcot's statement as the correct one. Then in the present case, the time at which the arthritis appeared would exclude the cerebral origin of the lesion. When the patient was first seen

little importance was given to the pain in the wrist, because it was so slight. It is not improbable that it was the first symptom of a rheumatic attack, and if so, the subsequent joint affection would belong to the same.

The pain in the wrist and the affection of the joints of the non-paralyzed side, would confirm the diagnosis of rheumatism. My notes make no mention of the nature of the pain in the side, excepting that the physician spoke of it as pleuritic.

My diagnosis, when I first saw the patient, was embolism; on the second visit after the joint affection had developed, I considered the case rheumatic, with probably the formation of a clot in the heart. The febrile state at the second visit did not assist in the diagnosis, as it might be present if there was no rheumatism; but the reported sweating rather confirmed the existence of rheumatism. The autopsy showed the correctness of the diagnosis of embolism, but could not decide as to the existence of rheumatism.

In contrast to this, so far as relates to the arthritic symptoms, may be placed a case in which the affection of the joints was undoubtedly due to the cerebral lesion. The arthritis did not appear until a much longer time subsequent to the paralysis, and was confined to the paralyzed limbs. The case is likewise interesting, as there were cerebral symptoms preceding the definite attack by a long time, but apparently not serious enough to give rise to much apprehension. I was inclined to look upon the case as one of obstruction of an artery by thrombosis, giving rise to softening, possibly several small arteries were thus occluded at different times.

Mr. A. was seen in April the previous summer, and in November, and again in January he had had attacks of cerebral disturbance, that in the summer being less severe than the others. The disturbance consisted in a difficulty in collecting and arranging his thoughts; he could follow one train of reasoning, but could not combine it with others. In the last attack he made absurd mistakes when dressing, getting his clothes upside down. In January, he fell once or twice, has had some headache, not much dizziness, rather confusion of mental power. He went away for a rest, but per-

sisted in using his brain, engaging in considerable mental work. About three days before I saw him after speaking in public, on going on the street he was dizzy, and later the left index finger felt numb. He denied being dizzy often, yet in the next breath said he was never attacked while speaking, or excited in debate, only afterwards; and several times he spoke as if he had frequent attacks of discomfort in his head. Two days before I saw him he rose and dressed; later, he fell on the floor. There was weakness of the left leg, the face was drawn a little to the right, the tongue was protruded straight, the pupils acted well, the eyes moved naturally, the left hand was weaker than the right, and there was diminished sensitiveness in the left hand. The mind was evidently weakened, and he was rather childish; he could not keep his eyes steady for examination. Afterwards the cerebral symptoms became more severe. I saw him only once, and several weeks later he died. Before death the joints, especially the knee and hip of the paralyzed side, were the seat of very severe pain; there was very little swelling, if any. There was no autopsy.

Another form of trophic change after cerebral lesion, causing hemiplegia, and which is rare, was seen in the following case.

Mrs. B. was first seen September, 1874; she had had considerable trouble, and was much depressed by her husband's death. Four or five weeks previously she had taken a long ride in the rain, with the wind blowing upon the left side of her face. Immediately after the face was drawn to the right, the left eye could not be closed. When seen there was left facial paralysis, all the branches of the seventh being affected, the eye not closed. At that time she said she never had had any trouble in hands or feet, but was not as strong as formerly; had no tingling, pricking, nor numbness in limbs. The sensation of the face seemed the same on both sides, eyesight was not affected; no diplopia; tongue protruded straight; no difficulty in deglutition, nor in respiration; taste was retained on both sides. The reaction of the facial muscles on the left to the faradic current was quite lost, excepting a very slight action about the mouth. The muscles on left of face responded to eight cells, slightly to six. Subsequently, she complained

occasionally of being "dizzy in her legs and weak." She was troubled very much with dryness of the mouth. She recovered the ability to close the eye, excepting a very narrow chink. The face, when at rest, looked nearly natural, and yet later there was scarcely any distortion during conversation.

This was undoubtedly an ordinary case of facial paralysis from exposure to cold. The paralysis of the upper branch of the facial, the loss, complete and early, of the faradic contractility, the absence of other symptoms, sufficiently indicate that the lesion was in the course of the nerve.

In January, 1877, I was called again to see her, by the advice of Dr. Reynolds. She said she had been feeling unusually well until September, when, after taking a bath, without pain, without loss of consciousness, without previous headache, dizziness, or "sleepy" sensation, she lost the use of her left hand, and soon after the left leg, and she slid down gently. After a while she gained power to walk; the power of using the hand was recovered only very slowly. When seen in January, all the motions of the hands and fingers were possible but were executed slowly, excepting that the hand could not be fully closed. The power of grasping was not strong, but with the fingers half flexed there was considerable power in pulling. To the faradic current the muscles acted generally less readily than on the opposite side, especially the extensors and the flexors, the interossei responded better, the extensor indicis and abductor policis very poorly, the flexors of the fingers very poorly also, but the flexors of the wrist well; the biceps acted poorly. The muscles of the leg were not compared with the opposite side, but their reaction was evidently below the normal, especially so in the case of the tibialis anticus. The left arm was very much smaller than the right, and the left leg was somewhat diminished in size. There was slight contraction of the extensor muscles of the fingers, preventing complete flexion, otherwise no contraction.

It is stated by nearly all authors, that after cerebral lesion the electrical reaction of the muscles is normal, and there is comparatively little or no wasting of the muscles. When hemiplegia, however, occurs in childhood, there may be retarded development, and hence one side may become larger

than the other. In this third case, there was a very unusual disturbance of nutrition. The first attack was clearly simple facial paralysis from exposure, and ran its regular course. It would hardly seem possible that there should have been any connection between the two attacks. The second attack, judging from the patient's account and from the account given by her physician, as well as by the general course of the symptoms, was clearly of cerebral origin, probably was a hemorrhage. After several months there is found atrophy of the arm and leg, and decided loss of electrical reaction in certain muscles, this loss of electrical reaction being greater in the leg than in the arm.

There have been two cases, similar in this respect, reported, both with autopsies; one by Charcot (Legons sur les maladies du systeme nerveux t. I.; 1872, p. 55), the other by A. Pitres (Arch. de Physiol.; 1876, p. 657). In both of these there was descending secondary degeneration of the lateral columns, and in both there was found also destruction of the cells of the anterior cornu on the paralyzed side. In both there was contraction of the paralyzed muscles. In the present case there was a slight contraction, though not of the flexors as is most common, but of the extensors. It is reasonable to conclude, that with secondary sclerosis of the lateral columns, there has been also destruction of some of the cells of the anterior horn. It is not so easy to recognize slight contractions in the foot as in the arm; but as the patient walks very slowly with short steps, with more apparent feebleness than the amount of paralysis would account for, there may well be a degree of stiffness dependent upon muscular contraction of the muscles of the leg. The loss of electrical reaction being greater in the leg than the arm, would indicate that the cells in the lumbar enlargement are more extensively affected than those of the cervical.

In striking contrast with this condition of the arm and leg is that of a young woman who, twenty months ago or more, had an attack of right hemiplegia with aphasia. After about three months the aphasia slowly disappeared and partial power was recovered over the limbs, but then began to appear a contraction of the fingers, hand, and fore-arm of the affected side,

There has been for more than a year almost entire loss of voluntary control over the hand and fingers; the fingers are almost constantly closed, though they can be extended passively. Now, after these many months of such entire paralysis, the affected arm is larger than the other by nearly a quarter of an inch, and all the muscles respond readily to electricity, showing that the nutrient or trophic cells in the anterior cornu are not affected.

It may be well to add, that after long-continued but rather irregular treatment with electricity, there was decided improvement in the case of Mrs. B.; the muscles responded more readily to electricity, but not quite so well as those of the sound side. Considerable power was regained, and much of the tremor disappeared.

A Case of Lesion of the Facial Nerve with Anomalous Electrical Reaction.

The following case is of interest owing to the anomalous reaction of the muscles to electricity. The usual result of an injury to a nerve is, immediate loss of voluntary control over the muscles supplied by that nerve; later, loss of reaction in both nerve and muscles to the electrical stimulation. The power of the will generally returns first, or at the same time, with the return of the power of reacting to electricity.

E. S. was first seen at the City Hospital, in the Department for Nervous Diseases, in April. The history she gave was, that four years ago she had a tumor removed from the left side of her neck, just below the ear. There is a scar at that place, just behind the angle and ramus of the jaw. She said that her face was subsequently drawn over to the right. When seen, there was partial paralysis of the left side of the face, the will having scarcely any control over the muscular action; the left eye could be only partially closed. All the muscles responded to a strong faradic current, when the electrode was applied directly to the muscles. When the electrode was placed behind the scar, *i. e.*, over the proximal end of the nerve with reference to the seat of the lesion, all the muscles

of the face acted to a current of moderate intensity; when the electrode was placed in front of the scar, over the distal end of the nerve, a very strong current was required to excite contraction in the muscles. When a current that merely caused slight contraction, as applied in front of the scar, was used behind the scar, very violent contractions of the facial muscles were produced. This peculiarity was noticed several times at intervals of weeks.

This case would give support to the views lately advanced, that nerve fibres may be capable of conducting an irritation when they are not in a state to respond to an irritation—the receptive function is a different one from the conductive. Here the nerve fibres were regenerated sufficiently to convey the stimulus afforded by the electricity when applied to the healthy nerve, but they were not in a condition to readily receive that stimulation directly.

Under treatment, there was considerable improvement in the power of the will over the muscles, and the eye could be nearly, or at times quite closed, but there was no change in the above electrical reactions while the patient was under observation.

Neurological Correspondence.

Medico-Legal Society—Dr. Finnell on Chloroform Administration; Dr. Mann on Moral Responsibility; Mr. Cephas Brainard, Points in Law and Medicine. *Neurological Society*—Dr. E. C. Seguin, Bulbar Paralysis; Dr. Spitzka, General Paresis; Discussions.

New York City, Dec. 26, 1877.

Mr. Editor:—I have the pleasure of forwarding much that has occurred in neurological and medico-legal circles, during the last three months, which, I think, will be found of interest and importance. I had hoped to have sent you several more valuable papers, but they are undergoing revision by their authors.

I would call especial attention to the question of the manner, consequences and responsibility of the administration of anæsthetics, and particularly to the remarks by legal gentlemen, as set forth in the discussion of Dr. Finnell's paper.

THE MEDICO-LEGAL SOCIETY.

Dr. T. C. Finnell, in a paper before this society, contended that physicians and surgeons should be held to a stricter accountability in the administration of anæsthetics. He was of the opinion that they were not handled with the skill and judgment which their dangerous properties required. So many young men were employed in administering them that many lives were recklessly sacrificed. It was unquestionably the fault of the surgeon who trusted what was an important duty to inexperienced hands. The uncontrovertible signs of approaching danger were not sufficiently watched—such as the condition of the pulse and respiration. Anæsthesia was adopted with the object of producing an insensibility to pain, whereas in almost every case an insensibility bordering on death was usually the result, so that the patient often died even before the knife was used by the surgeon.

Dr. Finnell believed that all members of the profession should be held responsible, as the time was not far distant when lawyers—even their associates of the same society—would be bringing suits against them for malpractice.

A physician in administering chloroform, which results fatally, in Dr. Finnell's opinion, ought to be considered as the responsible agent of causing death, and he thought it would be beneficial to the profession were some law enacted which would take them to task for culpable negligence.

In the majority of cases which had come under the doctor's observation, the surgeon was an independent factor, leaving the responsible work to an assistant. Every case should be treated carefully, and forceps, or any other instrument, should not be used until counsel had been taken from some colleague showing the necessity thereof; for it was a great habit of surgeons because the patient was under the influence of anæsthesia, to use force, thus endangering his recovery. This was the theory of Prof. Gurney, who was utterly opposed to allowing the whole system to be prostrated.

The doctor concluded by advocating more moderation in the use of anæsthesia.

Prof. Hamilton thought that Dr. Finnell had shown much courage in attacking what was commonly considered by the profession at large as the favorite means of alleviating pain. There is no remedy perhaps within the knowledge of man which seems to have accomplished so much good, and for which so great indebtedness is felt as for the various forms of anæsthetics; but Dr. Finnell has dared to throw a shadow of doubt upon them. He has dared to intimate that there is danger in their use; and he is right—they are dangerous agents. Whether the surgeon is to be held responsible under the law or not, is a question which the world must decide. The essayist thought that surgeons should be held responsible for deaths occurring under their use. Prof. H. was not sure but that he was right, if they did not teach others of this danger. He assumed the position that no agent capable of annihilating pain can be discovered which will not at the same time compromise life and health. The very annihilation of sensation itself impairs the health of the organs of the body; he had no question about it, and often during the administration of these agents, nothing but God's providence prevents them from being plunged into eternity! But, on the other hand, there are many things to be said in favor of its use. The main lesson to be learned is caution.

Daniel S. Riddle, Esq., said, as regards the enactment of further laws on this subject, it was not necessary. There are sufficient laws already. The difficulty is in enforcing them. If there is carelessness on the part of the doctor, he should be held responsible. It is the same with lawyers. It belongs to the profession of medicine to say whether these agents have been carelessly used; and if they have been carelessly used, it is their duty to produce, as well as to indicate, the person who uses these great powers carelessly. "If we lawyers," he remarked facetiously, "find out that you are carelessly using these things, it is our duty to "pitch in!"

Mr. Coroner Ellinger thought it would be a hard matter to hold doctors responsible, because it is hard to state scientifically where the responsibility can rest. It ought at least to be shown that there was conscious negligence in the performance of duty. The condition of the patient should be taken into account, and that must be left to the discretion and knowledge of the attending surgeon. He thought that the medical student ought to be taught the danger of the agent he employs, how and when to use it judiciously, and he should be required to secure a certificate to the effect that he has been so instructed before being let loose upon the public. The public would then know that he possesses a knowledge of the agent which he employs. Besides, the physician would be conscious of a certain moral responsibility, for he held that the moral responsibility which every medical gentleman must feel is greater than the responsibilities placed upon him by the laws of the land.

Dr. Finnell further remarked that when he graduated in medicine, twenty-seven years ago, together with another student, he was in the habit of administering ether for one of the best surgeons America ever knew—Gross, of Philadelphia. It was Dr. Gross' business to attend to the operation, and it was Dr. F.'s business to administer the anæsthetic. He was in the habit of "sousing" the patient, and how much damage and mischief he did in ten years he cannot begin to say. He advocated the proper instruction of the student in the use of anæsthetics; but the surgeon should always supervise their administration.

Dr. C. S. Wood thought from the remarks made that we ought to get down on our knees and call on God almighty for mercy! Every one will admit that more lives have been saved by the use of anæsthetics than have been destroyed by them. He believed that at least ten lives had been saved from shock to the nervous system, which precedes and accompanies an operation, where one had been destroyed by anæsthetics.

Jacob F. Miller, Esq., agreed with those who advocated care in the use of these agents. Man is living in the midst of dangerous forces, and will continue to use them though of necessity many deaths occur; but in order to rest a case against the user, it is necessary to show negligence. Negligence is the gist of the action. The physician, surgeon, or lawyer, contracts for the ordinary skill and care of his profession. He does not contract for any extraordinary skill. The law does not hold him any more responsible than that. It would be unreasonable to do so, because few persons could safely practice their profession, and if any person should use anæsthetics, and the patient should die, that is not sufficient to charge him with the responsibility. He thought all would admit that if a man not having the ordinary skill of his profession, should by unskillful administration of anæsthetics cause the death of the patient, he should be prevented from doing further damage by a suit for malpractice. Would it not be better to stop him by such procedure? Shall a man be allowed to use such dangerous forces just as he pleases—let the consequences be what they may? People consult physicians because they say they have the requisite skill to use these things. They hold themselves out to the community as having this skill; and they ought to possess it if they do not, and harm results from it, they ought to be held responsible. The coroner says that physicians are actuated by moral responsibility—which is just no responsibility at all! The quack will go on with his practices until he is stopped by the law. Where is his moral responsibility? What does he care? His practices only go to show that he has no moral responsibility. That lawyers should check such practices, is due to the profession, to the public, and to God! But before they take a case of malpractice, they ought to be convinced that there is malpractice.

It may be all very well to say that negligence is the gist of an action. If it cannot be shown that there is negligence, the case should not be taken; for when the case arrives at the courts, you must show that the defendant is guilty of neglect; and that is done by calling upon a physician who is able to say where negligence has been committed, and that he is guilty of it. This evidence is necessary—lawyers cannot get along without it.

Mr. Max. F. Eller spoke of the *fact* that for any action as many "experts" could be obtained by one side as the other—provided enough money is paid for such *expert* testimony! Some will say the patient should have been notified of the danger; others, that he ought not. Some will say the chloroform killed him; others, not. For that reason he thought that before making any more laws regarding the proper administration of anæsthetics, those which already exist should be administered in a better manner; and physicians should be a little more careful how they administer anæsthetics. With all due respect to Dr. Finnell—although he had never held himself up as a physician—yet he did not think "sousing" a patient was quite the correct thing! Therefore he thought the need was not that there should be more law, but that there should be more care. That is the remedy; and it lies with the doctors themselves to correct the evil.

Mr. Eller referred to the popular fallacy that chloroform could be used successfully for the purpose of effecting robbery. He thought that that delusion ought to be dispelled; for the time between the actual administration of chloroform and the period of annihilation of sensation is sufficiently long to render the accomplishment of the object impossible. Such a plea is used by criminals to shield themselves from the consequences of their own crimes.

Dr. E. C. Mann, of this city, read a paper on Mental Responsibility, and on the Diagnosis of Insanity in criminal cases. The main points of the paper are the following:

At the present day medico-legal cases are becoming very frequent, in which it is necessary to ascertain as to the insanity of a person accused of a criminal act, in its relation to his civil capacity and responsibility for criminal actions, and also

as to feigned or concealed insanity. It becomes, therefore, a very interesting question as to what test of insanity the law should recognize as a valid defense in criminal cases. This question, although one which it seems difficult to settle satisfactorily, and which judges, lawyers and medical experts are constantly disputing, assumes every day greater interest and wider significance, owing to the increase of insanity in our country disproportionate to the increase of population, which has taken place during the past twenty years, and which will continue to take place. Without inserting dry statistics, it is sufficient to say that a comparison of the increase of population from 1850 to 1870, with the increase of the number of the insane during the same period, reveals an increase of insanity over that of population of about 12 per cent. In the foreign elements this is due to marked changes in the habit of living, the changes of food, increased intemperance, working more indoors, living in badly ventilated tenements, and disappointments in not succeeding in business, etc., as they had expected to do in America, which are causes, all of which combined, tend to impair health, break down the nervous system, and tend insensibly toward insanity in the offspring. The increase of insanity among our own population is due largely to a change from a vigorous, well-balanced organization, to an undue predominance of the nervous temperament, which is gradually taking place in successive generations. The educational pressure on the young to the neglect of physical exercise, the increasing artificial and unnatural habits of living, the great excitement and competition in business, are all tending to induce and multiply nervous diseases, many of which must terminate in insanity. These causes and the evils resulting from them, are propagated by the laws of inheritance in an aggravated and intensified form. Insanity is also appearing gradually at an earlier age than formerly. In former years the average period at which the greatest number became insane ranged between the ages of thirty and forty, but an analysis of statistics shows that this average age is coming on at an earlier period, generally appearing between the years of twenty and thirty. This is owing to hereditary influences, which have gradually become intensified by the violation of

physical laws during early life, want of proper training, or too high pressure in education, and is also due largely to the great mental activity and strain upon the nervous system that appertain to the present age and state of civilization, and which tend to a rapid decay of the nervous system. With many persons it is but a step from extreme nervous susceptibility to downright hysteria, and from that to overt insanity. The question of mental responsibility in its relation to criminal cases, is one of great interest, and presents a wide field for study and investigation. The facts of criminal psychology have led the writer to regard the impulse of criminal natures in the light of natural laws, and there is, beyond all doubt, an anthropological change which lies at the foundation of criminal propensities. There is a deficient cerebral organization which lies at the foundation of these criminal natures, which occasions the disposition to an abnormal moral constitution. The dislike of work and the love of enjoyment are impulses, which, when combined, lead especially to crime — when that other constitution or development is wanting which is necessary to the foundation of a powerful feeling of what is right. A further fundamental element, which stands in psycho-physical contrast to dislike of work, is an excessive physical consciousness of strength, which leads to arrogance, and thereby to the pleasure of measuring strength against the weak. This impulse leads to the love of bullying, cruelty and murder, if the higher intellect is absent which should turn the feeling of strength in a right direction, and there is also absent a complete ethical consciousness which should prevent misuse of power. This ethical weakness may be congenital, as has been remarked, or it may arise from deficient education. In the domain of vices we meet with, a peculiar condition of the central nervous system, which results in a temporary criminal impulse returning with a certain regularity. Such criminals are temporarily seized with the deepest remorse, and are fortified with best resolutions. They behave for a time in the most exemplary manner until they relapse again, which relapse is unanimously attributed by them to an irresistable impulse. This state of *moral epilepsy* is of great significance in the psychology of crime, as a physiologist is led to institute

a comparison between such cases and several states of disease, in which a peculiar type is observable, consisting in the fact that attacks of illness of more or less duration alternate with more or less long, and generally for a time preponderant, healthy intermissions. In a broad sense, one may designate all these pathological states as *epileptiform*, hence the term "*Moral Epilepsy*," which has been adopted above. Leaving this interesting question of the psychology of crime, to proceed with the diagnosis of insanity in criminals, we would ask if the true basis for jurists to proceed upon is not *the protection of the existence of normal persons against the ethically degenerate?* And the necessary degree of this protection is, most certainly, an essential measure for the severity of the punishment. The first trial of note where there was the question of insanity advanced, was in 1723, when the trial of Arnold for shooting at Lord Onslow occurred. Although it was shown that Arnold had been of weak understanding from his birth, and that he was doubtless insane, the jury brought in a verdict of guilty, and Arnold would have been executed had it not been for the intercession of Lord Onslow. The language of the charge to the jury in this case was in conformity to the rule laid down by Lord Hale, that partial insanity does not excuse a person from the consequence of his act, and that only a total deprivation of reason can furnish such excuse. In the year 1800 the celebrated trial of Hatfield, for shooting at the king in Drury Lane Theatre, excited much interest. Although it was proved that in 1793 Hatfield, who was a dragoon, had received a number of severe wounds which had caused partial insanity, so that he was dismissed from the service, and that since that time he had had periodic attacks of insanity, and had been confined as a lunatic, the prosecuting attorney laid down the established rule, that a total absence of memory and understanding could alone shield the prisoner from punishment, and appealed to the jury for a conviction on that ground. It was only through the brilliancy of the advocate, (afterward Lord Erskine) that the prisoner was acquitted. This trial had a good effect upon the judiciary, as in the year 1812, in the trial of Bellingham for the murder of Spencer Percival, Lord Mansfield laid down the law that the

capability of distinguishing between right and wrong was the test for determining the prisoner's responsibility, thus discarding the old theory of an entire absence of all mental power and substituting this in its place. Afterward the theory of a general knowledge between right and wrong was modified, and the element introduced that the prisoner must know the difference between right and wrong at the time of, and with regard to the particular act for which he is on trial, in order to render him responsible, and this test has been preserved to the present time. In the early history of our own country the same barbarism in the treatment of the insane prevailed which darkens the pages of English history. In Gov. Winthrop's History of New England, the case of Dorothy Dalbye is mentioned. She was executed for killing her child. She was, beyond all doubt, an insane woman, but this fact was not recognized by Gov. Winthrop, who says of her that "she was so possessed with Satan that he persuaded her by his delusions, which she listened to as revelations from God, to break the neck of her own child that she might free it from future misery." Such was the ignorance and prejudice of the early history of our country. We are at the present day very far from a correct understanding of the workings of the insane mind, for in the recent trial of Scannall the law was laid down as enunciated by the Court of Appeals in 1865 in case of Willis v. The People, which held that a person was not insane who knew right from wrong, and that the act he was committing was a violation of law, and wrong in itself. This theory of right and wrong is utterly inadequate to meet a large class of cases. There are certain cases familiar to all specialists in insanity which suffer from impulse in insanity with a homocidal or suicidal monomania. These patients, without appreciable disorder of the intellect, are impelled by a terrible *vis a tergo*, a morbid, uncontrollable impulse, to desperate acts of suicide or homicide. These patients are often fully aware of their morbid state, appreciate perfectly the nature of the act towards which they are impelled, and feel deeply the horror of their situation, and yet if not prevented by restraint, will inevitably committ acts of suicide or homicide. A very remarkable case was under the care of the

writer, of a man who would at stated times acknowledge that he felt an irresistible desire to kill some one, and would voluntarily enter an asylum and remain there until this morbid impulse had passed away, which was generally a period of one or two months. He has often told the writer that his life was made miserable by the idea that at some time this overwhelming impulse would come upon him so suddenly that he should commit some desperate homicidal act, but is not prepared to voluntarily incarcerate himself in an asylum for life, as his lucid intervals sometimes lasted for months at a time. The law as laid down at present would not decide this man to be insane, as he fully appreciates the difference between right and wrong, and the nature and consequences of any homicidal act that he may in the future commit. Such cases, which are not at all uncommon, serve to show what fearful injustice may be done under the name of justice, when the conclusion is based upon a metaphysical test which is proved by medical observation to be false in its application to the unsound mind.

There is still another form of insanity denominated "Moral Insanity," in which the intellectual faculties are intact, no delusions or hallucinations existing, but where the moral sense seems utterly obliterated. Such persons have no true moral feeling. This is disorder of the mind produced by disease of the brain, and is an unquestionable form of insanity, as it often precedes other forms of insanity, in which intellectual derangement is well marked, as acute mania or general paralysis. In some of these cases there is a modified responsibility, the degree of such responsibility being determined by the particular circumstances of each individual case. One difficult but important question to be solved is, as is the civil and criminal responsibility of women who plead insanity before courts of justice, and who are often afflicted with kleptomania, pyromania, or who are infanticides, as a result of sexual trouble and disease of the pelvic organs. Such women under all reasonable conditions are entitled to the benefit of the doubt, because of their defective mental integrity, caused perhaps by pregnancy, or by the subsequent emotional excitement attending parturition, which intensifies the cerebral disorder in a brain already morbidly active. With women, extreme nervous sus-

ceptibility readily lapses into insanity. In the sexual evolution, in the parturient period, in lactation, strange thoughts, extraordinary feelings, unreasonable appetites, criminal and suicidal impulses, may haunt a mind at other times innocent and pure. It is probable also that young unmarried women guilty of killing their own new-born offspring, are so distracted by conflicting feelings, sharpened to morbid acuteness by the great physiological movement of parturition, as to be hardly responsible for their acts. We come now to the question of the diagnosis of insanity. In most diseases we examine physical signs and symptoms, and determine by our senses the existence of such diseases. In insanity, on the contrary, we have to be guided chiefly by our knowledge of the normal functions of the mind, and in an examination have to rely on our intellect, rather than on our senses; although, of course, the latter are called in to assist us. It is, however, very often extremely difficult to decide with certainty, as medical experts are expected to do, as to the existence of mental disease. In making an examination of a person accused of crime, and in whom insanity is suspected, the person should be visited by the medical examiner, who should draw him into a pleasant conversation, and inquire as to previous attacks of insanity, hereditary history, then into any predisposing causes of insanity, such as intemperance, vocation, habit, etc., which may have operated in the production of insanity. Also as to injuries of the head or spine, which may have occurred, sunstroke, etc. The nervous system should then be examined for the existence of any such diseases as paralysis, epilepsy, catalepsy, or hysteria. The different senses beginning with sight should also be examined, and in this way it may be discovered if there are hallucinations or illusions pertaining to any of the senses. A great many cases are on the border line which separates sanity from insanity, and it often requires the nicest discrimination to determine whether such a patient has passed this border line. The writer would suggest a series of eight questions, which, if adopted by jurists in criminal cases, would prove a most efficient and just test as to the existence of insanity in any given case, viz.:

1. Have the prisoner's volitions, impulses, or acts been de-

termined or influenced at all by insanity, and are his mental functions—thought, feeling and action, so deranged, either together or separately, as to incapacitate him for the relations of life?

2. Does the prisoner come of a stock whose nervous constitution has been vitiated by some defect or ailment calculated to impair its efficiency or derange its operations?

3. Has the prisoner been noticed to display mental infirmities or peculiarities which were due either to hereditary transmission or present mental derangement?

4. Has the prisoner the ability to control mental action, or has he not sufficient mental power to control the sudden impulses of his disordered mind, and does he act under the blind influence of evil impulses which he can neither regulate nor control?

5. Has the act been influenced *at all* by hereditary taint which has become intensified, so that the morbid element has become quickened into overpowering activity, and so that the moral senses have been overborne by the superior force derived from disease?

6. Was the act affected by, or the product of insane delusion?

7. Was the act performed without adequate incentive or motive?

8. Does the prisoner manifest excitement or depression, moody, difficult temper, extraordinary proneness to jealousy and suspicion, a habit of unseasonably disregarding ordinary ways, customs and observances, an habitual extravagance of thought and feeling, an inability to appreciate nice moral distinctions, and finally, does he give way to gusts of passion and reckless indulgence of appetite?

Some, or all of these are found, generally, in connection with transmitted mental infirmity. It may be argued that these mental defects signify not mental unsoundness, but human imperfection. Certainly if we take these manifestations, any one of them singly and alone, we cannot claim such a one as invariably an indication of insanity, but, on the other hand, under certain circumstances, each one of them may be an unmistakable sign of insanity, or rather of a morbid cerebral

state, which may readily lapse into insanity. The disappointments and calamities of life obviously act with greater effect upon an unstable mental organization, these causes of disturbance meeting with a powerful, co-operating cause in the constitutional predisposition. Sometimes a crime, even when there have been no previous symptoms to indicate disease, marks the period when an insane tendency has passed into actual insanity—when a weak organ has given way under the strain put upon it. There is a class of persons with a peculiar, nervous temperament, who inhabit the border land between crime and insanity, one portion of which exhibit some insanity, but more of vice, and the other portion of which exhibit some vice, but a preponderance of insanity, and it is very difficult to form a just estimate of the moral responsibility of such persons, especially when we reflect upon the fact that moral feeling is a function of organization, and is as essentially dependent upon the integrity of that part of the nervous system which ministers to its manifestations, as is any other display of mental function. The writer has met with cases in which, as a result of parental insanity, there has been a seemingly complete absence of moral sense and feeling in the offspring, and this has been a true congenital deprivation, or a moral imbecility, so to speak; of course such children can hardly fail to become criminals. In this connection it is interesting to note that moral degeneracy often follows as a sequence upon disease or injury to the brain. A severe attack of insanity sometimes produces the same effect, the intellectual faculties remaining as acute as ever, while the moral sense becomes obliterated.

When such persons are acquitted on trial of a criminal act on the ground of insanity, they should be remanded to medical custody, and should never be set at liberty until the medical superintendent of asylums deem them fully recovered; but the commonest justice plainly indicates that such custodial restraint be of a medical and not of a penal nature. It is a very difficult thing for the laity to realize how sane a person may appear, who all the while has a greater derangement than was even suspected until something happens to elicit the evidence of it, such as an attack of illness or severe mental strain, and some unconquerable impulse seizes him, and some homi-

cidal or suicidal act results to the great surprise of every one. In the same manner inebriety often appears in maturity as a result of ill health, mental shock, etc., and it becomes an interesting question as to the degree of moral and criminal responsibility which attaches to inebriates, as inebriety often depends upon an abnormal organic development of the nervous system that has descended from generation to generation gaining in intensity until it manifests itself in active inebriety, and there must certainly be a modified responsibility when homicidal or suicidal acts are committed during periods of such abnormal cerebration. In such cases a criminal act may be committed in consequence of cerebro-mental disease, without any apparent lesion of the perceptive and reasoning powers. In these cases also, the mental disorder is of a sudden and transitory character, not preceded by any symptoms calculated to excite suspicion of insanity. It is a transitory mania, or sudden paroxysm, without antecedent manifestation, the duration of the morbid state being short and the cessation sudden. In these cases the criminal acts are generally monstrous, unpremeditated, motiveless, and entirely out of keeping, with the previous character and habit of thought of the individual. Such attacks are transient in proportion to their violence, and transition occurs in the completion of the act of violence. There is an instantaneous abeyance of judgment and reason during which period the person is actuated by mad and unconquerable impulses.

We will consider, finally, the medico-legal importance of epileptiform attacks, which may be partial in character, and which may not reach convulsive activity except so far as the mind is concerned. These attacks always display periodicity, and after the paroxysm there is an intermediate stage, during which, in most cases, the person remains in a confused state, perhaps for some hours, and is apt subsequently to retain only a vague and general notion of the preceding events. Thus in a homicide by shooting, the murderer would be likely to be roused by the sound of the pistol shot, and to remember it, although he would not very likely remember the altercation at all, or what passed between them. A case occurred recently of considerable interest from a medico-legal point of

view, in which a murder was committed during an epileptiform seizure, or rather, more strictly speaking, during a state of transitory moral epilepsy, which was the result of a previous sun-stroke, the immediate exciting cause being an attack of illness and the taking of a small quantity of alcoholic stimulus, which, it is well known, acts as a poison upon persons who have been sun-struck. This state of "moral epilepsy" is a morbid affection of the mind centres, which destroys the healthy co-ordination of ideas, and occasions a spasmodic or convulsive mental action. The will cannot always restrain, however much it may strive to do so, a morbid idea which has reached a convulsive activity, although there may be all the while a clear consciousness of its morbid nature. The case just referred to had complained of pains in the head and sleeplessness, which had displayed marked periodicity, and which had been accompanied with great irritability of temper, excited by trifles and seemingly unconnected with personal antipathies. As has been previously stated, the person alluded to had been suffering from quite a severe illness, and after taking a small quantity of alcoholic stimulus went out to walk. He met a friend with whom he had been familiar for years, and a discussion arose as to the respective merits of certain politicians, when the discussion becoming excited, the man pulled out a revolver and shot his friend. He then went in a confused and dazed state and sat for some hours on a dock near a river, and subsequently went home, and burst into tears and informed his wife of the sad occurrence, and gave himself up at the police station. There was no simulation of insanity by pretending to be incoherent, or by strange actions, and no attempt, either on the part of himself or wife, to pretend that the act was an insane one. There was, however, a total blank in the prisoner's mind respecting the events immediately preceding the pistol shot, which seemed to have aroused his attention at the time, and he had no recollection of the fact that he had sat on the dock for some time afterward, as he was seen to do. The writer was consulted as an expert, and upon ascertaining the prisoner's previous history, gave it as his opinion that there had existed for months previous to the occurrence a profound moral or affective derangement, which, from its marked peri-

odicity, was evidently epileptiform in character, and that the sudden homicidal outburst supplied the interpretation of the previously obscure attacks of recurrent derangement. There had evidently been induced by the sun-stroke in this case, an epileptiform neurosis, which had been manifesting itself for months, chiefly by irritability, suspicion, moroseness, and perversion of character, with periodic exacerbations of excitement, all foreign to the man previous to the attack of sun-stroke. It is well known among specialists in insanity that this epileptiform neurosis often exists for a long time in an undeveloped or marked form, and that this neurosis is, moreover, connected with both homicidal and suicidal mania. Such attacks are often noticed to occur periodically for some time before the access of genuine epilepsy. I have often witnessed, in cases under my charge, abortive or incomplete epileptiform attacks, where there were no convulsions, and where there was no complete loss of consciousness. I have noticed in such cases either a momentary terror, slight incoherence, a gust of passion, or a mental blank, the patient perhaps stopping in the middle of a sentence. The patient would then be himself again, quite unconscious of what had happened to him. Accompanying this confusion of ideas, may be, as I have remarked, instantaneous impulses, either of a suicidal or homicidal nature. Owing to the writings of Hughlings Jackson, Maudsley, Russell Reynolds, Hammond, Trousseau, Falret, Esquirol and others, epileptic vertigo is a recognized disease. There is abundant testimony to show that during such seizures persons may perform actions, and even speak and answer questions, automatically. There are numerous examples in the works of the above authors, proving that in an unconscious condition, persons can progress from odd or eccentric actions, to deeds of violence, suicide or murder—being unable to remember the circumstances afterwards, and therefore irresponsible for their actions. This class of patients I have always found irritable, easily excited, very emotional without adequate external causes, easily losing their train of thought, and often unable to collect or fix their thoughts. Such cases have told me that they felt themselves changed in character, and have acknowledged that they often felt impelled to strange and violent acts, by some

power which they could neither understand nor resist. Such patients may entertain delusions of fear and persecution, and commit criminal deeds as a result of such delusions. When such cases, in their terror or distress of mind, commit some violent deed, they either experience immediate relief, as was the case with one patient under my care, who was only relieved by suddenly breaking out a pane of glass, when his paroxysm would subside, or they continued in a state of excitement unconscious, or very imperfectly conscious, of the gravity of their acts. When they become conscious again, their memory is apt to be very uncertain as to preceding events. Griesinger says: "Individuals hitherto perfectly sane and in the full possession of their intellects, are suddenly and without any assignable cause, seized with the most anxious and painful emotions, and with a homicidal impulse as inexplicable to themselves as to others." Maudsley says: "Let it be borne in mind, then, that there are latent tendencies to insanity which may not discover the least overt evidence of their existence, except under the strain of a great calamity, or of some bodily disorder, and that the outbreak of actual disease may then be the first positive symptom of unsoundness." The question as to the degree of mental responsibility attaching to such cases is one of great interest to psychologists, and also to jurists, and one to which it is hoped, in the future, much more attention may be directed, than has been given to it in the past."

Mr. Cephas Brainerd, of New York, read a paper on some points touching law and medicine. I give you a full synopsis. He said:

The compliment involved in the invitation to read a paper before this society, which has been extended to me, must have had its origin in the kindly impulse of some friend; certainly not in the supposition that I possess any power to add to your stock of knowledge by the presentation of any acquisitions of my own, or by any process of original reasoning to throw light on the problems of society as discussed here.

An examination of the elaborate and able essays, already printed by members and friends of the Society, would quickly dispel any notions which a writer might entertain at the out-

set, of his ability to impress you with a display of his own knowledge and research.

At best I am but an "apprentice," and here, as yet, I am but a looker-on. My connection with the Society has been all too brief to make me anything but that. Suffer me then in that capacity to submit a suggestion or two of the most simple character, springing from my limited acquaintance with your work and its objects.

Two learned professions meet with common objects; the two professions whose daily service comes most nearly home to the business and bosoms of all men in the routine of common life. Surely the object of this Association of the two professions is not solely the simple but comparatively selfish one of acquiring a given quantum of knowledge, as the miser accumulates gold. Rather this is a branch or section of the great, the universal social science association of civilized lands, composed of the members of the two great and learned professions whose lines of labor most frequently touch each other as regularly pursued. Its past discussions show it to be an advocate of what is sometimes called a social science policy. This is well defined in one of the most remarkable of recent books, the letters and other writings of Edward Denison, M. P. He says (p. 206): "It aims at utilizing, for the purposes of imperial, national, municipal, and individual life, the great stores of knowledge in every department of philosophy and science, which the mental activity of the last half century has created and accumulated, but which have not yet been employed to diminish the suffering and increase the happiness of humanity at large." To this end how advantageous is the calm and dispassionate comparison of views between the two professions meeting in this Society. How essential their substantial agreement upon topics common to both, before the great stores of knowledge adverted to by Mr. Denison are really employed to diminish suffering and increase happiness. To be most usefully employed there must be an incorporation of the results of these accumulations in legislative action, and in the common practices of mankind. The largest possible circulation of the topical papers read here contributes powerfully and directly to the latter result, but only moderately, and I think

inconsiderably to the former. So far as my examination has extended, very little has been done or said here looking to practical legislation. Not that I design to suggest that the Society engage in the business of procuring legislation generally, though no reason has occurred to me why whithin reasonable limits it might not properly do so.

The time seems peculiarly favorable for obtaining definite legislative action guaranteeing these reforms in the law which the great advances in practical knowledge show to be desirable.

The limitations which some believed were set upon legislation, and which hedged about supposed rights are yielding to the needs of the time. This change in judicial view, and rule began some years since, but has recently become a settled principle of judicial action. Perhaps the strongest early manifestation appeared in the police cases as long ago as 1857 (*People v. Draper* 15 *N. Y. R.* 532) when our police was upon a better and more efficient footing.

It appeared again in 1865, when an attempt was made to overthrow the law which gives to parties whose property is destroyed by mobs, a right of action against the municipality where mobs hold sway. (*Darlington v. the Mayor* 31 *N. Y. R.* 165) All remember the strenuous opposition, founded on constitutional objections, that was made to the health legislation affecting the city (*See Met. Bd. of Health v. Heister.* 37 *N. Y. R.* 661, *and kindred cases*). Notwithstanding this opposition we have the improved health code. Cattle are no longer driven in large droves through the crowded streets, and slaughter houses have ceased to infect, infest and demoralize the town. The demands of the body of the people for improved modes of travel, as incorporated in legislative action, and sought to be satisfied by capitalists under legislative permission are no longer thwarted by claims of supposed private right. Such technical claims our courts hold are not to stand in the way of these improvements which the age really requires (*Rapid transit cases; not yet reported*). The disposition manifested by our Court of Appeals appears in the highest court in the nation. The legislative wisdom of some states provided, as was thought, for a better use of railroads in

the interest of all parties. Individual right again raised constitutional objections, and the Supreme Court in the Granger cases (94 *U. S. R.* 113-187) recognized and sustained the right of the body of the people to some consideration in the administration of *quasi* public trusts.

Some of these decisions may yet be subjected to criticism, but they will undoubtedly all stand the test of time, and trial, and they show that the current of judicial ruling in this country is contrary to the technical spirit, the over-squeamish regard for supposed private right which lies at the basis of the Wynhamer case here (3 *Kernans R.* 278) and *Alkyns v. Randolph* 31 *Vermont R.* 226) in the State of Vermont, which are the very quintessence of the chronic conservatism of the past.

It is doubtless safe to assume that this is not formed as a total abstinence society; that it was not organized for the purpose of joining in any temperance crusade. An examination of your records will show, however, that a considerable proportion of the papers read here, have treated of the effects of the use of alcohol upon men. A cursory examination of these papers has not disclosed to me any observation upon this use from the philanthropic stand-point, or any appeal to the class of considerations most in use among so-called reformers. The treatment has been wholly scientific or professional. The essays treat of "Alcoholism," "Methomania," "Dipsomania" as diseases, as forms of insanity.

Of "Methomania," one of your essayists says that it "is a manifestation of brain disease, and that brain disease involves general impairment of the mental faculties, and consequently a form of insanity. Any man drunk and absent from business that he knows to be important, committing a crime in that condition, should be treated not as a criminal but as a maniac."

Another of your essayists says: "In a criminal case the question must be carefully examined, for if the guilty party has had attacks of delirium tremens, if it is ascertained that he is a dipsomaniac, he ought to be confined in an asylum as any other insane, and no matter how well he may appear to be while confined, it is not till after a long time, probably ten years, before he can renounce spirits, or that he may be set at

liberty without danger to himself or to society. No matter how much he might promise, swear that he would not touch another glass, the inclination before that time is stronger than he; he cannot resist it as long as he can procure it."

The same essayist sets forth the results, near and more remote, of the use of alcohol as follows:

"Now in ninety cases out of a hundred, drunkenness is first contracted by invitation and politeness, for it is considered very impolite by some individuals to refuse to drink when invited to 'join in.' It soon becomes a habit, and then constitutes one of the most incurable diseases. Every additional glass is one more stitch in that other Nessus tunic, called chronic alcoholism, from which, when once entangled in its folds, it is impossible to come out, and Hercules-like, the drunkard dies in the most wretched agony.

The disease is not even ended by his death, but its influence extends from generation to generation, until the extinction of his race. Morel reports many examples of this fact. According to him the sequence is as follows:

First generation: Immorality, depravity, excess in the use of alcoholic liquors, moral debasement.

Second generation: Hereditary drunkenness, paroxysms of mania, general paralysis.

Third generation: Sobriety, hypochondria, melancholy, systematic ideas of being persecuted, homicidal tendency.

Fourth generation: Intelligence slightly developed, first accession of mania at sixteen years of age, stupidity, subsequent idiocy, and probably extinction of the family.

But quotations need not be multiplied. Putting aside all moral questions, all considerations philanthropic in their character, it has been shown by the essays before this Society, as it has been shown elsewhere, that certain uses of alcohol touch the civil administration, and the practice of the law at a great variety of points, but strikingly at the following:

1. As to testamentary capacity.
2. As to fitness to give evidence.
3. As to competency to contract obligations of a binding character.
4. As to general capacity for wise business management.

5. As to criminal tendencies.
6. As to responsibility for acts of a criminal character.
7. As productive of poverty and pauperism.

And as to all this matter the medical profession has brought from its store-house of facts and experience a vast array of information, and resting upon this a mass of professional conclusion which cannot be controverted or denied.

It may well be assumed that their call now is to the legal profession for the payment of that debt which Lord Bacon declares every one of its votaries owe to it, viz., for the intervention of the lawyer in aid of every proper movement for the modification or the cure of so great an evil. We are ready to take a position in regard to the proposed changes affecting the minutest details of the daily practice. We are not slow to discuss the question whether a summons signed by an attorney, or a writ in the name of the people, is the proper and convenient mode for instituting a suit, and rightly so. If any craft known to civilized society, throughout its long and continuously brightening history, has been ready with all its forces, natural and acquired, to serve the cause of progress, that craft is the law, and ready it has ever been to enter that service with neither staff nor scrip, nor money in its purse from those it thus serves. Surely the law should not be behind-hand in efforts to secure a mitigation, if not the eradication, of this curse upon civil and governmental administration.

Not as reformers, not as temperance men in the technical and narrow sense in which these words are used in every-day life, but upon the same principle, and in the same way and with the same ardor with which we enter upon the advocacy of any great reform in civil affairs. How many times have we all draughted and followed them through the legislature, at our own charges. How many times in the interest of society, but at our own cost, have we all taken part in efforts to procure what we thought good legislation, or to defeat bad.

Yet here is a practice which in its effects touches the lawyer more nearly than the members of any other profession save that of medicine. That profession has brought here the wealth of its learning, and laid it before the Society; it has done at least a part of its duty, but I have not observed in any of our

papers from my own profession any suggestions for a legislative scheme to cure the evil, to remove the wrong as touching the administration of the law. It seems to me this can properly be expected, not in any radical or ultra reformatory shape, but as digested by the calm reflection, matured by the large wisdom and experience, and sustained and approved by what Mr. Choate in perhaps the best of his public addresses termed "The Conservative Force of the American Bar."

It is not for me to do this, but it may not be improper to suggest the question whether any idea has been yet presented better than that which gives the law an executory principle by conferring a right of action in favor of the person injured by the acts or neglects of the drunkard against the seller of the liquor used by him. May I also suggest a radical inquiry as to the difference discoverable upon principles of statesmanship and constitutional law between the common bar-room and the gambling hell, or between the sale of liquor in the former and the sale of prussic acid and arsenic at the drug store? And if no difference can be discovered, why is not properly the same kind of legislation applicable to all?

Some suggestions made here looking to the subject of ventilation, as applied to public school buildings, indicate the prevalence of a sentiment among the members of the Society which perhaps justifies the additional observations which I desire to make. The members of the medical profession more than any other calling are brought into immediate contact with the poorer classes. Sore sickness is incident to every family, rich or poor, and perhaps especially incident to the destitute and ignorant; and in the ministrations of the "Doctor" it is a peculiar glory of the profession that none are neglected. On such occasions the physician sees the best side of human nature, the best feelings of the heart are aroused, and it is easy to infer from the condition of mind as exhibited by the lowest classes in their afflictions, the capacity for improvement and progress of which they are capable. No one doubts the civilizing effects of pure air and tidy, even if not artistic, surroundings. If any proof were necessary, the extraordinary experiment of Miss Octavia Hill, in which she was assisted notably by Mr. Ruskin, which resulted in the purification of some of

the worst and most debased dwellings in London and in the real civilization of their inhabitants, and which transformed those dwellings into interest-paying investments, when before they had been a source of loss, is sufficient proof. But while it seems to me that the medical profession, in and of itself, cannot accomplish the needed reforms in the crowded tenements, it can do so with the help of the lawyer if he will wisely use his means for influencing and framing legislation, his opportunities of touching public sentiment at so many points, and especially the frequent calls upon him by capitalists for advice in regard to the mode of treating property and disposing of investments. The two professions co-operating can accomplish almost any desirable result in this direction. Is it not then proper matter for consideration how much by legislation and how much by operation upon public sentiment can be done toward improving the dwellings of the poorest, and thus facilitating the practice of the physician among the destitute classes, and rendering his generous service more useful? Attention has not been given to the management of the commonest tenement houses as it should be; effort is not made to keep them cleanly and to keep their inhabitants in the same condition; how much can the Medico-Legal Society do in this direction?

But there is another matter connected with this suggestion as to which there should be legislation at once. In the opening of the upper part of the city for dwellings by the building of the elevated railroads, and in the desertion of a section of the city about the Battery by the more profitable classes of business, a field is opened for a reform which it is desirable should be accomplished.

No houses are now built, of a convenient character, which are within the reach of single families of laborers and artisans of the better class, if they are now to live anywhere in the city it must be in tenements more or less crowded. There is no reason why the sections of the city which I have specified should not be re-distributed or re-laid out so that small houses can be conveniently built having abundance of light and air, which, in their rentals would be within the reach of the average mechanic for his single family. The advantage to civili-

zation—to good neighborhood—to health—to every department of social economy—secured by such a reform, would be incalculable.

Let another matter be brought to your attention. It is perhaps mainly to be touched by public sentiment; but, in that regard, public sentiment should be educated. The police courts and the justices' courts bring the civil institution, we call the State, into immediate contact with the largest body of our people—those the least educated—those who know least of the principles upon which a state is organized. Their knowledge of the law, of its administration, and its practice, is derived almost wholly from these courts. If they are to respect the institutions of the country, they are to learn to respect them from the administration of law as seen by them. They never see, except at rare intervals, and in a casual way, the higher courts of the country. They have no more idea of the appearance of the Supreme Court of the United States, of the dignity of its Justices, the simple majesty of its proceedings, the solemnity of its debates, and the integrity which characterizes it, than they may have of the local disadvantages as a place of human habitation of the moon. All they know of the law, they know from the police and civil justice, and from the lawyer who practices before him.

Then I say, with the educated classes, and especially with the members of these two professions, one engaged in the administration of law, and the other dealing with these poorer classes to such a vast extent under circumstances when they are most susceptible, there should be a decided, a positive, a persistent effort to elevate the condition of these courts. I make no criticism upon any man now presiding in them, but I do say that they should be conducted with a dignity, with a sobriety, with a steady reference to the best principles of the law and to the best principles of administration such as is not to be found in all of them at the present time. The judge upon the bench should understand that he represents the State in one of its highest and most important forms of legal administration.

The controversies of the poor about a few dollars are more important to them than the controversies of the rich over

their thousands, and their controversies should be treated with the same consideration, with the same gravity with which the Chief Justice of the Supreme Court treats a controversy before him. If this were done, these courts as a means of elevating society, and of improving morals, of keeping peace, would be of incalculable value. Our duty here is, by the creation and cultivation of a proper public sentiment, to so elevate and to ennoble them that they become constant and healthful teachers. A justice in one of these courts, whose conduct is what I have indicated it should be, ought to be looked upon by society as one of its greatest benefactors. He should not be spoken of with contempt. His education, his conduct, his surroundings, should be such that he can be welcomed to any society which he desired to enter. The terms "Civil" and "Police" Justice should not be on the lips of certain of our people, terms of contempt, but they should be an epithet of honor, and when we come to that we shall have made a long step onward in the elevation of man.

This is a little outside of the ordinary line of your essays: it will not, however, do hurt, possibly it may be of some little good.

Law and medicine in their administration deal with individuals. The lawyer acquires a profound appreciation of the value of individual and personal right. The physician has an exalted estimate of the single man,—not only as possessed of mental capacity—of an immortal soul—but as possessed of a body "fearfully and wonderfully made" which is the subject of the profoundest study on the part of those who desire to be worthy of their calling. It is the valuation in both professions of the individual man which gives life and force and moral elevation to their activity and which invokes the exercise of their best faculties. What I have said touches the value of the individual and goes to its just appreciation, and I would fain see realized by all the force of the promise and declaration of God Himself, when He says, "I will make man more precious than gold—even a man, than the golden wedge of Ophir."

NEUROLOGICAL SOCIETY.

The following cases have been reported to the Society:

By Dr. E. C. Seguin: Bulbar Paralysis.

(A typical case of labio-glosso-pharyngeal paralysis.)

Male, aged 67. Patient of Dr. McCready, seen in consultation Sept. 25th, 1876. He was a steady drinker. Probably no syphilis. At least a year ago great difficulty in swallowing began, especially of liquids, and it steadily increased. In the winter of 1875-6 Dr. McCready was suddenly called, and found patient in extreme and dangerous orthopnœa, without cardiac or pulmonary cause. The respiration was of the Stokes-Cheyne type—a few rapid acts of breathing succeeded by a long pause, with shallow and short inspirations. Ever since, breathing has been more or less of this type. Children have noticed staggering gait and stooping in the last few months, and in the same period a degree of imperfection in articulation has shown itself.

On Sept. 19, partial right hemiplegia without loss of consciousness. Dr. McCready saw patient shortly afterward, and made sure that although the power of articulation was nearly abolished, there was no aphasia. After this attack the breathing became much more difficult, and deglutition has been nearly abolished; almost nothing being swallowed until to-night. Food has been administered per rectum.

I found the patient in a state of semi-stupor, from which he could be roused. The pupils were normal; expression dull; lower part of face expressionless. Mouth hangs half open, and its right corner and the cheek drops. The breathing is shallow and rapid for a few seconds, then almost imperceptible (Stokes-Cheyne type). The grasp of both hands is weak, that of right hand weakest. In attempting to talk patient makes great effort, but the sounds are almost inarticulate and guttural. He names daughters, days of week, etc., and seems not to be at all aphasic. Labial sounds are best made. Cannot distend cheeks with air or whistle, but purses lips fairly well. Tongue is only partly projected; it shows neither atrophy nor tremor.

Autopsy showed uniform atrophy of cerebrum, sub-arachnoid effusion, *etat criblé* in extreme degree. No other lesion appreciable to naked eye.

The following case of epithelioma (colesteoma) of the cerebral dura mater was presented by Dr. J. C. Shaw, of Brooklyn, N. Y. The lady from which these specimens were taken was a patient of Dr. Mathewson's. I made the post-mortem. The history of the case I shall give briefly, and only such points as are of interest to us, as the case will be reported at full length elsewhere, by Dr. Mathewson.

Mrs. C., aged 27, married. Was seen first Nov. 16, 1876, having had suppurative inflammation of middle ear for years; for past few days has had pain in left ear, and examination shows hard whitish polypi projecting through perforation in the drum membrane, otherwise condition good. The pain and discharge continued during the winter. On March 18, 1877, she complained of pain in the occipital region. There was no swelling or tenderness over mastoid. She was seen in consultation by Dr. Roosa, who thought there was no evidence of disease of mastoid. After a short time, Dr. Mathewson performed Wild's incision, hoping that it might relieve the pain, as the patient was now becoming much emaciated from her sufferings. To his astonishment, he found a very large hole in the mastoid, which contained pus and broken-down bone. She went on in this condition until she died—complaining, for many weeks before death, of diffuse intense headache, and had to be kept continually under the influence of morphia. She presented no cerebral symptoms other than the headache.

In removing the brain, there was seen projecting from the floor of the skull, just over the petrous portion of the temporal bone on right side, a small whitish tumor about the size of a hazel nut, and which felt soft; it made a depression on the under surface of the temporo-sphenoidal lobe. The internal surface of the drum covering the whole of the temporal bone, extending back to the occipital and down into the foramen magnum, and over the wing of the sphenoid and sella turcica presented an opaque, dull, soaked appearance, and was somewhat detached from the bone.

On removing that part of the dura covering the temporal bone, it was found that the whole mastoid and petrous portion

of the temporal bone was gone, and the removed specimen was covered with a lot of offensively smelling pus.

Let me premise by saying that this work was done by me in the Neurological Department of the School of Histology in this city, under the direction of our president.

The points of interest connected with this case are:

What was the starting point of this growth? How is it we had no neuralgia (of the facial branches) of the fifth nerve? Why had we not those grouping of symptoms which we designate as Menière's disease?

In answer to the first of these questions, it appears to me that the growth must have had its origin in the middle ear, and extended to the dura, and as it progressed and involved the dura lining the mastoid, cutting off its nutrition and causing slow death of the bone, and this appears to me to be the reason why the ordinary symptoms of inflammation of the mastoid were not present.

Dr. E. C. Spitzka presented the following specimens illustrative of general paresis of the insane:

The Doctor referred to the fact that he had offered a contribution to the study of general paresis, at the February meeting, and stated that as on that occasion he merely endeavored to establish a parallelism between lesions and symptoms, he had omitted giving the minute histology of the changes in this disease. It was his purpose to fill up the gap this evening. He began by asserting that his researches permitted him to give a patho-anatomical definition of the disease, he would term it a *progressive periencephalitis* with an inconstant factor of *diffuse leptomeningitis*, and an unessential one of *pachymeningitis*. It was not a gradually established and progressive process, but was characterized by a series of fluxionary hyperæmias, occuring at intervals, becoming more and more aggravated, until arterial and capillary stases of an intense degree were produced at each attack, until at length, this vasenlar change reached a degree comparable to a capillary apoplexy, which usually terminated the history of the case, unless intercurrent affections intervened. Furthermore, as this hyperæmia was provoked by a paralysis of the muscular coat of the vessels, however evan-

escent in character, he would term it a vaso-motor affection. Specimens were exhibited from a patient, who had died at the height of a maniacal attack, in incipient paresis. The vessels were filled to distension with blood discs, whose outline was clearly distinguishable. The adventitia and contignous neuroglia was infiltrated with a material, whose exact chemical nature was not ascertained, but which stained with a beautiful pink flush in carmine, the infiltration was diffuse, and not sharply demarcated, protagon spheres were found in the adventitia, and the diffuse infiltration was probably a diffusion of protagon, or of a derivative of protagon. Similar spheres could often be found in the pericellular spaces around the large pyramidal nerve cells, and they were supposed by him to be the result of retarded lymph flow, and to be organic precipitates from the products of tissue metamorphosis.

A second series of sections and freshly prepared specimens, from patients further advanced in the disease, showed how the corpuscles had become fused into an opalescent mass, staining deeply in carmine and hæmatoxylin, occasionally a whole capillary district was found the seat of this stasis, and in others, the mass was seen undergoing separation, into oval, cylindrical, or round fragments, which, as they floated on in the channel, became impacted at the bifurcation of vessels, to undergo still further subdivision, until they were divided into fragments sometimes smaller than a red blood-corpuscle. Dr. Spitzka had never observed these masses to form in vessels of a larger calibre, than those vertical arterioles, which enter the cortex from the pia mater. He hence deprecated terming the masses seen in the stage of resolution, "emboli." Rarely did they produce obliteration of a vascular area, unless secondary changes in the vascular walls (which invariably took place latterly) occurred. Where obliteration of an arteriole could be demonstrated, (usually at the depth of the sulci) the ganglionic cells were found granular, atrophic, or even diminished in number, their place being occupied by lymphoid or neuroglia corpuscles. This condition of the blood was a true stasis. The agglutination of the blood discs in rolls, was not to be ranked with the lesion; in fact, the latter was not neces-

sarily pathological. Many of the specimens exhibited the permanent changes of the nerve elements, especially the atrophy of the spindle-shaped cells of the fifth cortical layer, the increase of lymphoid bodies along the lines of the vessels. This increase was frequently so marked that the vessels were almost hidden from view by the numerous adventitial and peri-adventitial free lymphoid bodies. These bodies, once exuded, did not remain stable, but underwent a closely demonstrable metamorphosis into fixed connective tissue cells. They were irregular, with a thin protoplasm, one or two distinct nuclei, and numerous, delicate, stiff processes; many of these bodies appeared spider-shaped, and hence they have been termed "Spinnenzellen" by Deiters. Other authors, as Boll and Jastrowitz, have termed them "Saftzellen." Now, these bodies in certain instances underwent a peculiar change; one of the processes, attached at one extremity to the spider-cell, by the other to the outer aspect of a delicate vascular wall, could be seen to become hollow from the vessel, towards the cell: in others, the process was distinctly tubular, and the cell-body itself formed a contamination of this tube; and in still others blood corpuscles were found massed in the new channel.

The observation of Lubimoff, that in general paresis a *new formation* of capillaries occurs, was thus confirmed, although Dr. Spitzka had at first been inclined to doubt such an occurrence. Still he would not attribute any important role to this production of new vascular tracts, as they could never vicarate for those destroyed. The adhesion of the pia to the cortex, so common in cases dying from the disease, was shown to depend on an increase of the "Stutzfasern" of the outer cortical layer, and the development of fixed connective tissue cells therein. In one case a whole column of converging fibrillæ could be traced to some depth in the cortex, rooted in the pia. As a result of the increased density of the neuroglia fibrils, spaces were demarcated in the neuroglia which contained a material also found in normal brains, of a slightly different refractive index than the surrounding tissue. This appearance became much more marked, the longer the specimen was hardened,

and although Dr. Spitzka considered it to represent a deviation from the normal bio-chemical processes, yet its interpretation was still doubtful.

He then exhibited specimens of the medulla oblongata; several large sections from a patient who had manifested the symptom "alalia" revealed a sclerosis of a very limited extent. The doctor urged that this case illustrated how independent of the *size* of a lesion the resulting symptoms were, provided an important locality was effected. In this case, although the sclerosis was merely just visible to the naked eye, it had totally obliterated the raphe medullæ oblongatæ. Through this area pass those fibres which place the automatic nucleus hypoglossi under the control of the hemispheres. This path being interrupted, explains how voluntary control of the lingual motions was suspended, while trophic disturbances and the automatic motions (as in chewing) were absent, for the muscles and nerve-roots themselves were not markedly abnormal. The Doctor did not discuss the spinal lesions, as there would be a demonstration of these by another gentleman at the next meeting.

Dr. Seguin asked Dr. Spitzka whether he had heard of the researches of a recent writer in the "Centralblatt," who stated that marked lesions occurred in the facial and abducens nuclei which were often overlooked.

Dr. Spitzka replied that he himself had hundreds of sections from this region in his possession; that he had found lesions, and that Meynert and others had found similar conditions many years ago.

Dr. Seguin: This writer's special point was that the degenerated area of the hemispheres and lower centres were continuous.

Dr. Spitzka: I have not read the paper referred to, and must ask Dr. Seguin whether this continuousness of the degenerative areas was described as similar to secondary degenerations, for he had called attention to such a relation nearly a year ago.

Dr. Seguin thought that the writer had not referred to this but to a primary relation.

Dr. Spitzka stated that he had examined successive sections

from the third ventricle down to below the pyramidal decussation, and had satisfied himself that there might be a perfectly healthy interval between a lesion of the cord and medulla on the one hand and the higher lesions on the other. He further stated that to demonstrate this was not difficult, as even if the investigator did not possess a microtome, he could, if familiar with the normal anatomy of the parts, pick out the suspected locality and obtain smaller but equally conclusive sections freehand.

"The Doctor wished it to be understood that his description only applied to the typical form of progressive pareses, not to those varieties of paralytic insanity due to alcoholism, syphilis, and secondary to spinal disease. As to the vascular condition of capillary stasis it was found in alcoholism of long standing, as well as in those dying from puerperal fever. There was nothing contradictory in this, as alcoholism often passed into paresis, and that form, occurring with puerperal fever, seemed to be incompatible with the conditions of life, and did not enter into our calculations accordingly. He had frequently found vascular engorgements almost reaching the degree of stasis in maniacal patients, and perhaps the fact that maniacal attacks occur in the puerperal condition may not be without some relation to the condition of the vessels in those dying of puerperal fever."

Dr. Seguin exhibited sections of the medulla oblongata of a male patient aged 67 years, who had died after exhibiting many symptoms of lesion of the medulla, such as imperfect deglutition and articulation, Stokes-Cheyne respiration, staggering gait, and who also had right hemiplegia without aphasia shortly before death.

The autopsy showed an uniformly atrophied wet brain, with *état-criblé*, but without any gross lesion capable of causing symptoms. The sections of the medulla oblongata, however, show microscopic lesions which make the case quite clear; the nuclei of origin of the hypoglossus, pneumogastric, and glosso-pharyngeal on both sides, are in a state of degeneration and atrophy. Few cells are left in the hypoglossal nuclei, and many cells in the vagus and glosso-pharyngeal nuclei are filled with granulations. The case will soon be published in full.

Reviews and Bibliographical Notices.

I.—MAUDSLEY: PHYSIOLOGY OF THE MIND.

THE PHYSIOLOGY OF THE MIND. Being the First Part of a Third Edition, Revised, Enlarged, and, in great part, Rewritten, of the Physiology and Pathology of the Mind. By Henry Maudsley, M. D. New York: D. Appleton & Co. 1878. 547 pages.

Notwithstanding this work in a somewhat different form, has been long before the reading public, and is, hence, comparatively well known, we shall not be deterred from giving considerable space to a notice of that portion of the new edition now before us.

This is noticeably less confident and aggressive in tone, if not less contemptuous in its references to "metaphysics" and the "introspective psychology" than was the earlier edition, but it contains about the same amount in the way of quotations from Goethe, and the old English physiologieal psychologists, and the school of Herbart, and the same almost obtrusive assumptions of the Baconian spirit and phraseology. We fancy also we detect in this re-reading, less, perhaps, of a certain peculiarity in an otherwise remarkably excellent and graphic style, which we cannot easily characterize, but which may be described as at times a kind of fine strategy, in statement and expression consciously adopted, we are almost ready to suppose, for the purpose of rendering his writings striking and effective. So far forth as such qualities are exhibited with the appearances of spontaneity they may be interesting or even delightful, but the moment they appear in another garb, they are likely, at the least, to become monotonous. And we must confess, whether right or wrong, to have experieneed something akin to the latter feeling in reading the book, and for the somewhat vague reason we have just tried to name.

But taken altogether, this work of Dr. Maudsley's, is perhaps the most interesting and suggestive contribution to the subject of which it treats, that has appeared in the English language for many years. We deteet many ehanges in this new edition as compared with the earlier ones, and generally for the better, but find less in the way of a reference to recent researches in respect to the nervous system than was to have been expected in such a

work, issued in the year 1877. We feel that for some reason Dr. Maudsley cannot have been a very active reader of medical literature during the past decade. On this account we must think the work suffers somewhat. But it contains so much in the way of good matter and good thought, and bears so strongly the impress of its author's individuality, as to compensate largely for any supposed short-comings in the fullness of its survey of recent progress in the anatomy and physiology of the nervous system.

Perhaps no other part of the work, will more challenge the attention of the thoughtful reader, than the first chapter, "On the Method of the Study of Mind." This chapter of 76 pages, is chiefly devoted to an unsparing—even a contemptuous—criticism of the method which it is alleged was formerly pursued in the study of mind; namely, the "psychological" or "introspective," and *per contra*, a warm and almost unqualified defence of the "objective" or "physiological" method of the study of mind. We cannot at present enter into a full examination of the questions which arise on the ground referred to, and which have been the occasion of so much discussion from time immemmorial, even down to this hour, but will not let the opportunity pass for examining them with some detail, in the light in which they are placed by the declarations of Dr. Maudsley.

The "introspective" psychology as is well known is mainly based on the phenomena manifested in consciousness. The observer looks within to behold the nature and order of thought and feeling, under the same or varying conditions, external and internal, and accepting the so-called "revelations" or "deliverances" of consciousness *as facts*, proceeds to deal with them (as with any other kind of facts), by analysis and classification, so as to *systematize* our knowledge derived from this source. If what we can discover as done or happening within the "sphere of consciousness" may be regarded as true when judged by ordinary standards, in other words, if these things can be shown to be *facts* as some have considered them to be, there could be no doubt, it would seem, whether *such* facts are as susceptible of scientific treatment as the facts of the physical world are admitted to be. But here arises at once the chief difficulty. Dr. Maudsley, after the example of M. Comte, not to mention others before him, seeks to discredit, not only the facts of so-called self-consciousness, but by consequence, consciousness itself. Thus is the attempt made to remove the very grounds of the "introspective" psychology. For if its revelations are essentially contradictory and false, as they are declared in the plainest terms to be, by Dr. Maudsley, truly a science of psychology can never be built up on such data. One of the chief characteristics of the work before us, is the hostility displayed in it, to consciousness, and the phenomena which appear within its limits. No opportunity however oblique, is missed, for disparaging it, as a faculty of knowledge. But let us proceed to a

discussion of Dr. Maudsley's positions in regard to this question and see if they can bear the tests of even a cursory examination.

In order to place his views before the reader, we will quote a a number of passages selected with some care, from the mass of his utterances on the subject in hand. What *is* consciousness according to Dr. Maudsley?

The reader would hardly believe it beforehand, but it is not possible to find in the entire work, a full and explicit definition of this important term, though he admits "we are involved in the confusion which comes of the want of an exact agreement as to what is meant by consciousness; we suffer from the want of an exact definition in which men agree." (P. 204, *Am. Ed.*) And yet there is not, as already said, a clear definition of consciousness, in the book. The following passages may serve as well perhaps as any others, to convey our author's meaning:

"Whoever endeavors faithfully and firmly to obtain a definite idea of what is meant by consciousness, will find it nowise so easy a matter as the frequent and ready use of the word might imply. * * * What consciousness is will appear better if its relations be closely examined without prejudice. It will then be seen that it is not separable from knowledge, that it exists only as a part of the concrete mental act; that it has no more power of withdrawing from the particular phenomenon, and of taking full and fair observation of it, than a boy has of jumping over his own shadow. Consciousness is not a faculty or substance, but a quality or attribute of the concrete mental act. There is no consciousness without something of which one is conscious, no abstract consciousness without contents, and it may exist in different degrees of intensity, or it may be absent altogether. In so far as there is consciousness there is certainly mental activity; but it is not true that in so far as there is mental activity there is consciousness; it is only with a certain intensity of representation or conception that consciousness appears." (P. 45–6) "It can hardly be doubted that too much has been made of consciousness in times past, and that instead of mental phenomena revolving round it as the sun of the system, it is rather a sort of satellite of mind,—the indicator which makes known what is being done, not the agent in doing it." (P. 245).

Other passages of similar import might be quoted, but not one which would in brief space throw any stronger light on the real nature of consciousness than those already presented. But from them we may gather, that consciousness is really a subordinate, even an outlying,—faculty of the mind. The distinctions between it and other faculties of mind, are so drawn as to imply that they were made in view of certain errors on the part of the "introspective" psychologists, which we feel sure would be repudiated by such psychologists as but little better than travesties of their real doctrines as to the nature of consciousness. Dr. Maudsley, in this and other parts of his

work, as we shall presently try to show, is combatting in great measure difficulties of his own raising.

We will now endeavor to define what we understand consciousness to be, and then bring our definition face to face with Dr. Maudsley's objections to a reliance on it, in order to see whether consciousness must be relegated to the outer darkness which he has prepared for it.

To define this term "consciousness," is truly a difficult task, as Dr. Maudsley has said, and yet it may be done, with some approach towards clearness. In making an attempt at a definition it will not be possible to enter into a history of the use of the term, by different writers, and in different schools of psychology. By *consciousness* we understand that faculty which mind has, of becoming aware of certain of its own states and acts. Any acts or states of mind, which are not revealed in consciousness, either directly or *indirectly*, are never known, or never become a part of our knowledge. We hear much of "unconsciousness" mental action, or to state it in physiological terms, "unconscious cerebration," and hence much of mind and its operations which lie beyond the confines of consciousness. All this may be admitted. But if *certain of the results* of these unconscious operations of mind are not somehow and sometime projected into the domain of consciousness, and made manifest in its light, they could never be known at all.

It makes no difference whether the final *object* of knowledge is a conscious act or state of the mind, and is hence properly *subjective*, or whether it is in the outer world, and hence is in the fullest sense of the word extra-mental or *objective*, it can only be known or truly felt, in consciousness.

Consciousness does not manufacture its own contents. It is not a creative, but simply a cognitive faculty. It has nothing more to do in creating the mental states or in performing the mental acts, which it cognizes, than the eye has in creating the luminous rays which affect it in vision. It simply knows and feels such acts as occur within, or states of feeling as arise into, its sphere, whether from within or from without by the way of the senses. No matter from what source, even if from the depths of the mind's unconscious activities, or by way of sense observation, all objects of knowledge must be at least indirectly represented in consciousness if they ever are known. Consciousness being passive as regards its objects, since it does not create them. It takes them as they come. If they are in the outer world, and are inadequately represented by defective or diseased sense organs, consciousness is not to blame for the distorted data given it. Unconscious mental action, if it is represented in consciousness, must be in a fragmentary way. From what we *know* of such unconscious action, by reason of its having been made partly known in consciousness, we *infer* what the whole unconscious process must have been. But processes of knowledge into which inference enters largely, are peculiarly

liable to include error. But consciousness is not responsible for the errors that arise in either of these ways.

Whether or not the mental states and acts, which are revealed in consciousness arise from, or represent, extra-mental or more strictly, extra-conscious objects, the mental conditions whatever may be their origin, are apprehended in consciousness *as they are*. What power has consciousness of making or modifying its contents or objects? Of what may we be conscious? Solely of certain of our mental states or acts, and of these alone. And it matters not whether the states of mind of which we are conscious are produced by external or internal causes, all we are conscious of is the resultant act or state of mind, only this and nothing more. Then where is the propriety of applying the term "self" to consciousness, implying thereby that we may be *conscious* of the non-ego? We know well what the position has been and yet is, of the Natural Realists, chief among whom, in later days, was Sir W. Hamilton, who held that we are conscious not only of the ego, but even of a *material* non-ego. We do not certainly know what are Dr. Maudsley's opinions on this subject, but we do know he uses this term of the Natural Realists, and that when it is used advisedly, it involves one of the most serious errors which can be entertained on this subject. There is no consciousness beside *self*-consciousness.

There would seem to be in the minds of many a serious difficulty as to the relation of consciousness to the other faculties of the mind. It is often spoken of as if it were *one* of the faculties of knowledge. It is *the* faculty of knowledge, or *cognitive* faculty. Beside it there is none other. It is not only the general faculty of *knowledge*, but also of *feeling*. In it we have *combined*, knowing and feeling. In its action we have presented the paradox of the thing knowing as being the thing known or conversly the thing known as being the thing knowing. In the play of consciousness the mind knows itself as knowing, and feels itself as feeling. Here, if anywhere knowing and feeling blend in one concrete act. There is no such thing as knowing or feeling out of consciousness. Every mental state or act, however simple, or however complex, in which we truly feel or know, consciousness is *necessarily* present. There may be many mental operations apparently or really conducted outside of consciousness, either forgotten in a common amnesia, or never made known except in some of their remoter results in consciousness, but we could never know this as a fact, or anything as to the nature of such operations, unless they are somehow represented in consciousness. But we cannot prolong this part of the discussion any farther, important as we believe it to be. We will only take the space to say, by way of recapitulation, that if the statements made are correct, that consciousness is the sole general faculty of knowledge and of feeling, that it does not make its own objects,

it is not the *creative* faculty of mind, and only cognizes or feels *mental conditions*, however well they may represent, or however much they may misrepresent their extra-conscious objects, whether these objects are of the ego or non-ego. It may be obliterated for a time, but if so, most if not all the other mental faculties go with it, but do not always return with it. We know well there have been cases which would seem to lead to a conclusion different from the one last stated, but we also know they will not bear the tests of a rigid scrutiny.

Then bearing in mind the few hints given above as to the nature and relations of consciousness, let us proceed to examine some of Dr. Maudsley's objections to consciousness as a faculty of knowledge, and find out if we can what he would supplant it with or trust in its stead. Dr. Maudsley almost seems to have felt himself charged with the mission of giving the *coup de grâce* to "metaphysics" and the "introspective psychology." But this cannot be done in the way Dr. Maudsley has attempted it, save by a disparagement of consciousness, which he seems to regard as the enchanted ground in which a certain class of ill balanced thinkers inevitably are fated to lose themselves. Consciousness is to the "philosophers" and "metaphysicians," what the Bible is to the religious sects. From both, the most bitterly opposed and widely different parties draw their choicest materials. The only hope for a permanent peace lies in getting both out of the way, and in trusting instead in the respective cases to the "light of nature" and the "objective method." But what are Dr. Maudsley's objections to depending on consciousness, or its "revelations?"

They are to begin with:

1. That "consciousness, which does not even tell us that we have a brain, is certainly incompetent to give any account of the essential material conditions which underlie every mental manifestation, and determine the character of it." "The most ingenious introspectionist could never discover from the revelations of self-consciousness that the cause of a particular mood of mind lay in the liver or in the heart, or in some other part of the body; nor could he gain from them the least inkling how essentially the operations of these organs affect the characters of the individual, the actual constitution of his ego, etc., etc."

This really old objection to consciousness and its "testimony," has always appeared to us one of the most singular that could be made. If any considerable body of thinkers had ever pretended that consciousness can directly inform us of the brain and its structure, as it can make known to us states and acts of mind, there would have been some place for such an objection. But who among even the most misguided of the "introspective" psychologists ever made a claim so absurd?

Who that views consciousness aright can have imagined it as directly knowing brain or brain structure? By the very terms

of its case, as it has always seemed to us, such things are absolutely beyond a direct manifestation in consciousness. And yet, Dr. Maudsley gravely sets himself down to discuss this question, as if it was a recognized article in the creed of the "introspective" psychologists! We know as well as we can know, that "psychologists" have hitherto (and they have not been alone in this matter) neglected a study of the nervous system, in health and disease, in man and the lower animals, and that they have failed in a healthy, practical progress, and hence, have in various degrees in different cases missed the full-orbed method of a study of mind. But what great writer ever laid himself open to these strictures of Dr. Maudsley?

But let us reverse the process, and suppose *if we can* (for the sake of a parallel but contrary case), an individual *without a mental experience*, in short without consciousness to investigate a liver, or heart, or even a *brain*, down to the finest structure of the cortex cerebri, and what would he know of mind, of consciousness, for example? Or suppose we permit Dr. Maudsley to examine the structure of the brain, what information would it give him of the really vast mass of mental acts and states which *are made known in consciousness?* Says he: "No observation of the brain, no investigation of its chemical activities, gives us the least information respecting the states of feeling that are connected with them; as has been aptly remarked, it is certain that the anatomist and physiologist might pass centuries in studying the brain and nerves, without ever suspecting what a pleasure or a pain is, *if they have not felt both.* Even vivisections teach us nothing *except by the interpretation which we give them through observation of our own mental processes*," (P. 62.)

The truth of the case could not be better stated. A study of the nervous organism, however exhaustive, would be utterly barren of results as regards the facts, not to mention the nature of mental operations, unless the observer should supply from his inward *conscious* experience the keys to the interpretation. But the fact that this *is* done, is almost uniformly ignored or forgotten by those who profess to study mind by the "objective" method. Not a single fact which is brought to light in this way but derives its final significance from what has been quietly infused into it from the resources of our experiences in consciousness.

But is it certain that consciousness cannot inform us as regards brain structure or other facts of the physical organism? It is certain that it cannot cognize these things *directly.* They cannot be manifested in consciousness, as are certain of our mental states and acts. But it is just as certain that they can never be known unless *represented* in consciousness somehow. If the observer should study the brain which is probably the instrument of our higher mental operations, shall it not be by looking at it with the eye, or handling it with the hand? We may re-

fine and enlarge our vision by the use of the microscope, or by the use of re-agents may render the delicate structure of the brain more apparent than it is naturally. But in any case we must use our senses. If we do, it seems needless to say, that those sense impressions which reach consciousness are those alone which we can *know*.

It follows, then, that even in our "objective" studies, we must depend on consciousness, and it alone. Its states may not truly represent things as they are in the outer world, or may not reveal all that belongs to our mental lives, but this is not the fault of consciousness. If it does not show our inward experiences as they are, and is hence not to be trusted, how shall we trust it as regards our sense experiences of outward things? If we do not trust it, to what faculty shall we go, and to what faculty would Dr. Maudsley send us? Would the reader believe it, but Dr. Maudsley does not, in fact, point out any faculty by which he would supplant it. But if these remarks are true, is it not clear that Dr. Maudsley's statement is in a certain sense really incorrect, when he declares consciousness can teach us nothing as to structure of the brain?

2. It is objected again that "to direct consciousness inwardly to the observation of a particular state of mind is to isolate that activity for the time, to cut it off from its relations, and, therefore, to render it unnatural. In order to observe its own action, it is necessary that the mind pause from activity, and yet it is the train of activity that is to be observed. So long as you cannot effect the pause necessary for self-contemplation, there cannot be a sufficient observation of the current of activity. If the pause is effected, then there can be nothing to observe, there would then be no consciousness, for consciousness is awakened by the transition from one physical or mental state to another. This cannot be accounted a vain and theoretical objection, for the results of introspection too surely confirm its validity." (P. 17.)

This objection to consciousness as the faculty of knowledge, is based throughout on a singular misapprehension as to its nature. It is here confounded with the voluntary activity of the mind.

Is it true, that "in order to observe its own action, it is necessary that the mind pause from activity?" Most certainly not. The only things manifested in consciousness, are acts and states of mind. No movement or affection of the mind it is probable can occur within the domain of consciousness, but it is known; for the capacity to feel and know certain of its own changes, however they may be produced, is ingrained, to use a physical illustration of mind, in the very tissue of mind. Consciousness is not a faculty which must or can place itself *vis a vis* with some form of mental activity, in order to look upon it *abextra*. All mental activities of which we are or may be conscious, occur in its very midst, permeated by it. It is not true that

"consciousness is awakened *by the transition* from one physical or mental state to another." It is awakened *by the states themselves*, and not the "transitions" from one to another. It is not awakened by "physical" as contrasted with "mental" states. It may be and is aroused by mental states produced by physical causes, but not by the physical conditions *themselves*.

So far, therefore, from its being true, that in order for consciousness to give its attention to any form of mental activity, that the activity must cease, the very reverse is true. When activity ceases within the sphere of consciousness, the consciousness ceases, and *vice versa*. The objection *is* a "vain and fanciful one," and misreads because it misconceives the whole case.

Then it is said that "there is no agreement among those who have acquired the power of introspection," or, in other words, the habit of looking within consciousness. This is only partly true. There is but little difference among psychologists as to what are the *facts* of mental experience, though there have been and there are, wide differences as to modes of dealing with them. But this objection lies with more or less force against all sciences depending on sense observation, as well as against the "introspective psychology."

Over and over again it is asserted that consciousness is a wholly unreliable witness, as to what passes within its domain. But we do not hesitate to assert just the contrary. The fault does not lie with consciousness, but rather in the distorted and fragmentary way in which things are often *represented*, or even misrepresented in consciousness, through defects, or disease of the sense mechanisms, and of the organic seats of mental activity, or in defective processes of reasoning on data given in consciousness. And so on for many other objections to consciousness, or a reliance on it. They are all of them based apparently on erroneous notions of its nature and office.

Dr. Maudsley does not appear to notice sufficiently that in impeaching consciousness in one aspect of the case, he discredits it for all. He does not appear to recognize that there is no faculty of knowledge besides consciousness, whether for things external or internal*.

It seems to us that Dr. Maudsley, in his zeal in behalf of the "objective" study of mind, has unintentionally overstated his case, and in so doing has shown more of the spirit of the partisan than of the judge. Dr. Maudsley is certainly right in directing attention with solemn emphasis to a study of the *material mechanism* of mind, which has been comparatively neglected, but seems to us to have thoroughly lost his balance in his formal utterances in respect to the "method of the study of mind." He, and all those (and they are many) who occupy a similar position, commit just as great an error in abandoning

*See Metaphysics of Sir W. Hamilton, p. 397 Et. Seq.

contemptuously the abused "introspective" method, as the older psychologists did in neglecting a study of the nervous system. In short, we feel profoundly convinced that in this case, as in so many others, the truth is not wholly with either party, but lies between them.

To be studied aright, mind must be studied from both sides. Each party must accord to the other equal honesty of aim, and equal intelligence, each permitting the other to make its own definitions, pursue its own methods, and name its own standards of utility. Introspective psychologists must not taunt the anatomist and physiologist, because he fails, as fail he must, to lay bare a thought at the point of the dissecting knife, or to bring into view the fine interior qualitative characteristics of an emotion. Nor, on the other hand, must the anatomist and physiologist pronounce as illusory the objects of the psychologist, because they cannot be brought to the (to such objects) impossible tests to which we can bring those objects which have mensurable, and therefore *quantitative*, as well as *qualitative*, relations. But we cannot pursue this important and tempting theme farther. We will doubtless find abundant opportunity for returning to it, whenever it can be profitably discussed.

But what is mind, according to Dr. Maudsley? He says: "Mind, used in the sense of substance or essence, and brain, used in the sense of organ of mental function, are, at bottom, two names for the same substance." Again, "What is it which, in a physiological sense, we call mind? Not the material products of cerebral activity which pass as excretions into the blood, but the marvelous energy which cannot be grasped or handled." "So likewise with regard to the manifold phenomena of mind: by observation of them and abstraction from the particular we get the general conception, or the *essential idea* of mind, an idea which has *no more existence out of the mind* than any other abstract idea, or general term." Or again, "Instead of mind being a wondrous spiritual entity, the independent source of power, and self-sufficient cause of causes, an honest observation proves incontestably that it is the most dependent of all the natural forces. It is the highest development of force, and to its existence all the lower natural forces are indisputably pre-requisite."

Mind is "nothing more than *a general term*, denoting the sense of those functions of brain which are accompanied by consciousness, and which are commonly described as thought, feeling and will. To deal with mind as a force in nature apart from the consideration of the matter through the changes of which it is manifested, is truly no less vain and absurd than it would confessedly be to attempt to handle electricity and gravitation as forces apart from the changes in matter by which alone we know them."

"The theory of mind as a spiritual entity, which, having an existence independent of body, uses this as its instrument, owes its origin, as other metaphysical entities have owed their origin to this tendency to convert abstractions into entities.'

But these quotations will perhaps serve to show what are Dr. Maudsley's opinions as to the nature of mind. They seem to show clearly that he regards it as simply a mere function of brain, and that it perishes, probably, with the dissolution of the brain. There is no such "spiritual entity" as has been called "mind" by some, for this latter is simply a "metaphysical entity," an "abstraction." We cannot enter into a discussion of this subject, farther than to say that we differ deeply from Dr. Maudsley on this subject, even on scientific grounds. The hypothesis of the existence of mind as at least an *intelligent permanent force*, is, in our judgment, a reasonable one, and we feel prepared to maintain it. But we must reserve until a future occasion a discussion of this much agitated question.

Dr. Maudsley regards the doctrine of the "freedom of the will" as simply "nonsense." Here, again, we can only record our square dissent, and if Dr. Maudsley has reasons for rejecting the doctrine in question, we also have what seem good reasons for maintaining it. But it is impossible to discuss such a question within the limits of an ordinary book review.

Matter is declared to be active, possibly *self* active. For he says: "it were wise to ponder well the remarkable operations of which matter is capable, and to reflect upon the wonderful *works which it is continually doing before our eyes.*" Does matter in the final analysis of the case, act on its own account, or is it simply acted on? We have no hesitation as to which opinion to adopt—it is the latter. We believe that no more fundamental error could be made, in this case, than to declare matter *as active*—self-active. But this is what Dr. Maudsley does, or seems to do, in common with so many others.

As a sample of psychological exposition, we would refer to Dr Maudsley's explanation of the origin of a "moral sense."

Says he: "Whence is derived the beginning or first shoot of a moral sense? The answer which may be thought not so fit, which nevertheless I propose to make, is that the root of the moral sense must be sought in the instinct of self-propagation. By the gratification of this instinct, *notwithstanding that it is an act of pure self-indulgence*, the individual does not appropriate matter to himself and increase, but dissipates energy, *giving off from himself something*, which goes to propagate the species, the aim of the instinct being not to benefit the individual—for though its indulgence gratifies him, *he is the less*, by his gratification—but to inveigle him through self-gratification to continue the kind, it is not appropriative, but distributive; not *egoistic*, so to speak, but *altruistic*." (P. 398.)

But how about the female, in such a case, who does not give, but receives, in the most thoroughly egoistic manner? This is what may be called the "seminal" theory of morals, and is surely as material an explanation of a mental fact as could be desired. But we forbear the comment which it so strongly invites.

Dr. Maudsley is evidently not only a thorough-going evolu-

tionist, but a disciple in the school of Comte. He is apparently more thoroughly imbued with the spirit of the "Philosophie Positive" of that author, than with that of the "Novum Organum" of Bacon.

But notwithstanding the formal, and we will even say "doctrinal" errors which pervade the book, it is one of great value, is especially suggestive in its full and graphic accounts of the nervous mechanism in its real or supposed relations to mental action. We can do no less than join the general commendation which it has received, and which on many accounts it merits.

Notwithstanding the wide differences between our own mode of viewing many of the subjects treated and those of our author (and we have by no means exhausted them in this notice), we cannot close without expressing our high sense of the service done by Dr. Maudsley in the province of physiological psychology and mental medicine.

II.—TRANSACTIONS OF THE AMERICAN NEUROLOGICAL ASSOCIATION.

TRANSACTIONS OF THE AMERICAN NEUROLOGICAL ASSOCIATION FOR 1877. Edited by E. C. Seguin, M. D. Vol. II. New York: G. P. Putnam's Sons.

This unpretentious volume of some 200 pages, consists of a resume of the discussions, besides twelve papers presented at the meeting of 1877, and ten of the previous year. The highest praise we can bestow on it is to acknowledge that each article can bear a rigid criticism, which, unfortunately, can be said of but few American society proceedings. But while admiring the originality of the entire work, we ought not to overlook, also, that the different papers are all characterized by an absence of that supreme disdain of foreign literature so prevalent in American writings.

The first paper, by Dr. N. B. Emerson, calls attention to sciatica of syphilitic origin, an affection but little recognized. It is a good instance of a purely clinical study unsupported by post-mortem confirmation. Nevertheless, it seems to us that such papers would gain by greater conciseness of style.

In an article on the "odor of the human body in certain nervous affections," Dr. Hammond relates several cases in which some parts of the skin exhaled an odor of violets or pineapple. An analysis of the sweat rendered it probable that the odor was due to traces of butyric ether. In one case the desired suppression

of the odoriferous perspiration was affected by the continued use of salicylate of soda.

Under the title, "Spinal irritation in children as related to true and false arthropathies," Dr. V. P. Gibney cites forty cases, many of which, however, correspond by no means to the description of that ill-defined group of symptoms known as spinal irritation. Some evidently, are cases of spondylitis, others are instances of chorea. Such was also the opinion of many members who took part in the discussion. The import of the paper is, that there existed symptoms of disease of different joints together with nervous disturbances, especially tender spots over some vertebræ, and that the majority of the cases were relieved, and many cured by treatment directed to a supposed spinal trouble, mostly in the shape of blisters. The therapeutic lesson to be derived from these cases is a very instructive one. The pathological relations, however, are nothing more than suggestive. That real joint diseases may have a nervous origin is proven by their occurrence in ataxic troubles. The cases of Dr. Gibney, however, inform us neither as to the anatomical changes in the joints nor as to the condition of the spinal cord. Some of the cases remind strongly of the nervous mimicry of joint diseases described by Brodie, Esmarck, Paget, and others, to which authors, however, we find no reference in the paper.

Of Dr. Schmidt's paper on "Nervous ganglionic bodies," but little can be said. The accompanying diagram is of but slight service.

Dr. Beard's report on the "Epidemic Tetanus of Eastern Long Island," culminates in the following statements:

For the last three-quarters of a century there has been an endemic tetanus in certain portions of Suffolk county. It consists of both the traumatic and spontaneous varieties, and affects animals as well as man. This endemic abounds most in the towns of the south side, especially at the Hamptons; is less common in the central towns, and on the north side and in Montauk does not exist. The endemic has been on the decline for the past ten or fifteen years, and in the central portion (excepting Riverhead) no longer exists. For the causation there are three conceivable theories: geology, the use of fish on the land as manure, and dampness in the air. The theory based upon geology, and advocated by Dr. B. D. Carpenter, who has studied the subject, appears to be disproved by the decline in recent years. Geological conditions are constant factors. In favor of the fish on the land theory, is the occurrence of the disease in those localities where fish are the most used, and the decline of the disease with the decline in the use of fresh fish as manure. Dr. Beard thinks, however, that the facts up to date seem to favor the theory of dampness in the air, combined with the local dampness of the soil. Dr. Beard would treat a case of tetanus by Calabar bean (English preparation), in small doses every hour, or

half hour, so as to affect the pupil, and at the same time apply ice-bags to the spine. As suggested by Dr. Carpenter, the patient should be kept absolutely quiet if possible. The local application of the oil of turpentine to all wounds is practiced by a number of the physicians of Suffolk county, and is to be recommended. The apprehension felt by New York surgeons, that tetanus is likely to follow surgical operations in Suffolk county, is not justifiable by the facts as they now stand. That the interest in the paper would have been materially increased by judicious condensation (instead of 25 pages), is scarcely doubtful.

Dr. E. Dupuy's paper on hereditary epilepsy recapitulates, in an interesting manner, the well-known phenomena of epilepsy artificially produced in the Guinea pig by Brown-Sequard's method. As regards its hereditary transmission, Dupuy claims to have met with it but very rarely: but does his experience entitle him to deny the validity of the somewhat contradictory facts observed by Obersteiner? He further states that such young Guinea pigs are never born epileptic, but become so after six or seven weeks. He has also seen the vice transmitted through five generations in leaps, of some twenty descendants of epileptic parents, but those were affected with the disease. As regard's Dupuy's hypothesis on the transmission of the morbid tendency through Balbiani's "vesicule embryogenique," nothing can be said further than that it is purely gratuitous.

A second paper, by the same author, on the "Seat of vaso-motor centres," is more revolutionary in its tendency than is justifiable by the facts adduced. As much as we can divine, Dr. Dupuy's conception is that vaso-motor fibres do not at all originate in the spinal cord, but take their origin in the ganglionic cells of the arachnoid membrane. But of the authors quoted by him, neither Claude Bernard, nor Chauveau, have proven that vaso-motor nerves do not arise from the spinal cord; nor indeed, do they claim it. That nerve fibres controlling the vessels of the cord itself, re-enter the cord coming from some trunk outside of the rachidian cavity, is a fact that has really no bearing on the question, as to whether vaso motor nerves originate in the spinal cord. If Dupuy has really observed the phenomena following hemisection of the cord, to result simply from an experimental lesion of the membrane of the medulla, the mere statement will not suffice; a fully detailed account of the experiment must be demanded before any claim can be made on our belief. Finally, as regards his experiment on the Gasserian ganglion, which Dr. Dupuy mentions, personal experience has shown us what little value there is in results of irritation in a region as difficult of access as that ganglion.

In the following papers by Dr. J. C. Shaw, on "Ataxia in a child," and "Intra-cranial tumors," Dr. E. Seguin on "Post-paralytic chorea, and "Localized cerebral lesions, and Dr. F. P. Kinnicut, on "Hemianæsthesia," the interest centres in the cases, and no brief abstract would do justice to them.

The following papers are dated 1876, for which year no separate volume appeared. Dr. S. G. Webber's paper on "Paralysis after acute diseases," consists of a few well-studied personal observations, followed by an extensive bibliographical review. Of the cases of paralysis after acute diseases, some are so slight or of such short duration, that their pathology has entirely escaped us. Others more severe and lasting, can be referred to lesions (inflammatory) of the anterior spinal cornua, or the anterior white columns. In the former case there will be muscular atrophy and loss of electrical reaction, and this may be limited to only a few muscles. Destruction of the white columns, on the other hand, causes paralysis without atrophy. A neuritis alone, as the cause of the paralysis, would be of course without spinal symptoms.

Dr. Hammond describes an interesting form of muscular incoordination in a small child, occurring only on attempting to walk.

A case of hysterical trismus is narrated by Dr. Shaw, which terminated favorably under the full hypodermic use of atropia.

Dr. Cross summing up his study on the intra-ocular circulation on epilepsy, admits that during the intervals nothing characteristic is seen. Immediately after the fit, he observed retinal congestion, which lasted some hours. During the paroxysm, the fundus has been seen but once, and in that case Hughlings Jackson found retinal ischaemia.

A following paper by Dr. Emerson comprises a full bibliographical account of Tetany together with two personal observations.

Some interesting new observations are communicated by Dr. S. Weir Mitchell in his remarks on the "transmission of electric influence across the middle line of the body." On applying the electrodes on one side of the face not far from the middle line, contractions occurred in the muscles of the opposite side. Mere reflex action was excluded in cases of facial paralysis, where of course the centrifugal route of any reflex was cut off, while the muscles still reacted easily to a constant current passing through the other side of the face. At Mitchell's request, Prof. Barker repeated such experiments with a galvanometer and found that induction currents may be easily developed at a distance through imperfect conductors by very moderate (galvanic) currents. These *distant* contractions are therefore the result of secondary currents induced in the tissues at some distance from the primary current.

Two casuistic papers by Dr. Shaw cannot well be made the subject of an abstract.

Dr. J. J. Putnam has repeated the experiments of Brown-Sequard and Dupuy on the use of carbonic acid gas, in the treatment of headache, with no very satisfactory results. Out of nine cases but two were relieved by the inhalation of the gas through the nostrils.

The last paper is by Dr. Jewell on Athetosis, in which he attempts to trace the symptoms of his case to their cause. Such a study is quite fascinating and in fact very useful in guiding us in what direction to continue the research. But as long as the so-called functional disorders have not found their true explanation in autopsies, and since they cannot be satisfactorily reproduced in animals, all attempts at their pathogeny must necessarily suffer from great uncertainty. In terminating Dr. Jewell proposes to identify athetosis with post-hemiplegic chorea; as compared with ordinary chorea, the prognosis in athetosis is decidedly more grave.

If, in presenting these brief abstracts, we have rather neglected the purely clinical papers, it is not from any want of appreciation. Reports of cases, the essential features of which are stated in concise terms as they should be, bear no further condensation without loss of interest.

The American Neurological Association has during these three years of its existence, lived up to the project of its founders both in the work it has accomplished and in the impulse it has given.

H. G

III.—THE INTERNATIONAL MEDICAL CONGRESS.

Transactions of the International Medical Congress of Philadelphia, 1876. Edited for the Congress by John Ashhurst, Jr., A. M., M. D. Philadelphia: Printed for the Congress, 1877. 1,153 pages.

1. The "Transactions" of the first International Medical Congress held on American soil lie before us, and we have every reason to be proud, not only of the encomiums awarded to American medical science by the distinguished foreign delegates in their papers and addresses, but also of the valuable work done in the various sections. Many of the addresses, especially those by Woodward and Chaille, were excellent *resumés* of the work done in certain fields on this side of the Atlantic, and the sections on Biology, and many of the special branches, report considerable progress. It is to be regretted that, as will be more fully shown when we come to review that field, the only section whose work falls more especially within the scope of this Review, than which no other deals with a higher, nobler, or more suggestive subject, that on Mental Diseases, should not only not have reached mediocrity, but absolutely fallen far below par.

It is daily becoming more and more apparent that the immense amount of clinical and pathological material accumulated, and

accumulating in our insane asylums, is literally allowed to run to waste, and that such clinical and pathological contributions as are made by those in charge of these institutions, with few exceptions, are either abstracted surreptitiously from European journals, or, if *quasi* original, are still more worthless. Unless a thorough and wide-spread reform be carried out in asylum management and asylum appointments, unless every scientific requirement be complied with in instituting this reform, paying due attention not merely to the farm stock, the engine-house, the gas works and the water supply of the asylum building, but also to proper mental and physical hygiene and the therapeutics, etiology, forensic bearing, clinical history and pathology of insanity, we fear that that branch of medicine, psychiatry, will remain what it is, one of the disgraces of American medicine!

In saying this, we do not place ourselves upon any administrative dogmatic basis, being equally opposed, for example, to the non-use as to the unquestionable existing abuse of "restraint," but would take higher ground that asylums are not to be considered as mere boarding houses for the insane, nor should their medical officers degenerate to wardens or keepers, as is the case with at least one of the New York municipal hospitals for the insane within our knowledge. We insist that the whole community, and more especially the outside medical profession, have a right to demand that those who occupy such responsible positions should be capable men, able to furnish their quota to social and scientific progress. When we reflect that some of our larger State and municipal institutions have opportunities for making nearly a hundred autopsies annually of subjects, many of whom present otherwise rare anomalies of the encephalon and spinal medulla, we hardly know whether to become ironical or indignant, especially when we recollect the fact that some legislatures, more munificently than wisely liberal, have devoted considerable sums of money to pathological laboratories in insane asylums, at an expenditure of thousands of dollars, without a single valuable result.

These facts would lead to a very unfavorable conclusion as to the mental status of asylum superintendents, were it not for the fact that we are aware that the deliberations of their Association are controlled by a factious minority. And furthermore, we are inclined to hope that a number of the members of that association themselves will be willing to assist in any true reform. Has not one of the profoundest clinical observers, Pliny Earle, himself a medical superintendent, given those of his colleagues inclined to clamorous pretensions, the grandest rebuke that could be given? We sincerely trust to be able to record similar able and truthful exposès in the future. They foreshadow the beginning of the end of the close corporation sway in asylum matters.

Before proceeding to review the papers read before Section X., we will notice the two addresses published in the first part of

the volume, whose titles indicate a relation to the subject of insanity.

Address on Medical Jurisprudence with Notes and a Bibliographical Index.—By Stanford C. Chaille, A. M., M. D.—This is an excellent description of the rise and progress of Medical Jurisprudence, not only in the United States, but also in the Old World. It deals very impartially and objectively with the defects in our manner of conducting medico-legal cases, and deservedly criticises the character of the expert testimony often accepted in our courts. On the whole, the author, styling Germany the mother of medico-legal science, recommends the systems there adopted. We can point to Austria as having followed Germany's examples, and to France, whose leading experts are anxiously endeavoring to introduce the same system. The most significant fact to which the author calls attention is, that of forty-six medical schools in this country, twenty-one do not even profess to teach the subject, and that of those which do, only fourteen have chairs especially devoted to it, of which number five have this chair filled by *lawyers intending to teach to medical students a medical subject.* We would heartily endorse the statement that "to require all graduates in medicine to be competent experts as well as practitioners, has long been, and is daily becoming, more impracticable," and that it reveals a complete misapprehension of the scope of forensic medicine, on the part of those who expect it.

The literature appended to this paper does not profess to be complete, but the most important treatises are enumerated, however, we notice the omission of the valuable monographs of Maschka and Skrzeczka.

Address on Mental Hygiene.—By John P. Gray, M. D., Medical Superintendent of the Utica Asylum.—This rather general paper hardly comes within the scope of this review, as it is conspicuous for dealing with almost every sublunary subject, other than the one indicated in the title; with the exception of an extensive quotation from Maudsley, it would be difficult indeed, to find anything especially bearing on mental hygiene in this address, unless it be the religious question, which Dr. Gray has somewhat forcibly dragged into the discussion.

Altogether, the author seems to have written more for entertainment than for instruction, and to accomplish his object has mustered up all his historical and ethnological erudition. To those who are familiar with the mistakes made by Dr. Gray in the field of pathology, it will not appear surprising that a few trifling errors should have crept into his historical statements. Thus the ancient races are said to have had no regard for sanitary science or individual comfort! The leader of the German army is termed "*Frederick* Wilhelm!" Thomas Jefferson, whose religious opinions were on a level with those of Thomas Paine, is styled one of the eminently religious forefathers of our

Republic! Finally, William Penn is held up as the root from which all our feeble virtues have sprung! A casual glance at Macaulay would have convinced Dr. Gray that Penn, justly despised by his co-religionists, bribed by the most contemptible of the bad race of the Stuarts, and a passionate lover of public executions and other revolting spectacles, (Macaulay, History of England), was a very improper example to select, as illustrating the influence of religion on the mind. We failed, although living at the time, to feel that thrill of horror experienced by "millions of Protestants" when Monsignore Darboy was executed. Whatever feeling was manifested on that occasion, was sympathy for a human being, killed by order of an irregular tribunal, for no specified crime, and we do not doubt that had Thiers, Gambetta or Victor Hugo, been in the position of the Archbishop, our regret would have taken a much stronger form, that of indignation. There are numerous ill-founded conclusions in the paper which time forbids us to detail, but we would call attention to one passage, which shows how apt a disciple of Thomas Gradgrind and Bradley Headstone Dr. Gray might become: "The power of attention in the majority of children in any community is not much aroused in ordinary life, and they often look *stupid and dull* on this account. These children enter school, and their attention to a few simple exercises in common awakens the power of attention, and soon, at the *tap of a rule, the sound of a musical note*, or the word of the teacher, the whole school responds." In the above the italics are our own, further inference we leave to the reader.

PAPERS READ BEFORE SECTION TEN.

The Section on Mental Diseases was established by the American Medical Association, with the intention of bringing those engaged in the treatment of mental affections into closer communion with the general body of the profession. It was intended that by this means, not only the general body of the profession might profit, by new and original work done in this section, especially in the clinical and pathological departments, but that the medical superintendents themselves might lose the exclusiveness so characteristic of that distinguished body, and give their assistants an opportunity for presenting their share of the work. The latter has not been done, and such outsiders as venture within the precincts of this section are either ignored or their contributions are suppressed, for fear that a leaven of originality, foreign to the character of their meeting, might be introduced, or in order to conceal the fact that in the pathology of insanity better work is done outside of asylums than in them.

While in every other department of medical science the International Medical Congress called forth valuable and original work, it failed, in the instance of this section, to produce much more than the average amount of crude pathology and adminis-

trative generalities, for which this section has been always conspicuous. The building of water closets, laying of drain pipes, construction of patent settees and cribs, are no doubt very important items to an administrative medical officer, but we would suggest that the proper place for the discussion of such topics is the annual meeting of the Asylum Association, and that the work of the section before us is justly expected to be of a higher and more scientific character.

The first paper is

The microscopic study of the brain. By Walter Kempster, M. D., Medical Superintendent of the Oshkosh asylum. In reading this paper, we hardly know whether to be most surprised at the author's misinterpretations of normal appearances, at his painstaking description of artificial "lesions," or at the confident manner with which he ignores some of the fundamental facts of anatomy. It is not surprising that the author should be able to find lesions in every case of insanity, when he considers granules on the vascular adventitia, which are found in *every sane brain*, as the change producing insanity in most of his cases. As illustrating into what grave errors one ignorant of the normal anatomy of the brain may fall, we refer to page 1092, where the writer is struck by the fact that "pathologically enlarged nerve cells" occur only in the parietal lobe: this fact is to be explained by the occurrence, at just this locality exclusively, of the "normal gigantic pyramidal nerve cell," (Meynert, Major) which Dr. Kempster accordingly finds to be characteristic of insanity, although his brain is as well provided with these bodies, we hope, as every normal brain should be. The columns of Türck appear, in the author's opinion, to occur diffusely throughout the "cerebral tissue." We always considered these to be spinal columns, and the error may be explained by supposing the author to have confounded the "columns of Türck" with the "secondary degenerations of Türck" (p. 1095). He also is ignorant of the existence of the normal pericellular spaces, for he describes them as characteristic of melancholia, on page 1094.

In so far as his descriptions apply to actual pathological changes, we find a marked contrast between his and our own conclusions. He attributes active inflammatory and degenerative changes to dementia, both acute and chronic, while in acute mania and progressive paresis he finds rather passive conditions. We, on the contrary, have found, and are borne out in our conclusions by Lockhart Clarke, Meynert, Magnan, Schüle, Lubimoff and Sankey, with a host of others, that active and florid clinical conditions are marked by active and furibund pathological processes. Thus, hyperæmia and incipient stasis is found in acute mania, intense inflammatory disturbance in paresis, while in acute dementia, properly so called, we have found no lesion whatever: as to terminal dementia, the changes bearing a relation to the symptoms are the residua of long past chronic

or acute affections. From this marked contrast between our respective results, only one conclusion can be drawn; namely, that Dr. Kempster has failed to diagnosticate his cases correctly *intra vitam*, and this, our impression, is strengthened on looking at his asylum report, where we find but two cases of progressive paresis recognized out of over five hundred patients. The intense changes found in his dementia cases would thus be explained; they were doubtless unrecognized cases of paresis, in which the element of dementia predominated.

His ideas on the normal and abnormal cerebral circulation are so confusedly expressed, that we must confess to not seeing our way clearly. One change described is particularly enigmatical; it is said to increase "till the vessel looks like a mass of granular material, through which its outline can sometimes be dimly traced." (p. 1087). And here is a nut for the logician to crack: "I have not found the tortuous vessels in the brains of the lower animals, nor in the cerebral tissue of those who have died sane, except in the brain of a man executed for murder, and who had been insane some years before." (p. 1085). We mentally ask ourselves the question, if tortuosity of the vessels is only found with insanity, was not the man insane at the time of his execution, and was Dr. Kempster the expert who pronounced this man sane?

It is to be regretted that the author has not read the later essays of Obersteiner, (*Med. Jahrbucher*, 77–76, and *Wiener Med. Zeitung*, 76) where nearly all of Kempster's vascular changes are described as occurring in over one hundred and fifty brains of sane persons. He has also mistaken the changes of senility for those of chronic mania; we have in the purest form of chronic mania, (*primare Verrucktheit, folie raissonante*) found no pathological, but rather teratological changes; asymmetry of the brain, heterotopia of the gray substance, congenital defects in the claustrum, etc., (W. and S. Tuke Prize Essay). We cannot in these limits notice a greater part of the paper which is devoted to the consideration of normal anatomical conditions, with which every graduate in medicine is supposed to be acquainted, but for a brief consideration of which the author seems to have been justified by the subsequent discussion, in which his audience showed that to them these matters were new. (P. 1,096).

Lack of perspicuity is a prominent fault of the paper. "Some large masses were found in the cortex," in acute mania, (p. 1,093) their nature being left to the reader to guess. As to the round masses described by the author in connection with nearly every form of insanity, we regret to inform him that these are apt to occur in decomposition of the cerebral tissues, and that by the use of absolute alcohol, leucin and cerebrin are precipitated in the neuroglia in spheroidal sub-crystalline bodies. But for this grave error of finding these bodies (which can be produced in the brains of the lower animals) to be characteristic of

insanity, the author has the excuse of a precedent. Gray, in his monograph on the "Pathology of Insanity," has printed photographs of such artificial precipitates, regarding which Westphal (*Virchow-Hirsch's Jahresbericht*, 1874) justly remarks, that they bear no relation to insanity, nor could he find anything in them, that they were intended to show. A statement we heartily endorse. Still, these results appeared sufficiently important to Dr. Kempster to republish them with hardly any modification, even of his language, in the Wisconsin State Medical Society's Transactions, with this difference, that in the Philadelphia paper he states that he has made one hundred and fifteen autopsies; in the Wisconsin paper this number has increased to "over two hundred," so that in the interval the doctor has had an autopsy almost daily. We envy him this material, we regret the paucity and erroneous nature of his results, and we would venture to suggest, that one case, properly interpreted, is worth more than two hundred, examined at random. Nowhere in the paper is there the slightest attempt at drawing a parallelism between the lesions and the symptoms of insanity, and where, as in the case of hallucinations, he would attribute these symptoms to destruction of nerve cells and fibres, he is unphysiological. Hallucinations imply an *anatomically intact* receptive mechanism, whose function is *temporarily perverted!* In concluding, the author congratulates himself on having begun his investigation when but little was known about the subject, that is about ten years ago; this is excusable in one to whom the European literature of the last ten years, does not seem to have been accessible. We would inform the writer that ten years ago (1867) appeared the excellent "Studien über das Pathologisch-Anatomische Material der Wiener Landenirrenanstalt," by Meynert, embodying the results of labors begun before Dr. Kempster could possibly have even thought of commencing similar work. The author asserts that he has latterly become an advocate of the strictly somatic origin of insanity, thus virtually excluding the possible operation of all other causes. In this we perceive the influence of that coarser materialism to which the Utica school, of which the author before us is a pupil, has recently been converted.

One strikingly original observation in the paper, and with this we shall close our review, (which has grown lengthy, merely because the JOURNAL deems it its duty to thoroughly expose, once and for all, claims which are without foundation, and theories which have no basis) is that *stellate crystalline bodies with a nucleus and long processes*, occur in the insane brain. The author doubtless intended thus to describe the well-known "*Spinnen-zellen*" or "*Saft-zellen*" of Boll and Jastrowitz. If these are crystalline, then *adieu* to all our pathological and physiological traditions.

The Responsibility of the Insane for Their Criminal Acts. By Isaac Ray, M. D., Philadelphia. The Nestor of American forensic alienists has here enunciated those views with which most interested in the subject are familiar. We consequently forbear discussing them here, especially as an abstract of the author's views appeared in the last number of the JOURNAL. We see that Dr. Ray insists upon the possible separate existence of moral insanity, and that the attempt by Dr. Gray to sneer this down failed.

The Simulation of Insanity by the Insane. By C. H. Hughes, M. D. This is the only clinical contribution made in the section, and we congratulate the author upon having called attention to a much neglected subject, and one which, as the writer states, is of great forensic interest. Ignorance of the possible simulation of insanity by the insane, might lead the expert rashly to infer, on discovering such simulation, that the suspected individual must be sane! We have seen a far more marked case than the one detailed by Dr. Hughes of a criminal lunatic, with all the signs of degeneration enumerated by Ford, who feigned religious insanity. Pelman, in the "*Irrenfreund*, 1874, No. X.," first distinctly describes the simulation of insanity by the insane, and some of his cases resemble the case given by the author.

On the Best Mode of Providing for the Subjects of Chronic Insanity. By Charles F. Nichols, M. D., Superintendent of the Government Hospital for the Insane. The paper itself deals with purely administrative points, and we wish to call attention to one single matter alone, which was brought up in the discussion. The opinion was expressed, and apparently generally assented to, that it would be advantageous to provide separate asylums for the separate sexes, under different superintendents. Nothing could illustrate more fully the principles by which the members of this section are governed, than this point. A medical superintendent, who recognizes the fact that no alienist is a perfect alienist until he has studied insanity in both sexes, who is aware that their etiology, clinical history, and treatment of mental complaints, offer certain points of comparison and contrast, and who would wish to perfect himself in his clinical knowledge, could never assent to such a procedure.

And how is clinical instruction to be carried on, if the teacher derives his material from one single sex? Apparently the scientific element of insanity does not enter into the calculations of most medical superintendents.

E. C. SPITZKA.

SHORTER NOTICES.

I. Medical and Surgical Reports of the Boston City Hospital. Second series. Boston, 1877. Published by the Board of Trustees. Edited by David W. Cheever, M. D., and F. W. Draper, M. D. 316 pages.

II. Lectures on Fevers. By Alfred L. Loomis, A. M., M. D. New York: Wm. Wood & Co., 1877. 403 pages.

III. Nurse and Patient, and Camp Cure. By S. Weir Mitchell, M. D. (*Reprinted from Lippincott's Magazine.*) Philadelphia, 1877: J. B. Lippincott & Co. 73 pages.

IV. Cutaneous and Veneral Memoranda. By Henry G. Piffard, A. M., M. D., and George Henry Fox, A. M., M. D. New York: Wm. Wood & Co., 1877. 296 pages.

V. Ein Beitrag zur Aetologie der Epilepsie. Von Dr. Neftel in New York. (*Separatabdruck aus dem Archiv fuer. Psychiatrie.*) (*On the Actiology of Epilepsy.*) 16 pages.

I. This is a very handsomely gotten up volume, and contains, besides elaborate plans and descriptions of the hospital in its present completed condition, a number of papers of interest by the members of the medical and surgical staff. Among these are the following, which come within the special scope of our journal: "On Certain Diseases of the Nervous Centres," by Robert T. Edes, M. D.; "Diseases of the Brain in its Relation to Inflammations of the Ear," by J. Orne Green, M. D.; and "Sclerosis of the Spinal Cord," by S. G. Webber, M. D. The first of these is a collection of several clinical cases, rather briefly reported, of cerebral syphilis, tumor, and of sclerosis of the posterior columns of the spinal cord, and, except some of the first mentioned disorders, accompanied with autopsies, several of which are illustrated by plates. Dr. Green's paper is not a "casuistic" article, but he gives a discussion of the general subject of brain disease arising from purulent otitis, with a pretty fair statement of the present state of our knowledge on the subject. Dr. Webber narrates five cases of spinal and cerebro-spinal sclerosis, only one of which, however, is complete with the autopsy. One of the cases is diagnosed as of the amyotrophic lateral sclerosis of Charcot, the others are examples of the more familiar forms of locomotor ataxy and cerebro-spinal sclerosis, in one of which the clinical symptoms in the earlier stage suggests the diagnosis of tumor of the brain. The discussion of these cases, so far as it goes, is well done.

The volume, as a whole, is a very creditable one to the medical men who are its authors, and to the corporation under whose auspices it is published.

II. This work is the republication, with slight revisions, of a series of lectures delivered in the medical department of the University of the City of New York to the class of 1876-7. As they are here reproduced, as given to the students, they are plain, practical, and clearly expressed, and give in a permanent form the instructions of an able teacher. At the close of the work is an extended list of works relating to the subject, but this, since it does not include many articles in journals, cannot be considered anything like an exhaustive bibliography.

III. Messrs. Lippincott & Co. have reproduced and put in more permanent form in this little book, two papers by Dr. Weir Mitchell, both of which, and more particularly the first, are likely to be profitable reading for the general as well as the medical public. In the first, that entitled "Nurse and Patient," some of the essentials of the proper care of the sick are very well set forth, and in a style that should insure for the essay many readers who rarely look into print for edification rather than entertainment. The second article is not less good, and even more popular in its nature, and together they form a very attractive and readable and an undoubtedly useful *brochure*.

IV. Drs. Piffard and Fox have produced in convenient and very condensed form a collection of the principal practical points in relation to cutaneous and veneral diseases. It cannot of course supersede more extended and thorough works, from a study of which alone one can become even a well-posted general practitioner, and we would not wish in our notice to convey any idea that it can be in any way a substitute for such to the student or the physician. But as a collection of useful memoranda for handy reference, we can give it our recommendation.

V. This paper has appeared almost verbatim, in an English form, in the "Transactions of the International Medical Congress," held at Philadelphia year before last. It consists of the account and discussion of a case of epilepsy of traumatic origin, and the points made by the author are summed up in the following conclusions:

1. "From the above-described case the possibility has been demonstrated that from traumatic influences on the head, without injury to the brain, epilepsy may be produced in a perfectly healthy person free from any hereditary neuropathic predisposition.

2. "Further investigations are required for the demonstration of the fact that such injuries in childhood furnish the etiological factors of epilepsy in many, perhaps the great majority of cases."

Editorial Department.

DR. EDWARD H. CLARKE.

ON the evening of November 30th, Dr. Edward H. Clarke, one of the editors of this JOURNAL, closed his earthly career, dying from cancer of the rectum. For two years or more he bore his sufferings patiently, spoke hopefully, and though often in great pain, continued to take an active interest in professional studies.

He was born at Norton, Mass., in 1820. His father was a clergyman, the Rev. Pitt Clarke; his mother was Mary Jones Stimson, and Dr. Clarke was her youngest son. He graduated at Harvard in 1841, first in his class, and received the degree of M. D. from the University of Pennsylvania in 1846. In 1850 he assisted in starting the Boylston Medical School in Boston, for the purpose of affording medical students the advantages of a more thorough education. In 1855, he was appointed professor of materia medica at Harvard, and held that position until 1872, when he resigned. He was then chosen one of the board of overseers of the University, and held that office until his death.

He paid much attention to diseases of the ear, and was at first almost, if not quite, alone in that branch of medicine in Boston, but probably his greatest success was obtained in connection with obscure diseases of the nervous system.

As a physician, Dr. Clarke gained an eminence in the community such as few attain; he won the confidence and attachment of his patients, and his opinion was frequently sought in consultation by his colleagues. With an accurate knowledge of diseases and the action of drugs, he had also an intimate acquaintance with human nature.

As an instructor he was a favorite with the students. He had the rare faculty of giving interest to a comparatively dry and, of itself, uninteresting subject. His lectures were carefully prepared; his habit of making notes of new drugs, or of newly discovered properties in old drugs, and filing them away, gave him the power to make his lectures fresh and interesting. But not only drugs claimed his attention; he also considered the effects of food, clothing, ventilation and bathing.

His writings have not been numerous, the one which has obtained the widest circulation and excited the most discussion, both in this country and in England, is of comparatively recent date, "Sex in Education." After this followed "The Building of a Brain." During his sickness, he wrote an article as a contribution to the centennial history of American medicine. During the latter part of his sickness, he was engaged in psychological studies, in which he was much interested, having, during his extensive practice, seen many cases exhibiting curious psychological phenomena. Of more strictly medical papers, there have been comparatively few from his pen; one was a small brochure, written in conjunction with Dr. Robert Amory, upon the Action of the Bromides.

We close this brief notice of our colleague with a word of regret that it devolves upon us so soon to chronicle his decease; and tender sympathy for the daughter, so lately motherless, now doubly orphaned.

In the October number of the *British and Foreign Medico-Chirurgical Review*, its publishers insert a notice of its discontinuance, after a distinguished career, and in which they announce the opinion that the "day for quarterlies has gone by, and in the face of the daily and weekly periodicals, a quarterly, with its thoughtful articles and well-digested reviews, is no longer appreciated as formerly."

Much has been made of this statement by certain weekly medical periodicals. But we are not convinced of its correctness. We have no doubt but that a sufficiently large field re-

mains to quarterlies to justify, if not demand their continuance. and hence we see no reason for changing the form of our JOURNAL. We will say, further, that its continued existence is not dependent on its subscription list, and that it will be the steady aim of its editors to make it worthy of the support of the profession, and they feel confident whatever its form may be, it will in the course of time find a permanent sphere of usefulness. They have every reason to feel gratified at the favor with which it is received in the higher walks of the profession.

IN mentioning Dr. E. C. Spitzka as our correspondent in our last issue it was not intended to convey the idea that he is our regular New York neurological correspondent. That position has from the first been ably filled by Dr. George W. Wells.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

ANASTOMOSES OF GANGLIONIC CELLS.—J. Carriere *Arch. f. mikr Anat.* XIV. 125, abstr. in *Centralblatt*, No. 37, 1877) isolated the elements of the anterior spinal cornua, (of a calf of four weeks) in dilute solutions of biehromate of potassium and chromate of ammonium, and succeeded in finding seven specimens containing indubitable anastomoses of ganglionic cells. Cells of different sizes commensurate with each other, both of long processes and short bridges. Occasionally C. found a web-like expansion of the protoplasm between the separating branches of some ganglionic processes.

INDUCED CONTRACTION.—J. J. Friedrich (Note by Hering in *Weiner Acad. Sitzungsberichtr.* LXXIV., ‖413) has experimentally found, that the tetanus caused by the make and break of a constant current applied to the nerve will not induce a secondary tetanus in a sensitive nerve-muscle preparation. Still, the conclusion is not warranted that this mode of tetanus is a continuous process, as compared with the tetanus produced by interrupted irritation. For it is possible that the separate vibrations do not occur synchronously in the different fibres of the muscle, and hence produce interference. A *single* induced contraction (Zuckung) was often induced by the make—tetanus rarely by the break. In some cases a single induced contraction occurred during the closure of a powerful ascending current, which produced *no* contraction at all in the primary muscle. Occasionally Friedrich found also an induced contraction at the end of a make—tetanus on breaking or reversing the current.

Strychnia-tetanus of muscles of mammals and frogs caused, when tonic, an induced contraction but no tetanus; when clonic, several single contractions. Frequently, also, no induced contractions occurred at all. Similarly Hering observed that the tetanic contractions of the diaphragm induced a single initial contraction of the nerve-muscle preparation, but no tetanus. The heart induces only single contractions, but evidently this result does not determine whether the cardiac contractions are single or tetanic in nature.

Several experiments have been made by Morat and Toussaint in Bernard's laboratory, (*Comptes Rendus* No. 22, LXXXIII., No. 2 and 12)

showing that voluntary contraction of muscles of the frog induces either no secondary contraction at all, or a single initial or several single contractions.

On irritating the nerve of the primary muscle with an induced current, the interruptions of which are of exactly the frequency necessary to maintain a constant tetanus, the induced tetanus of the secondary muscle will not be perfectly constant. It becomes constant, however, on increasing the number of interruptions. On increasing the number still further, there occurs instead, in the secondary nerve-muscle preparation, a brief initial tetanus, or merely a single initial contraction. The same occurs when the separate contractions of the primary muscle are prolonged by fatigue. In the latter case the electric intensity of the primary muscle is weakened by the fusion of the separate negative variation due to each contraction. Like Friedrich, the authors also found the tetanus of constant currents accompanied by a single initial, rarely terminal induced contraction. (*Centralblatt*, No. 37, 1877.)

The Cerebral Convolutions.—Prof. Ad. Pansch mentions in the *Centralblatt*, (No. 36, 1877) the general results of several years researches on the cerebral convolutions.

I. The furrows and convolutions of the cerebrum are the manifestation of folds, the mode of origin of which is but partly known.

II. The arrangement of the folds follows certain laws, and is always the same in the same class of animals. According to the type of the cerebral convolutions, mammals can be divided into smaller and larger groups. Certain variations occur in every type, their number increasing with the complication of the type.

III. The first appearance of furrows on the smooth brain of the fœtus consists usually of short, sharp incisures. Their elongation is either direct or due to the fusing of adjacent furrows. Later furrows also arise as shallow indentations.

IV. Not all furrows originate simultaneously; their development requires periods of different length before and after birth, according to the species.

V. The furrows first in appearance are in any one class, or even in a larger group, almost always the same, and in the same position, (constant primary furrow). This is most strictly the case among the simpler types, less so among the more complicated ones. All furrows arising subsequently are subjected to greater variation in shape and position, the more the later their appearance, and the more complicated the type.

VI. The growth in depth of the furrows in the course of their development is a rather uniform process. The furrows last in appearance remain shallow, the earlier ones are always deep. Thus the history of development of the furrows can be traced in the adult brain by their depth.

VII. The growth of two convolutions separated by a furrow is usually uniform, and thus the walls of the furrow become perpendicular to the surface of the cortex. But irregularities also occur, so that either the

furrow remaining perpendicular, one convolution exceeds the other in heighth, and the cortex becomes uneven, or the cortex remaining smooth, the furrow assumes an oblique direction, and one convolution overtops the other, (formation of opercula). The latter process, as well as the union or non-union of separate furrows, give rise to the variations in the species or individuals.

VIII. A subsequent disappearance of existing furrows occurs but rarely, (and never after the fifth month).

On the strength of these theorems, the author insists on the following methods in the study of the brain:

1. The study of the cerebral convolutions must be accompanied by a consideration of their development. Especially is this requisite in the classification of the cortex. Where fœtal brains are not to be had, the study of the depth of the furrows can take their place.

2. The furrows and their depth are the first and most important object of every research and description. The primary and secondary convolutions thus come to be considered naturally as divisions of the cortex, limited and separated by primary, secondary or tertiary furrows. Deceptions due to the external superficial appearance of convolutions should be avoided.

3. Geometrical drawings ought to be made in large numbers. If they include the four sides of the specimen, they are a perfect substitute for the original. Artistic shading is not only superfluous, but even harmful; the depth, however, of the furrows ought to be a part of the drawing.

4. The next problem ought to be extended observations on the position, shape, and depth of the primary furrows. Only when this has been completed can the detailed investigation of the less constant secondary furrows and convolutions be of use. G.

The Motor Centres for the Members.—At the session of the Acad. de Médicine, Paris, Oct. 23 (reported in *Le Progrès Médical*). M. Bourdon read a paper entitled, "Clinical Researches on the Motor Centres for the Members." M. Gosselin, in his report on the memoirs of M. M. Lucas, Championniere, Proust and Terrillon, having raised the question of cerebral localization, M. Bourdon undertook some investigations on the subject, with the idea of testing by clinical observations the results of experiments made upon animals; his study was directed especially to the motor centres for the limbs. Having observed a very clear and precise case of brachial monoplegia, he compared it with all others as apparently conclusive to him, many of the cases never having been published.

He cited first his observation, reported by M. Darrolles, *interne* of the hospitals. An old man, of a very much deteriorated constitution, after a simple attack of vertigo, was suddenly affected with paralysis limited to the muscles of the forearm and hand of the right side, with preservation of the sensibility. Speech was quickly recovered; but the partial paralysis of the superior member persisted until death. At the autopsy,

there was found a very superficial small hemorrhagic *foyer* in the upper part of the ascending frontal convolution of the left hemisphere, and in the vicinity no lesion of the opto-striated bodies. In a second case, also unpublished, communicated by M. Verneuil, a mason, following a fall which produced a fracture of the skull, presented a paralysis of both arms, and at the autopsy there were found two *foyers* of meningo-encephalitis; one on the upper third of the left ascending frontal convolution, and the other on the right ascending parietal convolution. The author next reported a dozen observations of brachial monoplegia, described by different writers, and all accompanied with autopsies. In analyzing the nervous symptoms presented by the patients, there are found special characteristics of paralysis of cortical origin: disassociation, progressive march, successive appearance, instability of the paralytic phenomena, these always leaving the sensibility uninvolved, and almost never being accompanied with loss of consciousness As to the very various anatomical lesions, they were all located in the cortical motor region, but instead of occupying the upper third of the ascending frontal convolution of the superior two-thirds of the ascending parietal, that is, in the circumscribed region in which MM. Carville and Duret have located the motor centre for the arm in man, from their experiments upon animals; and in place of being situated on the middle third of the ascending frontal, as it should be, according to the localization proposed by MM. Charcot and Pitres in their recent memoir, these lesions were disseminated over the whole height of the two ascending convolutions and in their neighborhood. But, and this point is worthy of note, whenever there was paralysis of the face with that of the arms (six observations) a lesion existed in the middle or lower part of the ascending frontal convolution, that is, more or less near the second frontal convolution where is generally accorded the motor centre for the face. The region occupied by all these lesions, although rather extensive, appeared to constitute the true motor centre for the arm; only in getting these observations of cortical hemiplegia, M. Bourdon recognized that in the majority of cases, the anatomical alteration occupied the superior third of the ascending frontal, and the upper two-thirds of the ascending parietal convolutions, conformably to the localization adopted by MM. Charcot and Pitres, for the combined movements of the leg and arm; but in a good number of cases, the autopsy revealed lesions having the same locality in cases of only brachial monoplegia. The author then engages in the investigations whether, independently of its situation, certain characters of the anatomical alterations may not account for this difference in the paralytic phenomena. Contrary to what might be supposed, the facts did not show to him that the extension of the paralysis from the arm to the leg, had any relation with any increase of extent of the lesion either in surface or depth.

The study of brachial monoplegias not having given as satisfactory a result as he had expected, M. Bourdon sought to see whether amputations of the arm, with their necessarily very simple encephalic lesion, would not be better suited than cerebral affections to reveal the exact situation of the motor centres. If it be true, as from certain facts of arrest of devel-

opment we might be led to think, that the default of action of a member causes, after a while, an atrophy of that part of the brain which controls its movements, the author ought to be able to find in cases of old amputations, an atrophic lesion of the surface of the brain always occupying the same point, Unfortunately he was able to collect but two observations followed by autopsies; one by M. Chnquet, and the other by M. H. C. de Boyer, both *internes* of the hospitals.

Nevertheless in these cases there was found an atrophy occupying the upper part of the two ascending convolutions, that is to say, over a portion of the region occupied by the lesions of brachial monoplegia, and this augments the value of those lesions as regards localizations. In a second chapter M. Bourdon treats of the movements of the lower limbs for the purpose of finding whether in man there really exists a motor centre distinct from that for the arm, as experiments upon animals permit us to suppose to be the case; and he seeks to support this supposition by clinical facts of paralysis limited to one limb. But those recorded in the books are either not accompanied with the accounts of the autopsies or the location of the lesion is barely indicated. In lack of sufficiently conclusive cases of monoplegia, the author has collected three cases of amputation, and one of arrest of development of the lower limb. In the three first, observed by M. Luys, there was found an atrophy located in the upper part of the ascending frontal convolution; in the case of arrest of development, published by M. Landouzy, the atrophy occupied the upper portion of the ascending parietal convolution.

After giving his conclusions, readily deduced from the preceding facts, M. Bourdon closes with the following practical considerations. In consequence of the considerable extent of the motor centre for the arm, a paralysis limited to this member does not indicate with sufficient precision the part of the cranium to which the trephine should be applied. Nevertheless, if there is added to the brachial monoplegia a paralysis of the lower face, or an aphasia, we have, according to M. Bourdon, a great chance of meeting the lesion by operating over the middle portion of the Rolandian line, as recommended by M. Lucas-Championniere. As to the counsel given by that surgeon, to apply the trephine near the summit of the fissure of Rolando, in case of paralysis of the lower member, the observations cited tend to prove that it is well founded, but the case is otherwise when he advises operation behind the fissure, since the facts demonstrate that the anatomical alteration is more often anterior than posterior to the line which serves as a landmark for the surgeon.

THE REAL ORIGIN OF THE FACIAL.—At the session of the Soc. de Biologie Oct. 20, (rep. in *Le Progres Médical)* M. Duval communicated the results of his investigations as to the true point of origin of the facial nerve. In sections perpendicular to the axis of the medulla we trace the course of this nerve, beginning at its point of emergence, back toward the posterior extremity of the raphe; from there pass fibres which take a recurrent course towards a nucleus, the true nucleus of origin of the facial.

The facial, therefore, forms a horseshoe, of which the first and third portions are horizontal, and the second, more short, is vertical. The nucleus of the external motor oculi nerve is situated in the concavity of this horseshoe, and it gives some fibres to the facial. Finally, in sections parallel to the floor of the fourth ventricle, MM. Duval and Graux have been able to follow fibres going from this nucleus to radicular fibres of the motor oculi communis of the opposite side. In this way they explain the facts of functional paralysis of one internal rectus associated with paralysis of the external rectus of the opposite side. The third cranial pair has, therefore, a double source of innervation.

THE PHYSIOLOGY OF THE BRAIN.—Orchansky, *Inaug. Diss.*, St. Petersburg, 1877. (Abstr. in *St. Petersburger Med. Wochenschr*, No. 41). This dissertation comprises three parts: A historical sketch of the question of psycho-motor centres; a critical review of the doctrines regarding these centres; and the researches of the author, with his conclusions. From the historical *resumé*, it appears that experimenters have adopted different opinions, on account of the various methods (electric current, chromic acid, jets of water) employed. He lays down the following, after a thorough critical analysis of the views presented on the psycho-motor centres: 1. A physical localization of the electric current in special regions of the brain is to be considered as probable. 2. There is a great probability of the psychic organs in the surface of the brain, and of motor organs in the more deeply situated portions. 3. The character of the processes following irritation of the brain is ganglionic, *i. e*, central. 4. The improbability of a specific character and indivisibility of these centres 5. The obscurity of the method of transmission of the excitation from the psychic to the motor centres. 6. The separation of the cortex into motor and non-motor parts, rests probably upon an anatomical basis, but is still little known. 7. The cause of the presence of non-motor sections with the motor centres in the motor regions is still unknown.

The author's own experiments were performed on dogs and rabbits. For the solution of the question of the possibility of the localization of the electric current in the brain, the author applied the needle-shaped platinum electrodes to the brain itself, at the distance of from two to four millimetres from each other, and he found that with a weak current, (20-30 mm slide of the helix) with close approach of the electrodes to each other, and superficial penetration of the electrodes into the brain-substance, the current could be well localized. A repetition of Hitzig's and Ferrier's experiments gave the following results: A gentle, careful contact of the particular points of the cortex with small blunt electrodes caused muscular contractions; that the motor zone is better isolated in dogs than in cats and rabbits and that each excitable point is surrounded by a less excitable tract. The author found in rabbits, points irritation of which produced homolateral contractions. (Hitzig found only contralateral and bilateral ones). He noticed contractions of the cutaneous muscles at the same time with those of the trunk, and likewise a direct relation between the strength

of the contraction and the superficies of the metallic closing by the Neef hammer. The greater this was, the stronger was the contraction, the closures must, however, follow each other rather rapidly to produce this effect. The distinction of the motor zone depends, according to the author's opinion, upon the difference in thickness of the gray cortical substance, which is thinner in the furrows for the vessels of the pia, and those parts not covered by visible vessels are the ones, according to Hitzig, in which are situated the psychomotor centres. Besides it thus happens that the fibres of the white substance, which always stand perpendicular to the gray, come to lie, in relation to the superficies of the brain, at various angles of 0° to 90°; and then, according to Dubois Reymond and Bernstein, the current is so much the weaker in its action on the nerve as the angle formed by the two approaches a right angle, so it must be accordingly more ineffective in those parts of the cerebral convolutions where the cortical layer is thickest.

For the answer to the question as to the mode of transmission of the nerve irritation from the so-called centres, the author undertook a series of experiments, and came to the conclusion that this transmission took place, when weak or medium currents were employed, in a physiological way through the ganglion cells, but that with strong currents the possibility of a direct excitation of the white nerve fibres could not be excluded.

The Development of the Nerves in Vertebrates.—Mr. Balfour's discovery that the spinal nerves of sharks and rays are developed as outgrowths from the central nervous system has been followed by a similar revelation with regard to birds. Mr. (now Dr.) A. M. Marshall (of Cambridge) has given an account of investigations respecting the origin of nerves in the fowl (*Jour. Anat.*, Apl., 1877), describing a longitudinal ridge arising on the summit of the neural canal, and giving off paired processes, the rudiments of the posterior roots of the spinal nerves. Hensen has made analogous observations on the spinal nerves of the rabbit. The anterior roots arise later, distinct from one another, as processes from the spinal cord. Mr. Balfour has endeavored to solve the difficult question of the relations of the cranial to spinal nerves. He finds, as yet, no traces in the brain of anything comparable to anterior roots of nerves; all the nerves are posterior roots. The fifth, or trigeminal, arises from the dorsal summit of the hind brain very early, just like the dorsal root of a spinal nerve. This nerve also, instead of being a compound one, is, at any rate in its origin perfectly simple. The auditory nerve and the facial arise by one common root. The glosso-pharyngeal and vagus have a series of distinct roots. This number, and their origin like so many separate spinal nerves, opens up interesting questions in regard to the primitive segmentation of the head, and the loss or condensation of segments in the evolution of the vertebrates. Dr. Marshall's observations on the cranial nerves of the chick, so far as they go, correspond to Mr. Balfour's. It appears that there is no definite indication of a limit between head and trunk afforded by the central nervous cord, by the outgrowths from it, or by the mode of

development of the nerves. It is open for consideration whether the absence of anterior roots to the cranial nerves may not furnish such a limit. this would be very convenient for morphology. *Nature*, Aug. 30, 1877.

Cerebral Localizations.—The following are the conclusions of a communication by Dr. W. H. Broadbent, of London, to the International Medical Congress at Geneva, Switzerland, (Sept. 9–15) as reported in the *Gaz des Hopitaux*, No. 130, 1877:

1. *Motor zone of the cerebral cortex.* 1. The physiological experiments, based either upon electrical excitation or upon destruction or ablation, practised by Hitzig, Ferrier, Carville and Duret, Nothnagel and others, have established the fact that there exists a certain zone in the cortical portion of the cerebral hemispheres, which is in intimate relation with the nuclei of the motor nerves, of the medulla and spinal cord.

This zone is situated in the ape and in man, around the fissure of Rolando, and principally in the two marginal or ascending convolutions which border on this groove.

At this horizon exist regions, more or less defined and circumscribed, specially related to the leg, the arm, the face, etc.

2. The pathology of this zone accords in a very remarkable manner with the results of physiological experimentation.

Partial or irritant lesions may produce partial epilepsy, with or without transient or permanent hemiplegia.

More extensive lesions, with more or less complete destruction of the gray cortical substance, may cause a monoplegia or hemiplegia.

3. While admitting the localization of a motor zone, and of motor centres on the surface of the hemisphere, it ought to be added that the communication between the cells of this region and those of the anterior horns of gray matter cannot be a direct one.

The co-ordinated character and the sequence or succession of the movements which are provoked by the electrical excitation, the character of the paralysis produced by the ablation of a cortical centre, its intermittence, short duration, the tendency of the partial symptomatic epilepsy to commence in the hand, although the situation of the lesion may be different, the loss of time in the transmission of an electric excitation, are so many reasons for the supposition of a ganglion between the cerebral cortex, and the roots of the motor nerves.

The cortical motor centres are the points of departure for the descending voluntary impulses, the starting places for ideo-motor actions. The cells of the motor zone (which resemble those of the anterior horns of the cord) are the apparatuses by which the dictates of the intelligence are formulated for expression or for transmission outwards.

The best example of the action of a motor centre is afforded by the centre for speech, which was the first and the most important of the cerebral localizations. Aphasia without paralysis of the lips or tongue, is an example of what we call vicarious supply, or functional substitution, although as such it is incomplete.

4. According to the experiments of Ferrier and Hitzig, there exists in the vertical portion of the cortex a centre for perception.

This zone extends from the angular gyrus, which is the visual perceptive centre, along the sub-sylvian convolution of the temporo-sphenoidal lobe (in which is found the centre for audition) to the point of this lobe.

Thence the zone returns through the lobuli and the unciform gyrus, of which it occupies the internal border, towards the occipital lobe in which it probably occupies the calcarine gyrus.

Pathology has not yet furnished precise indications in regard to this question. We know, nevertheless, that complete destruction of the occipital lobe is without effect on sensibility.

5. The convolutions of the frontal and occipital lobes, those of the insula, those of the internal face of the hemispheres, those of the suborbital lobule, do not respond to electrical excitations. Lesions of these parts of the cortex do not have constant results, and are, for the most part, without effect.

The ablation of the two frontal lobes appears to diminish the spontaneity and activity of the intelligence; that of the occipital extremity of the hemispheres seems to abolish the appetite, but these effects do not afford any satisfactory idea of the functions of these lobes.

6. There appears to be no special cerebral vaso-motor centre; the vaso-motor apparatus is in relation with the general motor system, and, like it, has its centres in the cord, in the central ganglia and in the convolutions, these centres are subordinate one to another.

In the same way there are neither centres nor special trophic nerves. Trophic influence is common to the whole nervous system.

II. *Opto-striate bodies.* 1. The corpus striatum certainly exercises a motor function; a great part of the motor fibres of the foot of the cerebral peduncle terminate in this ganglion. Faradic excitation of the corpus striatum causes general contraction of all the muscles of the opposite side of the body; its experimental or pathological destruction causes hemiplegia.

It is, according to the author, the intermediary between the hemisphere and the cord. Its cells, grouped and combined by the sensory-motor education, are the apparatuses by which the cortico-motor centres transmit their orders to the muscles.

2. The optic thalamus is an intermediary ganglion for the transmission of sensory impressions from the nuclei of the sensory nerves to the perceptive centres of the cerebral cortex. In spite of the numerous objections that may be raised, this interpretation has in its favor the results of experimentation and of pathological anatomy. If the localization of the perceptive centres is correct, it is anatomically impossible, according to the researches of the author, that there should be any other route of communication between the hemispherical bundles than through the optic thalamus. The facts of hemianæsthesia are perfectly in accord with this hypothesis.

The opto-striate bodies are therefore instruments of the hemispheres, and also an automatic apparatus.

As a general interpretation of symptoms, furnished by this manner of view, we may say that:

(*a*) *Paralysis* is due to a rupture, either of cells or fibres, belonging to the mechanism of the motor nervous apparatus.

(*b*) *Anæsthesia* is due to a rupture in the sensory nervous mechanism.

(*c*) *Tremor* is the effect of a hindrance in the conductibility of the white fibres.

(*d*) *Convulsions* (including those of chorea) are the result of an irritation of the gray substance.

(*e*) *Precocious* and *transitory contractures* are the effect of pressure on a ganglion.

Since the entire nervous system is a vast mechanism of cells and fibres, its functional action ought to be described in terms of cells and fibres, we thus avoid the ambiguities so hurtful to progress, and obtain a true physiological localization.

In some remarks on Dr. Broadbent's communication, Prof. Schiff stated his opinion that the true motor centres are not directly excitable by electrical, chemical, or mechanical irritation, and that the only means to determine the function of any part of the brain was by extirpation. He had extirpated the motor centre for the face in dogs; the movements were preserved, but tactile sensibility was abolished, and from the phenomena here observed he concluded that the so-called motor centres were only centres for tactile sensibility. He had made this the subject of experiments, the results of which he had given in the second edition of his *Lecons sur l'Encephale.* These conclusions are perhaps not applicable to man, who, with his superior organization, may perhaps have motor centres on the surface of the brain.

The Influence of Centrifugal Action on the Circulation of the Brain.—M. Salathé reported to a recent meeting of the Paris Academy of Sciences, the results of some experiments performed by him to test the effects of position and centrifugal forces on the condition of the blood supply of the brain in rabbits, He found that if he suspended a rabbit, head downward, even for many hours, death did not ensue, but it was certain to follow rapidly the suspension of the animal in the opposite position, with the head upward. Death was due in this case to cerebral anæmia.

If the rabbit was placed on a rapidly turning plane, it succumbed to either cerebral congestion or anæmia, according as its head was directed toward the periphery or toward the centre of the turning plane. If it was fastened laterally, no serious consequences followed.

The following are the titles of some other recent papers on the Anatomy and Physiology of the Nervous System:

Lautenbach, On the Functions of the Cerebral Lobes, *Am. Jour. Med. Science*, October, 1877; Walsh, The Anatomy of the Brachial Plexus,

Ibid; OUDIN, Perforation of the Cranium by Gun-shot Wound, Experiments on the Cerebral Pulsations, *Revue Mensuelle*, December, 1877; STRUMPELL, On the Theory of Sleep; *Pflugers Archiv.* L. XI., Oct. 23, 1877; NOTHNAGEL, Experimental Researches on the Functions of the Brain; VIRCHOW'S *Archiv.* LXXI. III; KUESSNER, Vaso-motor Centres in the Cerebral Cortex, *Centralbl.f. d. Med. Wissensch.* No. 45, 1877; RETZIUS, Contribution to our Knowledge of the Nerve Tubes in the Plagiostomes, *Nordiskt Med. Archiv.* No. 23; SEELIGMUELLER, On the Study of the Motor Functions of the Cortex, and its Clinical Utilization, *Deutsche Med. Wochenschr.*, Nos. 47 and 48; OTT, The Inhibitory Function of the Spinal Cord, *Philadelphia Med. Times*, Dec. 8; MAJOR, The Structure of the Island of Reil in Apes, *Lancet*, (Am. repr.) October, 1877.

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

OPHTHALMOSCOPIC APPEARANCES IN INSANITY.—Ophthalmoscopic examination in insanity was made by Klein, *Wien. Med. Presse*, 1877; No. 3. (Abstr. in *Centralblatt*, No. 29,) in 134 patients, of whom 42 were afflicted with general paresis, 19 with mania, 19 with epilepsy, 17 with alcoholism, 4 with apoplexy, 6 with melancholia, 1 with ataxia, and 26 with divers forms. In 89 cases positive results were obtained, which he divides into two categories, one of 31 and the other of 58 cases. In the first class K found 9 times retinitis, 8 times discoloration of the optic nerve, 6 times atrophy, and 8 times hyperæmia of the retina. The second class consisted of 29 congenital anomalies, and 29 cases of a peculiar opacity resembling the senile metamorphosis of the retina. Since this change was found in 18 cases out of 42 of general paresis, K. terms it *retinitis paralytica*. It was also found, however, though more rarely, in other forms of insanity. During an epileptic attack, K. observed retinal ischæmia and clonic spasm of the iris.

MYELITIS,—At the session of the International Medical Congress in Geneva, in September last (rep. in *La France Médicale*), M. Proust, of Paris, presented in his own name and that of M. Joffroy, a memoir entitled "Contribution to the Study of Acute Myelitis," an observation of a case of apoplectiform onset, apparently succeeding a fall, which was really only its first symptom. He summed up in the following conclusions:

1. In a nosological point of view, myelitis frequently presents an abrupt beginning, which we at times attribute to spinal hemorrhage.

2. In a medico-chirurgical point of view, the fall which is sometimes the first symptom of the myelitis with apoplectiform commencement, may cause a more considerable *role* to be attributed to traumatism than is its due. Cases of this kind have been considered as commotions and contusions of the cord.

3. In a histological point of view, acute myelitis is characterized by alterations of all the elements, cells, and nerve-tubes, vessels, and conjunctive tissue. As regards the nerve-tube, the inflammation is characterized by hypertrophy of the cylinder axis; as regards the cell, sometimes by hypertrophy, sometimes by atrophy with abundant deposit of pigmentary granulations in the protoplasm. The proliferation of the nuclei of the connective tissue, rather abundant in the gray substance, takes only a small part in the inflammatory lesions of the white substance. Myelitis is, therefore, parenchymatous as well as interstitial.

In the discussion which followed, several of the speakers disagreed with M. Proust,—regarding traumatism as a more frequent cause of the affection than he had allowed, and considering it needful to use much reserve in admitting the existence of acute spontaneous myelitis.

Aphasia with Intermittent Right Syphilitic Hemiplegia.—M. Ch. Mauriac *Brochure.* Paris, 1877, (rev. in *La France Medicale.*) The diagnosis of aphasia with intermittent right hemiplegia of syphilitic origin, offers many points to be cleared up, and first, that of the seat of the lesion. According to M. Mauriac, the meninges are invaded by a gummatous syphilitic hyperplasia at the horizon of the left frontal convolutions and in the fissure of Sylvius. This hyperplasia compresses, invades, produces congestion in, and finally softens the convolutions and lobule of the insula, as well as the external portion of the gray substance of the striated body. Hence the disorder of the faculty of speech which appears to be localized more particularly in the third left frontal convolution, and the right hemiplegia consecutive to the lesion of the insula and the corpus striatum.

In a review by Tarnowsky of fifty-three cases of syphilitic aphasia, the aphasia coincided eighteen times with a right and fourteen times with a left hemiplegia. M. Mauriac finds these fourteen cases exceptional, but does not seek to give any explanation of them. Nevertheless, says he, it is not necessary to assume the probable syphilitic character of an aphasia accompanied with left-sided hemiplegia.

A more capital difference between the coincidences of syphilitic aphasia and those of non-syphilitic origin, consists in the dissemination, the irregular scattering, of the most various nervous disturbances.

In one patient observed by the author, the aphasia and the right hemiplegia were intermittent. They occurred in crises, four, five, or six times a day, at varying intervals, and lasting on an average four or five minutes, and this without any irradiation over other portions of the nervous system. The cerebral affection remained thus intermittent during one month, finally becoming continuous. This intermittence depends, without doubt,

on the variations of volume undergone by the syphilitic meningo-cerebral lesion, before the disorganization of the brain matter itself had taken place.

Does mercury cause a predisposition to the nervous accidents of syphilis, as the statistics of Dr. Jullien appear to indicate? M. Mauriac considers this conclusion an extreme one, and objects to such figures. He adds, nevertheless, that all his patients received mercurial treatment during the earlier stages of the disease.

The work is especially a contribution to the study of cerebral localization.

Clitrophobia.—Dr. Antigono Raggi, *Rivista Clinica de Bologna*, Sept. 1877, proposes the name clitrophobia for a condition analogous to the agoraphobia of Westphal, but in which the subject has no fear of wide spaces, but dreads the opposite, narrow, confined localities. He describes three cases of the disease.

Moral Insanity.—Moritz Gauster, *Wiener Klinik* III., iv., (abstr. by M. Labadie-Lagrave in *Rev. des Sci. Méd.*) We have learned to designate as "moral insanity" a complex of symptoms, recognized as a morbid entity only when it has lasted over a number of years.

Esquirol and Gratmann, and after them Pritchard, Morel, Solbrig, Kraffi-Ebing, Maudsley, etc., have called attention to the congenital psychic degeneration in certain individuals. Pinel ranges these cases in the *mania raisonnante*. Pritchard, in 1842, was the first who gave to this affection the name "moral insanity." Morel called attention to the point that the instinctive immorality is not always the result of degeneration, but that it may be occasioned by hypochondria or by vicious habits.

The morbid states in question have been badly classified; they have been considered as insanity without ideas of greatness, or as monomania, an emotional aberration, a form of dementia in which the intellectual sphere is hardly involved, or in appearance only; finally, as insanity affecting only the feelings and the will.

In close examination of these cases we find that the most striking symptoms, and those on which the diagnosis is based, are the following:

The patients are often morally perverted from their infancy, they are headstrong, malicious, disobedient, irascible to a very high degree, lying, and neglectful. They frequently manifest a tendency to violence and brutality.

In the adults we find a great aptitude to certain careers, and particularly a marked vocation to mechanical pursuits. They often speak and act in a sensible manner, and we consider them as being of sound mind. But we learn that they delight in intrigue and mischief, that they give themselves suddenly to excess in drink and venery, that they are extremely excitable and passionate, and that they attribute their excesses and passion to others, especially to their parents and those whom they maltreat. They have an inclination to pass themselves off as heroes and martyrs.

Their explanations often appear logical, but close examination reveals that in reality their judgment is much enfeebled. They are frequently hy-

pochondriacs, and sooner or later there is manifested in them a mania of persecutions.

In an anthropological point of view, this degeneration reveals itself frequently by very evident signs, such as irregular development of the cranium, asymmetry of the face, very large or small ears, adherent lobules of the ear, rudimentary dental dispositions, etc.

Hereditary antecedents are also frequently ascertained.

In a purely pathological point of view, we find deformities of the skull, hydrocephalus, lasting or transitory paretic manifestations on the part of the facial nerves, convulsive manifestations like epilepsy or epileptoid states, disorders of pupillary innervation, etc.

The diagnostic points vary according as we have to do with the acquired or the congenital form. In the latter case we note that in the infancy of the patient those about him complain of his indocility and his irascibility, circumstances which the physician should make use of. In general we will observe psychic or purely nervous troubles in the ascendant. In the infant itself we will see either deformities or an extreme irascibility, convulsive states, spasms of the face coming on under the influence of the least excitement, attacks of extreme excitability without any cause, agitation, etc.

The prognosis is in all cases very unfavorable. We can at best only hope to calm the super-excitable condition of the patient and accustom him to obtain greater control over himself. For this purpose he should, above all, be kept, when possible, from the disastrous effects of alcoholic and venereal excesses. He should be kept engaged in appropriate occupation, and taken away from his associates.

In cases with epileptiform attacks, we sometimes obtain good results from bromide of potassium and narcotics. Finally, we render a great service, both to these patients themselves and to society, by placing them as promptly as possible in an insane asylum.

The Pathological Histology of General Paralysis.—Dr. C. Laufenauer, *Centralblatt f. d. Med. Wissenschaften*, No. 37, 1877. As regards the pathologico-histological discoveries in dementia paralytica, they have been rather thoroughly treated by many authors, and their results in turn have been confirmed by many others. It would be a laborious work to merely recapitulate them. But in regard to the regions of the pons and medulla there has been astonishingly little reported, while the conditions in the brain and cord have formed the basis of many investigations. This great deficiency—let us say, of negative pathological findings—in the massed collection of motor and sensory nuclei in the pons and the medulla, is the more remarkable, since paralytic conditions of the motor and sensory apparatus are hardly ever absent as cardinal symptoms of dementia paralytica.

Meynert attempts to explain this fact, in that, pointing out the indisputable connection of the oculomotorius, trochlearis, abducens, facialis and hypoglossus with the raphe, he considers these fibres as crossed voli-

tional routes leading to more highly situated brain centres, consequently if certain brain tracts are diseased, irritative phenomena, hemiparesis or total paralysis may occur in these routes of voluntary conduction without any pathological alterations in the nerve nuclei, an analogous condition to that after amputation of a foot in adults, in which case disease of the nerve cells of the gray spinal substance has never been found.

Observations of actual disorder of the nerve nuclei of the pons and medulla have thus far been extremely rare. Lubimoff, that industrious investigator of the brain in general paralysis, found affections of the nuclei of the facial and hypoglossus in two cases, but did not give a detailed description. Besides this, he observed a formation of connective tissue around the olive, (*Virchow's Archiv.*, LVII.) Jessen (*Cbl.* 1877, 225) and according to his statement, Liouville reports similar discoveries.

The following pathological appearances which I met with in one case, in the course of a purely anatomical preparation of several brains of paralytics in Prof. Meynert's laboratory, permits me to increase by one case the above stated pathological *Casuistic* of general paralysis.

The patient in question presented during life the characteristic symptoms of the disease, together with a total paralysis of the left and a slight hemiparesis of the right facial; paralysis of the right and insufficiency of the left abducens, and a very decided disturbance of speech. After hardening and separation of the pons and medulla into transparent carmine stained sections, I found the following pathological alteration: In the left facialis-abducens nucleus, the protoplasm of the nerve nuclei was diseased, had lost its capacity to imbibe carmine, causing many of the nuclei to appear spotted; the processes were brittle. But the greater part was in highest degree sclerosed and atrophied; certain nerve nuclei exhibited the yellow atrophy described by Charcot. The facialis-abducens nucleus was particularly deficient in cells, and crossed by strong connective tissue septa. In the lower facialis nucleus existed similar alterations. The outgoing root fibres were atrophied. In the right facialis nucleus the same alterations existed as in the left. In other regions of the pons were massed granulations.

The two nuclei of the hypoglossus exhibited a still more marked sclerosis and atrophy of nerve cells than in those described, together with Charcot's yellow atrophy of certain cells, and notable proliferation of connective tissue. In other regions of the medulla were granular bodies. In the gray floor were frequent remains of capillary hemorrhages.

These remarkable pathological alterations stand in glaring contrast to the other negative findings in general paralytics.

It appears, therefore, that in this case of progressive paralysis, we have to do with a central disorder of the nerve nuclei described.

Lucid Intervals in Insanity.—Bigot, *Brochure*, Paris, 1877, (abstr. in *Revue des Sciences Medicales*). This memoir is devoted to the analysis of the equivocal states, which vary but little from perfect sanity, but which

come under the category of and represent one phase of mental alienation. This so-called reasoning insanity is not properly an insanity, but is one of the necessary periods of mental trouble, whatever it may be or become. There also exists, according to the author, a stage of incubation in which the insanity is not yet characterized, and in which it commences with incomplete manifestations.

This reasoning vesanic disorder marks the evolution or involution of every classified insanity, and it may exist alone and disappear before the appearance of the ordinary attack.

Bigot divides this *folie raisonnante* into lucid, para-lucid, and pseudo-lucid forms.

Lucidity. The patient is more or less clearly aware of the morbid conception; if he conceals it we are unable to detect it, if he acknowledges it, it is in such a way that we hesitate to believe him insane.

Pseudo-lucidity. The patient is just conscious of his insanity. He accepts the insane notion, but understands that it is for his interest to hide it.

Para-lucidity. The patient does not conceal his delusion in which he maintains an obstinate faith, to spare himself from *ennui*.

Bigot also classifies those predisposed. He distinguishes intellectual and moral anomalies, of which he separates two types (he insufficient and the unstable) from that which he calls the direct forms, passional anomalies or manias, and the indirect or oblique mental disorders.

En resumé, says the author, there are reasoning maniacs, there is no reasoning insanity. The definite attack is frequently slow or brief, and this gives a great value to the intermediate states. Those patients who remain very long in an uncertain mental condition are most frequently abnormal forms of insanity. There are prolonged lucid intervals in every kind of mental alienation; there are good, bad, and indifferent. Between reason and confirmed insanity there is every shade of reasoning power; *natura non facit saltus*.

Dementia Paralytica.—W. Jessen (*Centralblatt*, 31st March, 1877) examined microscopically the pons and medulla in two cases of dementia paralytica, although no lesions were seen with the unaided eye. The morbid changes were considerable, especially in the region of the inferior decussation of the pyramids. They consisted in proliferation of the epithelium lining the central canal, and in destruction of the nerve-fibres and ganglionic cells.

Optic Neuritis in Acute Infantile Meningitis.—Dr. H. Parinaud, *Brochure*, Paris, 1877, (noticed in *Gaz. des Hopitaux*). *Conclusions.* Optic neuritis in the acute meningitis of infancy has all the clinical and anatomical characters of strangulated neuritis, such as we observe in the different conditions in which the intra-cranial pressure is increased. It is

not the result of any inflammatory alterations affecting the optic nerves in their intra-cranial course, but is due to the hydrocephalus, which is a frequent complication of acute meningitis, and which always accompanies the neuritis. The œdema of the optic nerve that characterizes the alteration, improperly designated neuritis, appears to us to be of the same nature as the cerebral œdema observed under the same conditions, and produced by an obstruction to the lymphatic circulation.

Senile Tremor.—At the session of the Soc. Méd. des Hopitaux, July 13, (rep. in *L'Union Médicale*), M. Luys offered a communication in regard to that special form of trembling which is usually described as peculiar to old age, and is known under the title of senile tremor. He maintained that this as a distinct form, and as peculiar to old age, did not really exist; in ten years careful observation of senile pathology in his service, both in the Bicetre and in the Salpetriére, and he had never observed any tremor in healthy old persons. He considered that senility was by no means necessarily accompanied with tremor. He had had, among others, under observation, a woman ninety-nine years of age who showed no signs of tremor. He attributed the partial tremors which existed in certain cases in the muscles of the hand and those of the neck, to incomplete forms of paralysis agitans, and to localized sclerosis in certain portions of the medulla or the pons.

The Head Symptoms in Locomotor Ataxy.—The following is the analysis of a recent memoir by Dr. A. Pierret (52 pp. Paris, J. B. Bailliere 1877) as given in *La France Médicale*, No. 79, 1877.

For many years M. Pierret has studied locomotor ataxy in point of view of localization. He had already reached the result that the posterior column of the cord, in man, must be divided into two regions, physiologically distinct, the median bundle and the radicular zones. To the latter appertain all the tabetic phenomena, properly speaking, the fulgurant pains, the anæsthesia, and the inco-ordination of movement; to the former, a sort of flaccid paralysis of the lower members, rendering the upright position difficult or impossible. Locomotor ataxia is, therefore, a symptom, a systematic affection, in the sense that it attacks certain parts of the cord constituting an anatomical system, that of the sensory fibres. It may arise at any part of this system whatever, it is therefore a good idea to seek to find in the entirety of the nervous system those regions which physiologically represent the posterior radicular zones of the spinal axis. For this purpose M. Pierret has given in this *brochure* an anatomical and clinical study of the trigeminus.

The trigeminus nerve, in its soft or sensory portion, must be considered as representing the posterior roots of nearly all the motor nerves of the face. Indeed, as M. Pierret shows, this sensory root takes its rise on the prolongation of the posterior radicular zones of the spinal cord. On the

other hand, we find, in a certain number of cases, functional lesions of the trigeminus in the course of locomotor ataxy. This nerve acts in all ways like a posterior root.

The cephalic disorders of locomotor ataxy, depend either upon sensibility or motility; the author studies successively these two orders of symptoms.

The pains of the face are of the fulgurant and persistent and continuous types. The orbital branches are the ones most frequently involved. The pains often leave after them a zone of hyperæsthesia. The continuous pain is the most frequent, it exists around the orbits; when more generalized it may simulate a hemicrania, so much the more in that it is accompanied with photophobia, flow of tears and vaso-motor disorders. The eye itself is not free from these pains, and certain patients have asked that theirs be extirpated. In certain cases there exists at the same time anæsthesia of the mucous membrane and of the skin.

The paralysis and pseudo-paralysis which show themselves in the course of locomotor ataxy may affect all the muscles; they are transitory or little pronounced. The author notices in this connection how the integrity of the centripetal system is essential for the good execution of the movements of the life of relation, and also how direct are the relations between the posterior, spinal or bulbar (trigeminal) roots and the corresponding motor roots (cranial nerves). But the original cause of locomotor ataxia appears to be an irritation located primitively in the sensory fibres of the nervous system; we ought not therefore to be astonished at the appearance of paralysis of sensory origin in the domain of the motor nerves of the trigeminal group.

We may also observe motor inco-ordination of the muscles innervated by the motor branches of the trigeminal group.

Paralyses are more frequent in the muscles of the members or in those of the eye, but, on account of their locality and their frequently transitory nature, they are sometimes difficult to observe. It is therefore an error to suppose that ataxia of movement is independent of all paralysis.

The one that is left of all the theories proposed for the explanation of the co-ordination of movements is the law of Duchenne on the *role* of antagonist muscles. In every muscular movement the forces may be reduced to two, one of which produces the movement, and the other moderates it, and if the movement becomes irregular and exaggerated, it is because one of the two antagonists acts too little, whether it acts on the bone, the eyes, or the skin.

The following are among the recently published papers on the Pathology of the Nervous System and Mind, and their Pathological Anatomy:

Bull, Choked Disk following Injuries to the Head, *Am. Jour. Med. Sci.* October, 1877; Mills, Spasmodic Torticollis; Pitres, Contribution to the Study of the Anomalies of Sclerosis in Disseminated Patches, *Revue*

Mensuelle, December, 1877; LEPINE, Note in regard to Cerebral Glosso-Labio-Laryngeal Paralysis of the Pseudo Bulbar Form, *Ibid;* LANGE, Athetosis, *Hospitals Tidende* Nos. 47 and 48; FRITSCH, On the Question of Primary Chronic Mania, (*Verrücktheit*) *Psychiatrisches Centralblatt*, Oct. 1877; RUSSELL-REYNOLDS, Some Affections of the Nervous System Dependent upon a Gouty Habit, *Brit. Med. Journal*, Dec. 15; BACON, Case of Athetosis, *Ibid;* HOLLIS, On Sleeplessness and its Treatment, *Practitioner*, December; DURET, Cerebral Commotion, *Le Progres Médical*, No. 50, 1877: CHERON, Chemical Modifications of the Urine in Paralysis Agitans, *Ibid*, No. 48; DURET, On the Pathological Physiology of Cerebral Traumatisms, *Ibid*, No. 49; NIXON, Concussion of the Brain, *Pacific Med. and Surg. Journal*, December, 1877; WOOD, Chorea: Its Etiology and Varieties, with a New Treatment, *N. Y. Med. Record*, Dec. 15.

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c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

ACTION OF ANÆSTHETICS.—H. Ranke (*Centralblatt*, 25th Aug., 1877,) mentions his older experiments in which he anticipated the recent results of Binz, that certain hypnotics, morphine, chloral, ether, and chloroform produce a coagulation in the cerebral ganglion-cells. Experimenting in 1867 with chloroform, ether, and amylene, he had found that during prolonged narcosis the irritability is lost first in the nerve, subsequently in the muscle, and that finally rigidity occurs. He had also observed that the vapor of these anæsthetics coagulates solutions of myosine and cerebral albuminoids. Differing from Binz, he could coagulate neither the ganglion-cell nor a solution of it by means of morphine.

More recent experiments have shown that rigidity is produced in muscles by injections into the artery of ether, amylene, bromoform, chloroform, and solutions of bromal and chloral. Comparative injections of water, various salts, alcohol, etc., did not coagulate the living muscle. Ranke, therefore, advances the hypothesis that the narcotic effects of these substances depend on a similar temporary coagulation produced in the ganglion-cells. (No mention is made that the same hypothesis is maintained since many years by Cl. Bernard on similar grounds.]

INJURIOUS EFFECTS FROM ELECTRIC TREATMENT.—Dr. D. F. Lincoln (*Boston Med. and Surg. Journal*) calls attention to the effects that are sometimes produced by electrical treatment in aggravating certain symptoms and producing new ones, indicating an occasional injurious action. This injurious effect he attributes to over-stimulation of the nerves and ganglion, the action of electricity being generally stimulant to the nervous system. He gives several cases in which this injurious effect followed electric treatment.

The undesirable symptoms which are sometimes noticed during the application of electricity may not be injurious, however uncomfortable they are for the time—they do not, Dr. Lincoln thinks, absolutely indicate that the treatment is not doing good.

Salicylate of Soda.—MM. Bochefontaine and Chabert have experimented upon the physiological action of salicylate of soda, and recently reported their results to the Acad. des Sciences, Paris. According to these investigators, it is neither a cardiac nor a muscular poison, nor does it act on the extremities of the nerves. Its action appears to be limited to the gray substance.

Action of Pilocarpine and Atropine on the Sweat Glands.—Dr. B. Luchsinger (Pfluger's Archiv., Bd. 15, S. 482,) has continued his researches on the mechanism of perspiration in the cat. Pilocarpine, the alkaloid of jaborandi injected subcutaneously in the dose of 0.01 grm. (1-7 grain), causes violent perspiration of the paws. That this is in part due to stimulation of the nerve-terminations is proven by the persistence of sweating after section of the sciatic nerve. If, however, a sufficient time has elapsed since the section to permit complete degeneration of the nerve, the corresponding sweat glands are no longer excited to action by pilocarpine. But the alkaloid stimulates also the origin of the sweat nerves in the cord, since on cutting off the blood supply to the hind legs and severing the cord from the medulla, pilocarpine will still induce perspiration.

The effects of the above mentioned dose are checked by 0.003 grm. atropine. Even electric stimulation of the sciatic nerve fails now to excite the sweat glands. But if, now, another dose of 0.01 grm. pilocarpine is injected under the skin of one paw, the secretion returns at that spot and irritation of the nerve is again followed by perspiration, while the paralytic effects of atropine are still manifest in the glands of the other foot. These well executed experiments prove a perfect reciprocal antagonism between pilocarpine and atropine in the action on the sweat glands. H. G.

Neurotomy.—At the International Medical Congress at Geneva (Sept. 9 15, 1877, rep. in *Gaz des Hopitaux*), M. Letievant defended the operation of neurotomy in rebellious cases of neuralgia. It was, he said, by no means as unreliable a means as had been stated, and no more dangerous than any other insignificant wound. Care should be taken not to cut the nerve until it was fairly laid open to view, and that no vessels, etc., are included in the section, to avoid hemorrhage. Care should also be taken to not tear or roughly pull the nerve so as to shock its encephalic roots.

M. Letivant has performed twenty-two neurotomies in sixteen cases and twelve individuals. In all the cases it was performed on account of intense and persistent pains, resisting all other treatment. The duration of

these neuralgias was twenty years, twelve years, five years, several years, and many months. In ten of the twelve persons a cure was obtained. There were two cases of rapid relapse, in these the method was not persistently tried. The cures had been watched and followed for the periods of five years, three years and a half, and eighteen months. In two cases only the immediate effects could be observed.

M. Letievant concluded with the expression of his opinion that neurotomy admitted in theory, has by its success forced itself to be admitted in practice.

Camphor.—C. Wiedeman, *Arch. f. exp. Path.* VI., 216, (abstr. in *Centralblatt*, No. 37.) The well known epileptiform attacks which follow camphor poisoning in mammals, do not occur in rabbits if the cervical cord is separated from the medulla, and must, therefore, have their start from above this point of section, and not be due to the spinal cord. In winter frogs no convulsions appear. In summer frogs a convulsive extension of the posterior extremities may be produced by mechanical irritation of the head or back shortly before the appearance of the camphor paralysis. (Harnack and Witkowsky have also observed fibrillary contractions in frogs from the administration of camphor.) The reflexes exist with increased intensity for a while after voluntary movements have become very much weakened, but delayed; they are lost first in the stage of general paralysis. Electric irritation of the medulla or the cord then has little or no effect, faradic excitation of the sciatic is also of little effect, while, on the contrary, direct excitation of the muscles produces powerful contractions. If one limb exclusive of the nerve was separated from the body before the poisoning, then this nerve remained normally irritable. But contractions of this limb can still be excited from the cord. (The author has not undertaken the more suitable experiment for the testing of this question, by exclusion of the blood supply). He is of the opinion that there is a curare-like action of the camphor on the terminations of the motor nerves and a paralysis of the longitudinal and (*reflex*) cross-conduction of the cord, which he considers to be the causes of the almost absolute absence of convulsions in the frogs. The stoppage of the heart (in frogs) by muscarine is counteracted or hindered by camphor (Harnack). Irritation of the vagus also produced no stoppage of the heart. As Harnack and Witkowsky found with physostigmin, which has a similar action on the heart, so also with camphor, after the introduction of substances paralysant to the muscles (tartrate of copper and soda), stoppage of the heart was again produced by irritation of the vagus and by muscarine, which H. and W. took as a proof that such substances as physostigmin excite the cardiac muscle, and that the inhibitory influence of the vagus is too weak to contend againt this super-excitation, but when the muscle is weakened by the poison, excitation of the vagus by electricity, or muscarine again obtains the upper hand, and again the inhibition of the heart's action takes place. Also in curarized cats in which artificial respiration was kept up, there was seen under the action of cam-

phor an increase of blood pressure with very notable variations at irregular periods. Neither of these, the increase or the variations, took place in animals whose cervical cords had been divided; the pressure fell, on the other hand, under the influence of the camphor. The author, hence, concludes that it directly excites the vaso-motor centre in the medulla. In frogs (corresponding to the spinal paralysis) there is a dilatation of the mesenteric artery under the influence of camphor, whether after previous constriction or not the author leaves uncertain. Rabbits, in general, exhibit the same phenomena as cats. In dogs, the influence on the blood pressure is less obvious. If both vagi are divided in the cats, the increase of pressure and the variations are absent. In mammals, it appears from the author's experiments, the heart does not seem to be influenced by camphor in any degree worth mentioning. Dogs bore the administration of from 12 to 20 grammes per day for a week, without showing any odor of camphor in the urine or fœces (the convulsions were endured without damage). There occurs in the urine an acid, which, with the presence of ammonia, is precipitated by acetate of lead. The quantity of this acid is the greater, the greater quantity of camphor administered. Wiedemann did not succeed in isolating and fully investigating this acid. It is soluble in water and alcohol, less so, or not at all, in ether; it does not crystallize, and it contains nitrogen. When heated with dilute mineral acids, it resolves itself into (1,) a non-nitrogenous substance soluble in ether, indifferent towards acids and alkalies, and melting and subliming with heat, and (2,) a substance quickly reducing copper and bismuth oxides in alkaline solution. Digestion experiments gave only negative results.

Apomorphine in Epilepsy.—E. Vallender, *Berlin Klin. Wochenschrift*, 1877, No. 14, (Abst. in *Centralblatt*, No. 28) succeeded in aborting epileptic seizures by the subcutaneous injection of apomorphine during the aura. The quantity used was one-fourth of a syringe of a 1 per cent. solution, (about 1-24th grain). In one case the previously frequent attacks were entirely prevented for a time, by methodic administration of the apormorphine.

A New Treatment of Tetanus.—In view of the unsatisfactory results ordinarily obtained in the treatment of tetanus by all the methods thus far employed, Dr. de Renzi (of Génes) has undertaken, with the aim of finding some more efficacious treatment of this affection, some very interesting experiments on the strychnine tetanus of frogs, which are, as is well known, very susceptible to the influence of this drug He has thus been enabled to lay down a rational method of treatment of this terrible disease.

First, we will give in a few words the results of these experiments:

1. Tetanus is more severe in animals kept in full light than in those kept in darkness. This difference is not very marked.

2. The convulsions of tetanus develop themselves more rapidly, and with more intensity in animals which are constantly being disturbed, than in those that are kept quiet. The influence of mechanical excitation is much more marked under the influence of light.

3. Small frogs poisoned with a twentieth of a milligramme of strychnia, readily succumb if roughly handled, but they may recover if they are left in perfect quiet.

M. de Renzi concludes from these experiments that absolute repose is the most rational treatment of tetanus. In fact, during the academic year 1872–3, he treated in his clinic two cases of tetanus. The first, treated with successive doses of chloral and repeated injections of curare, terminated with death; the second, treated almost exclusively by absolute repose, ended in recovery. In the following year, of two cases of which he had the care, one recovered under treatment by absolute quiet, the other, though treated in the same way, succumbed. Finally, during the year just finished, M. de Renzi received a fourth case in his clinic, which, like the three preceding ones, was treated by the method of absolute quiet, and this time with perfect success.

En resumé, according to the author, the following is the proper procedure in the treatment of a case of tetanus.

1. Place the patient in a perfectly dark room, and only open the door partially once in four hours, to pass in food and drink.

2. Stop up the external ears with wax, and recommend the patient to rest as tranquilly as is possible for him.

3. Every hour give him out of a suitable vessel, soup, an egg, and two spoonfuls of white wine. For drink, water, with a very small quantity of wine.

4. To quiet pain give a little belladonna and ergot.

5. Have a carpet on the floor of the room. *Gaz. Med. de Paris* (*Gaz. des Hopitaux*, No. 123, Oct, 23, 1877.)

The Gombi Arrow Poison.—In a recent number of the *Bulletin Mensuelle de la Societé de Acclimation* of Paris, M. M. E. Hardy gives a detailed account of researches and experiments on the active principle of the poison obtained from the seeds of *Strophanthus hispidus*. This plant, which belongs to the poisonous order Apocynaceae, was first observed by Houdetot, a French naturalist in Senegambia, afterwards by Smeathmann, near Sierra Leone; by Baikie, at Mupé; by Griffon de Bellay, at Gaboon; and by Gustav Mann, in Western Tropical Africa. It is a climber, with a hollow cylindrical stem, and grows in the forests, where it ascends to the summits of the highest trees. The oblong, nearly sessile, opposite leaves, are from ten to twelve centimetres long by five wide, and are covered with hairs, particularly on the under surface. The yellow flowers are borne on terminal cymes. The fruit is a cylindrical follicle, somewhat thicker than the thumb, and contains from one hundred to two hundred oval seeds. By means of a fruit given them by the Paris Society, MM. Hardy and Gallois have discovered that the active principle is not, as was

supposed, an alkaloid, and for it the name *Strophantine* given to it some years ago by Dr. Fraser, is retained. Besides, they succeeded in isolating a substance presenting the characters of an alkaloid, but which did not seem to possess any marked physiological properties; for this they propose the name *Inéine*. The former is very poisonous, a single crystal placed under the skin of a frog's foot causing the cessation of the heart's action in a few moments. Even after this has taken place the animal still possesses the power of motion, and it is only after respiration has become impossible, owing to the interruption of circulation in the nervous centres, that death ensues from paralysis of the heart. These observations, though yet incomplete, accord pretty well with facts recorded by different authors, and seem to prove that *strophantine* is really the poisonous agent in *Strophanthus hispidus*. The most elaborate experiments on the poison found at the extremity of the arrows, (used by the natives, both in war and hunting) are those conducted by MM. Carville and Polaillon in the laboratory of M. Vulpian. They were made on various classes of animals, and show that the deadly action is much more rapid in mammals and birds than in mollusks, crustaceans, and fishes. On frogs under the influence of curare the poison acts much more slowly, though the respective actions of the two substances do not neutralize each other. *Nature*, Oct. 11, 1877.

The Poisonous Properties of the Bulbous Agaric.—At the séance of the Acad. de Médicine, Paris, Oct. 16, 1877, (rep. in *Gaz. des Hopitaux* No. 121) M. Gubler read for M. Oré some extracts from a paper on poisoning by the bulbous agaric, (*Amanita bulbosa*) the same being in support of the following propositions:

1. That the symptoms observed during life, in animals who were under the influence of this fungus, presented, in their convulsive phenomena, very great analogies with those produced by strychnia.

2. That the lesions observed at the autopsy, consisting in more or less pronounced congestions, with ecchymoses, ulcerations, etc., and occupying the whole extent of the gastro-intestinal mucous membrane, likewise offered a great resemblance to the lesions we observe in animals who have succumbed to the influence of that alkaloid.

3. Studying the manner in which the poisonous principle of the agaric and that of strychnia act in solution towards powdered carbon, M. Oré has shown that if we throw the two solutions on a filter after stirring them up with this powder the latter has the property of retaining both.

4. He finally seeks to prove that if we inject into the veins of one dog a solution of strychnia, and in another the acidulated water in which bulbous agarics have been macerated, death of both animals occurs with the same rapidity, and is preceded by absolutely similar symptoms.

The Antagonism of Atropine and Muscarine.—The following is a note communicated to the Acad. des Sciences, Paris, at the session of Oct. 8, 1877, by Mr. J. L. Prevost, of Geneva, as reported in the *Bull. Gen. de*

Thérapeutique, Oct. 30. "The experiments of which I give here the summary from part of a study on physiological antagonism, which I reported to the biological section of the International Congress for the Medical Sciences (Geneva, 5th session, Sept. 9–15) on the 14th of September last.

"It has been known, since the experiments of MM. Schmiedeburg and Kopp, that atropine is the antagonist and antidote to muscarine. In fact, atropine is not only capable of causing all the symptoms produced by muscarine to disappear, but this agent may be considered as its antidote, and it delays the death produced by a toxic dose of muscarine. My experiments have only confirmed, in this respect, the observations of MM. Schmiedeburg and Kopp, and others.

"The mutual antagonism of the two poisons has been denied almost up to the present, by various experimenters, who have all maintained that muscarine is without effect on animals who have received even the minimum dose of atropine.

"My experiments do not support this opinion, but show that large doses of muscarine are sufficient to produce toxic effects in animals previously atropinized. A number of the experiments consisted in injecting locally into the arteries of the sub-maxillary gland (procedure of M. Haidenhain) a strong dose of muscarine, in cats previously chloralized, and into the veins of which a dose of from one to five milligrammes of atropine had been injected.

"As soon as a dose of from 10 to 20 centigrammes of muscarine was reached I have seen a decided salivation produced just as if the animal had taken no atropine.

"In injecting muscarine into the peripheral end of a branch of the mesenteric artery, I have seen tetaniform vermicular contractions produced in the corresponding intestinal loop, in spite of the previous administration of atropine to the animals experimented upon (cats, rabbits, fowls, pigeons). This result was less constant and striking than was the salivation.

"In many of the experiments, the injection into the venous circulation of cats, previously atropinized, of large doses of muscarine produced the salivary, ocular, intestinal, and respiratory symptoms of the latter.

"In a cat, operated on July 31, 1877, I have seen the effect of muscarine shown twice in succession in spite of the previous injection of atropine; the second time the dose being as much as five milligrammes. Very large doses of muscarine are requisite to produce this result.

"Two milligrammes of sulphate of atropine, previously injected were neutralized by 76 centigrammes of muscarine.

"Five milligrammes of sulphate of atropine, injected into the same vein, suspended the action of the muscarine, which reappeared after an injection of 2.20 gr. of muscarine. The cat thus experimented upon, received altogether and in succession seven milligrammes of sulphate of atropine and three grammes of muscarine.

"This experiment was successfully repeated on other animals.

"These experiments permit me to conclude that the antagonism of atropine and muscarine is a mutual one, and that sufficiently large doses of

muscarine will produce the effects of the poison in animals previously atropinized, a fact that has hitherto been denied by experimenters.

"My experiments were made only in view of the antagonism of the two poisons, and do not permit me to affirm that muscarine in large doses is the antidote to atropine, as atropine is the antidote to muscarine even in feeble doses."

Bromide of Potassium.—G. Klosz. *Archiv. f. exper. Path.*, etc., VI. p. 1. (Abstr. in *Centralblatt* No. 31) confirms the sedative action of K. Br. on man, and the destruction of reflex sensibility of the pharynx. On an empty stomach the effects are more intense, but also the local action (producing diarrhœa, etc.). Dull headache, with loss of memory and impaired coordination of the movements of the tongue were usually observed as the effect of K. Br., but never sleepiness. Any action on the eye was not observed. In frogs, the voluntary muscles were finally paralyzed by K. Br., which paralysis was prevented by previous ligation of the blood-vessels. But even after cutting off the blood supply the motor nerves are in the end paralyzed, evidently a centrifugal paralysis. In rabbits, reflex sensibility was annihilated, before the occurrence of spasms (produced by cardiac paralysis). Hence K. concludes that reflex sensibility is destroyed before motor conductibility. Frogs can, therefore, perform voluntary movements when reflex movements are no longer possible. The dulling of cerebral sensibility K. compares to this annihilation of reflex power.

In order to estimate the share of either K. or Br. in these results, Klosz administered on the one hand a corresponding quantity of Na Br, on the other hand K Cl. The influence on pulse and temperature was equally possessed by K. Br. and K. Cl., much less by Na. Br.; it is hence referable mainly to potassium, which is also the active agent in the paralysis of muscle and nerve. The diminution of cerebral and reflex sensibility, on the other hand, is the result of the influence of bromine, due, no doubt, to a specific action of the nerve substance. On the experience of Prof. Ellefsen, K. recommends small doses of K. Br. against unilateral neuralgia of the trigeminus.

Other recent papers on the Therapeutics of the Nervous System and Mind:

Roberts, Treatment of Traumatic Tetanus, *Am. Jour. Med Sci.* Oct. 1877; Liebreich, Observations on the Use of Chloral, *Lancet*, (Am. repr.) October, 1877; Besnier, Subcutaneous Injections of Chloroform, and particularly their Employment in the Treatment of Pain; *Bull. Gen. de Thérap.* No. 30; Ayer, Result of the Brown-Séquard Treatment in Twelve Cases of Epilepsy, *Boston Med. and Surg. Jour.* Dec. 24; Seguin, A Contribution to the Therapeutics of Migraine, *New York Med. Record.* Dec. 8; Engel, The Diagnosis and Treatment of Some Forms of Syphilitic Nervous Affections, *Phil. Med. Times*, Dec. 22.

BOOKS, ETC., RECEIVED.

Lecons sur la Physiologie et l'Anatomie Compareé de l'Homme et des Animaux. Faites a la Faculté des Sciences de Paris. Par H. Milne Edwards. Tome Douzieme Deuxieme partie. Fonctions de Relation (suite) Ouie; Vue; Voix. Paris. 1876-7. 659 pages.

Lecons sur les Maladies du Systeme Nerveux faites a la Salpetrière. Par J. M. Charcot. Recueillies et Publiecs par Bourneville, Redacteur en chef du *Progrès Médical*. 4me. Fasc. Paris, 1877.

Arbeiten aus der Physiologischen Anstalt zu Leipzig, Elfter Jahrgang, 1876. Mitgetheilt durch C. Ludwig. Mit IV. Tafelen und 34 Holzschnitter. Leipzig, 1877. 176 pages.

Handbuch de Speciellen Pathologie und Therapie. Herausgegeben von Dr. H. v. Ziemssen, Elfter Band. Zweite Hälfte. Krankheiten des Ruckenmarks. Zweite Abtheirlung. Von Prof. Wilhelm Erb in Heidelberg. Mit 14 Holzschnitten. Leipzig, 1877. 404 pages.

The Science and Art of Surgery: Being a Treatise on Surgical Injuries, Diseases, and Operations. By John Eric Erichsen, F. R. S., F. R. C. S., etc. Revised by the author from the Seventh and enlarged English edition. Illustrated with eight hundred and sixty engravings on wood. 2 vols; 947 and 989 pages. Phil delphia: Henry C. Lea. 1878. Chicago: Jansen, McClurg & Co.

Lectures on Fevers. By Alfred L. Loomis, A. M., M. D. New York. 1877: William Wood & Co. Chicago: Jansen, McClurg & Co. 403 pages.

Medical and Surgical Reports of the Boston City Hospital. Second series. Boston, 1877. Published by the Board of Trustees. 316 pages.

Transactions of the International Medical Congress of Philadelphia. 1876. Edited for the Congress by John Ashhurst, Jr., A. M., M. D., Philadelphia. Printed for the Congress, 1877. 1,153 pages.

Diseases of the Nervous System: Their Prevalence and Pathology. By Julius Althaus, M. D., M. R. C. P., London, etc. New York: G. P. Putnam's Sons. 1878. 366 pages. Chicago: Jansen, McClurg & Co.

Sycosis: Prize Essay for 1877 of the Bellevue Hospital Medical College Alumni Association. By A. R. Robinson, M. B., L. R. C. P., and S. Edin. (reprinted from the New York Medical Journal, August and September, 1877.)

The General Subject of Quarantine, with particular reference to Cholera and Yellow Fever. By John M. Woodworth, M. D., Surgeon-General, Mercantile Marine Hospital Service, United States of America. (Extracted from the Transactions of the International Medical Congress. Philadelphia, September, 1876.)

The Safety of Ships and of those who travel in them. By John M. Woodworth, M. D., Supervising Surg.-Gen. U. S. Marine Hospital Service. A Paper read at the annual meeting of the American Public Health Association, held in Boston, Oct. 5, 1876. (reprinted from Vol. III. Public Health Papers of the American Pub. Health Association.) Cambridge, 1877.

Contributions to the History of Medical Education and Medical Institutions in the United States of America. 1776–1876. Special Report prepared for the United States Bureau of Education. By N. S. Davis, A. M., M. D. Washington Government Printing Office, 1877.

Address on the Progress of Medical Education in the United States of America, during the century commencing in 1776. Delivered before the International Medical Congress at Philadelphia, Sept. 9, 1876. By N. S. Davis, A. M., M. D., of Chicago, Ill. (Extracted from the Transactions). Philadelphia, 1876.

Medical Gynecology. Annual Address by the President, Fordyce Barker, M.D., of New York. (Reprint from Vol. II., Gynecological Transactions, 1877).

Mental Hygiene for Pupil and Teacher. A Lecture delivered before the Normal School at Chapel Hill, North Carolina, Aug. 4th, 1877. By Eugene Grissom, M. D., L L. D. Raleigh, 1877.

Address on Mental Disorders. Delivered before the Medical Society of the State of Pennsylvania, at its Annual Session, held in Harrisburg, June, 1877. By John Curwen, M. D. (Extracted from the Transactions). Philadelphia, 1877.

Cutaneous and Venereal Memoranda. By Henry G. Piffard, A. M., M. D., and George Henry Fox, A. M., M. D. New York: William Wood & Co. 1877. 301 pages. 12mo. Chicago: Jansen, McClurg & Co.

Eighteenth Annual Report of the Board of Trustees of the Wisconsin State Hospital for the Insane, for the fiscal year ending Sept. 30, 1877.

Third Annual Report of the Executive Committee of the Asylum at Walnut Hill, Hartford, Conn., at their annual meeting, Oct. 8, 1877. Also Petition to the Legislature.

Twenty-second Annual Report of the Trustees of the State Lunatic Hospital at Northampton, for the year ending Sept. 30, 1877.

Fifth Annual Report of the Northern Hospital for the Insane of the State of Wisconsin, for the fiscal year ending Sept. 30, 1877, with an Appendix giving a compilation of the Laws of the State of Wisconsin on the Subject of the Insane. Madison, 1877. 127 pages.

Exposition of Facts. By A. Y. P. Garnett, M. D. (To the Medical Profession of Washington and Georgetown, D. C.) Pamphlet. 38 pages.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fuer Path. Anatomie, Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
British Medical Journal
Bulletin Générale de Thérapeutique.
Centralblatt f. d. Med. Wissenschaften.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Frenocomia di Reggio.
Gazetta Medica de Roma.
Gazette des Hopitaux.
Gazette Médicale de Bordeaux.
Glasgow Medical Journal
Hygiea.
Hospitals Tidende.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Mind.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Rivista Sperimentale di Freniatria e de Medicina Legale.
Revue Médicale du Nord-Est.
Revue Mensuelle de Médecine et de Chirurgie.
Revue des Sciences Medicales.
Schmidt's Jahrbuecher der In. und Auslandischen Gesammten Medicin.
St. Petersburger Med. Wochenschrift.
The Practitioner.
Upsala Lakareforenings Forehandlinger.
Vierteljahreschrift fuer die Prakt. Heilkunde.

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 Medical Weekly.
American Naturalist.
American Practitioner.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Buffalo Medical Journal.
Canada Medical Record.
Canadian Journal of Medical Sciences.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Medical Journal.
Maryland Medical Journal.
Medical Brief.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Nashville Journal of Medicine.
New Remedies.
New York Medical Journal.
Ohio Medical and Lung Journal.
Pacific Medical and Surgical Journal.
Pharmacist.
Philadelphia Medical Times.
Quarterly Journal of Inebriety.
Richmond and Louisville Medical Journal.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.

THE

JOURNAL

OF

Nervous and Mental Disease.

Vol. V. APRIL, 1878. No. 2.

Original Articles, Selections and Translations.

Art. I.—REFORM IN THE SCIENTIFIC STUDY OF PSYCHIATRY.

By Edward C. Spitzka, M. D., New York.

(*An Address delivered before the N. Y. Neurological Society, March 4th*, 1878.)

THERE are certain questions related to insanity which do not find a place in purely clinical and pathological memoranda, and which therefore have been rather neglected by our special societies and journals. Some of these questions, however, are of such fundamental importance, that even at the risk of diverting an undue proportion of your time to insanity, a subject which has already furnished the themes for more than half the papers read before the Society, I have resolved to ask your attention for them this evening.

I propose to-night to exhibit the organic connection existing between psychiatry or mental pathology, and that branch of general pathology which relates to the nervous system. I intend further to examine in how far attention to this subject is likely to benefit the general practitioner; how far the claim of a certain circle to monopolize the subject is justifiable, either in equity or on practical grounds; and finally, what

arrangements could and should be made to render psychiatry as available to the profession at large as other specialties have been rendered.

"Mental disease" is merely a symptomatic term, as "mental" and "moral" are adjectives founded on abstractions. With the abstract, medicine does not profess to deal, and accordingly the great masters of modern science have ever sought for a proper material basis for such symptomatic conceptions, before according them a place in the field of medicine. It then was found, in complete accordance with physiological presumptions, that where demonstrable lesions were discovered, between which and mental symptoms a direct relation could be established, their seat was in the hemispheres of the brain. It was further found, that in many instances in which structural lesions of these organs were not discoverable, that anomalies of the circulatory apparatus, involving nutritive disturbances of the encephalon, could be either demonstrated or logically inferred to have existed during life. In accordance with the laws governing inferences, investigators were justified in locating the somatic disturbances producing insanity, whether of a structural, nutritive or dynamic character, whether primary or secondary, to distinct lesions, in the prosencephalon.

Accordingly the topographical area within which the mental pathologist is to conduct his difficult and interesting researches, constitutes but a segment of that great system which comprises the legitimate domain of the general neurologist. Neurology deals with the whole nervous system, mental pathology merely with a part; and since the anatomical provinces with which the two are concerned bear to each other the relations of a part to a whole, it follows *cæteris paribus*, that also clinically, psychiatry is but a subsidiary branch of neurology.

The more closely we examine into the subject, the more shall we find the above relation maintained in every respect, and that aside from the purely anatomical relation, there are numerous strong bonds of connection.

Among such links I may mention the closely similar etiology. We find alcoholism, syphilis, insolation, traumatic,

reflex and teratological influences, producing, in one series of subjects ordinary nervous affections, in another series, insanity. And this analogy as to causation is even verified in the varying influence of purely mental and moral causes, for while such causes ordinarily result in the production of mental disease, they in some instances may produce spinal symptoms. Occasionally the etiological relation assumes characters which cannot be explained away on the ground of mere coincidence, as in the case of locomotor ataxia compared with progressive paresis of the insane. Here it has been found, that not only has the same series of causes (with a single and that a consistent exception) been assigned for both, but that, as a dependent result, the relatively greater immunity of the female sex is in the same percentual proportion. The terminal stage of many spinal and cerebral diseases is insanity; locomotor ataxia often makes its exit under the mask of paralytic dementia, melancholia, or maniacal delirium. The dementia of epilepsy and choreic insanity corroborate this statement, and many of the hallucinatory, delirious, and amnesic conditions, occurring in the course of *ramollissements*, hemorrhages and meningitis, find their place here. Considering symptoms *per se*, many of the symptoms of insanity occur separately or combined in many diseases not classed under the head of insanity; and any attempt to consider the amnesia or delirium following an apoplexy, as something intrinsically distinct from the amnesia and delirium found respectively in dementia and maniacal excitement, is inadmissible. The acute maniacal condition, which occasionally supplants the ordinary convulsive epileptic attack, is the true equivalent of an epileptic explosion.

Not only is the causation and the symptomatology similar; not only does one frequently pass into the other, but the histological changes on which insanities, as well as other nervous diseases, depend, are the same; and it seems to be the seat, rather than the character of the lesion, which determines insanity in one man, ordinary nervous disease in another. In illustration of this, I need but point to tumors, disseminated sclerosis, syphilitic affections, and to perimyelitis as contrasted with periencephalitis.

How often is not a latent morbid tendency in the higher

centres called into insane activity by lesions in lower associated centres? How frequently does not an apoplexy or encephalitis situated low down, project a disordering influence on the cortical expanse, through centrifugal tracts in the line of the lesion? And may not such a disordering influence be situated even in the bodily periphery? Is not every relation between uterine and other peripheral irritation and resulting spinal motor, sensory or vasomotor disorder, repeated in the phenomena of hysterical, reflex and some cases of puerperal insanity?

In stating that the lesions producing insanity are similar to those of other nervous diseases, I should add, as a matter of course, that this similarity extends even to those forms of insanity and spinal disease, which both furnish us with the apparent pathological enigma of marked, furibund, and fatal symptoms, occurring without any demonstrable post-mortem lesion.

From a pathological and clinical point of view, therefore, as well as for several important practical reasons, the study of insanity should be considered a subdivision of neurology. A strictly separate study of either must be prejudicial to both, on account of their numerous and intimate relations. To make a special province of diseases of the liver, without considering these in their relation to cardiac and pulmonary insufficiency, gastric, hemorrhoidal, and enteric associated conditions, would be scarcely less absurd, than to treat of pulmonary and cardiac, gastric, enteric and hemorrhoidal affections, and neglecting their hepatic complications! Yet the former case is represented by him, who lectures on insanity without being familiar, or caring to familiarize himself, with nervous diseases in general; the latter by the neurologist, who is prevented by an unjust monopoly from considering mental disorder in conjunction with other nervous disorders!

As you are well aware, it is only under exceptional circumstances, if ever at all in America, that the teacher of nervous diseases can command the material essential to a thorough clinical and pathological demonstration of insanity.

This is chiefly on account of a feeling among a number of asylum superintendents, that they can claim to monopolize the

science of psychiatry, to exclude every non-asylum physician from this field, and that they alone are entitled to teach this subject in our medical schools. *A priori* there can be no fairer proposition than this: that he who has devoted his life-time to a given specialty, ought to have the first voice and the high privilege of instruction in that specialty. If capable, zealous and honest scientists establish a monopoly in scientific matters, even a monopoly may become endurable. But I would most strenously object, that every one who may have happened to possess the requisite social and political influence, to receive an asylum position, is therefore to be considered a psychiatrist. Such a conclusion, based on an acceptation of a discreditable *statu quo*, has been the great bane of American psychiatry, and I regret to say has been diligently fostered by that narrow circle of asylum physicians which furnishes the *ex cathedra* statements of the Asylum Association. To these statements too much *blind obedience* has been paid in the past, too little *attention of the proper kind* is paid to them at present.

Aside from the fact that their above claim, like all exclusive claims, is a selfish one, and that it precludes, as it is intended to preclude, the possibility of fair competition by capable men, it involves assumptions whose grounds must be carefully analyzed before they can be granted!

If asylum superintendents stand so high in scientific *morale* as to be able to determine themselves to be the only psychiatrists in America, surely they should have such results to show in proof of this, as would justify their self-implied omnipotence; and should we in the course of an objective and impartial inquiry, find that this claim cannot be sustained by facts, that on the contrary, the documentary evidence of asylum physicians, themselves, shows such claims to be ill-founded and fallacious in the highest degree, those who have provoked these criticisms by their assumptions can find no cause for complaint in the publicity which is given to the results of our investigation of the matter. For the institution of this investigation, those implicated have themselves alone to reproach.

It has long been a subject of comment and surprise, that nothing worthy of notice has proceeded from our insane asylums, in the fields of pathology and clinical observation.

The exceptions to this rule are so few that they just serve to prove it. There are nearly a hundred asylums in this country, many of which have opportunities for making from thirty to fifty autopsies annually, and a few, as many as a hundred or more. These autopsies, if systematically and properly made, would furnish valuable and suggestive data, not only in nervous and mental, but also in general pathology. One is justified, in view of this unpardonable waste of material, to employ the strongest terms of censure in characterizing the apathy and ignorance manifested by those concerned in this dereliction of scientific duty. There is no grander field for research than that of human comparative cerebral anatomy. The observation of variations in the relative development of the ganglia and hemispheres, the measurement of their relative dimensions, and the registration of their absolute and relative weight, the establishing of a relation between certain forms of cranial and thereon dependent cerebral asymmetries, or the tracing of both to inequalities or transpositions of the great vessels, with the delineation of the convolutions, constitute tasks which will contribute to the elucidation of the most important anthropological problems. As to the pathological usage of this material, I need hardly refer to the important gauges which the pathology of certain cerebral provinces furnishes of many physiological experiments and theories.

Even where autopsies are performed, they are usually made to satisfy purely formal considerations, such as an occasional coroner's inquest! Those who make the autopsy, are ignorant of any higher cerebral anatomy; the landmarks of the convolutions do not exist for their guidance, and the intricacies and topography of the pedunculer tracts are to them a *terra incognita*. It is but natural, under such circumstances, that the brain should be neither measured, weighed nor delineated; the lesions, if they be fortuitously discovered, pass unlocalized, and asymmetries or heterotopias pass unrecognized.

If we cast a glance at the present state of psychiatric literature, we find that, while in Great Britain and on the Continent, valuable monographs are daily being published, America is far behind, not only in the number but in the quality of its contributions to psychiatry, in striking contrast with its

well-deserved eminence in other specialties. American psychiatrical contributions are frequently abstracted from articles appearing in trans-atlantic journals, or if *quasi* original, are still more worthless.

While special reference to individual cases is not desirable in a paper of the present character, and I wish therefore to avoid all mention of persons or particular institutions, yet as I cannot expect you to take everything for granted, I have briefly adverted to a few monographs emanating from asylums, making scientific pretentions, and published in a journal devoted to asylum interests, to prove my very decided assertion, just made. With the proof of the correctness of that assertion, the stock argument employed by the inner asylum circle when excluding the non-asylum physician from the discussion of insanity, namely, that no one, who has not been in an asylum, can possibly know anything about the subject, falls to the ground. In fact, this argument, if accepted, would lead to the inevitable and ludicrous conclusion, that the asylum patients, who frequently enjoy a far more varied, extensive and constant "asylum experience" than even the superintendent himself, must therefore be experienced and competent alienists.

One of the first contributions to which my attention was called in this connection, is sufficiently characterized by its final conclusion, one which renders all comment superfluous: that "Phosphorus is to the brain in insanity, what iron is to the blood in anæmia!!!"

Another by a leading member of the innermost asylum circle, is ironically referred to by the reviewer in Virchow-Hirsch's *Retrospect* in these words: "This fortunate writer knows *all* the lesions of insanity" and the accompanying micro-photographs are stated "to show nothing that they are intended to show;" I may add that they show lesions which can be artificially produced in the brains of cats, dogs, sheep, and other animals not very liable to insanity, by using certain reagents. A crowning specimen of asylum work is furnished by an article dealing with that interesting form of insanity which is associated with progressive motor paresis. I do not dwell on the fact that this article is a fair specimen of text-book compilation, for in one respect at least, the

author has been strikingly original, if not revolutionary. He writes the clinical part with the aid of two assistants, and under the head of Pathology, adds a "summary of the existing state of knowledge on the subject, kindly furnished him" by a special pathologist of an asylum two hundred miles distant from his own. Perhaps he thus intended to inaugurate a new era in the wholesale manufacture of monographs; it may be convenient at times to resort to a division of labor in medicine, but how a logical relation is to be established between lesions and symptoms, when the clinical observations are made on one series of patients, and the pathological observations on an entirely different series, is to me, in the case of the ever-varying picture of cerebral diseases, simply incomprehensible.

I can assure you, that utterly beneath all criticism as these specimens of asylum literature are, they are by no means the very worst. The average articles seldom rise to such a lofty level, as pathological, clinical and therapeutical subjects, constitute in their modest and unpretentious horizon. Occasional melancholy lucubrations over deceased and lamented brother superintendents, or reminiscences of the newspapers published by asylum patients, contributions to what is termed "mental hygiene," strongly suggestive of the influence which the asylum chaplain has acquired over the asylum superintendent, and impassionate glorifications of "mechanical restraint" constitute the range of subjects which medical superintendents delight to read and write about.

If we look at their annual reports, we find that some of them wax enthusiastic over the prizes gained by their hogs and strawberries at agricultural fairs, while others give you the benefit of their historical ideas on insanity. Beginning with David and Solomon, they pass from Scripture to Homer, thence to Bedlam, and tracing the development of humanitarian sentiments to the present day, when unlucky legislatures were induced, through the expansive views of the superintendents regarding the insane millennium, to appropriate ruinously extravagant sums to the erection of an insane paradise, they kindly permit their trustees to publish such "historical" documents accompanied by caricatures of morbid brain tissue in the illustrated monthly magazines.

Judging by the average asylum reports, we are inclined to believe that certain superintendents are experts in gardening and farming (although the farm account frequently comes out on the wrong side of the ledger), tin roofing (although the roof and cupola is usually leaky), drain-pipe laying (although the grounds are often moist and unhealthy), engineering (though the wards are either too hot or too cold), history (though their facts are incorrect, and their inferences beyond all measure so); in short, experts at everything except the diagnosis, pathology and treatment of insanity.

But certainly that evidence which is the best calculated to show how far the claims of some medical superintendents to monopolize psychiatric clinical instruction is a just one—practically considered—is that furnished by the few instances, in which these gentlemen have exercised this supposed prerogative and monopoly; by their fruits shall ye know them! It is fortunate that they have published several of their lectures, which from the fact that they have been published may be taken as fair, if not the best samples of their didactic efforts; I say fortunate, because unless the lecturers in question had committed themselves in print, I should have fears that you might suspect me of testing your credulity.

On opening one of these specimens of a lecture held in 1876 (!) entitled "Feigned Insanity," I find that it begins with the statement—"that moral and feigned insanity are convertible terms" !!—Mind you, this is a superintendent, who possesses "asylum experience," that he presents such propositions to medical students, and that for the sake of a weak paradox, the customary resort of those who believe that they must make up for a lack of real knowledge by sensational statement, he falsifies the science of psychiatry to the extent of representing a morbid symptom, fully recognized and extensively treated of by Bucknill, Tuke, Ray, Maudsley, Crichton Browne, Krafft-Ebing, Meynert, Gauster, Morel, and in fact all authors that will be quoted in ages to come, as identical with a nonentity.

Such a statement does not surprise him who has been interrogated by that same "lecturer," whether "miliary sclerosis" and "miliary aneurisms" were not also convertible terms; but

aside from the suggestion that presents itself, that every science might be extremely simplified by having terms which possess the most opposite meanings rendered "convertible," we must come to the conclusion, that it is high time for the medical profession to awaken out of that lethargy, which has alone permitted ignorant, incapable and insincere tyros to drift into responsible didactic positions. That these "lectures" are empty pretenses would be sufficiently proven by several paradoxes and downright contradictions similar to the one just given; but if you will permit me, I shall illustrate this position which I have taken still further:

If in at least one of our larger municipal asylums, an investigator should happen to require an ophthalmoscope, in order to examine the retina of a patient suffering from paralytic, or epileptic insanity, or of one afflicted with cerebral sclerosis, he will be informed that such an instrument is not to be found in the whole institution, and that even if it possessed one, the chief medical officers would not know how to use it. There are valuable and suggestive inquiries being made abroad, on the variable electrical reaction in certain forms of insanity. Should the investigator wish to repeat them in these institutions, or to employ electricity therapeutically, he will be informed that no battery has ever entered the asylum precincts.

It is needless to add, that when two instruments possessing considerable practical diagnostic value are not in an institution, that those which are subservient to purely scientific research should be also wanting. A microscope, and appliances for weighing the brain, are unknown articles in many asylum inventories! Now, what is the defense made when these grievous shortcomings are brought to the notice of those guilty of the omission? That—forsooth—no appropriation has been made by the State or the Commissioners for scientific apparatus. I submit, that this is a frivolous evasion; a physician cognizant of his position as a medical officer, appreciative of the material at his disposal, and imbued with a genuine, not an affected interest in his noble specialty, will provide these instruments for himself. Their use is destined to improve his own knowledge, not that of the State authorities or the Commissioners of

Charities. While I fully recognize that a government can exercise no wiser liberality than that of fostering and encouraging scientific research, it is to be insisted that such liberality be displayed in the proper direction, and that those who claim governmental aid, also show their ability for the work to be carried on with such aid. We have had at least two melancholy experiences with the munificent but misspent liberality of past legislatures in this respect, and it is our duty to prevent, as far as we can, any further criminal waste of the public funds, in favor of crude and dishonest work, carried on in the interest of private advertisement.

As long as no governmental supervision of the insane, deserving the name, exists, I do not understand how the medical superintendents who are continually intriguing against such supervision, the only safety-guard against the abuse of trust, can fairly ask for an increase of these trusts. Until such supervision is established, scientific work ought to remain a matter of private zeal and enterprise. If the general practitioner, in spite of his numerous interruptions, can afford not only to supply himself with those instruments, but also to study and understand their use, there is no excuse for the medical superintendent, who with a remunerative salary, a well-defined routine of duty, and abundant, I designedly say, abundant leisure, neglecting to do the same.

If it were only in justice to the young men, who, fresh from college, enter an asylum, to acquire a year's or two years' hospital experience, the superintendent should acquire those faculties which fit him to direct the energies of the aspirant into proper channels, not as is the case, be actually inferior to the new-comer in medical and scientific culture.

As I have hinted, out of the one hundred asylums in this country, two have by skillful manipulations of the legislatures, succeeded in obtaining munificent appropriations for laboratories, and one of these in addition for a special pathologist. Altogether, at the latter institution $10,000 are annually expended on scientific work, including the State subsidy for a scientific journal published by the asylum officers. I am informed that this subsidy is *probably* $2,500; but the loose manner in which the financial account is notoriously kept,

does not permit this amount to be exactly ascertained. The printing of extra copies of the pseudo-scientific annual reports, in which insanity is traced to the sojourn of judges in crowded courtrooms, and priority in cerebral pathological research is claimed, side by side, with the statement, that their "microphotographs show details of structure, which are invisible even under the best microscope," may cost the State about $500 more. The salary of the special pathologist, with the support of his family, amounts to a further $2,500, and $3,000 per annum are appropriated for the current laboratory expenses. The cost of medicines in 1878 was $4,000, of which sum $1,500 can be safely put down to the "experiments" on the *positive* and *relative* nature of drugs. So that in the course of five years $50,000 may have been expended for investigations and publications. Now, let me briefly state what has been done in return for this unexampled liberality of the people of the State of New York; time will forbid me from going into details, and I therefore merely quote the language of a prominent medical weekly journal, which I can fully endorse, that aside from the fact that it was a weak and transparent evasion for a superintendent to publish under his own name, the work done by his special pathologist without acknowledgment, that the autopsies and microscopic examinations were fragmentary, crude and vague, and that one case intelligently reported was worth a score of imperfectly registered ones, such as those published by the asylum in ——.

Add to this the *leucin* precipitates to which I have referred; the patent ignorance of cerebral anatomy manifested by the special pathologist, who actually finds an atheromatous patch "at a point where the middle cerebral artery is given off from the basilar," a discovery not explicable on the grounds of a *lapsus calami;* and the manner in which absurd claims founded on incorrect and illogically interpreted observations are employed to falsify medical jurisprudence,* and widely circulated in the daily press, and there will remain but one general conclusion: that the work there done is not only without value, but absolutely misleading; that the claims advanced are founded on that happy combination of effrontery and ignor-

* In the case of the autopsies made on the murderers Waltz and Grappout.

ance, which currently passes under the designation of, and is certainly kin to charlatanism; in short, that the State has paid $50,000 for what is little better than the private advertisements of one medical superintendent.

Just enough work and no more has been done, in the two asylums in question, as will suffice to justify the further continuation of the respective appropriations, and evidences of seeming insincerity are not wanting. In the case of the western asylum, the superintendent in the course of a paper read September, 1876, before the International Medical Congress, reported having autopsied 115 insane subjects; in a second paper read before the Wisconsin State Medical Society, March, 1877, this number has risen to 200; and in a discussion (published) held at the meeting of the Asylum Association in July of that year, he reports 260 as the number. On deducting the original 115, we find that in ten months, that gentleman claimed to have made 145 autopsies. Now, looking at his report for the year beginning September, 1876, we find that in that *whole* twelve-month, there were 42 deaths in the asylum. There are here two alternatives: either that this superintendent sends his moribund patients home, and autopsies them there, thus diminishing his death-rate (a rather unlikely contingency), or that he makes 145 autopsies on less than 42 subjects! In either case giving some ground for the charge of deliberate deception!

It is a fact, but one to be extremely regretted, that in the only instances where pathological researches are systematically made in asylums, that whatever value they might have, is nullified by the slight dependence which we can place on the trustworthiness of those reporting their results. Errors, even palpable ignorance, may be pardoned; but misrepresentation should not be overlooked nor permitted to enjoy its fruits undisturbed.

To some grave insincerities we have adverted in the case of both asylums, but those alluded to as committed in the eastern one, are remarkable in that they are but the natural outcome of a system, which has harmoniously confused if not falsified the financial statements (JOURNAL OF NERVOUS AND MENTAL DISEASE, volume V., page 781), the statistics of recoveries, and

the influence of asylums on insanity in their vicinity, as well as the pathology of insanity!

Whether the superintendent of such an institution hides his restraining apparatus when distinguished foreign visitors come to see his asylum; whether he publishes the pathological labors of his assistant under his own name; whether he surreptitiously prevents the publication of scientific contributions by non-asylum physicians, merely because their results are truthfully reported and conflict with his own, or perhaps for the reason, that the writers are not also superintendents; or endeavors—I am happy to say, unsuccessfully—to suppress the reading of papers ventilating asylum matters, merely because the writer has been sufficiently incautious and fair-minded to give his opponents a fair notice, in order to ensure a discussion which *he* had no reason to fear; that superintendent exemplifies and personifies that spirit, dominating the Asylum Association, which systematically shuns inquiry, excludes competition, avoids open discussion, and opposes supervision, because it has the best reasons for fearing such inquiry, competition, discussion, and supervision.

It is the Asylum Association as a body which is responsible for the crude and unscientific classification of insanity, to be found in many asylum reports. Several superintendents seem to be unaware that such forms of insanity as progressive paresis* or *folie circulaire*, have an existence. We find suggestions emanating from that body, which conclusively prove that with many of its members, administrative hobbies form the first and scientific considerations their last object. Who that recognizes the importance of the thorough alienist's studying not one sex, nor one form of insanity, could have proposed that the sexes should be under separate superintendents, and that special institutions should be erected for the epileptic insane? And

*During the progress of the discussion of this paper, an assistant physician at one of our municipal asylums, had the temerity to verify this single statement. For this very objective and modest participation in the discussion, he has been discharged from his position by the Medical Superintendent, *who was present, and who, although repeatedly called upon, did not venture to deny a single statement of the paper.* Comment is superfluous.

yet all these propositions have been and are still made, on the grounds of pretended administrative convenience.

It is these gentlemen, many of whom from the moment of their entry into an asylum, become narrower in their views from day to day; who are lost in the contemplation of belittling routine duties; and whose activity reaches its acme, say, in a *ukase* issued concerning the facings of the uniforms to be worn by attendants; or the recognition of a dramatical or musical entertainment gotten up by some friends of the superintendent's, in the avowed interest of the patients, in reality for the amusement of a select circle of visitors, liberally invited on such occasions.

I have already mentioned one excuse with which the officers of certain asylums are ready, when the waste of pathological material in their asylums is referred to. Another excuse which is frequently given, is that the friends of patients will not give their consent to an autopsy. This statement is known to be farcical by those who make it. In our general hospitals, even patients who are not pauper patients, are autopsied to a considerable extent, and if the Asylum Association were serious in its desire for the medical culture of its members, it could, with its powerful influence, too often, alas, exercised for purposes which were not good, have accomplished that which English physicians have done, namely, rendered autopsies compulsory by legal enactment. But it seems that on the contrary, they persistently oppose such examination, and in one of our municipal institutions, the superintendent has *actually forbidden autopsies to be made*, on the pretended grounds, that dissecting material was rare in the medical schools! On the title page of that superintendent's annual report, we find that his institution boasts of a special pathologist; under the circumstances, this gentleman must enjoy an *otium cum dignitate*, if not a sinecure.

Another apology offered, is almost comical, namely, that the asylum superintendent is so overcrowded with routine duties, as to be unable to devote the necessary time to scientific work. How does this statement agree with the fact, that these officers have abundant time for engaging in lobbying, for carrying on private practice, although nominally salaried for attending to

their asylum duties; that they go on lecturing tours, attend flower parties and kindred social celebrations; compose sermons, historical compositions and *extemporaneous* after-dinner speeches, often the result of a three weeks' incubation?

Now, although these gentlemen have, on the insupportable ground of lack of leisure, tacitly confessed to not having accomplished anything in science, in other words, that they have not had the time to perfect their medical training, they still claim to monopolize the field of instruction offered by insanity. I think the alternative is a fair one, either to understand the subject one proposes to teach in all its bearings, or if one does not understand it, to leave it to those who do.

The material of our asylums is a rich material, but it will remain a dead material unless the general medical body examines the subject of asylum management from a medical and philanthropic point of view.

The general medical body has had every provocation for instituting such investigation, for it has not been dealt with very delicately by members of the Asylum Association. Those who study asylum reports, will every now and again discover covert and unjust attacks on the medical profession. The tone adopted by medical superintendents, when speaking of those whom they happen, through the accident of a diploma, to have the honor of being colleagues of, is strongly suggestive of the asylum atmosphere.

In one report, issued shortly after the new Lunacy Law came into operation, several examples of what purport to be the average grounds on which general practitioners send patients to asylums, are published. These reasons for considering the patients insane, are placed in as ridiculous a light as possible, for the edification, doubtless, of boon companions; but on looking at them carefully, we find that considering the general character of certificates of commitment, and the dimensions of the blank space left for the registration of these reasons, that the majority are correct, and need only be read aright. They are certainly, considered as reasons discovered prior to a patient's admission to an asylum, every bit as good and satisfactory as the reasons which the superintendent is able to give when required to testify in case of a writ of habeas

corpus* concerning a patient who has been under his care (!) for months.

When the American Medical Association invited the Asylum Association to join issues, and bring psychiatry into relation with general medicine, the invitation was not accepted, because, as several superintendents stated, such a procedure would be the *death* of their own association. They finally, through the efforts of some intelligent members, concluded to send delegates, and their association still exists, although the IX. Section of the American Medical Association has sunk to the level of a sub-committee of the asylum circle.

Perhaps the real reason of this refusal was the apprehension that the non-asylum physician would presumably take a closer interest in asylum matters, and that as a result, increased demands would be made on the scientific labor of those occupying asylum positions; from a certain point of view these would be very undesirable innovations. If an association stands on such a feeble basis, as to be in danger of dying when the general practitioner asks that which he has every right to demand, its *death* would not only *not* be a matter of regret, but a consummation devoutly to be wished for.

In fact, this association has sufficiently characterized itself, and revealed its true aims and purposes by formally resolving that further supervision than that exercised by the asylum autocrats themselves was unnecessary. At this boldness, and I should prefer to say *audacity*, Dr. Bucknill might well be astonished. This opposition is not only aimed at governmental, but also at medical supervision; with few exceptions, medical superintendents have persistently objected to visiting and advisory medical boards!

From all that which I have just stated, our mature conclusion must be, that the average medical superintendent of insane asylums, not appointed on the strength of general and scientific culture, deficient in anatomical and pathological training, with-

* In the Geisinger case, the superintendent was able to give but one reason, "spends money foolishly," and on cross-examination was found not to be able to state what pubescent insanity was. This is natural, when we recollect that it was only two days before that his assistant, being asked, informed him what was the patient's form of insanity.

ont a gennine interest in their noble specialty, untrustworthy as to their reported results, and not in that communion with the general medical profession which every liberal and broad-minded physician naturally seeks, are the *last* individnals in this world to whom the responsible duty of training the embryo practitioner in an important specialty should be entrusted.

Having negatived the exclusive claims of a special circle, it would remain for us to justify the proposed distribution of the material for clinical instruction presented by asylums, among those engaged in teaching nervous diseases in our schools, if such a justification were necessary. Happily, this is not the case; the list of names of our leading American neurological teachers includes excellent pathologists, sound logicians, acute clinical observers, men rich in every scientific attainment, and of world-wide fame. The general neurologist, who from the very nature of his studies is forced to pay attention to all scientific branches in relation to general nervous pathology, approaches also the subject of insanity, best prepared to deal with it exhaustively, and from every point of view.

Such contributions to our psychiatric literature which have an intrinsic value, have mostly come from these men; and they simply substantiate an experience, that the material for the best alienists is to be found among those who have been best trained as a preliminary in general medicine. Our own Rush was a prominent example of this experience; Esquirol was like Connolly, a thorough clinical scholar; Morel a far-sighted pathologist; Griesinger first attained eminence as a writer on fevers and other general affections, while Meynert and Westphal respectively reflect the influence of those great masters of modern medicine, Rokitansky and Virchow.

It might be asked why, even granting the propriety of treating psychiatry as a branch of neurology, I do not admit the feasibility of treating it under a separate teacher, on account of the vast scope of the subject? Those who would make this interpellation, could point to the example of many European universities, where a separate psychiatric clinic and professor for that clinic exists.

I am very willing to admit, that any plan which will ensure

a thorough and exhaustive study of an important subject, is the preferable one; and that for Vienna, Paris and Berlin, and all organized universities, this plan has not only worked well in the past, but deserves to be still further developed in the future. For American medical schools, such an arrangement would be entirely premature. With our single buildings, our crowded courses, and a staff of a dozen instructors at most, they are placed under entirely different necessities from the European schools, with their dozens of lecture rooms, dozens of courses simultaneously read, and a staff numbering from fifty to one hundred and fifty regular and other teachers. The latter, too, include among their auditory, especially in the case of the psychiatric clinic, more ripe practitioners and graduates than undergraduates.

Having to deal with the present, I unhesitatingly pronounce myself in favor of uniting psychiatry with neurology, in our college courses, and of liberally providing the teachers of these subjects with the requisite material. This should be effected by the same mechanism which is employed in the utilization of the material collected in our general hospitals: by the appointment of visiting physicians, having the same relative grade, functions and privileges enjoyed by visiting physicians of other hospitals. In the case of asylums containing any considerable number of female patients, one gynæcologist at least should be appointed in addition, in pursuit of the excellent suggestion of Storer,* for although that writer supposed a far larger proportion of cases of insanity to be influenced by uterine and ovarian conditions than actually is the case, yet the few thus caused may be cured by well directed gynæcological treatment, when otherwise, and in spite of strait-jackets, conium, chloral, bromides and opium, the insanity would speedily become inveterate.

The majority of such a staff would naturally consist of neurologists, and by preference, of such neurologists as are engaged in instruction in our regular medical schools. Such appointments would immediately raise the whole tone of asylums; sluggish and incompetent superintendents would soon be

* Storer: *Insanity in Women.*

weeded out; the suggestions of experienced and impartial men of high professional standing would lead to many ameliorations in the condition of asylum patients; the assistant physicians would receive a stimulus to study and research, and as from them would in the end be recruited the coming generation of superintendents, professional ability would at last become a question in the competition for asylum positions.

The increased opportunities which the medical student will then have of studying insanity from the actual patient, and of doing so under the guidance of a thorough clinical teacher—not as heretofore, of lecturers who memorize from Sankey, Blandford and other authors, the evening before their "lecture,"—will redound to the benefit of the profession directly and indirectly.

Directly, since the physician will not only more readily and early recognize insanity in general practice, and perhaps by appropriate measures, prevent the necessity of asylum incarceration, productive as it often is, of more harm than good, but he will also be able to determine whether a patient, whose insanity he is about to certify to, is really insane or not, with more certainty than heretofore.

Indirectly, in so far as the medical student with a special *penchant* for psychiatry, will then enter the hospital (let us trust *hospital*, no longer *house of detention*) with at least a rudimentary appreciation of the subject, not as at present, exhibit in connection with his superintendent the familiar figure of the blind leading the blind.

We might remain very well satisfied if we had accomplished thus much, but as every step to reform discloses fields for further reform, we may indicate at least one or two of these, prominently, one which closely interests the profession of the city of New York city. It is well known that certain delays occur in the transmission of patients to asylums, which are prejudicial not only to their mental condition, but present features revolting to humanity. The manner in which disorderly insane patients are beaten by the police, locked up in station houses and sent to the penitentiary or workhouse, before their insanity is discovered, can of course be obviated to a considerable extent, by greater attention on the part of

police and prison physicians. The poorer classes, when unable to obtain the means which would enable them to command the time and services of the two physicians necessary to certify as to the insanity of a relative, have no other way of protecting themselves and the patient himself, than by having their relative arrested like a felon, and taken to the city prison, whence he is transferred to the asylum. The necessity for such a procedure can be avoided; it is degrading, unnecessarily degrading to the patient, and demoralizes the relatives. If in our city hospitals, a patient becomes insane, he is thrown into cells, which are stated to be unfit for even the very worst criminals! Lonely underground "hospital" cells, police station-houses, and jail corridors are not proper places for the detention of the insane, even temporarily.

At the *Charité* of Berlin, and the *Krankenhaus* of Vienna, there are several wards for the reception of the insane, recently received from all parts of the vicinity. Chronic incurable cases can be speedily sent thence to regular asylums; those suffering from febrile delirium can be watched *and treated* at the same time, not, as not unfrequently happens in our vicinity, sent per steamboat to the Islands, and delivered to the asylums there situated in a moribund condition, and occasionally the bearers of contagious or infectious diseases! In recent and curable cases, the necessity and expense of sending to an asylum are thus frequently entirely obviated.

You already perceive in this brief sketch of a great abuse, several hints as to possible demands for psychiatrical knowledge in the prison and police physicians of the future. Such demands enter into the calculations of every one conversant with State medicine, and they only serve to illustrate the need for a more thorough psychiatrical curriculum in our medical schools.

These reception wards for fresh and temporary cases, constitute the ideal psychiatric clinic. The teacher who has such a clinic under his control, makes his diagnosis in the presence of his pupil, and the latter is brought face to face with insanity as it will present itself to him in the emergencies of his later professional career. Besides this, cases of questionable or simulated insanity, occurring in course of medico-legal

practice, could be advantageously placed here for supervision and observation, on a plan similar to that adopted on the continent. If you provide such an appendix of a general hospital in addition, with an analytical laboratory and appliances for conducting what are called "coroner's cases," you have all the component parts of a true "medico-legal" clinic.

In previous writings which have dealt with this subject, and which have advocated the institution of clinical instruction in insanity, whether with sincere or with selfish motives, it has been stated, in enumerating the advantages therefrom resulting, that insanity would be considerably diminished by being properly treated in the beginning. You may ask why I do not repeat this encouraging statement. For the simple reason that however well-meant it may be in some cases, it is entirely erroneous, and exhibits an unfamiliarity with the subject truly unpardonable in the case of those who have had "experience" with insanity. One of these writers states "the evils of the lack of attention to the pathology of insanity, and want of knowledge of the true character of the disease, are much greater than the public *are* aware of. If the result was only an indifference to the whole subject by medical men, it would be bad enough, but it is far worse. It is the application of a false theory, which not only deprives the sufferer of proper treatment, which he has a right to claim, but subjects him to wrong and injurious treatment, and especially to neglect of remedies in the beginning, when the disease is in its curable stage."

This is truly grandiloquent language, but it is nothing more than the same cant which is repeated *ad infinitum*, and used to substantiate the propriety of erecting needlessly expensive palaces for the insane, on the ground that their erection will eradicate insanity from the land. It also accuses the general medical body of neglecting the study of insanity; but who is to blame for this unquestionably existing neglect, in the light of past and recent revelations?

What is meant by "false theory"? What by the "curable stage of insanity"? What by the want of knowledge of the true nature of the disease, said to be much greater than the public is aware?

What the author means by these terms, we can hardly guess from his language: what we understand by them is as follows:

The "want of knowledge of the true nature of the disease" which is actually greater than the public is aware of, is to be found in the asylum whence that statement originated, an asylum whose superintendent, and pathologist, in ignorance of the pathological writings of celebrated authors on insanity, make a test of insanity by an autopsy, and proceed to say on the strength of its negative evidence that the subject thereof must have been sane.

By "false theories" we mean crude, illogical and commonplace evolutions from the inner consciousness of an asylum superintendent about crowded court-rooms and religious mental hygiene, and which go so far as to abolish or discontinue employment as a therapeutic agent in insanity, and leaves the patients to that monotonous and dreary idleness which may drive any imprisoned and comparatively sane man mad. By the "curable stage of insanity," we mean nothing at all, no more in fact than does the writer, for such a stage does not exist. There are various clinical forms of insanity, some of which have no stage at all, some of which are always curable at all stages, others which are incurable at all stages, and others, which if improperly managed in the beginning may pass to further and worse developments; the latter forms are proportionately rare. This discrimination the writer quoted does not appear to make, either from ignorance of the clinical history of insanity, or for reasons best known to himself. What finally we mean by the "proper treatment which the patient has a right to claim" is, first, that proper diagnosis be made; secondly, that the causation be considered and treatment applied to the root of the evil where this is discovered, or on proper symptomatic indications otherwise. We do not think that the crib-beds in extensive use at the institution in question, constitute the "proper treatment which the patient has a right to claim," but think with Professor Meynert that the struggles of the patient, his mental anxiety, and the horizontal position induced by these beds increase cerebral congestion to such a degree, so not only to occasionally cause death in a comatose or delirious condition, but in those cases

which pass out of the acute exacerbation (not because of but in spite of the treatment) to leave permanent pernicious results behind.

We conscientiously believe that the principles of proper treatment demand, that many cases of chronic mania, of paralytic insanity and of terminal dementia, cases which can be best treated at home, should not be sent to asylums, and that other patients in asylums should be dismissed as soon as it becomes evident that it can be done without risk and to their benefit. The surroundings of his family, the occupation of his vocation, often have a far better influence on the patient than the grated windows, crib-beds, bleak walls, gruff attendants, narcotics and insane surroundings of an asylum. It is to be feared, that in accepting what a leading medical superintendent is continually bringing before the Asylum Association, the Section of Mental Diseases in the American Association, and State Medical Society, as *his* physical theory of insanity, he loses sight altogether of the influence of moral and mental therapeutics. I know and I am properly resigned to the consequences, that in saying this I fall within that gentleman's much abused category of "people who speak about asylums, but do not and cannot know anything about them," but unfortunately for such assertion the position I have assumed is the position of Meynert, of Westphal, of Pinel, of Connolly, and of Tuke.

Instead of making promises which cannot be kept, and holding forth unrealizable prospects of the day when our State, having been financially ruined by erecting asylums on every hill-top, insanity shall be crushed out of existence (in which case I suppose that asylums, not being any longer required for their original purpose, will be transformed into armories for our militia, and poor-houses for our pauperized citizens), let us rather look the question squarely and honestly in the face, and determine what actually can be done! In the first place, the medical student, as well as the medical superintendent, can be taught insanity sufficiently thoroughly as not to be at a loss when mental alienation occurs in his practice as a complication of other diseases. He can be taught that when consulted by a family physician in certain instances of puerperal insanity,

and mild melancholia, it will not be always necessary to submit a delicately-nurtured patient to the shame and exposure of a commitment and transportation to a distant asylum. He can learn under competent teachers what are the causes of insanity, and where such causes are avoidable, how they can be avoided. As one of the advisers of society, he will have much to say on hygiene, on education, and on various other questions related to mental development, of individuals and whole communities. The knowledge, and the acknowledged probity of the general medical body, are looked upon as a safety-guard against abuses committed by members of that body, no matter how much those members may have wished to remove themselves from the supervision of their fellows. Accordingly this general body, if by special training fitted for such a duty, will be able to keep a careful watch on asylum matters, and in regard to hygiene, dietary, medication, and humanaritan considerations, constitute a more effectual prophylaxis against abuse than any supervision now existing. There is hardly a specialty in medicine which will not profit by the opportunities thus given of extending the scope of its investigation. Otologists and ophthalmologists have already turned their attention to insanity; renowned obstetricians and gynæcologists have offered interesting contributions to the subject of insanity in women. And the syphilographer has yet a wide field of exploration before him in regard to the various insanities due to impalpable changes occurring in the primary fever on the one hand, and to Heubner's changes in the vessels of the cortex, during later periods, on the other. When State medicine shall become a recognized field in our country, every district physician will be required to furnish statistics of insanity in his district. The statistics of insanity are among the most instructive and valuable which a State can publish. They enable us to watch the influence of pernicious causes, be they in the way of morbid religious excitement, of corrupt, sensational, and superficial methods of education, or of debauchery and drunkenness. Such statistics, in fact, point out clearly what habits should be avoided, and what popular systems require improvement. On the basis of this precise and reliable information, a general equitable and useful distribution of provisions

for the insane can be made amongst the various sections of a country.

No longer will organized coteries, by causing an artificial demand for asylums, on the plea that only expensive structures will serve that purpose, be able to crowd out more than half the insane in the State from the benefits of asylum treatment. No longer will the discharge of dangerous lunatics be permitted to swell the recovered lists. No longer will the manipulated statistics of a leading superintendent in this State be able to strew sand in the eyes of the public, and to serve as an excuse for preposterous demands.

And now, after having successively given my reasons for considering psychiatry as a branch of neurology, for negativing the fallacious claims of an exclusive circle, for recommending systematic instruction in psychiatry, and making some changes in AT LEAST the medical supervision of asylums, it behooves me to say a few words in the light of a personal apology.

In dealing with the flagrant and obvious shortcomings of medical officers, I have employed terms which it is customary to censure as being unnecessarily harsh. It is my impression, however, that if it has once been determined to thoroughly expose an imperfect or corrupt system, and if the results of such inquiry are founded on indisputable facts, that the abuses should be stigmatized as abuses, fearlessly and openly. Each separate word in the English language has its special meaning, and when I have used the terms ignorance, charlatanism, insincerity, and neglect, I have employed them because no other words could characterize so aptly the conditions to which I found it necessary to allude in the course of this inquiry. I have used these terms deliberately, and shall present, if challenged, the detailed proof which I have hitherto not given, as it would have necessitated that mention of names which it has been my chief purpose to avoid.

Previous and far abler writers, writers more familiar with the subject than myself, have arrived at many of the conclusions of this paper, and for the extreme delicacy and courtesy which they have shown the inner asylum circle by clothing their criticisms in the mildest language, have doubtless been

considered by the exponents of that circle not as generous, but as timid critics. Accordingly they have vituperated, maligned, or ignored these generous critics in a manner sufficiently characteristic, and which is exemplified by the manner in which the able writings of Drs. Storer, Folsom and Wilbur have been reviewed in the *Asylum Journal*.

Other strictures written in a perfectly objective and impersonal manner, it has been endeavored to disarm, on the score of a supposed personal motive existing in the minds of the authors; but strange to say, the arguments of these writers have not been disproved, they have not even been discussed!! Grave charges against the financial management, and against the reliability of asylum statistics have been recently published in the most widely circulated of American medical journals, which have not hesitated at mentioning the special superintendent and institution involved, and yet that superintendent has been, and is, dumb.

To this, as to all antecedent papers of a similar character, no other answer than the chorus already echoed *ad nauseam* from asylum to asylum—that its writer has never been an asylum superintendent, and can consequently know nothing about insanity, is expected. Since this is the sole argument which I have ever heard the defenders of the impeached system employ, let me ask, what is in this mysterious "asylum experience" that prevents those not possessing a superintendency from judging of asylum matters? What is the difference between a hard-working, able and trustworthy assistant physician of an asylum, and his superintendent, selected to fill his position on grounds of nepotism and political favor? It is a well-known fact that *this* "asylum experience" argument is not used abroad! The *British Medico-Psychological Association* counts among its members not only superintendents, but also their assistants and physicians in general practice; the *Medicinisch-Psychologische Gesellschaft* of Berlin, and *Verein fuer Psychiatrie* of Vienna, are similarly constituted; but it remained for the American Association of Medical Superintendents to announce that, on principles analogous to those which govern "trades unions," only medical superintendents could be members. No doubt the younger members of the profession, who

occupy the position of assistants, were excluded in order to prevent the rebellious tendencies of energetic and original workers from running loose and opposing the benevolent tendencies of certain superintendents, of keeping asylum matters in the same old conservative rut.

Is it "asylum experience" to examine whether the fence is high enough to prevent lunatics from jumping over? Is it "asylum experience" to go through the wards of a large asylum once a day, or, as has been found in a few instances, *once a month*, and to have no further relations with the patients than to pass by and receive their reverential salute, *à la* Grand Mogul? Is it "asylum experience" to let the assistant physician compile the really laborious part of the annual report, and to rely on him for information when an emergency calls him before court as a witness?

I think that the earnest psychiatric student, whether within or without asylum walls, has at least as good a right to discuss the subject as he whose claims to being a psychiatrist rest on the accident of appointment.

Where the argument of "asylum experience" has failed in its past application, the only resort of the factious minority which rules the Asylum Association has been to stigmatize all who ventured to question the immaculate perfection of asylum superintendents, as persons of *doubtful sanity*, or as meddlesome and troublesome intriguers, animated by personal motives.

Such taunts are but the cries of helpless indignation, and we can well afford to let impotence howl its very worst! Turning neither to the right or the left for such feeble opposition of the exponents of a corrupt system, founded on a traditional *laissez aller*, not on merit or real power, let us consider but the one question, whether the interests at stake in the scientific cultivation of psychiatry, are not too numerous, too noble and too important, to be left to be neglected by shallow pretenders and ignorant indifferentists.

To illustrate the implied inference, that excellent scientific work may be carried on without State aid, and *per contra*, that the existence of State aid does not *always* ensure genuine results, let me point to an asylum in England, the West Riding of Yorkshire, and contrast the work there done and

the manner in which it is done, with the same as performed in a prominent asylum, quite distinctly alluded to in this paper. At the West Riding Asylum, a number of enlightened and well-trained, enthusiastic and industrious young physicians engage each in some individual research, and assisted by the greater experience of the superintendent (such a man as Crichton Browne or Herbert Major, for example), publish their results under their own names, in the excellent reports issued by that institution. They receive no State aid whatever; their investigations are conducted in the purest interest of science, not for purposes of advertisement or of deceiving the public by proving the infallible (?) skill of an expert, who neglects the most vital points in his inquiry, and since he must prove his position at all hazards, resorts to the dubious and pliable testimony of an imperfectly performed autopsy (Waltz case, Grappout case; see NEUROLOGICAL CORRESPONDENCE). They lose no time in amusing themselves with the photographing of artificial precipitates, and their investigations extend to the domain of experimental physiology as well as to comparative cerebral anatomy. Herbert Major, the present medical chief, has not scorned to study the intimate structure of the Island of Reil in the Cynocephalus. The head of a similar psychiatric school, Meynert, of Vienna, has furnished perhaps more toward a proper anatomy of the brain, both of the surface and the deeper parts, both of man and other vertebrates, than any other anatomist now living! That such labors, instead of being prejudicial to, are really auxiliary to more practical inquiries, is demonstrated by the fact that therapeutical and clinical discoveries, possessing the highest value, are made at these very institutions.

At the American asylum to which reference is made, a special pathologist is appointed, *who is not a medical man;* he is sometimes styled "Professor," but of what—or where—is not stated! His labors are *not* published under his name; they cost the State $7,000 annually, as far as we can obtain a clear insight into the matter, and consist of thirty pages of desultory, fragmentary, vague, and utterly inconsistent cases, reported with the pathological findings, in the annual reports; sometimes only six pages are thus filled, and occasionally pamphlets are issued similar to one referred to above.

At the West Riding Asylum, besides the superintendent and his assistants, a number of so-called "clinical clerks" do medical service, gratuitously; their sole recompense consists in the opportunities for observation and inquiry offered by the material collected in the asylum, and the privilege of utilizing this material under the skilled direction of a scientific alienist. Our New York city asylum superintendents often complain that they cannot induce even recent graduates to serve at their asylums; why is it that the West Riding of Yorkshire, remote from any capital city or medical school, can not only command gratuitous services, but even select the best men from among a large number of competitors? The answer is clearly, that in the latter instance, the enthusiasm, learning and integrity of the medical chief, offer the highest inducements to the scholar, while such inducement does not exist where an indifferent, superficial man, owing his position merely to political buffoonery, is the medical head of the asylum, as in the former case.

ART. II.—ON THE THERAPEUTIC USES OF SOME OF THE RANUNCULACEÆ, ESPECIALLY IN NERVOUS DISEASES.

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(*Read before the Medical Society of the County of New York, Dec. 17th*, 1877.)

THE principal varieties of Ranunculus or crow-foot tribe are 1st. Ranunculus acris or flammula; 2d, Helleborus niger; 3d, Delphinium staphisagria; 4th, Aconitum napellus; 5th, Cimicifuga racemosa; 6th, Pulsatilla; 7th, Podophyllum peltatum; 8th, Hydrastis canadensis; 9th, Coptis trifoliata.

It will be seen that some of the mildest as well as the most powerful remedies are included in this botanical class. Thus:

COPTIS TRIFOLIATA (OR GOLD THREAD)

has few special virtues beyond those of the simple bitters to recommend it, except perhaps some of the antiperiodic powers of the *berberina* which is contained in it. It is not an astringent, for no tannin has been found in it; but, like several other plants, characterized by bitterness and a yellow color, it contains the alkaloid BERBERINA, than which few, if any of the known alkaloids, are so widely diffused in the vegetable kingdom.

BERBERINA

is especially abundant in the hydrastis canadensis, calumba and other plants, and from its extensive diffusion in numerous vegetable remedies there is little doubt that it is possessed of valuable curative properties. It may be desirable to test thoroughly the antiperiodic virtues of berberina; but until *chinoidine*, the black and intensely bitter residue left after the alka-

loids have been crystallized out of the mother liquor of cinchona bark, have been thoroughly utilized, this may be unnecessary.

CHINOIDINE

probably contains amorphous quinine and cinchonia, besides quinidia and cinchonidia. It is an excellent tonic and antiperiodic, and on account of its cheapness is largely used, not only in some of the charitable institutions, but is being largely introduced into private practice. It is supposed by some that chinoidine is more effective in chronic malarial disease than quinine itself; especially when important changes have been produced in internal organs, including the liver, spleen, and cerebro-spinal axis. The dose of berberina is from 1 to 10 grains; that of chinoidine, or its sulphate, in malarial diseases, about 5 grains every four hours, night and day, without any reference to paroxysm, intermission, remission or exacerbations.

HYDRASTIS CANADENSIS.

Many of the peculiar virtues of this remedy are doubtless due to the alkaloid berberina, which is contained in it in the proportion of about four per cent.; in fact, the so-called hydrastin of the eclectics is really the muriate of berberina, while genuine hydrastiä is the active principle of the plant, barring berberina, and is distinguished for the resemblance of its action both to quinine and pulsatilla.

The hydrochlorate of berberina is said to be a tonic with a special affinity to the mucous membranes, in many diseases of which, including catarrh of the eyes, ears, nose, throat, stomach and bowels, and even those of the gall ducts and genito-urinary passages, is said to have been efficacious. But all this must be erroneous. The fluid and solid extract, and other preparations of hydrastis canadensis may, and probably do have these beneficial effects, but berberina and its salts do not. Berberina is almost the sole active principle of the simple tonic calumba, while hydrastin, or hydrastia, the true alkaloid base of the hydrastis root, has those interesting effects which make it resemble quinine as an antiperiodic, and pulsatilla is a specific remedy for many diseases of the mucous membranes.

The ordinary uses of hydrastis canadensis are so well detailed

by Bartholow and the eclectic and homœopathic physicians, that it is not necessary to repeat many of them here. We will merely state that it is said to act much like quinine on the head, and to rank next to quinine in the cure of recent malarial diseases; while it is said to excel that remedy in the removal of those obstinate and obstructive complications of gastro-intestinal and portal disturbance which renders some intermittents so intractable. It is only excelled by the muriate of ammonia and pulsatilla when given in association with quinine.

Although hydrastis is said to exert a marvelous control over chronic catarrh, and even erosions of the mucous membranes, wherever situated, from the throat to the uterus or anus, it is singularly enough said to be specific for chronic constipation when dependent upon deficient secretion of intestinal mucus, when the stools are dry and hard; while torpor of the muscular coat is not removed by it, but requires the addition of ergot, nux vomica, physostigma, actea, and other like-acting remedies. As an antiseptic it is said to be only surpassed by quinine and salycilic acid. It is finally said to be useful in those glandular swellings which arise from absorption from diseased mucous membranes, and also in some varieties of epithelial cancer.

ACTEA RACEMOSA.

Although this remedy has been used in the regular school since 1831, its physiological action is not well understood. Its taste and smell are much like that of opium, for which it has been substituted in many nervous affections, especially chorea, melancholy, and some rheumatic affections. Unlike opium, it promotes the appetite and digestion, and increases the contractility of unstriped muscular fibre, somewhat like ergot, but more energetically, and rather tends to produce free action of the bowels. Like opium, it increases perspiration, and very large doses influences the brain, causing considerable soporific and anodyne effects, while it also slows the heart-beats, but increases their force and elevates the tension of the arterial system.

It has been used largely, and apparently successfully, in chorea, both of the rheumatic and hysterical varieties; in spinal

irritation coming up from rheumatic affections of the uterus, or of the spinal marrow and its membranes. It is one of the numberless remedies for amenorrhœa and the crowd of nervous symptoms which sometimes attend it.

Its power to relieve other kinds of pain is apparently well established. Bartholow says, in neuralgias of the third pair, arising from cold, in rheumatic headache and ovarian neuralgias succeeding to suppressed or arrested menstruation, it is frequently effective. But in rheumatic neuralgias, I prefer a combination of aconite and colchicum, from abundant experience, although I am often obliged to add a small quantity of morphine. A prescription which I often use, and with increased confidence is, tincture of the root of aconite, 1 drachm; tincture of the seeds of colchicum, 7 drachms, in doses of five to twenty drops, frequently repeated. Occasionally I diminish the colchicum by 1 drachm, and substitute Magendie's solution of morphine.

The pains of dysmenorrhœa of the congestive or rheumatic variety are relieved by actea, although it has been used to promote parturient efforts and induce uterine contraction. It is also said to be useful in subinvolution of the uterus.

In acute rheumatism it so often fails, that the common saying, "Actea will not act here," has grown into a household word.

In some catarrhal rheumatic affections it is said to be useful, but is probably less so than pulsatilla and hydrastis canadensis. But Bartholow says it is an excellent expectorant in bronchitis, catarrhal and caseous pneumonia, and incipient phthisis. He uses a combination of 4 drachms of the fluid extract of actea racemosa, with 2 drachms of deodorized tincture of opium, with 10 drachms of the syrup of tolu, in teaspoonful doses every 4 hours. Also in common colds, and acute catarrh or bronchitis, but it is very doubtful whether it is any more useful than the common and pleasant mixture of 1 ounce of syrup of squills, with an equal quantity of paregoric, in 2 ounces of syrup of wild cherry.

PULSATILLA,

it is well known, is one of the oldest remedies. It was recommended by Dioscorides in foul ulcers and inflammation of the

eyes, and Pliny advised it in headache, affections of the eyes, and in malarious fevers. But it was on the authority of Baron Stoerck, in 1751, or four years before Hahnemann was born, that it was again introduced into practice. He experimented with it on healthy men and women, and on his own person. He used the expressed juice of the fresh plant when it was in its greatest activity, and also used triturations with sugar of milk, just as potash is used to triturate ipecac and opium in Dover's powder. He selected sugar of milk as a hard, gritty substance, which would readily comminute and tear asunder vegetable remedies when triturated with it, and also because it was a simple or indifferent substance, which would not alter or impair the virtues of the medicines manipulated with it. Hahnemann appropriated this and other remedies, including aconite, from Stoerck, and never gave him proper credit. He also got his ideas about the aggravations of diseases by remedies from Stoerck, who having suffered much for two years from a violent contusion of his eye, took pulsatilla on the recommendation of Dioscorides and others, and soon noticed a severe lancinating pain in the part affected, which he construed into a favorable omen, an opinion which he says was confirmed by his own recovery and that of others under like circumstances. The diagnosis of diseases of the eye was not as accurate in 1776 as it is in 1876, but Stoerck claims that two cases of partial amaurosis and ten of opacities of the cornea, were either much benefited or cured by him with pulsatilla.

Stoerck also noticed that several of his female patients, while taking pulsatilla experimentally, or for other diseases, were relieved of amenorrhœa of from one to three years' standing. Hahnemann also appropriated this experience, and pulsatilla, although it has no homœopathic relation whatever to amenorrhœa, is much relied upon by the members of his school. Of course all so-called cures of amenorrhœa with infinitesimal doses must be regarded as recoveries by the unaided operation of nature.

Hahnemann was preceded by Paracelsus in the exclusive adoption of the law, *similia similibus curantur*. Yet he never gives him credit for it. He appropriated his ideas about experiments with drugs on the healthy from Baron Stoerck, also

his mode of preparation of many of his medicines by expressed juices and trituration, and also his notions about aggravations, yet gave no credit to Stoerck. His vagaries about infinitesimal doses he got from Mesmer, and also gives him no credit.

The history of therapeutics, in a nutshell, is that Hippocrates claimed that all remedies which acted at all *differently* from the diseases they were to relieve, might cure them, be the difference so slight as almost to amount to complete similarity (for similarity always includes some difference), or so great as to amount to antagonism, for that is merely the greatest degree of difference.

Galen established the law *contraria contrariis curantur*, and rejected all remedies that did not act exactly opposite to what he supposed to be the action of the disease.

Paracelsus took the other extreme, and assumed that remedies which acted similarly yet somewhat differently would also cure.

Hahnemann based his system upon appropriations from Paracelsus, Stoerck and Mesmer, but especially by numberless quotations from the older materia medicas. If these be stricken out, nothing of Hahnemannism will remain, for his own experiments upon himself and others were of the most trivial kind. Not one symptom in one hundred recorded in his materia medica, or that of Hering and other credulous disciples, is or can be proven. They are wanting in all scientific probability and proof, and must be utterly discarded as attempted frauds upon the medical world. It is not right for any one to palm off any and every notion which may enter his brain, and any and every sensation which he may feel as the undoubted and positive effects of drugs.

Pulsatilla is said to rival hydrastis and the alkalies and balsams in its action on all the mucous membranes. It also acts upon the skin, causing profuse and offensive sweats, also some eruptions. Baron Stoerck recommended it in various rheumatic affections arising from check of perspiration, and it is generally supposed to be a remedy of prime import in catarrhal-rheumatic disorders, including some synovial affections, for mucus is not secreted by mucous membranes alone, but is always found in small quantities in synovial fluid. Senega

and squills are allied remedies, and the former, like pulsatilla, has also attained some reputation in functional amenorrhœa arising from atony, torpor and excessive thinness and delicacy of the uterine mucous membrane, which together with ovarian excitement plays such an important part in this menstrual process. It is also supposed to act specifically upon the testicles, ovaries and female breasts.

Anemonine, the active principle of pulsatilla, is also found in helleborus niger, and is said to produce marked hyperæmia of the membranes of the brain and cord, especially in the neighborhood of the medulla oblongata. It requires from 5 to 10 grains to kill a rabbit, and is given in doses of $\frac{1}{2}$ or 1 grain, in some coughs, rheumatic gout, and other affections.

HELLEBORUS NIGER,

is also one of the oldest remedies, which was once the great purgative of the ancients, before the discovery of aloes, jalap, colocynth, calomel, rhubarb, podophyllum, etc. It was thought to be especially useful in all those disorders which were supposed to arise from a redundance of black bile, such as melancholia, hypochondriasis, even mania, epilepsy, dropsy from diseases of the liver, etc.

It was so celebrated that every year numerous patients traveled to Anticyra, where the best hellebore was supposed to grow, as people now go to take Saratoga, Cheltenham, Epsom, Pullna, Friedrichshall, Hunyadi and other purgative mineral waters.

As a drastic purgative it was used in dropsy, and Hahnemann and his disciples have gravely recommended infinitesimal doses of it, especially in dropsy of the brain, to which it has no more homœopathic relation than croton oil or elaterium. All such reputed cures must be regarded as recoveries; but as the majority of the cases of hydrocephalus proceed from general tuberculosis and tubercular meningitis, there are necessarily few where a correct diagnosis is made.

Hellebore has two active principles, called helleborin and helleborein, but one of them is equivalent to anemonine. The great force of the remedy is spent upon the gastro-intestinal mucous membrane, especially of the colon and rectum;

while in female animals the uterine mucous membrane is invariably found congested. It is now used almost exclusively in amenorrhœa, or post-scarlatinal dropsy, in both of which it is useful.

PODOPHYLLUM PELTATUM

is supposed to act decidedly upon the gastro-intestinal mucous membrane, especially upon the duodenum, and secondarily upon the liver, as large doses are followed by free discharges of bile. It may act by continuous sympathy from the duodenum; or some of it may be absorbed by the portal circulation and carried to the liver. Its action is said to be slow and long sustained, as its action is not apt to be followed by constipation, but rather by a regular action of the bowels, for some time subsequently.

But constipation is often a nervous affection,' as after section of the right pneumogastric nerve (which is distributed to the intestines) in the cervical region, the most powerful cathartics, like croton oil, calomel, podophyllin, jalap, etc., fail to produce purgation, even in doses sufficient to cause death. In these cases nux vomica, ignatia, ergot, physostygma, and other remedies of like character are required, although the addition of ordinary purgatives may be required at first in order to produce necessary palliative relief.

Anstie found that when an alcoholic solution of podophyllin was introduced into the peritoneal cavity, no inflammation was caused there; but the same preparation caused intense hyperæmia of the small intestines, which usually ceased at the ileo-cæcal valve.

DELPHINIUM STAPHISAGRIA,

corrupted into stavesacre, is the rival of aconite in many neuralgic affections. Philips uses the tincture of the seeds in obstinate neuralgias of the facial nerve, and of the superficial cervical; he says he often succeeds when all other remedies have failed.

ACONITE

is so well known as hardly to require mention except that it was a favorite remedy with the ancients, and like pulsatilla, was re-introduced by Baron Stoerck, by experiments on the healthy

and sick, in 1742, or thirteen years before Hahnemann was born. Stoerck called particular attention to its sudorific power, and recommended it in fevers, inflammations, but especially in rheumatic and neuralgic affections. It is a powerful arterial and cardiac sedative, but has not the slightest homœopathic relation to fever or inflammation; it is almost the direct antagonist of these diseases, and all the cures reported from the use of infinitesimal doses, and they number thousands and hundreds of thousands, must be regarded as recoveries by the unaided operations of nature.

Hahnemann did not discover its peculiar action on the nerves of sensation, and my experience leads me to believe that the long-continued use of small doses is far more useful in the most obstinate and chronic forms of neuralgia, especially of the rheumatic variety, than is generally supposed. Gubler assigns it an important role in congestive neuralgias and also in those which he calls *aerodynie*, which have their seat in those parts where the Pacinian corpuscules are most numerous. He says that it is more useful than large doses of morphia or atropia.

Stoerck described aconite as a nervine, and narcotic, diuretic and diaphoretic, and recommended it especially in acute and chronic rheumatism, and in all fevers and inflammations arising from rheumatic causes, such as exposure to cold and wet; in gout, neuralgia, and to relieve the pains of scirrhus.

ART. III.—SOME TROPHIC DISTURBANCES OF THE INSANE.

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(*ILLUSTRATED.*)

TROPHIC disturbances are so frequently connected with neuroses, that it is not very surprising to find similar disturbances associated with insanity, also a disease of the central nervous system.

Any attempt to collate these and establish their relation to insanity must from the circumstances of the case be more or less imperfect. The subject has not specially attracted attention, notwithstanding the fact that it has important medico-legal relations, which latter circumstance may serve as an apology for the presentation of this paper. Resting chiefly on my own observations, as it does, it is necessarily imperfect.

The trophic disturbances found in the insane fall naturally into two classes, those essential to the disease and connected with its symptoms, and those modified by coexistent insanity, but found also in allied neurotic conditions. The trophic disturbances which are peculiar to insanity are hæmatoma auris, the hæmatoma of the lower intestines, the so-called electrical condition of the hair, and finally, peculiar changes in the hair and skin.

There are other peripheral abnormalities in strongly marked hereditary cases, but these are teratological in their nature, and are merely alluded to here to exclude them from consideration.

The insane ear is a marked phenomenon, and every writer on insanity has thought it necessary to make some allusion to this condition. The symptoms are so well known that it is scarcely necessary to describe them. What is chiefly note-

worthy about hæmatoma auris seem to me comprisable in the following four questions:

First, what is its frequency, and in what forms of insanity is it likely to occur? Secondly, is it peculiar to the insane? Thirdly, what are its pathology and relations to prognosis? Fourthly, what is its etiology?

In my observation, embracing twenty-two hundred and forty-three cases of insanity, it has occurred but forty-eight times, or in about two per cent. of the cases.

The following table gives an answer to the first question as far as my experience extends:

FORM OF INSANITY.	No. of Cases.	Cases of Othæmatoma.
Mania, Acute	260	4
*Mania, Chronic	467	6
Melancholia, Acute	301	1
Melancholia, Chronic	260	5
†Folie Circulaire	50	6
Hebephrenia	112	7
‡Dementia, Senile	82	1
Dementia, Terminal	334	5
Katatonia	46	1
Epileptic Insanity	101	5
§General Paresis	284	7
	2,297	48

* Once bilaterally. † Four times. ‡ Before a maniacal attack. § Twice.

From this it would appear as if those forms of insanity most liable to vaso-motor changes were those in which the insane ear oftenest occurred. I may say in passing that I have observed it in idiots and imbeciles, and the best marked case of idiocy on Randall's Island has the remains of a true othæmatoma.

Roosa and Loring have observed three cases of othæmatoma in the sane. I am not acquainted with the histories of these cases, but one of a similar kind which came under my observation, though not insane, clearly belonged to that class of beings whom Maudsley has placed under the head of insane temperament.

From my own observation I am inclined to accept Hun's opinion: "An individual who is thought to be sane, but who has an othæmatoma, must be the victim of a grave cerebral disease."

According to Virchow the essential morbid process is a softening induced by the general disturbance of nutrition, or possibly by local injuries of the cartilage. He says, "The older authors described the affection as erysipelas of the auricle occurring in the insane." It was supposed that in the hyperæmia and general change in the system there ensued a subperichondrial hemorrhage, but in true othæmatoma the hemorrhage is inter-cartilaginous. In my own observation, the latter has been the case only in the instance of the othæmatomata occurring in general paresis, and was, I think, a complicating trophic disturbance.

The prognosis, when hæmatoma auris occurs, is said to be grave; but it seems to me that the othæmatoma has no absolute bearing on prognosis, occurring, as it does, chiefly among the chronic cases.

Of Hun's twenty-four (24) cases, eight were cases of general paresis, six melancholia, four acute mania, four chronic mania, two dementia. Nine of these died in the asylum, nine were discharged unimproved, and the remaining six ended in dementia. The last number of the *West Riding Reports* gives an account of two cases of othæmatoma which ended in recovery, as did three cases that came under my observation, but such are, as might be expected, rather rare. My own three, and those mentioned in the *West Riding Reports*, are the only cases known to me amongst two hundred and twenty othæmatomata. In one of my cases which recovered, the othæmatoma responded readily to treatment by numerous slight punctures, which were attended with very little resultant deformity. This case was as follows:

G. C., single, German, æt. 26, boatman, intemperate, fair education, admitted July 8th. Excitable, violent and destructive, very incoherent. Has been in good physical health up to about two weeks ago, when he was thrown out of employment. He encountered much difficulty in searching for work, met with many slights, grew morose, went on a prolonged spree, after which became abstinent, and remained so two days before admission, when he suddenly became excited, pitched furniture out of the window and grew very violent. On admission, came in handcuffs, was incoherent and talkative, but answered some questions connectedly.

He had a bruise on the right side of his head. He continued incoherent, destructive and loquacious for about two weeks after admission, at which time there appeared an hæmatoma on the left ear. In a week he began to improve, and about the fourth of August, was considered recovered; the hæmatoma, which had been treated as before described, had disappeared, causing slight deformity, only noticeable on close inspection.

He was sent out on furlough, but not returning a month afterwards, and still continuing well, was discharged recovered.

In another case the appearance of othæmatoma preceded by two weeks the sudden and complete disappearance of delusions, and actions based on them.

A third passed into dementia, or rather melancholia, with stupor, the sudden disappearance of which was preceded by the occurrence of an othæmatoma on the left ear.

The etiology is a very much disputed question. Roosa holds the compromise view that there are two distinct forms, in one of which the swelling, etc., are produced by violence, the other by nervous causes arising from the condition of mental alienation. Virchow's views have already been given.

Gudden has shown that the auricles of ancient statues are frequently ornamented by tumors resembling the othæmatoma. The statues of Hercules, Pollux and the Trojan Hector, are said to have these tumors. These are claimed to be characteristic of the ancient boxers, yet, as Roosa remarks, the English authorities, living in the land of pugilists, scarcely mention them. The mental condition of the ancient boxers we do not know, and the fact that the English boxers seldom or never have had the tumors, fairly offsets any conclusion to be drawn from the statues of ancient boxers.

Gudden seems to believe othæmatoma is generally due to violence, and disputes the basis of explanation given by L. Meyer, that the reticular cartilage of the ear in all ages, always contains vessels. Hun claims that traumatic causes always produce a different form of othæmatoma from the hæmatoma of the insane, and that the causes of the latter are twofold, cerebral congestion and centripetal irritation of the system by the emotions.

I have seen the traumatic form, but this resembled most the

ordinary blood boil, not the true hæmatoma. There were in three cases on one ear true othæmatoma of undoubtedly idiopathic origin, while traumatic causes had produced on the other ear swelling and ecchymosis, which contrasted markedly with the hæmatoma. Brown-Sequard believes that othæmatoma is due to disease at the base of the brain, as is illustrated experimentally by the fact that section of the restiform body of the medulla oblongata in guinea pigs will cause hemorrhage, followed by gangrene, which latter condition would not occur, from the greater resisting power of the human ear.

Sankey gives the following explanation: "There are several veins which pass through the mastoid process of the temporal bone in an oblique direction, and so join the veins of the inner table, or empty their contents directly into the lateral sinus at the base of the skull."

It is also well known that the bony case of the skull in lunatics becomes more dense and compact, and thus arises a constriction or obliteration of the veins passing through the bone and œdema of the parts whence the veins come. When this swelling occurs, it therefore indicates densification (Sankey's term) of the skull bone has taken place. Hæmatoma of the lower intestines, as found in the chronic insane, occurs most frequently in chronic mania and general paresis. Its pathology is much the same as the hæmatoma of the ear, although no suspicion of traumatism can be here raised. Unless the patient be attacked by a slight diarrhea, the change in the intestine will not be suspected; but should this diarrhea occur three hours after, suddenly there will be noticed a bloody discharge which is very soon succeeded by peritonitis, and the physician at the autopsy will be astonished by the extent of the perforation which slight symptoms have given rise to.

The so-called electrical condition of the hair is apparently a very trifling circumstance, yet any one who has had much experience with the chronic insane will not hesitate to assign great prognostic importance to it. According to some, after the rough hair has occurred, a change to smooth is the evidence of a favorable mental alteration.

I cannot say aught as to truth of this, but of the opposite change I can speak most positively. Several times has an

apparent improvement in the patient's mental condition suggested the propriety of an early discharge, but the erect coarse hair, contrasting markedly with the smooth sleek condition in which it was when the patient entered the asylum, has caused a hesitation in discharging him which the subsequent history of the patient fully justified.

Three patients whose mental condition, as far as could be ascertained, justified their discharge, but in whom the erect hair had persisted, returned within one week after their discharge as recovered.

The changes in this condition are interestingly narrated by Charles Darwin, in his "Expression of the Emotions," pp. 295–297: "As I did not feel sure whether writers of fiction might not have applied to man what they had often observed in animals, I begged for information from Dr. Crichton Browne, with respect to the insane.

"Dr. Browne remarks that the bristling of the hair, which is so common in the insane, is not always associated with terror. It is perhaps most frequently seen in chronic maniacs who rave incoherently and have destructive impulses; but it is during their paroxysms of violence that the bristling is most observable.

"The fact of the hair becoming erect under the influence both of rage and fear, agrees perfectly with what we have seen in the lower animals.

"Dr. Browne adduces several cases in evidence. Thus, with a man now in the asylum, before the recurrence of each maniacal paroxysm the hair rises up from his forehead like the mane of a Shetland pony. He has sent me two photographs of two women taken in the interval between their paroxysms, and he adds, with respect to one of these women, 'that the state of her hair is a sure and convenient criterion of her mental condition.'

"Dr. Browne mentions an empirical confirmation of the relation which exists in the insane between the state of their hair and minds, that the wife of a medical man, who has charge of a lady suffering from acute melancholia, with a strong fear of death for herself, her husband and children, reported verbally to him, the day before receiving my letter, as follows: 'I

think Mrs. —— will soon improve, for her hair is getting smooth, and I always notice that our patients get better whenever their hair ceases to be rough and unmanageable.' Dr. Browne attributes the persistently rough condition of the hair in many insane persons in part to their minds being always somewhat disturbed, and in part to the effects of habit; that is, to the hair being frequently and strongly erected during their many recurrent paroxysms.

"In patients in whom the bristling of the hair is extreme, the disease is generally permanent and mortal; but in others, in whom the bristling is moderate, as soon as they recover their health of mind the hair recovers its smoothness."

Temporary pigmentation of the skin occurs in other conditions than insanity, but permanent alterations are rare.

The disappearance of pigment in the negro race has been reported in but few cases, in most of which there was some change in the patient's mental condition, judging by the report.

Both these conditions have been observed in about thirty-five cases at the New York City asylum, the most interesting of which are the following:

CASE I.—G. H., æt. 18; single, abstinent, no nervous taint in family, admitted to N. Y. C. A., Dec. 1874, with the following history: He had always been healthy up to the age of 16, when he had much diarrhea and was much run down. About two weeks after recovery from this, a large carbuncle made its appearance over the third cervical vertebræ, on the left side of the neck, which did not heal up for over a year, but finally all disappeared, leaving a very deep scar, which is still visible. He was a masturbater, and three weeks after recovery from the carbuncle, while engaged in this practice one day, felt, as it were, something crack, and very soon after was attacked with chorea-like twitching of the left side, and after which his skin and hair changed in color on that side, sharply demarcated, irregular brown specks of from one-fourth to two inches in diameter appearing on the same side, the hair became gray in patches, in sharp contrast with the youthful brown. He had pathetic delusions of grandeur, and was very theatrical in his way of acting at this time. On admission the patient complained first, of a curious sensation coming from his neck,

then of a desire to laugh, then of sudden destructiveness. He is morbidly anxious about the changes in his hair, and skin, and had periods, about twice in twenty-four hours, when he would tear everything in his reach; if engaged in eating would stop, drop his knife and fork, and as suddenly seize them, making the most horrid grimaces. He broke the cups and saucers and threw them about wildly. He would at one time assume the most diverse, statuesque attitudes, and when he had done wrong, would pass into a cataleptoid state, always preceded by pain starting from cicatrix of carbuncle and running along left side of head. He still remains in this condition.

CASE II.—D. N., cook, æt. 56, negro, intemperate, single, was attacked about three years before, first with a sudden inability to see more than half an object at a time in the left eye. His memory became at the same time somewhat impaired. He could not recollect the tools of his trade, but if told them, could remember their names for some time after. His intelligence was somewhat impaired, though he continued to perform his business until two years before admission, when he was suddenly seized with a fit, his left side, at the time, appeared paralyzed, and he was unable to give even his own name, but was able to write it partially, the greater portion being composed of figures; this was about six months before admission. From the aphasia and hemiplegia he soon recovered, but gradually his skin became white in patches all over the left side, and his hair was grayish in spots on the same side. The change was very marked on the left side of the forehead. Unlike other cases of aphasia he was not much alarmed at his condition, but appeared very hilarious and jovial; soon after the disappearance of the aphasia, which occurred six months after admission, he was found to have very stupid ideas of grandeur, and the physical symptoms were those of general paresis. He was transferred about this time to another institution and lost sight of.

CASE III.—J. A., Scotch, type-setter, æt. 36. Three months before admission, early in 1876, had lead colic, succeeded by an attack of drop wrist, followed in turn by hemiplegia and amnesic aphasia; is said to have recovered from this under anti-saturnine treatment, but very slight spots began to appear on neck and

face with localized changes in hair and vertex. On admission, presented the usual symptoms, mental and physical, of progressive paresis. Four months after admission, he began to complain of a band-like sensation at first lumbar vertebræ, with electric pains, and his locomotion was markedly impaired. At this time he was found to have forgotten the tools used in his trade, and was at length confined to bed. His skin had become markedly clear and white on the face and neck, in marked contrast to the normal dark hue of the rest of the body.

A fourth case, a negro, was in the habit of being wet cupped over the fifth cervical vertebræ for several years. Three years before admission, Aug., 1876, he stopped the practice and had a severe attack of cerebral congestion, accompanied with enlargement of a gland in the neck and temporary aphasia. These symptoms passed off in about a year, but were succeeded by marked symptoms of progressive paresis, accompanied by alterations in the color of hair and skin, most marked in the side of the facial abnormality, and beginning at the centre of the forehead. The symptoms of paresis, to a great extent, passed off under the use of wet cups, but the patient is in a condition of dementia, and the changes in the skin and hair remain, but do not extend further. These changes in the hair and skin are peculiar to hebephrenia, chronic mania, and general paresis, and like the othæmatoma appear most frequently in forms where vaso-motor changes occur. Of the thirty-five, ten were paresis, ten hebephrenia, one folie circulaire, two chronic melancholia, six chronic mania and four epileptic insanity, and two terminal dementia. Even in chronic mania certain vaso-motor changes were evidently the basis of abnormal sensations, which preceded at the place of these sensations the trophic changes.

CASE V.—J. B. S., æt. 34. British North American, moderate drinker, single, sailor. Father moderate drinker, died of apoplexy. Family very intelligent, mother fifty-two years old when patient was born. The patient since birth has been eccentric, and for four years previous to admission, was subject to brief attacks of mental excitement, which amounted almost to insanity. About two years before admission, became

insane after exposure on Pacific Ocean; remained so for one year, and has never been the same since. Admitted to Asylum Sept. 11, 1877. On admission, answers questions reliably, but has the insane manner. Probably has delusions; no emotional disturbance. He lately arrived in New York, spent his money, and was summarily arrested for lying on a sofa in a dry goods store. He is slightly incoherent in general conversation, and has a singular way of answering which is not natural to him; is reticent in regard to the immediate effect of his past life; has no hallucinations of hearing or vision and is in good physical health. He became excitable, and continued so with intervals of relative calmness for about thirteen months. This excitement appeared to affect his general health, and he was thought to be sick, but this was found to be subjective, and owing to abnormal sensations of electricity. Five days he refused to eat his food from a tin dish, but declined to give his reasons therefor. The active maniacal symptoms ceased about this time, and the abnormal sensations were soon followed by local changes in pigmentation at the points of sensation. In the course of the following month he complained of severe pains in the head, followed by localized changes in the hair at the spots where the pain was felt. The patient continues in this condition.

One condition peculiar to insanity is the deficient capillary circulation of the extremities, found in the stupor following atonic melancholia and epilepsy; this results in about five per cent. of the cases in gangrene. The trophic changes modified by, but not peculiar to insanity are mostly confined to general paresis, epileptic insanity, though some have appeared in hebephrenia and katatonia.

(The enlargement of the thyroid gland which occurred during the initial symptoms of the latter disease in one case mentioned in my paper on katatonia, I refer without hesitation to this origin, as may also be said of a synosteosis of the joints of the great toe, which occurred during the progress of a period of excitement in a case of hebephrenia.)

These changes are peculiar skin affections, necrosis of the bones, of idiopathic origin, fragility of the ribs and other bones, necrosis of the lungs, peculiar gangrene differing in

course from the ordinary kind, peculiar arthropathies and a tendency of injuries to heal with suppuration and almost by first intention.

There are other conditions which certainly appear to have some relation to the trophic disturbances, the asymmetrical temperature in paresis and presence of scorbutus without apparent cause.

General paresis is likely to come under the observation of the physician in two forms: in one there is a peculiar clearness of skin and tendency to emaciation with a likelihood of slight injuries taking on gangrenous action; the other is the ordinary form.

It is in the former that the trophic disturbances take place with greatest rapidity, that "bullæ" form with greatest quickness and proceed to gangrene.

Prominent amongst the disturbances is fragility of the ribs. This occurs in about 25 per cent. of the paretics, but frequently passes unnoticed even at the post-mortem.

It may sometimes lead to serious consequences. A patient enters the asylum, is placed in a room with other patients, or even in a single padded room, tumbles about and is found in the morning with several fractured ribs. A very striking instance of this lately happened at the asylum with which I am connected. A paretic, about 40 years of age, an ex-politician of the lower grade, was admitted, passed one night relatively quiet, being under restraint: but the night following the restraint was removed, he attempted to choke a fellow patient. An attendant was hurriedly summoned, drew him off, and a struggle ensued. The result was that the paretic received fractures of eleven ribs, more or less comminuted; a condition of things which the exterior of the body would scarcely lead us to expect. A sufficient explanation of which is the fact that there was found at the post-mortem to be as much ossification of the costal cartilages as if the man was eighty.

This condition, I may add in passing, has been the subject of medico-legal investigation in England. In the patient whose spinal cord has first been shown, an attempt to open the thoracic cavity with a cartilage knife resulted—although no great violence was used—in fracture of six of the ribs on one

side and three on the other; the ribs being very fragile and spongy. His history is as follows:

CASE VI.—H. C., American nativity, of intemperate habits, ordinary education, was admitted August, 1876,—antecedent history not obtainable—is somewhat excitable but rather inclined to be jovial, is in fair physical condition, but the skin has the peculiar clearness of the asthenic type of general paresis. The patient refused food, and had a voracious appetite alternately. He was very destructive and had rather exalted ideas at times. Bullæ began to appear on the joints of the toes, and fingers of the right side, and proceeded rapidly to gangrene as the patient continued to walk about. He complained, at times, of shifting pains. His co-ordinating power towards the last was very much impaired, and he became markedly ataxic. There was no history, and evidence of the existence of syphilis was wanting. The right tibia became painlessly and gradually enlarged. His tendency to get out of bed rendered night watching necessary, and he was transferred to the hospital ward. Soon after his transfer there he became very fat, in marked contrast with his previous condition. The enlargement was not marked in the arms, although the thighs and hips appeared much increased. This condition continued for about a month, when he began to grow thinner, more and more bullæ appeared on the extremities on unexposed surfaces, especially on the right side, and these soon spread into gangrene. The patient died December, 1876, six months after admission, and the following appearances were noted at the autopsy:

Thoracic cavity: condition of ribs already described; lungs and heart normal; abdominal cavity: kidneys normal; spleen and liver normal; intestines as usual; head: cranium thin and brittle; dura mater was thickened and presented other evidences of pachymeningitis, more marked on the hemispherical convexity than was the dura of spinal cord. The leptomeninges were thickened and infiltrated, more on the posterior than on the anterior aspect of the cord. On the fresh specimen a reddish-gray discoloration of the region bordering the collateral sulcus was observed in every part of the cord examined; to this was added in the upper cervical cord a patch of the same

color on the lateral columns, more marked on the left side near the reticular processes.

A similar condition was observed in the centre of each pyramid of the medulla oblongata extending through the vertical fibre of the pons to the pes pedunculi cerebri. All these patches were diffuse, did not sink below the level of the surrounding substance when cut, and appeared to be firm.

Examination of the fresh tissue showed nothing which was not found in the prepared section from the hardened specimen. To the naked eye the gray matter appeared perfectly healthy. In addition to the lesion already described in the medulla oblongata, there was a bloody suffusion of an area corresponding to a diagonal paralellogram drawn from the left pyramid upward and outward towards the restiform column; this was lost in the hardening process by reason of a retraction in the solution of bichromate of potash in which it was preserved.

On microscopical examination, a slight degeneration similar to that found in the posterior columns was found in both lateral columns, even in the upper dorsal portion of the cord, where to the naked eye these funiculi appeared healthy. The areas principally diseased occupied the centre of the column of Goll and the regions bordering the collateral sulcus. They exhibited an exquisite example of the vesicular degeneration of Leyden, regarding which Dr. Spitzka (under whose direction the investigation was made) entertains the opinion (believed to be supported by longitudinal section) that the spaces characteristic of this lesion are tubular, and consist of fluidified coagulable contents, occupying the place of degenerated nerve fibres.

This view is borne out by the numerous transitions which existed between nerve fibres with a hypertrophied myelin and these cavities, many of which latter proved their derivation from the former by containing one or two apparently normal axis cylinders.

That the cavities contained fluid, or at least semi-fluid contents, was proven by their having granule cells in their centre and showing a concentric lamination under good illumination. These cells are presumably white corpuscles which have taken up some of the products of myelin disintegration, and there is

a strict line of division to be drawn between them and many of the swelled granule cells, and granular bodies of various authors, which can be conclusively proven to be fragments of disintegrated nerve tubules, whose granular appearance is due to necrobiotic and retrograde change, whose apparent nucleus is a fragment of the axis cylinder. There was an immense

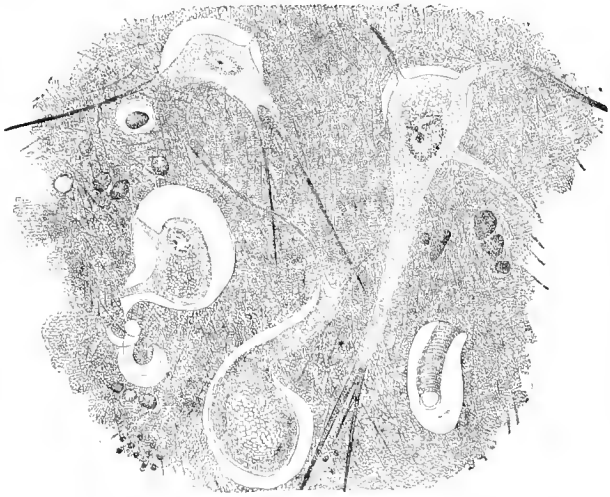

FIGURE 1.

increase in the morbid areas of Frommann's cells, and these were hypertrophied, the connective tissue septa were thickened and the vascular walls sclerotic. The posterior median fissure of the cord presented some dilatations which may be regarded as the result partly of inflammatory adhesions of the opposite walls of this fissure, and partly of an obstructed lymph outflow. The most suggestive changes were connected with the nerve cells of the anterior cornua; they were preternaturally brittle, and contained immense accumulations of a yellow granular material, the precise nature of which it would be rash

Figure 1 represents the changes in the large nerve cells of the anterior cornua; in the upper part is a healthy cell; a second shows the nucleus still existing side by side with the granular deposit, and in the lower part is one whose nucleus seems to have disappeared entirely; the other exhibits two such granular bodies in one cell.

The pericellular spaces are seen to be enlarged (their contour is incorrectly rendered too sharp).

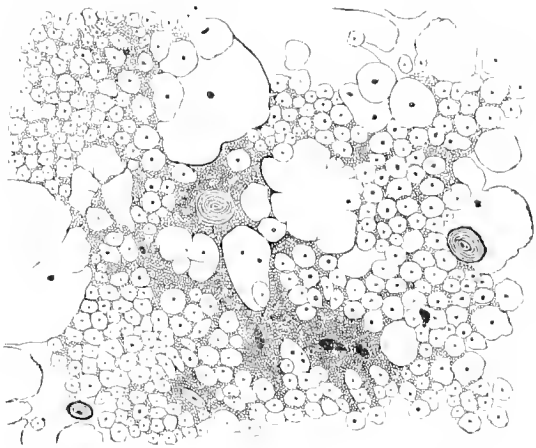

FIGURE 2

to surmise; in some cases no nucleus could be found, and on the whole the bodies were diminished in number as well as size.

One case where necrosis of the lungs, and some arthropathies shortly to be alluded to, existed, was the following:

CASE VII.—W. T., black, U. S., æt. 49, admitted July, 1875. Has been intemperate. Social relations not good. Two years before admission had a fit, after which he was quarrelsome and obstinate, but not violent. Three weeks before admission, had an attack of apoplexy, followed by slight hemiplegia, but without marked impairment of intellect. A month after this he was incoherent, had loss of memory, and showed inability to fix his attention. On admission, his intelligence was considerably impaired, he being incapable of answering simple questions. He had quiet and limited incoherence. His pupils were unequal, his locomotion impaired, and his speech hesitating.

He improved during the first two months after admission, and had a voracious appetite. One morning, at the beginning

Figure 2 shows the changes of the white substance in the centre of the deeper part of the funiculus gracilis. The sclerotic vessels whose lumen, in some instances, almost obliterated, are clearly recognizable. The fine neurilemma is hypertrophied (rendered too granular in the cut), and large nuclei are found scattered here and there in the newly-formed formless connective tissue.

of the third month, it was noticed that he had some difficulty in speech; at about half past nine, the same morning, commenced crying, as he was totally unable to speak, and had lost entirely power over right side; at the same time there were noticed simultaneous momentary muscular contractions on that side. He used the left arm as if he had not lost his intelligence, and had his usual power over left side.

The following day the aphasia continued and he suffered much distress from this; pulse bounding, full and rapid. He was ordered five minims tr. aconite every hour; at about 3 A. M., on the following morning, he commenced yelling out loudly; the movements, although continuing, were not simultaneous, and the pulse was approaching its normal frequency.

Three months following, the aphasia had disappeared, and the patient complained of grating sensation in the knee joint of the right side; this was followed by a uniform painless enlargement of the head of tibia. Similar enlargements were noticeable on the phalango-metatarsal and metacarpal articulation of that side. The patient was relatively coherent, had apparently slight loss of memory and some emotional disturbance. He had another apoplectic attack, followed by very extravagant delusions of wealth and importance.

He was destructive and violent, but soon quieted down. In about six months he had another apoplectic attack, followed by aphasia and left hemiplegia, with the usual fetid expectoration of necrosis of the lungs. From this attack the patient died. His lungs were found, as had been anticipated, necrotic.

The brain showed far advanced periencephalitic change. The cortex was adherent to the pia. The nerve cells atrophic and diminished in number. There was loss of the fusiform cells at base of sulci, and a dirty yellowish discoloration of the white substance was found in the fresh specimen.

The peduncular tracts were not examined. Spinal cord, dura mater thickened irregularly, and fused with the dense lamina of the arachnoid. Over the exit of the fourth dorsal nerve was a transparent atrophic oval spot in this membrane about three millimetres broad. The whole area of the transverse section of the white columns was of a dirty yellowish tint, and the cut surface was irregular. At various regions, prin-

cipally in the dorsal portion of the cord periphery, wedge-shaped sclerotic patches of slight extent, and not very sharp demarcation, could be found; from the altitude of the origin of first to the eighth dorsal nerves, there was a softening in the centre of the left lateral column extending longitudinally, and of only one and a half millimetres in diameter. This softening could not be preserved in the subsequently prepared microscopic specimen, and it is impossible to state what the lesion really was; suffice it to say, that the posterior columns presented a lesser degree of the same change as found in the former case of C, but the same change found in the posterior columns was found also in the lateral and even the anterior, so that the changes were here more diffuse.

The anterior nerve roots few in number. The ganglion cells of the anterior cornua were diminished, and they manifested a peculiar sclerotic condition; in one case took hardly any carmine staining in the centre, which was filled with a yellowish mass, no nucleus visible.

Some abnormal appearances were noticeable in neighboring cells, and those of Clarke's columns, all changes being most marked in lower dorsal portion of cord. The degenerated condition of the cells differed somewhat in appearance from that found in C. In the latter big, lumpy granular masses were found, here they rather resembled the so-called amyloid substance. In this case the interspinal ganglia examined in the fresh state showed pigmentary and granular nerve cells.

Double nuclei were very frequent. Pigmentation in the form of a clump near the nucleus, was also common. The cell-capsules appeared thickened, their nuclei increased. The number of free granules between the nerve bodies were decidedly increased. Anatomico-pathological diagnosis—chronic periencephalo-myelo-meningitis, softening of a part of the left lateral column, atrophy of the nerve cells in the anterior cornua, pigmentary changes of cells in Clarke's column, chronic degenerative change in the interspinal ganglia.

This would contrast somewhat with the diagnosis in the case of C, where it is evident there were not only the same chronic periencephalo-myelo-meningitis, granular and pigmentary degeneration of nerve cells in anterior columns, but

also sclerosis with vesicular degeneration of the columns of Goll, and the centre of the funiculi-cuneati, together with undetermined unilateral changes in the reticular field of the medulla oblongata.

Arthropathies of the nature of those already referred to as observed by Charcot, Ball, and J. K. Mitchell, in locomotor ataxia, have been found in about fifteen cases of general paresis, nine of which were more properly locomotor ataxia and paresis.

One, a woman, had epilepsy preceding menstruation, which ceased at the climacteric, and was followed by paresis.

The remaining five were ordinary cases of paresis.

The acne which occurs in many cases of hebephrenia is one form of skin disease, evidently of this nature, and I may here state, that although I watched carefully, I have never seen acne follow the use of the bromide either in the paretic or the epileptic insane.

One case of herpes zoster appeared on the thigh, following the course of the middle cutaneous nerve, in an epileptic; this regularly disappeared in the period immediately antecedent to an attack, but returned subsequently; it however totally vanished under the use of bromide of potassium, and the fluid extract of conium. One case, a paretic, had preceding a maniacal attack a similar form of herpes following the course of the musculo-spiral nerve. With the reduction of frequency of the maniacal attacks the eruption grew less frequent, and when the patient had sunk into a condition of dementia it disappeared altogether.

A skin eruption which appeared in one case resembled markedly at first, the roseola of scarlet fever; each bright red spot was soon studded with slight vesicles, exuding a colorless fluid, which gradually dried up, and formed a whitish rash in the centre. It appeared under the following circumstances: A patient, 46 years old, had been struck on the head by a policeman, in the year 1846. This had rendered him temporarily unconscious. He recovered, but became more irritable than he had been previous to the injury. In the year 1867, he became markedly depressed, and asylum treatment becoming necessary, was admitted to the City Lunatic Asylum: was discharged, improved, during the same year, but readmitted in

1868; was again discharged, improved, and remained in fair mental health until the year 1874, when he became suddenly violent; tried to cook ice, and his daughter interfering, he attempted to cut her throat.

He was transferred in consequence to the City Asylum for Insane, August, 1874. On admission, was in a condition of typical melancholia with frenzy.

He gradually became less depressed and emotional, was more cheerful, and about two years after physical and mental symptoms of paresis appeared, the former most marked on the left side.

The eruption preceded these by two months. It lasted for eighteen months and then disappeared, as did all the symptoms of paresis in the following two months. The patient improved very much, and appeared almost recovered, when he was transferred to another institution.

And now I am approaching the most difficult part of my subject.

The relation of trophic symptoms and the lesions on which they may be supposed to depend. Difficult as is the referring of motor and sensory disturbances to pathological changes, it is still more difficult to detect those which constitute the basis of trophic anomalies. Not only is the whole subject of trophic and vaso-motor conditions one of the most obscure in physiology, as well as pathology, but we here have another difficulty to deal with; these forms of insanity most frequently marked by intense peripheral nutritive anomalies, present so many other symptoms related to central lesions, that we have to proceed with the greatest care in selecting any one or several lesions as the basis of the trophic disturbance.

Fortunately the experience of those who have made pathological examinations in cases of nervous affections not connected with insanity, here comes to our aid. And as regards the conclusions I have been able to draw from my post-mortem examinations of general paresis, I have been able to select as probably trophic those changes which are analogous to those discovered, in not a few instances, of progressive locomotor ataxia.

That this similarity in the dependence of analogous symptoms on similar lesions, is in perfect accordance with the

general similarity between these two diseases regarding many other points, is obvious.

Our general conclusion is, that the chronic and slowly progressing affections, such as progressive muscular atrophy, muscular lipomatosis, the marbling of the extremities, phlegmonous intractable ulcers, atonic decubitus, and the various changes of the bones and joints, whether in the direction of osteomalacia, premature and excessive ossification, hydrops articuli or thickening of the articular extremities of the long bones, are all referable to pathological conditions of the gray nerve cells and neuroglia of the anterior cornua of the cord.

We are far from being able to distinguish between the presumably different pathological conditions which determine the different character of peripheral anomalies. We can only make this general statement. With regard to the sudden visceral complications occurring without certain local cause, and frequently without local predisposition, such as pulmonary gangrene, pleural ecchymosis, necrotic changes in the kidney, hæmatoma of the lower intestines, I would in accordance with the suggestion of a writer in this journal* attribute them to sudden and rapidly occurring conditions.

The often immense hemorrhages, inflammatory softenings, the intense and extreme periencephalitis, the arachnoid hemorrhages, and inflammation of the dura, occurring in progressive paresis, present us with all these conditions, which in the light of recent researches of Brown-Sequard, and others, stand in relation to distant visceral complications. And the occurrence of arachnoid blood cysts in a certain category of imbecile, epileptic and terminal dementia cases probably exercise a similar influence.

These trophic disturbances form links in the chain connecting insanity with the other branches of neurology, of which insanity is but one of the higher branches.

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* Psychological Pathology of Progressive Paresis, JOURNAL OF NERVOUS AND MENTAL DISEASE, January, 1877.

ART. IV.—THE PARALYSES OF POTT'S DISEASE—BEING A CLINICAL STUDY OF FIFTY-EIGHT CASES.

BY V. P. GIBNEY, A. M., M. D.,

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(*Read before the N. Y. Neurological Society, December* 3, 1877.)

NEARLY a hundred years ago a carious disease of the vertebræ, accompanied occasionally with paraplegia, was so accurately described and so successfully treated by Percival Pott, that the profession, in a pardonable enthusiasm, called the disease after his name, and in *the issue* the cripple placed all his hopes. It was then thought that a specific had been found and that this offspring of scrofula was to be henceforth strangled in its infancy. Yet in all these years the surgeon has had to grapple with the deformity, the physician has had abundant opportunity of studying the paralyses consecutive thereto, and within the last decade the neurologist has enhanced the value of its study by physiological experiment, aided by the researches of pathology. To the labors of such accurate observers as Leyden and Rosenthal, Charcot and Michaud, are we indebted for those minute investigations into the role played by the cord and its envelopes in the pathogeny of this disease.

Before proceeding farther, let me state that my reason for using the term "paralyses" in my title rather than the one more commonly employed, *i. e.*, "paraplegias," is because the latter term is included in the former.

The various neuralgias and pseudo-neuralgias, the arthropathies and all those peripheral lesions dependent on irritation of nerves at their roots or in the foramina of exit have been already too well described for me to make aught more than this bare reference in the analysis of the cases I have had the

opportunity of observing. Michaud, in an inaugural dissertation,* details all the symptoms with which one is accustomed to meet, and enters into such careful explanation of the phenomena presenting, that it would seem an act of presumption in me to attempt any contribution at all to the literature of the subject did not our hospital furnish an unusually large number of cases from which to draw information.

Dr. Julius Althaus, in a work recently from the press,† states in the form of proposition with statistical data appended, that "Diseases of the nervous system occupy the fourth rank among maladies destructive of human life." I never see a case of cervical or dorsal caries, but the question instinctively suggests itself: Will the child become paraplegic? Formerly, I feared exceedingly if the gibbosity chanced to be great; now, this forms no guide in the prognosis. Why does disease of the bony column invade the contents of the canal? The ligaments, the perimeningeal tissues and the dura mater, one would think, were sufficient to protect the cord from encroachment, yet we find that these very envelopes play a large part in the propagation of the inflammatory process. In the dead house we often wonder why paralysis has not followed so extensive a carious process and so great an angular deformity.

The locality of the caries in the cervical or upper dorsal regions, is certainly the great predisposing cause of the paralyses in question. From notes of two hundred and ninety-five cases of Pott's disease I find sixty-two producing paralysis more or less complete. The number wherein the disease was seated above the middle dorsal region was one hundred and eighty-nine, and in this group the sixty-two paralytics are included, only three or four being associated with disease involving the lumbar vertebræ. About one-half, then, of the patients affected with caries in the cervical or upper dorsal regions were at one time paralyzed, while nearly one-fifth of the whole number, irrespective of locality, were thus afflicted.

The vertebræ of the upper portion of the spinal column are small, comparatively, and are closely related to important

* Sur la Meningite et la Myelite dans le Mal Vertebral. Paris: 1871.

† Diseases of the Nervous System. London: 1877.

nerve centres. Passing over other reasons why the motor columns should be the ones chiefly implicated, I would state the fact as pointed out by Olivier,* that the anterior portion of the cord is held close to the posterior face of the vertebræ by the spinal roots, whilst the posterior portion is five or six lines from the corresponding face of the canal. It is also known that the sensory fibres are near the central deep portions of the cord, while the motor impulses are transmitted by the superficial layers.

Sex offers no predisposition. Of the fifty-eight cases on which my analysis is based, thirty were males and twenty-eight females. I, of course, recognize a nervous diathesis, yet I am unable to state anything positively from a statistical point of view concerning such a diathesis occurring in the patients under observation. That a strumous diathesis may stand in a causative relationship, no one will deny; in fact, this is regarded as lying at the base of many of the so-called neuroses. The exciting causes may be exposure to miasmatic influences, a depraved condition of health and direct injuries, as a blow, a sudden jar, or sudden falling together of two vertebræ through loss of intermediate bodies. When this latter produces the paralysis there is no preceding paresis and hence the ease in diagnosticating this as the cause. Pure mechanical compression, of course, acts by arresting the transmission of motor and sensory impulses, and a slow compression may cut off the blood supply. This absence of blood supply is most beautifully shown in a specimen of cord now in the pathological cabinet of the Hospital for the Ruptured and Crippled. The vessels above and below the point of compression could be distinctly seen, while between these points none could be found. The case was presented by me at the Pathological Society, April 21, 1875.† From such, it is easy to see how a reflex paralysis could follow.

Pathology.—A study of the symptoms peculiar to lesions of the different tissues in the neighborhood of the diseased vertebræ, aided by a knowledge of the facts taught by neuro-

* Quoted by H. C. Wood, Am. Clin. Lectures, Vol. I., p. 279.

† Trans. N. Y. Path. Society, Vol. I., p. 56.

physiology, enables one to state with a great degree of precision the pathology in individual cases. In modern text books we have chapters on peripachymeningitis, on pachymeningitis, on meningitis and the inflammations of all such organic structures. The subject of neuralgia and of pseudo-neuralgia is constantly before the profession in many entertaining forms. The theory of the French school of pathologists is well fortified by the facts collected, and when one is satisfied as to the mode of reproduction of the impaired nerve elements, it (the theory) becomes easy of acceptation.

Let me state as clearly as I can the theory. First there is the caseous osteitis of the bodies of the vertebræ, attacking by contiguity the perimeningeal areolar tissue, the vertebral ligaments of course becoming involved in the morbid process—then the dura mater speedily partakes of the pathological changes—a "pachymeningite externe" being the result—this change characterized by vegetations on its external surface and consecutive caseous alterations, which blend with the caseous detritus in the bone and thickening of the dura just named, from a chronic inflammatory process. The cord becomes compressed, a focus of myelitis is, as a consequence, induced, and from this focus a transverse myelitis followed by a fasciculated sclerosis ascending through the posterior columns and descending through the lateral and anterior.

This caseous external pachymeningitis in Pott's disease as a constant factor, readily explains many of the symptoms peculiar to peripheral neuralgias and paralyses. Michaud believes that in all cases of angular deformity *myelitis* is the rule, and states that it may exist without giving immediate rise to paraplegia,* claiming to have proven that myelitis is early in its development and may even precede the paralysis.

From a clinical study of the cases now under consideration I think I can approximatively arrive at the different lesions. *Three* seem to have been due to direct pressure from the bony angle itself; *twelve* gave the history and progress of cases of reflex paralysis due to obstruction of the blood supply; the *remaining number* were, I believe, from a myelitis by slow compression.

* Sur la Meningite et la Myelite. Paris, 1871; pp. 17, 29.

The following case so well illustrates the absence of blood-vessels in the part compressed, and likewise the secondary degeneration, that I shall take this opportunity of reporting it for the second time, the more especially to give prominence to the microscopical examination recently made by my friend Dr. Seguin and not published in connection with the first report—I shall purposely pass over the history, referring such of my readers as are interested to the transactions of the New York Pathological Society, Vol. I., p. 56:

CASE I. *Caries of dorsal vertebræ; paraplegia; death from myelitis; cord compressed; ascending and descending degeneration.*

Male, æt. 6, caries eighteen months' standing, marked gibbosity, paraplegia four months. * * * * * *

"Lying in the spinal concavity was found a sac, measuring two inches vertically and three and a half laterally. The contents—cheesy, curdy mass. The walls were formed by the anterior common ligaments, thickened connective tissue, and pleura. On making a vertical section of the spinal column from before backward, the body of the eighth dorsal was found entirely gone, the seventh nearly so, and the sixth and ninth partially. Pultaceous matter lay in place of the eighth dorsal and pressed on the spinal cord, which at this point was found anæmic, yellowish and smaller in size. The absence of blood-vessels here was notable. A portion of the gastrocnemius muscle under the microscope showed granular and fatty globules, the muscular striæ were effaced, and occasionally there was a homogeneous mass."

Of the microscopical examination of the cord eighteen months later, after being well preserved in Mueller's fluid, Dr. Seguin thus writes:

"The sections were cut from four points, A—above the seat of pressure; 1. On the upper part of the specimen, *i. e.*, the middle of the cervical enlargement; 2. In the upper dorsal region at a point twenty millimetres above the limit of compression. B. Below the seat of pressure; 3. At a point twenty millimetres below the lower limit of compression, *i. e.*, in the lower dorsal region; 4. In the lower part of the middle of the lumbar enlargement. No sections were cut from the

disorganized and atrophied portion of the cord, because I thought it well not to injure the specimen, and because I doubted if any thing instructive was to be found there.

"The above sections were examined in two ways: 1. In saturated solution of acetate of potassa, without staining. This was to show the granular bodies. 2. By Clarke's method, to show the atrophy of nerve tube in parts, and the sclerosis of the neuralgia. Sections No. 1, treated by acetate of potassa, showed very exquisitely the lesion of ascending degeneration. The columns of Goll, or posterior median columns, were filled with granular bodies. Sections No. 2, seemed quite extensively altered, granular bodies being found in almost all parts of a section—probably from pressure effects causing ischæmia of the parts. Besides the above, granular bodies were found in small number in the external part of the postero-lateral columns—the ascending cerebellar fasciculi of Flechsig.

"Below the seat of pressure, sections 3 and 4 showed the usual descending degenerative changes in the white columns, the mass of granular bodies occupying the outer and posterior part of the antero-lateral columns. The sections prepared by staining with carmine and by Clarke's method afterward, showed the same ascending and descending degeneration as evidenced, not by granular bodies, but by atrophy of nerve fibres and increase of the neuroglia. No lesion existed in the gray matter and the cells of the anterior horns seemed normal. In other words, this examination shows that except at the seat of pressure, there were no lesions other than those of ascending and descending degeneration."

To Dr. Seguin I have also given two specimens of cord taken from patients with marked angular spinal prominence, but who never had any paraplegia. The Doctor made a careful microscopical examination, and was unable to find even a trace of myelitis opposite the point of osseous lesion or in sections made above and below. One of the patients died from post-œsophageal abscess, and was reported at the Pathological Society by my fellow *interne* Dr. J. W. Crenshaw;*

* Transactions, Vol. I., p. 232.

the other died of catarrhal pneumonia, and the notes of the case with post-mortem examination I presented to the same society March 22, 1876.*

As illustrative of the influence a collection of pus may have on a spinal cord when lying in the immediate vicinity, I cite the following case, which was presented to the Pathological Society April 8, 1874, by a former *interne* of the hospital, Dr. Chas. Milne, but which has not as yet been published, it being the paper of a candidate for admission.

CASE II. *Dorsal caries with great deformity; residual abscess in left cervical region opening into a bronchus; paraplegia with recovery; death by suppurative pleuro-pneumonia; autopsy.*

Male, æt. 6, admitted to the hospital May 17, 1872, affected with caries of the vertebræ, the angular prominence extending from the seventh cervical to the eighth dorsal, two and one-half years' standing; incomplete paraplegia one year, and on admission, boy totally unable to stand alone, though when assisted could take a few steps; no atrophy or flaccidity, but spasm and increase of reflex excitability, with a certain degree of hyperæsthesia; a well marked tumor in left anterior cervical triangular space, feeling like a mass of infiltrated glands and imparting the pulsation of the underlying artery. The exact date of the first appearance of this tumor with reference to the beginning of the paraplegia was not ascertained; at any rate the tumor increased in size after his admission, and in proportion to this increase was the improvement in the paraplegia. The notes read, "some improvement," "abscess enlarging." January 14, 1873, the abscess opened into the left lung, as proven by the symptoms and signs (the minutiæ of which I do not record here for lack of space), and the microscopical examination of the sputa. Despite this pulmonary complication his power of locomotion continued to return, and by March 3, 1874, he had recovered perfect use of his lower extremities. The pus, however, was not completely expectorated, and on the date just named he was removed, dying six days later at home. The autopsy was made thirty-six

**Med. Record*, April 29, 1876, p. 290.

hours after death, and I was assisted by Dr. A. C. Graham, now of Dallas, Texas. We had great difficulty in getting permission to examine even the thorax and spinal column, and even this work the family supervised. The result was, as a consequence, extremely unsatisfactory, yet there was found extensive suppuration of the left lung, a large quantity of pus in the pleural cavity, an old inflammation of the dura and perimeningeal tissue, a narrowing of the spinal cord opposite the remains of the bodies of the seventh and eighth dorsal vertebræ. The specimens were preserved in alcohol, and a microscopical examination was not made.

When we have the extensive cord changes, the destruction of nerve tubes, etc., how is a cure brought about? It must be remembered that the sclerosis involves the neuroglia, and that the secondary degeneration takes the place of the white fasciculi constricting the nerve tubes. Even should the envelope of myeline be destroyed, impulses can be transmitted through the axis cylinder, and should this be destroyed, the degeneration existing in tracts or bundles, other nerve tubes which remain intact may serve for the transmission of impulses. A case at present in the hospital has an incomplete cervical paraplegia with paræsthesia and a complete paraplegia. I can find no pupillary or retinal disturbance of any kind: here I think the paralysis of the superior extremities is peripheral, that of the inferior, central. A patient whose case I have already published,* got a complete paraplegia by slow compression, recovered power within a twelve-month, and while convalescing a true spinal paralysis attacked the right inferior extremity, atrophy soon followed, and by the electrical test the diagnosis was fully confirmed. The pathological process seems to have been about as follows: a transverse myelitis existed above the dorso-lumbar enlargement, this resolved in due time, but the descending sclerosis continued its march, until the enlargement just named had been reached; a focus of myelitis was here induced and the cells of the right anterior horn underwent the usual changes peculiar to this form of paralysis. The child is still under treatment, and the condition of the limb presents few features of encouragement.

Philadelphia Medical Times, Dec. 9, 1876.

The symptoms.—In my analysis I find the average duration of the caries before any signs of paralysis were developed to be three years, the shortest period four and one-half months, the longest eleven years. In thirteen the paralysis never became complete, and the duration of this paresis ranged between four weeks and twenty-two months. Twenty-four presented complete paralysis, and in this number are included four which had likewise incomplete cervical paraplegia. In no instance have I met with a cervical paraplegia uncomplicated. All, with the exception of three or four, were first paretic, *i. e.*, the paralysis was preceded by a paresis. I very much regret the incompleteness of our records respecting the condition of the limbs throughout the entire course of the paralysis, yet some few in different stages I have recently examined, and from these I find the rule to hold good—a certain degree of flaccidity at first with little or no spasms; later the rigidity of the extensors easily excited into an epileptiform condition, the exaggeration of the reflex irritability, changes in the integument, paræsthesia, etc., etc.

In more than one instance I found the reflex power excited by pressure over the inner side of thigh, lower third. Instead of getting the "cremaster reflex" of Jastrowitz,* I produced sudden flexion of the thigh on pelvis and leg on thigh followed within a few moments by the same movements on the opposite side. The sinew reflex of Erb and Westphal, known as the "knee phenomenon," was not found in one or two of the cases wherein the lumbar enlargement was the seat of lesion, thus indicating, at least, that the crural nerve was lacking in its integrity. The centre, however, for reflex motions being in the lumbar cord, disease here must either diminish or completely suppress any of the reflexes.†

The limbs may remain free from muscular spasm indefinitely. Tremors on the least passive motion are often excited, and in one case this was so great at the first examination as to lead to the diagnosis of paralysis agitans. While these peculiar movements resemble the spinal epilepsy of Brown-Sequard,

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* *Berlin. klin. Wochenschrift*, No. 31, 1875.

† JOURNAL OF NERVOUS AND MENTAL DISEASE, January, 1877, p. 189.

Rosenthal* prefers to designate them as spinal reflex spasms, rather than use a term Brown-Sequard intended for a distinct pathological lesion. Without having dwelt on the symptoms which have been so accurately described by writers from the days of Pott to the present time, I have simply referred to a few not generally specified.

Diagnosis.—It would seem that an observer with ordinary acumen could not err in diagnosticating this form of paralysis from other kinds. I mean, of course, to include under "this form" the paralysis due to compression of the cord, be the cause what it may. Tumors of the various types, cancer of the vertebræ, hemorrhages into the canal—all give rise to a group of symptoms which simulate very closely those of paralyses from Pott's disease; yet by a careful examination, a good history and a knowledge of the physiology of the spinal cord, a differential diagnosis can be made in those cases of unusual obscurity.

An observation I have to make, and in this I do not claim any priority, is that deformity of the inferior extremities from contraction of certain muscles, generally the thigh flexor group, causing the patient to stoop forward resting hands on knees for support, does not constitute a paralysis. More than once I have had this gait described to me as that of paralysis, or paresis. True, there is difficulty in walking, yet no loss of motion—simply a peripheral nerve irritation causing neuralgias and contractions. When the paresis follows by many years the apparent cure of the spinal disease, the paralysis which ultimately ensues may be confounded with other forms. This summer there appeared in the *Lancet* (June 23, 1877), "A Case of a New Form of Pseudo-paraplegia Ending in True Paralysis." This title was followed by "Caries of Dorsal Vertebræ; Death; Autopsy." The report is by Dr. William Alexander and Mr. James Barr. The history of the injury, the increase in the deformity, the pseudo-neuralgias, the periarticular swellings, the paresis at times greater and at times less, the escape at first of the sensory nerves, the later rigidity, the typical extension of the leg and foot in tonic spasm when the

*"Cervical Paraplegia;" JOUR. NERV. AND MENT. DIS., Jan., 1877, p. 96.

skin was irritated, the sudden and sharp flexions, the diffusion of the electrical current, later still the paræsthesia, the fulgurating pains, bed sores and death, then the autopsy, at which was found loss of half the bodies of the eleventh and twelfth dorsal vertebræ, a caseous sac, the cord compressed against the posterior part of the spinal canal by inspissated pus above and below by a piece of bone, the ramolissement with destruction of nerve tubes,—all this points so clearly and unmistakably to a paraplegia from the vertebral caries, that I wonder how such a case could have been reported as one of a "*new form* of pseudo-paraplegia."

THE TREATMENT.

When one has made up his mind fully as to the pathology of an individual case the treatment theoretically resolves itself into

1. The arrest of the mechanical pressure if this be suspected. This object cannot be better accomplished than by the application of a suitable spinal support fitted accurately, and having for its cardinal principle the arrest of all motion at the seat of disease. The apparatus with which all of our cases have been treated is a simple steel frame consisting of two horizontal bars, the one extending from axilla to axilla posteriorly, the other parallel and grasping the ilio-costal spaces just above the iliac crests, while these bars are connected by four vertical ones—two from axilla to crest and one on either side of spinal column about two and a half or three inches apart. This is covered with shaved sheep skin and lined with muslin, of which like material fronts are made to lace along median line anteriorly. The brace is lengthened to suit the locality of the caries, and if the cervical or superior dorsal vertebræ be the seat of disease a steel bar is attached to the body support under consideration, passing from the lower horizontal bar at its centre vertically to the upper one, curving to clear the occiput, and terminating in a small bar to which chin and occipital straps are attached. This supports the weight of the head, relieving the column of the pressure. For a further description with diagram I cannot do better than refer to "Orthopœdia," by James Knight, M. D., pp.

323, 324. For twenty years the Doctor has used this apparatus, and I can bear testimony to its utility when not abused. A case or two in point for illustration:

CASE III. *Cervico-dorsal caries with paraplegia almost complete, of six weeks' standing; recovery in ten days.*

Female, æt. 11, admitted June 20, 1870. The spinal caries was of six months' duration, *i. e.*, the deformity had been observed during that period, though the entire length of time could not be ascertained. Already a marked angular prominence was present. For six weeks she had been unable to walk unless strongly supported, and at date of admission, if properly balanced, she could stand alone for a moment; on further examination there was found pretty complete motor paralysis of all the muscles of both lower extremities, while sensation was about normal. A body support with the head spring was applied; within a week and by *July* 1—ten days after her entrance to the hospital—she was *walking alone.* By Oct. 16th she could walk and run with entire freedom. She remained under treatment until the 12th of June, 1871, two months prior to which time the head spring was removed, and was discharged, no sign of relapse occurring.

CASE IV. *Gibbosity from 6th cervical to 3d dorsal; complete paraplegia, of six months' standing; walking one month and three days after admission; recovery of paraplegia complete; subsequent death from tuberculosis; no autopsy.*

Female, æt. 6, anæmic and coming from a family with an excellent tuberculous history, admitted to the hospital June 12, 1872. The caries reported to have followed a slight injury one year previously. The treatment was begun about six months later, cupping being the agent. Loss of power was observed three weeks after its employment and soon became complete. Applied to the out-door department of the hospital for relief a month or six weeks prior to admission; brace and head spring applied, but the mother persisted in leaving it off, and future treatment was made conditional on placing child in hospital. This was done, and the examination on day of entrance discovered total inability to stand or to execute any voluntary movements of the inferior extremities, flaccidity of muscles, and if any excessive reflex irritability, spasm or par-

alysis of sensation existed, this was not recorded in the note book. The head spring was readjusted, a better fit being secured, and constitutional medication was included in the treatment. *July 15th*—one month and three days from the date of admission—the child with a little assistance walked across the floor. *Oct. 12th.* Walking alone and with ease. *May 1st*, 1873. Head support removed, and on *May 29th*, brace removed. *May 30th.* Discharged cured. *Dec. 4th*, 1874. Readmitted after a lapse of eighteen months, having experienced no relapse in that period. Now the mother fancied a returning loss of power, but on examination the limbs were found unaffected, but the patient was suffering from general tuberculosis. Remained a few days, rapidly sinking all the while, and was removed moribund by the mother, who feared a post-mortem. Death occurred between the hospital and the home, and no autopsy was permitted.

Suspension with the view of producing extension and the use of the plaster jacket seem as popular now as were the issues three-quarters of a century ago. I know that immediate cures are currently reported, yet I have searched with some degree of diligence for cases sufficiently well recorded to be of scientific value, and my search has been discouragingly fruitless. Still I can readily imagine spontaneous relief when the angular deformity is the direct cause of the paralysis. Instances of dislocation are abundant wherein reduction was followed by brilliant results. If the pathology lay in a cutting off of the superficial circulation at the point compressed, then I can see how extension would prove serviceable. When, however, a meningeo-myelitis is in active progress, I can see some objection to extending the parts, to rupturing bands of adhesion, to the separation of approximating sections of vertebral bodies. In such cases nature's efforts at repair must be respected, and I know of no better mode of meeting the indication than by accepting the situation and fitting an apparatus simply as a support. The following may serve to elucidate the point:

CASE V. *Sudden paraplegia as result of removal of support; recovery in five months.*

Male, æt. 9, out patient, had apparatus applied, Dec. 15, 1873, for spinal caries, dorsal region with marked angular

deformity; wore this without any symptoms of interest occurring until Jan. 15, 1875. He became tired of apparatus, and the mother without advice removed it; he felt next day a "giving away" sensation in the hump, and almost immediately lost power in the inferior extremities. The angle was found much sharper, and the paralysis was easily explained. The boy was kept in recumbent posture for a few days until a well fitting brace could be applied, after which—a month at least—power began to return, he could stand and with aid walk a few steps.

May 12, 1875.—He walks unassisted, and up to the present time has had no relapse. The case just recorded has this additional point of interest, viz.: the length of time that support is required; it furnishes a powerful text against the practice of removal of apparatus and pronouncing a cure within short spaces of time.

2. The use of remedies either internally or externally known to be effective in controlling the inflammatory process.

If the inflammation be acute, ergot stands foremost in controlling lesions of the cord or its envelopes. This drug has been used in many of the cases treated at the hospital, and in recent ones wherein the inflammation was presumably acute, relief has followed promptly. Yet in those same patients on the occurrence of relapse the same medicine has most signally failed. Remembering, as we should, that the disease is either subacute or chronic in a large proportion of the cases, the inutility of ergot becomes manifest.

Belladonna has been used in several instances with seeming success, yet our experience permits us to say nothing laudatory of this as a curative agent. In the earlier stages, though prior to the paresis, belladonna has proved of undoubted service in the relief of those neuralgias so distressing to the patient.

For chronic myelitis or meningitis, nitrate of silver and the iodide of potassium have seemed to produce the best results.

The actual cautery was used on two patients by Dr. Eugene Dupuy in the out-door department. One little girl, aged 9 years, with dorsal caries five years' standing, and complete paraplegia of fourteen months' standing, was touched with the white hot iron twice a week for a month, and no relief whatever was

experienced. At the present time, six months later, she remains paraplegic with all the usual phenomena.*

Another aged 9 years, cervico-dorsal caries seven years, paraplegia five years, though incomplete a portion of this time, was treated with a single application of three or four points to either side of the spinous processes. No immediate or remote relief has followed. We were unable to get the child to submit to a second operation. I am not unmindful of the merits claimed for this by those two competent observers, Prof. Charcot and Dr. Michaud. In the *British Medical Journal*, Dec. 26th, 1874, I find an abstract of a lecture by the former, and it is reported that a Polish girl, aged 15, was sent from Warsaw to Charcot in order that she might have the cautery used for a paraplegia. She passed through Berlin, and consulted Langenbeck, who sought to dissuade her from the object of her journey. She reached Paris however, underwent two operations at the hands of Charcot, and walked within a fortnight. Soon afterwards she visited Langenbeck on her return homeward, and the great surgeon was convicted of his error. Now this case is very imperfectly recorded, and, of course, may have no scientific value whatever. I have studied the cases in the original memoirs, however, and must confess to a great degree of doubt as to the curative effects of the cautery. Michaud cites a case of "dorsal Pott's, paraplegia with contracture, cure by repeated applications of the hot iron." The patient was first treated by M. Guerin shortly after the invasion of the paraplegia; in three months, worse, entering Hotel Dieu with complete paraplegia; then there was spontaneous recovery which did not long continue. Finally, after sojourning a while at la Pitie she entered la Salpetriere, Jan. 4, 1864,

*This case was complicated by an immense abscess, painless, over right scapula, which was not opened. After her removal from the hospital (Oct. 16th, 1877), she fell into the hands of Dr. W. G. Russell, of Brooklyn, who thought of applying the plaster of paris jacket, but was obliged to postpone it on account of the abscess. This finally opened, and on Feb. 2d I heard, through Dr. Russell, of the return of power in her limbs. On the 14th, she was brought to me completely free from all paralysis. In Dr. R.'s note, he expressed the opinion that had the jacket been applied he would certainly have published the case as a remarkable recovery under that treatment.—V. P. G.

with complete loss of power in inferior extremities. *January* 10, first application. Feb. 20, a second. Beginning of April, a third. "After the first cauterization the pains in the loins, which the patient had experienced, were much less; after the second the contracture diminished a little and the limbs commenced to elongate. Finally, *three months* [italics mine] after the third cauterization the patient began to raise herself by the aid of crutches."* The recovery from this date was gradual but incomplete.

A second instance cited: "disease of dorsal spine, recurring symptoms, applications of the points of iron, cure of the paraplegia." Patient, female, æt. 27, entered Salpetriere in Charcot's service, Jan. 27, '69. Disease, paraplegia eighteen months' standing; first application Feb. 5th, four or five points; March 20th, improvement said to be marked—the paræsthesiæ shown in arms on admission have disappeared, patient can easily sit up; not able to walk. July 17th, two more applications. Middle of August, patient began to walk between the beds, and six weeks later was walking without support. Then typhoid fever supervened. Now this case is reported as cured with the hot iron.

Another, female, æt. 50, entered hospital in service of Charcot, May, 1862; paraplegia with flaccidity, two years standing. May 20th, ordered nitrate of silver, which was not followed by any decided results. On June 8th, 1863, partial recovery having taken place, an application of hot iron was made. Two months later, patient improving all the while, able to walk with ease.

One other instance: "Lumbar Pott's, paraplegia with flaccidity, spinal arthritis, cure by points of iron."

Female, aged 21, entered Salpetriere, June 21, 1871; neuralgia began four years previously, and two years later paresis, paraplegia becoming complete in the month of May, 1869, two years prior to admission to hospital. Even at this time (May, 1869), at another hospital, she was treated with the iron heated to white heat, with very little success. At date of admission to Salpetriere the paraplegia said to be not complete. First cauterization shortly after admission; at the end

* Michaud, loc. cit., p. 71.

of three days was able to walk a little with support; then followed œdema, without albuminuria, interfering with progress of case. Second cauterization Sept. 1st, 1871, and after this (length of time not mentioned) walked a little with aid. Nov. 2, 1871, considered about cured. Such is the testimony I have been enabled to gather from those who speak most sanguinely in favor of the cautery.

3. The protection of the paralyzed members from irritants, and the restoration of function by local applications. I pass purposely the use of warm clothing and such things as the good sense of the mother or the nurse would suggest, to that much vaunted remedy for all forms of paralysis—*electricity.* Galvanism and Faradism are advised by nearly all writers on this subject, especially during convalescence. These currents have become so popular as diagnostic aids, that the notes of no case are considered complete without a record of the action of the muscles and nerves to the current. For this reason some of our patients have been subjected to this test, and others while convalescing have been electrized simply to expedite the cure. That much ill may follow the use of this powerful agent, I have come to believe, after several years of what I may justly call experience. I have often, in mentally reviewing cases thus treated, come to the conclusion that I have done harm; still I propose not, in this connection, to do aught but state facts.

CASE VI. *Dorsal caries with gibbosity; paraplegia complete; partial recovery in two months; treatment during convalescence by Faradism; relapse and death.*

Female, æt. 9, strumous disease of upper dorsal vertebræ, three years' standing; admitted July 18, 1870. There was found complete loss of power in both inferior extremities, with easily excited spasm of flexors of the leg. There was total inability to stand, or to execute any voluntary movements in any of the muscles with the exception of the flexors of the great toe. Sensation was perfect and reflex irritability exaggerated. A spinal brace with head-spring attached was soon applied—within a week—and on *June 31st*, thirteen days after admission, the child could stand by a chair. *Sept. 3d.* Since the last observation, the patient has experienced an exhausting diarrhœa, and the note to-day is: "child seems well,

but condition of limbs about same as on entry." *Sept.* 11*th.* "Begins to walk a little with the aid of a chair." *Oct.* 18*th.* The improvement has continued and voluntary movements of the feet can now be executed with ease. *Dec.* 13*th.* "Pretty good movements throughout entire lower extremities." 1871, *Jan.* 30*th.* Within past fortnight an exanthem, very like to that of measles, has appeared, and it is recorded that the child is quite feeble, though all spasmodic or excessive reflex actions have disappeared. *Feb.* 10*th.* Tendones Achillis slow in yielding and apparatus for feet ordered. *April* 12*th.* Stands alone and walks freely with a chair. By the latter part of August permanent improvement seemed to have taken place, and to complete the cure Faradism to the muscles was begun and continued tri-weekly for a month or six weeks. From this time it is reported on *Nov.* 23, that the "girl's walk became gradually worse and worse, and about *Nov.* 1*st* a small slough appeared upon the right hip. This seemed to be the result of constant sitting and defective innervation. The patient is confined to the bed, * * * is cheerful, has a good appetite, and the sensation of the lower extremities is pretty well retained." *Dec.* 28*th.* "Removed by the parents. For several days an aggravating ulcerative stomatitis. There is also a gangrenous slough over each nates; unable to walk, or even stand with assistance." It was reported at the hospital that the patient died at home a short time subsequent to removal. No autopsy.

In the case of "new form of pseudo-paraplegia," reported in the *Lancet*, and to which I have already referred in the illustration of another branch of my subject, the electrical treatment was employed, both currents, and it is significant to note the patient's steady decline after its first employment.

Rosenthal reports a case, which is quoted by Michaud: female, presenting a gibbosity of dorsal vertebræ sixth to the ninth; the motor paralysis was not complete, the patient could take a few steps, but with great difficulty. *The electrical contractility was normal, as to muscular and cutaneous electro-sensibility it was wanting.* It is related, that three months later the paraplegia was complete and the patient succumbed.*

* Michaud, loc. cit., p. 43.

The particulars are not given, and the electrical examination, presumably thorough in the hands of so zealous a scientist as Rosenthal, may have had nothing to do with the result. The case whose notes I shall now record was examined electrically.

CASE VII. *Dorsal caries, marked angular prominence; paraplegia three years, then signs of recovery; electrical examination followed by relapse; interstitial nephritis and death; no autopsy.*

Male, æt. 3½, admitted April 24, 1873, in a paraplegic condition of some eight months' standing, while the signs of Pott's disease were of about sixteen months' standing. Very little recorded of his family history. It was noted on admission that he had no power to stand even when assisted; that the limbs were not atrophied, and that they were quite rigidly extended on the slightest provocation. Apparatus was applied, including support for the head, and it was not until January, 1875, that any signs of improvement could be observed. At that time he began to stand with a little assistance and even to take a step or two. The clothing, however, emitted a strong urinous odor, signifying paresis of the vesical sphincter. Faithful efforts to induce him to stand and walk were attended with only partial success, and by *Jan.* 19, 1876, there was little change worthy of record. The spasmodic actions were still prominent and tremors were easily induced. The right lower extremity was certainly more under the control of the will than at date of last observation.

Nov. 19th. Condition much about the same; he takes a few steps every day by holding to a chair which is drawn slowly along. To-day, an electrical examination is made. To galvanism, no response through the muscles, but response good through the nerves. Faradic contractility in muscles normal. There is no flaccidity, but rigidity of adductors of thigh and extensors of foot. The odor of urine from clothing very strong.

Dec. 1. Nurse reports a slight febrile movement every evening, and temperature this P. M. 101°.

Dec. 8. The boy has not attempted to stand or to walk since the electrical examination. The paralysis seems complete. *March* 7, 1877. No signs of improvement since last observation; slight œdema of feet, urine incontinent. *March* 14.

Œdema of feet continues, skin glossy; urine straw colored, of a specific gravity 1,008, albuminous fifty per cent., renal epithelium and phosphates in abundance, but no casts found. Since he has had no congestion abscess it is fair to presume that the nephritis depends either directly on a cord lesion, or on a cystitis, the inflammation extending through the ureter. *July* 1. For past week or ten days has been failing rapidly, and this afternoon died, apparently of œdema of the lungs. No autopsy.

CASE VIII. *Dorsal caries with incomplete paraplegia; improvement interrupted by electrical examination.*

Female, æt. 7 years—admitted Sept. 5, 1876, with an unreliable history. The weakness in inferior extremities was reported to have existed about eleven months. An examination was made on the day of admission and results: Unable to stand without assistance, though when well supported stands and takes a few steps shuffling one foot around the other; spinous process of vertebræ included between last cervical and eighth dorsal project in a sharp curve, the summit of which is an inch from the vertical bearing; no tenderness on pressure or percussion at any point; in dorsal decubitus can flex the right thigh to an angle of about 150°, while the left is flexed to over a larger arc, though neither can be held in this position any length of time; with legs flexed can flex both thighs to a right angle; power to extend thighs about normal, while that to extend legs about half what the normal should be; the "foot phenomenon" well marked on right side, less marked on left; *i.e.*, tremor is induced by passive dorso-flexion of the foot.

December 1. Shortly after admission apparatus was applied, and by the middle of October the child was standing and walking with the aid of a chair. Two weeks ago the case was examined electrically; faradic contractility was found normal, while the galvanic was diminished. During the past week the patient has grown decidedly worse; she is now unable to stand or walk at all. She became very helpless, had incontinence of urine and fæces, and various remedies were used with tardy results; this condition, in fact, continued until the summer of present year, when she began to regain very slowly

the power lost, and by *December* 3, 1877, she has so far recovered that walking by the aid of a chair is comparatively easy. The child has walked recently a short distance alone. A perfect cure is confidently expected.*

I am prepared to admit an abuse of the current, for instance, the employment of the ascending galvanic current when the descending should be employed, the faradic when the galvanic alone is indicated, and *vice versa;* I am not sure that I used the currents with that discrimination in the earlier cases as I have in the later ones; still, granting all this, I am unwilling, as yet, to admit in the present unsettled state of our knowledge of the various phenomena of electricity, that a descending current will always descend through just the nerves and muscles we desire, and will never ascend unless we so place our poles with that object in view; I am not prepared to admit the possibility of localizing a current to a nerve or a muscle in a member in which the reflex irritability is as great as it is in members paralyzed from Pott's disease. If straining at stool, a tickling of the foot, the mere contact of the finger to parts of the limb, a puff of wind even, will call into play the most violent tonic spasm through the ready conveyance of such impressions through sensory nerves to those reflex centres, I fail to see how so powerful an agent, even in its milder forms, can be used with impunity. Too much stimulation of the nerve centres can induce congestion, and a congestion long continued must inevitably result in a myelitis.

Dr. F. D. Lincoln, in a recent number of the Boston *Medical and Surgical Journal*,† after reporting several cases in which injury followed the use of electrical treatment, makes the following pertinent remarks:

"Another effect of too concentrated galvanic currents is the production of superficial eschars rather slow in healing. It is well to say to those who have had little experience that these may be produced in a few minutes without any warning pain, in certain patients whose cutaneous perceptions are weakened. * * * * * Benedikt, who treats this subject

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* This case has recovered since the reading of this paper.

† October 25, 1877, p. 469, et seq.

with a remarkably free hand, uses the following words: "The electric current is *contraindicated* in cases, where in spite of all precautions, it is not borne well: for instance, in a few cases of tabes and hysteria, where it provokes violent symptoms of irritation; in cerebral affections when symptoms of congestion appear. * * * * * * *An increase of the bad symptoms ought on no account to occur during the electrical treatment.* If it does occur, the intensity,* etc., must be modified. * * * * * A general rule respecting the intensity of the current is, that *painful currents* are *not only unnecessary but*, as a rule, *harmful.* * * * * * General convulsions, spasmodic tension of the muscles, giddiness, pain, paralysis, cerebral hemorrhage, etc., etc., are frequent consequences of too painful currents."

I could adduce cases from the current medical literature and add still further proof in support of the position I have taken, but time and space forbid. Now, if all the instances I have brought forward be mere coincidences, and if the agent were used at an unfortunate period, I can still claim that a certain jeopardy is incurred, and I claim that it is not only useless but unscientific to employ the current in cases where convalescence seems well established. Such practice I say is not only attended with a certain degree of danger, but brings a valuable remedy into undeserved disrepute.

PROGNOSIS.

Of the fifty-eight cases thirteen have died, six dying from myelitis, five from other diseases subsequent to the establishment of a cure, and two from tuberculosis before the complete restoration of power to the limbs. The mortality, then, is about $10\frac{1}{2}$ per cent. Twenty-nine recovered from the paraplegia; eight of these relapsed and all recovered again save one, who is still under treatment, along with the remaining fourteen still unrelieved.

The idea seems quite prevalent that paralyses from Pott's disease are incurable. I could report several cases had I time

* Electrotherapie, 1874, p. 132.

to show how hopeful even the most hopelessly appearing were. Some have been paralyzed four, five and ten years, and then have made a recovery; some have had grave eye lesions—pupillary disturbance, optic nerve atrophy, and have made a fair recovery, one had marked cystitis and a single irrigation afforded permanent relief—I have notes before me now of grave myelitis wherein perfect recovery is recorded.

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ART. V.—A CONTRIBUTION TO THE PATHOLOGICAL ANATOMY OF DISSEMINATED CEREBRO-SPINAL SCLEROSIS.

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(*A paper before New York Neurological Society, Feb.*, 1878.)

MR. PRESIDENT AND GENTLEMEN: This thesis is based upon two cases. Case I. came under the observa- of Dr. Van Derveer, of Albany; the history is as follows:

Thos. Grogan, accountant, æt. 29, unmarried. Mother died at the age of 49, during her climacteric period, from the exhaustion following sciatica. One brother died of phthisis pulmonalis at the age of 26. Several brothers and sisters died during infancy. The father, a brother and a sister are still living, and in good health. Saw the case first in Feb., 1870, when the following facts were elicited:

Habits have always been good; no sexual excess or masturbation, and has not been exposed to venereal diseases. Has been quite studious and had acquired a good education. Was fond of society, and spent many of his evenings at dances up to the time he was taken sick.

In the latter part of December, 1867, his attention was first attracted to a sharp pain about an inch back of the right eye. He was impressed with the idea that he had strained the eye and began wearing a pair of spectacles. While attempting to hang a picture, he noticed that he could not see it distinctly enough to determine whether it was hung properly or not. In April, 1868, had frequent nocturnal emissions, but did not apply for treatment. While attending a military funeral in June, of this year, with a prominent organization of this city, he experienced no trouble in his power of locomotion until he had walked about five blocks, when he began to stagger like a drunken man, and after several ineffectual attempts to march steadily, was obliged to leave the ranks. He immediately returned home, and consulted his family physician, who thought that he was probably suffering from a partial sun-stroke. No particular treatment was ordered aside from rest, and partially recovering his strength he returned again to his office duties. In August he was much annoyed by a feeling of internal warmth in the lower part of the left leg, but adopted no treatment. Went to Pennsylvania in October to secure rest and recruit his health. Any unusual excitement at this time would cause severe nervous tremors and a sensation "akin to creeping of the flesh." During his visit he suffered much pain in the left ankle and thought he had sprained it, but could not tell when or how he had done so.

He returned to his home about Nov. 1st, and applied himself closely to business until the middle of February, 1869, when his strength failing he gave up business by the advice of his physician. Was confined to the house during three weeks, regarding his weakness as due to overwork. During the winter of 1868-'69, Dr. Alden March applied the moxa several times to his spine. At the close of his three weeks' confinement he was again able to take exercise in the open air, and walked with the aid of a cane. In the course of four or five weeks he was again much prostrated, so much so that he himself entertained serious apprehensions concerning his recovery. He was now troubled with more or less flatulence, dizziness, loss of sight and constipation. Improved somewhat in May, and continued so until the middle of August. Went to Sharon

Springs and took sulphur baths up to 100° Fahr. daily during four weeks, and drank at the same time from one to two quarts of sulphur water each day. This produced great prostration, and he was brought home in four weeks greatly debilitated, and much reduced in strength and flesh, and now for the first time in the history of his disease was he unable to walk without support. He first came under my observation in February, 1870, when he presented the following more prominent symptoms: Strength somewhat improved since his return from the Springs, and he can now, with great effort, walk alone, but gravitates in an irregular way from side to side while doing so. Complains of great weakness in his arms and legs, and a feeling of great constriction about the body. Cannot see to read, but can distinguish a friend at a distance across the street. His feet are very sensitive to tickling. Standing with his feet together, when his eyes are closed, he thinks he would fall if left alone. Will not trust himself to come down stairs without watching his feet closely, and feels a sensation as if walking upon cushions. Drags his feet when walking. Can converse with little or no effort. Enjoys conversation, is humorous, and fond of quoting from Shakespeare. Is unable to whistle on account of a spasmodic twitching of the muscles on the right side of the face when he attempts it. Can put out his tongue in all directions. Very difficult for him to attempt to pronounce the words, "truly moral." Generally sleeps well the latter part of the night; bowels constipated; pulse and respiration normal. Urine is passed without trouble, and at regular intervals in usual quantity; is acid, has a specific gravity of 1020; is heavily loaded with phosphates. Has seminal emissions from four to six times a month. Surface of skin is very hyperæsthesic. Upon strong percussion along the spinal tract, feels some pain. Ordered twenty minims fluid extract ergot three times daily, with good, generous diet, also directed him to take an occasional saline cathartic. Continued this treatment for three months and he thought he was improving, but, in fact, had not so good use of his limbs as before.

June 1. Can whistle somewhat more distinctly, but complains that he is losing control of his arms. Feeds himself with difficulty. When asked to touch his nose with either in-

dex finger, his hand describes a semi-circle, and as he approximates the finger to the nose, it is done with a sudden jerk.

Ordered a pill, consisting of 1-32 gr. strychnia, ¼ gr. capsicum, 1 gr. ferri hydrogen. redact., and, as he does not sleep, 15 grs. brom. potass., to be taken at bed time. Has frequent erections and great sexual desires. Urine still abundantly phosphatic in character. Continued the above treatment until Sept. 1st, with no amelioration, and a gradual loss of power of co-ordinating the lower limbs. Unable to walk alone, and walks with difficulty when supported; drags his legs along, advancing the heel first with a jerk. Ordered phosphor. acid and strychnia, with brom. potass. at night.

Dr. Clymer saw him about Oct. 1st, 1871, and observed the following more marked symptoms in his disease: Tone of voice drawling; brain seems weakened, and nystagmus of the eyeballs is apparent. Has, in addition, spinal epilepsy; it having only a general connection with the sclerosis. The latter condition only occasionally present. Sensation in both limbs impaired. The muscular will power in the right limb is good, but much weakened in the left, and in the latter, sensation is confused and tardy. Sensation of heat or cold, or the impression of a sharp instrument reaches the brain much sooner from the one part than another. There is characteristic dragging of the feet, and the will power is inadequate to give the proper stimulus to the muscles, yet he displays considerable control of the leg, when attempting to extend or flex it. Took most of the time until Oct., 1872, a pill, consisting of 1-50 gr. phosphor., 1-32 gr. strychnia, 1 gr. ferr. hyd. red., but with no apparent benefit. Oct. 1, 1872, Dr. Clymer saw him a second time. The spinal epilepsy still continues, but is not so marked. The muscular will power is much weakened. Has lost much in flesh, and at times there have been well marked symptoms of paralysis of the right side of the face. Cannot whistle and talks very indistinctly at times. Urine phosphatic in character and bowels much constipated. No nocturnal emissions; no sexual desires. Anæsthesia is very decided and sensation confused. Little treatment resorted to from this time until his death, except to keep his bowels regulated, and occasionally quinine to improve his appetite. Has to be fed, as he cannot

carry anything to his mouth, having little if any control of his hands. Sight much impaired. Three months before his death his urine was withdrawn several times, and then again a week previous to his death. It became very difficult for him to talk three months before his death, but at no time were well marked symptoms of aphasia observed. His trouble in speaking seemed to be due to paralysis of the muscles. In Sept., 1871, Dr. Stevens examined his eyes with the ophthalmoscope and diagnosticated sclerosis of the optic nerve.

Patient died Feb. 21st, 1874. At the autopsy—no general post-mortem being allowed—only the brain and spinal cord were examined, which were removed entire and sent to Dr. Seguin immediately.*

Case II. came under Dr. Seguin's observation.

Female, æt. 23. Single. Seen Oct. 20, 1873. A nervous girl, with occasional irregularity of menstruation, but no dysmenorrhœa. At times hysterical laughter and tears; never convulsive attack. In July, 1871, while out walking, after having climbed a number of walls, felt weak and awkward in right leg; thought she had sprained her knee. There is not enough evidence to support this statement. Ever since she has had weak right leg, without anæsthesia or numbness; at times more use of leg than at others; almost cured once or twice; of late has required help of crutch, or friend's arm in walking. When I examined Miss P., I found paresis of right leg, the loss of power being marked at ankle and toes. There was doubtful weakness of the right hand. I could not make out that the knee joint was affected. The muscles of the right leg showed a slight diminution of reaction to the faradic current, and this agent also showed that sensibility to pain was a little dull in leg and foot.

In view of the history of the case, the capricious development of the palsy, the absence of reliable signs of central disease, the presence of a strong neurotic element in the family, and the fact that strong emotions had been acting upon her, I concluded that the patient had a functional palsy of an hysterical nature. Strychnia was given her and faradism used.

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* History by Dr. Van Derveer.

The specific effects of strychnia appeared and the patient was decidedly tetanized for a while; this passed off, and when I last saw the patient, on Dec. 11th, she was in about the same state as at the beginning of treatment. The unfavorable effects of the treatment led me then to believe that the patient had an obscure central lesion, probably sclerosis. In March or April, 1874, patient rapidly grew worse, becoming paraplegic, and her hands showing paresis. In July she was placed in an irregular water-cure house, where extensive bed sores formed in consequence of want of care and of cold applications to the palsied parts. (She had continuous applications for several days.) Exhaustion and pyemia caused death, August 1st. The post-mortem examination showed disseminated sclerosis of the spinal cord. The brain not examined. Dr. Chas. A. Leale, of this city, treated the patient during July, after the bed sores had formed, and I made the autopsy at his request and that of Deputy Coroner Dr. Shine.

After hardening in bichromate of potash, sections were made in various regions of the encephalon and cord in Case I. In the brain proper, small patches or nodules of sclerosis 1 to 3 mm. in diameter exist in various parts of the white substance of the hemispheres; and there are a few just under the gray cortex. In the right nucleus caudatus, near the posterior margin, are several nodules; in the right occipital lobe just outside of the posterior horn of the lateral ventricle, extending well back toward the convolutions at the apex of the lobe, is a long sclerosed patch.

Pons and Medulla.—The upper part of the pons varolii and crura have not been critically examined. In the medulla oblongata the sclerosis appears in the following parts:

1. At the level of the apex of the fourth ventricle (Fig. 5), and below, a patch about 2 mm. exists in the very centre of the section across the median raphe.

2. In a section made at a point 6 mm. above the apex of the fourth ventricle (Fig. 6), a large patch of sclerosis invades the floor of the ventricle, including the mass of gray matter which gives origin to the hypoglossus, par vagum, and glossopharyngeal.

Few cells of the hypoglossus nucleus are visible, and these are small and rounded.

3. In a section made through the point of origin of the 6th and 7th nerves at about 2 mm. above the inferior border of the pons varolii, the sclerosis is found in about the same location, viz.: round about the nucleus of origin of these nerves under the floor of the fourth ventricle (Fig 7).

Cord.—Sections have been made in the cervical, dorsal and lumbar regions, stained in carmine and hematosin, and mounted in Canada balsam. With a low power or with the naked eye the following distribution of the lesion can be made out:

In the cervical region (Fig. 1), the sclerosis involves the following districts: Almost all the right anterior column, and the entire anterior horn, the posterior part of the left anterior column, and the whole of the anterior horn, the posterior part of the lateral column, a narrow band of cortical sclerosis of both posterior columns, the lower part of the columns of Tuerck near the commissure; all the gray commissure involved in the disease.

In a section a little below this in the *cervical region* (Fig. 2), the location of the lesion is somewhat different. The most decided disease is confined to the anterior part of the left anterior column, a small part of the external edge of the anterior horn, the posterior part of the lateral column, the columns of Goll and the gray commissure.

In the dorsal region (Fig. 3), decided sclerosis of outer part of left anterior column and the whole of the anterior horn, the columns of Tuerck, and a slighter sclerosis of all the cord posterior to a line drawn through the central canal.

In the lumbar region (Fig. 4), the right anterior column and border of the anterior horn, the posterior part of the right lateral column, both posterior columns, gray commissure, and slight sclerosis of the periphery of left anterior horn.

In Case II., only the spinal cord was obtained.

In the cervical region (Fig. 8), the following districts are diseased: The columns of Tuerck, posterior part of right anterior column, the upper part of right column of Burdach, the columns of Goll, posterior part of left anterior column.

Sections just below above in the cervical region (Fig. 9), show

the following distribution: The columns of Tuerck, part of right anterior and lateral column, the entire posterior columns, the periphery of the right anterior horn.

Dorsal region (Fig. 10), columns of Tuerck, the periphery of both anterior horns; the gray commissure, both lateral columns and both posterior columns.

In the lumbar region (Fig. 11), the columns of Tuerck, right lateral column, posterior part of left anterior column, entire posterior column, the periphery of both anterior horns. It will be seen from this description that the sclerosed nodules are of various sizes, and occupy the most diverse regions of the cerebro-spinal system, involving the gray as well as the white matter.

Histology.—The histologic study will include the two cases.

For convenience of study and description we will divide the morbid process into three (3) stages. This division, although arbitrary, can nevertheless be observed on a close study of the specimens, and aids very much in a description and the understanding of the various conditions which are observed in the many diseased regions of the cerebro-spinal substance: *

The first stage will comprise the very earliest changes which are to be observed.

The second stage in which the morbid process has advanced considerably.

The third stage in which the most extensive changes are to be seen.

The Neuroglia.—1st Stage. An examination of the neuroglia of the white matter at this stage, shows an increase in the size of the nuclei, their number is also slightly increased; there is an increase of the protoplasm around the nuclei; the quantity varying very much in different cells, in most of the cells it is only a little greater than normal, in a few it is enormously increased, as can be seen in Fig. 12, representing two cells seen in the anterior columns of the dorsal region in Case I. The nuclei have not only undergone increase in their size, but they have also assumed the most diverse shapes; in those cells

*The medulla and pons were examined by Dr. Seguin, the two cords and brain by Dr. Shaw.

Fig 1.

Fig 2.

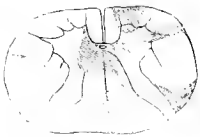

Fig 3.

Fig 4.

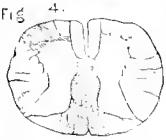

Fig 5.

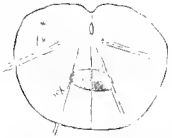

Fig 6

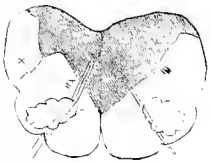

Fig. 7.

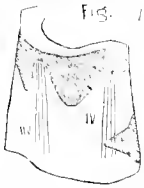

Fig 8

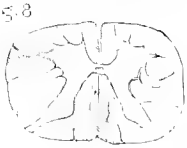

Fig 9.

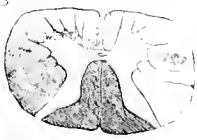

Fig 10.

Fig 11.

Fig 12.

Fig 15.

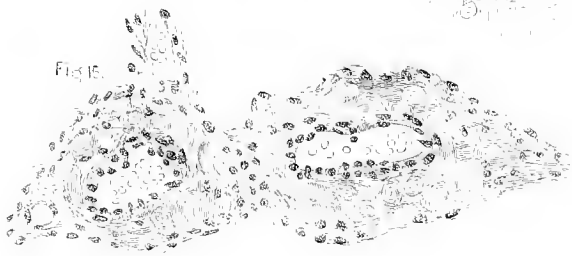

Fig. 13

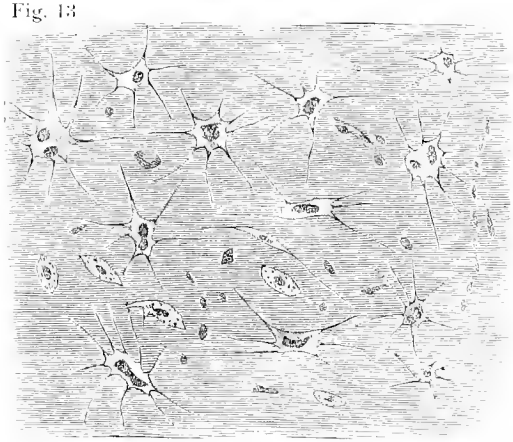

Fig. 14

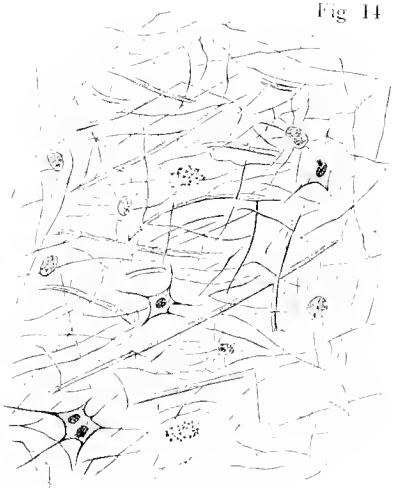

Fig 16

where the protoplasm is very much increased, the nucleus is found at the extreme edge, as if the protoplasm had increased all on one side; we also begin to notice slight processes from the protoplasm; even now there is an appearance of increase in the size of the fasciculi of tissue running between the various areas of nerve tubes. At this time all the surrounding nerve tubes appear to be normal, their axis cylinders and myelinic sheath are perfectly distinct. In the gray matter at this period, the alterations are very similar: a slight increase in the size of the nuclei and the protoplasm around them, and beginning to be visible processes from the protoplasm.

In the second stage, the nuclei and their surrounding protoplasm have increased in size, their processes are now much larger, and more distinct and apparently more numerous; and there is altogether a much larger number of these branching cells visible. There is now present a diffuse granular appearance; no special fibrillation to be seen; a very close observation shows that the axis cylinders are present but that their myeline sheaths have gone. (The granular appearance at this period is very probably due to the breaking up of the myelinic sheath into a granular looking material which becomes diffused among the surrounding tissue, and this perhaps also explains why we do not see the increased fibrillation of the neuroglia.) The process at this period is the same throughout the brain and cord; in the gray as well as in the white matter.

In the third stage, these branched cells have grown to an enormous size, their processes are numerous and sometimes of immense thickness and length (Fig. 13); now very little granular material is present, and we see distinctly the enormous increase of neuroglia fibres, and which are of exceeding coarseness; there appears to be no evidence of anastomosis between them. In a stage a little advanced of this (Fig. 14), these branched cells appear much less numerous, they are less distinct, their nuclei are less sharply stained, their outlines are not so perfect, and they sometimes appear as imperfect bodies; they in their turn, appear to have lived their day, and are now undergoing decay.

Nerve Fibres.—In the second of the artificial periods, which

we have created for convenience, we see the first changes in the nerve fibres, their myeline sheath is undergoing disorganization; but their axis cylinders still remain.

In the third period, when the increase of neuroglia fibres is at its greatest height, we still see among its coarse fibres numerous axis cylinders, in good preservation (Fig. 14); this preservation of the axis cylinders, is considered almost distinctive of disseminated sclerosis by Professor Charcot. Charcot states that the axis cylinders have become hypertrophied; this is a matter which appears open to question.

Ganglion Cells.—The following description holds good for the ganglion cells in the entire cord. In the decidedly diseased portions there is a remarkable absence of processes; but it happens occasionally, even in a markedly sclerosed area, that a cell will be seen whose process is apparently normal, and can be traced for a considerable distance; most of the cells contain more or less yellow pigment, some are almost entirely filled with it, and no nucleus is visible; in other cells the pigment is seen encroaching on the nucleus; in some cells the nucleus is seen with clear and definite outline; in many other cells they present an indistinct jagged edge, and occasionally they appear to be making an effort at budding.

In some cells all the nucleus appears to have undergone a form of dissolution, as it presents a very broken appearance. In a number of cells the nucleus is displaced, sometimes almost into one of the large processes, in others to the extreme edge of the cell. In a markedly sclerosed area in the anterior horn of the cervical region, the cells have undergone a simple diminution in size, so that they are reduced to at least one-third. The cells in the columns of Clarke have undergone the same changes. The alteration in the cells is greatest in Case II.

Vessels.—In Case I. the muscular coat of the arteries are very much hypertrophied, there is also some increase in the intima and adventitia and an increase in the number of nuclei; the lumen of the artery is therefore rendered much smaller than normal. (Fig. 15.)

In Case II. the arteries have also undergone the same changes; although the muscular coat is not nearly as much hypertrophied, and the lumen of the artery is much larger.

In comparing the arterial changes in these two cases we find that in Case I. the arterial change is greatest, and the sclerosis less marked, whilst in Case II. the sclerosis is most decided and the arterial change much less.

The nuclei of these branching cells are very sharply stained by the hematosin, whilst the body is very lightly stained; they have slightly granular contents; these nuclei present the same appearance at all stages of the process. The cells contain 1, 2, 3 and even 4 nuclei of irregular shape; and often nuclei are seen with partial constrictions on them as if they were about to divide; the nucleus is most usually placed to one side of the cell. The processes which are given off from these cells are very numerous, and always leave the cell by a broad base, tapering off to a hair-like extremity, which becomes lost in the surrounding tissue. The shape of the cells, and the manner in which the processes are given off, varies very much; some cells are to be seen with only two processes, one at each end.

Other cells have a rather long body, and many processes given off from each end; but the majority of cells have processes from all sides. Two of the cells seen are of enormous size, having a process of very great thickness, which can be seen for quite a distance and does not taper off like the other processes. (Fig. 16.)

There are besides these branching cells small nuclei in more or less large number and of a variety of shapes; but mostly round and sharply stained. These curious branching cells have been seen by other observers. Lubimoff and Mierzejewski have described them in the cerebral substance of general paralytics. Charcot and Gombault in a case of syphilitic disease of the protuberance. Pierret in a case of myelitis. Debove appears to have seen them in a sclerosed patch of the ependyma ventriculorum. They are to be seen in a case of central myelitis with cavities from a patient of Dr. Seguin's.

Lubimoff, Beitraege zur patholog. Anatomie der allgem. progressiven Paralyse, *Archiv fuer Psych. und Nervenk.*, Band 4.

Mierzejewski, Etude sur les lesions cerebrales dans la Paralysie Generale, *Archives de Physiologie*, Tome 2, 1875.

They have been figured by Jastrowitz* in an article on "Study of Encephalitis and Myelitis in young children." Adler† has also figured them. The cells figured by Jastrowitz are very similar to those seen by us. Mierzejewski describes these cells as being connected with the walls of blood vessels. We have observed them in the vicinity of blood vessels and sending their processes towards them, but it is very difficult to determine in this case whether they unite with the wall of the vessel or not. They have no special predilection for the neighborhood of vessels, but are to be seen every where. Pierret describes them as anastomosing; we have not seen any anastomoses between our cells.

Frommann figures, in sclerosis of the cord, cells somewhat similar; but they have fewer processes and are very much smaller than the cells observed in our cases; 500 to 950 diameters made them distinct; their protoplasm is not so abundant.

The interest attaching to these cells is in knowing if they are normal elements of the neuroglia which have become hypertrophied. In looking over the records of pathological cases which have preceded, and the many recent observations in normal histology of the neuroglia, there appears good reason for believing that these are really normal elements of the neuroglia which have become hypertrophied. The cells, with hair-like processes, figured by Boll‡ and Deiters. Those depicted by Frommann, although containing a much larger number of fine processes and a nucleus surrounded with very little protoplasm, might be supposed to have undergone a modification so as to present the appearances of our cells.

Butzke§ figures cells with processes from the normal neuroglia, which on the whole, have a much closer resemblance to the cells just described by us than any others we have seen.

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* Jastrowitz, *Archiv fuer Psych. und Nervenkrankheiten*, Band 3.

† Adler, Ueber einige path. Veraenderungen an den Hirngefaessen Geisteskranken, *Archiv fuer Psych. und Nervenkrankheiten*, Band 5.

‡ Boll, Die Histiologie und Histiogenese der nervosen Centralorgane, *Archiv fuer Psych. und Nervenkrankheiten*, Band 4.

§ Butzke, Studien ueber den feineren Bau der Grosshirnrinde, *Archiv fuer Psych. und Nervenkrankheiten*, Band 3.

His cells have a good sized nucleus with a considerable amount of protoplasm around them, and numerous fine processes with one or two large ones which do not taper off to fine points, and resemble very much the large one figured by us, as seen in the posterior part of the lateral column in the cervical region.

Explanation of Figures.—Figures 1 to 11. Sections from various regions of both spinal cords, showing the various distribution of the sclerosed patches. Verick No. 2. Eye piece 3.

Fig. 12. A neuroglia cell in the white matter of the spinal cord; increase in the size of the nucleus, with an enormous increase of the protoplasm around it. 1st stage.

Fig. 13. From a section through the corpus striatum; a large number of branching cells are seen distributed in a somewhat coarse mesh of neuroglia fibres; one or two cells proper to the corpus striatum undergoing degeneration. 2d stage.

Fig. 14. Section from the brain; axis cylinders without a trace of myeline, lying in a very coarse, loosely arranged neuroglia fibre; a few branching cells showing all stages of degeneration. 3d stage.

Fig. 15. Showing transverse section of artery with hypertrophied muscular coat, and an increase of nuclei in all the coats.

Fig. 16. A very large branching neuroglia cell seen in the white matter of the cord; two nuclei are to be seen, one of which shows a constriction in the middle as if it were about to divide. Verick No. 7. Eye piece No. 3.

Art. VI.—CONTRIBUTION TO THE STUDY OF OSSIFICATION OF THE MENINGES.

By J. N. De Hart, M. D.,

Second Assistant Physician to the Hospital for Insane, Mendota, Wis.

CALCAREOUS plates situated on the cerebral membranes are frequently found in epileptics, and in lepto-meningitis of long standing as well as in pachy-meningitis. True ossification is rather rare, and it usually begins on the inner surface of the cranial bones, and presents itself in the shape of spiculæ of bone. In calcareous plates bone corpuscles are never found; in spiculæ and bony tumors they are always present.

The former owe their origin merely to a deposit of calcareous salts in exudative inflammatory products: the latter are the result of a true organizing action, through the medium of cells, exactly as in normal bone. Ossification in the membranes of the brain is of very rare occurrence, but calcareous degeneration of their exudation laminæ sometimes takes place, as it is known to occur even in ganglionic cells of the brain.

Erlenmeyer found the commissure of the optic nerves hardened, by deposits of calcareous matter in the brain of a monomaniac, who had died with epileptiform convulsions. It had been first deposited about the small arteries, and the connective tissue; the cells had afterward been occupied and made opaque by fine granules, of what appeared to be phosphate of lime.

Forster in his atlas of Pathological Anatomy, describes calcified cells found in the gray substance of the lumbar enlargement of the spinal cord, of a boy whose lower extremities were paralyzed.

Heschl, in *Schmidt's Jahrbuecher*, 1863, is the only one, so far as I am able to find, who met with what he calls an ossification of cells in the brain of a patient, æt. 26 years, who died melancholic: they were in the compact substance surrounding a small hemorrhagic cavity in the cortical part of the right cerebral hemisphere. He used hydrochloric acid to dissolve the granular contents, and this left the cells with a pale outline in view.

The following case is one in which, on making an autopsy, there were found to exist several genuine ossifications of the falx cerebri.

T. S. L., was admitted to the State Hospital for Insane July 1, 1876, æt. 35 years. Born in England, married, tailor. The certificate states that this attack, which is the first, commenced three months since. He was formerly very intemperate, but has not drank much for the last three months. The disease is increasing. He fears that he is going to be taken away by some person unknown. Is not suicidal nor homicidal. Is neither destructive of clothing nor of filthy habits. Physical health very much enfeebled. He was rather passionate and smoked to excess. Has had one stroke of paralysis (but could

not ascertain whether it was paraplegia or hemiplegia). No relatives have been insane. Venereal excess is supposed to be the predisposing cause of this attack. On the morning of the 12th of July, he was unable to rise from the bed. His pulse was beating rapidly at the rate of 105 per minute, and the temperature of his body was 107°. The bladder was distended with urine and a catheter was introduced: about three quarts of bloody urine was withdrawn. The next day the scrotum was swollen, and urine discolored with blood was continually dribbling from the bladder. A catheter was again introduced and he was relieved of one quart of urine. His appetite was very fair, pulse and temperature nearly normal.

On the 14th the inflammation of the scrotum had somewhat abated. Although he continued to take considerable nourishment, yet he failed rapidly; he soon became comatose, and after remaining in this condition for several hours, died at 5 P. M.

This patient being in such an enfeebled condition at the time of his admission, and never rallying, it was impossible to obtain any facts concerning his previous history except as stated in his application.

An autopsy was made 22 hours after death, with the following results: The cadaveric rigidity was well marked. The lungs, spleen, and kidneys were normal. The heart was hypertrophied and fatty. The bladder was greatly enlarged, and its walls thickened; the mucous membrane, and in some places the muscular tissue, was ulcerated. It was very evident that the patient had suffered for a long time from chronic cystitis, with ulceration as a result. The scalp was normal. The calvarium moderately thick. The falx cerebri was found to be partly ossified; the ossified portion measured two inches in length and three-fourths of an inch at its widest point; some spiculæ were detached from this portion. The brain otherwise was in a normal condition, as far as the naked eye could determine.

In preparing these spiculæ for microscopic examination, I selected the chromic acid solution (2 gr. to 1 oz. of water) in preference to using hydrochloric (dilute) acid. The decalcification takes place slowly, according to the strength of the solution, and this should be changed every few days as the

acid dissolves the lime more rapidly when this is frequently renewed.

On microscopic examination these spiculæ were found to be true bone. Some sections of the ossified portion were placed in a similar solution for a few days: sections were then made, and stained with carmine in glycerine. They were then mounted in glycerine and submitted to an examination, which revealed the presence of the laminæ, Haversian canals and lacunæ of true bone.

Beginning as these growths do from that membrane which acts the part of a periosteum for the internal surface of the cranium, we are inclined to attribute the production of genuine bony plates in the dura (as well as its falciform processes) to a relapse in the direction of its previous formative activity in infantile life.

A connecting link between this osseous heterotopia and the normal production of bone is furnished by those triangular portions of bone occasionally found by the side or to the side of, and below the posterior clinoid processes, in the tentorium cerebelli. Histologically they do not differ from the hyperplastic spiculæ just described. Morphologically, however, they constitute an analogue of the *bony* tentorium of the lower mammalia and are more frequent in the negro than the Caucasian, although even in the latter, not infrequent, as shown by the older anatomists.

Neurological Correspondence.

By Geo. W. Wells, A. M., M. D.,
of New York City.

Messrs Editors:—During the last three months, discussions suitable for this Department have touched upon questions relating to insanity and kindred topics.

One of the most important papers read—of which we give a very full abstract—discusses the proposition whether or not, a person is necessarily insane because he commits suicide? The paper is more important because it was written by the solicitor of one of our largest Insurance Companies. It will be published in pamphlet form at an early date.

We have been compelled to omit, for want of space, many interesting and valuable offerings; but hope to be able to make amends in future.

THE MEDICO-LEGAL SOCIETY.

Dr. A. E. Macdonald read a paper entitled: "Two cases of Homicide; defense, Insanity." We present the following *resume:*

It has long been an acknowledged fact, stated by almost every essayist upon the subject, that the estimation of the weight of a plea of insanity, as affecting responsibility for crime committed, is apt to be embarrassed by other and extraneous influences, holding no legitimate relation to it, and yet materially interfering with its results. In other words—that the solution of the question given is not always such as would be arrived at were it answered simply upon its own merits and without reference to contingencies and consequences. Even when, as under the most favorable conditions, the problem is submitted to experts alone, they are apt to be swayed, though possibly but slightly, and probably quite unconsciously, by such facts as their retention by one or other of the two parties to the controversy.

When from experts we pass to juries, the number and potency of such extraneous agencies is greatly enlarged, and prejudices of various kinds, social, political, even religious views as to the propriety of capital punishment, and a host of other influences are apt to shape a verdict, which nominally turns upon the existence or non-existence of insanity alone.

Beyond these influences affecting individuals, there are still others which affect communities, making perhaps the practice in a given locality to vary from time to time, or that of different localities to exhibit marked contrasts.

Of the former we have many examples in the history of the criminal trials of various States, and examples, too, which bear a striking resemblance one to the other. As a rule, the alterations or fluctuations between too great leniency and too great severity in the receipt of the plea of insanity have been simply reactions one from the other—the escape of two or three criminals leading to the conviction of the next two or three, on perhaps less evidence, and that again reaching in the direction of greater leniency.

The frequently quoted case in New Jersey, where the acquittal of a woman on the ground of insanity, who was clearly not insane, led to such a revolution of public feeling as to produce the conviction and execution of a man who clearly *was* insane, affords a familiar instance of this change; and other instances are not wanting in the annals of the courts of our own State and city.

Perhaps the influence of extraneous conditions is most patent in those cases where murder has followed and revenged seduction; where the husband or brother of the woman seduced has taken the life of her seducer. In such cases, the plea of insanity has commonly been entered, and the jury, evidently holding that the punishment of the seducer was a righteous one, but not having the temerity to say so flatly, has gladly availed itself of the opportunity which the plea of insanity afforded it of accomplishing its purpose in an indirect and less startling manner.

Apart from the proposition with which he set out, that the manner in which the plea of insanity will be accepted will differ with different communities and reflect that difference in

the action of the jury, the comments of the press, and the temper of the people—there is one other point which should be instanced briefly, namely: That confinement in an asylum is not speedily enough secured for those who have given indubitable evidence of insanity, and especially for those in whom such evidences have been accompanied by or taken the form of violence. The history of homicides and other deeds of violence committed by the insane, out of institutions, shows that the perpetrators are not, as a rule, those who have suffered from sudden and acute attacks of the disease, but those who have long been recognized as affected, but mistakenly regarded as harmless [the so-called "insane though harmless" class].

The newspaper reports which reach us from day to day, of violent acts on the part of the insane, commonly terminate with the statement that it had been observed for some time that the perpetrator's mind was affected—although no danger of any act of violence was apprehended. When cases of this nature are of such frequent occurrence, there must be clearly either a defect in the law, or laxity in its administration. The relatives of insane persons should be required by the law to take measures to place them under restraint; and the proper officers should compel them to observe it.

And of equal importance to the securing of the placing in confinement of dangerous lunatics, is the securing of the continuance of such confinement as long as the dangerous tendency shall continue. If a large number of the acts of violence committed by the insane, are traceable to those who should have been sent to asylums and have *not* been, many others are traceable to those who should not have been discharged from asylums and *have* been.

He thought that the laws of commitment might appropriately be modified so as to throw greater safeguards about the discharge from an asylum of patients coming to it with a clear and direct history of violent tendencies. As it is, the Superintendent of each asylum has to assume the entire responsibility, and when his judgment, and a desire for the public safety lead him to hesitate, he does not always receive that support from the courts and the public which he ought to command. He had himself had personal experience of this within the past

few days. A young lad was sent to the asylum in October last, presenting in a marked degree that association of symptoms which is described by some writers as constituting the insanity of pubescence—and with them such an exaggeration of ideas as, coupled with other symptoms, suggested the possibility of commencing paresis, although the boy's age—18,—was, of course against that possibility. In the affidavit of the father praying for his son's confinement, he stated that he was "violent and dangerous to his friends and family," and several acts of violence, leading to his being twice arrested, and confined in the Tombs, were reported to him, as was also a previous alleged attack of the disease. After three and a half months in the asylum, he was clearly, to his mind, still insane, "retaining, for instance, the delusion that he could make $20,000 a year by starting a business in which he had no experience, and for the commencement of which he had no capital;" and as he had at different times shown violence, was full of threats, and manifested resentment and change of feeling toward the members of his family when they saw him. The Doctor declined to assume the responsibility of his discharge, a writ of habeas corpus, examination before a referee, testimony of a physician who saw him for half an hour, and thought him "as sane as any man," followed—and so a boy, who certainly will not stay out six months and who will more than likely commit some act of violence in the mean time, is let loose upon the community; while the newspapers represent the Doctor as saying exactly what he did *not* say—and another point in insane asylum abuses is exemplified with Charles Reade and the usual garnishings. In this regard the Superintendents of State asylums are more fortunately situated than are those of county institutions; for the former can exact from the relatives of insane inmates desiring to remove them, bonds, whereby they become responsible for any damage to persons or property that may follow the proposed liberation. Often the demand for such bonds and the suggestion of personal responsibility are sufficient to deter the applicant, and in any case the Superintendent is protected.

The Superintendent of a county asylum has no safeguard, and cannot shift his responsibility should any casualty follow a

premature or ill-advised discharge. In this matter, at least, they should surely be made equal; and even more strenuous laws relieving them both from the sole responsibility, and upholding them in cases where their judgment led them to retain a patient, would not be out of place.

It undoubtedly seems hard that a man who appears quiet and harmless in an asylum should be retained there against the wishes of his family; but it should never be forgotten that the quiet and regularity of asylum life, and the restraint of asylum discipline, have much to do with his condition, and that the excitement of a return to former surroundings and modes of life are likely soon to cause a return of violence.

Thus much in the case of men whose acts of violence prior to confinement have not gone to the extent of bringing them within the reach of the law. Of those who have committed or attempted homicide and secured acquittal upon the ground of insanity, the confinement should, in his judgment, endure for life, or at least until there can be a certainty, not only that the existing attack has been recovered from, but that the danger of a recurrence has passed.

Much of the scandal that attaches to the plea of insanity in criminal trials arises from the brief period that in many cases elapses between the prisoner's acquittal and his discharge "as cured" from the asylum to which he may have been sent.

Hardships to individuals may arise from stricter practice in this regard; but in this, as in other social exigencies, the happiness of the individual should be subordinated to the safety of the many.

Hon. O. H. Palmer recently read a paper before the Medico-Legal Society on "Suicide Not an Evidence of Insanity." We give the main points of his argument as follows:

He did not know how it may be among scientific and professional men, but he thought he was justified in saying that in the average mind there is a strong conviction that suicide is invariably the result of an insane impulse—in other words, that the act itself is conclusive evidence of insanity. He ventured to say that in half the cases, if you put the question to an unprofessional man and ask an opinion, this will be the reply. The common belief is that no man will take his own

life unless demented or insane. It is because we cannot readily comprehend that one in his right mind would throw away his life, which we regard as a precious blessing, and take the chances of a future existence wherein, according to our Christian doctrine, the very act of *felo de se*, by a responsible being, is accounted a heinous crime deserving infinite punishment. The object of the paper was to show the unsoundness of this too generally received opinion, and to prove from facts and authority that it is not well founded.

The manifestation of states of mind is not uniform, but it is as varied and dissimilar as the expression of the human face or conformation of the human brain. There is no mathematical standard for the mind to which we can apply the square and compass, and determine and measure it. The diversity of manifestation is immense, unmeasurable, unascertainable. But this does not prove insanity or derangement of the normal condition of the intellect. Sanity is the normal condition of the mind in all its diversities and varied character. This is law as well as logic. This is now too well established in this country to be doubted or questioned, and yet outside of the literature of the courts it has but few believers. If any one doubts the fact, let him attend the impanelling of the jury in a case where the question is to be tried, when it is important to exclude from the jury-box such jurors as believe that suicide is of itself evidence of insanity, and he will no longer doubt. It is well known that life insurance companies decline to insure against suicide, and provide in their contract that if the insured feloniously destroy his own life they will not be liable. It is a risk they will not assume. The question is, therefore, frequently presented in the courts, and the provision is invariably sought to be avoided by alleging insanity. Hence the great importance of a correct understanding of the question in this discussion. Unless the popular belief can be corrected, the protection sought by the contract is of no avail, and the provision might as well be abandoned. It is believed that the stability of any system of insurance depends upon the right of the underwriter to determine what risks he will insure against, as well as upon an intelligent administration and application of the laws governing and protecting it. Dis-

belief in the doctrine that sanity is the normal condition of the mind, and belief in the doctrine that self-destruction arises from insanity, are heresies in the public mind which all reflecting men will say should be corrected. The true doctrine in respect to both of these heresies is abundantly established by the decisions of the higher courts, State and National. *Vide* Coffee *v.* The Home Life Ins. Co., 35 N. Y. R. 314; Weed *v.* The Mut. Benefit L. I. Co.; Van Zandt *v.* Same, 55 N. Y. R. 169; McClure *v.* The Mut. Life Ins. Co., 55 N. Y. R. 651.; Terry *v.* Same, 15 Wallace, 580; Charter Oak L. I. Co. *v.* Rodd in the U. S. Sup. Ct. These cases establish the doctrine that there is no presumption of law, *prima facie*, or otherwise, that self-destruction arises from insanity.

The law also regards suicide, *felo de se*, as a crime, putting it in the same category with murder. It has been so held for many centuries, not only by the state but by the church. By the common law also, if any one encourage or assist another in the commission of suicide, he is guilty of murder as a principal; and, by the ancient common law, a will of personal property was made void by the testator's subsequent act of self-destruction.

Now these laws must be founded upon the idea of suicide without insanity, otherwise they would be monstrous.

If suicide were invariably the result of insanity, according to what may be termed the present popular belief, the law-makers and religious teachers for many centuries have been guilty of the most enormous crimes. Abundant evidence is found by reference to text books to sustain this position, and the doctrine has been handed down through the courts for many generations, and one would suppose it was now too well established to be questioned; but, strange as it may appear, not long since a judge in one of the New York courts deliberately held, in an action upon a life insurance policy, that suicide *per se* was evidence of insanity, and so ruled in the case before him, and that the burden of proof that the *felo de se* was not insane was upon the defendant. That judge would, of course, have ruled, if the question had arisen in respect to the ancient Stoic or Epicurean philosophers who destroyed themselves, claiming the act to be one of heroism, that they

were all insane. Among the ancients suicide was considered neither criminal nor dishonorable. The ancients, at all events, did not agree with the New York judge in considering the suicide necessarily insane!

The subject of suicide has as yet but a scanty literature, especially in this country, and the little that does exist is not very satisfactory. Many physicians have touched upon the subject, for instance Dr. Maudsley in his "Responsibility in Mental Diseases;" but physicians generally begin to consider the subject on its diseased side, and almost universally incline to the belief that suicide is positive proof of insanity, which idea is not the pure outgrowth of modern science, but was held by one Moore, a Vicar of Oxford, as early as 1790, who says in a book printed in that year: "But some who are ever desirous of leaning toward the side of humanity, are inclined to judge that the very act of suicide, being so horrid and unnatural, implies a subversion of the brain, or a species of madness. This, however, is deciding too favorably of the matter."

The severity of the penalties inflicted upon the bodies, estates and descendants of the *felo de se*, has undoubtedly had a powerful influence in propagating the belief that suicide was the result of insanity. It was to avoid those horrid penalties that coroners' juries were originally accustomed to bring in verdicts that the suicide was insane. Bentham says "English juries did not hesitate to violate their oaths by declaring the suicide *non compos*." At the time mercy seemed to make this finding a necessity. It was the outcome of humanity in defiance of a barbarous and revolting law. Although the necessity has passed, the result of this forced education is yet manifest, and the coroner's jury of to-day, especially if it can be impressed with the idea that benefit is to accrue to the sorrowing friends, is almost invariably inclined to return the verdict stereotyped three hundred years ago, "Suicide while laboring under temporary aberration of mind."

Lecky, in his "History of European Morals," says: "The effect of the pagan examples may frequently be detected in the last words or writings of the suicides. Philip Strozzi, when accused of the assassination of Alexander I., of Tuscany, killed himself through fear that torture might extort from him

revelations injurious to his friends, and he left behind him a paper, in which, among other things, he commended his soul to God, with the prayer that, if no higher boon could be granted, he might at least be permitted to have his place with Cato, of Utica, and the other great suicides of antiquity."

The ancient heathen philosophers are not the only advocates of the crime of suicide. It has had its defenders, and powerful ones too, in more modern times—Rousseau, Madame de Staël, Gibbon, Hume, Dr. Donne, Montaigne, Montesquieu, and others. We do not know nor can we tell the extent of the influence of the teaching of these apologists of suicide upon the thousands that annually seek as a matter of choice, this relief from the ills of life while in the full possession of all their mental powers. It would be a happy relief, were it true, to think that none but the insane shuffled off the mortal coil in that way.

Even under British rule in India to-day, suicide is very common, and more or less tolerated of necessity by the authorities. Among the many castes in that country is one held in high esteem by reason of the great importance of the service its members perform as carrier-messengers and mail-carriers. Strange as it may seem, *suicide has been the protection* of this caste against brigands and highwaymen from time immemorial. A peculiar and terrific custom of this caste is maintained as a necessary safeguard to its calling. A carrier who has been robbed is deemed to have been despoiled of what is a thousand times dearer to him than life, his honor; and in the presence of the robber, after vainly giving him full warning and an opportunity to restore the property stolen from him, he kills himself; thereupon the whole caste of the carrier rises as one man and swears a remorseless vendetta against the thief, his family, kinsman, friends, and village, until the last soul of them is exterminated. There is no other atonement. The carrier entering the service of the public bound himself, by the most solemn and fearful oaths, under the sanction of his religion, to protect his caste and to punish any attempt to dishonor one of its members; the Brahmins consecrated this service by pronouncing the most appalling anathemas against the transgressor, and so the custody and transportation of prop-

erty by one of their caste passed into a law. At this day, a package, however valuable, in the hands of one of these men is safer than though guarded by an army; much safer than public moneys under the control of politicians or trust funds in the care of some savings banks. It might be a blessing if we had a similar caste in this country to declare a relentless vendetta against thieves and robbers.

It was his purpose in the ontset to present the statistics of this increasing and appalling crime in our own country; but to his great disappointment and amazement, he finds it impossible. He has looked into the United States Census Reports. He could give from them, the number of the insane and the idiotic; the number of deaths by consumption and many other diseases; but not a word as to deaths by suicide. This subject, which he thinks is one of the greatest importance in vital statistics, seems to have been wholly ignored. There are no reliable data in this country that he can find to enable him to lay before the reader the facts which, if available in detail, would fully verify the position he has assumed.

In many of the European countries, there is less embarrassment. In Germany and France, especially, great care is taken to ascertain and preserve the facts bearing upon the question. It is a shame that it has hitherto been so much neglected here. One of the most celebrated German writers on vital statistics, and who is of the highest authority on that subject the world over, Alexander Oettengen, has collected and collated the statistics of suicide in Germany and many other European countries, and has deduced therefrom some interesting general laws. He maintains that suicide is one of the strongest proofs of freedom of the individual will; that the possibility of taking one's own life, either from disgust or from higher motives, as in the manner of the Buddhists or Stoics, is evidence that a man is his own master; that the commission of crime, as well as suicide, in many cases shows strength of will; that, notwithstanding the love of life, the tendency to suicide remains, and the number of suicides increases; that the increase in European countries is from 3 to 5 per cent., while the increase of population is less than 2 per cent.; that this tendency varies according to the season of the year; that it is more frequent

in the hot summer months than in winter; that the heat seems to increase the tendency and debilitate the physical system or power of resistance; that every sinner carries the germ of suicide in him; that the bad deed itself is to be regarded as the ripe fruit which is shaken from the tree by the storms of social life; that while myriads have the germ or tendency to suicide in their hearts, it is only developed by circumstances; that society, generally, is involved in the responsibility of the increase of this tendency; that the rich, by helping the poor, succoring the distressed, inculcating sobriety and religion, might materially mitigate the growing evil; and that as each age has its tendency to crime, so also it has its tendency to suicide.

Buckle, in his "History of Civilization," says, it is "an astonishing fact that all the evidence we possess respecting it points to one great conclusion, and can leave no doubt on our minds that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances." He maintains that there exists a regularity in the entire moral conduct of a given society—that the crimes of murder and suicide, which might well be supposed the most arbitrary and irregular of all the offenses, are "committed with as much regularity and bear as uniform a relation to certain known circumstances as do the movement of the tides and the rotation of the seasons."

He illustrates this by contrasting the circumstances which surround the respective criminals.

Of the crime of murder: "When we consider that this, though generally the crowning act of a long career of vice, is often the immediate result of what seems a sudden impulse; and when premeditated, its committal, even with the least chance of impunity, requires a rare combination of favorable circumstances, for which the criminal will frequently wait; that he has thus to bide his time, and look for opportunities he cannot control; that when the time has come his heart may fail him; that the question whether or not he shall commit the crime may depend on a balance of conflicting motives, such as fear of the law, a dread of the penalties held out by religion,

the prickings of his own conscience, the apprehension of future remorse, the love of gain, jealousy, revenge, desperation —when we put all these things together there arises such a complication of causes that we might reasonably despair of detecting any order or method in the result of those subtle and shifting agencies by which murder is either caused or prevented."

Of the crime of suicide: "Among public and registered crimes there is none which seems so completely dependent on the individual as suicide. Attempts to murder or to rob may be, and constantly are, successfully resisted; baffled sometimes by the party attacked, sometimes by the officers of justice. But an attempt to commit suicide is much less liable to interruption. The man who is determined to kill himself is not prevented at the last moment by the struggles of an enemy; and as he can easily guard against the interference of the civil power, his act becomes, as it were, isolated; it is cut off from foreign disturbances, and seems more clearly the product of his own volition than any other offense could possibly be. We may also add that, unlike crimes in general, it is rarely caused by the instigation of confederates; so that men, not being goaded into it by their companions, are uninfluenced by one great class of external associations, which might hamper what is termed freedom of will. It may, therefore, very naturally be thought impracticable to refer suicide to general principles, or to detect any thing like regularity in an offense which is so eccentric, so solitary, so impossible to control by legislation, and which the most vigilant police can do nothing to eliminate." Yet he finds the same general law that in a given state of society a certain number of persons will commit murder, and a certain number put an end to their own lives by suicide.

He had seen a statement which strikingly illustrates this fact, namely, that in the year 1793 suicide became an epidemic in the city of Versailles, in France, and raged to such an extent that in that single city there were 1,300 cases during that year. Also, that at one time it broke out in the army of the first Napoleon, and threatened to decimate his forces, and was only checked by the emperor's strong personal appeal to the

patriotism, pride, and courage of his soldiers. Von Oettengen shows also that the suicidal tendency varies with the days of the week and hours of the day; that on Saturday fewer men take their lives, that being the day when wages are paid and Sunday is before them; that on Monday and Tuesday, the per cent. is much greater; that the difference between men and women in this respect is very characteristic: that the woman oftener commits suicide on Sunday, when the vagabond man leaves her to her care and sorrow—very seldom on Saturday, cleaning day, or on Monday, when her week's work begins; that, on the contrary, when the man's work begins, the percentage among men increases; that race and social circumstances have an important bearing upon the question; while in one of the provinces of France the deaths by suicide is 298 in a million of inhabitants, in Corsica, where murder is the common pastime, it is but 13 8-10; in Scandinavia, 126; among the Germans, 112; among the Roman races, 80; among the Sclaves, 47; and in Prussia, 215.

Suicide occurs more frequently among prisoners and persons who lead bad lives; next among servants and soldiers; next among those who come in contact with the varnish and luxury of civilization, without being inwardly elevated or having developed self-reliance, for the reason that superficial culture leaves them less able to resist overwhelming calamities.

In respect to suicide, the ratio of women to men is as one to three; that in respect to crime as one to five; the tendency to suicide increases with age, the number being far less between 16 and 40 than between 50 and 70.

It is stated on the authority of Dr. De Boismont that since the beginning of the present century not less than 100,000 Frenchmen have committed suicide. I am inclined to think that large as the number is, it is understated. The statistics for the single year 1876 show the number for that year alone was 5,567, of whom 4,435 were men and 1,132 women; 29 were men under the age of 16, and 98 over 80; 1,828 were peasants, 1,038 of the working classes, 228 domestics, 987 of the liberal professions.

Among the causes assigned are the following: Drunkenness,

1,443; afflicted with incurable diseases, 798; domestic broils, 633; dread of poverty, 329. *Less than one-third of the whole number in the sad list is charged to insanity.*

It will be seen that all these facts when analyzed—and he might add many more—prove inevitably the fallacy of the theory that suicide is the result of insanity. There has been a custom among the Japanese which the most inveterate believer in the doctrine that suicide is *per se* evidence of insanity, would find it difficult to reconcile with his theory. If the oriental desired to inflict sure and summary punishment upon his enemy, he would kill himself upon his enemy's front doorstep, and such enemy was thereupon in duty bound to take his own life.

While the Jews and Persians share in our horror of suicide,—for they set the highest value upon the earthly existence,—a totally different feeling obtains among the Chinese, Japanese, Hindoos, Fijians, American Indians, Malayans, and other nations. With these, the superior blessings of the future life over those of this form a part of the popular religious belief. The taking off of one's self, under certain circumstances, is viewed as an act not merely pardonable but heroic. To pretend that he who commits it under such predispositions and surroundings is *non compos mentis* is clearly absurd. Says Elam, in his work entitled "A Physician's Problems:" "In our former investigations, also, we judged of the degeneration of the people in part by the excess of crime and the great frequency of suicide, but we cannot with propriety apply that test to the oriental people; we cannot consider their statistics as equally significant with the records of crime in western nations, seeing that many of those acts which with us are referable to crime or mental alienation are, among the orientals, to be considered as attached to mistaken notions of morals and religion, or as originating in peculiar legislative enactments."

Says the Abbè Huc, in his "Chinese Empire": "It is almost impossible to imagine the readiness with which the Chinese commit suicide. It requires only the merest trifle or a word to induce him to hang or drown himself, these being the two kinds of suicide most in favor. In other countries, when

a man wishes to avenge himself on his enemy, he tries to kill him: in China he kills himself. There are various reasons for this. In the first place, the Chinese government holds the person responsible for the crime of suicide who gave the offense which caused it. It follows, from this, that if any one wishes to avenge himself on his enemy, he has but to kill himself to work him the direst woe. He falls into the hands of the executive, who at least torture and ruin him, if not take his life. The family of the suicide, likewise, generally obtains large pecuniary compensation; and it is not rare to see wretched beings, who are devoted to their families, go and deliberately commit suicide at the house of some rich person."

This pagan custom of self-immolation, through family devotion, has its counterpart and following even in civilized and Christianized nations. What life-insurance man does not know that many insured persons have unquestionably done the same thing, relying on rich insurance companies to provide for their families? This was not exactly the feeling of the western gambler who, notwithstanding his clear apprehension of the question, declined to play at a game where he had to die to beat the bank.

The spirit of self-sacrifice for the benefit of those we love is as old as history and as fresh as to-day. Curtius plunged into the yawning gulf to save his country. The pelican, which picks its own bleeding breast to nourish its savage young, has been adopted as the symbol and the title of one of the largest life offices of Great Britain; and who shall speak other than reverently of that played-out rake who took his own life, after liberally insuring it, in order, as he said, to enable his widow to start again with a younger and more vigorous man?

Hecker records, in his "Epidemics," that in the fourteenth century, during an epidemic of persecution of the Jews on the false and preposterous charge that they had poisoned the air and all the springs and wells, the poor Israelites deliberately immolated themselves by thousands. In some places they fired their own quarter of the town, and so perished. At Strassburg, two thousand were burnt alive in their own burial-ground. "At Esslingen, the whole Jewish community burned themselves in their synagogue; and mothers were seen throw-

ing their own children on the pile, to prevent their being baptized, and then precipitating themselves into the flames." If ever there were the marks of deliberation and sanity attendant upon suicide, these cases present them.

Hegesias, a stoic philosopher of the time of the Ptolemies, gained the title of "The Orator of Death," from the eloquence with which he preached contempt of life and the blessings of death. "So intense," says Lecky, "was the fascination he cast around the tomb, that his disciples embraced with rapture the consequences of his doctrine; multitudes freed themselves by suicide from the troubles of the world." The fashion at last attained so perilous an extension that Ptolemy had to banish the philosopher from Alexandria.

Among the examples given by Dr. Winslow, in his "Anatomy of Suicide," and Esquirol, in his essay on suicide in the "*Dictionnaire des Sciences Medicales*," are some which most conclusively show that while an individual may be prompted by a mesmeric sensitiveness, or other cause, to fall in with an epidemic of suicide, yet the act is wholly controllable. For instance, "The ladies of Miletus committed suicide in great numbers, because their husbands and lovers were detained by the wars. At one time there was an epidemic of drowning among the women of Lyons. They could assign no cause for this singular tendency; it was checked by the order that all who drowned themselves should be publicly exposed in the market-place. That at Miletus was stopped by a similar device. The ladies generally hung themselves, and the magistrates ordered that in every future case the body should be dragged naked through the town by the ropes employed for the purpose. *There were no more suicides;* the apprehensions of an outraged modesty were quite sufficient to check the suicidal epidemic, thus proving that it had been a mere stupid fashion, all the time controllable."

In the Veda, the scriptures of the Brahminic religion, the act is not mentioned, but for ages it has received a distinct ecclesiastical endorsement in the approval of suttee, or widow-burning, and the blessings bestowed by the Brahmins upon those who have taken their own lives in what they regard as a good cause.

"If we wish to understand the religions of the ancient nations of the world," says Prof. Max Muller, "we must take into account their national character. Nations who value life so little as the Hindoos and some of the American and Malay nations, could not feel the same horror of human sacrifices, for instance, which would be felt by a Jew; and the voluntary death of the widow would inspire her nearest relations with no other feeling than that of compassion and regret at seeing a young bride follow her husband into a distant land. She herself would feel that, in following her husband into death, she was only doing what every other widow would do; she was only doing her duty.

"In India, where men in the prime of life throw themselves under the car of Juggernaut to be crushed to death by the idol they believe in; where the plaintiff who cannot get redress starves himself to death at the door of his judge; where the philosopher who thinks he has learned all which this world can teach him, and who longs for absorption into the Deity, quietly steps into the Ganges. In such a country, however, much as we may condemn these practices, we must be on our guard, and not judge the strange religions of such strange creatures according to our own more sober code of morality.

"Let a man once be impressed with a belief that this life is but a prison, and that he has but to break through its walls in order to breathe the fresh and pure air of a higher life; let him once consider it cowardice to shrink from this act, and a proof of courage and of a firm faith in God to rush back to that eternal source whence he came; and let those views be countenanced by a whole nation, sanctioned by priests, and hallowed by poets, and however we may blame and loathe the custom of religious suicides, we shall be bound to confess that to such a man, and to whole nations of such men, the most cruel rites will have a very different meaning from what they would have to us. . . . They contain a religious element, and presuppose a belief in immortality, and an indifference with regard to worldly pleasures, which, if directed in a different channel, might produce martyrs and heroes."

Thus, this master scholar shows us that not merely is suicide

among the orientals no evidence of insanity, but it is not even a crime.

His experience for the past five years in the investigation of cases of suicide has forced upon his mind the conclusion *that but a comparatively small number of suicides even in this country is attributable to insanity.*

While it may not be proper to say that suicide, like the small-pox or yellow-fever, is a disease, nor that it is contagious, yet there are times and states, so to speak, of the social atmosphere when the propensity seems to prevail to a most alarming extent—when man's attachment to life ceases, when shadows seem to pass over the bright side of his existence, when hopes of happiness or fortune are blighted, when misfortunes seem to multiply and become insurmountable, when life seems to have proved a total failure, when pride and ambition have been blasted, it is then the wicked thought enters the mind that death is preferable to such a life, and the sad result, deliberately, intelligently and ingeniously planned, follows. It is not insanity, but a deliberate purpose to escape ills which to the suicide seem overwhelming, and which he has not the fortitude to bear.

The speaker had in his mind several practical illustrations to sustain this theory coming under his personal observation. For instance, during the last year a gentleman of culture and intelligence became bankrupt and attempted suicide by means of chloroform and morphine, and endeavored to foist the crime of attempted murder and burglary on some unknown housebreaker. His life was insured for $50,000 and most of his policies were of recent date. Also the case of Captain Colvocoresses, in 1872, at Bridgeport, Ct., whose life was insured for $195,500 in twenty different companies, and the policies were of recent date. Also of Monroe Snyder, of Bethlehem, Pa., insured for $65,000.

With a healthy and intelligent public opinion, continued the Judge, no such fraud could be consummated, nor outrage perpetrated. He asked to be excused for speaking strongly on this subject. In his judgment this demoralization of public sentiment is a disgrace to our civilization. While the sentiment which moves us to protect the weak against the strong

is worthy of the highest commendation, a feeling that ennobles human nature when intelligently exercised, he had no sympathy with its abuse and perversion, nor should it be permitted, at this day and age, to encourage the perpetration of crimes or legalize frauds.

In conclusion, he remarked that the importance of the question is becoming more and more manifest. The suicidal mania is spreading beyond all precedent, and it becomes the duty of the moralist, the philanthropist, and the statesman to study the subject. The barriers to self-destruction seem to be giving way. The great protection in a society like ours, with its high elevations and deep depressions, has been in the profound religious conviction of our people that suicide is a pronounced sin, abhorrent to Christianity and severely denounced in the Word of God. In this age of free-thinking scientific investigation and universal criticism, one by one the great truths contained in the Bible, the corner-stone of our religious system, are assailed or doubted, and the faith which has been handed down from generation to generation questioned. The consequences are inevitable. Destroy the faith of men in the Bible and the great truths it teaches, remove the restraints of religion *and teach annihilation*, and you will reap, *without the aid of insanity*, a harvest of suicides that will astonish the world.

Prof. Hammond thought that most physicians would agree with the learned author, except in the one point, that he expresses the opinion that physicians generally are of the opinion that suicide is an evidence of insanity. He has found it quite the contrary. The vast majority of medical writers hold that suicide is no evidence of insanity. He has had an opportunity of ascertaining the fact that a great many sane men at some period of their lives have contemplated suicide. He thought that many would admit that the same shadow has passed across their own mind. Suicide is the result sometimes of bad physical health. He had known cases where attempts at suicide were made because of nocturnal emissions—fearing that they would lose their venereal powers—a thing that some men value more than brains.

The learned author has given some examples of the fact

that suicide is epidemic. He thought all present could bear him out that it is not only epidemic but more or less under the influence of example. As in the case of those who followed the example of Dr. Wells in cutting their femoral artery.

Dr. John H. Packard, of Philadelphia, read a paper "On the mode of infliction of the Death Penalty." The following is a full synopsis:

Much as Christianity and modern civilization have done for human nature, they have not so far refined and purified it as to warrant the abolition of the death-penalty for the crime of murder. At least, such seems to be the plain inference from the current history of our time. Without dwelling upon the daily reports of violence and ruffianism, certainly not growing less numerous or milder in form, he would mention the fact that a London telegram of March 14, says that a bill to abolish capital punishment was on that day put on its second reading in the British House of Commons, and defeated by a vote of 263 to 64.

Another item within a day or two was to the effect that the death-penalty was to be restored to the statute books of the State of Iowa, the experiment of dispensing with it having proved a failure.

He was well aware that much has been and may be said on what seems to be the humanitarian side of this question, which indeed is too large to be entered into here, even if it came within the purpose of the present paper, or if he felt himself capable of handling it. He however called attention to the fact that in the Mosaic history the Divine edict—"Whoso sheddeth man's blood, by man shall his blood be shed"*—was given as a distinct and separate utterance, anterior to any system of temporary or ceremonial law, and without any hint of limitation by time or by circumstances which might arise in the later history of the race.†

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* Genesis, chap. ix: 6.

† A recent number of an English periodical, (its name having escaped his memory, and he has failed to find it by such search as his time has permitted,) contained a very forcible *resumé* of the existing reasons for the retention of the death-penalty.

As matter of fact, in almost all civilized communities, it is ordained that the deliberate taking of human life shall involve the forfeiture of the life of the offender; the object being to prevent crime by the fear of punishment.

But there are two entirely different stand-points from which to view this matter. One is that taken by the degraded ruffian, who looks upon law and order as oppressive shackles, to be resisted whenever there is a chance of doing so successfully; who regards his being as a sort of struggle with society, and if in this warfare he is brought to the gallows, accepts his defeat as he would the loss of a prize-fight. He is overpowered by superior odds, but not conquered; and if he "dies game," he is a hero in the estimation of his compeers. The other is the point taken by the moral and self-respecting part of the community. For them the punishment is in the condemnation; in the ignominy of forfeiture of the right to live.

These two points, although thus stated separately, are extremes between which there are various grades; and the phrase "the fear of punishment" expresses the combination of the two which operates to preserve the security of life in communities.

Of course the mere terror of physical death is combined with the double aspect of capital punishment just mentioned, to a greater or less degree, in very many minds. In some it would swallow up all considerations of defiance of society, of disgrace, or of distress to relations, etc. But this is an element which is not, and cannot be recognized in any legal view of the matter. If it were not so—if the pain of physical death has anything to do with the deterrent effect of capital punishment,—then it would be better to restore all the fearful list of tortures, burning, breaking on the wheel, and the rest.

He thought it might be stated positively, without fear of contradiction, that just so far as the physical terrors of execution are enhanced, the moral force of the procedure is weakened. This is true in regard to its effect upon even the lowest and most brutal classes of society. It is the calm, dispassionate, inexorable working of retributive justice which holds them in check, so far as fear of punishment can do so.

The subject of the modes of carrying out the extreme sen-

tence of the law was brought before this society some years ago by Dr. Alonzo Calkins, in an able paper entitled "Judicial Executions for Capital Crimes."* The ground taken by him was adverse to the method now in use in this country and in Great Britain; and his forcible exposition of its objectionable features left nothing for the lecturer to say in regard to this portion of the topic. His views must be shared by all in whom the finer sentiments of humanity have not been blunted by actual contact with cruelty and vice.

While, however, he had nothing to say as to the mere process of death by hanging, there are some matters connected with it to which he would allude.

The strong feeling which exists in many minds against this mode of judicial death often adds to the difficulty of procuring convictions even upon very positive evidences of guilt. It also adds very much to the force of the sympathy which is apt to be developed for criminals under sentence, and increases the frequency of pardons, by which the salutary effect of the law is seriously lessened. Under this head may be mentioned the facilitating of the abuse of the pardoning power, for political or private reasons; since this would be less likely to occur, if it did not fall in with public sentiment.

(There is a curious usage in the State of Pennsylvania, and perhaps in other States also, by which, if the Governor in whose term a convicted murderer is sentenced, goes out of office without signing the death-warrant, his successors likewise abstain from doing so. A man was recently liberated, who was sentenced twenty-six years ago, but the sentence was never carried out for the reason just stated. So far as he can learn, it is mere usage, without any sanction by legal enactment.)

Between hanging and the other modes of execution at present in use in civilized countries, there is really no choice. They are all alike in affording an opportunity for bravado on the part of the criminal, and in the appearance at least of unseemly violence on the part of the officers of the law.

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*Published in the *Quarterly Journal of Psychological Med.* for April, 1869. Also see Transactions of the Society, Vol. I.

The ignominy of death on the gallows has always been regarded as one of its chief terrors; yet the body only is affected by it, and the stigma is merely conventional. Popular clamor, or partisan hatred, may condemn a pure-minded patriot to this so-called shameful ending, and yet succeeding generations will only the more honor and cherish his memory. The infamy comes from the judicial character of the act; from the fact that the man himself, by reason of his crimes, is declared by his fellow-men to be unworthy of existence among them.

Some of our State Legislatures have followed the example of the British Government, in ordaining that all executions shall be carried out *in private*; but in many parts of the country the spectacle of "hanging" is still allowed to have its brutalizing effect upon all who choose to come to see it.

Within forty-eight hours of the writing of these lines, the telegraph informs us, the hanging of a colored man for murder, in a town in Tennessee, was witnessed by ten thousand persons. And in the paper of this morning occurs the following item:

"'Gus. Johnson was hanged at Atlanta, Ga., yesterday, in the presence of a large crowd, for the murder of a colored ferryman. He showed brutal indifference, confessed that he had committed four murders, and ascended the scaffold with a cigar in his mouth."

He makes no comments upon these two statements, except to record his solemn belief that the mere reading of those few lines which constitute the second one has already sown additional seed of evil in the minds of thousands of the young and half-educated, all over this country, which will come up with the rest in the great crop of crime.

But even where executions are supposed to be private, they are so only in name. They are indeed, carried on *within the walls* of a prison; but persons manage to get admission as deputy sheriffs, or on some other pretense, and at every such scene *the reporters of the daily press* are present in force, as if by right, to vie with each other in giving graphic and—to use the detestable word of the day—"sensational" accounts of the miserable affair. By their agency the public, young and

old, far and near, are enabled to gloat over details which cannot but be loathsome to the better part of human nature; which have a fearful fascination for the *young* and inexperienced, and which have unquestionably been widely influential in promoting crime.

He appealed to all to whom these words may come, whether they are not conscious of the harmful effects of such reading upon themselves.

Now in view of the general admission that the present mode of inflicting the extreme penalty of the law is objectionable, an apology can hardly be needed for an *attempt to point out a better way.*

What he would suggest is that there should be fitted up in the jail a small room, which can be made *air tight.* By means of very simple arrangements carbonic oxide can be substituted for the air of this room; and a person confined in it would thus die the easiest and quietest death known to science. He would simply cease to exist.

A jury should be appointed to identify the convicted person. In their presence he should be conducted to the room, and placed in it, entering by a sliding door, of plate-glass it may be, protected by a netting of iron wire. The process of rendering the air irrespirable should then be begun, and would very soon be completed. In less than ten minutes fresh air might be introduced, by opening the sliding door and reversing the former process, and the same jury, again identifying the body, could certify to the fact of the execution having been duly carried out.

In 1874, Mr. Coleman Sellers suggested the adoption of this carbonic oxide poisoning as the method of killing the dogs taken up in the city of Philadelphia and unredeemed; and it has been found to answer the purpose perfectly. By the kindness of Mr. Marett, the Superintendent of the Dog Shelter, he has inspected the process more than once. At his last visit he carefully timed its duration, and found that of forty-three dogs of various sizes, introduced into the room, the air being pure, every one had ceased to breathe three minutes after the gas began to flow. Two dogs, weighing about 45 pounds each, were put in singly, and the time was exactly the same for both,—110 seconds.

More exact details of the method can readily be furnished if desired. It is essentially the same as that within a few years so often adopted by suicides in France. To his own mind it seems that it presents many advantages on the score of humanity, propriety, and efficiency.

Dogs killed in this way manifest scarcely any evidence of suffering, not more than he has seen in inducing anæsthesia by ether, in other dogs, for the purpose of surgical operations. The death would necessarily be private. Probably there would be little if any struggling to be witnessed. As to the certainty of the extinction of life, there can be no question, for reasons known to every tyro in physiology. Between the sickly sentimentality which would spare merciless murderers, and the brutal ferocity which would exult over their dying agonies, there seems to be a just and wise medium, where the law can take its stand, vindicating itself, protecting society, and yet inflicting no needless torture on the unhappy criminal. The subject is one which well merits earnest thought, and his only excuse for presuming to deal with it is the duty which lies on every man to contribute his mite to the subduing of the evil that is in the world.

THE NEUROLOGICAL SOCIETY.

Dr. Shaw, of Brooklyn, read a paper entitled "A Contribution to the Pathological Anatomy of Disseminated Cerebro-Spinal Sclerosis," the joint labor of himself, Prof. E. C. Seguin and Dr. Van Derveer, of Albany, N. Y.

The cases quoted presented the usual clinical history of disseminated sclerosis. The brain and two spinal cords were examined. Lesions were found in various regions of the cord and brain in the gray as well as the white substance. Histological examination showed more or less disappearance of ganglion cells of the regions involved in the sclerosis; disappearance of the myelinic sheath of the nerves, with a preservation of the axis cylinders in an advanced stage of the morbid process.

The cells of the neuroglia showed increase of their protoplasm and enlargement of their nuclei, multiplication of their nuclei and numerous processes from the protoplasm. These

spider-like cells almost entirely filled the field, and were surrounded by coarse, loosely arranged neuroglia fibres.

The spider-like cells observed are similar to those observed by others in general paralysis, myelitis, &c.

Dr. Shaw demonstrated some peculiar markings of medullatia nerve fibres. The specimens which had been freshly stained with hyperosmic acid exhibited peculiar fissures extending obliquely through the myelin but not passing beyond it. In the discussion which followed, Drs. E. C. Seguin and Shaw expressed their opinion that the markings in question were of an artificial nature.

Dr. Seesel stated that he considered these markings as natural but Ranvier's rings as artificial.

Dr. Seguin demonstrated an unique case of facial paralysis (the patient being present). The patient, a boy, æt. about 14 years, had suffered from chronic otorrhœa for a long period and had lost by caries, a part of the temporal bone—including the cochlea and semicircular canals. Previous to this, he had complete facial paralysis, on the left side. One remarkable feature of the case was that as Dr. Seguin stated, after such a severe affection the nerve should regain its function. He had the year before shown all the characteristic reactions of traumatic nerve paralysis—yet at the present time could voluntarily move the left facial muscles to a slight extent. Dr. Seguin also referred to another remarkable feature of the case —namely, that notwithstanding the total destruction of the semicircular canals on one side, there were no symptoms of Meniere's disease.

THE MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Dr. John C. Peters read a paper on the Therapeutic uses of some of the Ranunculaceæ, especially in nervous diseases, which appears in previous pages of this number of the JOURNAL.

The following remarks were made in discussion:

Dr. Piffard said that not being in general practice, but confining himself to his specialty, he had little occasion to employ the drugs mentioned by Dr. Peters, with the exception of Pulsatilla nigricans. Since the rehabilitation of this drug by

Stoerck, in 1771, it had maintained its position in most of the continental pharmacopeias, though not in those of England or this country. The plant contains two active principals, *anemonin* and *anemonic* acid. The American variety of the plant *P. nuttaliana* had been found by Mr. Miller, a Philadelphia pharmacist, to contain them also. He could only obtain them, however, from the fresh, but not from the dry plant. Conium, gelsemium and some others are likewise known to be more active in the fresh than in the dry condition. Dr. Piffard stated that he had found pulsatilla exceedingly useful in epididymitis. From an experience covering about a dozen cases treated during the year with pulsatilla exclusively (tincture of the fresh plant), he thought that the average duration of the disease was reduced nearly if not quite one-half. The first and most marked effect was on the pain. Under the use of pulsatilla this rapidly subsided, a marked diminution occurring in some cases after the second or third dose. Dr. Piffard believed that most of the ext. pulsatilla found in the shops was comparatively inert.

Geo. Henry Fox, M. D., has used the tincture of pulsatilla of late in the treatment of a score or more cases of epididymitis, and has been surprised at the result. He has given small and frequently repeated doses, and in the majority of cases a speedy diminution or complete cessation of the pain has followed. Thinking at first that the improvement might have been due to rest or to the natural subsidence of the inflammation, he has repeatedly tested the remedy, as follows: When patient has taken the pulsatilla for a day or two and reported a marked improvement, he has substituted plain alcohol for the tincture (without informing the patient of the change), and has noticed the pain come and go, or at least appear modified in intensity, according as the alcohol or the pulsatilla was being taken. To those who do not obtain very brilliant results from tobacco-poulticing, strapping, etc., he would heartily recommend the use of this remedy.

Dr. Putnam Jacobi remarked that two suggestions occurred to her in connection with Dr. Peters' paper. The first concerned the theory of the mechanism by which podophyllin and other resinous cathartics, promoted the excretion of the

bile, and even seemed to increase the quantity secreted. The theory quoted and endorsed by Dr. Peters, that these drugs acted by irritating the duodenal extremity of the ductus choledochus was often advanced, but seemed to the speaker entirely destitute of proof. It was to be remembered that all mucous surfaces were connected in a reflex manner with both muscular and glandular tissues.

That is, impressions made on the sensory nerves of a mucous membrane, after transmission by central ganglia, were reflected back along motor fibres, partly to the muscular coat overlying the membrane, partly to the glands imbedded in it. The first destination was of course easily susceptible of anatomical demonstration; the second had, so far as the speaker knew, only received anatomical demonstration in the case of the salivary glands, where nervous fibrillæ had been traced into the protoplasm of the gland cells. But the physiological proofs of such nervous connection abounded. Further, that the fibrillæ supplying glands were motor, as in the case of the chorda tympani for the submaxillary, was also extremely probable.

Now it was of course well known that, philosophically speaking, the liver and pancreas stood in exactly the same relation to the digestive tract as the microscopic glands imbedded in the mucous membrane. These larger glands were merely diverticula from that membrane. It was therefore to be presumed that their secretion was regulated in the same way, namely, by means of impressions primarily made upon the mucous membrane, then reflected to the secreting cells of the glands. In the case of the liver, irritation of the duodenum would be transmitted to the hepatic territory of the solar plexus, thence reflected along the fibres of the hepatic plexus, following the blood vessels to the liver. It was, in the speaker's opinion, probable that the hyperæmia of the liver and biliary passages which so often accompanied gastro-duodenal catarrhs, was caused in this reflex manner, and not by spread of inflammation by contiguity along the ductus choledochus. The same reasoning would apply to all cases generally alleged in favor of the common theory, since all ducts, whose extremities are supposed to be irritated, open upon mucous

membranes, the natural recipients of sensory impressions, the internal tegmenta of the body.

It was to be presumed, in accordance with this theory, that podophyllin was particularly efficacious in causing a flow of bile, because its irritating effect was experienced as soon as it entered the duodenum; while aloes, which seemed to act only upon the colon and rectum, probably underwent some change during its passage through the small intestine, after which only did it become possessed of irritating properties.

The speaker wished to make one remark in regard to aconite. She had recently had occasion, in repeating Boeck's experiments on cardiac poisons, to witness the remarkable appearances assumed by the heart of the frog after three doses of aconite. It did not, as was sometimes carelessly asserted, slacken the pulse by paralyzing the vagus, but by direct paralysis, after transient stimulus, of excited motor ganglia of the heart. The ventricle therefore gradually lost its power of contractility, and became greatly distended, but with red blood, which continued to pour into it from the lungs. The appearance was so peculiar and characteristic that any one familiarized with it could at once pronounce upon the nature of the poison, from seeing the heart of a frog exposed.

Now it was noticeable that the therapeutical effects derived from aconite were not derived from this physiological action, which indeed stood in their way. In such inflammations as pneumonia and pericarditis, where paralysis of the heart was the great danger to be feared, the speaker much doubted, in spite of assertions to the contrary, whether aconite could, with impunity, be given in large enough doses to produce an important effect. But the unquestionable and remarkable influence of aconite upon acute amygdalitis and other superficial inflammations of mucous surfaces, seemed to be due, not to any effect produced on the cardiac ganglia, but to the analogous sedative action on the peripheric extremities of sensitive nerves, which was equally characteristic of the drug. The speaker called attention to this fact as an illustration of the danger of too hastily concluding at a therapeutical result from a physiological action. Few drugs produced but one appreciable effect; and the problem always presented itself to de-

cide, which, among the multiple actions demonstrated in the laboratory, might be called into play to useful purpose at the bedside.

George M. Beard, M. D., read a paper entitled "Hygiene in Chronic Nervous Diseases," before the Medical Society of the County of Kings (Brooklyn, N. Y.), of which we give the following synopsis. He said:

Of the three influences that relieve and cure chronic nervous diseases—*medicine*, *mind* and *hygiene* or environment, the first is oftentimes of the least importance. He considered the hygiene of this class of diseases under three heads—*cerebral* hygiene, *digestive* hygiene and *sexual* hygiene. In their hygienic relations functional nervous diseases may be divided into three classes:

1. Those that could be cured by a simple change of life—indoor to out-door—with vigorous muscular exercise, or travel and no medicine.

2. Those that needed a moderate degree of exercise, and various medical treatment.

3. Those that require *absolute* and more or less prolonged rest in bed.

Under this latter head come many cases of so-named spinal and cerebral irritation, hysteria and neurasthenia, and choreic and allied maladies. The test of skill in the treatment and management of functional nervous diseases is to determine to which of these three classes any case belongs. Mistakes have often been made by giving the first the treatment of the third class, and *vice versa*.

Patients in the second class and those that combine moderate exercise in their accustomed business with various treatment, need to be cautioned always not to over-do—to stop short of severe fatigue, and should have the mental counter-irritation that is called mental diversion.

When patients of the third class are kept in bed they should be fed liberally and very often—a little at a time, and should have general faradization and central galvanization, and also, if possible, systematized manipulation or massage. Long ago Doctor Beard had observed that many nervous patients were better on Sundays than on other days—could digest more food and

were less distressed. It is well to give some patients of this third class sixty or ninety or more consecutive Sabbaths. His experience coincided with that of Dr. Weir Mitchell in regard to the increase of weight under this treatment. It is not always necessary to use massage, and exclusion from friends, though an advantage, was not always imperative. In old times, when this bed-treatment was more used than now, it was much abused—was advised for cases of the first and second class, and was combined with starvation and bleeding instead of feeding. Among the great existing causes of functional nervous disease is *financial worry*. For cases of the second class, successful and happy intellectual labor is beneficial to the tired brain, just as exercise is to the weakened muscle.

In all these cases, the prognosis much depends on the intellectual character of the patient. An exclusively vegetable and fish diet, including oysters, milk and eggs is of value in some cases of these disorders. It does not feed the intellect, as Agassiz suggested, but has a calming and sedative influence, like bromide of potassium. Animal food infuses energy, but these patients need to be slowed down.

Speaking of hygiene he said: Among the many departments of hygiene, the study of which has been systematically neglected by the profession, is that of the function of generation and of the sexes in their relation to each other. In relation to chronic nervous diseases, sexual hygiene is of all importance. In acute disorders it has little direct influence and therefore medical science has passed it by. The inductive investigation of the subject is a very difficult and delicate matter, and is complicated with elements of error without number. The practical conclusions on this subject that seem to be required are these:

1. In savage, barbarous and semi-civilized lands the sexual appetite can be, and is, and always has been indulged in by both sexes, not only in the natural way, but in all sorts of unnatural ways to enormous excess without traceable harm to the nervous system. To a less degree this is true of the lower orders in civilized lands—as slaves, sailors and peasantry.

2. The brain-working, in-door living classes of civilization find it necessary to observe the same cautions in this respect as

in regard to diet. They can bear only a share of what to the savage or the slave is a matter of indifference.

3. There are individual idiosyncrasies in this regard. Some who are very feeble can bear much sexual indulgence, just as some who cannot raise their heads in bed or take any stimulants or tonics, can eat and digest large quantities of food, or bear any amount of alcohol or electricity.

4. Sexual intercourse is a tonic and sedative, and like other tonics and sedatives, it induces sleep, or at least quietness, and increases the disposition and capacity for work. When carried to relative excess, that is excess for the individual at the time, it may produce primary nervousness and wakefulness and secondarily, headache, neuralgia, and various symptoms of exhaustion. The appetite for sex, like the appetite for food, cannot well be regulated by arithmetic: but whenever any of the symptoms follow indulgence, they suggest excess at the time. At another time, under different circumstances, the same indulgence for the same individual may do no harm.

5. The evil effects of over-persistent sexual excess are usually temporary and recoverable. The German notion that structural and incurable disorders, as locomotor ataxy and progressive muscular atrophy are caused primarily by sexual excess, is not sustained by Dr. Beard's observation. Functional excess of any kind causes functional, not structural disease. Just as excessive brain work, uncomplicated with worry, may cause cerebrasthenia, or brain exhaustion, just as excessive eating may cause a tonic dyspepsia, just so excessive sexual indulgence may cause sexual exhaustion either local or general; but none of these types of functional debility are usually of a permanent character; removing or modifying the cause, in time removes or modifies the effect. Almost all healthy children over-eat; almost all healthy men at times over-use their brain and sexual organs; but only a few are permanently harmed thereby. He referred to the general law to which there are exceptions now and then.

He was once consulted by a poor, miserable, sad, middle-aged, debilitated man who had evidently become so by a long continued indulgence in the habit of masturbation; but in his case and indeed in all similar cases, the objective evil of his

habit had been reinforced by the subjective evil of dwelling upon and magnifying it. Proper advice and treatment, and sexual regime including marriage, would have cured even him and may even yet do so.

It would appear that the organ of thought, the organs of digestion and the organs of generation can bear and were designed to bear, a vast amount of abuse without permanent injury; were it not so, the human race would disappear from the earth.

In regard to the relative harmlessness of the natural and unnatural methods of sexual excitement, it may be said that masturbation is more likely to injure for these two reasons: First, it can be practiced in early years—even in childhood; and secondly, it can be practiced at any time and alone and therefore more frequently. These two causes more probably than the difference in the acts themselves (although there is *per se* an important difference) allows for the temporary or permanent debility that follows long continued masturbation.

Likewise involuntary emissions at night occurring now and then, are physiological—not injurious; but when occurring frequently, as a large number of times a month—and it is dreaded and worried over by the victim, they produce exhaustion.

Under this head he would say, also, that there is no question but that an important proportion of cases of functional nervous diseases in both sexes are brought on or are made worse by sexual excess and disturbance as a rule. This applies to the age between fifteen and forty-five, when the sexual system is most active and most liable to abuse.

6. Excessive sexual indulgence or abuse, acting on a *strong* constitution, produces *local* functional disorders of the sexual organs—impotence in its various grades; acting upon a nervous, delicate constitution, it produces general nervous exhaustion.

The worst cases of impotence the Doctor has ever seen have been in men of "iron frames." The feeble, finely organized constitution cannot abuse itself long enough to produce impotence, the consequence of excess *so soon* shows itself upon the general system, that it is impossible to produce local disease.

In writers' cramp—a malady which he is now studying especially—the same law holds. Writers' cramp is a disease of the comparatively strong; the feeble and nervous cannot write hard enough or long enough to get the disease; fatigue of the system warns them in time to stop short in time and take it easy.

Thus far he has been talking of the chronic nervous disorders of adults—in children, however, some of the same general principles apply. The infantile nervous system differs somewhat from that of the adult in its susceptibility. Young children for example do not have neuralgia or sick headache or hysteria. They can bear chloroform without harm, and also electricity in larger doses than adults. Both of the latter points are important.

He also referred to the influence of music as a hygienic agent, which he has quite recently had an opportunity of observing. In co operation with the celebrated pianist, Mr. Patterson, and Dr. Wilson, he had made some experiments in the hygienic or remedial influence of music on the insane. The results were interesting, and in some cases remarkable—and would suggest that other forms of nervous disease less severe than the insanity of the asylums—as melancholia, mental depression attending various disorders, cerebral congestion, and anæmia and hysteria—might be advantageously treated in the same way—at home and among their friends.

With the insane at Blackwell's Island, the experiments made these conclusions probable:

1. Instrumental music exercises on melancholia and mania at least temporary beneficial effects—varying with the nature of the music and the temperament of the patient. The savagely violent sometimes become pleasant and calm; the melancholic and stolid may begin to weep or become cheerful and chatty. An overdose, like an overdose of any other hygienic or remedial agent, may have injurious effects. Some who are at once made worse by noisy music may be calmed by a music of a sad and quiet character.

2. It is not impossible that the regular and continuous use of music for an hour or so a day or less frequently, might have a permanent influence over violent and unmanageable cases.

3. It is probable that in the higher classes of society, among the finely organized and intellectual, the results of systematic musical treatment would be more satisfactory than with the lower classes of foreigners or those upon whom experiments were made.

The hygienic value of pleasant sights has long been recognized and practiced.

The following was received separately from Dr. Wells' letter:

At the meeting of the Neurological Society, March 4th, Dr. E. C. Spitzka read a paper on the Scientific Study of Psychiatry, which elicited the following remarks:

After the reading of Dr. Spitzka's paper, Dr. Hammond arose and moved that a committee be appointed to examine into the question and to report on the feasibility of carrying out Dr. Spitzka's suggestions. He stated that the subject was too important to be disposed of in the usual manner.

The President therefore appointed as such a committee Drs. Hammond, J. C. Peters (President of the County Medical Society) and J. C. Shaw to report at the next meeting.

He then called upon a representative of the American Association of Medical Superintendents, there present, to speak.

Dr. A. E. Macdonald, Medical Superintendent of the City Asylum for the Insane, stated that he could not discuss the subject that evening, but would do so if the paper were accepted by the Society.

The President remarked that the question before the Society was not whether the paper was to be accepted or not, it was consequently open to discussion like any other paper, as it contained scientific matter outside of its specifications.

Dr. Kiernan being called upon, stated that with regard to most of the points in the paper he was not at liberty to deliver an opinion, and that not being a Superintendent, he did not feel that any points in the paper were directed against himself. He could confirm many of the writer's statements, especially as he had himself had the honor of conducting Superintendents through the asylum at which he served, who did not know what progressive paresis was.

Dr. Wilbur, Superintendent of the Asylum for Idiots, at Syracuse, was called upon and said:

Medical statistics in relation to insane asylums are of direct interest to every member of the profession. Attention is therefore called to some that appear in the report of the Utica Insane Hospital for the year 1877.

The main portion of the report of the superintendent of that institution is a plea for the immediate completion of the three expensive hospitals for the insane now in process of construction. The nature of the argument used may be gathered from the following passages:

"It is painful to reflect that the provision made by the State for the early reception of all cases of insanity is still so limited. Probably it is not an over-estimate to say that one-half of those who become insane each year are allowed to pass into the chronic stages of the disease, without any attempt at proper treatment. The two great impediments in the way are limited accommodations, the distance traversed and expense incurred in reaching asylums."

An extract is then given from a paper of Dr. Jarvis, published in 1866, which shows that "the number of patients committed, gradually and steadily diminishes with the distance from the hospital." It is then added—"The facts and conclusions of that paper have been verified in the experience of this State year by year, with fatal consequences to individuals and immense detriment to the public finances. This may be seen in the vast number of chronic and incurable insane accumulating in the poor-houses and chronic asylums of the State.

"A large number of the counties, especially those remote from asylums, illustrate the unwise delay in providing for the acute cases. Take for instance Chautauqua county, which had in its county-house, in 1868, only thirty chronic insane; according to the report of the Board of State Charities, Nov. 1, 1876, she had 83, yet in all these years Chautauqua county sent to the Utica asylum but 14 patients, only five of which were returned to the county-house. This would leave an increase of 48, in addition to those that had died during eight years.

"Oneida county, in 1868, had 105 chronic insane, and had only increased the number, Nov. 1, 1876, to 167, and during

these eight years sent to this asylum no less than 256 patients. Thus Chautauqua county increased her insane 176⅔ per cent., while Oneida county increased only 59 per cent."

Enough has been cited to show the tenor of the argument—which is, that insanity in its early stages is a very curable disease; in its later stages intractable. That if insane hospitals were so multiplied as to be within easy reach from all parts of the State, chronic insanity would be greatly reduced, with immense advantage to the public finances. The corroborative evidence is furnished by the comparative statistics of Chautauqua and Oneida counties. The first, cut off from the privileges of the Utica Hospital by its remoteness, is now suffering the consequences in the alarming percentage of increase in the number of chronic insane supported at public charge. The other, blessed in its proximity to the same institution and availing itself largely of its privileges, has had but a moderate increase in the number of its chronic insane.

First of the argument; the fallacy of which has been frequently exposed. It may briefly be stated:

A certain but small proportion of the insane become so, suddenly. The symptoms in these cases are so marked, so violent, and so demanding an immediate committal to an asylum, that they are sent at once. Of this class, it may be said that, as a rule, they get well or die at an early stage of the disease.

On the other hand, with the majority of those who become insane, the disease comes on gradually and insidiously, and becomes seated before any marked change or outbreak prompts the friends to send them to an asylum. This form of insanity is not a curable disease, and so, however much hospitals are multiplied, recourse will not be had to them for the patient's relief till too late for cure. It is chronic mania from the start or from the first observation.

Now for the statistics given in this report in support of the argument.

An intelligent and honest search for the facts in the case would not have contented itself with comparing the percentage of increase of chronic insanity in the county poor-houses, in only two counties, *for a single* year. That would be—so

to speak—generalizing from a single fact, or rather half a fact; for only a portion of the chronic insane are in the county-houses.

There are other comparisons that suggest themselves and are readily made.

Let us supply the defect.

The following table is made up from the reports of the Board of State Charities:

NO. OF CHRONIC INSANE IN COUNTY POOR-HOUSES.

YEAR.	CHAUTAUQUA CO.	ONEIDA CO.
1868	30	105
1869	57	97
1870	62	120
1871	56	135
1872	68	132
1873	71	140
1874	62	141
1875	63	163
1876	83	167

It will be seen by this table, that if the year 1869 had been chosen, the percentage of increase in chronic insane in seven years, would have stood, Chautauqua Co., 46 per cent., and Oneida Co., 72 per cent.

The explanation of the relative position of the two counties in 1868, is this, and it depends upon a fact well known at Utica. In 1862 Oneida county built a new and comfortable county insane asylum, and all the chronic insane previously in custody of friends were sent there before 1868.

Chautauqua county took the same steps in 1867. And then a similar result followed, as is seen by the sudden increase of patients from 30 to 57.

The report of the Board of State Charities for 1868 also mentions that of the 105 patients in the Oneida county asylum *all but eight* had received treatment at Utica.

Take another table made up from the report of the Board of State Charities:

Number of chronic insane supported at public charge in proportion to the whole population: Oneida Co., one to 683;

Chautauqua Co., one to 782; the whole State, except New York and Brooklyn, one to 1168.

But a previous report of the Utica asylum furnishes us with the materials for a still wider generalization or a still better comparison. In that, there is a table, showing the extent to which the different counties have sent patients to the Utica asylum. From this I have selected 16 counties, containing a population of 883,067, that have sent patients in the highest proportion; namely, one to every 4,860 of the population.

I have also taken 13 others with a population of 734,712 that have sent the least number; that is, one to every 17,078 of the population. In other words, the former district has sent proportionately between three and four times as many patients to Utica as the latter.

In the first district, or near by and availing itself freely of the Utica asylum, the percentage of increase of chronic insane from 1868 to 1876 was over 75 per cent. In the remoter district the same percentage was only 45 per cent.

Comparing this Utica district with the remoter district and with the State at large—except New York City and Brooklyn—in another respect; that is, the ratio of chronic insane to the population, it reads thus:

Oneida county, one to 683; near-by counties, one to 1055; remote counties, one to every 1305; State at large, one to every 1168.

It is not my object to show that no more hospital accommodations for the insane, in this State, are needed, but that the argument for such increase presented in the Utica report is a fallacious one; and the statistics given utterly unreliable. To make a point, the statistics of the Board of State Charities are entirely perverted.

In conclusion, it may be added that the multiplication of insane hospitals will not materially reduce the number of chronic insane. The hope of essential relief in that direction must be based upon a profounder social philosophy.

Dr. Eastman, of the Worcester asylum, a man of large experience in the treatment of insanity, after a careful analysis of the statistics of several asylums, has expressed a much more reliable opinion, in the following language:

"These careful investigations lead to the conclusion, that of the whole number of cases of insanity less than one-half are really cured, many of which relapse; and it is extremely doubtful if under the most propitious circumstances any possibility exists of increasing the proportion of recoveries much above fifty per cent.!"

Dr. Earle, of the Northampton asylum, sets his foot down with still more emphasis upon such specious expectations of recovery, by saying substantially, "that if there were an insane hospital upon every hill-top, of every county, it would not materially alter the amount of chronic insanity in the land."

Parties in the incipient stages of the most common form of insanity will not have recourse to such institutions, till there is some modification in their prevailing aspect and administration. Now restraint is stamped upon every part and portion of the system; the high enclosure, the guarded gate, the grated windows, the locked doors, the isolation from friends and the complete dependence upon the caprice of the medical superintendent. In other words, these features of duress and isolation must be softened. Facility of commitment and release must be equally provided. Advantage must be taken of special medical experience and skill, from outside, for the varied complications of other diseases with the predominating malady. And over all the details of the inner life of such institutions, must shine the rays of an efficient and searching governmental supervision.

Dr. Seguin, the President, then stated that he had expected to discuss the paper in the midst of a general discussion. He could strongly endorse the position taken by Dr. Spitzka, his paper was ordinary as to discussion, but extraordinary as to merit. He had several years previous been present at a meeting of the Asylum Association, and had noticed that no scientific papers or discussions there occurred, in fact there was absolutely no provision for scientific contributions made. In the preamble of their Constitution, we look in vain for the word "Science." He thought that some credit was due to one asylum which, whatever might be the questionable character of the work there done, had at least the merit of having introduced systematic pathological examinations, but that the work of the special patholo-

gist was not acknowledged to be his work, but was published under the name of the Superintendent. He thought that Dr. Spitzka had been unjust, if his remarks applied to assistant physicians at asylums, as these gentlemen to his knowledge were overworked with routine duties, such as correspondence, etc., as well as surgical and general medical work. They were also either poorly or not at all salaried, so that they could not provide themselves with the requisite instruments for diagnosis and research, which Dr. Spitzka states they should provide themselves with. As to the Superintendents themselves, he thought that they, having delegated their duties to the juniors, had plenty of leisure. In fact, in one instance, the Superintendent not only absented himself from his asylum to attend to medico-legal cases and other cases of insanity, but also to general nervous diseases, in a consultation practice extending over the whole State.

Dr. Spitzka remarked that nothing was further from his thoughts than an intention to criticize the assistant physicians of asylums. He fully appreciated their position, correctly described by Dr. Seguin. With regard to the State Asylum, whose Superintendent claims to have inaugurated pathological researches, first in America, if not in the world! he would say that the work was not commenced by the present special pathologist; in fact a Western Superintendent, once an assistant in that institution, raises this claim; but it seems that the first researches, several of which were valuable, while others remained unfinished owing to the resignation of the investigator, were undertaken by Dr. Hun, of Albany.

MEDICO-LEGAL SOCIETY.

After the reading of Dr. Spitzka's paper, a lively discussion followed, in the course of which the debaters, Drs. Hammond, Hannan, and Mr. Riddle strongly sustained the position of Dr. Spitzka, and in accordance with a unanimously carried motion, a committee was appointed, with powers to memorialize the Legislature regarding the alleged abuses, and instructed to append a copy of Dr. Spitzka's paper to the memorial in question.

Reviews and Bibliographical Notices.

I—LEWES: THE PHYSICAL BASIS OF MIND.

THE PHYSICAL BASIS OF MIND. With Illustrations. Being the Second Series of Problems of Life and Mind. By George Henry Lewes. Boston, 1877. 556 pages.

In glancing at the title of this work we were led at once to recall the famous lecture of Prof. Huxley, on the "Physical basis of Life," and all the more so, seeing that Mr. Lewes employs much the same arguments, at least in the preliminary portions of his book, which were used in that lecture.

This volume, while it may be considered as a separate work, is really the latest one of an indefinite series of volumes, the first two having been published under the title—"Problems of Life and Mind." These earlier volumes were composed of separate essays, having the appearance, rather than the fact, of organic connection or arrangement, upon the whole rather tiresome and prolix, and difficult to subordinate to the plain signification of title which had been given them.

Mr. Lewes has been long known as a somewhat brilliant essayist, as a writer in a rather lively and epigrammatic style, as a gentleman of wide culture, possessing strong but rather erratic sympathies, and as having made the laudable endeavor in his culture to secure a ready passport to the scientific, philosophical, and literary domains as contrasted. Apparently he has tried with a set purpose to wipe out the reproach of being a mere metaphysician, or a mere devotee of science, or of literature in general. Has he not written a popular exposition of Comte's "Philosophie Positive," and best of all a "Biographical History of Philosophy," not to mention other productions from the philosophical side? Then has he not produced a charming "Physiology of Common Life," and numerous essays, besides what is contained in the three volumes alluded to in the opening of this notice? Then, finally, has he not given us a notable "Life of Goethe," and manifested other unmistakable evidences of a lively sympathy with the tides and subtle movements in the wide field of general literature?

But to the medical man he is known, to a certain extent, as a writer on physiological subjects. It would seem, therefore, that as possessing both the literary ability and philosophical and physiological information, he was well fitted to the task of stating at

least clearly and concisely, the materialistic or semi-materialistic side of the discussion of the relations of mind and matter. But to those who look for such a statement, we are of the opinion that the book will be a serious disappointment. The argument is certainly far from concise, there are endless repetitions and digressions, and the outcome of the whole is not always easy, at first sight, to define.

The first part of the book, comprising rather more than one-fourth of its contents, is given to the discussion of the very general biological question of the "nature of life." In the language of Mr. Lewes, this section "deals with the specialty of organic phenomena as distinguished from the inorganic." Every problem of mind, he says, is also a problem of life relating to a special group of vital properties, hence the propriety of defining the meaning of the term life before discussing the nature of mental phenomena. Is this definition possible in the sense that would seem to be implied in the title of this section?

Mr. Lewes, here as elsewhere in his more recent writings, classes himself as an "organicist," as distinguished from a vitalist, an animist, or a materialist. The exact signification and justice of this term with these restrictions is not altogether clear. If we understand the author, he, as an organicist, recognizes the peculiar phenomena of vitality as exhibited in the organism, does not attempt to explain them in accordance with physical or chemical laws, but still rejects any assumption of an extra material principle. How far this latter course agrees with a rejection of a modified materialism may be questioned, but we will follow his own language. After stating that there are two diametrically opposed conceptions of vitality, the metaphysiological or extra-organic, that vitality is a vital principle or a force, and what he calls the physiological or organic conception, which he himself adopts, he says, "The conception of an entity must be rejected, because it is metempirical and unverifiable." Starting from this stand-point nearly everything must be rejected, since all our knowledge rests on original assumptions, and we cannot see after reading the author's book that he himself adopts the reasonable course, according to this view, of universal skepticism. And while we cannot, with our present capacities and means of knowledge, state what this assumed vital principle is, yet reasoning by exclusion, we can be sure that it is not one of the ordinary dynamic agencies amenable to physical investigation, which we see elsewhere at work in nature. Mr. Lewes' remarks immediately following the sentence we have quoted, are as follows: "The conception of a force must be rejected because it is irreconcilable with any definite idea we have of force. What the term force signifies in physics and chemistry, namely, mass animated by velocity or *directed pressure*, which is the activity of the agent, is precisely what these vitalists pertinaciously exclude. They assume a force which has nothing in common with mass and velocity; which is not a *resultant, but*

a principle; which instead of being a *directed quantity* is itself autonomous and *directive*, shaping matter into organization, and endowing it with powers not assignable to matter. If this vital force has any mass at its back, it is a spiritual mass; if it is directed, the direction issues from a 'mind somewhere.' Now this conception is purely metempirical." We can fully coincide with what our author says as to the application of the term force; force, properly speaking, is a word not applicable here. The forces at work in organisms are the ordinary forces of nature, but there is something else in living organisms which baffles all definition in terms of physical science, that is not correlated with any known force, can be indefinitely extended, and is completely lost at death. And though it is never found except in connection with organization, there is no reasoning which appears to us conclusive that it is derived from it, and the presumption seems to be altogether in favor of the reverse being the case. The bold statement, therefore, that the conception of a vital principle is purely metempirical seems to us a very poor way of disposing of the something with which we have here to deal. Every theory that we can devise to account for some of the most familiar physiological facts in this connection, such, for example, as the possibilities contained in the ovum or spermatozoon, must necessarily be purely metempirical. Inference has its place, even in this department of science, and when by diagnosis by exclusion, we have only determined what vitality is not, it is not so unreasonable to admit the alternative, though it may not be otherwise demonstrable. The famous Huxleyan comparison of vitality with aquosity is not a good one; we can account for the properties of water or any other compound, by the use of the theory of atoms, the metempirical character of which is very little objected to; but this is not so convenient to explain at once all the phenomena of organisms in which matter is held in so many different chemical combinations and physical conditions, to say nothing of vital processes and complex orders and arrangements.

Coleridge has said, "To account for life is one thing; to explain life another." In the first we have to state its antecedents, its cause; in the second, in the sense in which he used the term, we have only to reduce it to "its simplest and most comprehensive form or mode of action." The title of this section, "The nature of life," would seem to imply both of these, certainly a sufficiently ambitious task. How does Mr. Lewes accomplish it? He quotes a large number of definitions by various authors, ending with the one he most favors, that of Herbert Spencer, that "Life is the continuous adjustment of internal relations to external relations," which for all the words imply may as well refer to a cooling globe as to an organism, and this he supplements with the following: "Life is the functional activity of an organism in relation to its medium, as a synthesis of three terms: structure, aliment and instrument; it is the sum of the functions which are

the resultants of vitality; vitality being the sum of the properties of matter in the state of organization." That is, in simple terms, life is the functional activity of the organism and vitality is the sequent of organization. In a subsequent chapter this latter idea is still further maintained, and the author criticizes Mr. Spencer, who was inclined to admit that function might precede structure. We are obliged to again quote our author's own language. "Mr. Spencer, as I have already suggested, seems to have been led into his view by not keeping distinctly present to his mind the differences between properties of tissue and function, the activity of an organ. 'That function takes precedence of structure,' he says, 'seems implied in the definition of life. If life consists of inner actions so adjusted as to balance outer actions—if the actions are the *substance* of life, while the adjustment constitutes its *form*; then may we not say that the actions formed must come before that which formed them—that the continuous change which is the basis of function must come before the structure which brings the functions into shape?' The separation of actions formed from that which forms them is inadmissible. An action cannot come *before* the agent; it is the agent in act. The continuous change, which is the basis of vitality, is a change of molecular arrangements; and the organ which gives a special *direction* to the vital activity, e. g., which shapes the property of contractility into the function of prehension, this organ must itself be formed before it can manifest this function. It is true that in one sense the organs are formed by, or are differentiated in, a pre-existent organism; true that the general activity of living substance must precede the special activity of any organ, as the expansion of steam must precede any steam engine action; but the general activity depends on the general structure, and the special actions on the special structure. If by organization we are to understand not simply organized substance, but a more or less complex arrangement of that substance into separate organs, the question is tantamount to asking whether the simplest animals and plants have life. And to ask the question, whether life precedes organic substance, is tantamount to asking whether the convex aspect of a curve precedes the concave; or, whether the motions of a body precede the body! To disengage ourselves from the complicated suggestions of such a word as life, let us consider one of the vital phenomena, contraction. This is a phenomenon manifested by simple protoplasm, and by the highly differentiated form of protoplasm known as muscle. In one sense it would be correct to say that contractility as a general property of tissue, precedes contraction, which is specialized in muscle. But it would be absurd to say that *muscular* contraction preceded the existence of muscle, and formed it. The contractions of the protoplasm are not the same as muscular contractions, any more than the hand of a baby is the same as a man's; the general property which both have in common depends on the substance both have

in common, the special property which belongs to the muscle depends on its special structure. An infinite activity of the contractile protoplasm would be incompetent to form a muscle, unless it were accompanied by that peculiar change in structure which constitutes muscle. The teakettle might boil forever without producing a steam engine or the actions of a steam engine. That which is true of one function is true of all functions, and true of life, which is the sum of vital activities." (p. 85.)

We reproduce this long passage for the reason that it gives as concisely and clearly as is anywhere else given in as many consecutive sentences, the author's position in regard to the mutual relations of vitality and organization, and, as such, since we are examining his book, it is worthy of a careful analysis. In the first place the view here stated, as we understand it, is hardly consistent with the language of the general definition of life and vitality which was quoted as given in a preceding chapter. If life is only a sum of the functions which are the resultants of vitality, and the latter is the sum of the properties of organized matter, then according to this definition some dead matter has life, for it is not specified that the properties must be vital properties,—to make such a distinction would emasculate the definition by leaving the word "vital" still unexplained. Of course such a conclusion cannot be admitted, and the definition is therefore faulty. But, if organized matter can exist for a time and under certain conditions without vitality, and consequently without functions, this fact is certainly not in favor of the theory that life proceeds from organization, for according to that, organization ought to be always attended by vitality.

Again, to ask the question whether the simplest animals and plants have life, might be positively reasonable, according to this definition, since, according to Mr. Lewes' distinction of properties and functions, assimilation, evolution (?), sensibility and contractility, being classed among the former (p. 87), they can scarcely be said to have any functions; and therefore while they possess organization and vitality, they have no functions and therefore no life. So much for some of the defects of the definition.

In the second place, we cannot see either the strength or the consistency of the author's argument from the phenomenon of contraction. While, of course, muscular contraction requires the existence of muscle, as indeed every specialized function requires its special organ, and the word "muscular" necessarily implies the existence of muscle, the illustration is a very inapt one in many respects. Muscular contraction is only the most specialized form of contraction, and in spite of Mr. Lewes' denials, we hold that the contractions of simple tissues are not necessarily different from those of muscle. If the contractility of the protoplasm is utilized for a definite purpose and is not merely a substantial quality, it is functionally identical with the muscular contraction. To say that an infinite activity of con-

tractile protoplasm is incompetent to form a muscle unless accompanied by the peculiar change in structure which constitutes muscle, is to state a truism, absolutely irrelevant to the question. If, however, there is anything in natural selection and evolution, which our author only ignores when it is convenient, this activity of protoplasm has very much to do with producing this change of structure. Most of all, however, we object to the assumption that organization precedes life, by Mr. Lewes, because it is an assumption, and any argument to make it otherwise must necessarily, in our opinion, be reasoning in a circle. And as an assumption it is a purely metempirical one, which he, of all writers, ought to be least guilty of admitting.

There are several other special matters deserving notice in this section of the volume, such for example, as the author's use of terms, some rash general statements, etc., but we must pass them by. The general criticisms will be made when we come to speak of the book as a whole. The second division now claims our attention.

This section, entitled "The Nervous Mechanism," is devoted to what the author styles "the objective analysis of the conditions of mental processes," or, in other words, to the statement and support of his views regarding the physiology of the nervous system, with special reference to the phenomena of mind. This is the portion of the book in which we would look for the most of value in the way of information and the interpretation of established facts. Mr. Lewes has some claims to be considered as a physiologist, and these are here certainly made as prominent as possible. The principal of these claims are the invention of the term *neurility*, and the substitution for what he calls "the superstition of the nerve-cell" of a hypothesis to us not any more reasonable, and lacking even the necessary evidences to entitle it to consideration as opposed to the long accepted views. As these are the main features to be noticed in this section of the book, we shall try to examine them somewhat in detail. First as to *neurility*. This term was proposed by Mr. Lewes in 1859, to express the peculiar vital property of nerves. As the invention of a new name is a very small matter, unless it involves and expresses a new idea, it is needful to see how far this word *neurility* meets these conditions. As here stated the word is applied only to the peripheral nervous system, that is, as we understand, to the nerves proper and not to the ganglionic cells or larger collections of cells, and it is the corresponding neural property to contractility in the muscle. In another place it is defined as "the propagation of molecular change," and elsewhere as the "property of transmitting stimulation." The special property of the centres is said to be sensibility. Now confining this term *neurility* to the nerves alone, and adopting our author's own definition of it, we see no particular difference in its signification from that of the term *conductibility*, as applied to nerve fibres, from certainly a very early period in physiolog-

ical investigations. Its advantages are in being more special and perhaps more convenient, but it has the disadvantage of not conveying at once any definite idea of its meaning, in this respect being far inferior to contractility or conductibility, which need no translation. We see nothing therefore in the conception implied in this term that is especially novel, but we have no doubt that it will be generally accepted as expressing better than any other single word the vital property of conductibility in nerves and nothing else. That it will immortalize its author other than in a very small way, we doubt, and we hardly appreciate his motives in placing the stress upon it that he does; there is nothing that we see that justifies it, excepting the fact, upon which he felicitates himself, that it has received the sanction of adoption by so eminent an authority in physiology as Professor Vulpian.

In the most general sense in which the word may be used, applying it to the whole nervous system, it implies all nervous action, and is to us too vague and extensive in its signification to be a very valuable addition to our physiological vocabulary. For in spite of what Mr. Lewes says as to the artificial nature of the separation, we cannot admit with him an identity of properties between the nerve centres and the nerve fibres. And this brings us naturally to the second innovation in nerve physiology proposed by our author in his tilt against "the superstition of the nerve cell." The fact of the retardation of nervous impulse by nerve cells, however we may interpret it, certainly indicates a difference in the conductibility, otherwise the neurility, of the cell and the fibre. If this is not so, what is the use of the cell, and why do we have these two varieties of nervous tissue? Then we have the facts demonstrated many years since by Schiff, that a nerve may lose its ability to react to direct external stimulation, while still responding freely to that incited from its centres in a reflex way, which would seem to indicate a decided difference in degree, at least, between the two, of capacity to react to stimuli. Mr. Lewes certainly accepts what seems to us the correct view as to the conduction of nerves, that it is indifferently in either direction, and that their function depends solely on their terminal apparatuses, but what does he make of the central terminal mechanism? It would seem, according to his view, that centres are not essential to nerve action, that a fibre might start from a tactile corpuscle, for example, and terminate in a muscle, and that without the intervention of any centre whatever, a peripheral irritation of the skin might cause contraction of the muscle, and *vice versa*. To suppose this condition of affairs in, at least, the higher organisms, would be an example of "imaginary anatomy" worse than any of that which he so freely condemns. These are only a few of the objections that occur to us to the notions of nerve physiology advanced in the book before us; many more might be adduced, and of the arguments brought forward in defence of the author's views not one is con-

clusive, nor are the interpretations of the facts on which they are based trustworthy, or the facts themselves altogether incontestable. Indeed, Mr. Lewes has a way of adopting rash generalizations and is now and then guilty of involuntary inaccuracies of statement which makes us distrust his competency for the task he has given himself. As illustrations of these peculiarities we may mention his apparent acceptance of the view that the motor nerve fibre blends directly with the muscular tissue, one which has no support whatever from the latest and best investigations, and such statements as that on page 302, in which it is assumed that the gray substance of the cord is alone essential to motor and sensory transmission, or that on page 235, where Erb and Westphal are quoted as saying that reflex movements follow gentle percussion of the *patella*. His reading appears to be very extensive, but he is very far from being always judicious or judicial in the references he brings forward to support his views—it might be considered too gratuitous an assumption, to suppose him ignorant of many important investigations, the results of which do not especially favor his own hypotheses.

We are therefore not particularly alarmed at the revolutionary ideas of our author in nerve physiology, for we cannot perceive that he has made out his case as well as seems to be his own opinion. As an innovator he has to sustain the burden of proof, and even were his views the ones generally accepted, the facts, as we see them, would incline us to favor opinions not identical with them. To recapitulate. Mr. Lewes holds that identity of tissue everywhere indicates identity of property. Nerve tissue, i. e., the essential part of it, apart from its investments, being everywhere the same, its properties must be the same everywhere, hence he recognizes no difference in the special property of the nerve cell and the nerve fibre, that of both being included under one head, that of neurility. He admits, however, the existence of nerve centres, and for convenience, not as a real distinction, he changes the name of their neurility to sensibility. He admits that nerves conduct indifferently in either direction, their functions being determined by their terminal apparatuses, apparently, in all this, ignoring the fact that the central terminal apparatus is necessarily a nervous one, which, being of the same tissue as the nerves themselves, ought therefore not to possess any special property to affect their function. Indeed, with this distinction which the author makes between function and property, we cannot conceive in what way the former is ever derived from the latter as he says it is, nor do we find in his work any explanation that is at all satisfactory, and notwithstanding the stress he lays upon this distinction, it seems to us there is a curious confusion, if not of the ideas, at least of the terms which represent them, and at all events, a lack of adequate definition. We have already tried to ascertain from the author's words the meaning of his term neurility, and concluded from the most specially expressed definitions, that it was about equivalent to conductibility, but we

find it transformed to sensibility, which "on its subjective side is sentience" (p. 220). Again it is said, "sensibility or sentience is the abstract term which expresses all possible varieties of sensation." Still again, the author says that "in the normal organism sensibility means feeling, or consciousness" (p. 402), hence, according to this extension of terms, neurility includes consciousness. This may be a criticism of language rather than of ideas, but it is perfectly justifiable, when, as in the present case, we can, through a series of terms used convertibly, reduce consciousness to conductibility.

These two points of *neurility* and the revolutionary ideas concerning the nerve cell are the ones most strikingly put in this part of the work. We trust that our estimate of them is sufficiently expressed in what has been already said. As to the first, the stress laid upon it by Mr. Lewes is our only excuse for giving any extended notice, and we can say but little more as regards the second, for it will, in our opinion, hardly win for itself much detailed consideration among physiologists. With our present knowledge of the nervous system, it is only surprising that so well informed a man as Mr. Lewes should be inclined to advance and support it.

In the two remaining sections of this work, the author takes up the subjects of animal automatism and the reflex theory. The general argument may be stated to be an attempt to carry the domain of psychology over the whole nervous system, following out after a fashion an idea, that in other ways has been somewhat developed by Pflueger and several other physiologists. Indeed this is a natural inference from what we have already noticed of his views; if he regards consciousness as the same as sensibility, or only an aspect or special mode of the same, and sensibility as holding the same relation to neurility, which in its general sense may be called a universal property of nerve tissue, then, of course, mind is likewise universally distributed throughout the nervous system. But the admission that consciousness is or may be a special mode of sentience confined to the brain, is in our opinion as good as giving away his case, for in whatever way we make our classification, or draw the line, the special character of consciousness is the vital point.

That such an unsatisfactory method of explaining away such a fact as consciousness, or rather such a substitution of a phrase for an argument, can be so satisfactory to Mr. Lewes, indicates a decided defect in his judgment, or, what is more likely, a predisposition to accept any summary disposal of an inconvenient fact.

There are many statements and opinions in these portions of the work with which we can agree, but we are very far from being able (though we think ourselves free from all prejudice in regard to the matter) to accept his general conclusions, as offered here, not, indeed, any more than we were those of the preceding sections. Our disagreement with the work, therefore, is a general one; the author belongs to a section of a school of philoso-

phy, to which, no matter how eminent and numerous its followers may be, we could never reconcile ourselves. The book before us is the result of much thought tinged throughout with the philosophical prepossessions of the author, and exhibiting his incapacity, as it appears to us, to correctly appreciate, in all respects, the subject of the relations of the mind and body. He seems to us to ignore completely a whole class of mental facts, or what we positively believe to be such; indeed, he would deny altogether the existence of what we may call our spiritual capacities, which though not directly dependent upon our external sensations, we believe to be as truly included among the faculties related to cerebral functions as are intellection and memory. That he is not without an occasional inkling of these we may perhaps believe on his own testimony* given elsewhere, where he relates how once he underwent a temporary conversion to a spiritualistic view of matter and mind. As an illustration of an idiosyncrasy of the author and of his spasmodic sensibility in this direction, we may quote his own words. He says,† "Although my tone of thought is profoundly opposed to spiritualism, I can consistently say that no effort has been wanting on my part to seek out its strongest arguments in the works of all the great teachers. Indeed, there was one brief period when I was very near a conversion. The idea of a noumenal mind, as something distinct from mental phenomena—a something diffused through the organism, giving unity to consciousness, very different from the unity of a machine, flashed upon me one morning with a sudden and novel force, quite unlike the shadowy vagueness with which it had heretofore been conceived. For some minutes I was motionless in a rapt state of thrilled surprise. I seemed standing at the entrance of a new path, leading to new issues, with a vast horizon. The convictions of a life seemed tottering. A tremulous eagerness, suffused with the keen light of discovery, yet mingled with cross-lights and hesitations, stirred me, and from that moment I have understood something of sudden conversions."

The idea of a noumenal mind does not strike the ordinary or even the usual type of extraordinary thinker with the suddenness and novel force above described in such graphic terms, and it indicates a rather peculiar mental constitution when we hear of the "convictions of a lifetime" tottering from the effects of such explosive cerebration as that described. It will help, it appears to us, to account for many other peculiarities and inconsistencies in our author's later writings, some of which we have noticed in this review.

Mr. Lewes goes on to say, in the context of the paragraph quoted from the *Fortnightly Review*, that the intense thought which followed this event seriously affected his health; that he ransacked the authorities and gradually returned to his old stand-

* *Fortnightly Review*, April, 1876.
† l. c.

point. We will quote his own words again. He says: "I saw that the distinction between a noumenal mind and mental phenomena was a purely logical distinction, transformed into a real distinction; it was the separation of an abstraction from its concretes, such as we make when we separate the abstraction substance from concrete qualities, and this separation, effected logically, we erect into a real distinction by substantializing the abstraction which is then supposed to precede and produce the concretes from which it is raised. The noumenal mind had thus no more warrant than a machine principle apart from all machines, or a vital principle apart from all vital phenomena."

Of course we differ with the above, but have neither the time nor space, nor yet the inclination to enter into the metaphysical discussion it involves. The two passages illustrate the normal tendency of the author's "cerebration" and its impulsive aberrations in other directions. Its lack of proper regulation is noticeable all through the present work—the frequent repetitions, the following out of cross suggestions, and the stress laid upon comparatively unimportant matter and the strenuous advocacy of unsupportable theories, which we have noticed, are all in evidence of this fact. Mr. Lewes is a suggestive thinker, but the suggestiveness of his line of thought runs away with him, and we often have as the result a very diluted and occasionally tedious argument.

In conclusion, we have to say, that the "Physical Basis of Mind" is as unexplained after the reading of the work, as it was before, since we cannot admit Mr. Lewes' assumption that, because the phenomena of mind are never seen except in connection with organized matter, they must be considered as caused by it.

II.—THE CONFERENCE OF CHARITIES.

Proceedings of the Conference of Charities—Held in connection with the general meeting of the American Social Science Association, at Saratoga, September, 1877. Boston, Mass.: A. Williams & Co. 1877. 170 pages.

This is the fourth annual report of a body formally consisting of the State Boards of Charity which may be represented in it, but largely made up of delegates from other boards, associations and institutions of a kindred character. Its proceedings have gained in interest and importance with each succeeding year, and such parts as deal with insanity and the insane may properly receive our consideration.

The model upon which State Boards of Charity in this country have been formed, is that of the English Commissioners in Lunacy. To its high intelligence and ability, together with an

entire independence of party and class interest, the acknowledged superiority of the English system of provision for the insane is, no doubt, chiefly due. This is now admitted not only by the public, but by the medical profession, which, as may be supposed, did not at first relish the intrusion of legal and lay elements into what was regarded as the proper sphere of medical men. The enlightened and exhaustive reports of the *Lancet* Commission, lately republished from the *Lancet*, in two large octavo volumes, bear the following testimony:

"Nothing has impressed us more forcibly during the present inquiry, so far as it has yet been carried, than the permanent and increasing value of the service rendered to the cause of humanity, and indirectly to the interests of medical science, by the Commissioners in Lunacy." Yet the legal powers of these commissioners are as limited as those of our State Boards of Charities. They can do little but advise, and remonstrate against abuses, personally and through their reports. How is it, then, that they have been able to accomplish so much for the insane of England, while the efforts of our State Boards, with members perhaps equally intelligent and philanthropic, are only just beginning to be felt, at the end of from ten to fifteen years? This question is answered by the able writer of the reports just mentioned. "The commissioners can only recommend," he says, "and the manner in which their recommendations are too commonly resisted, shows clearly that unless they were backed by an irresistible power their labors would be ineffectual. *The unseen force is public opinion.* The publication of their reports is a vast power for good, and to the steady pressure of this agency the success of the work in progress is mainly due."

In this country State Boards, or Commissioners of Charities, exist in nine States. The oldest is the Massachusetts Board, established in 1863. It is composed of six members, and two of those first appointed, Hon. F. B. Sanborn and Dr. Nathan Allen, are still leading and active in its duties. To them, and to the brilliant and philanthropic Dr. S. G. Howe, for many years its president, this board owes in great part its present practical control over the charities of the State, and its influence as a model to other States.

We have observed that the establishment of this form of supervision of insane asylums in England was not unopposed. "The College of Physicians," says the *Lancet* Commissioner, "had an abundant opportunity of displaying its zeal and sagacity for the protection of the insane, and the advancement of this department of medical science and practice; but it neglected the opportunity, and discreditably failed in its duty to the public and the profession alike."

This fact in the history of lunacy reform in England, thirty years ago, is now repeated in the persistent efforts of the association of Medical Superintendents of Insane Asylums to prevent the creation of Boards of Charities by our State governments.

Not only has this opposition been expressed in elaborate arguments and formal resolutions at their annual meetings, but the members of the association in the several States have not failed to labor personally for this end, with legislatures and executives. By such means, and through the influence of a journal published mainly at the expense of the State of New York, a member of the association was able to delay several years the creation of a Board of Charities in that State. Such a board, the second in the United States, was, however, finally established in 1867, and was welcomed in the report of the Utica Asylum for that year with the assurance that by "the aid of the series of propositions of the association of medical superintendents of institutions for the insane on the construction and organization of asylums, it could suggest such improvements in county-houses and receptacles as would greatly ameliorate the condition of the inmates."

But the success which has attended the efforts of asylum officials in New York to confine the labors of the State Board to the insane in county-houses and receptacles, can be fully explained only in the light of the universal corruption in public affairs, which has prevailed down to a very recent date. In the *personnel* of the State Board at its establishment, New York was equally fortunate with Massachusetts. No better appointments could have been made than those of Chancellor Pruyn, President Anderson, and Judge Dwight. But the absolute control of the State by thieves, under the leadership of the infamous Tweed, quickly followed, and an asylum "ring" was able to maintain its claim, by the aid of other corrupt rings, to a large share of plunder. It is an open secret that the two asylums, authorized at Buffalo and Poughkeepsie, were sops flung by Tweed to politicians who could not be directly bought with money. Yet they were as much a portion of the public spoil as the appropriations for the New York court house, or for "repairs and improvements" on the canals, prisons, and asylums already built. Not less than five millions will be required to complete these new asylums, which were designed for 400 patients each, at an estimated cost for both of $1,600,000. The palatial character of their designs had been condemned by all authorities in asylum-construction, and even the medical experts who led the lobbying and log-rolling for them, had recorded themselves in favor of small and inexpensive buildings for the insane. The opposition of the Board of Charities was met by a movement to abolish it, which was nearly successful, and was defeated in the legislature with the greatest difficulty by the friends of reform. In 1870, a check was given to the board by the addition to it, *ex-officio*, of three State officers, these the pliant tools of the canal ring. Under the reform administrations of Governors Tilden and Robinson, this device has happily turned to be the plague of its inventors. Another attempt of this kind was made in 1873, by the creation of a commissioner in lunacy, who should supersede the board in its functions towards the insane. This was successful, and the

office, with a large salary, was given to a non-expert whose relations to the asylum ring were apparently of a friendly character. We shall see, further on, how well this office has served its purpose to the ring as a buffer against the Board of Charities, and how it has been used to set forth doubtful facts and arguments in aid of their designs upon the State treasury.

We must now return to the report before us, but as the two papers which it belongs to us to notice are both by distinguished New York specialists, our view of the field of insanity must still be confined chiefly to that State.

The first of these papers, "On Insanity," was prepared by Dr. John B. Chapin, medical superintendent of the Willard Asylum, at Ovid, N. Y., as chairman of a committee appointed at a previous meeting to report upon that subject. Hence it has not the free range of a personal essay, although the limits of orthodox doctrine in American psychiatry are not seldom transgressed, as we should expect from one who has dared to achieve success in the building and administration of an asylum for the chronic insane. He attacks at the outset the problem of the nature of insanity, and its relations to brain disease. Upon this subject his views seem to us much more in accordance with sound doctrine than those of the Utica school of pathologists, to which he is opposed, and we shall follow him in a brief examination of it.

In his chapter on the pathology of insanity, in a work which holds the highest place as an authority, Dr. Bucknill has written as follows: *

"The widely differing opinions which have been entertained by the ablest physicians respecting the pathology of insanity, clearly show that there is some difficulty at the bottom of the question, greater than that which has existed with regard to the nature of other classes of disease. The source of this difficulty is not hard to find. A rational pathology must ever be founded upon the basis of physiology. It is, indeed, a kind of physiology; it is an account of the abnormalities of organization and of function, which as much depends on the natural laws of our being as do those of health."

In a treatise for professional reading written at the present time, the language above quoted would seem trite and superfluous. No medical man can now forget that health and disease are merely relative terms, imperfectly descriptive of facts in one science, that of physiology. But when the chapter from which it is taken was first published, some thirty years ago, it was necessary to correct a misapprehension common among medical men, and particularly among those devoted to the treatment of the insane. The pretended science of cerebral physiology, under the name of phrenology, had indeed become thoroughly discredited at that time. No recognized authority in medicine conceded its claim to explain the relations between mind and matter in

* Bucknill and Tuke on Insanity, p. 341.

health. But its terms and theories had not yet been banished from the study of cerebral pathology. That they still linger to such a degree as to render wholly worthless the observations and experience of some whose position affords them the best opportunities for that study, appears from the arguments which Dr. Chapin thinks it necessary to urge, and the fact can hardly have escaped the notice of any one at all familiar with asylum reports, and other current writings of lunacy specialists. Whether this is mainly due to the unprogressive tendencies of a narrow specialty, or to the debasing influence of politics, from which, unfortunately, our insane asylums are not yet free, cannot now be considered. Certain it is that the grossest fallacies in cerebral physiology lie at the very foundations of the doctrines of cerebral pathology at which Dr Chapin's criticism is aimed. As these doctrines are supported by studies made at the Utica asylum, on the authority of the State and at its expense, they must be examined without regard to private and personal considerations. We may say, also, that though nominally proceeding from Dr. Gray, the superintendent, it is not easy to suppose him personally responsible for such aberrations from primary scientific theory as abound in the publications of that asylum.

This point we may illustrate, in passing, by reference to a paper bearing his name in the *Journal of Insanity* for January, 1877. It is entitled, "Pathological Researches," and is in the form of a summary of the contributions made to the pathology of insanity by ten years of study, under State patronage. In fact, it is an attempt to describe those degenerative changes in the brain which the microscope, in the hands of distinguished foreign observers, chiefly German, have lately revealed to us. The writer has certainly failed to follow the German observers in his descriptions, whether from lack of familiarity with the German language, or the English, or with his subject, is not clear. But this is of little consequence. The appearances attempted to be described are found equally in the brains of the sane and the insane, and, of course, have no relation whatever to insanity. What we wish to notice is the following, from the opening paragraph of the essay:

"It is impossible to define the pathology of insanity in distinct terms, as it is still a subject of investigation. The boundary of our knowledge is not only limited, in this field, but, as any one will find who will take occasion to read the subject up, the nature of the changes are (*sic*) still not satisfactorily solved. Indeed, those changes are just what we are investigating, in the hope of substituting positive knowledge, by examination of the actual lesions, for the various speculative theories." It would not perhaps be hypercritical to remark, that if "the boundary of our knowledge is limited," then surely our knowledge itself is not limited. With this understanding only does the apposition of thought marked by the word "but" in the sentence become intelligible.

To gain "a positive knowledge" of disordered mental actions by examining "the actual lesions of the brain," is however the hope of Dr. Gray, even if the language be not his own. Eight years ago, at his request, a law was passed by the legislature, authorizing the addition of a special pathologist to the medical staff of the Utica asylum, for the purpose of making investigations into the pathology of insanity. This law is still in force, and the expenditures under it during the year covered by the last annual report, amount *to more than five thousand dollars.* It was proposed, first, to ascertain by post-mortem examination of the brains of insane persons, "whether there are specific changes in the brain in insanity."* At the end of eight years no decided progress is reported, and we may reasonably suppose it has been abandoned. It is now more than twenty years since one of the greatest of the physiological psychologists, Griesinger, was forced to admit that the question, whether there is any specific alteration in insanity, "must not only be answered in the negative, but considered as *a priori* false."† Mental pathology has certainly not returned since this was written, to the theories of the phrenologists, and we can imagine no excuse for holding out the hope of discovering that which the first principles of science at the present day forbid us to suppose exists.

Dr. Gray speaks explicitly of the somatic phenomena referred to as "the morbid processes denominated insanity," and proposes to "solve the problem of mental disease under the microscope." As his physiological doctrines must be one with those of his pathology, he would then describe sane thought and feeling as "the healthy cerebral processes denominated sanity." By the same rule, he would also undertake to solve the problem of healthy mental action under the microscope. Now, that mental action and molecular changes in the brain are identical, or even of kindred origin, is, of course, wholly inconceivable. That there is some definite relation between them is an accepted scientific theory, but one which it is utterly impossible, in the nature of things, to demonstrate. This is not denied by a single authority, even among physiologists of the most materialistic school at the present day. If the ultimate molecules of the brain were not several thousand times too small for our microscopes, still we should be unable to understand their relation to the facts of consciousness.

Another question proposed by Dr. Gray, is as follows: "Are there post-mortem appearances in the brains of those who die insane which would justify the assumption that morbid cerebral changes were the potential and only ultimate causation of insanity?"‡ It is probably intended to ask whether tangible lesions of the brain, of some kind, are not always the proximate cause—

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* *Journal of Insanity*, vol. XXVII., p. 406.
† Mental Pathology and Therapeutics, p. 413.
‡ *Journal of Insanity*, vol. XXVII., p. 406.

not "the pretended and only ultimate causation"—of insanity. The negative of this question was abundantly proved many years ago, and the observations of the Utica asylum have certainly not cast any doubt upon the unanimous verdict of cerebral pathologists in regard to it. But what he has failed to revive as a possible hypothesis, Dr. Gray does not fail to lay down as a positive doctrine; namely, that insanity is a disease of the brain, marked by morbid changes in its structure, *in consequence of which* there is more or less prolonged disturbance of the mind.* The case of Waltz, in which this doctrine is offered as a legal test of insanity, has already been referred to in the JOURNAL. As an attempt to support a doubtful opinion with a more than doubtful doctrine, we agree with Dr. Spitzka in considering it not without excuse. But in the case of Grappont, who committed suicide in Watertown, N. Y., Nov. 12, 1876, on the night following his conviction of murder, even such an excuse was wanting. At the trial, in which the defense was insanity, Dr. Gray was not called, yet an examination of the brain, under his direction, was held to prove the sanity of the criminal, on the ground that "in a case of true insanity the peripheric districts of the nervous centres *must show* some evidences of pathological alterations in their structure," and that these did not appear.

We have only to remark, first, that Dr. Gray's own reports contain many records of post-mortem observations in typical cases of insanity, in which there was an entire absence of the pathological appearances referred to. And, secondly, that "the numerous morbid changes in the nerve structure," found in Grappont's brain, and properly held to be no proof of his insanity—remains of degenerated vessels, amyloid and pigmentary infiltrations, etc.—are exactly those given in the reports as illustrations of the pathology of insanity.†

But we shall be asked whether the large number of facts in the pathological anatomy of the brain, which have been accumulated with so much labor and cost at the Utica asylum, and are so absolutely worthless for their intended purpose, may not still have some real value. We reply, that a truly scientific theory of these facts in the mind of the observer would possibly have enabled him to throw light upon some of the problems of mental disorders by comparing the morbid changes found, with symptoms observed during life. But the published cases afford us no evidence that they were made in view of a real scientific purpose, and they must therefore be wholly condemned. Nine-tenths of the observations must indeed be condemned at sight, without reference to the defect noticed. They consist of alterations in

* Id., p. 397.

† We feel bound to say, that while we agree in the main with the views of our contributor, we do not fully share in those which he appears to entertain in regard to the pathology of insanity. So far as we understand them, we are disposed to regard more favorably the views of Dr. Gray on this subject, than does our contributor.—EDS.

connective tissue, pigmentary deposits, and the various degenerative changes of age and feeble vitality, which, as we have already observed, it is almost impossible to imagine in any way connected with mental disorders.

If we are justified in saying so much of Dr. Gray's observations on the brains of the insane, need we add anything in regard to those proposed by him upon "the skin—its temperature, color, elasticity, sensibility, etc., in the several forms and stages of the disease" (insanity)?* How can we laugh at the provings of homœopathy or the calculations of astrology, if we claim for such facts the least serious consideration?

And yet we find the New York State Commissioner in Lunacy, in his report for 1874, ranking the theories and conclusions we have described as "among the proudest triumphs achieved by positive philosophy in its relation to medicine; by which the pathology of insanity has been placed in the field of rational induction, and its treatment in consequence advanced with a success commensurate to the skill with which the laws of natural philosophy are applied to the physiology of the human body."†

Such is the pathology of insanity, the practical and scientific validity of which, after a careful examination, Dr. Chapin is compelled to deny. His conclusion deserves the more weight as being that of an advanced and independent section of lunacy specialists in this country, and not a personal judgment merely. It is one, we have no doubt, in which all who have learned what appear to be the true principles of cerebral physiology must agree, upon a like candid survey of the subject.

Dr. Chapin next considers the care and treatment of the insane, under which is included the design and cost of asylum buildings. Upon this latter topic, as is well known, lunacy specialists are divided into two parties. One of them demands hospital-asylums so-called, or large and costly buildings of a specified general design and construction for all the insane. It declares that insanity is a bodily disease, easily curable by hospital treatment in its early stages; that it is practicable, by providing these asylums for all the insane, to cure a large proportion of their number every year; and that in this way the burden of chronic insanity

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* *Journal of Insanity*, vol. XXVII., p. 384.

† Whether with Dr. Chapin, we have not erred in regarding too seriously the publications of the Utica Asylum in the form of pathological researches, may perhaps be questioned. Medical institutions, in their public character, are unfortunately permitted to advance their objects by methods and arguments which would be condemned by the profession if made use of in behalf of private interests. Would it not have been the least possible injustice to Dr. Gray if we had implicitly regarded his pathological writings as simply lobbying and advertising productions? Surely no medical man, as such, would at this day pretend that the specific lesion of insanity is a legitimate object of scientific research. And not less certainly would it be a positive insult to scientific students, seriously to pretend, that the problem of insanity during life can be solved by a post-mortem examination of the brain.

may be steadily diminished, until it finally becomes insignificant. No one can point to the experience of any community in proof of this assertion. On the contrary, it is everywhere directly contradicted by lunacy statistics. The fact is undeniable that the number of incurable insane has constantly increased in communities whose asylum provision has been the most ample, and of the best grade. Why persevere, then, in the impossible task of building palatial asylums to accommodate all the insane, while three-fourths of them lack even decent care and maintenance?

To this the opposed party add, that they hold the view of insanity as a functional disease of the brain, merely as an hypothesis under which to study the analogies of morbid, mental and cerebral phenomena. For all practical purposes, they say, it must be considered simply as chronic, mental disorder. Of its treatment by medicines, they believe that little or nothing is known, while even mental influences, which can do as much to ameliorate symptoms, are of little effect in cutting short an attack. The aim of this party is to provide each of the several classes of the insane with suitable care and maintenance. Three-fourths of all the insane, it is conceded, belong to the indigent and pauper classes. No one believes it the duty of government to furnish them with residences at a cost of from $2,000 to $5,000 each, when they were in their normal, mental condition. And now that they have become unable to appreciate the comforts and decencies of a cottage even, why provide a palace for them? There can be no doubt that they enjoy better a simpler and more natural mode of living. Will it not probably afford them at least an equal chance of recovery?

This is the course of reasoning which has led to numerous variations from the orthodox type of insane hospitals, such as farm asylums, cottage asylums, and special asylums for the incurable insane. All these have been combined, by the sagacity and courage of Dr. Chapin, in the celebrated Willard Asylum, at Ovid, N. Y. Meanwhile the liberal alienists make no war upon hospital asylums. In spite of the unfounded, and by no means harmless, theories of medication for mental disorders with which they are too often identified in this country, for a large class of acute cases, and for a few of the chronic class even, they are indispensable. On the question of the cost of construction of asylum, Dr. Chapin presents in a strong light the necessity and advantage of economy. But his success in building the Willard Asylum for less than $1,000 per patient, at a time when the cost of building was nearly double what it is at present, is worth volumes upon the subject. It should be remarked that, although designed for the chronic insane, this asylum really lacks nothing that is necessary in a building for the treatment of acute cases.

In Dr. Wilbur's essay on "Buildings for the Insane," the subject is discussed in the able and forcible style of the writer, and with a thorough understanding of the questions involved. There

is no longer room for dispute as to the advantages or disadvantages of separate provision for the chronic class. The only possible mode of providing for all the insane, the relief so urgently demanded, is that of a practical separation of the two classes. Dr. Wilbur also strongly recommends the more regular and general employment of the insane, and predicts that "in the American asylum of the future, occupation will certainly be a prominent feature of the moral treatment." If this should prove true, which we think most probable, it will lead to the further division of the great class of the chronic insane into sub-classes, and to small asylums, as a most important feature of the new system.

Another improvement in asylums of the future, according to Dr. Wilbur, will be a more appropriate design for their buildings. The main central edifice, with wings; the "long monotonous corridor," which is the day-room of the inmates, with sleeping-rooms on one or both sides, "all with heavily-barred windows to the last window," are especially condemned. He continues: "Restraint, jealous and irritating is written on the doorposts of every institution. Everywhere the most expensive mode of meeting the desired end, whether in heating, ventilation or water-supply. Everywhere sumptuous arrangements for the accommodation of the officers."

Dr. Wilbur's objection to the unhomelike features of the stereotyped asylum plan will be generally concurred in. The fault of asylums in suggesting restraint or confinement by locked doors, window guards, etc., appears to us less important. According to our experience a large proportion of the insane, whose feelings and actions are partly guided by normal perceptions, not only expect and excuse these appearances of restraint, but find a certain sense of assistance and security in them. The same may be said of the various means for restraining the person, in use in asylums. We have not seldom had patients apply for the restraint of the muff and belt at the coming on of a paroxysm, and complaints from recovered patients of the imposition of personal restraint, as such, in their treatment, are certainly rare. We believe, further, that more than one defect in the administration of our asylums must be remedied before the general disuse of restraints can be carried out with advantage or safety. Among these is the practice of over-crowding asylums, particularly the wards for violent cases. Where rooms closely calculated for twenty patients of this class are made to hold twenty-five, or even thirty, the excess sleeping on the floors of dining-rooms and corridors, there will be a greatly increased excitability, and restraints may be absolutely necessary to prevent dangerous violence. This practice of over-crowding is mainly due to the great lack of asylum accommodation, consequent upon the policy of expending for a single asylum enough to build two or three of the same capacity. It is in part due, also, to the necessity of securing a large income, to meet the

large cost of maintenance in hospital asylums. The new hospitals at Buffalo and Ponghkeepsie were designed for 400 patients each, their capacity is now stated at 600, and they cannot be made self-supporting, in the usual manner, even if this number be crowded into their wards.

Another difficulty in the way of the present abolition of restraints in our asylums, is the cost of the additional force of attendants which would be required. In proportion to the number of violent cases in their wards, the number of attendants in English and French asylums is much larger than in ours.

But we wish only to point to certain reforms which must go hand in hand with that of the disuse of mechanical restraints. That the too free use of these means in our asylums amounts to a serious abuse, we have before admitted. The deception practised upon Dr. Bucknill, at the Utica Asylum, as pointed out by Dr. Wilbur, could only have been designed to cover such an abuse. Dr. Bucknill found "not a single patient in restraint or seclusion," in passing through the wards, and was told by Dr. Gray "that he did not use restraints." A denial of this language has been published by Dr. Gray, but there seem to be grounds for the belief of Dr. Wilbur, that about forty rooms containing crib-beds were closed to Dr. Bucknill, and a large amount of restraints temporarily removed for the occasion. Direct and positive evidence has been given us that, at the date of Dr. Bucknill's visit, four crib-beds and five muffs were in daily use on a single violent ward for twenty patients; and that on the morning of the visit, by direction of Dr. Gray, all restraints were removed from the patients of this ward, and those in cribs taken from them and from their rooms, the doors of which were then locked. We need hardly repeat, that a policy of concealment and deception cannot be necessary under a proper use of restraints. Nor will it be easy for the profession to condone such a policy in respect to any of the methods of insane asylums. Nothing but the utmost candor and truthfulness on the part of their officers will secure for them that absolute confidence which the Association of Medical Superintendents demand of both the profession and the public. It will not be believed that the doctoring of medical and financial statistics which have been described, are for scientific and philanthropic purposes. No one can doubt that the "improved plans" and "crooked" accounts of savings banks and life insurance companies were both false science and false philanthropy. And surely such arguments cannot be needed to induce the great State of New York to provide properly for its insane!

Of the cost of maintenance of the insane, Dr. Chapin has little to say. But here, also, his work at the Willard Asylum is more to the point than words. For the 1,300 patients now under his care, the grade of maintenance is probably equal to the average of State Asylums, and at one-half the cost of that at Utica. But the subject of asylum maintenance, which has received little

attention in the reports of Dr. Gray, has been made a study by Dr. Chapin, for many years. In an essay published by him in 1868, he presents, in a tabular form, the weekly cost of support in five asylums of States lying adjacent to New York, the average being $3.89. At the bottom of the column the cost at Utica is given as $5.09. This table is copied by Dr. Gray in his report for 1869, and also a similar one by Dr. Charles A. Lee, in which the cost at Utica is given at $5.53. Dr. Gray's comment is as follows: "Neither of these statements are [*sic*] correct as regards Utica, whatever the facts may be touching the other institutions mentioned. Public institutions can well bear the discussions of the questions of expense of the care of the dependent."

And yet the subject of maintenance, as we have remarked, is not freely discussed by Dr. Gray, for reasons which may probably be suggested by our analysis of his expenditures in the last volume [p. 781, Vol. IV.] of the JOURNAL. In the methods we have pointed out may also be found the reasons why such experts as Drs. Chapin and Lee differ in so simple a matter as the calculation of a weekly cost. They also help to explain a considerable difference in the results of several calculations of the Utica Asylum costs for 1874, all of which were made at the asylum. This cost is given by the State Commissioner in Lunacy as $4, by Dr. Bucknill as $4.50, and by the State Board of Charities as $5.42; while the true rate, based upon the ordinary expenditures, according to a universal rule, is $7.18.

But we must pass over many topics of interest in these excellent papers to return for a moment to one already touched upon at the opening of this review. In his closing remarks, Dr. Chapin dwells upon the necessity of some direct, governmental supervision of asylums, corresponding to that of the English Commissioners in Lunacy. We quote as follows: "The high standing of the British asylums—their advanced ideas in treatment, their freedom from use of restraint, occupation of patients, the high tone of the literature of the medical profession, the immunity of the asylums from political management, and the confidence and universal esteem in which they are held—are largely if not mainly due to the beneficent and overshadowing influence of the Commissioners in Lunacy. The influence of a similar intermediary board needs to be felt, and would be welcome, in this country, though it is doubtful whether the lunacy interests alone, of any State, are of sufficient magnitude to warrant the creation of such boards. They may be, as in many States, properly confided to *Boards of Public Charities*, possessing all the needful powers of a lunacy commission, representing all the public charities, in their benevolent, financial, and medical relations to the community, elevating them, and bringing them in these respects in best accord with the policy of the State."

Among the important functions of such a board, is that of inquiring into the various abuses which public opinion is so prone to charge upon asylums, and which in other countries have been

so often exposed. Of these Dr. Chapin writes: "While errors in judgment may not infrequently occur in the asylum, we believe what may be properly termed abuses are of rare occurrence, and when they do exist, cannot, and do not, remain concealed."

We believe, with the writer, that the abuse most often connected with asylums in the public mind—that of the incarceration of sane persons in them—is almost wholly an imaginary one. In a large experience we have known only two or three attempts at this abuse, and in each case they were easily foiled. But there is an abuse similar to this, to which American asylums are peculiarly liable, and which we have no doubt prevails to a serious extent. The legal steps necessary for commitment to an asylum are sufficient to insure that no sane person shall be so committed, and the admission of any one without the proper legal forms would fix a responsibility upon the officers of the asylum which they would not dare to accept. But there is a large class of persons of nervous temperament, who become insane under some excitement or shock, or perhaps from acute bodily disorder, and who are very properly removed from the conditions which have determined their attack, to the care of an asylum. Once admitted, however, with the proper forms, the law provides no further safeguard for them. The medical superintendent, whom the law prudently forbids any voice in deciding the mental state of the patient with reference to his admission, is now given a power over him more perfect and absolute than any other human being over one of his kind. No matter how speedy or complete his recovery may be, sanity and liberty have no existence for the patient except in the opinion of the superintendent. And it must be remembered that the facts upon which a medical expert bases his opinion he cannot fully describe to others, or even to himself. This is the mystery of "expertness," which is beyond the reach of criticism, and a perfect shield from responsibility.

The veteran psychologist, Dr. Earle, of the Northampton (Mass.) Lunatic Hospital, in one of his late reports, has stated his belief that "in a given number of patients discharged from a hospital for the insane, the number reported as recovered may differ at least twenty-five per cent., according to the man who may act as the judge of their mental condition." He believes, in other words, that self-interest and vanity operate thus largely, "even though unconsciously," to increase the number reported recovered in the returns of insane asylums. But if these motives may operate to such a degree in deciding the condition of discharged patients, why may they not also operate in deciding their condition with reference to the question of discharge? And in an asylum, for instance, whose annual deficiency can be reckoned on with absolute certainty, is it not possible that pecuniary considerations should operate, "even unconsciously," against the verdict of recovered in the case of a convalescent, and therefore

profitable, patient. Can we suppose financial straitness a less powerful motive than professional vanity? We have elsewhere found one superintendent covertly dropping from his weekly cost upwards of $50,000 yearly for successive years; and it would be easy, by a comparison of his statistics, to show the probable addition of, not twenty-five, but one hundred per cent. to the true number of his recoveries. How far these misrepresentations may be conscious or "unconscious," it would be vain to inquire. But the point we wish to make is, that the office of deciding the all-important question of the sanity and liberty of a fellow-being should be absolutely separate from that of "cooking" the medical and financial statistics of an asylum.

In part, at least, this reform should not be difficult. But the proffer by the New York State Board of Charities of its disinterested service as an equalizing board in the distribution of public funds among the several asylums of the State, has thus far been rejected. By combination with each other, and with the canal and prison interests, these asylums appear to find lobbying for themselves much the more satisfactory practice. Can it be supposed, indeed, that they should not know how much money they want better than the State Board? But it is satisfactory to know that combinations of this kind are rapidly losing their hold on the State treasury. The canals and prisons have already been rescued from the hands of robbers, and it is not possible that the charitable institutions of a great State should much longer continue, even in part, the prey of medical and local politicians.

But no one acquainted with the wholly unchecked opportunities and temptations of our asylum system, and the general rottenness of public institutions, will require a further discussion of this unpleasant subject.

Such abuses as the excessive use of restraints, the thoughtless use of poisonous drugs for quieting patients, the overcrowding of wards, the too long detention of the convalescent, and extravagant and corrupt expenditures, beyond all doubt exist, and must be reformed. It has been urged, by the Association of Medical Superintendents, that the local managers of asylums are sufficient for all needful supervision of them. But it is notorious that these exist, in general, only as lobbying and whitewashing committees, and the public has ceased even to look to them for the performance of any other functions. It is, we repeat, of State Boards of Charities that reform in the management and supervision of the insane is demanded. That this reform has been so long in coming to the most helpless and pitiable of all those who claim public charity and protection, is charged, not without grounds, upon a branch of the medical profession. It is all the more proper, therefore, that the movement to place our system of provision for this class upon a level with that of European countries, should be led by medical men; and the efforts of Drs. Wilbur and Chapin to that end deserve the thanks of their professional brethren.

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III.—DARWINISM TESTED BY LANGUAGE.

DARWINISM TESTED BY LANGUAGE. By Frederick Bateman, M.D. London, 1877.

What is a man? "manners make the man," says one; "dress makes the man," says another. Dr. Franklin defined him as a "tool-using animal;" somebody else, as "an animal with a religion." All these and several other distinctive characteristics have been shown to be insufficient or erroneous, and now Dr. Bateman, whom we know well as the author of an excellent monograph on aphasia, argues for the existence of another essential point of difference in the fact, as he declares, that the former possesses the power of using articulate language while the latter has no such faculty. By this test Dr. Bateman proposes to try the Darwinian hypothesis of the descent of man, and he asserts that here is the difference, not in degree but in kind, which Darwin demanded should be furnished before he would consent to renounce his views.

Before proceeding to the discussion of the many interesting points adduced by Dr. Bateman in support of his opinion, it may be well to inquire whether, if the animals lower in the scale than man did speak, they would be any the more men, and if man were to be rendered incapable of thus expressing himself, he would be any the less a member of the genus Homo than he is now. It may seem absurd to raise such a question, but yet it is very evident that if Dr. Bateman be correct in the inferences he has drawn, it is one which rather forcibly stares us in the face.

We have been looking for some radical difference between man and the brute. Dr. Darwin and his followers say there is no such difference. Dr. Bateman, on the contrary, tells us there is an essential one in articulate language. Every animal, therefore, that possesses the faculty of communicating ideas or expressing emotions by words is a man; every animal that is not thus endowed is not a man.

The point is by no means a new one. Locke had it very clearly in his mind when he said, "I think I may be confident that whoever should see a creature of his own shape or make, though it had no more reason all his life than a cat or a parrot, would call him still a man; or whoever should hear a cat or a parrot discourse, reason and philosophize, would call or think it nothing but a cat or a parrot; and say, the one was a dull, irrational man and the other a very intelligent, rational parrot; and then he goes on to relate a story on the authority of Prince Maurice, which, whether true or not, serves equally well for an illustration:

"When it came first into the room where the Prince was with a great many Dutchmen about him, it said presently, 'What a

company of white men are here!' They asked it what it thought that man was, pointing to the Prince. It answered 'Some General or other.' When they brought it close to him he asked it, 'D'ou venez vous?' It answered, 'De Marinnan.' The Prince, 'A qui estes vous?' The parrot, 'A un Portugais.' The Prince, 'Que fais-tu la?' 'Je garde les poulles.' The Prince laughed and said, 'Vous gardez les poulles?' The parrot answered, 'Oui moi et je scai bien faire,' and made the cluck four or five times that people use to make to chickens when they call them."

"And," continues Locke, "I ask any one else who thinks such a story fit to be told whether,—if this parrot and all of its kind had always talked, as we have a Prince's word for it this one did,—whether I say, they would not have passed for a race of rational animals; but yet, whether, for all that, they would have been allowed to be men and not parrots? For I presume it is not the idea of a thinking or rational being alone that makes the idea of a man in most people's sense, but of a body so and so shaped, joined to it."*

This latter appears to us to be exactly the point. It is not so much the mental faculties, or the intellectual characteristics that constitute the chief features of difference between man and the other animals, as it is the structure of the body and the degree of development of the several organs by which both mental and physical acts are performed; and we say this with a full conception of the immense distance there is between the mind of the lowest man and that of the highest monkey. But in the one case the difference relates solely to mentality, while in the other it concerns the organs from which the mentality comes, and those others through which it must act. Mind in the abstract, that is, without considering it in its necessary connection with a nervous system as an effect with its cause, is something of which we can form no distinct idea. We can only regard it as the product of nervous action, and hence, however much we might be astounded by its manifestations, it is in reality insignificant in comparison with the little cells which, by their aggregation, go to make up the structure of gray nerve-tissue, and by which it is evolved. The source of a spring is more important than the spring itself; a galvanic battery is to the thoughtful person a greater subject for astonishment than the force which it sets in action; the brain of a Newton is far more wonderful than the mighty intellect—a part only of its force—which comes from it.

And this is not all, for even if the lower animals were endowed with brains as large and as complex in structure as that of man, they would still in the inadaptibility of their organs for the purposes for which man uses his, be immeasurably his inferiors. No quantity or quality of brain would enable a horse to write a letter, or to get rid of an offensive substance in his stomach by vomiting; though a dog were as wise as Socrates he could not

* An Essay concerning Human Understanding, Chap. XXVII., §8.

play a tune on the violin, and though an ape might possess the majestic cerebral development of a Cuvier his hand would be none the more adapted to the performance of that infinite variety of delicate manipulations which marks the action of this organ in man. These, to be sure, are only differences of degree, and though by successive development in the course of ages they would probably disappear, they nevertheless at present exist.

Again, there are men, undoubted men, who in their manners, customs and modes of life are more akin to the brute than to the genus in which, by a consideration of their structure and appearance, we are forced to place them. Thus Sir John Lubbock quotes Dalton as asserting* that there are wild men living in the interior of Borneo, absolutely in a state of nature, who neither cultivate the ground nor live in huts; who neither eat rice nor salt, and who do not associate with each other, but rove about the woods like wild beasts: the sexes meet in the jungle, or the man carries away a woman from some company. When the children are old enough to shift for themselves, they usually separate, neither one afterwards thinking of the other. At night they sleep under some large tree, the branches of which hang low; on these they fasten the children in a kind of swing; around the tree they make a fire to keep off the wild beasts and snakes. They cover themselves with a piece of bark, and in this, also, they wrap their children; it is soft and warm, but will not keep out the rain. The poor creatures are looked on and treated by the other Dyaks as wild beasts.

Piron,† in describing the natives of Van Diemen's Land, speaks of them as being without laws or any system of regular government, without arts, even being destitute of all knowledge of agriculture or of working in metals, and not even having subjected any of the lower animals to their service. They wear no clothes, have no definite habitations, and no shelter from the weather beyond a rude shed which they make of bark.

Darwin‡ regards the Fuegians as the most miserable creatures he has ever beheld. "These Fuegians," he says, "in the canoe were quite naked, and even one full grown woman was absolutely so. It was raining heavily, and the fresh water, together with the spray, trickled down her body. In another harbor, not far distant, a woman who was suckling a recently born child, came one day alongside the vessel and remained there out of mere curiosity, while the sleet fell and thawed on her naked bosom, and on the skin of her naked baby! These poor wretches were stunted in their growth; their hideous faces bedaubed with white paint, their skin filthy and greasy, their hair entangled, their voices discordant, and their gestures violent; viewing such men, one can hardly make oneself believe that they are fellow-crea-

*Origin of Civilization and the Primitive Condition of Man. Second edition. London, 1870; p. 8.

† Voyage de decouvertes aux Terres Australes, t. i., chap. 20.

‡ Journal of Researches, etc. New York, 1871, p. 213.

tures and inhabitants of the same world. It is a common subject of conjecture what pleasure in life some of the lower animals can enjoy; how much more reasonably the same question may be asked with respect to these barbarians! At night five or six human beings, naked and scarcely protected from the wind and rain of this tempestuous climate, sleep on the wet ground coiled up like animals. Whenever it is low water, winter or summer, night or day, they must rise to pick shell-fish from the rocks; and the women either dive or collect sea eggs, or sit patiently in their canoes, and with a baited hair-line without any hook, jerk out little fish. If a seal is killed, or the floating carcass of a putrid whale discovered, it is a feast; and such miserable food is assisted by a few tasteless berries and fungi."

To call such creatures men is a degradation of the term, it must be confessed, and yet in structure they resemble more closely an average civilized Caucasian than they do the most perfectly developed of the anthropomorphous apes—no one studying them thoroughly, or glancing at them cursorily, would for an instant doubt their position among animals. But it is by no means certain that in mental development there is as much difference as there is in physical characteristics. Indeed, in some respects, they are lower than the brutes above which we place them. They are remorselessly cruel; their women and children are treated with a degree of barbarity of which the monkey is incapable; the affections are undeveloped, in fact there is scarcely a trace of any generous or ennobling quality in their degraded natures. They are ignorant of all sense of justice, or of right or wrong; they are entirely destitute of religious feelings, and have no conception of being superior or very different from the beasts by which they are surrounded. They can talk, but their language consists of guttural monosyllables scarcely more articulate than the cluckings of a hen, and some of them have no abstract terms, no idea of time, no names for each other, and no words for numbers exceeding three. Indeed, so far as language goes, there are greater differences between them and civilized man than there is between them and the gorilla.

We are hence led to conclude that Dr. Bateman's distinction is not of the importance which he has given it. But his book is too earnest, and written in too scientific a spirit to be dismissed thus abruptly. He has presented all the facts in the case, as he understands them, with singular fairness, and he deserves equally earnest and honest treatment. Unless he gets this, unless his position is met and logically combated, his essay is very certain to be seized upon by the unphysiological, sciolistic and conspicuously inexact anti-evolutionist, and held up to the admiring gaze of his adherents as presenting arguments which the heterodox Darwinian cannot answer. We propose, therefore, to show by an appeal to anatomy and physiology that his assumed test is not one by which the doctrine of evolution is to be overthrown.

But before proceeding to the discussion of Dr. Bateman's

observations and arguments, it will probably not be out of place if we give a brief account of what has been done in recent years towards the localization of the faculty of speech in a particular part of the brain; and this is the more important because, as we shall see, the point in regard to the value of language as a test of the essential difference between man and other animals rests, in a great measure, on this matter of localization. If, in fact, it can be shown that there is a part of the brain whose function it is to preside over the faculty of articulate speech, and that this part exists in apes as well as in man, we have, as Dr. Bateman repeatedly admits, a strong argument for the hypothesis of evolution. Develop the organ in the latter and they will speak, or to use Dr. Bateman's exact language, "It might be said that the ape possesses the rudiments of speech in an undeveloped form, and that in subsequent generations by the process of evolution this fold [the third frontal convolution, though of course, the argument is equally good for any other locality], would become more developed and the ape would speak; in fact would become a man!" As to a talking ape being necessarily a man, we have already expressed our doubts, but as we shall have occasion to return to this point, we pass it by for the present.

Previous to Gall, there had been no attempt to define with precision the seat of the faculty of speech, or even to prove that there was such a faculty. But in the early part of the nineteenth century this able cerebrologist not only declared that there was a region of the brain specially devoted to language, but that it was that part of the anterior lobe on each side which rests upon the roof of the orbit. A large development of the brain, in this locality, would of course be attended with a depression of the bony plate upon which it rests; the cavity of the orbit would therefore be diminished in capacity and the eye would be rendered prominent. He therefore announced that the existence of a special ability for memorizing words, and for studying and using language, was marked by prominence of the eyeballs.

In reality Gall considered that there were two organs of language, the one originating the idea of words, the other a talent for philology and for acquiring the spirit of language. Dr. Spurzheim however admits but one organ, lying posteriorly on the supra-orbital plate, and this view is accepted by Combe and other distinguished phrenological authorities. Several cases were brought forward in support of this theory of localization, but none of them were of a very decided character, and hence Gall's views met with very slight favor outside of the class of his immediate followers.

As we have seen, Gall placed the organ of language in a limited part of the anterior lobe of each hemisphere. This was the first attempt at localization. But shortly afterward Bouillaud, who had collected a large number of cases of injuries and diseases of the brain, announced that in very many of these cases in which the anterior lobes were the seat of the lesion, there was

the concomitant of derangement of the faculty of speech, and he also confirmed what had been noticed by others, that absolute loss of the power of speech may exist without there being any noticeable impairment of mind or derangement of body. He likewise clearly pointed out the fact, that the faculty of speech embraces two distinct series of phenomena, the one relating to the power of creating words as representations of ideas and of recollecting them; the other to the co-ordination of the movements necessary for the articulation of these words. The first of these he called internal, the second external speech.

Bouillaud supported his views by the citation of numerous cases—in all, one hundred and three—of lesion of one or other of the anterior lobes in which there was also some derangement of the faculty of speech, and he finally offered a prize of five hundred francs to any one who would adduce a case of profound injury or disease to this region of the brain in which there was not also some trouble of articulation. The prize was claimed by Velpeau, who called attention to the fact that he had in 1843, described the case and exhibited the brain of a wig maker, who, up to the moment of his death, was in full possession of his mental faculties, and who was remarkable for his loquacity, but in whom both anterior lobes were entirely destroyed by a tumor. Inquiry however showed that a very considerable portion of each lobe was intact.

Nevertheless a sufficient number of instances are on record which establish the fact in the most indubitable manner, that Bouillaud was too general in his localization, and that the anterior lobes may suffer the most frightful injuries without the faculty of speech being notably impaired. Among these is the case related by Dr. Harlow, of Vermont, in which a tamping iron was by the premature discharge of a blast driven through a man's skull. Notwithstanding the severity of the injury the man resumed his consciousness in a few minutes, was put into a cart, driven three-quarters of a mile, when he got out and walked into his house. He lived for twelve and a half years afterwards, and then died, after having had several convulsions. His cranium is now in the Warren Anatomical Museum, at Boston, and an examination of it shows that the iron entering the base of the skull on the left side had gone entirely through the left anterior lobe of the brain and made its exit at the top of the head. The only aberration of speech noticed in this individual, was his having become exceedingly profane in conversation. If the faculty of language resides in the whole of the anterior lobe, as Bouillaud contends, such an immunity could scarcely have existed. There can be no doubt, therefore, from a consideration of this, and many other cases, that Bouillaud was wrong in claiming that injury of any part of the anterior lobes is necessarily followed by some derangement of the faculty of speech. Subsequently, however, he admitted that the organ of language may occupy the posterior part of each lobe only.

In 1836, Dr. M. Dax read a paper before the Medical Congress which met that year at Montpellier, in which he asserted that the faculty of language was seated, not as Gall and Bouillaud had concluded, in both anterior lobes, but that it occupied only the left anterior lobe. He based this opinion upon one hundred and forty cases of aphasia* attended with paralysis, in which the want of power was confined to the right side of the body; showing therefore that the lesion causing it and the aberration of speech was situated on the left side of the brain. This paper never attracted much attention, notwithstanding its importance, a circumstance very often the result of burying scientific communications in the archives of insignificant societies.

Thus far we see the organ of language placed in the left anterior lobe. It must be confessed, however, that this location was by no means accepted by even a considerable body of anatomists and physiologists, while the great majority either opposed the localization theory, or regarded it with contempt, as a phrenological vagary.

But, in 1861, M. Gratiolet, in discussing before the Anthropological Society of Paris, a question relative to the comparative development of the brain and mind among different races, brought up the subject of cerebral localization to which he announced himself as being strongly opposed. M. Aubertin, on the contrary, contended that the localization of the faculty of speech, at least, was definitely established through the researches of Bouillaud in the anterior lobes. In support of this view, he adduced cases which had already been brought forward, and cited others in addition, which went to show that loss of speech was the consequence of traumatic lesion of these parts of the brain. His adversaries cited other cases in which persons had preserved the faculty of language, notwithstanding extensive injuries of the anterior lobes. M. Aubertin responded, that if such profound and extensive lesions had not interfered with speech, it was because that part of the lobes in which the organ is situated was not involved, and he then cited the case of a patient in the Hospital for Incurables, who, for many years, had been deprived of the power of speech, and he declared that he would renounce the doctrine of Bouillaud if the autopsy of this patient did not reveal the existence of disease of the anterior lobes. The patient in question was under the charge of M. Broca, and the latter, a decided opponent, accepted M. Aubertin's challenge, and declared that when the man died the examination should be made. Sometime afterward the patient died, the *post-mortem* examination was made, and the lesion was found to occupy the left anterior lobe.

And now the contest waxed warmer; M. Broca, who had been a most determined opponent of Bouillaud's views of localization,

* Ἀ, privation, and φασις speech. Aphasia may be defined as a condition produced by an affection of the brain, in which the idea of language or of its expression is impaired.

became converted. Taking as his principal case the one to which M. Aubertin had pinned his faith, he read, in 1861, before the Anatomical Society, of Paris, a memoir,* in which he not only contended for the restriction of the organ of speech to the left anterior lobe, but carried the differentiation still further by assigning it to a limited part of this lobe, the posterior part of the third frontal convolution. Cases were now brought forward in all parts of the civilized world—our own country contributing some of the most important—going to establish the truth of Broca's views, and the literature of the subject increased enormously.

But, although many instances were adduced going to show that lesion of the third left frontal convolution caused derangement of the faculty of speech, several cases of undoubted aphasia were cited, in which during life the accompanying paralysis had been on the left side of the body, or, in which, on *post-mortem* examination, the third left frontal convolution was found to be perfectly healthy. Again there were cases in which this portion of the brain was found to be entirely disorganized, but yet, in which there was no resultant aphasia; one such case—and there are quite a large number—is, of course, sufficient to decide the question against Broca's doctrine, and, therefore, notwithstanding the very striking evidence which may be adduced in its favor, facts show beyond a doubt that he assigned to the organ of language a too restricted habitation. But with all that has been said against Broca's hypothesis, we are not, on that account, unwarranted in assigning a certain definite locality to the organ of language. On the contrary, the discussion of the subject, and the vast array of cases brought forward, enable us to fix the position of that organ with as much certainty as is necessary for the establishment of the relation of cause and effect. Dr. Bateman, therefore, in attacking the doctrine of the third frontal convolution, and arguing that because Broca's views are shown to be untenable, we are without data for speech localization in the brain, is contending against a theory which it may be said no one—probably not even Broca himself—now entertains. If we can show that there is any locality in the brain with which the function of speech stands in direct relation, and if we further show that this portion of the cerebrum exists in the ape, though undeveloped, we do a great deal, as Dr. Bateman admits, towards explaining why this animal, which stands in the zoological scale so close to man does not, like him, express his ideas and emotions by articulate language.

The anterior lobe of each hemisphere of the brain is separated from the middle lobe by a fissure called the fissure of Sylvius. In it is situated the middle cerebral artery which supplies blood to the posterior part of the anterior lobe, the anterior part of the

*Sur le siege de la faculté de langage articulé, etc. *Bulletin de la Societé Anatomique*, Tome IV., 1861.

middle lobe, and to the lobe of the insula—a small mass of brain tissue placed in the fissure and sometimes called, after its discoverer, the island of Reil. This latter is found in no other animal than in man and the monkey. In the latter, however, it is very slightly developed, and has been said by Huxley and others to present no trace of convolutions. Now there is not a single well authenticated case of injury or disease involving all this region nourished by the middle cerebral artery on both sides in which there was not derangement of the faculty of speech, and there is not a single case of aphasia on record in which if any lesion at all was found in the brain, that lesion was not situated in some part of this region on one or other side of the brain. It is true, there are cases of aphasia occasionally met with in which minute examination after death, fails to reveal the existence of any lesion at all, but we may very properly disregard these, for the like fact exists frequently with other cases of undoubted cerebral disease. In such instances the disorder—probably some derangement of circulation acting instantaneously and for a short time—disappears with the cessation of life.

This region may, after Kussmaul,* be called the "speech tract." The third frontal convolution, the island of Reil, the corpus striatum and the anterior part of the middle lobe, all of which have been shown to be intimately connected with the faculty of articulate language, are parts of this "speech tract."

We are now prepared to consider Dr. Bateman's arguments in favor of the view that articulate speech is a distinctive quality of man, and to meet his objections to the localization of the faculty of language in any part of the brain. W. A. H.

(TO BE CONTINUED.)

IV.—LUDWIG'S ARBEITEN.

ARBEITEN AUS DER PHYSIOLOGISCHEN ANSTALT ZU LEIPZIG. Elfter Jahrgang, 1876. Mitgetheilt durch C. Ludwig. Leipzig: S. Hirzel, 1877. (*Report of the Physiological Institute at Leipzig. Edited by C. Ludwig.*)

Like all its predecessors, the present report of the work done in Ludwig's laboratory is a model of scientific accuracy. Every point investigated therein, however modest its title, and however unimportant its bearings may seem, is a permanent acquisition to physiology.

In the first article, Mr. Ch. S. Minot describes the method of passing a current of defibrinated blood through the vessels of a freshly excised muscle (biceps and semi-tendinosus of the dog). The vessels were first washed out with serum; hereupon a current of arterialized blood was passed through them. The irritability of the muscle could thus be maintained for several hours.

*Stoerungen der Sprache, Leipzig, 1877.

Analysis of the amount of CO_2 contained in the blood before and after the transfusion, showed an increase of CO_2 by from 1 to 5 per cent. *The amount of CO_2 gained by the blood in its passage through the muscles was found to be independent of the state of rest, fatigue or activity (electric stimulation) of the muscle.*

In a second paper, Dr. F. A. Falk studies the action of laudanosin, an alkaloid of opium discovered by Hesse (1871), on the circulation. The alkaloid was injected in aqueous solution into the veins of rabbits, which had previously been curarized to avoid the spasms of laudanosin. Small doses, 2½ to 5 milligrammes, both elevate considerably the arterial pressure and increase the frequency of the pulse. These effects are due to stimulation of the vaso-motor centre, and of the accelerator fibres of the heart.

The vagus is not affected by this quantity. Large doses, however, have the contrary effect. They reduce both the arterial tension and the frequency of the pulse. This peculiar reversion of phenomena is caused by the paralytic influence which the poison exerts on the heart itself, when present in large quantity.

Dr. W. H. Gaskell investigated the action of the vaso-motor nerves of the extensor muscles of the leg by measuring the amount of blood flowing through the veins. A canula was so inserted into a venous branch that, during an observation, the quantity of blood issuing from it could be registered, while during the interval the blood current followed its normal path. Large dogs could thus be observed for a long time without suffering a fatal loss of blood. Section of the motor nerve of the muscle increased considerably the amount of blood passing through the muscle. Within a minute, however, the rapidity of the current diminished again, but still it kept above the rate of the uninjured muscle. The natural explanation seems to be that the nerve-trunk contains both vaso-dilator and vaso-constrictor fibres. The former are temporarily irritated by the section, and their influence prevails during the first minute. Subsequently, however, the removal of the tonus of the constrictor fibres maintains still the increase of circulation as compared with the uninjured muscle. The effects of faradization of the motor nerve are very striking. The contraction of the muscle expels suddenly a jet of blood, evidently mechanically. During a short interval the current is much diminished—a period corresponding to the refilling of the suddenly emptied vessels. After this, however, the amount of blood issuing from the canula is enormously increased and remains so during the entire period of contraction. On interrupting the stimulation, the blood-current diminishes again, but still it does not regain its original rate until after some time. This series of phenomena agrees well with the hypothesis of vaso-motor fibres, which, when stimulated, prevail over the simultaneously excited *feebler* vaso-constrictor nerves. Finally it was found that the vaso-motor nerves of the muscle were paralyzed by curare.

A similar method was employed by Max V. Frey, in studying

the circulation in the submaxillary gland. Here, also, the amount of blood issuing from a venous branch was registered. Stimulation of the sympathetic nerve expelled a sudden slight jet of blood; the result, evidently, of the sudden contraction of the arteries. After this the current was reduced, reaching its minimum in about twenty-five seconds, both when the stimulation lasted fifty seconds, or on the other hand only ten seconds. Within a minute after cessation of the stimulus, the current regained its original rate. Stimulation of the chorda tympani increased the rapidity considerably, but only for a short time, since the irritability of that nerve is rapidly exhausted. Curare, in ordinary doses, does not paralyze these nerves. On stimulating simultaneously both the sympathetic and the chorda tympani with electric currents of equal strength, the action of the sympathetic masks completely that of the other nerve. As long as the stimulation continues, the blood-current is diminished in the same way, as if the sympathetic alone were excited. But on interrupting the faradization of both nerves, the action of the sympathetic stops abruptly, and the after effects of the excited chorda tympani are manifested by a considerable increase in the rapidity of the circulation.

F. Klug examined the sense of temperature in the skin by means of a thermo-æsthesiometer, an æsthesiometer, the points of which could be kept at any temperature, by means of a current of water passing through them. The distance was noted at which the points could just be recognized as two. As a general result it was found that the cutaneous sensibility increased with the difference in temperature between the skin and the instrument. The least sensibility was manifested when the two points had a temperature of 20° C.; the same was the case when one point was cooled to 5° C., while the other was warmed to 50° C. The most acute sensibility was found when both points were cooled to 5° C., or warmed to 50° C., and in the latter case applied to the skin, cooled by immersion in cold water.

The volume contains besides, three articles on the passage of lymph, and one on the histology of the lymphatic capillaries.

H. G.

V.—CEREBRAL HYPERÆMIA.

THE RESULT OF MENTAL STRAIN AND EMOTIONAL DISTURBANCE. By W. A. HAMMOND, M. D. G. P. Putnam's Sons, New York. 12 mo. 108 pages.

In this monograph, from the prolific pen of Dr. Hammond, we have certain views, long entertained by him, enunciated once again and much amplified. As the title rather clearly indicates, the subject is that of the excessive and sustained hyperæmia, which results from over-work or over-excitation of the nervous

system, more particularly the nervous centres within the cranial cavity.

That nervous action or excitation is attended by an increased circulation of blood in those parts which are the seats of such action or excitation, is now so well known as to be beyond dispute, it would seem. It is impossible to think or feel without some change in the character of the circulation of the parts which are the seats of the thought or feeling. It is, also, on the basis of present knowledge, to be recognized as true, that over-action, or over-excitation, will lead to a corresponding morbid increase in the quantity of the blood in the parts involved. More action involves more tissue waste. More tissue waste involves a more profuse supply of nutritive materials, and this means more blood; and, finally, as a matter of course, hyperæmia, which differs in seat, degree and duration, according to the seat, intensity and duration of the action. "Cerebral hyperæmia" is nothing new, and hence any considerable value the work is likely to possess, we should expect to find under the heads of symptoms, pathological details, or practical management, hygienic and medical. After the preface and a short introduction, a chapter is devoted to "symptoms." But good as the account is, it does not materially differ, except in fullness, from that given already in the author's work on "Diseases of the Nervous System."

It will be impossible to discuss, in this notice, the really difficult questions which would at once arise, if the attempt should be seriously made to connect, in a casual way, the various morbid phenomena in Dr. Hammond's list of "symptoms" of cerebral hyperæmia, with that condition. Many of these symptoms may, according to our own observation, be connected with conditions quite different from hyperæmia, or are even thus far inexplicable.

Next comes a chapter on "Differential Diagnosis." This is quite an instructive one, but we cannot tarry to discuss it.

The next brief chapter is given to "Causes." But it is the most unsatisfactory in the book.

The next chapter, of less than one page, is given to "Prognosis," which, under suitable treatment, is properly said to be favorable.

That on "Morbid Anatomy and Pathology," is brief, and upon the whole clear, but too brief and devoid of details to be of much service to the exacting professional reader.

The chapter on "Treatment" is practical and valuable, and will be the first, and we were about to say the only one to which too many physicians will turn; too many of whom only want a *name* for a morbid state, say "cerebral hyperæmia," and then, without any farther consideration, a "recipe." To the work itself we must refer the reader for brief but valuable directions as to treatment. We can heartily commend it to such of our readers as may be seeking information on the subject of which it treats.

But we must say we feel disappointed that Dr. Hammond has apparently thrown away the opportunity which the publication of this *brochure* afforded, of more fully expounding the inner mechanism of the class of cases to which the work relates. Having said thus much, we now proceed to try and justify our feeling of disappointment. Dr. Hammond has—so it seems to us—missed, in great measure, one of the prime factors of the class of cases he has partially described. There is not simply hyperæmia in such cases. There is, besides, a *nutritive lesion*, which in its genesis antedates the hyperæmia, and which, except in its later developments, has no dependence on the hyperæmia. In proof of which, it may be said, among other things, that the lesion of which we speak is entirely consistent with *anæmia*. In our opinion, the nutritive lesion mentioned outranks in importance the hyperæmia in the majority of the cases described by Dr. Hammond.

The order of phenomena, in uncomplicated cases, seems to us to be as follows: Action of the brain, as already said, whether in thought or emotion, leads to waste of brain tissue. In the ordinary way, this waste creates a demand for an extra supply of nutritive materials, and hence, an extra supply of blood; and this, when it occurs, is hyperæmia.

In the ordinary way, rest, especially during sleep, gives the needed opportunity for repair. As this process goes on, the former hyperæmia subsides, giving place to a relative anæmia. This is the true order of phenomena in health. But if the periods of action or excitation of the brain are too prolonged, or too intense, waste becomes excessive, and *pari passu*, the hyperæmia becomes excessive. If the subsequent period of rest is too short in which for repair to be made, the hyperæmia, which follows in the wake of brain waste, does not fully subside until the next period for activity sets in, and the process of waste once again goes on, and is added to the former waste, and all the while the hyperæmia persists, and becomes more pronounced and permanent, as the vessels by prolonged distension lose their tonus. The excessive blood supply is at first given in obedience to a local organic *demand*, but at last depends on the *passive* distended condition of the blood-vessels, which have at last lost their tonus. Still the excessive waste remains. The distended vessels; the excessive amount of blood in the part, the nutrition of which has suffered; the slow, imperfect circulation; the tendency for mere physical exudation to preponderate over the delicate normal interchanges between blood within and tissue without the vessels, impedes or wholly abolishes the normal processes of repair.

Here, then, we have a true cerebrasthenia, the two principal factors in which are excessive waste of tissue and hyperæmia, at first active, and last of all passive. The former does not primarily depend on the latter, which is at first a mere incident in the case, and not until its later stages does it rise into striking prominence. The action and the resulting waste are the *causes*

of the hyperæmia. The worn and wasted condition of the brain-structure may continue after the hyperæmia has subsided, and may, from first to last, exist with anæmia rather than hyperæmia. The nutritive lesion we have alluded to and insisted on, is the undertone of the picture. In the class of cases described by Dr. Hammond, it is the first element to appear and the last to disappear. But in the work of Dr. Hammond's, this element is in great measure disregarded. Hyperæmia is made the sole important factor, from beginning to end; but incorrectly so, it seems to us. The treatment found most beneficial confirms the view we have hinted at. It is not enough to use artificial means for removing the hyperæmia, but attention is found necessary as to diet and medicines, such as phosphorus, iron, strychnia, quinine, etc., to improve general, and more particularly, nerve nutrition. It is found necessary to secure more rest, especially of the brain. Cessation or change of occupation are necessary.

But we cannot push this discussion farther. We have intended simply to bring into clearer relief than Dr. Hammond seems to have done, what, taken altogether, seems to us to be—except in the more advanced stages, or acuter forms of hyperæmia—the most fundamental, if not the most important factor in the large class of cases referred to by Dr. Hammond. As is the rule with the writings of Dr. Hammond, the style is a model for clearness and ease.

SHORTER NOTICES.

I. The Science and Art of Surgery. Being a Treatise on Surgical Injuries, Diseases and Operations. By John Eric Erichsen, F. R. S., F. R. C. S., etc. Revised by the author from the Seventh and enlarged English Edition. Illustrated with Eight Hundred and Sixty-two Engravings on Wood. 2 vols., 947 and 989 pages. Henry C. Lea, Philadelphia, 1878. Chicago: Jansen, McClurg & Co.

II. The Elements of Therapeutics. A Clinical Guide to the Action of Medicines. By Dr. C. Binz, Prof. of Pharmacology in the University of Bonn. Translated from the Fifth German Edition, and edited with additions, in conformity with the British and American Pharmacopeias. By Edward E. Sparks, M. A., M. B., Oxon, etc. 347 pages. Wm. Wood & Co., New York, 1878. Chicago: Jansen, McClurg & Co.

III. Landmarks—Medical and Surgical. By Luther Holden, F. R. C. S., etc. From the Second English Edition. Henry C. Lea, Philadelphia, 1878. Chicago: Jansen, McClurg & Co.

IV. Die vascularen und perivascularen Spaltraume des Gehirns. By Dr. Med. Julius Hoffmann, of New York. Berlin, 1878. (*The vascular and perivascular spaces in the brain.*)

V. Mortuary Experience of the Mutual Life Insurance Company of New York, with Tabulated Reports and an Analysis of the Causes of Death. By G. S. Winston, M. D., W. R. Gillette, M. D., E. J. Marsh, M. D. Volume II. New York: 1877. Printed by order of the Board of Trustees.

VI. Studies in Pathological Anatomy. By Francis Delafield, M. D., Adjunct Professor of Pathology and Practical Medicine, College of Physicians and Surgeons. Nos. 1 and 2. New York: Wm. Wood & Co., 1878.

I. These two bulky volumes deserve a longer and more elaborate notice than it is our privilege to accord them in this journal. It is not necessary, however, to dilate on the merits of this standard text-book, which is probably as well known to the medical profession as any other on the same subject. The revision of this American from the latest English edition, by the author himself, is a fact in its favor, and we find abundant reference to the later results of American surgery on looking over the volumes. The work deserves, and there is every reason to believe that it will obtain, even an increased popularity amongst the medical profession of this country.

II. This work contains in a small compass an account of all, or nearly all, the remedies recognized in the German, British and the United States Pharmacopeias, and as such, it will prove quite convenient for occasional reference to the practitioner. As a student's text-book, however, it is altogether too brief to be satisfactory, except, indeed, to such as find brevity in such subjects always acceptable. It is certainly no substitute for such large works as those of G. B. Wood, Ringer, Pareira, and Stille; to say nothing of the admirable work of H. C. Wood, which only requires certain amendments to come very nearly to the ideal treatise, and the very convenient student's manual of Bartholow. But it is certainly a very handy compendium, including the accounts of a much larger number of medicinal substances and preparations than are usually included in books of this kind, and one that will certainly be of use to the practitioner in this country, where the preparations of the German as well as of the United States Pharmacopeia are often in use.

The remarks on the therapeutic uses of the drugs are indeed often too brief to be useful. If we take those in regard to aconite, for example, we have the following: "This drug is sometimes recommended (1) as an anodyne in neuralgia, especially when it affects the fifth nerve; and (2) to relieve the pain and fever of acute rheumatism. Nothing definite, however, is known about it." This is certainly inadequate and incorrect enough for the merest cram book; it is perhaps the worst treatment of an important remedial agent that we can find in the volume, but there are other cases that approach it.

III. The Medical and Surgical Landmarks is a useful *resumé* of a number of the more important topographical anatomical data

useful to the physician and surgeon in a convenient form. That it has reached a second edition in two years is a sufficient evidence of its popularity and value. The only suggestion we have to make is that in future editions the external topography of the skull corresponding to the principal convolutions and centres be also added. This suggestion, however, is probably needless, as these points will doubtless be attended to in any event in future editions.

IV. The author has undertaken the very commendable task of reducing the problematical descriptions of earlier authors to a uniform and comprehensible basis. After giving a brief notice of the views of Hasse, Kölliker, Bruch, Henle, Virchow, Robin, His, Frommann, Huguenin, Roth, Obersteiner, Golgi, Boll, Tuke, and Arndt, he proceeds to analyze a recent article by an American writer, Mr. Theodore Deecke, of Utica. It may interest our readers to know, that Dr. Hoffmann finds that Mr. Deecke has not only not discovered (as he claims) anything new; nay, that his views are not even original, but that he has woefully confused such views of other writers as he has chosen to adopt. Indeed, we consider the fact with which Dr. Hoffmann concludes the discussion of Mr. Deecke's contribution, as characteristic of a certain species of microscopic work; namely, that Mr. Deecke recommends *water* as an *indifferent* medium for the examination of cerebral tissues. Dr. Hoffmann remarks that water may be an indifferent fluid for drinking purposes, but that no competent pathologist has ever regarded, or could ever regard it as *indifferent* for histological purposes.

We can indorse every conclusion arrived at by the author, except one, where he states, that "for the present it is of no importance to determine the connection between the perivascular and pericellular spaces." We also fail to find any notice of the fact, that in the very finest cerebral capillaries, the walls consist simply of a structureless protoplasmic tube.

V. This volume is the result of a detailed study of the statistics of the mortuary experience of the New York Mutual Life Insurance Company for thirty years, and is the continuation of the former volume, reviewed in this journal (July, 1876). We need not say much in addition to what was said in the former notice; a very large portion of this volume is occupied with a discussion of the mortality from consumption, and proportionally small space is given to that from other affections. In the remarks on the mortality from nervous diseases, a striking point is the vagueness with which these affections are designated in the certificates of death, and this is remarked by the compilers of the volume. We do not, however, see exactly the point where they class "softening of the brain" and "epilepsy" as equally indefinite terms. Though not as sumptuous a volume as its predecessor, this is a very handsomely gotten up book. Both typography and paper are alike elegant.

VI. We have received the first two numbers of this work, containing respectively three and five plates of enlarged microscopic sections, illustrating connective tissue and the structural anatomy of the plura, with accompanying text. Carried out on the plan here indicated, the work can hardly fail to be useful, and with its subscription price, accessible to the profession.

Editorial Department.

IN this issue of the JOURNAL two papers appear, which rather sharply criticize the management of Asylums for the Insane, from various points of view, and also, in some respects the workings and past policy of the *Association of Asylum Superintendents*, in this country. The chief aims of the writers of the articles in question (to which we invite the attention of our readers), appear to be to discuss administrative reforms in the conduct of our asylums, to point out and denounce the practical misapplication of the vast funds contributed by State and municipal bodies for the support of the insane, and to animadvert on the low scientific state of American Psychiatry, as compared with that of other countries, or with the actual privileges for scientific investigation enjoyed by many Superintendents of Asylums for the Insane; and finally, to rebuke the apparently exclusive spirit which has for a long period *seemed* to pervade the Association of Superintendents as regards not only the profession in general, but even in respect to talented assistants in the asylums, and other eminent and capable members of the profession whose only lack of qualification to a place in the Association seems to have been or to be the absence of official relation as chief to an asylum. To set forth these points seem to have been the principal aims of the authors of the papers referred to.

As regards the first point, viz.: administrative reforms, the question is too large and too complicated for discussion within narrow limits. But we presume no well informed and thinking official of experience in practical asylum management would deny that reforms in this domain are not only possible, but needed. In the present state of this class of questions, it is difficult to discuss them from the outside, without giving

some color to the suspicion that the animus of the discussion is a personal one.

The discussion of the modes of managing the complicated and delicate interests of thousands of our fellow-citizens, who have been brought into that pitiable state in which it becomes necessary to deprive them in some manner of their personal liberty of action, can hardly be carried on, even in the most euphemistic manner, without arousing,—perhaps quite naturally,—the feeling that the fate, or at least the reputation of a *man*, is involved with that of his *method*. His competency or his integrity, one or both, are seemingly involved, and under such circumstances a discussion must be burdened as a rule with painful difficulties. The only way to avoid them is by a reform which will secure a thoughtful division of responsibility, instead of concentrating it so nearly in the hands of one man. This has been done in other countries, and should be done in all, especially our own. The closest and most intelligent *scientific* (not political or simply financial) supervision, should be exercised over all our great Asylums for the Insane. Until this is accomplished there will be ground for suspicion, and there ought to be intelligent and unsparing criticisms of all real or apparent administrative deficiencies and abuses, but we cannot now discuss these questions. We may return to them in the future.

As regards the second point—*the misapplication of funds contributed for the support of the insane*—we cannot speak too strongly. We now refer to the expenditure of sums of money, exceeding one thousand dollars per patient, in providing quarters for the pauper insane.

Thus, while one-third of the insane of a State may be provided for in a palace, the other two-thirds are permitted to drag out a simply horrible existence in county almshouses and jails, or remain a private burden. No defense of the thoughtless extravagance practiced in provision for the insane in almost every State from Maine to Texas can be made, which can stand for a moment in the presence of the solemn and distressing facts which we have now under our hands touching this subject. Almost any criticism, however severe or per-

sonal, whoever it may be directed against, which may contribute to thoroughly arousing the public to a clear recognition of the gross and ignorant misapplications which have been made of the large funds contributed for the support of the insane, seems justifiable. But this is a subject, also, to which we expect to give detailed attention in the near future.

As to the third point or that of the present scientific state of American Psychiatry, criticism is eminently in place. None have a higher intelligent respect than we have for the gentlemen as a body, who now occupy the positions of chiefs of our asylums for the insane.

Among them are men who have adorned the medical profession, and would adorn any. But the low state at which scientific medicine has been, and yet is, in general, in this country, does not find an exception among our alienists as a body. But what makes the low scientific state of American Psychiatry so deplorable, is its high and difficult character as compared with other branches of medicine, and the comparatively bad results, which arise to the individual from incompency, or a lack of faithfulness to so high a trust. There are but few medical men in the profession who have such control and closeness of relation to their patients as are enjoyed by the chiefs of asylums for the insane. There are no other positions, which require on the part of those who fill them, higher scientific qualifications. We do not now speak of the severity of the test to which the confidence of friends is subjected, when they consign members of their own families to the practically absolute control of those who may be to them entire strangers. We do not now speak of the legal and other disadvantages of the insane, in relation to those who have control of them. These are necessary incidents of the situation, and it is greatly to the credit of the authorities at asylums, that so few real abuses of authority are reported, when the opportunities are so great. But we now have in mind the exceptionally high requirements implied in the case of those who are charged with the medical care of the insane, and *per contra*, the exceptionally low state, of a truly scientific Psychiatry in this country. There can be no possible doubt but that too little zeal in behalf of the scientific aspect of this noble de-

partment of medicine, has been shown by American alienists as a body. As already intimated, there have been striking individual exceptions to these statements. Among them we may name Drs. Gray and Kempster. However much their methods or results may be open to criticism, the *aim* of their labors, and their industry, all should commend. But the fact still remains, that the same lethargy, which affects the whole profession as regards its higher scientific duties and labors, affects our body of alienists, and we see no reason why this state of things should not be the subject of searching criticism.

With such views we can commend the general aims of the authors of the articles we publish, though not necessarily approving in every instance, the phraseology or modes of statement.

As to the last point,—the alleged exclusiveness of the Asylum Association, in its relations to the profession at large—so far as it is true, it is unfortunate to say the least. We doubt not it will soon disappear, in emulation of the less exclusive spirit of similar associations abroad.

CLAUDE BERNARD.

The death of this eminent physiologist occurred February 10th of this year, from pyelo-nephritis. Notices of his decease have already appeared in various journals, in this country as well as abroad. We had prepared an extended account of his life and labors as a physiologist, but it has been crowded out for want of space. But we would not neglect the opportunity of testifying our admiration for his remarkable talents and achievements. But few men, in the history of science, have united such capacities as an investigator and thinker.

After the discussion which took place at the last meeting of the *British Medico-Psychological Association*, concerning the policy and editorship of the *Journal of Mental Science*, we were prepared to hear of the projection of a new journal, devoted to the interests of Neurological and Mental Medicine. Since the last issue of our journal, we have received the pro-

spectus of a new periodical, to be issued in London, under the somewhat fanciful title of "*Brain*."

This title however does not adequately represent its scope, as the following quotation from the prospectus will serve to show:

"On the Continent and in America there are many journals which treat specially of diseases of the Nervous System, but in this country, although in addition to the ordinary medical periodicals, we have journals dealing in Mind and Mental disease, there are none which include in their scope all that relates to the Anatomy, Physiology, Pathology, and Therapeutics of the Nervous System. These will form the subject matter of 'Brain'—a title not intended as restrictive, but as representative of the whole Nervous System, peripheral and central, as the sub-title, 'Journal of Neurology,' indicates. The functions and diseases of the nervous system will be discussed, both in their physiological and psychological aspects: but mental phenomena will be treated only in correlation with their anatomical substrata, and mental disease will be investigated as far as possible by the methods applicable to nervous diseases in general."

It has a distinguished corps of editors, headed by Dr. J. C. Bucknill. The other gentlemen on its editorial staff are Drs. Crichton Browne, Ferrier and Hughlings Jackson. A long list of promised contributors is published, which includes many well known names in England and France. Naturally we approve heartily of the plan of the proposed journal, since it so nearly approaches that of our own. Such a journal was evidently to our minds needed in Great Britain, and it is with no ordinary satisfaction that we regard the promised appearance of "*Brain*," which can hardly fail of taking at once high rank in that department of medical science to which it is devoted.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

Functions of the Brain.—At the session of the Société de Biologie, Dec. 23 (rep. in *Gaz. des Hopitaux*), M. Francois Franck presented a communication in his own name, and that of M. Pitres, on "the experimental analysis of the movements provoked by excitation of regions of the gray substance of the brain."

These researches still being carried on, have already furnished precise results: 1. *On the comparison of the retardation of the movements produced by the excitation of the regions of the gray substance that constitute the psycho-motor centres of the members, and by the excitation of the subjacent white substance.* 2. *On the constancy of the retardation of the movements, whether the excitations be strong or weak, single or multiple.* 3. *On the quickness of transmissions in the cord of motor incitations from the head.*

M. Franck gave a *resumé* of the experiments which established these three points, and exhibited the graphic traces obtained in each series of researches, and insisted on the special character of those experiments in which the graphic methods of precision employed in M. Marey's laboratory were utilized.

1. *Retardation of the movements according to the precise instant of excitation of the gray and white substance.*

When, in a dog, we irritate that region of the gray matter, excitation of which causes localized movements of the anterior member of the opposite side, taking at the same time on a rapidly rotating registering cylinder, a trace of the movement produced, with electric signals and divisions of time in hundredths of a second, we find the delay of the movements is .065 second (the average figure) after the commencement of the excitation.

This, which may be called the total retardation, is made up of time lost in the muscular action (say .01 second), of the duration of the transmission in the 20 centimetres of nerve between the muscle and the cervical spinal enlargement (perhaps .01 second), and finally the duration of the nervous transmission in the 26 centimetres which represent the distance between the point of excitation in the cortex and the centre of the cervico-dorsal region of the cord. Making all deductions, we have, as the duration of transmission from the external surface of the gray cortex to the origin of the nerves of the anterior member, about 9-200, or it may be, 1-22 of a second, as the nearest figure.

But if we remove in the same animal the thin layer of gray matter irritated in the preceding experiment, so that the irritation is applied directly

to the subjacent white bundles, we find that the time is materially reduced; in place of 9-200 of a second we have only 6-200 of a second; thus the removal of barely two millimetres thickness of gray matter has reduced the delay in the appearance of the movement one-third. We must therefore admit that this gray cortical layer forms a serious obstacle to the nervous transmission, and this is an important character of the really central portions of the nervous system. In the presence of this fact, we cannot admit, with some authors, that the excitation made on the surface of the convolutions only causes movements by exciting the underlying white fibres; *the gray substance acts, not as a conductor but as a centre.*

In noting this fact, MM. Franck and Pitres bring forward a new argument for the theory of the genuine, really central, action of excitable points in the cortical gray substance, a theory already well supported by pathological facts, such as those of monoplegia consecutive to destructive lesions of circumscribed points, the loss of excitability of the subjacent white bundles after the removal of these centres, and by the secondary degenerations of the cord to which Prof. Charcot has called attention in his lectures (*Progrès Médical*, Jan., 1876), three observations of which were communicated to the Societé de Biologie (Oct., 1876).

Still another fact is to be added to those above stated, as establishing the value of this gray substance as a centre: it is the much greater excitability of this gray substance as compared with the white fibres underneath it; this point has been demonstrated by MM. Putnam and Carville and Duret, and verified by MM. Pitres and Francois Franck; the comparative traces exhibited to the society showed the lesser excitability of the white substance. It is therefore needful that the gray matter, as an active agent, should add to the excitation it received itself, to act on the underlying white fibres. This property of reinforcing an excitation, is also characteristic of the really central portions of the nervous system.

2. *The delay of the movement after the beginning of the excitation is constant*, whether the latter be strong or feeble; the same delay is met with whether we employ single or multiple excitations, a single induction discharge or a series of successive discharges. The tracings exhibited by M. Franck prove the reality of the phenomena. It is necessary to add, that the authors have seen in all their experiments the movement of a member when its centre was excited, or the white fibres underlying, even with excitations of the briefest duration, lasting only a thousandth of a second; this is not in agreement with the results announced by Schiff in the appendix to his lectures (Florence, 1873).

3. *Quickness of transmission in the cord of excitations starting from the brain.*—In exciting, *simultaneously* with the same induced currents, the centres for the anterior and posterior members of the same side we cause two movements, which are *successive*.

In a large dog the distance between the centre for the left fore paw and the origin of the nerves of the brachial plexus (middle portion of the cervico-dorsal swelling of the cord), was twenty-six centimetres, the length of the nerve between the cord and the muscles acted upon being twenty centimetres, there was found a delay in the movement caused by the excitation of .065 of a second; deducting from this total retardation the time lost by the

muscle in contracting and that of the transmission in the twenty centimetres of nerve, there remains 9-200 of a second, say 1-22 of a second, which figure expresses the rapidity of transmission of the excitation as far as to the middle of the cervico-dorsal enlargement.

In the same animal the movement of the posterior paw only takes place about 22-200 of a second later than the cortical excitation; deducting from this the duration of the transmission down the thirty centimetres of sciatic (3-200 second) and the time lost by the muscle in contraction, there remain 17-200 of a second as the duration of the transmission of the excitation between the point of gray cortical substance excited and the middle portion of the lumbar enlargement of the cord.

But we have seen that to reach the middle of the cervico-dorsal enlargement, the cortical excitation occupied 9-200 of a second, we must therefore deduct this from the last figure obtained, which gives for the time employed in traversing the 40 centimetres of cord separating the origin of the brachial plexus from the origin of the sciatic 8-200, or 1-25 of a second.

A very simple calculation permits us to deduce from this rapidity in 40 centimetres the quickness in one metre, and we have for one metre 1-10 of a second, consequently motor excitations are transmitted through the cord at *the rate of only ten metres per second.*

Next, in what part of the cord does this transmission of ten metres per second take place? Is it in the white substance or in the gray? This question, which implies the track followed in the cord by the motor impulses, coming from the gray cortical substance of the brain, seems solved by the clinical observations already alluded to; the secondary degenerations of the cord, consecutive to destruction of the cortical motor regions, occupy, as is well known, the posterior portion of the lateral columns.

This is, therefore, in all probability, the route followed by the conductors of voluntary motor excitations, and the indicated figure of ten metres per second applies to the white fibres of the lateral columns and not to the gray substance of the cord. It is needful, nevertheless, to allow something for the partial delay of the transmission in the nuclei of the anterior cornua in relation with the motor roots of the lower member, a retardation which can be studied and ascertained by special experiments on the unilateral reflexes.

At a later meeting, the 29th of December, of the same society, another communication was presented from MM. Pitres and Franck, the substance of which we extract from *Le Progrès Médical*, on the conditions of the production and of the generalization of convulsive phenomena of cortical origin.

I. *In regard to some phenomena following partial destruction of the motor cortical zone* in the dog. If we destroy in the brain of a dog a tract of gray substance, the excitation of which causes limited movements in one of the members on the opposite side of the body, and then after some days if we irritate the white fibres thus deprived of their connections with the gray matter, we find them become completely inexcitable (Albertoni and Michieli). MM. Franck and Pitres have sought to find how much time is required to produce this loss of conductibility.

The next day after the operation they found a notable decrease of excitability; on the second and third day it was necessary to employ energetic currents to cause even slight contractions in the member, and after between 90 and 100 hours the excitability is completely abolished. In the dog, it is also in the course of the fourth day that the peripheral end of a divided nerve ceases to be irritable (Waller, Longet, Ranvier). This analogy between central and peripheral nervous fibres when separated from their trophic centres is not without interest.

While the white matter underlying the centre gradually loses its excitability, the gray matter of the adjoining centres becomes turgescent and inflamed, and, if the inflammation is not intense enough to profoundly alter the structure, we can still see a considerable augmentation of its excitability. Partial epilepsy is now produced with the greatest facility. It is now, often, only needed to make the animal walk, to exercise pressure over the skin covering the cerebral wound, or pass a sponge over the uncovered portion of the brain, to produce the convulsive attacks. It is very curious to observe the gray substance reacting convulsively under the influence of mechanical agents, which in the physiological condition cause no motor reaction whatever.

In these conditions the partial epilepsy may be limited to the parts whose cerebral centres have become abnormally excited, and may respect the corresponding member to the destroyed centre. On the 13th of December, MM. Pitres and Franck uncovered the right posterior marginal convolution of a large adult dog, and removed with a curette the zone of gray matter, excitation of which caused movements limited to the left fore limb. The next day the animal exhibited the troubles of motility and of sensibility in this member that we always meet with after ablation of the cortical motor centres. After it was made to walk an instant it was seized with an attack of partial epilepsy, characterized by convulsive jerks, in the left posterior member, and in the left side of the face and neck. The limb, the centre for which had been removed, remained flaccid and immobile during the whole attack. The rapid loss of excitability of the white substance subjacent to the cortical centre destroyed, and the exaggeration of the excitability of the gray substance in the vicinity of a circumscribed lesion, explain a number of pathological facts. The first of these phenomena permits us to understand how very extensive destroying lesions, including the whole gray matter of the cortical motor zone, do not give rise to partial epilepsy, and show also, that, when there is a limited destroying lesion in this zone itself, the partial epilepsy is not the result of any irritation transmitted by the subjacent white fibres, but that it is due to the increased excitability of neighboring centres.

The second accounts for the apparently contradictory fact that generally the lesions that cause partial epilepsy in man, are located near, but outside of the motor zone, and explains why in certain cases of monoplegia of cortical origin, accompanied with partial epilepsy, the convulsions respect the paralyzed member, or at least, begin in other parts of the body.

II. *Concerning certain associated bilateral movements provoked by unilateral excitations of the brain.* When we electrize the cortical motor zone of a dog, whose convolutions are very excitable, it rather frequently happens

that the movements provoked are not limited to the member corresponding to the centre excited. If we place, for example, the rheophores on the centre for the left fore leg, we may obtain muscular contractions, not only in this limb, but also in the opposite one, and sometimes even in the two hind limbs. These associated or generalized movements cannot be attributed to a too great intensity of the exciting current, since they are produced even with a very feeble current, hardly perceptible by the tongue. It is also certain that it is not the result of a shock transmitted mechanically to the remainder of the body by a sudden contraction of a member, for MM. Franck and Pitres have caused them to disappear in one foot by severing its nerves. To explain their production we may have recourse to one of the three following hypotheses :

1. We may assume that nerve fibres leave each centre, transmitting excitations directly to the limbs on both sides of the body. But this hypothesis is opposed by a great number of physiological and pathological facts, and particularly with that of the constant and absolute hemilaterality of the phenomena consecutive to the unilateral destruction of the cortical centres.

2. We may assume that the cortical centres of the two sides of the brain are connected by commissural fibres, and that the excitation of one of these centres may, on the one hand, affect the member on the opposite side of the body, and on the other, the corresponding motor centre of the opposite hemisphere. But this hypothesis is invalidated by the following experiment : MM. Franck and Pitres removed the cortical centre for the left fore limb in the right hemisphere of a dog. Four days later they exposed in the left hemisphere the centre for the right leg, and, exciting it, they obtained bilateral movements.

3. The third hypothesis is the most probable one. It is probable that there exist outside of the cerebrum, in the pons or the medulla, centres for motor association through which a unilateral excitation of a limited tract may be transmitted to the other side of the body, or even be generalized. It seems very probable that a similar mechanism acts in generalizing the attacks of partial epilepsy. M. Albertoni has shown that after the destruction of the cortical motor zone of one side, and the loss of excitability of the subjacent white substance, we may cause generalized epileptiform attacks in the dog by excitation of the hemisphere of the opposite side.

MM. Franck and Pitres have repeated this experiment, and have obtained the same results. In a dog six days after the complete ablation of the cortical centre for the left fore limb, they exposed the motor zone of the opposite side, and provoked an attack of partial epilepsy which became generalized, and in the course of which they noticed that the member whose cortical motor centre had been destroyed six days before was always very noticeably convulsed

These facts seem to show that convulsions of cortical origin are not produced directly by lesions of the brain, but that these lesions act on extra-cerebral centres, centres in which excitations of a very limited part may extend and generalize their effects. These explanations, moreover, leave to partial epilepsy all its clinical importance, and all its diagnostic value ; they only enable us to understand the complexity of the elements that come into play in its production and those that permit its generalization.

At the session of the Soc. de Biologie, Dec. 30th (rep. in *Le Progrès Médical*), MM. Franck and Pitres offered another communication on the effects of limited excitation of the bundles of the centrum ovale and the internal capsule. The study of the pathology of the centrum ovale of the hemispheres has led to the opinion that the white fibres which leave the excitable regions of the gray substance of the cortex, and connect them with the central ganglia, are grouped into distinct bundles, preserving throughout their course through the white substance of the brain, their functional independence. Numerous pathological observations show, in fact, in the most positive manner, that limited lesions of the centrum ovale may give rise to monoplegias, which certainly would not occur were not the fibres underlying the cortical motor centres grouped in separate fascicles

MM. Franck and Pitres have undertaken to test, experimentally, this hypothesis, and the result of their investigations has been to verify it. They exposed in a dog the motor zone, and after fixing accurately the location of the principal psychomotor centres, they made horizontal sections of the hemisphere, exciting the different uncovered portions of the white substance after each mutilation. In so doing, they always found that it was possible to obtain, by exciting sufficiently limited points of this white substance, isolated movements altogether similar to those obtained in the beginning of the experiment by irritation of the different regions of the gray substance.

At the base of the radiant crown of Reil, and in the internal capsule itself, the bundles of white fibres still preserve their functional independence. They are in juxtaposition without mingling, and the separate excitation of different bundles causes movements limited to certain muscular groups on the opposite side of the body. It is hardly necessary to state that to obtain these the excitations must be localized in very limited spaces; if the electrodes are more than from two to four millimetres apart, very extensive and confused movements are produced.

In the dog the anterior half of the surface of the internal capsule is alone excitable, and its bundles are grouped in the following manner :

1. Well in front we find the fibres, excitation of which causes movements of the face and eyelids of the opposite side; then we have from before backwards.

2. The bundle of fibres for the fore limb.

3. A rather extensive bundle, excitation of which, causes movements of both limbs of the opposite side.

4. A very minute bundle to the posterior member alone; and, finally,

5. At the horizon of the posterior portion of the nucleus caudatus we find a very well marked bundle of fibres, excitation of which causes the isolated movement of raising the ear of the opposite side.

These experiments confirm, as regards the physiology of the centrum ovale, the ideas suggested by careful study of pathological observations. As regards the internal capsule, they seem at first to be opposed by the fact that hemiplegias caused by lesions of this tract are always total. But in reality this disaccordance is only apparent. The internal capsule, in fact, is a narrow junction of various routes of conduction, and in order to obtain limited movements by its excitation, it is needful that the electrodes should be very closely approached to each other. But the pathological alterations

of the capsule, in the great majority of cases, are gross lesions, such as hemorrhages or patches of softening, and in such conditions the hemiplegia is complete, since the lesion is too extensive to involve only a single one of the many functionally distinct bundles of which the capsule is made up. It is highly probable that a very limited lesion of the capsule might give rise to a limited paralysis on the opposite side of the body. But cases of this kind are so extremely rare that no well authenticated case exists in our literature.

The existence of numerous functionally distinct fasciculi in the internal capsule, therefore, does not conflict in any way with the law laid down by Charcot, according to which central lesions extending into the internal capsule always produce a complete hemiplegia of the opposite side of the body. The physiological results of MM. Pitres and Franck, enable us to understand certain facts hitherto unsatisfactorily explained. Every one who has observed many cases of hemiplegia knows that it is not very unusual to see, in cases of total hemiplegia, a very marked predominance of the paralysis in the face, or in the members. One patient, who by the inertia, or rigidity of the members, is unable to leave his couch, can yet execute certain movements with his arm; another, whose arm is absolutely inert, or fixed by a secondary contracture into a permanent state of flexion, can nevertheless walk with comparative facility. Cases of this kind most frequently coincide with central lesions affecting the internal capsule, and are explained probably by the fact that not all the fibres that enter into its formation are affected in the same degree. An analysis of some observations collected during the past year in the service of M. Charcot, seem even to indicate the paralysis predominates in the superior member when the lesion is most marked in the anterior portions of the internal capsule, and in the leg when it affects especially the posterior part of its motor tract.

At the meeting of Jan. 26 (rep. in *Gaz. des Hopitaux*, No. 12), M. Franck presented in the names of M. Pitres and himself the results of their experiments as to the inexcitability of the lenticular nucleus and the exaggerated excitability of the internal capsule.

1. When we localize the excitation to the gray matter of the striate body, exposed by the ablation of a cube of brain substance, we provoke no movement. At the moment when the electrodes come in contact with the surface of the section of the internal capsule, the movement takes place with the characters of brusqueness and generalization already indicated. This fact has been frequently verified by MM. Pitres and Franck, and was evident from the tracings submitted to the society. It follows from it, that, when we apply the excitation to the corpus striatum, it is not the striate body itself that is excited, but the white fibres it contains. In fact, in pushing more and more deeply into the brain, electrodes isolated to their points, and in following the floor of the lateral ventricle, we find that the excitation remains for an instant without effect, and that the movement then occurs all at once and throughout the whole body: this motor explosion coincides with the instant that the electrodes touch the internal capsule.

2. When we compare the intensity of the motor phenomena, produced

by even slight excitation of the capsule, with the relatively feeble reaction caused by stronger excitations applied to the white bundles of the centrum ovale, we are struck with the hyperexcitability of the fibres at the horizon of the capsule. MM. Pitres and Franck offer the following hypothesis for the interpretation of the phenomena, which they will endeavor to test, and which already is supported by one rather conclusive experiment; the anterior portion of the internal capsule contains not only fibres from the cortical motor zone, but also those furnished by the corpus striatum constituting an adjunct system, whence the more violent reaction from equal excitations when we excite the capsule than when we excite the white fibres of the centrum ovale. In a dog, whose cortical centre for the left anterior limb had been removed for six months, and the corresponding fibres had lost their excitability, MM. Franck and Pitres produced movements in the left fore leg by exciting the capsule, that is to say, without doubt, the excitation of healthy fibres from the striate body. Further experiments are in progress and will be published.

H. Nothnagel (*Virchow's Archiv*, 71 Bd., Heft III.) treats in the present article of the results of division of the internal capsule in the rabbit. The anterior part of it, between the nucleus lenticularis and nucleus caudatus, was transversely divided with a protected knife.

Class A. Both nuclei lenticulares were previously destroyed by injection of chromic acid. When the characteristic symptoms, absence of all spontaneous movements, were developed, division of the two capsules was performed. The result was total paralysis of the anterior extremities. (According to the experiments there seems to have been also anæsthesia of these parts, but Nothnagel does not refer to that in his remarks.)

Class B. Simple division of both internal capsules. With the closest scrutiny Nothnagel could not detect any difference whatever between the operated animals and uninjured rabbits.

Division of one capsule alone gave rise to similar symptoms as the destruction of the corpus striatum, though of less intensity, viz.: distortion of the spinal column with the concavity turned towards the side of the lesion, and deviation of the extremities also towards the injured side.

Nothnagel therefore concludes that motor tracts pass both through the corpus striatum and the internal capsule, and that destruction of either of these parts alone does not suffice to produce any marked paralysis in rabbits.

Hermann Munk, *Berliner klin. Wochenschr.*, No. 35, 1877 (abstr. in *Revue des Sci. Méd.*)

On the 23d of March, 1877, the author communicated to the Physiological Society of Berlin the results of his first investigations. He extirpated, in medium sized dogs, disks of cortical substance, 15 millimetres in diameter, and 2 millimetres in thickness, from the surface of the parietal, occipital, and temporal lobes. In some cases he removed symmetrical portions of the two hemispheres successively, in others simultaneously.

The first fact deduced from these experiments was that the cortical surface examined included an anterior tract purely motor, and a posterior one

purely sensory. The line separating these might be drawn vertically from the sylvian fissure to the falx cerebri; ablation of parts in front of this line (parietal lobe) always caused motor disturbances, while those performed posterior to it produced nothing of the sort. On the other hand, extirpations of parts posterior to this line caused psychic blindness (*Seelenblindheit*) if located in the occipital lobe, near its postero-superior extremity, and a psychic deafness (*Seelentaubheit*) when in the temporal lobe near its inferior extremity. In the first case the animal lost the commemorative images (*Erinnerungsbilder*) of visual impressions; in the second, those of auditive impressions. Extirpation of parts in front or below the postero-superior extremity of the occipital lobe as well as those between this point and the inferior extremity of the temporal lobe, were without effect.

Munk also observed that the motor disorders and the psychic blindness disappeared progressively in the space of from four to six weeks, and ended in leaving no appreciable symptoms; the animals operated upon appearing in no way different from others. He was able also to follow very neatly the manner in which the blind animals regained their vision. This last circumstance led to the conjecture that the visual and auditory spheres extend rather beyond the occipital and temporal lobes, and that within them the commemorative images are arranged, according to the order of perceptions, around a central point, from which they extend toward the periphery, and after the extirpation of the above mentioned points which contain the whole or a majority of the images existing at the actual moment, the remainder of the tract in the vicinity supplies itself with new ones.

In continuing his investigations the author took a number of pups of the same litter, and on the fourth or sixth day after birth he destroyed in one-third of them the eye, and in another third the ear, in some of one, and in others, of both sides. The remaining third were not mutilated. A very small proportion of the pups operated upon, were hindered in their development, but the majority grew as fast or better than the uninjured ones. At the end of eight to eleven weeks he sacrificed all of the dogs which had developed normally, whether mutilated or not. In the blind dogs, the occipital lobe, already recognized as the sphere of vision, had suffered an arrest of development, the same was the case with the temporal lobe in the deaf dogs. On the other hand, the temporal lobe in the blind dogs and the occipital lobe in the deaf ones were exaggerated in volume so that the volume of the hemispheres was not notably diminished. In comparing the blind animals with the deaf ones it was noticed that the temporal lobe in the former was developed toward the falx, and the occipital lobe in the latter was most prominent toward the temporal region.

The various commemorative images have a fixed seat, localized in the cortex. Thus after extirpation of the visual centre at the upper posterior angle of the occipital lobe, Munk twice noticed, with the loss of all other commemorative images, the preservation of a single one in its integrity. In one dog it was the image of the bucket from which he was accustomed to drink, in the other it was that of the motion which made him offer his paw.

Finally in his last experiments the author has been able to note the gradual return of the auditive perceptions.

THE INNERVATION OF THE UTERUS.—Von Basch and Hoffmann, *Med. Jahrbuech.*, 1877, heft IV. (abstr. in *Gaz. Med. di Roma*) have studied the innervation of the uterus in the dog with some interesting results. They refer the uterine nerves to two sources: one set from the union of branches from the posterior mesenteric ganglion and the inferior hypogastric plexus, and the other, the *nervi erigentes* of Eckhardt, from the sacral plexus. In forty-nine careful experiments on curarized dogs the following were the principal phenomena observed:

Stimulation of the hypogastric nerves constantly caused movement of the cervix downwards, and at the same time, but not always, the mouth of the uterus opened; both of these being in the opinion of the authors, only passive results of the contraction of the circular fibres of the uterine neck, which was the immediate effect of the excitation of these nerves.

The *nervi erigentes* have undoubtedly a motor function. In many cases their stimulation produced shortening of the body and stretching of the neck and contraction of the vagina. It was remarked that during the stimulation a quantity of vaginal mucus exuded from the split vagina into the abdomen. The orifice of the uterus is always closed during the stimulation of these nerves; if open at the commencement, it closes. The authors consider the stretching of the cervix and closure of the mouth as passive results of the contraction of the longitudinal muscular fibres under the excitation of the *nervi erigentes*. These are therefore antagonists of the hypogastric nerves which act on the circular fibres. The irritation of the lumbar cord has the same effect as that of the *nervi erigentes*.

Suspension of the respiration produces a movement of the cervix and opening of the mouth, phenomena referred to irritation of the hypogastric nerves. This effect is inconstant, but blood charged with carbonic acid excites the hypogastric nerves more strongly and frequently than the *nervi erigentes*. Excitation of the sciatic causes, almost always complicated motor phenomena, but in most cases those dependent on the excitation of the hypogastric nerves predominate.

Although the movement of the cervix and the changes of form of the uterine orifice are thus shown to be dependent on these two sets of nerves, they may also occur independent of either of them. If the body of the uterus in repose is scratched with the finger or sound the cervix will advance and the mouth dilate. This is brought about in a reflex way from ganglia in the upper portion of the vagina.

The vessels of the uterus are innervated by the same nerves which supply its muscular fibres. The hypogastric nerves supply the vaso-constrictors and the *nervi erigentes* the dilators. Stimulation of the hypogastrics causes a disappearance of the arterial and capillary injection of the uterus, the veins remaining full, and during the irritation the flow from a cut on the cervix becomes less and stops. The opposite effect follows excitation of the *nervi erigentes*, the organ becomes strongly injected, wounds bleed more freely and the temperature of the body is slightly increased.

The vaso-constrictor fibres in the hypogastric nerves are derived from the splanchnic nerves; excitation of these nerves has the same effect so long as the hypogastrics are intact, but is ineffectual when they are divided.

Contraction of the uterine vessels may also be produced in a reflex way by

irritation of the sciatic. In this case it also seems to depend on the hypogastrics, for if these are cut we have, not a constriction, but an active dilatation due to reflex influence of the *nervi erigentes*. When these last are divided, irritation of the sciatic is without effect on the vascular condition of the uterus.

The Inhibitory Nervous System in Newborn Infants. Otto Soltmann, *Jahrb. f. Kinderheilk.*, XI. 101, 115 (abstr. in *Revue des Sci. Méd.*).

In a previous memoir entitled "The Functions of the Brain in the Newborn," the author attempted to prove that all the movements of newborn infants were involuntary, instinctive and automatic. He showed besides that even in the reflexes, the brain was inactive, and the reflex movements were entirely spinal in their origin. Thus the closing of the eyes caused by touching their surface, is not seen in newborn animals until after they have once learned the danger of the contact.

The brain in so far as it is the organ of thought, will and intelligence, appears to be absolutely inactive in the newborn, and the author was able to determine experimentally in very young animals, the absence of psychomotor centres in the cerebral cortex. Whence it is evident that they possess, as is generally admitted, an excessive predisposition to reflex movements, a *spasmophilism*, because the brain is, after a fashion, deprived of will, and cannot exercise any control over the reflexes.

But we know, moreover, that outside of the mechanism of arrest of the organ of thought, there are in the brain other centres of arrest analogous to those in the thalamus of the frog described by Setschenow.

In 1869 Simonoff discovered a similar mechanism in the anterior lobes of the hemispheres in mammals.

In irritating the parts in question with the galvanic current he was able to prevent reflex movements. But Soltmann, repeating Simonoff's experiments on newborn animals, discovered that these centres of arrest were lacking in them. The brain of the newborn animal is incapable of transmitting to the cord the excitations designed to moderate the reflexes which have their seat in the latter organ. This fact is evidently very significant as regards the pathogeny of convulsions.

It remains now to see how the cord comports itself as regards the centres of arrest in the newborn. Already Goltz and Nothnagel, and later Levisson, by dividing the cord just below the medulla were able to demonstrate that the reflex movements of adduction or extension backwards of the feet, which we observe in animals when we lightly touch the arms or claws, do not appear when we pinch the limbs just before the groin or the trunk or when we bandage them. But Soltmann shows that under the same circumstances in newborn animals these movements instead of being abolished are, on the contrary, exaggerated. The apparatus of arrest in the cord does not exist in the newborn.

On the other hand, Soltmann, exciting galvanically the vagus nerve or the inhibitory nerve of the heart in animals of a certain age and in those newly born, observed that in these last the nerve was much more slender, it was needful also to employ much more intense currents than in the former,

to produce a slowing of the heart, without moreover succeeding in arresting its contractions.

The energy of the vagus is therefore insufficient in the newborn, and this is undoubtedly the reason why there is in them such an excessive frequency of the cardiac pulse as compared with adults, and why it undergoes such extensive alterations. We may explain also by this the frequency and the irregularity of the pulse in the newly born, and that other fact of daily observation, viz. : that we see under every slight influence a notable acceleration of the movements of the heart. The considerations are applicable to the irregularity and variable frequency of the respiratory movements in the newborn.

It is well known that in the basilar meningitis of infants of a certain age, there is in the first period a very characteristic slowing of the pulse due to irritation of the vagus by the exudation, and in the third stage of the disease an enormous acceleration due to the compression of the fibres of the vagus by the exudation and the consequent paralysis of the nerve. But in the newly born infant these symptoms are different from the usual type ; from the commencement of the trouble the pulse does not cease to be irregular and intermittent, a peculiarity which explains sufficiently the condition of the nervous system of arrest in very young infants, and which has given rise to errors of diagnosis.

Collateral Innervation.—This term is applied by S. Stricker (*Wiener Med. Jahrb.*, 1877, § 415) to the gradual restoration of vascular tonus after the destruction of the greater part of the vaso-motor nerves of that district.

On dividing the cord of a dog in the lower dorsal region, there follows a hyperæmia of long duration of both hind legs. This is due to the section of vaso-constrictor nerves, whose centre is higher up in the cord. If it were dependent on the traumatic irritation of vaso-dilator nerves, hyperæmia would only remain a few minutes. This hyperæmia passes off in the course of a few days. The restoration of vascular tonus is an instance of collateral innervation ; it is due to the activity of nerves, whose centre is higher up in the cord, and which leave the cord above the point of section. The proof of this is furnished by the fact that a second section of the cord at the level of the sixth rib, gives rise to a new and permanent hyperæmia of the hind legs. The same result may be obtained by division of the sciatic nerve, in which case also the still intact vaso-motor fibres are severed from their vessels. The reason why these accessory nerves do not enter into activity immediately after section of the principal vaso-motor strands, Stricker has not yet been able to explain. (*Centralblatt*, 1878, No. 3.)

Nerve Tubes of the Plagiostomi.—Gustaf Retzius, *Nordiskt Med. Arkiv.* IX., IV., No. 23, 1877, has investigated the structure of the nerve tubes in the rays. Contrary to the description of M. Ranvier, according to which, of the nuclei, three or more of which are found in each segment of the sheath of Schwann, only one properly belongs to it, the others to a special external envelope, Retzius found all the nuclei situated on the internal face of the sheath of Schwann, with the sole exception of those that pertain

to the homologue of the fibrillary sheath and are scattered here and there among the tubes. He proves, moreover, that an evident analogy exists between the situation and the reciprocal distances of these nuclei, and the dispositions that Axel Key and the author have already described in the nerve tubes of the pike. The nerve tubes of cartilaginous and osseous fishes are therefore constructed according to the same fundamental plan in all these relations.

The Physiology of the Medulla.—MM. Laborde, Matthias Duval and Graux have undertaken a series of researches for the purpose of clearing up certain obscure points in regard to the physiology and structure of the medulla, and particularly: 1. On the associated movements of the eyes. 2. On the trophic phenomena and sensibility of the eye, of the nose, the ear and the face in general.

M. Laborde, in giving the results of his researches to the Society of Biology, insisted first on the difficulties of experimentation in the region of the nervous axis; so much the greater since, when we have to do with such delicate observations as those of the ocular movements, modifications of the sensibility of the eye and nostrils, etc., previous mutilations needful to expose the bulb will not answer, the functional integrity of the subject is needful. To explore the medulla experimentally, not being able to fully expose it, we are forced to get at it indirectly through the occipito-atloid space. It is moreover impossible, in this case, to explore the floor of the fourth ventricle without touching the cerebellum. A mere glance at the anatomical topography of the part is enough to demonstrate this. But it is easy to clearly separate the functional disorders attributable to lesions of the cerebellum from those due to lesions of the medulla.

M. Laborde selected for his experiments a triangular point when he wished to make a simple puncture, or a narrow blade when he wished to make a section, but whatever it was, the instrument had to be mounted on a long and narrow handle.

The results of these investigations relate to the majority of the functions of the medulla, either as it is an autonomous centre, or a centre of origin of the nerves.

As an autonomous functional centre, M. Laborde only speaks of it in its capacity as influencing the respiratory mechanism; in many experiments, even at the moment of the production of the lesion, there was observed such an instantaneous arrest of the respiratory movements, as to lead to the belief that the so-called vital point had been touched. But the hand over the precordial region perceived very distinctly the cardiac pulsations which continue with their usual force, rhythm, and frequency, and then, after a few seconds, one or two minutes or more, the respiratory movements gradually take on their amplitude. This respiratory syncope has been in some cases long enough to cause the complete asphyxic transformation of arterial into nervous blood. The experimental lesion in these cases is always in the inferior portion of the floor of the fourth ventricle, very near, or on the point of the calamus around the raphe. Successive sections made at the horizon of the lesion, show that it principally involves the points of origin

of the pneumogastric. M. Laborde and his colleagues will take up this line of research at a later date.

M. Laborde comes next to the influence of the medulla upon the associated movements of the eyes. An attentive observation of these various ocular deviations, enables us to classify them into two distinct groups.

1. The first group includes the disassociated or asynergic deviations in which there is a complete rupture of equilibrium of the associated ocular movements; it is a veritable ataxia or inco ordination of these movements. This group comprehends all the cases of double convergent or divergent strabismus, especially the last, and all in which one of the eyes is directed upward, the other downward, one directed outward and upward, the other inward and downward, etc., and *vice versa*. These have been always the result of a lesion of the cerebellum. Nevertheless, an analogous result may be observed when it affects the lateral columns of the medulla (restiform bodies), which is to be explained by the fact that it is the prolongation of the cerebellar fibres that in this case are affected. The disassociated deviations, those which constitute a veritable ocular ataxia, are essentially in the functional domain of the cerebellum.

2. The second group comprises the associated, synergic, conjugated deviations. The conjugated strabismuses are nothing else than a more permanent form of the usual associated movements of binocular vision.

After some anatomical and physiological considerations on the different pairs of ocular muscles, and on the mechanism of associated movements in the physiological condition, M. Laborde deems that he can sum up the results obtained in the following propositions: Every time, says he, that the experimental lesion affects a part sufficiently near, in its inferior limit, to the nucleus of the sixth pair, to touch that nucleus, and especially whenever it directly occupies that nucleus, there is immediately, even at the moment of the production of the lesion, conjugate deviation or strabismus, constantly, in these cases, accompanied by nystagmus, and the more or less pronounced turning of the head toward the injured side.

This is the chief result, but this proposition implies two distinct orders of facts; a first, in which the conjugate deviation is produced by functional irritation, and a second in which it is due to paralysis. In the first the deviation is produced toward the side of the lesion, which has not destroyed the nucleus, or separated it from its radicular fibres, and which acts only by irritation. In the second case the deviation is toward the opposite side from the lesion, which has completely destroyed the nucleus, or cut off communication with its radicular fibres, producing motor paralysis.

The difference in the deviation in these two cases is easily explained: in the first, the rectus externus, under the influence of the excitation of the nucleus of the sixth pair, which innervates it, contracts more or less violently and carries outwards the corresponding orbit (divergent strabismus), while the rectus internus of the opposite side, by its morbid contraction under the influence of the same functional irritation draws the eye inwards (convergent strabismus). The result is a conjugate strabismus to the right if the lesion is on that side, and to the left if on that side. In the second case, on the other hand, the same muscles become paralyzed by the complete section of the fibres by which they are innervated, the antagonistic

muscles act, and the conjugate deviation toward the side opposite the lesion. According to this a double lesion affecting at once the right and the left sides ought to have a negative result as regards the ocular deviations, and this is in fact what occurs.

The influence of the internal recti over the conjugate deviation of the eyes is not usually so great as that of the external recti, which seems to show that it is more easy to affect by an experimental lesion the radicular fibres proper of the external motor oculi than the original anastomotic fibres of the motor oculi communis; or rather, that there is a marked predominance of the first over the second.

However it may be, the conclusion which is clearly drawn from these experimental facts, is that the nucleus of origin of the sixth nerve sends anastomotic fibres to the common motor oculi of the opposite side, and that it is to this arrangement that the functional association of the eyes in binocular vision is due. The results of experimental physiology confirm those of anatomy. Clinical observation also supports this demonstration, as was proven by a case reported later by M. Graux. *Gaz. des Hopitaux*, Dec. 9.

In a later communication (*Gaz. des Hopitaux*, No. 2, 1878), M. Laborde gives the results of the investigations of M. Duval and himself, in regard to the trophic and sensory disorders following the experimental lesion of the descending root of the trigeminus. If the lesion is made in the nucleus of the sixth, it is very difficult to avoid injuring that of the facial nerve also, and producing facial paralysis. Now if the injury is made just a little toward the thickness of the lateral pyramid of the restiform body, another set of symptoms appear, conjunctival congestion and inflammation coming on with great rapidity, the eye being completely destroyed in the rabbit in twenty-four hours. The same trophic trouble is observed in the pituitary membrane and the lining membrane of the middle ear. These phenomena indicate that at the injured point exist radicular fibres which preside over nutrition in these parts, and these fibres are exactly those of the descending branch of the fifth nerve, demonstrated by M. Duval. To determine the exact inferior limit of these fibres MM. Laborde and Duval made repeated sections of the medulla from above downward, and found that they did not extend below the point of the calamus, if, indeed, they extend so far.

The following is the summary of the results of all these investigations:

1. That the medullary nucleus of the sixth nerve contains, and sends anastomotic fibres to the nucleus of the corresponding nerve of the opposite side.

These fibres by unifying and rendering synergic the functional exercise, that is, the simultaneous contraction of the external rectus of one side and the internal rectus of the other, insure the associated movements of the eyes in binocular vision.

These associated movements appear to have their functional centre of origin in the medulla, in that part where the said nucleus exists, while it is in the cerebellum or the medullary prolongations of the cerebellar fibres that the principal co-ordinating power for the ocular movements appears to reside.

2. The constant production of complete anæsthesia and trophic dis-

orders of the eye after experimental lesion, affecting seriously the upper half of the lateral pyramids, demonstrates the existence of medullary fibres belonging to the descending root of the trigeminus.

Reflex Inhibition of Salivary Secretion.—John Pawlow, *Pflüger's Archiv.* LX., IV. and V., Jan., 1878.

In agreement with previous experimenters Pawlow found the salivary secretion of the curarized dog increased by strong irritation of the sciatic nerve. But on *diminishing* the strength of the exciting current, a point is reached at which irritation of the sciatic nerve inhibits the action of the gland. A similar inhibition is observed by exposure of the peritoneal cavity and by raising a portion of the gut out of the cavity. This effect is due to inhibition at the central origin of the chorda tympani, since excitation of the lingual nerve, which otherwise results in copious reflex salivation, is of much less influence on the gland during the continuance of the inhibitory irritation. The author also states that curare, known to increase salivation, will finally check it in very large doses.

Effects of Section of the Spinal Cord upon Animal Temperature.—We copy the following from the New York *Medical Record* of Jan. 19:

Some time since the sum of five hundred dollars was granted to Professor H. C. Wood, Jr., of Philadelphia, by the Smithsonian Institution, to enable him to prosecute with thoroughness and success a series of experiments relative to the effects of artificial lesions of the various parts of the nervous system upon the evolution and production of animal heat; the results of the experiments to be published in the contributions of the Smithsonian Institution.

Through the kindness of Dr. Wood we are enabled to lay before the medical profession his results, so far as concerns *the section of the cord* upon animal heat.

As is well known, experiments of much the same nature as those which we are about to describe were instituted in 1837 by Sir Benjamin Brodie, and later by Bernard, Schiff, Naunyn and Quincke, Rosenthal, and Binz. According to these authorities it would seem that if the spinal cord of a rabbit be cut in the lower cervical region (the air of the room being lower than the temperature of the animal's body), the animal's temperature at once falls so that at the moment of death its heat is below normal. If, however, the external temperature be high, and the animal be wrapped in raw cotton, the fall of temperature is succeeded by a rise, so that the animal dies in a state of intense fever. To settle this point a crucial experiment was undertaken by Naunyn and Quincke, which seemed to prove beyond a doubt that the rise of temperature was to some extent certainly dependent upon the operation.

In conducting his researches, Dr. Wood has confined himself exclusively to experiments upon the dog, other commonly employed animals being too small for the performance of delicate experiments upon the brain and nerves.

A wooden box was constructed, open at the top, and about three feet square; also two metallic boxes, the larger of them also open at the top, and three inches smaller each way than the wooden box; the other about three inches smaller each way than the first and having its top piece perforated with three holes,—a large hole in the centre, and, on each side of this, smaller apertures into which gutta-percha tubes could be screwed. The larger metallic box was then placed inside of the wooden chest, and the interstices surrounding it carefully and tightly packed with sawdust. Water enough was poured into it—*i. e.*, the larger metal box—completely to surround the smaller metal box when placed inside of it. After cutting its cord, the dog was then placed within the innermost box, the gutta percha tubes were screwed into place, and a closely fitting lid to the whole apparatus so adjusted as to cut off both the dog and the water surrounding the vessel which held it from the outer air of the room. A suction pump was attached to the end of one of the gutta percha tubes (the end of the other tube being left patulous) so that air might be drawn through the metal box which held the animal. A cut-off from the tube of exit was so arranged that specimens of air could be received into it, and analyzed for heat by a thermometer, and for moisture by a calcium tube. The difference in the number of heat units existing in the air, after its passage through the box, and in the water surrounding the box which held the dog whose cord was cut, was then estimated by means of the following formula: Q=w t sp. h.; Q=heat units; w=weight of air or water; t=rise of temperature of air or water; and sp. h.=specific heat.

The bulk of the air was reduced to the bulk of air at 32°. This was weighed and the specific heat obtained. After a number of painstaking and laborious experiments, Professor Wood has been able to reach the following conclusions, viz.: *At a low external temperature, after section of the cord, there is increased evolution with diminished production of heat; at a high external temperature, both the production and evolution of heat are diminished. After section of the cord the animal dies in winter of cold, and in summer lives much longer on account of the high external heat.* It will be at once noted that this fact has a very important application to narcotic poisoning, to collapse, as seen, after exhaustion by heat, etc., and to thermic fever; the hot bath being the best treatment for collapse and narcotic poison, and the cold bath for thermic fever. (Collapse from heat and thermic fever are usually confounded. In collapse the internal temperature is below normal, while in thermic fever it is above.)

Professor Wood has also discovered the fact that the instant fall of temperature, after section of the cord, is due to *vaso-motor paralysis, producing paralysis of the arterioles at the surface, thus obliterating the superficial cold layer of the body, and removing the only barrier to the animal's internal heat.* The Doctor has thus shown that *temperature is no guide to the amount of heat given off in such cases, and that fever simply reduces itself to a question of the difference in the relation of the heat-producing and heat-elevating powers; that the heat-elevating power is no longer compensatory to the heat-producing when the cord is cut.*

The experiments of Owsjannikow, which located the governing vaso-motor centre of the brain in a space whose upper boundary is one or two millimetres below the corpora quadrigemina, and whose lower boundary is from

four to five millimetres above the point of the calamus scriptorius (*Berichte*, etc., Bd. XXIII.) have received full confirmation at Dr. Wood's hands.

Vaso-motor Centres in the Cortex.—Dr. B. Kuessner, *Centralbl. f. d. Med. Wissensch.*, No. 45, 1877, announces that he has performed a great number of experiments to test the question of the existence of vaso motor centres in the cortex of the hemispheres, as described by Eulenberg and Landois (*Virchow's Archiv*, LXVIII., and Eulenberg, *Berliner klin. Wochensch.*, Nos. 42, 43, 1876), and which were confirmed by Hitzig in a short note in the *Centralblatt*, 1876, No. 18, but had obtained throughout only negative results. A full account of his experiments will be published in the *Archiv fuer Psychiatrie u. Nervenkrankheiten.*

Nerve Terminations in the Tactile Corpuscles.—At the session of the French Academy of Sciences, Nov 26, 1877 (rep. in *L'Union Médicale*), M. Cl. Bernard presented, for M. Ranvier, a communication on the distribution or rather the termination of the nerves in the tactile corpuscles. Up to the present, it has been believed that the sensory nerves entered into the cutaneous terminal cells, in the form of a swelling or tumefication, and that the sense of tact is exercised directly on these cells. M. Ranvier thinks that this is not the case. By coloring the final nerve ramifications black with gold salts he had seen terminal filaments ending between two cells without entering them; there was no continuity between the cells and the nerves. The cells play the part of organs of protection or reinforcement. This remains to be studied. At present, the question is a purely anatomical one, and may be formulated as follows: The nerves terminate by free extremities between two cells, and, consequently, we always find more cells than nerve terminations. It was in the bill of the duck, a very sensitive organ, that M. Ranvier had brought to light this arrangement.

Clarke's Columns.—Previous researches have rendered it probable, that the so-called lateral cerebellar fibres which connect Clarke's columns with Flechsig's lateral cerebellar tracts, take their origin in the cells of the columns of Clarke. But this supposition has not yet been fully proven. This has been now accomplished by Dr. A. Pick (*Centralblatt*, 1878, No. 2). In the normal cord of a child of one and one-half years, hardened in bichromate of ammonia and colored with carmine, he found the cells of the columns of Clarke (in the lumbar portion) giving rise each to one process directed forward and inward. This process soon changed its course and joined the horizontal cerebellar fibres.

Pick points to the importance of the simultaneous occurrence of degeneration in the lateral cerebellar tracts and of partial atrophy of the columns of Clarke, observed in some cases on record.

Nerves of Sweat-Glands.—F. Nawrocki (*Centralblatt f. d. Med. Wiss.*, 1878, No. 1 and 2), repeated and extended the experiments of Luchsinger Like Luchsinger he operated on young cats and traced the course of the

nerves both by direct irritation and by section with subsequent excitation of the centre by warmth. As the result he found in the medulla oblongata a centre for the sweat-nerves of both fore and hind legs. From this the fibres for the anterior extremity follow the cord, leave it at the fourth dorsal vertebra, enter the ganglion stellatum and thence pass into the brachial plexus. Following the ulnar and median nerves they are found inequally distributed in these two nerves in different animals. The fibres going to the hind legs pass in the cord up to its lumbar portion, thence enter the abdominal sympathetic and finally join the sciatic nerve.

In the following number of the *Centralblatt* (No. 3), Luchsinger mentions that he has also traced the fibres of the fore leg along the same paths.

The following are the titles of some recent memoirs on the Anatomy and Physiology of the Nervous System :

Ott, Nerve Stretching; Its Physiological Action, *Ohio Med. and Surg. Journal*, Feb.; The Effect of Electrotonus on the Rapidity of the Transmission of Nerve Force, *Phil. Med. Times*, Feb. 2; Dietl and Vintschgau, The State of the Physiological Reaction-period under the Influence of Morphine, Coffee, and Wine, *Pflueger's Archiv*, XVI., VIII., Feb.; Kowalewski and Nawrocki, Sensory Nerves of the Muscles, *Centralbl. f. d. Med. Wissensch.*, No. 9; Luchsinger, On the Sweat Secretion, *Ibid.*

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

Sensibility in Tabes dorsalis.—E. Remak (*Arch. f. Psych.*, etc., VII., II. 3) observed the following new phenomenon in the case of a man afflicted for years with ataxia. On searching for the minimum current, just sufficient to produce the feeling of formication on applying the faradic brush to the skin of the thigh, he found, that the sensibility at first quite acute, soon became exhausted. This fatigue necessitated such an increase in the strength of current for perception, that finally the current, in order to be at all perceptible, had to be quite painful. This peculiar ready exhaustibility of the tactile nerves, existed also for the sensations of contact, pressure and temperature. Since Remak found this condition likewise in the plantar skin, he suggests that it may be the cause of the frequent complaint in ataxia of an *elastic* sensation underneath the feet. (*Centralblatt*, No. 3, 1878.)

Epileptic Attack from Action of Vagus, by Dr. O. Langendorff and stud. R. Zander. (*Centralblatt*, No. 4, 1878.)

While it is difficult to obtain complete arrest of the heart in the normal rabbit by irritation of the peripheral end of the divided vagus, this result

is easily obtained after the use of chloral. On irritating the nerve in animals whose voluntary movements had been suppressed by a dose of about 0.5 grm. of chloral, Langendorff and Zander observed the following result: The heart being arrested in diastole, there occurred after the lapse of ten or fifteen seconds a complete epileptic attack. All details corresponded exactly to the description by Kussmaul of epileptic fit caused by hemorrhage or cerebral anæmia. The latter is evidently the cause of the phenomenon similar to ordinary anæmic epilepsy; these spasms also are prevented by complete narcosis. Hence the above dose of chloral cannot be exceeded.

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Intra-ventricular Hemorrhages.—C. Gallopain, *Thèse de Paris*, 1877 (*Ann. Méd. Psychologiques*, Nov. 1877). The author studies the intra-ventricular hemorrhages of cerebral origin for the purpose of determining their value and exact signification, in short to elucidate the pathogeny of the symptom of contracture which we usually observe in these cases. We cannot do better than to give the conclusions of his paper.

The hemorrhagic *foyers* in the optic thalamus are the ones that are most frequently accompanied with inundation of the ventricles.

Convulsive phenomena occur in more than half the cases of ventricular hemorrhage.

There is no relation as of cause and effect between the ventricular inundation and the convulsive symptoms

The appearance of these latter accidents only indicates the more or less intimate relations of the foyer with the excito-motor centres.

Finally, in the great majority of cases of intra-ventricular hemorrhages accompanied with convulsive phenomena, these latter are due to an irritation of the cerebral peduncles, an irritation which is often a direct one.

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Writers' Cramp.—At the meeting of the Royal Medical and Chirurgical Society of London, Feb. 12 (rep. in *Brit. Med. Journal*), Dr G. V Poore read a paper entitled, An Analysis of Seventy-five Cases of "Writers' Cramp" and Impaired Writing Power. The report of his paper is as follows:

In seventy-four of these cases, the condition of the hand completely overshadowed any other disease, whether general or local, from which the patients at the time were suffering. Most of the cases merited the name of "Writers' Cramp" or had been so called; but the author purposely included a few cases which obviously did not merit the name. The cases fell naturally into six groups: 1. Paralytic (six cases); 2. Spasmodic (five cases); 3. Degenerative (nine cases); 4. Neuralgic or Neuritic (nineteen cases); 5. Writers' Cramp (thirty-two cases); 6. Anomalous (four cases). The cases were arranged in a tabulated form. It was shown that, since the ulnar nerve supplies thirteen and a half out of the eighteen intrinsic muscles of the hand, its integrity is very necessary (more necessary than that of any other nerve of the hand) for all delicate manipulation, especially writing. The spasms which affect the hand, and which are particularly prone to follow attacks of hemiplegia, owe sometimes, there is good reason to believe, their character, if not their origin, to a faulty antagonism (due to a secondary paralysis or

paresis) among the muscles of the paralyzed limb. Although it is commonly received that such spasms are due to disturbance of the gray cerebral matter, it is well to look also to the peripheral aspects of the question. Provided a nervous impulse issuing from the brain be distributed in a limb to equally irritable muscles which mutually antagonize each other, it is difficult to conceive that spasm of definite form should be produced; but should the equilibrium of antagonization in the limb be destroyed by a secondary lesion, the production of definite spasm is easily conceivable, especially when voluntary control is lessened by a lesion of the central ganglia. In some cases of localized spasm there is no evidence of central change, and it is theoretically possible that the action of a disordered centre on a healthy periphera and the reaction of disordered periphera on a healthy centre may be identical in their results. Loss of writing power is often the first and most prominent symptom of degenerative change occurring in the spinal cord or brain. The neuritic or neuralgic group is characterized by a painful and tender condition of the nerves of the limb, which may be induced solely by overwork, but more frequently by a strain or similar injury combined with exposure to cold and a depressed state of health. Of the nineteen cases of this group twelve were females. Any attempt to use the arm either for coarse or fine acts produced fatigue, pain, and neuralgia. It is not always easy to distinguish these cases from true "writers' cramp," and, indeed, there cannot be said to be any hard and fast line between the two groups; but it is characteristic of the neuralgic group that—1. The symptoms involve a wider area; 2. The symptoms are sometimes induced without excessive exercise of any function; 3. Nerve tenderness or neuralgia is a prominent symptom. In the group of true "writers' cramp" considerable care is necessary to detect peripheral evidence of mischief; but the author stated that, in every case of impaired writing power which he had seen there had been evidence more or less marked of derangement of one or more of the muscles used for writing. This evidence consisted of—1. Obvious failure to use certain muscles efficiently either for writing or for some other less complicated act; 2. The occurrence of consentaneous movements or tremor when certain muscles were put in action. 3. Depressed or exalted electric irritability; and 4. The occurrence of sensory derangement or nerve tenderness. The muscles which are most frequently involved are those of pen-prehension rather than those of pen-movement. Reviewing the cases as a whole, attention was directed—1. To the inferences which may be drawn from an inspection of the handwriting; 2. To the fact that joints were found to be implicated no less than twenty-one times, the joint affection being rheumatic, neuropathic, gouty, or due to strained position; 3. To the fact that a difficulty is not very infrequently hereditary, or developed very early in life; and that, 4. Any evidence of involvement of the nerve-centres is decidedly rare. "Writers' cramp" has been spoken of as a disease of "faulty co-ordination" and there can be no doubt that such is the case, for it is evident that the muscles used for writing fail to work orderly together. We are not, however, justified in assuming the existence of a special co-ordinating centre for the controlling of the act of writing; and the author had been unable to find evidence that this centre (supposing it to exist) ever gives way, leaving the periphera, except for the special co-ordinated act, in a state of perfect health. The exist-

ence of such a centre appeared to the author to be improbable, for the following reasons: 1. He had never seen a case of writers' cramp without peripheral evidence of change; and in the majority of instances there had been no evidence of change other than peripheral. 2. If there be a co-ordinating centre for writing, it must be created as it were by education. The co-ordination of writing, which we are many years in acquiring, must be distinguished from those co-ordinated movements (such as the symmetrical movement of the two eyes) which are wholly independent of education. The fact that no two people hold their pens exactly alike, and that it is scarcely more difficult to write with the toes than with the fingers is much against the probability of there being a writing centre. 3. Writers' cramp is never suddenly established as aphasia sometimes is. 4. It is almost certain that a purely peripheral lesion may cause all the symptoms of writers' cramp. 5. The fact that the left hand (if used for writing) sometimes suffers as well as the right, is no evidence that the change is central. In previous writings, the author had spoken of "writers' cramp" as a "fatigue disease," and he was still inclined to adhere to the word "fatigue" as a convenient expression for an easily recognizable and familiar condition, of the pathology of which we are uncertain. He was inclined to think that occasionally fatigue is the expression of hyperæmia or mild inflammation of a motor nerve, and that the same condition may be produced either by overwork or by accidental causes, such as cold, strain, "rheumatism" or injury. Fatigue especially attacks those muscles which are subjected to prolonged strain; and it is probable that the relative frequency of "writers' cramp," as compared with other professional ailments, is due to the fact that prolonged strain of certain muscles (those which hold and steady the pen) is inseparable from the act of writing. Finally, as to the position of "writers' cramp" in the catalogue of diseases, the author would feel inclined to class it with neuralgia, that is, with a disease the phenomena of which are purely local, but which we recognize as being due not only to conditions affecting the sensory area involved, but also to molecular change affecting any part of the sensory fibre, whether before or after its function with the nerve-centre. The author concluded by laying down certain principles of treatment for the various forms of impaired writing power.

Spinal Hemiplegia.—H. Köbner, *Deutsch. Arch. f. klin. Med.* (abstr. in *Revue des Sci. Méd.*) The author analyzes the 48 cases of spinal hemiplegia which he finds in medical literature. He adds to these two, before unpublished cases of his own, and from all these facts draws conclusions not notably differing from those formulated by Brown-Sequard.

On the side corresponding to the medullary lesion, are,

1. Motor paralysis, complete or incomplete, according to the gravity of the case.

2. Hyperæsthesia to touch, tickling, pain and temperature in the paralyzed side.

3. Anæsthetic zone in the region whose nerves rise just below the lesion.

4. A larger or smaller hyperæsthetic band above the anæsthetic zone.

5. Absolute or relative elevation of temperature in the paralyzed parts,

and, also, frequently in the parts where there is hyperæsthesia without paralysis.

6. Paralytic phenomena in the cervical sympathetic when the lesion is situated above the cervical enlargement.

On the side opposite the lesion we find,

1. Complete anæsthesia to touch, tickling and temperature.
2. Preservation of voluntary motion and muscular sensibility.
3. Narrow band of hyperæsthesia below the anæsthetized part.

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Syphilitic Nervous Disease.—Dr. Hugo Engel, in a paper on the diagnosis and treatment of syphilitic nervous affections, *Phila. Med. Times*, Dec. 22, 1877, sums up in regard to the former of these points as follows:

Syphilitic nervous affections which simulate grave and often incurable diseases from other sources, namely, acute meningitis, apoplexy from other causes, epilepsy, softening of the cord, etc., are recognized as specific in their nature, even if this nature be denied, by the following points:

1. The aspect of the case, as recognized by the features, the pulse, the temperature, the violence of symptoms, lacks that gravity which would attend a non-specific case of the same character.

2. There is total absence of any history, as usually elicitable in such affections, when not produced by lues, or the history points to certain warning symptoms like headache and vertigo, worse towards night, irritable temper, numbness, etc., totally different from similar cases of a non-specific nature. The patient either does, or will not know any causing element.

3. In paralysis the electro-muscular contractility is never totally abolished; and if it would seem so, it is quickly re-established under a specific treatment that would never have that rapid influence in non-syphilitic cases.

4. There is no wasting of tissue for a long time in the affected limbs.

5. The spleen is invariably enlarged.

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The following are some of the recently published papers on the Pathology of the Nervous System and Mind:

Reclus, The Paralytic Luxations of the Femur, *Revue Mensuelle*, March; Oudin, Atrophy of the Convolutions connected with Default of Usage of a Member, *Ibid*; Gasquet, On the Causes and Treatment of Drowsiness, *Practitioner*, March; Doutrebente, The Different Species of Remissions that occur in the Course of Progressive General Paralysis, *Annales Méd. Psych.*, March; Garimond, Contribution to the History of Epilepsy in its Relations to Mental Alienation, *Ibid.*; Shaw, Progressive Muscular Atrophy and its Pathology, *Proc. Med. Soc. of County of Kings*, April; Michetti, Herpetism and Insanity, *Archivio Italiano*, XV., I. and II., Jan. and March; Charcot, On Rhythmic Hysterical Chorea, *Progres Med.*, No. 7; Erb, An undescribed Form of Chronic Atrophic Spinal Paralysis (Poliomyelitis anterior chronica,) *Centralbl. f. Nervenheilk. Psychiatrie u. Gerichtl. Psychopathologie*, No. 3; Smoler, Meniere's Disease, *Ibid.*; Erichsen, Lectures on Injuries of the Head, *Lancet* (*Am. Rep'r*), March; Seppilli, Contribution to the Study of Cerebral Syphilis, *Rivista Sperimentale*, III., III. and IV. Oulmont, On Athetosis, *Revue Mensuelle*, Feb.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

CHLORAL.—Dr. Oscar Liebreich, *Lancet* (Am. Repr.), Oct., reckons the normal dose of chloral for simple insomnia at from 1·25 grammes to 2 grammes (= 19.3 to 35.8 grains). Of course there are individual peculiarities; thus, persons who can take a large quantity of alcohol, and are accustomed to its use, require a considerably larger dose, and weak and debilitated persons considerably less. It differs from opium and its alkaloids, in that it does not produce a tolerance, it produces the same effect on the organism after continued use as at first. The dose for continued use varies; for some, one gramme (= 15.4 grains) is enough, for others much more.

The unfortunate accidents that have been reported from the use of moderate doses of chloral, he attributes to impurity of the preparation, and counsels care as regards this point.

E. KAHLE, *Centralblatt f. d. med. Wissensch.*, publishes the results of experiments performed by him on frogs in Von Wittich's laboratory at Koenigsberg, as follows:

1. Of two equal doses of chloral hydrate, estimated just sufficient to produce narcosis, a moderately acidified solution acted as perfectly, but a little slower than the neutral one.

2. In lethal doses this difference in quickness of action of the acid and neutral solutions was observed. The pulse and respiration were retarded together with, finally, stoppage of the heart in diastole. The heart still reacts to mechanical irritations with a single contraction, for some time after the stoppage.

3. The appearance of narcosis is more rapid after direct introduction of the agent into the blood than after its injection into the lymph sack.

4. Relatively large doses of chloral hydrate (0.2 grammes and over in the frog) injected into the median abdominal vein, or into one lateral vein, cause within a few minutes a stoppage of the heart in rigid systole (the auricle ought to remain dilated in diastole). In spite of this absolute stoppage of the heart the respiration and reflex irritability are perfectly retained until the narcosis is complete.

5. If a solution of chloral hydrate, some 0.15 or over strong, is penciled over the external unwounded skin of the frog, narcosis and, later, a stoppage of the heart results. If, then, a solution of the strength of 0.2 is injected into the lateral vein, the heart passes by awkward and arhythmic contractions from the diastolic stoppage into a condition of systole.

6. If the heart is stopped in diastole by the application of a minimal quantity of muscarine on the outer integument of the frog, and afterwards the above mentioned chloral solution of 0.2 is injected into a lateral vein, the diastolic stoppage changes to arrest in systole.

7. By penciling the outer integument of the frog with a half gramme of a solution of atropia (1.0 : 100.0) a quickening of the cardiac action is produced (paralysis of the inhibitory centres); if a frog in this condition is treated (penciling on the outer skin) with a chloral solution, the already atropinized heart is found to be much more resistant to the action of the chloral than is that of the frog still unoperated upon. The arrest in diastole in this case appears much later.

The author is still engaged upon further experiments in this direction.

Dracontium.—Dr. H. C. Wood, Jr., *N. Y. Med. Record*, Dec. 15, has been testing the use of dracontium, or skunk cabbage, in chorea. He uses 60 to 90 drop doses of a tincture of the fresh root three times a day and has had good results.

The effects of the drug, he thinks, are due to some volatile principle contained in the plant. The fresh roots gathered in the fall should be made up at once into a tincture before drying.

Cannabis Indica.—Dr E. C. Seguin, *N. Y. Med. Record*, Dec. 8, 1877, records the use of cannabis indica as a method of treating the disease (not the attacks) known as migraine. The principle of the treatment is to keep the nervous system for a long time under the influence of the remedy, or, in other words, to make use of the "continued dose," as it is called by E. H. Clarke. To adult females he gives one-third of a grain before each meal, increasing the dose after a few weeks to one-half a grain. Males can generally commence with one-half a grain, and it may be increased to three-fourths of a grain after two or three weeks. The doses must be taken with the utmost regularity, and for a considerable period. As a rule no immediate appreciable effect is noticed, though he has seen some giddiness, etc., from an initial dose of one-half grain *ter die*.

As regards results of this treatment, Dr. Seguin thinks one-half of his cases have been relieved, which, considering the usual persistency of this disease may be considered as very fair success. He does not hesitate to say that, in his opinion, cannabis is nearly as effective in migraine as the bromides are in epilepsy. "Both *may* cure," he says, "and both *do* bring about remarkable interruptions in the series of attacks; both must be employed in the continuous dose. Cannabis in migraine is less effectual than the bromides in epilepsy, but, on the other hand, it is superior to them in not producing unpleasant or injurious effects."

He considers the action of cannabis in migraine analogous to that of belladonna in epilepsy, which is still often effective, though not so fashionable since the introduction of the more generally efficacious bromide treatment. He still employs it in combination with the bromides in some cases, and with advantage. He concludes by drawing a parallel between the actions of the two drugs, and the respective diseases in which they are thus useful.

1. Migraine and epilepsy are both nervous affections characterized by the occurrence of periodical attacks; the attacks, themselves, in both diseases are largely made up of vaso-motor disturbances; in both it is probable

that the medulla oblongata is primarly or secondarily diseased; both affections occur in the same families, and may be present at successive times in the same patient. The late Dr. Anstie has expressed the opinion that the two diseases are akin, and states that migraine may develop into genuine epilepsy. I have in my private case-books cases illustrating this proposition, and I am now treating a physician who states that after nocturnal epilepsy appeared, before beginning bromide treatment, his old migraine grew less frequent and less severe.

2. As regards the two remedies, cannabis and belladonna, both are intoxicants and deliriants; both dilate the pupil; and it is probable that the action of both upon the central nervous system, when administered in the shape of the continued dose, is very similar.

Jaborandi in Hydrophobia.—Dr. J. G. S. Cogshill, *Brit. Med. Journal*, Jan. 5, recommends on *a priori* grounds the use of jaborandi in hydrophobia. He thinks that by the action of this drug stimulating the skin and the salivary apparatus, and the function of the salivary organs which is almost suspended by the disease, could be restored, one of the most embarrassing of the symptoms relieved and the excretion of the poison materially aided.

A New Treatment of Facial Paralysis.—M. Mascarel, *Bull. Gen. de Thérap.*, proposes the following treatment for facial paralysis *a frigore*. The first day he inserts a platinum needle to the depth of a centimetre or a centimetre and a half in the direction of the stylo-mastoid foramen, and another horizontally into the superior fibres of the orbicularis, and a mild current is allowed to pass for ten, eighteen or twenty minutes. Contractions excessively strong are produced in the orbicularis and sometimes the lids close.

The second day, the same operation is performed, but the needle is inserted below the orbit.

On the third, fourth, fifth and sixth days the facial needle is successively plunged into the muscles which seem most refractory, the other being always kept near the stylo-mastoid foramen. After seven or eight days of this treatment the facial paralysis disappears. Three cases are narrated as illustrating the success of this treatment.

The Treatment of Epilepsy.—We take the following from *New Remedies*, Feb., 1878: A stated meeting of the Therapeutical Society was held at the rooms of the New York Academy of Medicine, on the 8th inst., and the Committee on Neurotics (Dr. E. C. Seguin, Chairman,) reported on the results of the use of potassium chloride in epilepsy; that in the cases in private practice, college clinics, and in the Hospital for Epileptics and Paralytics, where it had been tried after observing the effects of potassium bromide and placebos, it had appeared to be practically valueless in doses of 8 grains three times a day and 24 grains at bedtime.

The following synopsis of the tables reported shows the comparative influence of these remedies, and no treatment at all.

6 Males	3 months with placebos,	170 convulsions.
	3 " " pot. brom.,	51 "

8 Females	1 month	with placebos, 149 convulsions.
	1 "	" pot. brom., 43 "
3 Males	1 month	with pot. chlor., 115 convulsions.
	1 "	" pot. brom., 22 "
18 Females	1 month	with pot. chlor., 410 convulsions.
	1 "	" pot. brom., 285 "

The committee also made a preliminary report on the use of a mixture of bromides and chloral in epilepsy, the following formula having been used in most instances :

℞ Potassii Bromidi ..... ʒ i.
Chloral hydratis ..... ʒ ss.
Aquæ ..... fl ʒ vii.—M.

Teaspoonful doses three times daily.

The committee based their conclusions upon reports from the following named gentlemen : Dr. A. McLane Hamilton (13 cases), Dr. McBride (4 cases), Dr. J. H. Emerson (2 cases), Dr. Shaw of Brooklyn (6 or 8 cases), Dr. E. C. Seguin (2 cases). The reporters were united in the opinion that this combination enabled them to dispense with about two-thirds of the quantity of potassium bromide that had been required to produce an arrest of the paroxysm ; that the intervals were longer, the attacks diminished in severity and the accidents of bromism, to a great extent, avoided. The patients became more cheerful and lively, and presented in every way a better condition than when under treatment by the bromide alone. A discussion followed, in which Drs. E. R. Squibb, Wm. M. Thomson, E. C. Seguin, Hanks and Castle participated. The former gave his experience in the treatment of four cases of epilepsy. He had yet to hear of any case in which an explosion of *grand mal* had occurred while the patient was in a condition of bromism. In his cases the accidents of bromism had been avoided by giving the medicines in rapidly increasing doses until decided bromism had been produced, when it was suspended altogether for a few days, to be given in the same manner, and again suspended on the accession of bromism. Dr. Seguin could not think that it would ever be proper to omit the use of the bromide for a single dose, and Dr. Thomson said he insisted upon his patients making up their minds to continue the use of bromide uninterruptedly for at least eighteen months, or it would be useless to attempt the treatment at all. Drs. Hanks and Castle had employed a mixture of potassium bromide and chloral in other affections of the nervous system besides epilepsy, with the experience that the combination was more effective than a corresponding quantity of either remedy would have been.

The Treatment of General Paralysis.—L. Meyer, *Berl. klin. Wochensch.*, No. 21, 1877 (abstr. in *Revue des Sci. Méd.*) :

On the idea that general paralysis is a chronic encephalitis, beginning in the cortex of the anterior and convex portions of the hemispheres, or in the meninges, Meyer was led to try a derivative method of treatment which has afforded him good results. It is nothing else than a revival of a method applied by Jacobi in chronic mental diseases.

Over the great fontanelle he shaves a space on the scalp equal to one-half the hand. In the centre of this space he rubs a circle as large as a thaler

with antimonial ointment. He repeats this operation at the end of twenty-four hours, using an amount of ointment the size of a pea and a little piece of linen. After the first application of the ointment, which should be energetic, the strongly reddened surface is with a compress smeared with the same. The second friction is managed with much care, so as to save the skin in place. Generally these two frictions are sufficient, if not, the third a small quantity of the ointment is applied without friction, and a piece of linen to which it is applied may be left on the surface.

In the course of three or four days the tumefaction of the scalp extends to the forehead and even to the face, more rarely to the occiput or the nucha. The pustular eruption is insignificant, the pustules are small, concrete and dry rapidly.

After the swelling has become intense it may be covered with warm poultices. In a few days the suppuration is so ample that it completely separates the integuments which slough off spontaneously. Too resistant adherances are destroyed with caustics or the scissors. To reach this point usually fifteen days are required. From that time the suppuration is treated for two or three months with basilicon ointment. The patient may take during the same time moderate doses of iodide of potassium and should have a substantial diet. Furthermore, he should be in the open air as much as possible, employed in gardening, etc., only taking care to avoid exposure to extreme heat. Baths should be forbidden, as conducing to cerebral congestion.

During fifteen years this treatment has been employed by Meyer, in the cases of seventeen general paralytics, two of whom remain under treatment. Eight of the fifteen were cured, and their cases are given in the memoir. One of them, however, suffered a relapse after two years.

The periosteum may be affected and there may occur exfoliations, more or less considerable of the bone. The author met with this accident a number of times, but attaches to it no special importance. The loss of hair is readily concealed.

The good effects of this method are not to be accounted for by the irritation produced. In moderately insensible patients the pain lasts only some hours, up to the time when suppuration takes place. Meyer considers rather that there is a revulsive action, regulating the sanguine and lymphatic circulation and putting an end to stases and accelerating the absorption of exudations. It is certain that when the suppuration becomes plentiful the patients are relieved of the sense of intra-cranial pressure which had so much incommoded them.

In this connection we may allude to another abstract, in the *Revue des Sci. Med.*, of a paper by Sander, read before the Medico-Psychological Society of Berlin, May, 1876, in which the author considers some of the perils attending this treatment, in some cases, such as extended inflammation of the bone, hyperostoses, and local pachymeningitis. Still it might, with the results announced by Meyer, be amply justified in general paralysis.

Differences between Daturine and Atropine.—It is well known that Planta regarded these two alkaloids as chemically identical, since he thought their chemical compositions identical as well as their reactions and physiological properties. This view has been generally accepted, in spite of the memoirs of Schreff, Lemaitre, Bouchardat, demonstrating that important differences existed between what are called belladonna preparations. Von

Hager attributed the greater activity of English atropine to the presence of a great quantity of impurities, under the form of belladonnine. Erhardt, nevertheless, has shown that atropine and daturine crystallize differently, and Pohl now shows that their optic properties are not the same, daturine turning the plane of polarization to the left, while atropine makes no change. Besides this their reactions are not identical, the salts of atropine are precipitated by the chloride of platinum, but those of daturine are not. The reverse is the case with picric acid which precipitates only the daturine salts. For these reasons Pohl regards these substances as altogether distinct from one another. (*Deutsche Zeitschr. f. prakt. Med.*, Oct. 27, 1877.) *Bull. Gen. de Thérapeutique*, Jan. 30.

Galvanization of the Sympathetic.—G. Fischer, *Deutsches Archiv f. klin. Medicin*, XX., 175 (abstr. in *Centralblatt. f. d. med. Wissensch.*, No. 7). By a series of experiments, and the utilization of a favorable opportunity to experiment directly upon two men, the author sought to extend and verify some previous statements of his own in an earlier memoir. He came then to the conclusion that, while, indeed, it is possible to influence the intracranial circulation by the electric current applied through the skin of the neck, yet that the sympathetic, to which this result was usually attributed, was not responsible, and that instead of calling this method "galvanization of the sympathetic" it would be more correct to call it simply galvanization of the neck. Alterations in the blood supply of the brain may be produced, but this result depends neither upon the direction of the current, or upon polar action, and is even more decided with the faradic current. In galvanization of the neck not only are the sensory cutaneous nerves irritated, but also the centripetal vagus fibres, so that the action on the brain is an indirect one, due chiefly to excitation of the fibres of the pneumogastric. The sympathetic ganglia are not irritable otherwise than the main cervical bundles of the sympathetic, and as regards this last, the author discovered nothing in his observations that would justify the opinion that the main cord of the sympathetic reacts like a motor nerve to the constant current.

Though this method is incorrectly named, still we may use it, as we do many others empirically found of use, but not physiologically explained.

Among other articles on the Therapeutics of the Nervous System and Mind, we may mention the following:

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THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

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Archiv fuer Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fuer Path. Anatomie, Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
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The following domestic exchanges have been received:

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Maryland Medical Journal.
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Medical Record.
Medical and Surgical Reporter.
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St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.

THE

JOURNAL

OF

Nervous and Mental Disease.

Vol. V. JULY, 1878. No. 3.

Original Articles, Selections and Translations.

ART. I.—CASE OF REPEATED ATTACKS OF APOPLEXY WITH APHASIA.

BY DR. H. D. SCHMIDT,

PATHOLOGIST OF THE CHARITY HOSPITAL OF NEW ORLEANS, AND MEMBER OF THE AMERICAN NEUROLOGICAL ASSOCIATION.

(*WITH PLATE.*)

FOR a number of years, *aphasia*, that particular affection of the brain, which, in many cases, is found associated with, or directly resulting from cerebral apoplexy, has attracted the special interest of pathologists. The following case, which occurred in my private practice, and which terminated fatally about a year after the first cerebral disturbance, offered me ample opportunity, not only to closely observe and study the phenomena of the disease during life, but, moreover, to compare them with the results of an accurate and minute examination of the brain after death. It is for this reason that I consider it worthy of record.

On the morning of November 11th, 1876, I was called to Mr. F——, a gentleman of about fifty years of age, and with

whom I had been acquainted for more than thirteen years. I found him in a condition presenting symptoms of severe cerebral congestion. As I learned from a later account of the patient himself, he was taken by the disease during sleep, after having retired to bed at midnight, worn out by the duties of his business on the previous day; and without the knowledge of his family, who found him in this condition, when entering his room in the morning, not long before I was called. When I first saw him, he was in a state of partial consciousness; his tongue, swollen and almost black from congestion, was protruding from the mouth and tightly held by the teeth; and, in consequence, the patient was unable to articulate any word. There was left hemiplegia, with slight paralysis of the muscles of the inferior part of the face, the tongue, and the levator palpebræ on the same side. In examining the heart, a very slight murmur could be detected. From the position in which the patient had been found, and, also, from the appearance of the bed, I judged that the attack had been accompanied by convulsions. After a few days, the complete consciousness of the patient returned. The paralysis, also, gradually subsided, and about the ninth day from the commencement of the attack, he was able to attend to his business. With the exception of the skin covering the internal half of the left arm, and which presented a bluish color, similar to that of a bruise, no further trace was left of the disease. As this portion of the skin had experienced no violence, the bluish color must have been owing to a neuro-paralysis of the minute vessels of the skin, a condition which soon after subsided. Some facts which I observed during this attack, to be spoken of hereafter, led me to suspect disease of the arteries of the brain as the cause of this congestion; and, therefore, I cautioned the patient particularly against any excess in eating and drinking, or any other excitement. Nevertheless, a second attack, invited, perhaps, by an imprudent act of the patient, occurred on February 16th, 1877. His sense of hearing had been slightly affected for a number of years, but, possessing his full share of vanity, he tried to hide this defect as much as possible from the world. On the day previous to this second attack, he took a sudden notion to consult an aurist,

with whom he was acquainted, and who, not knowing his previous condition, passed an electrical current through his head. After this operation he passed a sleepless night, and, on the next day, felt so badly as to leave his office early in the afternoon and start for home. While in the street-car, he was seized with vertigo, but kept sufficient strength and consciousness to leave the car, and also reach his house, where he was prevented from falling to the ground by his wife, who had opened the door. This time he was struck with hemiplegia of the limbs on the right, and paralysis of the muscles of the inferior part of the face, the tongue, and levator palpebræ on the left side. The paralysis of the face must have occurred before he reached the house, for his face was distorted when his wife first beheld him. With difficulty he was put to bed, and I was called. I found him in the condition already stated, laboring under a partial stupor, though sufficiently conscious to understand what was said to him. However, he could not speak; he was affected with true aphasia; for although his tongue was drawn to the right side, he was able to protrude it and draw it back. The rapidity of the pulse was slightly increased, about ninety per minute, but it was rather weak. The respiration was peculiar; being normal for some minutes, it gradually became weaker, and, apparently, ceased altogether for from five to ten seconds. Then the thorax recommenced to expand, and, while the patient was heaving for breath, the respiration, accompanied by a slight stertor, became forced, after which it slowly returned to the normal standard. When asked, the patient complained of pain in the head, particularly in the parietal and frontal region of the right side, directly above the temples. When not disturbed he remained in a drowsy condition, from which he was, however, roused, from time to time, by the irregular performance of the respiratory function. At no time during this second attack, was there a complete loss of consciousness,—a phenomenon which I supposed to have existed during the first stage of the first attack, coming on, as will be remembered, during the sleep of the patient.

As regards the remedies immediately applied during this second attack, I will only state that they were the same as had

proved successful during the first, namely: cold to the head, leeches to the temples, and, afterward, the administration of bromide of sodium, with the addition of good nourishment in the form of essence of beef, milk, etc. However, if I had known the true condition of the minute, as well as that of the larger arteries, which the post-mortem examination afterwards revealed, but which, at this time, I only faintly suspected, I would have omitted the leeches, and probably, also, the bromide of sodium, and have contented myself with the application of cold to the head. But, as the first attack had passed over so favorably, and finding in this second one no loss of consciousness, I regarded the symptoms as those of hyperæmia, accompanied, perhaps, by a serous effusion into the subarachnoid space and the ventricles, which, by compressing the brain, might be the cause of the pain and feeling of fullness in the head, and, also, of the respiratory troubles.

On the next morning I found the patient slightly better. Although the respiration had greatly improved, being now regular, he was, nevertheless, lying in a state of drowsiness, from which he was aroused when he was addressed. He also made some attempts to answer; but, as he used wrong words of expression, he could not be understood; and the only way of ascertaining his wishes was in putting questions to him in various ways. At this time his language consisted only of one word, which was "paper."

Thus the case went on without much change until the fourth day, when, again, the respiration became more irregular, and the drowsiness also increased, even so much as to induce me to doubt a favorable issue. Not having observed any particular benefit derived from the bromide of sodium, I discontinued its use, and relied solely on rest, careful nursing and cold to the head, with special attention to the secretory and excretory functions; and to my satisfaction, the case took a more favorable turn; for the patient slowly improved in strength. But as the drowsiness disappeared and the mind became clearer, the symptoms characterizing aphasia were rendered more prominent. About three weeks from the first day of this attack, when the patient was able to sit up for a few hours, it was discovered that he had not only lost the faculty of express-

ing his ideas by articulated language, but he had also lost the knowledge of reading and writing. From this time on, however, an improvement gradually manifested itself in the paralyzed limbs; first in the lower, and then in the upper one; and, somewhat later, also in the face. The same was the case with the faculty of speech; though here the improvement consisted only in the recollection of the most common-place phrases, such as: "Good evening," "how goes it," "first rate," etc., for, as soon as the subject of the conversation was changed, the patient became confused and unable to continue it, unless the words which he endeavored to find, were, so to say, put upon his tongue. With the exception of the pain in the parietal and frontal region of the right side, which still persisted, the general health was as good as it could be expected to be. The circumstance of the pain in the head being located on the right side, induced me to regard this case as deviating from the general rule, though the patient was right-handed. About the middle of April, the paralysis of the limbs and face, with the exception of the levator palpebræ, had sufficiently disappeared to enable the patient to dress himself, and walk about the room; and soon afterwards he could leave the house and take a ride. At this time, also, he could write his name again, and read the heading of newspaper articles, printed in large letters. In fine, excepting a slight pain in the head, which still persisted, he felt well; his mind also seemed to be more invigorated. But, though he spoke more fluently and with less interruption; though he could read and understand insignificant articles in the newspapers, he still became confused and stopped short in the conversation as soon as it turned upon a subject whose discussion required more than ordinary phrases.

Upon my advice, based upon the principle of perfect rest of the mind, in order to give nature a chance of repairing the damages done, all visitors, though his personal friends were numerous, had been cautiously kept away from the patient during his illness; and having thus far witnessed the restorative powers of nature, I advised him not to re-enter society for some time, but to take his abode in the country as soon as the season would permit. In the beginning of May, therefore,

he went to a secluded place, free from all causes of excitement, where he lived in the house of a small farmer until about the middle of June, when he returned to New Orleans, and paid me a visit. He had gained in flesh, and his general health was excellent. His speech, also, had improved, so much, even, that a careless observer not acquainted with the circumstances would not have detected the defect at once. But, notwithstanding these improvements, whenever the conversation was continued for any length of time, the aphasia still appeared. A few days afterwards he went to "Point Clear," a quiet watering place with a good hotel on the shores of the Bay of Mobile, where, besides the excellent breeze coming from the gulf, he could also enjoy the stimulating effects of sea-bathing. There his condition improved so much, that, in the beginning of September, longing for more society, he returned to New Orleans.

The time which I had always feared, had now arrived: the time which certainly would test the stability of his recovery. His general health was still excellent; he was able to read, and also to write; but, as I had predicted to his family, not sufficiently well for the discharge of his daily duties. He could speak, but not enough to re-enter the active life of a cotton broker, a branch of business in which he had been engaged for many years. He felt his incapacity, and resigned his business. By the exertion of his many friends he received the office of supervisor in a cotton press; and all seemed to go well until one morning, about the middle of October, when, while dressing himself, he suddenly lost his consciousness, and fell upon the floor. When I saw him, soon after the attack, he had recovered his mind and felt well; and, on the next morning, went to his business as usual. On the morning of November 12th, at the moment of having finished dressing himself, and ready to leave the room, he fell again to the ground with loss of consciousness. I found him in bed; he had already recovered his consciousness. Excepting a few bruises, there were no alarming symptoms present; and, after two or three days, he resumed his daily occupation, until, finally, on November 21st, he was seized with the fatal attack, accompanied by all the symptoms of extensive cerebral hem-

orrhage. He had left his home in the morning, apparently well; but, about 2 o'clock P. M., while engaged at the press, he commenced to feel ill, and refusing to send for a carriage to take him home, he placed himself upon a chair in such a position as would afford him rest, and at the same time secure him from falling to the ground. Very probably he was seized with drowsiness soon after, for at 4 o'clock he fell from the chair in a state of unconsciousness. At 6 o'clock, his son, who had been called to the scene, brought him home. When I saw him, soon after, he lay in a state of deep coma, with a slow and full pulse, and stertorous respiration. The lid of his left eye was considerably swollen and discolored from a severe bruise of the skin of the superciliary region, upon which he had fallen. The pupils were contracted. There was no anæsthesia of the skin; on the contrary, irritation of the skin was followed by reflex action of the muscles. He moved his hand, and I was unable to detect any paralysis of the limbs. On the next morning, however, paralysis of the limbs on the left side could be easily detected; for, while the right hand was in motion from time to time, the left arm lay lifeless and never moved again. With the exception of the respiration becoming gradually weaker, more labored, and more stertorous, he remained in this unconscious condition until November 24th, at three o'clock A. M., when he died. For the interest which I had taken in the case I obtained the permission of the family to examine the brain. The results of this examination were as follows.

Post-Mortem Examination of the Brain.—When the brain was removed from the cranial cavity, an effusion of blood, partly fluid, partly coagulated, and amounting to about four fluid ounces, was found at the bottom of the cavity. There was nothing abnormal in the appearance of the dura mater. The examination of the pia mater showed at once the original cause of the disease,—degeneration of the blood-vessels. While most of the veins were filled with blood, a great number of the arteries, especially the larger ones, and not only those of the cerebrum but also some of the cerebellum, were affected with atheromatous disease. This degeneration was so general that it was even found upon the smaller branches, as

far as they could be distinguished by the naked eye. On the basilar artery, the intervals left between the atheromatous swellings or knots hardly measured more than one-fourth of an inch. The invasion of the disease upon the anterior, middle, and posterior cerebral arteries equalled that on the basilar, for, though the intervals in some places were of greater length, in others the dilatations of the vessels approached each other closely. A considerable number of the smaller arteries of the pia mater were filled with blood. In searching for the ruptured blood-vessels, from which the blood found in the cranial cavity might have escaped, two large, dark-brown or almost black-looking spots, presenting an ulcerated or lacerated appearance through the pia mater, were found upon the surface of the temporal lobe (Fig. 1). The one was situated in the middle temporal convolution, about two inches behind the apex of the lobe, and measured 7 x 9 mm. in diameter; the other, situated in the inferior temporal convolution, measured about 6 mm. in diameter.

After this macroscopic examination, the pons Varolii, medulla oblongata, and cerebellum were separated from the cerebrum by a horizontal section through the crura cerebri, directly above the pons; and then a transverse vertical section was made through the cerebrum, directly behind the tuber olfactorium. This section, passing through the corpus striatum, nucleus lentiformis, and claustrum of each hemisphere, revealed a large apoplectic cyst (Fig. 1), extending throughout the whole island of Reil of the left hemisphere. When the section was made, a moderate quantity of a brownish, rusty-colored fluid escaped from the cavity. After the escape of this fluid, the latter appeared collapsed, its walls almost touching each other. The cavity itself was lined by a distinct pseudo-membrane, about $\frac{1}{2}$ mm. in thickness. As this membrane was of a considerable consistence, and slightly elastic, it permitted itself to be separated from the adjacent structures without being torn. The inner surface of the cavity represented by the membrane, was rendered uneven by the presence of little elevations and depressions, and had a muddled appearance: in some places of a reddish, and in others of a lighter or darker brown color. The cyst, which was most probably

one of long standing, as will be seen directly, presented in the section the form of a slit, 3 ctm. long, and occupied, as the drawing shows, the place of a considerable portion of the claustrum and the capsule; the nervous substance, adjacent to its inferior third, was in a condition of softening, extending on its median side into the nucleus lentiformis, and laterally into the white substance of the island of Reil. A smaller cyst, about the size of a small pea, was found in the white substance of the same section, directly below the superior frontal sulcus, at the place where the white substance of the superior frontal convolution meets with that of the middle frontal convolution (Fig. 1). This cyst was not lined by a pseudo-membrane, but was probably formed by a process of softening of this substance. A portion of the latter, between the larger and smaller cyst, was also in a state of softening.

A transverse vertical section of the same hemisphere, ½ ctm. anterior to the island of Reil, revealed the existence of a third apoplectic cyst, also lined by a pseudo-membrane of the same nature as the one above described. This cyst represented a narrow cavity of an almost rectangular form (Fig. 3). Commencing in the apex of the superior frontal convolution, it extended along the margin of the gray substance, forming the median wall of the superior frontal sulcus, and, in entering that substance at the bottom of the sulcus, bent in nearly a right angle, and passed through the white substance of the middle frontal convolution. That part of the cyst contained in the superior frontal convolution, exceeded in length and depth the remaining portion in the adjoining convolution. Its length, from its beginning at the apex of the convolution to the bottom of the sulcus measured 23 mm.; the entire depth of this part of the cyst, in an antero-posterior direction, was 2½ ctm. The remaining part of the cyst in the adjoining convolution was quite shallow.

A third transverse vertical section, passing through the splenium of the corpus callosum, and through the lateral ventricles directly behind the commencement of the descending cornua, disclosed two more cysts in the same hemisphere; they were not lined by a pseudo-membrane, but formed by softening. The larger one, of an irregular rhomboidal form,

measured 11 mm. in length, and 8 mm. in width, and was situated between the bottom of the interparietal sulcus and the lateral wall of the left ventricle, extending beyond the apex of the latter. The smaller one, about 6 mm. in length, and 3 mm. in width, was situated in the median wall of this ventricle.

After this preliminary examination, the different portions into which the brain had been divided by the sections of the knife were put, for preservation, into a solution of bichromate of potassa—of a strength about two ounces of the salt to ten pints of water—to be subsequently examined with more leisure.

On the following day the pia mater was carefully removed from all the pieces resulting from the sections of the brain above described. But, in order to keep the cut surfaces of the different pieces into which the brain had been divided as level as possible, I determined to make no farther sections until they had, by the action of the preserving fluid, obtained sufficient consistency to preserve their form. Meanwhile I proceeded with the microscopical examinations.

In the right hemisphere, as may have been noticed, no lesion whatever had thus far been discovered by the sections made. But when, about a week afterwards, the middle and larger portion of that hemisphere was again divided by a transverse vertical section, passing about 4 mm. in front of the opening of the aqueduct of Sylvius into the third ventricle (Fig. 2), an enormous apoplectic cyst, the largest of all, was discovered. At the level of the section this cyst measured 3 ctm. in length, and $1\frac{1}{2}$ ctm. in width, reaching in its vertical direction, as the drawing will show, from the nucleus tegmenti into the highest portion of the nucleus lentiformis, and to a level with the posterior, or caudated portion of the corpus striatum. Posteriorly, the cavity extended throughout the whole posterior portion of the thalamus opticus, the pulvinarium, leaving nothing of this ganglion but its cortex, in the form of a thin shell; anteriorly it extended, in a somewhat lateral direction, about as far as the location of the anterior commissure; thus having destroyed the greater part of the thalamus opticus, a portion—perhaps the larger also—of the basis and tegmentum,

together with a portion of the nucleus lentiformis and the claustrum. The antero-posterior diameter of the cavity was about 3½ ctm. This extensive cavity was almost entirely filled with coagulated blood, sufficiently consistent to be cleanly cut by the knife in making the section; only a small portion of the blood escaped from the cavity in a fluid condition. After all the blood was removed by a gentle stream of water, it was found that the cavity must have been of recent date, as its walls were not lined by a pseudo-membrane, but quite soft. In looking for the original seat of this hemorrhage, a group of numerous punctiform apoplectic foyers were discovered in the substance of the tegmentum, along the inferior border of the cavity. It is obvious that the hemorrhage had arisen from these minute centres, and some time may have elapsed before the amount of effused blood was large enough to destroy the neighboring tissues by its pressure and form a cavity of such an extent. But, very probably, other minute hemorrhages occurred at the same time in the tissues already destroyed, the remains of which, in a softened condition, had been washed away with the clot of blood. As regards the latter, it is difficult to determine whether the coagulation had taken place before the death of the patient, or whether it may have been caused by the action of the preserving fluid. The white substance between the upper half of the claustrum, the nucleus lentiformis, and the canda of the corpus striatum, near the upper border of the cavity, was also undergoing the process of softening (Fig. 2).

The condition of the larger arteries and of their subordinate branches, as far as can be ascertained by a macroscopic examination, has already been stated; and, as the pathological process of their atheromatous degeneration is so well understood, it requires no further remarks. But, as regards the more minute vessels of the pia mater, a microscopical examination of small pieces of this membrane showed that the veins were filled with blood, while the arteries were empty. The greater number of the latter appeared normal, showing their component anatomical elements distinctly. A smaller number, however, appeared pale and faint, and also much contracted, with a caliber scarcely as wide as the thickness of their coats, and

almost too small for the passage of one row of blood corpuscles; they were entirely empty. This pale appearance may, perhaps, have been owing to a commencing atrophy, caused by the partial or total occlusion of one of the larger vessels.

In examining small portions of the softened nervous substance, as well as others taken from the substance in the neighborhood of the apoplectic cysts, or of the punctiform hemorrhages, microscopically, it was found that the greater number of minute blood-vessels, the capillaries, and particularly those vessels intermediate between the latter and the arterioles, and appearing to consist of only two coats, were undergoing degeneration. The degenerative process here commenced with the nuclei in the walls of the vessels. Those small traces of granular protoplasm, namely, which in the normal condition of the vessel may be detected upon the surface of these nuclei, abnormally increase in quantity; and it is this accumulation of protoplasm, causing an uneven thickness of the wall of the vessel (Figs. 4 and 5), which constitutes the first stage of the degenerative process. If the degeneration goes on further, the accumulations of protoplasm gradually undergo a fatty metamorphosis, giving rise to the final rupture in the walls of the vessel. These accumulations of protoplasm of course encroach upon the caliber of the vessels in proportion to their extent; and, if a large number of vessels are affected, they must finally seriously interfere with the capillary circulation of that region. In this manner minute thromboses will be formed in the capillary vessels; especially when nuclei, placed very near to each other, become affected, an occurrence which may result in the occlusion of the vessel. In the case under discussion I directly observed some of these capillary thromboses where the vessels in question were filled with a row of blood corpuscles, reaching to each side of the thrombosis. In the smaller arterioles, also, the degeneration may finally result in the occlusion of the vessel, as seen in Fig. 5. But, even if this does not occur, the diminution of the caliber and the unequal width of the vessels, together with the uneven condition of their inner surface, caused by the projection of the accumulated protoplasm, must eventually be followed

by a considerable disturbance of the circulation. Although, in this case, the fatty degeneration of the protoplasm had as yet not progressed as far as in other cases which I have observed, it was, nevertheless, sufficient to give rise to a softening of the mass, manifesting itself in the form of minute aneurisms (Fig. 4), and to terminate in a rupture of the walls, followed by an extravasation of blood.

It appears that the effusions of the blood found in the right hemisphere, and producing the cavity in the thalamus opticus, etc., as well as the other found at the base of the cavity of the cranium and coming from the temporal lobe, had their origin in capillary hemorrhage. A microscopical examination of the minute apoplectic foyers in the tegmentum (Fig. 2), showed that they were of quite a recent date, for though their margins were so well defined as to allow an easy separation from the surrounding tissues, they were, nevertheless, destitute of those fibrinous capsules by which they are, in many cases, ultimately surrounded if the patient survives the accident,—and time is given to the organization of the fibrin contained in the drop of blood which escaped from the vessel. The centres of these minute foyers appear to the naked eye somewhat lighter than the rest, a phenomenon which is probably owing to the colorless corpuscles accumulating there. The fine fibrillæ of the neuroglia, projecting beyond the periphery of those foyers separated from the surrounding tissues, appeared very distinct. In some instances the blood escaping from the opening in the vessel does not accumulate to form a single drop, but encases the little vessel by traveling for a certain distance along the surface of its wall; and, in others again, the blood is imbibed by the neighboring tissues. Such conditions, however, can only be revealed by microscopical examination.

As regards those two dark looking spots of an ulcerated or lacerated appearance, found in the left temporal lobe, and from which the hemorrhage into the cranial cavity had arisen, it was, after the careful removal of the pia mater, found that while the one represented the opening of a cavity the other was only a superficial lesion. The first (Fig. 1), situated in the gyrus temporalis medius, led into a cavity of about 4½ ctm. in length, and 2 ctm. in width, extending throughout the white

substance of this convolution. The entrance was filled with a dark brown mass, resembling an old clot of blood, but which a microscopical examination showed to consist of minute blood-vessels in the state of degeneration, as above described, and, also, of broken down nervous tissue. The whole mass extended into the cavity,—the course of which will be found indicated in the drawing by dots—filling up its middle third. The second lesion, a little smaller in diameter, was situated in the gyrus temporalis inferior, about one inch posteriorly to the former; it was entirely confined to the cortical layer, and filled like the first, with broken down nervous substance and diseased blood-vessels.

The microscopical examination regarding the structure of the pseudo-membrane, was interesting, as it distinctly showed that this membrane was entirely formed from the component elements of the original clot of blood. It consisted of an amorphous substance or matrix, in which a number of other elements were found embedded. Small portions of this substance, torn with fine needles into minute fragments, appeared to consist of minute irregularly oval or round bodies of a greenish tint, fused into minute irregularly shaped masses of various sizes, and held together by a hyaline substance (Fig. 6, *a*). Through this substance a moderate number of delicate and smooth fibrillæ were seen to pass. From a careful examination of these elements I am inclined to regard them as the colorless remains of the colored blood corpuscles of the clot, fused together by mutual adhesion into irregularly shaped groups or masses of different sizes, and the whole united by the hyaline substance—very probably derived from the liquor sanguinis of the clot—into the form of a pseudo-membrane. And this view seemed to be corroborated by the presence of numerous other bodies, mostly round in form, but differing much in diameter and, also, in color. These bodies (Fig. 6, *b*), were spherical aggregations of larger or smaller granules, being evidently the remains of the colored blood corpuscles, deprived of their coloring matter, and represented by their protoplasm only. Besides these bodies, a considerable number of individual isolated corpuscles were also observed, some of which, larger than the others, still showed a

double contour. A number of fine granules and molecules were also observed. The hæmatin, escaped from the colored blood corpuscles, was distributed, either formless or crystalized, throughout the membranous mass. Perhaps the greater number of these aggregations, formed by the protoplasmatic remains of the colored blood corpuscles, were colorless, possessing only a pale greenish tint; but there were others, especially the smaller ones, which, colored by the free hæmatin, appeared in various shades, from the light yellow to the dark brown or black. Some of the darkest seemed to be covered by a layer of very minute hæmatin crystals. The hæmatin, besides coloring these bodies, was also found both in the form of larger or smaller patches and in that of crystals of different sizes (Fig. 6, *c*); some of the latter appearing as minute dark molecules.

Besides the elements just described, there were also a number of minute blood-vessels met with in the pseudo-membrane. Some of them appeared almost unchanged in structure, and filled with blood corpuscles, while the greater number appeared very pale, being evidently in a state of disintegration; even blood corpuscles were observed in some of the latter, though with very pale outlines. The blood-vessels found in the membrane were most probably identical with those which formerly belonged to the nervous substance, displaced or destroyed by the presence of the clot. But while the greater number were obviously undergoing pathological changes, it appeared that others, filled with blood, had preserved the integrity of their structure.

A microscopical examination of the outer surface of the membrane attached to the adjacent nervous tissues, showed the latter in a condition of disintegration, a circumstance explaining the easy separation of the membrane, as stated before.

The microscopical examination of the walls of the large cavity in the right hemisphere, which, after being washed, still preserved an appearance, muddled, more or less, by brownish spots, owing to the diffusion of blood corpuscles throughout the nervous tissue, showed this tissue in a state of disintegration. It is the medulla of the nerve fibres which

seems to be most prone to decay, while the axis cylinders, and also the ganglionic bodies, consisting of protoplasm, offer the greatest resistance. Accordingly, the elements met with were fragments of very fine nerve fibres, either in a varicose form, or, deprived of their medulla, represented only by the naked axis cylinders. A number of larger axis cylinders, destitute of the medulla, were also observed. Some of the ganglionic bodies presented a natural appearance, while others appeared more pale, but the processes of most of them were torn. All these elements, together with fragments of diseased, or also, normal minute blood-vessels, usually filled with blood, were mingled with the disintegrated granular substance of the ganglionic nervous matter. A considerable number of more or less round patches with defined contours were also observed; they were fragments of granular substance, showing distinctly the minute groups of granules of which it consists.

Remarks.—The comparison of the clinical history with the results of an accurate post-mortem examination of the preceding case, reveals some points of interest relating to the pathology and, perhaps also, the treatment of organic cerebral disease worthy of a careful review and study. The cerebral hemorrhages, occurring at different times in this case, were evidently due to the existing disease of the smaller blood-vessels, and, from some similar cases which I have examined as thoroughly as this one, I am inclined to think that in the great majority of cases of cerebral hemorrhage, the hemorrhage, with disregard of its extent, is of a capillary origin. At any rate it may be asserted, that in all cases of cerebral apoplexy, not terminating fatally at the first attack, the hemorrhage is due to a rupture of capillaries, or of those vessels intermediate between the latter and the arterioles. A hemorrhage caused by a rupture of one of the larger cerebral arteries, or of one of their subordinate branches, must necessarily be fatal, as the force of the current of the blood through these vessels certainly predominates over the inherent power of coagulation possessed by the fibrin of the blood. Neither can the vessel close by its own contraction if the rupture was the result of a degeneration of its walls. The process of fatty degeneration of the arterial walls, commencing in chronic

endo-arteritis, is generally slow in its course—especially if accompanied by calcareous deposits—and a rupture of the degenerated artery would hardly take place without—in consequence of its degeneration—a preceding disturbance in the circulation of that part of the organ which it supplies with blood. But, as any irregularity in the performance of the circulatory function must necessarily affect the nutrition, not only of the nervous tissues, but also of the capillaries and arterioles, and induce or hasten in these minute vessels the degenerative process,—it is more probable that a capillary hemorrhage takes place before the degenerated portion of the larger vessel may yield to the pressure of the current of the blood produced by every contraction of the heart. However, as certain diseases affect only certain tissues, or tissues related to them, and as the endothelium of the inner coat, the *intima* of the larger blood-vessels, is almost identical with the delicate transparent walls of the capillaries, it is quite possible that the same process of degeneration may simultaneously take place in both kinds of vessels. Or, it may also occur that the minute blood-vessels be seriously affected, while the larger arteries preserve their integrity.

In the clinical history of this case I stated that I regarded and treated the symptoms accompanying the first attack as those of simple hyperæmia of the brain; and the rapid amelioration and disappearance of these symptoms seemed to speak in favor of this diagnosis. But since the true condition of the brain was revealed by the post-mortem examination, the correctness of that diagnosis may reasonably be questioned, as to whether these symptoms might not be attributed to a capillary apoplexy of a limited extent.

From the examination of some other cases I have reason to believe that capillary hemorrhages of small extent may take place in the brain, without being followed by symptoms of a serious character, such as a paralysis persisting for any length of time, or even complete loss of consciousness; the second attack of the present case also proves this assertion. It is possible, therefore, that the first attack depended on a similar cause. Moreover, as during this attack the paralysis of the limbs—regarding that of the face, being on the same side,

as an exception to the rule—was confined to the left side of the body, it is possible that the seat of the hemorrhage was in the right cerebral hemisphere, and in the same parts,—the thalamus opticus, tegmentum, basis, &c., which, during the last and fatal attack, were destroyed by a more severe hemorrhagic extravasation. The probability of this occurrence becomes more apparent by the fact of the paralysis of the limbs being on the same side in both attacks; and, from the great destruction of tissue, caused by the last hemorrhage in these parts, and the large cavity left behind, we may presume that, at a previous time, a capillary hemorrhage of limited extent may have occurred in the same place, preluding the last or fatal one.

In the beginning of this article I alluded to a certain fact observed, which had induced me to suspect a diseased condition of the blood-vessels of the brain already during the first attack. In examining the patient, namely, I noticed a considerable enlargement and very tortuous course of the anterior branch of the left temporal artery, sufficiently marked to render the vessel very prominent and to project considerably above the level of the skin. Until that time I had never directed any particular attention to this phenomenon, though it is frequently met with, and is owing to a dilatation of the vessel, indicating disease of its coats, and resembling in its nature an aneurism. From this fact observed I then presumed that some of the vessels within the cranium might be similarly affected, and, in some way or other, been involved in the existing hyperæmia. These suspicions were strengthened by meeting some weeks later a similar case, presenting the same phenomenon. This was a gentleman connected with one of the daily newspapers here, about forty years old, and whom I had known for nearly ten years. Some time previous to his visit to my office he had been struck with hemiplegia of the right limbs and paralysis of the face on the left side, which his physician had declared to be of syphilitic origin. The correctness of this diagnosis, however, I regarded as doubtful, as the patient assured me that, besides a small sore on the penis in his youth, he had never noticed any symptoms of syphilitic disease upon his body afterward. In this case, also,

I observed the dilated and tortuous condition of the anterior branch of the temporal artery, but on the right side of the head. In directing his attention to this fact, I learned that he had never taken notice of it before. With the exception of his right hand and of the muscles of the face, the paralysis had disappeared; and, though he was again able to write, hand and brain were soon fatigued by the exertion of the performance. Once more he paid me a visit, and then I saw him no more. But, about two months afterward, I learned from the newspapers that he had died from another attack of apoplexy, and that he had been found by the neighbors, who were called to the scene, on the floor, nearly under the bed; the latter circumstance indicating that this last attack had been accompanied by severe convulsions.

A third case upon which I observed this phenomenon, is a gentleman of about fifty years, or more, who informed me that he was subject to repeated attacks of chronic pains in the head, frequently accompanied by vertigo, and especially after having had some vexation. In stating these observations I do not mean to imply that the enlarged and tortuous condition of the branches of the temporal artery, as above described, is in all cases accompanied by disease of the smaller blood-vessels of the brain; I merely wish to direct attention to this phenomenon.

The simultaneous appearance of aphasia, accompanying the second attack of our case, directs our attention to that large apoplectic cyst, extending throughout the island of Reil, and, also, to the smaller one in the superior frontal convolution, both of which were lined by a pseudo-membrane, and situated in the left hemisphere. It is obvious that this second attack depended, also, on a capillary hemorrhage into these parts; and which, as the clinical history shows, appears to have taken place by degrees. The accumulation of the blood, slowly escaping from the minute ruptured vessels, gave rise to the formation of that extensive cavity, until it was finally arrested by the pressure of resistance of the surrounding healthy nervous tissues. When first formed it must have been of a greater extent in width than when found after death, a time when the process of reparation had been active for a full

year. The most remarkable feature of this extensive lesion is, that there was never a complete loss of consciousness, the prominent symptoms being only the hemiplegia and the aphasia, accompanied by drowsiness and deranged respiratory function. As the seat of the lesion was in one of those localities of the cerebrum to which, by observation, the faculty of language has been assigned, there was nothing remarkable in the phenomenon of aphasia. More difficult, however, is it to find the true explanation of the paralytic phenomena, manifesting themselves, during this attack, on the right side in the limbs, and on the left side in the face; for, there was no lesion discovered in the corpus striatum, or in those convolutions of the cortex forming the so-called "motor zone" of that hemisphere; neither was any found in the pons varolii, or in the medulla oblongata.

The recent views of a number of prominent pathologists, concerning the pathology of cerebral lesions accompanied with hemiplegia, have been deviating from each other to such an extent, as to render it difficult to the impartial observer to arrive at his own conclusions; more especially, as in support of the different explanations of the phenomena accompanying such lesions, numerous facts have been observed and cited on both sides of the question. As far as regards the case under discussion, it will be seen that, while some of the phenomena are of such a nature as to support the old theory of crossing paralysis, there are others, including those just mentioned, which speak in favor of Dr. Brown-Séquard's recent teachings.

The structure of the pseudo-membrane, revealed by the microscopic examination, is also of interest. From this examination it appeared that it was originally formed and still consisted of the remains of the anatomical elements of the blood, which had been filling the cavity; and that it was not held to the adjacent tissues by any inflammatory product. On the contrary these tissues appeared rather in a state of disintegration, holding, moreover, a considerable number of minute diseased blood-vessels. There remains no doubt but that the second attack was owing to the hemorrhage which gave rise to the cavity. And, as this occurred a whole year

before the death of the patient—during which time the membrane was formed at the surface of the clot by the organization of the fibrin, holding a portion of the colored blood corpuscles, while the rest of the clot was changed into that serous, rusty-colored liquid escaping from the cavity when the section was made—it would be interesting to know how long the patient might still have lived, without the appearance of symptoms caused by the presence of this cavity, and of such a character as to affect his physical condition—if no subsequent hemorrhages had occurred, cutting short his life. And, also, whether, in case of prolonged life, some inflammatory action sufficiently intense to promote the organization of the membrane into a healthy cicatrix, could have been expected to set in. As long as the degenerative process of the minute blood-vessels was not arrested, it is very improbable that such would have been the issue. At any rate, this case, as it was, shows how little the presence of a large apoplectic cavity in the cerebrum may influence the general health of the patient, for the one in question was an eater with a ravenous appetite up to his death.

Little can be said or suggested of the immediate cause of the third and fourth attacks. Although they were marked by loss of consciousness, this was only of a very short duration, and not accompanied by the slightest paralysis, nor by any other symptoms of a serious nature. As the patient recovered so rapidly, both attacks may have been caused by a momentary impediment in the circulation. The loss of consciousness would indicate some momentary disturbance, affecting the cortical layer of the cerebrum. Or, could it be possible that even these short attacks were caused by a minute hemorrhage into the cortical layer, and at the same place in the middle and inferior temporal convolutions where the last and fatal hemorrhage took its seat—and which might thus be regarded as the mere preludes of the fatal issue?

Finally, the extensive hemorrhages in the middle and inferior temporal convolutions of the left hemisphere, and in the thalamus opticus, tegmentum, basis, &c., of the right, must have occurred nearly at the same time. But, as the patient still lived, though unconsciously, more than sixty hours after

the commencement of the fatal attack, it may be presumed that the quantity of effused blood escaped from the vessels during this time very gradually; and as the paralysis of the limbs of the left side was not observed until on the morning following the attack, it is likely that the hemorrhagic effusion into the temporal lobe of the left hemisphere was the first that took place, and which deprived the patient of consciousness; it was soon followed by the other into the right hemisphere, producing the paralysis.

One of the most prominent symptoms in the clinical history of this case, was the aphasia, which appeared with the second attack. The ultimate relation existing between this affection and the mind, has made it an interesting subject of inquiry in human pathology; and it has furnished abundant material to incite the medical mind to a closer investigation of the true mechanism of speech, and the exact mode in which this operates. Not only have the different forms of the disease, with their characteristic symptoms, been thoroughly studied and discussed by some of the most able medical men; but, moreover, the process of language itself has also received its full share of attention. As regards the nature of this process, no great difference seems to exist in the views of different observers; for the main question remaining to be settled, concerns only the localization of this faculty in certain convolutions of the cerebrum. While, for example, some physiologists adopted the view of Broca, who regarded the inferior frontal convolution as the seat of the faculty of speech, others followed Meynert and Sander, and assigned this faculty to the island of Reil.

In the preceding case, the large cavity extending throughout the island of Reil, may, without hesitation, be regarded as the direct cause of the phenomenon of aphasia. And this view seems to be corroborated by the results of the examination of the cyst after death, showing that the improvement in the patient's speech corresponded to the gradual formation of the pseudo-membrane lining the cyst, and protecting the adjacent nervous tissues from farther injury. Thus far this case would speak in favor of the theory of Meynert and Sander, who place the faculty of speech in the island of Reil. The

localization of this faculty in the inferior frontal convolution, or in the island of Reil, seems to be generally admitted by the profession, and is also supported by numerous observations. Nevertheless, it is not an established fact; for the arguments of Dr. Brown-Séquard, in proof of its incorrectness, are certainly strong. As far as the localization of speech is concerned, these arguments appear to be well founded, and worthy to be examined by the unbiased observer; though his theory of functional homogeneousness of the entire cortex cerebri does not appear quite as satisfactory.

In the present case, the patient understood what was said, but could not find the words to express his ideas; and the few words which he articulated were wrong ones. He was not able, as Griesinger once remarked, to find the true image of sound to the image of vision. The nervous current, here, was interrupted while passing through those nerve fibres which connect the cortical centres of vision with those of hearing or sound; when passing, however, from the last named centres to the former, there was no interruption, for the patient was able to transform the words, impressed upon the centre of sound, into images of vision. Moreover, the patient was able to repeat the words which were spoken to him, a phenomenon which may be explained by presuming that the impression made by the spoken words upon the centre of sound, passed, not only to the centre of vision—upon which it left its image—but moreover, without interruption back again to the centre of sound, and thence to those subordinate centres presiding over the muscles of speech, in order to be, finally, transformed into the real, originally spoken words. When the disease was at its height the patient could neither read nor write. Written language, in reality, represents a combination of symbols appreciated only by the organ of vision in the cortex cerebri, upon which they become fixed in the form of visual images, which, by the will of the mind, may be subsequently revived, as long as the brain is in a normal condition. The loss of the understanding of written language, therefore, can be only explained by supposing the existence of disease in those commissural nerve fibres through which the image of a certain object, already fixed upon the mind, becomes associated with

its corresponding symbol, the written word; and of which, also, an impression already exists in that portion of the cortical layer of the cerebrum in which that faculty known as the memory of vision may have its seat.

The above sketch in explanation of the phenomena of aphasia I only drew for the purpose of showing the great complexity of the pathological process concerned in this affection, and the difficulty which attends the exact localization of particular functions in the cortical layer of the brain. In regarding the mind as a force resulting from a combination of the various impressions of external objects upon the substance of the cortical layer of the brain, through the medium of the organs of special sense, it becomes obvious that, in order to localize the faculties of this mental force, the true seat of the terminal elements of the special organs in the cortex cerebri should be known. This, however, is not the case, for, to the extent of my knowledge, no nerve fibre, or bundle of fibres, whether motor or sensory, has ever been traced beyond the corona radiata. Here they are joined by the fibres of the corpus callosum, in the company of which they pursue the rest of their course to the cortical layer. The real difficulty of tracing each set of these fibres separately to their final destination in the cortical layer, is owing to their radiating course, and to the impossibility of distinguishing them from each other. Not knowing, therefore, in what particular regions of the cortex cerebri we may find the terminal elements of the different organs of special sense, it remains impossible to assign a place to particular functions of the brain. Nevertheless, the facts elicited by the experiments of Hitzig, Fritsch, Ferrier and others, together with the pathological observations of Charcot and Pitres in relation to this subject—though, as yet, not explained in a manner to give general satisfaction—may be considered a step farther in this direction. These observations seem to demonstrate the existence of certain motor centres in the cortex cerebri, occupying an intermediate position between the mind and the motor ganglia of the cerebro-spinal axis, and performing the function of transforming the nervous stimuli received from the particular centres of the will, into motor impulses or energy, to be transmitted

through the motor ganglia to the muscles. Thus, the movements of the various groups of muscles of the body would be superintended and regulated through these psycho-motor centres. Although this theory is still far from being firmly established, it is supported by numerous observed facts, pointing, at least, to the probability of its correctness.

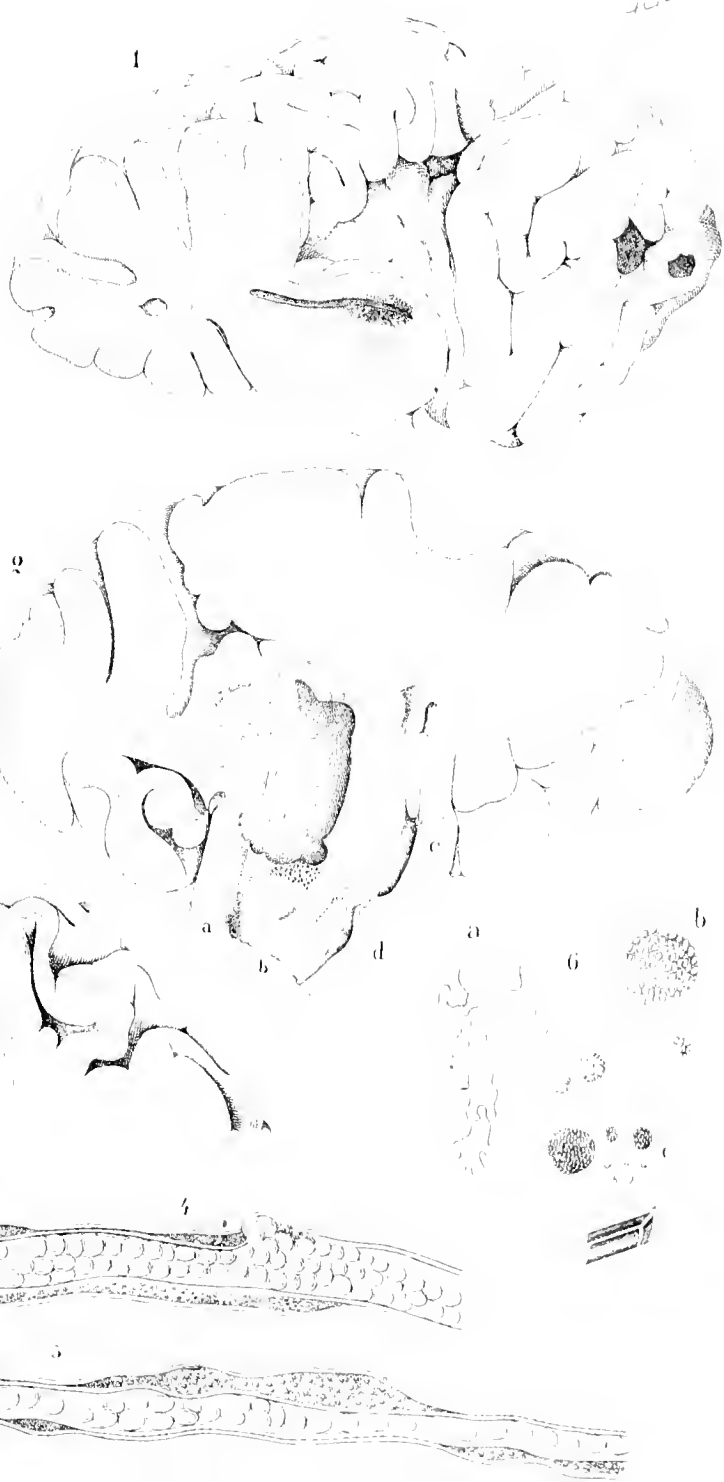
These remarks regarding the localization of the individual mental faculties, are equally applicable to the pretended localization of the faculty of speech. As I have remarked before, language, whether written or spoken, stands in the closest relationship with the special senses, but most especially with those of sight and hearing. In man the great majority of impressions of external objects upon the organ of the mind are made through the instrumentality of these senses; and, according to the comparative intensity of the peripheral stimulus, remain fixed for a shorter or longer period in the form of memory. From this it becomes obvious, that the terminal elements of the organs of vision and sound must extend over a larger region of the cortex cerebri than the third frontal convolution and the island of Reil combined. But as in the majority of cases of aphasia, examined after death, the lesions have been found in, or very near these convolutions, the idea must arise that these parts of the cortex stand, very probably, in some relation with the faculty of speech. And, being situated, at the same time, in close proximity with the so-called "psycho-motor zone," we might venture to regard these convolutions, also, as the psycho-motor centres of the faculty of speech. This supposition may be somewhat corroborated by the view of Meynert, regarding the connection of the claustrum with the nucleus of the auditory nerve. As regards the theory of the localization of the whole apparatus of language in the cortical substance of the third frontal convolution, or of the island of Reil, the arguments of Brown-Séquard, illustrated by his numerous cases, make it untenable. The absence of the island of Reil in the animal brain has been cited in support of this theory. It is not to language in general, however, that this lobe bears any relation, but rather to *articulated* language, a faculty possessed only by man; though it would not be going too far, in regarding even the chattering of the

monkey as a feeble attempt of articulating sounds. All animals, with the possible exception of the lowest classes and orders, possess a symbolic language, by the aid of which they communicate among themselves. In the lower animals these symbols of communication are very simple, consisting only of certain sounds, produced by the rubbing of the legs or wings upon each other, or by some special organs—and, also, in touching each other by means of their antennæ. And, in proof of their perfect understanding of these mutual signs, either by sound or touch, the wonderful actions and labors of the ants and bees bear witness. And, any one who will take both the trouble and pleasure of practically studying the psychology of our domestic animals only, who are constantly near us, as I have been doing for many years, will soon be convinced that they possess a language, consisting in a smaller or greater number of sounds—though not articulated, but varying from each other—by which they express their intentions and desires in the form of distinct ideas. This language, of course, is accompanied by certain expressions of the face, as well as movements made by the tail or other parts of the body.

Explanation of the Illustrations.—Figure 1. Representation of that portion of the left hemisphere of the cerebrum situated directly behind the tuber olfactorium, at which place a transverse vertical section was made. The specimen rests upon the median surface of the hemisphere; the operculum is lifted up, in order to bring the island of Reil into view. The large apoplectic cyst, occupying a considerable portion of the claustrum and capsule, and extending throughout the island of Reil, will be seen upon the cut surface. A smaller cyst is seen near the superior frontal sulcus. Two destructive lesions will be noticed upon the temporal lobe. The larger one represents a cavity, the direction of which is indicated by the dotted line.

Fig. 2. Representation of the posterior surface of a transverse vertical section, made through the right hemisphere, and passing about four millimetres in front of the opening of the aqueduct of Sylvius into the third ventricle. The median surface of the portion of this right hemisphere which the figure represents, will be found slightly bent, a disfiguration caused by the action of the solution of bichromate of potassa, in which the specimen had been lying about a week before this section was made. The large cavity, represented in the figure, also may have had its share in the withdrawal of the crus cerebri from the gyrus hippocampi; *a*, basis; *b*, tegmentum; *c*, median section of corpus callosum; *d*, median section of corpora quadrigemina.

Fig. 3. Representation of the posterior surface of a transverse vertical


1
2
a
b
c
d
3
a
b
4
5

section through the left hemisphere, passing one-half centimetre anterior to the island of Reil, and revealing an apoplectic cyst of a rectangular form, as seen in the figure.

Figs. 4 and 5. Representation of two minute degenerated blood-vessels. In the one (Fig. 4), the formation of a minute aneurism will be observed.

Fig. 6. Component elements of the pseudo-membrane lining the cyst in the island of Reil—*a* and *b*—probable remains of colored blood corpuscles of the clot; *c*, the same elements covered with minute hæmatin crystals, and in company of a large hæmatin crystal.

Figs. 1, 2, and 3, are represented reduced to three-fourths of the natural size. Figs. 4, 5, and 6, are magnified 420 diameters.

ART. II.—TUMOR IN THE CEREBELLUM.

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(Read before Boston Society of Medical Observation.)

MR. J. F. M——, æt. 30 years, was seen by request of Dr. W. H. H. Hastings, on August 23. On his father's side consumption was very prevalent. He has never had rheumatism nor venereal disease. Once he had a yellow skin. When five years old he fell over the bannisters and was partially insensible, but soon recovered and never noticed any bad effects from the fall. Last December he first became subject to attacks of dizziness and pain in the head. Before that he had worked hard and late. At times the attacks were severe. Last May he had a sickness which was called a slight attack of diphtheria; after it there was no paralysis, but the dizziness was worse, and the headache was more constant and more severe; it was increased by change of position, especially by lying down after having sat up. There was considerable pain in the eyes; no double vision. Pupils and eyes acted naturally. There was no facial paralysis, no mental disturbance; notwithstanding the very severe pain, the mind was clear almost to the very last. Tongue was protruded straight; there was no tremor of the facial muscles, no exaggerated reflex action.

Apparently there was a slight diminution of sensation in the legs, a very slight inco-ordination of the hands, with tremulousness on motion. When he tried to walk there was considerable festination; he went across the room half running, head leaning well forward, as if about to fall on his hands; turned towards the left in the arc of a rather large circle, and brought up against a table. He returned to the bedside in the same manner, bearing rather to the right. There was no irregular jerking of the legs. When supported, he walked better, without festination, but with evident effort, as if the legs were weak or tired. He could not stand with his feet near together, even with his eyes open. The ophthalmoscope showed the vessels clouded and the outline of the disks indistinct. There had been vomiting, and this occurred subsequently several times.

Dr. Hastings found the urine acid, 1030; normal, excepting that urates were abundant; and I learn from Dr. Hastings' notes, which he kindly allowed me to use, that he continued to suffer from severe headache, occurring in paroxysms, sometimes excruciating. About a week after he was first seen, he was very comfortable, the attacks of pain were shorter and less frequent, the pulse was 64, but in three or four days the headache was very severe again, pulse rose to 104. On the ninth of September, after an attack of pain, he could not talk as usual, or could not say what he wished to: for instance, when he wished to have the clothes taken off, he said, "take off the bed." He could not make himself understood by writing.

September 18 he died; previously he had several attacks of general spasms, stiffening out his whole body and limbs. On the morning of the 18th he had two attacks of unconsciousness. His mental powers were good up to the day before his death.

Autopsy about twelve hours after death.

The membranes were healthy; the vessels at the base were unchanged. The convolutions were flattened. The substance of the brain was firm and dry; the pia mater was dry. The ventricles contained an unusual amount of serum, which was not measured. Over the upper surface of the cerebellum on the median line, over a space of about an inch in diameter, the pia mater was considerably thickened and adhered to a tumor, which was about one inch in diameter, and entirely imbedded

in the cerebellum. The tumor retained very nearly the shape of the cerebellum, and was very nearly on the median line, only a very little more to the right than the left. The tumor was very soft, semi-translucent, its boundaries tolerably well defined No other changes were found.

Microscopically the tumor showed merely nuclei, or small cells, with nuclei about as large as the cell, and a small amount of fibrous tissue mingled with the cells. No changes have been found in the medulla, nor in the upper part of the cord; the restiform bodies, which are continuations of the inferior cerebellar peduncle, were unchanged. No examination has yet been made of the optic tracts.

The diagnosis made when the patient was first seen, was—tumor, probably in the posterior part of the brain or cerebellum. Before the autopsy, the locality was more defined—in the upper part of the cerebellum, or attached to the tentorium, pressing upon the cerebellum. I did not attempt to say on which side the new growth was.

The grounds upon which this diagnosis was made were: the severe and persistent headache, rendered worse by lying down; dizziness and vomiting. The inco-ordination in both arms and legs; the peculiar mode of walking with festination. Especially the retinal changes.

Such severe and persistent headache, from December to August, in a man of his age, is enough of itself alone to arouse solicitude; to cause a suspicion of cerebral lesion. This suspicion was made stronger by the presence of dizziness and vomiting. Inco-ordination is found in spinal diseases, but when associated with the headache, dizziness and vomiting, with such manifest festination, it pointed rather to cerebral lesion. The ophthalmoscope decided the diagnosis. Without this I could not have been so positive of the correctness of the diagnosis.

The diagnosis of the nature of the cerebral lesion was also guided by the retinal changes. Sclerosis is not accompanied with retinitis, but rather with atrophy; also there was no tremor and no mental disturbance. The symptoms were too slowly developed, and not sufficiently acute, to be caused by meningitis. There were not the symptoms of cerebral pres-

sure which would be found in hydrocephalus. A tumor was consistent with the symptoms.

The locality of the tumor was determined almost entirely by elimination. In the anterior lobes, mental disturbance, emotional disturbance, motor paralysis, or spasmodic action, would be likely to have been present. Had the tumor been at the base of the skull, some one or more of the cranial nerves would have been implicated. It must then be, that either the posterior lobe or the cerebellum was the seat of the tumor. The presence of inco-ordination and the peculiar gait decided for a locality which would implicate the cerebellum. Had the tumor been on the under surface of the cerebellum, some of the nerves arising from the medulla and pons or those organs must have been affected, and probably to a considerable degree. A morbid growth in the substance of the cerebellum, or pressing upon its upper surface, would also be likely to exert sufficient pressure upon the medulla to give rise to the slight symptoms referable to that region—slight impairment of sensation in the legs, and perhaps in a measure the weakness of the legs. Partly, therefore, by exclusion, and in part from the symptoms present, the diagnosis was made.

Many, perhaps most of the symptoms recorded in cases of tumor of the cerebellum, depend upon the secondary affection of other parts of the encephalic nervous centres. The extension of the disease to neighboring parts, the presence of multiple lesions, and, most of all, pressure, will explain the diversity of symptoms.

Headache is the most constant symptom, and it is very commonly occipital. Thus, in 201 cases, headache is expressly mentioned as occurring in 165. In 169 cases, where particulars are given, it was said to be occipital in 57 cases, frontal in 10, and was general, or the locality was not mentioned, in 60. Lussana (*Journal de Physiologie*, t. vi., 1863) says that in 128 cases, headache was rarely wanting. The character of the pain is one of great severity; language often being exhausted to express the patient's suffering. So severe is the pain, and so much is it increased by motion, that the patient will seem to be in a semi-coma, and will avoid speaking even, lest the pain should be increased. It may well be that such a patient might

be thought to have lost mental power. This pain is probably caused by pressure upon inflamed membranes; the cerebellum being closely confined beneath the tentorium, a very slight increase in size would exert a pressure which would give rise to severe pain.

An unusual quantity of serum was found in the ventricles in 30 cases, that is, a little more than 17 per cent. In 17 cases the middle lobe alone was affected; of these, 4 were accompanied with serum in the ventricles; that is, a little more than 23 per cent. One or other of the lateral lobes was affected in 104 cases without the middle lobe being mentioned. In 17 excess of serum occurred; that is, in somewhat over 16 per cent. Serum was found in rather over 28 per cent. of the cases where no mention is made of the particular locality of the lesion.

When it is taken into account that many autopsies are not very carefully reported, and that lesion of one lateral lobe may encroach upon the middle lobe, or may be situated near the median line, it is interesting to notice how large a proportion of cases with dilated ventricles had the middle lobe diseased. Pressure upon the vena Galeni, the straight sinus, or the torcular Herophili, will explain the presence of the serum; that is, pressure at the median line; but pressure on one lateral sinus might, and very likely would, be insufficient to cause the dropsy. The vena Galeni, it will be remembered, receive the blood from the corpus striatum and the choroid plexus, and empty into the straight sinus. But all these percentages must be taken *cum grano salis.*

Immediately connected with the hydrocephalic phenomena are the affections of the eyes. In 201 cases, ocular disturbance occurred 91 times,—nearly in one-half,—45 per cent. In many cases the sight was entirely lost; in others, only partially. As the eyes are rarely carefully tested, it may well be that many cases of slight defect of vision were unnoticed, and unless the ophthalmoscope was used, it would not be safe to say of any case that the optic nerve and retina were entirely free from disease; for not infrequently changes can be found when vision is unaffected. In the case I have reported, the patient was not aware of any defect of vision, yet changes

were found at the fundus of the eyes which materially assisted in a diagnosis.

As a rule, both eyes are affected, and generally in equal degree. In one case, however, reported by Vulpian (*Compt. Rend. de la Soc. de Biol.*, 1861), the left eye was blind; sight was retained in the right eye. In this case the right lobe of the cerebellum contained a tubercular mass the size of a pullet's egg; this extended a little beyond the median line. The right half of the floor of the fourth ventricle was softened, the median line exactly limiting this change. The left lateral ventricle contained much more serum than the right. The corpora quadrigemina were healthy.

This case confirms a statement made by Allbutt ("On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys," 1871, p. 160): "It would seem to me, then, that softening and pressure, rather than inflammation, are the agents of interference with vision. But it is to the interference with the venous circulation that I would attribute the amaurosis in a large number, if not in the majority of cases of cerebellar tumor." It is easily conceivable that pressure directly upon the corpora quadrigemina, by the tumor, may sometimes cause amaurosis. The pressure by ventricular fluid upon the optic tracts would generally affect both sides alike, hence vision is frequently affected in both eyes. Galezowski considers that the amaurosis is caused by the propagation of inflammation from the tumor through the superior cerebellar peduncles to the corpora quadrigemina. In the present case there may have been some retinitis, but it was by no means very marked; serous exudation; pressure, I should think, might have been a sufficient cause for the changes. If Galezowski's view were correct, one would expect much more frequently that the affection of sight would be unilateral. In either view, however, the amaurosis is not due to the lesion of the cerebellum, but is a symptom depending upon secondary changes elsewhere.

The pupils, when affected, are generally dilated; occasionally are contracted. Thus they are mentioned as dilated 37 times, as contracted only 4 times. It is not unlikely that, in many cases, the condition of the pupils is overlooked and no notice

is taken of them. Sometimes the condition of the retina or optic tracts are the cause of the dilatation; again, the state of the pupils may be due to other secondary changes,—it is not directly dependent upon lesion of the cerebellum.

Vomiting is a frequent symptom, occurring 93 times in 201 cases. Lussana, however, found it mentioned only 28 times in 128 cases. Vomiting is found in other lesions of the brain, and in various peripheral lesions. Though the vomiting may be dependent upon the cerebellar lesion, it is not sufficiently characteristic to serve as a very important aid to diagnosis. Macabiau agrees with Hillairet that it is due to pressure upon the pneumogastrics. Ladame thinks it not improbable that the vomiting is sympathetic. Should he not say reflex, from the lesion of the cerebellum?

In the case I have reported the mental powers were intact up to the day before death. As a rule, the intelligence is unaffected; when the condition is the reverse, it is because of changes outside of the cerebellum; either other lesions arise, or there is such an hydrocephalic collection of fluid in the ventricles that the pressure causes the coma. Delirium is very rare in uncomplicated cases. It must be considered that there may be secondary meningitis. Sensation is not generally seriously affected.

Disturbance of motor power is one of the most frequent symptoms; in frequency it ranks next to headache, occurring 136 times in 201 cases; and probably this does not express the whole truth, for in many cases the slighter forms of disturbance may have escaped the physician's notice. A patient found in bed with severe headache and restless with pain, which is increased by assuming an erect posture, may move his limbs freely and so be looked upon as free from motor symptoms; but his co-ordinating power has not been tested; or, he may be so weak as not to be able to stand, and no note is made of it.

The motor symptoms may consist either in mere weakness, —marked, but too general to be better defined—33 times; hemiplegia, 15 times; variously described changes which may all be included under the head of inco-ordination, 48 times; contraction and other abnormal changes, 11 times. These

symptoms were many times caused by pressure upon the medulla and pons or crura cerebri, or are to be explained by other lesions; this is especially the case with the cases of hemiplegia. The medulla oblongata lies so immediately beneath the cerebellum upon the bone, that a very slight increase in the size of the latter must necessarily exert a pressure upon the medulla.

Lussana sums up his views: "1. The alteration of the muscular sense is the constant and pathognomonic phenomenon of diseases of the cerebellum and of experimental destruction of that organ.

"2. As to other symptoms, they are inconstant, and vary in the affections of the cerebellum; they are generally wanting in the experiments made upon animals."

Allbutt says: "The cerebellum appears to me to be rather a reservoir of force where, by the means of the posterior columns, tension is stored up during times of repose to be given out during times of demand. Want of capacity of motion rather than palsy, is what we should look for in the loss of such an organ."

Fournié says: "We are disposed to see in the cerebellum an organ for re-enforcing the cerebral actions, designed also to supplement by a special excitation, the action of the will, when the latter is absent, as during sleep, or when it is employed in directing another action."

Some interference with the perfect functioning of the motor powers would then be expected in lesions of the cerebellum. The above analysis of symptoms in cases of tumor agrees with this. Dickinson found in sixteen cases of softening and congestion of the cerebellum where there were no tumors that "the only faculty which constantly suffers in consequence of destruction of the cerebellum is the power of voluntary movement." (*Brit. and For. Med. Chir. Rev.*, Vol. 36, 1865, p. 479.) It would merely be tedious to multiply quotations. In the present case there was weakness and inco-ordination. There was also festination. This has been noticed in only a few cases, two or three; some authorities say that retrograde motions may exist.

Rotatory motions were mentioned in four cases. This

symptom is of no diagnostic importance, as it is found in connection with lesion of many other portions of the encephalon.

Spasms and convulsions occurred in sixty-one cases, but are not of diagnostic value, as they occur in lesions situated in so many other localities.

From this attempt at an analysis of the symptoms of tumors of the cerebellum it appears that motor disturbance is the only phenomenon which can be said to be legitimately dependent upon lesion of the cerebellum itself; other symptoms are caused by pressure upon or secondary changes in other organs. Flint, I think, is rather too cautious in requiring lesion of half the organ to ensure disorder of movement. He says: "Every carefully observed case that we have been able to find in which there was uncomplicated disease or injury of the cerebellum, provided the disease or injury involved more than half the organ, presented great disorder in the general movements, particularly those of progression." ("Nervous System," p. 386.)

But for diagnosis, other than the motor symptoms must be considered. Of course the nature, severity and variety of symptoms will depend upon the size of the tumor, and its position. Severe headache, especially if occipital, should lead to a careful search for other symptoms; then loss of power in upper and lower extremities and especially ineo-ordination. Can the patient walk with his eyes shut without marked staggering; can he stand steadily with his eyes shut and his feet close together along their whole inner edges, so as to have a small base of support; can he stand on one foot with his eyes shut; with his eyes shut can he touch with the end of one finger any point of his body, as eye, ear, mouth or nose: can he perform complicated combined movements, as writing, playing piano, etc., with normal facility. It is in doubtful cases that these delicate tests are valuable. If hemiplegia and coarser paralyses are present there is no doubt about the motor disturbance, but such is not diagnostic of the cerebellar lesion, rather arise from pressure upon the medulla or from lesions distinct from the cerebellar lesion; yet it must be borne in mind that the ineo-ordination may be unilateral, and be as valuable a diagnostic sign as if bilateral, though such cases I be-

lieve are rare. Implication of the nerves arising from the pons and medulla will aid in locating the disease, but it may cause uncertainty as to whether the cerebellum itself is affected. And if there is also hemiplegia, there may be reasonable grounds for questioning whether that organ is primarily or even chiefly affected, and a history of the progress of the case may not clear up the doubt.

If to the headache and motor disturbance there is added affection of the eyes, and if retinal changes are found, not perhaps well marked retinitis, but swelling of the optic nerve and choked disk, there is additional reason for locating the disease in the cerebellum. Vomiting and dizziness would also aid in forming a diagnosis, and convulsions might be present. There would in uncomplicated cases be little or no affection of sensation, and the intellectual powers would be intact, unless there were symptoms of pressure due to hydrocephalus.

But no one of these symptoms of minor importance are of much assistance in locating the lesion in the cerebellum; it is only as combined together and with others that they are of value. I have seen complete amaurosis with the most extensive neuritis, exudation and hemorrhages, associated with tumor in the anterior lobe, but there was impaired intelligence, and lesion of individual cranial nerves, and no suspicion of cerebellar tumor. I have seen convulsions in lesions of the middle cerebral lobe, and vomiting is not uncommon in any cerebral affection.

ART. III.—A CLINICAL AND THERAPEUTICAL CONTRIBUTION TO OUR KNOWLEDGE OF CERVICAL PARAPLEGIA.

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I PRESENT the following histories of cases to illustrate the semeiology of cervical paraplegia, and to demonstrate that the disease, in some of its forms, may be checked or even cured.

CASE I.—Atrophic paralysis and anæsthesia in both hands; symptoms of weakness and stiffness in the legs; removal of these symptoms, and permanent arrest of the disease.

Dr. R——, U. S. Army, aged 41 years, first consulted me in 1873, and related the following history: Had always enjoyed good general health, never had syphilis, or been injured about head and spine, but had been much exposed to hardship and overwork in his army service. In December, 1870, he suddenly experienced diplopia with internal strabismus due to palsy of the left sixth nerve. This was not preceded or accompanied by neuralgia or by general symptoms. This affection gradually disappeared in the course of two months.

In July, 1871, the present disease made its appearance. Dr. R. was then in camp, and had been much exposed in severe marches. On arising one morning he noticed a marked degree of palsy in his left hand, the abduction of the thumb being impossible and opposition almost lost. At the same time both hands felt stiff and numb, though whether this was from palsy or from cold he could not decide. Dressing was very difficult that morning. The result of this attack was marked palsy of parts of the left hand and slight impairment of the motor functions in the right. The doctor cannot state positively what was the condition of sensibility in the right hand at that time. Three months later neuralgic pains ap

peared in the left thumb and forearm, and at the same time the thenar eminence on the left side began to waste rapidly. About the same time a diffused (corset-like) sense of constriction was noticed round about the chest, extending from beneath the clavicles to the lower ribs. At that time there was no palsy or wasting of the right hand; no symptoms in the face. The gait was unsteady, and fatigue easily produced. Closing eyes and attempting to walk in darkness made this unsteadiness greater. Late in the autumn of 1871 numbness (anæsthesia?) was first positively noticed in the ulnar distribution of the right hand, and this has since increased. At that time there was no numbness on the left side. This hand first became numb in the summer of 1872, one year after the palsy.

During the past six months the atrophy of the left hand has increased, as well as the anæsthesia. During this period the right thenar eminence has shown a beginning of atrophy about its centre (flexor brevis pollicis); the abductor indicis and the dorsal interossei also show some wasting. The fingers have been very awkward in use; the medius and annulus seeming to stick together. There has been much increase in the anæsthesia of the right hand. No general symptoms have appeared except moderate emaciation.

Examination.—The patient is a tall spare man of good muscular development, and no sign of disease except his wasted hands. Nothing abnormal about eyes, internally or externally. Facial muscles act well. On the left side there is a considerable area of partial anæsthesia in the range of distribution of the superficial branches of the supra-orbital, infra-orbital, and malar branches of the fifth nerve. Contact is hardly felt in this region, but painful impressions are perceived. The upper part of the trunk is the seat of much numbness, and some anæsthesia (?). At times it seems to patient as if a cuirass were round about him, extending as low down as the false ribs and the umbilicus. No such sensation lower down.

Both hands are the seat of muscular atrophy and anæsthesia distributed as follows: The left hand is extremely wasted. The small muscles of the thumb have disappeared with the exception of the inner part of the flexor brevis and adductor.

Complete extension and opposition of the thumb are impossible. The muscles of the hypothenar eminence, and the interossei are uniformly wasted. The atrophied muscles yield no contraction to the strongest faradic current, and only slight fibrillary contractions to the interrupted current of thirty-two elements of Stöhrer's galvanic battery.

The right hand exhibits very moderate wasting of the interossei, and a narrow streak ($\frac{1}{4}$ inch wide) of positive atrophy in the abductor and opponens pollicis. The interossei controlling the index and medius fingers are most affected. The various muscles of the hand, excepting the atrophied band in the thenar group, respond well to both currents. The handwriting is much altered and laborious; the patient feeling as if the difficulty were one of inco-ordination, though this is not strictly correct. On neither side is there the main-en-griffe which is so characteristic of extreme palsy of the interossei. The forearms and the rest of the body are free from paralysis or atrophy.

Sensibility is much impaired in both hands, but more in the right; so that we see in this case an imperfect example of the phenomena accompanying a lesion in one half of the cord. The degree of tactile anæsthesia is great, but pain and temperature are everywhere perceived when the stimulus is sufficiently strong. In the right upper extremity the loss of sensibility is in the inner (ulnar) side of the lower arm, forearm and two-thirds of the hand, the whole of the fingers and part of the thumb anteriorly. The left upper extremity exhibits a patch of anæsthesia a little different in shape. Anteriorly, the inner (ulnar) half of the lower arm and of the forearm is slightly anæsthetic, and the same is true of the same parts of the posterior aspect of the forearm and hand. In the hand the anæsthesia almost follows the distribution of the ulnar nerve.

There is a faint feeling of numbness in the remainder of the upper extremities as high as the acromion processes, but there is no true anæsthesia.

In the lower extremities there is nothing objectively abnormal. At times there is marked uncertainty in walking, patient feeling in danger of staggering against persons and

things. There is no ataxia, and while standing with eyes closed no great oscillation. The doctor was formerly a great walker, but now he is easily fatigued by half a mile of promenade. No rectal or vesical symptoms.

Diagnosis.—I reject progressive muscular atrophy, because of (1) the paralytic onset; (2) the occurrence of anæsthesia; (3) the limitation of disease to the hands; (4) the want of symmetry in the wasting. There was probably a small hemorrhage in the spinal cord at the time of sudden paralysis of the left hand. If there was a clot it must have been very small, and was located in the left anterior grey horn in the middle of the cervical enlargement. From this focus a myelitis has extended in a direction chiefly downward and across the median line. The most remarkable feature of the case is the grouping of symptoms in the order assigned by Brown-Séquard to spinal hemiplegia, i. e., more palsy on one side (same side as lesion of spinal cord), and more anæsthesia on the opposite side.

During the autumn and early winter of 1873 I treated Dr. R. systematically. The local treatment, having for its object the improvement of the atrophied muscles, consisted in thorough galvanization of the parts, friction, etc. A few fibres of the left thenar eminence seemed to revive and grow after weeks of patient care, but no real progress was made.

It was otherwise with the internal treatment. Under the use of nitrate of silver, arsenic, cod liver oil, etc., the myelitis was undoubtedly arrested. Before leaving for the Pacific coast early in 1874 Dr. R. could walk much, felt less inclination to stagger, was much less conscious of the cuirass feeling, and gained a great deal in general vigor.

Dec. 1, 1875. In the last eighteen months no medicines have been taken, yet the disease has made no progress. The hands are about the same, he has hardly any sense of constriction about the thorax, he walks perfectly well, and his health is good.

Looking over the history and progress of the case in the light of recent discoveries in the pathology of spinal paralysis, I am disposed to modify my first diagnosis somewhat. I adhere to my denial that the case was one of progressive

muscular atrophy, but doubt if at any time there was hemorrhage in the cord. The original lesion may have been a rapidly-developed limited myelitis in the left anterior grey horn in the lower cervical region, with subsequent chronic myelitis in various directions, chiefly across the median line, backward and downward. The case bears a certain resemblance to cases of acute spinal paralysis in the adult. The weight of evidence is very nearly equally in favor of both my hypotheses.

1877. I have several times met Dr. R. in the last two years, and he has always expressed himself as perfectly well except in his hands, which remain as they were in 1873. All signs of active myelitis have long since disappeared, and we may assume that the disease has definitely come to a stand-still.

CASE II.—Atrophic paralysis in both hands with slight anæsthesia; neuralgic pains in both arms; weakness of legs. Treatment by active counter-irritation, mercury and iodide of potassium, galvanism; cure.

Mrs. H., aged 53 years, was brought to me by my friend Dr. Conrad of this city, on July 30, 1877. I obtained the following history: Some time during January of the present year she began to experience pain and numbish sensations in the tips of the index and medius fingers, later in thumb and palm of hand. There were no abnormal sensations in the ring and little fingers. The left hand alone was at first affected. She thinks that previous to January she had had some pains in arms, but cannot describe them. These pains (those occurring in late winter) were followed by weakness and wasting of the hands.

In May, when seen by Dr. Conrad, there was the following condition: The right hand was only a little weak; the left was the seat of neuralgia and numbness as above described, the left thenar muscles were atrophied, and there was marked loss of power. Since that time the pains have been more clearly neuralgic, following the course of the median and ulnar nerves from the palms to the bends of the elbows, occurring in paroxysms every ten or thirty minutes, very seldom affecting both arms at the same time. There seemed to be more pain on the right side. About the middle of June numbness showed itself in the tips of the medius, index,

thumb and palm of right hand. No numbness in range of radial and ulnar nerves. Closing hands has made the pain worse; feeling in fingers and palms is a sore, scalded sensation. Has had some pain in the middle of the back below the shoulder; no spinal pain strictly speaking; no cerebral symptoms; no numbness in feet, but legs *have been weak;* no spinal epilepsy or cincture feeling. Complains of slight dysphagia.

Dr. Conrad has given the patient small doses of biniodide of mercury and iodide of potassium, and had applied an ascending stabile galvanic current from the hands to the back of the neck.

Examination.—Slight but distinct tactile anæsthesia in fingers supplied by median nerves (including inner half of annulus); for example on the tip of the medius finger the points of the æsthesiometer are distinguished only at 4—5 mm. apart. There is marked paresis of both forearms and hands. Left hand tremulous. The only atrophy visible is in the outer part of the left thenar eminence. The other muscles are weak but not visibly wasted. A faradic current applied to the median and ulnar nerves (nerve current) produces contractions in all muscles except the part of the left thenar eminence which is wasted. The median nerves are not tender or unduly irritable under pressure in any part of their course.

The patient has had several miscarriages and other symptoms which justify a suspicion of syphilis. Has been taking thirty grains of iodide of potassium a day, and galvanism.

I made the diagnosis of central myelitis in the upper part of the cervical enlargement, chiefly in the left half of the organ, involving the anterior grey horn. My advice was to ensure absolute rest for the hands and arms, patient not even to feed or dress herself; to apply mercury by inunction, and to give the iodide of potassium in much larger doses. The actual platinum cautery was shortly applied over the upper cervical vertebræ.

August 11. Again seen with Dr. Conrad. Patient is no worse as regards pain and atrophy. The legs are perhaps weaker. Mercurial ointment to the amount of ʒ ix. has been used without effect on the gums; has had ʒ ii. of iodide a day. Advise repetition of cautery every other day, the use of

℥ iii. iodide per diem, more inunction, also ℥ i. of Squibbs' fluid extract of ergot at bed time. Mrs. H. has been rather careless in respect to resting hands. She is strongly urged to do nothing whatever with them. Very mild galvanism to arms and spine.

Sept. 11. Patient is better in some respects. There is no increase of wasting, less pain and dysæsthesia in fingers. Legs are weak, but without increased reflex. In the night the hands become clenched. A little tremor is observed in the lips. Treatment has been faithfully pursued: counter-irritation, rest, biniodide of mercury and iodide of potassium in large doses; ergot up to ℥ ii. at night. The gums have been kept a little tender. During the winter iodide of potassium internally, and galvanism to the hands and spine in the shape of the ascending stabile current, constituted the treatment. Almost absolute rest was enforced. Improvement appeared and continued.

March 9. Seen with Dr. Conrad. Patient is practically cured. The muscular masses of the hands are fully restored; grasp is good. Very little neuralgic pain is now felt in the arms, but lately some pain has appeared in elbows and knuckles. There is a mere trace of numbish feeling in the fingers. Legs only feel weak after going up stairs. Continues iodide and galvanism.

A few weeks ago, in May, I met Dr. Conrad, and he informed me that with the exception of occasional neural pains, his patient was perfectly well.

In concluding this case I would express my thanks to Dr. Conrad for his courtesy in allowing me to make use of it, and would congratulate him upon the skill with which he carried out the treatment agreed upon.

CASE III.—Extreme anæsthesia of both upper extremities and of the upper part of the trunk; atrophic paralysis of the right hand and of many muscular groups of the left upper extremity; contraction of the left pupil. No symptoms in the lower limbs except rigidity in the left leg. Central myelitis in the cervical enlargement of the spinal cord, with probable formation of a cavity.

Annie M——, single, aged 23 years. Seen at the Manhattan

Hospital, May 18, 1878. When twelve years of age patient's ankles were weak for two years, but entirely recovered. Five years ago, when eighteen years old, she noticed numbness in the tips of the fingers of both hands, extending to the shoulders in the course of a few months. The legs were not numb. Next there began wasting of the left shoulder, hand and arm. Since at least two years the left arm has hung useless by her side. The right hand has wasted more recently. Has felt fibrillary contractions from the start, and believes that sensibility was early lost in the hands. The left leg is weak, and in the last three months it has been getting very stiff. At night her whole body jerks. Micturition is only slow; constipation is present; menses are regular. No dyspnœa or palpitation. Has some occipital headache. According to patient's statement, feeling in the feet and legs is normal, though at times the left foot tingles. Has never had neuralgic pains in arms or legs. General health good.

Examination.—Both pupils are small and equal in a bright light; in the shade the left does not expand, while the right does. No paralysis, atrophy, or anæsthesia in the face. Face not flushed. The upper part of the thorax and the shoulders present marked anæsthesia and analgesia; in the upper part of the back and shoulders behind, sensibility to touch and pricking is fair. In the arms, forearms and hands sensibility is wholly lost; patient has often burned herself without knowing it. The left upper extremity is extensively paralyzed, while the right is only partially so in the hand. State of muscles: On the right side only the interossei are weak and wasted. On the left side the following muscles are atrophied and palsied: interossei, biceps, brachialis anticus, supinator longus, supra and infra spinati, and deltoid. The flexors and extensors of fingers and wrists, the triceps and pectoralis are simply feeble. The scapulæ are not winged; there is a slight dorsal spinal curvature convexity to the right; no kyphosis; standing with eyes closed is difficult; left lower extremity is the seat of increased reflex and epileptoid trepidation. No atrophy or paralysis of lower limbs. Measurements: right calf, 32 c.; left, 31 c. Tendon (knee) reflex increased on both sides.

Re-examined in bed, May 19, 1878. The face presents only the symptoms above noted. The upper extremities, as high as the insertion of both deltoids, are perfectly anæsthetic to touch, reasonable pricking and pinching, and to firm grasp. On the upper thorax and back on the shoulders, she feels touch fairly well, but pricking very little. In front, normal sensibility reappears at the level of the fourth rib. On the back the limiting line is indistinct and seems to be somewhat below the spines of the scapulæ. Motor symptoms in arms as above. The lower limbs and the abdomen present no anæsthesia. Legs and thighs are well nourished; left calf very firm; left toes are "en griffe;" foot not inverted. Patient's mother states that her left leg is very stiff in bed and on first rising in the morning, but after walking it becomes more limber. Patient denies having a cincture feeling or dyspnœa. When numbness appears in the left leg (rarely) it extends to the knee, but patient qualifies the statement by saying that the feeling is more like cramp. At the age of twelve years it would seem that patient had an attack of palsy in the left leg below the knee; the leg and foot were swung heavily for awhile. At that time the left arm was not affected. She recovered perfectly in two years. Every symptom now observable in the left leg and foot has appeared within the past year. The temperature of the hands, taken for three minutes with a Casella thermometer, placed between the index and medius, is on the right side 94° 5′, on the left 95°.

Electrical examination, June 3d. Faradic current, right upper extremity. All muscles give good reaction except the outer group of dorsal interossei. Left upper extremity, good reaction in trapezius, pectorals, triceps; proper extensors of wrist and fingers; faint reaction in long flexors of wrist and fingers, inner third of deltoid, opponens pollicis; no reaction in hypothenar group, inner part of thenar group, interossei, supinator longus, and biceps.

It is plain that in this case there is a great lesion in the cervical enlargement of the spinal cord; probably a diffused central myelitis with formation of a cavity. The lesion was first developed in the æsthesodic tract of the cord, and is yet greater there than in the kinesodic system, though the anterior horns,

especially on the left side, have become involved. The cilio-spinal centre in the left side of the cord is injured, as shown by the contracted pupil. It is noteworthy that no symptoms of vasomotor paralysis are present, thus affording a demonstration of the independence of the cilio-spinal and facial vasomotor centres. The symptoms in the left lower extremity point to the existence of secondary descending degeneration in the lateral column.

It is very remarkable that with so much disease in the cervical enlargement, the various nervous conductors for the lower limbs and abdomen should not be interfered with. As regards the uppermost limit of the lesion, that cannot be above the origin of the fifth cervical nerve, as the diaphragm acts perfectly.

A few words as regards the pathology of these cases.

In all three the æsthesodic, kinesodic, and musculo-trophic tracts in the cervical enlargement were affected.

In cases II. and III. the affection was probably inflammatory—perhaps syphilitic in case II. In case I. a doubt may exist as to whether hemorrhage took place, or whether there was a suddenly developed (as in infantile poliomyelitis anterior) inflammatory lesion. Even if there was hemorrhage at first, a secondary adjacent inflammation occurred and presented many of the symptoms. I am disposed to believe that a central cavity has formed in the cervical region in case III., because of the resemblance of this case with the cases of central myelitis with formation of cavities reported by Schueppel, Hellopeau, and others.

As regards therapeutics, the exceedingly satisfactory issue in case II. was perhaps obtained because the lesion was essentially syphilitic, and the proper remedies were freely and persistently used. Yet I am disposed to attribute much of the recovery to the almost absolute rest enforced. On careful consideration, in view of the apparently progressive nature of the lesion, the issue in case I. is almost as gratifying. True, the atrophied and anesthetic hands were not restored, but symptoms which seemed to point to approaching general paralysis were permanently dispelled. Case III. has been but a few days under treatment, and is a very unfavorable one.

ART. IV.—A CASE OF CUTANEOUS DISEASE, CONFINED TO THE PALMAR SURFACE OF THE THUMB, TREATED BY SECTION OF FILAMENTS OF THE MEDIAN NERVE.

BY J. J. PUTNAM, M. D., BOSTON.

IN a paper published in the *Hospital Gazette*, March 1, 1878, Dr. Edward Wigglesworth reports an interesting case of disease confined to the skin of one thumb, and consisting essentially in the eruption of successive crops of pustules, or bullæ, containing pus, which began in the cutis vera as minute points, on the face of the thumb or along the margin of the nail, sometimes, apparently, in its bed, then enlarged and coalesced; finally, unless interfered with in their course, forming one great blister filled with pus, and covered with a thick layer of epidermis. The pulp of the thumb would swell during the height of the attack, then shrink to less than its normal size, and lose its natural firmness, feeling soft and flabby, and was tender to deep pressure. Although this state of things had existed for four years, at the time of the intervention of which I am about to speak, no other parts than those mentioned had become invaded, the inflammation confining itself within the distribution of the median nerve, except for the bed of the nail, which is supplied by the radial. The nail itself had suffered materially in its nutrition.

Except for one other case closely resembling this, and also under Dr. W's care, we had neither of us met with anything of exactly this sort, and were led, rather by exclusion than by any direct evidence, to the diagnosis of nerve-lesion of some kind. Both patients had, in fact, suffered slight injuries, which Dr. W. regarded as having possibly set up irritative changes in the nerve-trunks, though the connection was not clear.

The injury, in the case now in hand, was on the back of the

thumb, *i. e.*, in the distribution of the radial nerve, and had left behind only a linear pliable scar.

Both cases appeared to be, from time to time, materially helped by frequent local applications of galvanism, and one had finally recovered entirely, though no longer at that time under treatment.

The present case, however, having continued without permanent improvement under every possible treatment, we finally decided, as a last resort, to cut the sensitive twigs of the median nerve supplying the affected region, and this was accordingly done, skillfully, under the carbolic spray, by Dr. H. H. A. Beach, on March 29th, 1878—a quarter of an inch of each digital branch of the median being excised. The excised portions were afterwards examined, but no evidences of disease found in them. The first time that the dressings were removed after the operation—at the end of twenty-four hours—two new minute spots of the unwelcome disease showed themselves, tauntingly, in the midst of the anæsthetic patch of skin; on the following day they had enlarged, and, presently, others also made their appearance, and coalesced, running, in short, their usual course. The patient, who had been taking cod liver oil, was given in addition, phosph. of iron, qui. and strych., at Dr. Beach's suggestion. It was hoped that this first inflammation had been either really under way before, or that it was excited by irritations starting from the peripheral portion of the severed nerve—and this hope appeared to be well founded, for with the exception of a few spots which appeared within the first week or so after the operation, and quickly disappeared again, no further sign of the original trouble showed itself within the distribution of the median nerve. The wound healed sufficiently well, but left, as might have been expected, a dense, in part deeply-seated, scar. In portions of the area supplied by the radial nerve, some signs of irritation developed themselves after the operation, as I had feared would be the case. The patient suffered from occasional neuralgic pains along the back of the thumb, and this region, as well as the bed of the nail and the neighborhood of the scar, have been quite tender to pressure. Moreover, there had been a little suppuration under the nail, and, quite recently, a

minute pustule has made its appearance on the outside of the thumb near the nail. This tenderness, however, has always disappeared, in great part, when the thumb has been kept perfectly still by a light splint. The nail has, the patient thinks, grown more slowly since the operation than before; but, on the other hand, it has a healthier look, and the pulp of the thumb has also become firmer. In spite, therefore, of the signs of irritation, which manifest themselves in the distribution of the radial nerve, especially when the scar resulting from the operation is compressed, or is dragged upon by the action of the muscles, it looks at present as if the main disease were likely to be kept in abeyance, at least, till the peripheral end of the nerve shall have become regenerated.

The manner in which surgical interference in such case is of service, supposing it to prove so in fact, is still obscure, as is also the reason of the occasional favorable action of nerve sections in neuralgia.

In both classes of cases we, no doubt, are sometimes able to cut off a source of irritation to the suffering tissues, whether these be the skin or the nerve centres. Sometimes, however, nerve section seems to be of service where we do not cut off any source of irritation at all, strictly speaking, indeed where no such source of irritation exists, and for such cases as these we are obliged to assume that the operation acts by protecting the diseased nervous centres for a time from a set of excitations, physiological when regarded by themselves, but pathological when considered in relation to the irritable state of the central parts. In no other way, I think, can we explain the good effects claimed to attend the seemingly irrational treatment of some cases of blepharospasm by section of the supra-orbital nerve, where no source of irritation can be discovered in its distribution.

Again, in still other cases, those for instance of blepharospasm, which are said to be relieved by acu-pressure of the supra-orbital nerve, operative interference seems to exert an inhibitory action upon the diseased parts. What share in the favorable result is borne by each of these influences, it is often difficult to determine, though it would seem, theoretically, that we could sometimes decide as to one of them by ensuring

the diseased nervous centres complete rest, by mechanical or other means. This treatment is often attempted, to be sure, but, perhaps, not often carried out with such extreme precautions as seem necessary to ensure success in difficult cases.

Nearly three months have now elapsed since the operation, and the thumb can already be used somewhat without provoking trouble in the distribution of the median nerve, except at the very extremity of the thumb and along the nail, where a few spots have appeared at considerable intervals.* It is noticeable, in this connection, that a deep prick with a needle or a maximal irritation with a single fine wire connected with an induction battery, will occasionally provoke sharp pain at isolated points, even in the middle of the palmar surface of the thumb, still more along its borders. This condition was first examined for carefully, and discovered about six weeks after the operation. The recent spots of disease do not appear, however, to correspond especially with these sensitive points. There is no longer any neuralgia in the thumb, but redness, swelling, and soreness is easily provoked by irritant applications, or by too long-continued use.

*The longest intervals of spontaneous remission have been five or six weeks, but for the past two years there has been none longer than a week.

Art. V.—CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

By Edward C. Spitzka, M. D., of New York.

PART III.

The relation existing between the external shape, and the internal structure of certain parts of the Encephalon.

THE great law announced by Meynert, that the diameter of a fasciculus depends upon the mass of grey matter with which it is connected, is susceptible of some modification. The nearer we approach the highest centres, and the higher we pass up in the animal scale, the less exactly does this statement apply. I have compared the *pes pedunculi* of a human brain, whose cortex was almost as richly convoluted as that of Gauss, the astronomer's brain, with the *pes pedunculi* of an epileptic dement, whose convolutions were of the simplest type, and although I found a difference in favor of the former, yet this difference was by no means proportionate to the great preponderance of cortical grey matter.

On the other hand, with the increasing development of the cortex, the aggregate mass of projecting, associating and commissural fibres, constituting the *centrum semi-ovale*, grows more rapidly than the cortex itself. A richly convoluted brain has relatively more white substance than a poorly convoluted one, and every transverse section of the encephali of lower animals proves this; the higher we ascend the thinner becomes the relative thickness of the grey substance as compared with the white lamina entering it. We must of course consider the fact that, at the same time, the absolute thickness of the cortex increases, and not only this, but it also becomes far richer in cellular elements. I am able to fully confirm Meynert's statements that of the several layers constituting the cortex, that one which underlies the pia, designated by him as the *ependyma* formation, and which represents the most barren part of the grey matter as far as the cells are concerned, is thickest in the lowest animals. I find this ependyma formation to

measure from three-fourths to four-fifths of the entire cortical thickness, in the snapping turtle. In the common bat, it amounts to one-fourth on the convexity of the hemisphere; in the cat to one-eighth, and in the human brain it varies from one-twelfth to one-sixteenth.* While these figures confirm the proportionate progress of the cortical richness as to cells, they differ absolutely from those given by Meynert, who assigns:

1. For the human being, - - - $\frac{1}{8}$ — $\frac{1}{10}$
2. " " capuchin ape, - - - - $\frac{1}{6}$ — $\frac{1}{7}$
3. " " dog, - - - - - $\frac{1}{6}$
4. " " cat, - - - - - $\frac{1}{5}$
5. " " bat, - - - - - $\frac{1}{4}$
6. " " calf and deer, - - - - $\frac{1}{3}$

of the entire cortical thickness, to its outermost layer.

Although this fact, conjoined with several others, would tend to prove that the ganglionic cells of the cortex are its noblest, and most differentiated elements, and are intrinsically connected with conscious sensation, thought and action, yet I must confess that I have recently found reason for considering Henle's supposition that even the so-called neuroglia is of a nervous nature, as a not improbable one. A remarkable discovery made by a recent writer in the *Archiv fuer Wissenschaftliche Zoologie*, seems to confirm this view. He found that the symmetrical, globular and convoluted masses which lie in front of the cephalic ganglia, in intimate connection with them, and which represent the cerebrum of the vertebrata, in the bee, ant, and other hymenoptera, consist of two optically different substances, the outer being *grey*, the inner *white; neither this grey nor this white substance show any cells or fibres*, they are both composed of a fine molecular substance.

The functions of such molecular substance (chemically and optically almost identical with the protoplasm of the nerve cells), are probably similar to those of the latter, although doubtless

* In a macrocephalic idiot whose brain *weighed* 68 *ounces*, and which was fairly convoluted, the thickness of the ependyma layer was one-seventh of an inch.

far more rudimentary. The palpable lesions of insanity are often limited to this tissue!

Thus much for the present, regarding the general relations of the grey and white substance of the hemispheres, let us glance for a moment at the development of the convolutions. Aside from the well known fact that, *cæteris paribus*, large animals have a more richly convoluted brain than small animals, aside from the fact that the arrangement of the convolutions depends on the proportion which the diameters of the cranium bear to each other, and the influences which are exercised during embryonic development, we must accord some prominence to the influence of hereditary transmission, exercised in a manner independent of the mechanical surroundings. We find for example, crania among the pachydermata which are similar in their diameters to the crania of certain carnivora and rodentia, yet the convolutional arrangement, aside from a general resemblance in the parallelism and direction of the folds, is on an entirely different plan in all three of these groups. The extremely dolichocephalic fox resembles more closely the most brachycephalic of the cat tribe, than it does that of any herbivore, no matter how similar in its cranial diameters. If we examine such casts of crania of extinct species of animals as have been found by palæontologists* in the tertiary strata, notably of the marsupials and carnivora figured by Gervais, we find everywhere the same great family type expressed in all. It is suggestive in this connection that a living marsupial, one which strongly suggests the extinct branch from which the placental carnivora have been derived, possesses a strikingly carnivorous brain, as regards the convolutions (Thylacinus).

It is not a correct inference to suppose that special convolutions signify a greater development of special functional areas. The arrangement of the medullary strands on whose abutment and peripheral termination the function of any cor-

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* Some valuable information regarding the morphology of the convolutions may be expected from fossil crania, and American beds may contain many valuable specimens of this kind. I am informed that Princeton College possesses the supposed intracranial cast of a Dinoceras (Marsh).

tical territory depends, is too inconstant to admit of this; we shall find that such medullary strands may terminate in different cortical areas in different animals, and our logical deduction must be that, the simpler functions may be situated differently accordingly. Even the great fissures, such as the central, the intraparietal, the calcarine, the transverse occipital (of apes), do not always separate differently connected areas, for a bundle frequently divides at the base of such deep fissures, and gives off fibres to both the cortical areas separated by these fissures. With regard to the less important secondary gyri, their course and number, dependent on the combined influence of all the influences above referred to, is often varied by what might be termed *fortuitous* circumstances, namely, the position of vessels. While contrary to Reichert's views, it is now generally conceded that those sulci which characterize any given brain, are altogether independent of the position of vascular twigs, it seems to me that the less constant secondary and tertiary gyri are not always free from such an influence. I have frequently found that every transition exists between the simple groove in which the medium-sized vessels cross a large gyrus, and the shallow sulci which separate the latter into secondary folds. It could be urged against this that the vessel might have *secondarily* selected a pre-existing channel, but it is well known that in man at least, the tertiary and many of the secondary gyri, are formed after birth, when all the larger vascular twigs are already developed.

That the primary sulci which characterize a given brain are independent of such influence, can be readily seen in the case of the pteropus, an animal whose brain has not yet received due attention, and which it is my purpose to describe ere long. Here there are several rudimentary sulci which run parallel to the median fissure, while the arteries in their vicinity run a transverse course, and cross these fissures in a vertical direction. Here obviously, sulci and vessels are developed independently of each other, and where at the point of crossing of a vessel and a sulcus, one influences the course of the other, it is the former which is diverted from its direction, not the latter.

Altogether the convolutions of the cerebral convexity are

dependent on the mathematical principles enunciated by Wundt. These principles are to be modified in the case of the S-shaped convolution, or the cornu ammonis. The cornu ammonis is the first convolution proper formed in the lower mammalia. It is relatively larger in the Vespertilionidæ, and Muridæ than in other animals which I have examined in this direction. I believe that I have found its first trace in the turtles. It seems that in the latter class of reptiles, the free internal edge of the hemispheric vesicle is turned inwards, towards the ventricular cavity. It is on this part of the edge that a structure similar to that of the bat's cornu ammonis is found.

This condition is found at one period of embryonic existence in the mammalia. It is easy to understand that, when a body endowed with the elastic tension of organic substances has received a curved direction, it will, unless external circumstances interfere, continue its growth in that direction. It is this principle which produces a spiral cochlea from a simple horn-shaped sac. Spiral also would be the cornu ammonis, if it did not meet with resistance to further spiral extension before completing the first turn; that resistance is furnished by the meanwhile hypertrophied hemispheric wall, and the second part of the S is produced by a curved growth in the opposite direction. After the cornu ammonis has reached its perfection in the lower mammalia, it loses in relative importance in the higher, and I have remarked is proportionately less voluminous in the herbivora than even in man. The coincidence may be without any deeper significance, but it is certainly striking, that the more rapid the natural motions of a mammal, the larger relatively is its cornu ammonis; the bat and mouse are remarkable for the size of this convolution, and it diminishes gradually as we pass from the rabbit, to the dog, monkey, man, and larger herbivora.

Not only does the size of a fasciculus depend on the mass of the grey matter with which it is connected, but it also influences the size of other grey masses with which it establishes associations; the well known dependence of the lateral lobes of the cerebellum upon the opposite cerebral hemisphere is a well known instance. In the dog, we have moderately devel-

oped cerebral hemispheres, and consequently small cerebellar hemispheres, the vermis being proportionately large. In the Cynocephalus the hemispheres of the cerebellum are already sufficiently developed to conceal the *flocculus* from view, but the vermis is still fairly developed. In the anthropoid apes, as the chimpanzee, and in man, the hemispheres flare out with a globular border, and the vermis is rudimentary.

While these changes, dependent on the progressive increase in the size of the hemispheric dimensions, have taken place, the commissural tracts have undergone a like change. There is a wide chasm between the medulla and pons of the rabbit and those of man, yet this chasm is completely bridged over. In the rabbit, the crura cerebri are small, and the pons, which is derived from them, is likewise rudimentary; on both sides of the point where the anterior pyramids emerge, there is a band of transverse fibres known as the trapezium (Dean), which is about as deep as the pons itself. In the dog, the pons already preponderates over this trapezium, in the baboon, but a small edge of the latter is visible, *in the chimpanzee* and the human being it is completely hidden from view.

With the increase of the pes pedunculi and the parallel increase of the pons proper, and progressive concealment of the trapezium, the anterior pyramids gain in bulk; as they gain in bulk, they become more columnar in character, and a body which has hitherto lain behind them is pushed to the outer side. This is the explanation for the presence of a distinct olivary protuberance in man, and of its absence in the lower mammalia. It is not the olivary body which is wanting in the latter, but that position which would cause its protrusion is not given it. Another element which enters into this question of the prominence of the olive, is the development of the dentated nucleus of the olive itself; a simple lamina in the lower mammals, a rudely folded line in the dog, it increases to the richly crenulated nucleus of the human olive,* and consequently gives to the olivary prominence more bulk.

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* I am not prepared to confirm the statement of a recent French writer, that the olivary body of the dog corresponds only to the internal accessory olive of man.

The reason for this richer development of the olive in the highest mammals lies in its dependence on the cerebellar hemispheres, a dependence already hinted at in Part II., as due to its connection with the restiform decussation.

We thus perceive that anatomical peculiarities, apparently of the most independent and disconnected character, are, in reality, but an expression of one great harmony, and that the shape, volume, and relations of the basiecerebral parts are but the expression of the ratio of prosencephalic preponderance. This preponderance increases to such a degree in the anthropoid apes and man, that the cerebral hemispheres may well be likened to a great empire, on whose strength depends the proper subjection and prosperity of tributary states; a parallelism carried out in the case of unilateral atrophy of the cerebral hemisphere, where the opposite hemisphere of the cerebellum, olivary nucleus, brachium conjunctivum and anterior pyramids equally undergo diminution in volume, or, in the case of the imbecile or terminal dement in whom the rhythmical motions of lower animals once more become manifest.

In all the *amniote* vertebrates, the anterior inflection of the embryonic axis is the factor which determines the possible predominance of the prosencephalon. I say possible, because this preponderance is not always actual, especially in the case of reptiles, and to a lesser degree in birds. One *mechanical* reason is, that in the embryonic reptile and bird, the primordial ocular vesicle early attains such dimensions that the parallel developed mesencephalon must also predominate in growth, and this prevents the backward extension of the hemispheres, on which their ultimate increase depends. That another, not *purely* mechanical factor is here active, and that this is after all the most important one, is shown in the great difference which later obtains between the reptile and bird. In the former, the hemispheres possessing but a slight impetus to growth, remain stationary as soon as an extension backwards is prohibited by collision with the mesencephalon; in the bird, however, the hemispheres overcome this obstacle, and since it is impossible to crowd the corpora bigemina backwards, they separate them and push them downwards and laterally. This

explains the apparently anomalous position of the corpora bigemina in birds, a position which is carried to its furthest extreme in the parrot.

In examining into the question of the prejudicial influence of the early mesencephalic preponderance on cerebral growth, and referring the former to the coincident great size of the eye, we find that the latter element exercises another influence in the way of cerebral retardation, and that of a direct mechanical nature. If we examine the whole range of the amniota, we shall find, as a rule, that the larger the eye, and consequently the orbit, the narrower and more hollowed out is the anterior portion of the hemisphere. The rabbit has a narrower frontal lobe than the rat, the badger than the mole, the dog than the bear, the barn fowl than the parrot, and the aves, as a whole, than the mammalia.

In tracing cerebral morphology to certain facts of embryology, and to the influence of peripheral on central development, or, of external mechanical factors, we should not forget that while these influences can be legitimately considered to affect cerebral development, they are by no means primary causes. The dimensions of the cerebrum of a reptile are the result of the operation of the same great hereditary influence which determines the inflection of the basicranial axis, and the relatively early and preponderating growth of the eyeball. The various elements which *seem* to determine the special shape of a given brain, are but an expression of the mutual harmony existing between physiologically related organs. Where the ancestry has acquired a cerebral development beyond that which an allied animal group possesses, this development leaves a sufficiently powerful impress on the medullary trace of the descendant, to enable it at the expense of typical homology, to overcome prejudicial external influences, as in the case of the bird's brain just cited.

In comparing the various classes of vertebrate animals with each other, we find that each class has separate characteristics of its own, and that the earlier a special class has separated from the common vertebrate trunk in geological history, the more aberrant is its cerebral structure from that type which is considered the type characteristic of the vertebrate brain.

The bony fishes (Teliosts) which have become a separate group as early as the later Silurian age, for example, have lost the lateral ventricles entirely, their cerebral lobes are solid. In this fact I can find no reason for going so far as Wilder, who asserts the cerebral lobes of the teliost to be non-homologous with those of other vertebrates; he seems to have overlooked the fact, that originally the whole nervous system of such embryos, as in the case of the embryonic salmon and Gasterosteus, is apparently solid. It illustrates rather what changes may be effected in any organ through the lapse of time by, as yet, unknown agents.

The birds furnish another illustration. This class, according to the unanimous testimony of European and American explorers, constitutes the terminal developmental stage of a group of saurian reptiles which separated from the common reptilian stock at about the Triassic period. In all the immense lapse of time intervening between then and now, the optic lobes have become crowded to an anomalous position, and other changes have occurred to be yet considered.

The time will undoubtedly come, when we shall be able to construct the ontogeny and phyllogeny of the brain.

In drawing our inferences from cerebral structure, as to its function, we shall therefore have to consider not one, but innumerable collateral questions. The shape, volume and relations of the encephalon depend, first, upon the type of brain peculiar to the class of animals to which a given species belongs; second, to the aberrant influences which may have become gradually potentialized in the direct ancestry; thirdly, to mechanical influences, exercised upon the brain, and in accordance with ontogenetic harmony; fourthly, to physiological atrophies or hypertrophies of special peripheries: fifthly, to the influence exercised by various centres on each other; lastly, to individual variations. Many errors committed in the past, have been due to the fact that only one of these elements has been considered at a time. Thus the great embryologist, His, has been led to refer everything to the influences exercised by the curves of the embryonic axis, and the foldings of certain surfaces. The reformer of modern cerebral anatomy, Meynert, has, as we have mentioned, overlooked class distinctions in

referring the relative size of special strands or centres to corresponding developments of the periphery, and it is needless to refer to the numerous errors committed in regard to the convolutions, not only by those guilty of the notorious *lapsus* of phrenology, but also by recent morphologists.

We shall in the sequel, having closed these preliminary remarks, proceed to study the encephali of certain species of animals noted either for their peculiar zoological position, or for the development of special peripheries.

PART IV.—THE BRAIN OF THE MENOBRANCHUS.

Around the central canal of the cord in this amphibian we find from fifteen to thirty large, oblong bodies, which are closely crowded together. Even in deeply stained specimens one is very apt to overlook a delicate protoplasm surrounding them, and thus to consider as *cell equivalents* what are in reality merely the gigantic *nuclei* of *epithelial cells*. Where these cells flare apart at their base, other bodies, similar in size and appearance to the nuclei referred to, but without any demonstrable protoplasm, are intercalated with one extremity. Succeeding them, similar but more loosely aggregated bodies are disposed without any special order, and, with certain modifications, to be described, these extend to the apices of the grey cornua.

There are a few cells at the extremity of the anterior cornu which, from the anatomical relations of their processes to the nerve roots, are to be considered as genuine nerve cells; an occasional such cell is also found in the most posterior portion of the trigonum cervicale, or at the bases of the anterior and posterior fissures. Starting with these cells and passing to the central epithelium, we shall find every possible gradation between multipolar nerve cells and epithelial cells!

In order to understand this remarkable transition clearly, let us first study the two extremes of the transition series:

I. The nerve cell of the menobranchus consists of a large, finely granular central mass, nearly as large as, and in some instances even larger than, the immense red blood corpuscles of this species. This mass stains deeply in carmine, and

occasionally has the appearance of containing a more or less distinctly marked central body. Around it there is a thin, delicate shell of protoplasm, which is continuous with the axis cylinder and protoplasmic processes. Like the latter it does not stain very deeply in carmine or hæmatoxylin.

Although the central body preponderates so immensely over the outer substance, it must be considered as its nucleus, and this for the following reasons: 1st. The nerve cell processes never can be traced to have any connection with the former, but, on the contrary, their fibrillæ, when their course is clearly visible, can be always traced over or by its side, through the outer substance, to some other process, in a manner similar to that claimed by Beale for the nerve cells of the mammalia. 2d. It stains more readily and deeply in carmine. 3d. It is more granular than the substance which I designate as the protoplasm. 4th. Although sometimes drawn out to an angular shape, at a point where a nerve cell gradually becomes attenuated to a spindle form, it never extends into the processes proper.

II. The ependymal epithelial cell consists of a delicate, apparently hyaline protoplasm, which is thickest at that end of the cell which faces the central canal or the ventricular lumen, and thence gradually thins down until no longer visible at the root of the cell. The main mass of the latter is made up by the oblong nucleus already referred to, which stains to the same degree, and has the same optical appearances and dimensions as the nucleus of the nerve cell of the same animal; it differs from it in the sole respect that it is more oblong; but even here there are exceptions, while a few of the epithelia have spherical or ellipsoidal nuclei, some of the nerve cells have oblong ones.

As already stated, the series of bodies immediately underneath the ependymal epithelium no longer possess a demonstrable protoplasm. They resemble, in every respect, the epithelial nuclei, and are to be considered as *free nuclei*. The further outwards we pass the more spheroidal they become, and now they begin to assume peculiar relations to the delicate fibrillæ which run in their vicinity. While the mere enclosure of one of these free nuclei by a bundle of nerve fibrils might

be considered accidental *per se*, yet when we find that these nuclei are by preference in relation with those groups of fibrillæ which undergo a methodical concentration into axis cylinder and other processes, and when we find that those nuclei have become the rendezvous of such strands running in all the directions, and subdividing in all the manners of a typical multipolar nerve cell, and finally, when we perceive a delicate protoplasmic mold cast into the fibrillar network which encages the free nucleus, we will perceive a process by which the latter becomes elaborated into a nerve cell nucleus.

During this process it undergoes no visible histological metamorphosis whatever. Some of the compound bodies which arise from the condensation of fibrils around the nuclei, and which I consider the physiological if not the anatomical homologues of nerve cells, possess no protoplasm whatever; others show it but indistinctly; but, aside from this, they appear entirely identical with the cells described as nerve cells.

This transition can be much more finely seen in the medulla oblongata than in the spinal cord,—the nerve cells are larger, their processes more numerous, and the layer of subependymal cells is thicker. Although there are no nuclei for the cranial nerves, in the proper sense of the term, yet at the origins of the auditory and trigemini nerves this layer of cells and nuclei is drawn out to a peninsular formation, and here the finest transition forms can be observed. The raphe particularly furnishes fine examples, and I have several specimens of what appear to be cells with two axis cylinder processes, the one entering the other crossing the raphe.

The existence of demonstrable connecting links between epithelial and nerve cells, occurring as it does in the nervous system of an animal which represents a *larval type*, is exceedingly suggestive from an embryological point of view. Hensen* has conclusively traced the development of the nerve cells of the anterior cornu from the proliferating epithelium of the primitive medullary canal, and it appears that in the

*Hensen, "Beiträge zur Entwicklungsgeschichte des Kaninchens und Meerschweinchens;" *Zeitschrift fuer Anat. und Entwicklungsgeschichte*, II., 1875.

menobranchus we have an instance where this ontogenetic relation has remained permanent in a double sense,—first, as regards the undeveloped condition of the nerve cells themselves; secondly, as regards the permanent topographical union of these nerve cells with their mother bed, viz.: the free nuclei and epithelium.

The peculiar manner in which an, as it were fortuitous congregation of fibrils around a free nucleus, results in a compound formation approaching the full nerve cell *gradation* in structure, receives much light from the researches of Schmidt (New Orleans). It adds another to the numerous evidences which are accumulating in favor of the view, that in many, at least of the multipolar nerve cells, the nerve fibrils do not terminate in the nerve cell, but pass through it, so that the protoplasm constitutes merely a cement of the mass. This alters nothing, necessarily, of the physiological views entertained regarding nerve cells, but whether these are to be considered as cellular units in the traditional sense of the term, is a question.

While the medulla spinalis and oblongata contain a fair proportion of unquestionable nerve bodies, and these exhibit quite complex relations to nerve roots and other fasciculi, the higher centres show a much more rudimentary condition. Multipolar nerve cells do not occur here, and no subependymal white nerve substance is developed.

That the lobi optici should exhibit a rudimentary structure is not to be marveled at, since the sensory periphery which is projected in these lobes is itself rudimentary. (The eyes of the menobranchus are rudimentary and concealed under the epidermis.) It and the hemispheres have as a common character, a low epithelium on the ventricular aspect, which is followed by a dense layer of free nuclei, becoming more and more scattered the nearer we approach the surface. This whole thickness of the hemisphere is to be considered cortical substance, and the outer molecular layer, which presents the same appearance as the basis substance of the human cortex, corresponds to the first layer of Meynert. With this, the fact that, where medullary strands enter the cortex from the rudimentary thalamic region, they frequently send bundles or single fibres into this

outer layer, is in no contradiction. Even in the case of the highest animals, certain convolutions of the medial aspect of the hemispheres, which are of a lower structural type, have their white substance on the outside, and the lower mammals exhibit distinct fasciculi originating in this substance or passing through it (rat. rodentia). Among the round free nuclear bodies this neuroglia shows a fibrillary structure, and the closer we approach the ventricular floor, the more distinct this appearance becomes. Where an epithelial cell has been torn loose* by the section knife and isolated from its fellows, it is often possible to see such fibrils entering its basal protoplasm, and this is not infrequently observed to occur with individual attenuated free nuclei nearer the surface. In no case have I seen any indubitable evidence that such fibrils joined the substance of the free nuclei in thin sections. I can always detect a delicate protoplasm around the free nucleus, or where this is absent, trace the fibrils around and past the nucleus to their concentration point at the other extremity of the cell. At that extremity of these rudimentary ganglionic nuclei which is directed to the periphery of the hemisphere, they are drawn out to a point and thus assume a pyramidal appearance; it is possible, therefore, that they with the adjoining fibres may present the homologue of a tri- or multipolar pyramid, but I have discovered no distinct appearances of that kind either in the hemispheres or the optic lobes. In the relatively large olfactory lobes, such bodies are found, and altogether the structure here is better differentiated than in the hemispheres. Many of the cells are forced into parallelism with the convoluted nerve fibres, and together with them constitute *glomeruli olfactorii* such as have been described in higher animals. Here the nerve cells can also be traced from a central epithelium surrounding the ventricular cavity of this lobe.

The nerve fibres of the reticular formation in the medulla oblongata appear to anastomose by transverse and radiatory radicles, but as these very fibres run a wavy course, this appearance may be a delusion.

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*The aggregation of the free nuclei beneath the epithelia is very loose, and the latter, as well as the former, are easily separated in thin sections.

In closing the description of the histological peculiarities of the menobranchus, and before proceeding to describe the morphology of its encephalon, I would refer to a peculiar kind of globules found on the ventricular parietes. These are round globules, or amorphous aggregations of an opalescent greenish material, which, with or without a surrounding granular detritus cover the ependyma; the individual globules are as large as a human red blood corpuscle, and larger, up to twice these dimensions. I found one such body in the centre of the nucleus of an epithelial cell. As to their nature, whether they constitute a secretion, an exudation, or an artefact, I am unable to state anything. I have not verified their occurrence in the fresh specimen, but found them in specimens hardened in bichromate of potash and chromic acid combined, and equally in specimens which had not been touched by alcohol as in those in whose preparation that reagent had been employed.

The central nervous system of the *siren** exhibits the same histological peculiarities as that of the menobranchus, and we may assume that all the amphibia possessing permanent *branchiæ* possess the same. It would be very interesting in this connection to study the brain and cord of the *polypterus*. While the bony fish constitute an independent offshoot from the typical plan of cerebral development of the vertebrata, and the plagiostomi present us with other, but different typical modifications, the urodelous amphibia represent a nervous system of great simplicity it is true, but constructed on an identical plan with that of the sauropsida and mammalia. The study of such a primitive and larval nervous system consequently forms a convenient starting-point for a comprehension of the higher forms. As I shall show it will lead to a considerable modification of the projection theory.

* For two specimens of the siren and seven menobranchi I am indebted to the kindness of Charles Dorner, Ph. D., Superintendent of the N. Y. Aquarium.

ART. VI.—A NEW APPARATUS FOR GIVING ELASTIC SUPPORT TO THE HAND IN CASES OF WRIST-DROP.

BY J. J. PUTNAM, M. D., BOSTON.

(*Communication before the Am. Neurol. Ass'n, June*, 1878.)

THE advantages claimed for this simple apparatus over others meeting the same need, are: that it is capable of giving firmer support than any which I have seen, among which I would refer especially to the arrangement devised by our colleague, Dr. Van Bibber, of Baltimore, and that it is less likely to slip on the arm, or to get out of repair.

The apparatus is not in principle original, not differing essentially from that used by certain orthopædic surgeons for the attachment of the "rubber muscle." It is made and applied as follows: A piece of strong plaster (rubber plaster is the best) is cut about one and one-half inch wide, and of the length of the forearm. At one of its ends, which we may call the lower end, there are sewed on two narrower strips of plaster, about two feet in length and diverging widely from each other at their point of attachment, and likewise a strip of strong inelastic webbing, which should project some four inches beyond the end of the central strip of plaster, in the direction of its long axis. The plaster is then laid upon the forearm, the broad central strip running from about two and one-half inches above the root of the metacarpal bone of the index finger towards the external condyle of the humerus, the long narrow strips encircling the arm spirally, and the webbing tag extending down over the back of the hand. The plasters are then bandaged into place with a bandage which should be long enough to admit of the use of a few last turns to confine the next part of the apparatus in its proper place.*

This part is made and applied as follows: A piece of tin is

* The turns of the bandage should be securely sewed, or, it might perhaps be a good plan to impregnate them with water-glass.

cut to about the shape and size of the central plaster, and curved somewhat longitudinally, to give it stiffness, and there are then attached to it two buckles, one at each end, both with their tongues pointing downward in the same direction. This gutter of tin, as it might be called, is adapted to the arm, a slightly spiral twist being given it by the fingers, so that it shall follow the course of the central strip of plaster, and is then confined near its upper end by the remainder of the bandage. The webbing is then brought over the end of this tin splint and buckled on, being drawn up sufficiently tight to clear the tin from the wrist.

A strip of strong elastic webbing, doubled if necessary, is then carried from the buckle at the upper end of the tin, passing under a guide of wire at the lower end, to a similar buckle sewed securely on to the back of a strong leather glove near the root of the index finger, and the apparatus is complete, except for finishing touches, which naturally suggest themselves, in the direction of strength and neatness. It might perhaps be well to carry inelastic stays from the lower end of the tin splint to the sides of the hand, to limit lateral motion, but this I have not tried.

The apparatus is cheap and can be manufactured and applied by any intelligent patient. It would seem to be hot and cumbrous, and it certainly does not take entirely the place of living muscles, but an artisan who is enabled by it (as was the case with one of mine), to carry on, even though imperfectly, his trade, is not likely to complain seriously of these inconveniences.

American Neurological Association.

FOURTH ANNUAL (ADJOURNED) SESSION.

(*Officially Reported by Dr. M. Josiah Roberts.*)

FIRST DAY'S PROCEEDINGS: Report of the Hammond Prize Committee.—Amendments to the Constitution.—Time of the Association's annual session.—New by-laws.—The Jewell and Hammond prizes for 1879.—Business before the Association.—Dr. Spitzka on the lower olivary body in man, the anthropoid apes, and other mammals.—Dr. Putnam's case of probable hemorrhage into the cervical enlargement.—Case presented by Dr. Spitzka.—Galvanometer, with remarks upon, by Dr. Putnam.—Dr. Webber's suggestions as to the nomenclature of the affections of the spinal cord.—Dr. Rockwell's remarks based on a case of loss of vision and recovery in brain disease.—Dr. Seguin on cervical paraplegia.—Dr. Beard's exhibit of apparatus for the relief of writer's cramp.—Dr. Putnam's apparatus for wrist-drop.

SECOND DAY'S PROCEEDINGS: Report of nominating committee.—The Association's publications.—Dr. Emerson's case of probable aneurismal tumors of the brain, and discussion.—Dr. Spitzka's demonstration of the brain of the menobranchus.—Dr. Webber on syphilitic disease of the cord, case, specimens and discussion.—Dr. Beard on how to experiment upon living human beings.—Dr. Seguin's case of, and remarks upon, amyotrophic lateral sclerosis.—Dr. Gray on cerebral thermometry, and discussion upon the same.—Dr. Kinnicut's case of generalized athetosis.—Dr. Gibney, history of a case of cervical paraplegia, discussion.

THIRD DAY'S PROCEEDINGS: Business.—Dr. Putnam on intra-ocular circulation, discussion.—Dr Beard, cases and remarks.—Dr. Schmidt on the structure and function of the ganglionic bodies of the cerebro-spinal axis.—Dr. Spitzka on the signification of negative pathological evidence in insanity.

WEDNESDAY, JUNE 19TH.—AFTERNOON SESSION.

The American Neurological Association convened at the College of Physicians and Surgeons, in New York city, June 19th, 1878, and was called to order at 2:30 p. m.

Present at the session: Drs. Beard, Dalton, Edes, Emerson, Gibney, Gray, Hamilton, Hammond, Kinnicut, McBride, Mason, Putnam, Rockwell, Seguin, Spitzka, Van Bibber, and Webber.

In the absence of the President, Dr. J. S. Jewell, of Chicago, and the first Vice-President, Dr. F. T. Miles, of Baltimore, Dr. S. G. Webber, of Boston, became the president for this session.

As the minutes of the preceding session had been printed and distributed among the members of the Association, on motion, their reading was dispensed with.

REPORTS.

Dr. E. C. Seguin, of New York, in the absence of the chairman of the Hammond Prize Committee, reported that

two essays were offered for the prize, on the anatomical and physiological effects of strychnia on the brain, spinal cord, and the nerves. The prize of $250 was awarded to the essay signed, "Nux vomica," and upon opening the sealed envelope which accompanied the essay, bearing the same signature, the author was found to be Dr. E. C. Spitzka, of New York.

The Secretary reported that the following amendments had been adopted: First, As proposed by Dr. Kinnicut, that the officers of the Association shall be a president, a vice-president, a secretary, who shall also perform the duties of treasurer, and two counselors. Second, As proposed by Dr. Emerson, to open the annual session of the Association on the third Wednesday in June. The vote for this amendment was as follows: Twelve for the third Wednesday, ten for the second Wednesday in June, and one for September.

The Secretary also reminded the Association that two new by-laws had been adopted at the preceding session, as follows:

ART. 7. The Council shall be empowered to take cognizance of violations of the constitution and by-laws by members; of persistent failure to pay annual dues; and of willful neglect to attend the meetings of the Association; and shall recommend to the Association proper action in the premises, such as sending a final notice, or dropping from the roll, as the Association may determine by a two-thirds vote of the members present.

ART. 8. That any member of the Association who shall be absent from any two consecutive annual sessions of the Association shall thereby cease to be a member thereof, but may be reinstated by a unanimous vote of the Council.

Dr. Seguin stated that he had been authorized by Dr. J. S. Jewell to offer a prize of $200 for the best original researches into the physiology of the nerves of the uterus and ovaries for next year.

Dr. William A. Hammond, of New York, stated that he would renew his prize of $250 for the ensuing year for the best essay upon a subject which he would hereafter mention.

Upon motion, the resignation of Dr. Walter Hay, of Dubuque, Iowa, was accepted.

THE COMMITTEE ON NOMINATIONS,

as appointed by President Webber, was as follows: Drs. Edes, of Boston; Shaw, of Brooklyn; Mason, Gibney, and Beard, of New York.

Dr. Hammond proposed the name of Dr. Frederick Bateman, of Norwich, England, as honorary member.

Dr. E. C. Seguin, Recording Secretary and Treasurer of the Association, made his report, which was accepted.

Upon motion, the President appointed Drs. McBride and Putnam as auditing committee to examine the Treasurer's accounts.

A motion was carried authorizing the Secretary to arrange for the reading of papers.

There being no further miscellaneous business before the Association, Dr. E. C. Spitzka proceeded to read his paper on

THE LOWER OLIVARY BODY IN MAN, THE ANTHROPOID APES, AND OTHER MAMMALS.

The paper substantially states that the first trace of this body found in the animal series was the homologue of the internal accessory olive of man, and, as such, reached a high degree of development in the parrot. It was continuous below with a part of the anterior grey cornu of the spinal cord, and this continuity was well observed in successive transverse, as well as in longitudinal sections of the human olive. In the placental carnivora this internal accessory olive sent out a v-shaped fold which, in its turn, was everted externally. The v-shaped fold was the homologue of the dentated olivary nucleus in man, while the everted portion constituted the rudiment of the external accessory olive. On tracing the development of this body through the bear, seal, catarrhine and platyrrhine monkeys, the complete homology of these parts could be demonstrated, and Duval was consequently wrong in stating that the carnivora only possessed the internal accessory olive.

The higher we pass in the animal scale, the more the v-shaped portion becomes convoluted, while the internal accessory olive becomes smaller. The olivary body of the chimpanzee is immensely superior to that of all other animals, and

the differences between it and that of man are merely quantitative, and disappear when compared with the immense difference existing between the olive of the chimpanzee and of the baboon.

A slender bar of gelatinous nervous tissue is here seen to clearly connect the dentated nucleus with the internal accessory olive, and in some individuals of the human species this is equally clearly seen. There is considerable individual variation in the development of the human olive. In some the dentated nucleus is hardly more convoluted than in the chimpanzee; in others it is an extremely intricate body. Some interesting variations were observed in imbecile and insane subjects, particularly those suffering from hereditary or congenital insanities. Objections were made to Stilling's nomenclature, as adopted by Henle, but Meynert's views, except in so far as they referred to the seal, were indorsed.

Dr. Spitzka presented, in connection with the reading of his paper, a series of sections of olivary bodies arranged in order. Diagrams were exhibited showing the great similarity of the olivary bodies of the seal and bear.

REMARKS UPON DR. SPITZKA'S PAPER.

Dr. Putnam.—I would like to ask if there be any recent investigations which militate against or confirm Meynert's views relating to the connection of the olivary bodies with the posterior columns?

Dr. Spitzka.—There are none opposed to this view, but, on the contrary, every new discovery only confirms the opinion adopted from Deiters, by Meynert, that the restiform columns decussated through the olives, and the pathological experience that atrophy of one cerebellar hemisphere was apt to be associated with atrophy of the opposite olive, which is in perfect accord.

Dr. Webber.—I was in hopes to have finished the examination of a case in which there was atrophy of both lobes of the cerebellum, together with destruction of the transverse fibres of the pons, and to have presented the results to the Association, but have been unable to do so.

The second paper of the afternoon was by Dr. J. J. Putnam, of Boston.

A CASE OF PROBABLE HEMORRHAGE INTO THE CERVICAL ENLARGEMENT OF THE SPINAL CORD.

The patient, a female, more than thirty years of age, previously healthy, without constitutional disease, was seized one morning, after she had been menstruating a day or so, with a feeling of tightness about the chest which caused her to get quickly out of bed. She however immediately returned to her bed and was thereupon seized with violent vomiting and stertorous breathing, which lasted for some moments, followed by complete paralysis. Her temperature was elevated, pulse excessively rapid, pain in the back and shoulders, and paralysis essentially complete. This was the condition of the patient, when seen in consultation, two weeks after the attack. Two or three points were worthy of notice. She could move her arms and shoulders with comparative ease, but there was no motion of any account of the fingers or wrists of either side. Sensation was so affected that the application of a powerful electric current failed to excite pain. Dr. Putnam saw the patient some two months later. She had menstruated twice, and each time had attacks of vomiting as before. She could now both flex and extend the carpus and the hand, though motion in the fingers was entirely abolished.

Dr. Putnam thought it probable that the posterior white columns were intact, and there was little doubt but that the hemorrhage was in the cervical region of the spinal cord, its upper limit being the fourth or fifth cervical nerve. It was also probable that a post-mortem examination would not reveal the exact pathological condition, and that the distribution of the paralysis was not to be explained upon anatomical grounds alone. The mode of occurrence of hemorrhage at the cessation of the menses, had as yet not been made clear, and its explanation was not simplified, when we take into consideration the fact, that very large quantities of blood can be forced into the circulation without raising the pressure of the blood in the carotids, so long as the vasomotor centre remains intact.

REMARKS ON DR. PUTNAM'S PAPER.

Dr. Beard.—I was interested in the Doctor's conclusions, because in the past few years I have observed, in certain con-

ditions of the spinal cord where there was no inflammation, such as cases of spinal irritation, numbness of the ulnar nerve. Then, again, all of the fingers would get numb. Now, why is it that this nerve is affected to the exclusion of other nerves? I have not been able to answer, except there is a peculiar susceptibility of this nerve. I had a case last winter in whom exercise would produce this feeling.

Dr. Putnam.—I entirely concur with Dr. Beard, for I have observed quite a number of these cases of numbness in persons not subject to spinal irritation. In one case it occurred in a strong, healthy and well nourished man, also in several other individuals in good health, and again in some who were in bad health. In the cases that I have observed, the two middle fingers were affected, and occasionally the whole arm was involved, the attacks coming on after dark, or very early in the morning.

Dr. Beard.—Did you always examine the spine with great care?

Dr. Putnam.—I could not say positively that I did in the cases referred to, though I am in the habit of so doing.

Dr. Beard.—In my experience, spinal irritation occurs in four cases out of five.

Dr. Seguin.—As regards the pathology of the case described by Dr. Putnam, I am very glad that he used the word *probable*, for I am rather indisposed to admit, from suddenness of development, that there was hemorrhage, and I base this opinion upon what I think are facts. First, in infantile paralysis we have a sudden development of paralysis, and in twenty-eight cases there was only one case in which hemorrhage was found, and that was without a history. In a case of my own, observed when I was in the army in 1864, a man was brought into a hospital in this city, who had fallen down with complete paralysis below the neck. My diagnosis was hemorrhage into the spinal cord. Post-mortem examination showed error of judging from suddenness of palsy: the lesion was red softening, a condition which I believe is excessively rare. The specimen was exhibited in this lecture room by Prof. Clark. Besides, in hemorrhage into the spinal cord, we should have complete anæsthesia.

Dr. Putnam.—In regard to the feet, sensation was very much impaired.

Dr. Seguin.—Yet anæsthesia was not complete in your case.

Dr. Putnam.—I suppose that is a thing which is variable: a slight impulse is sufficient to give rise to impressions.

Dr. Seguin.—I admit that a small quantity of grey matter is sufficient to transmit sensation.

Dr. Putnam.—I would not hold with any tenacity to the pathology of the case.

There being no further discussion, Dr. Geo. M. Beard, of New York, proceeded to make some remarks upon

THE CAUSE OF THE TRANSMISSION OF ELECTRIC INFLUENCE ACROSS THE MIDDLE LINE OF THE BODY.

In Vol. II. of our transactions there was a short article by Dr. S. Weir Mitchell, of Philadelphia, entitled, "On the Transmission of Electric Influence Across the Median Line of the Body." The claim of Dr. Mitchell was, that when the electrodes were applied on the healthy side, the muscles on the diseased side would contract, the electric influence going across. The observation is correct; the only thing of interest is its causation and physical explanation. Dr. Mitchell thought it was a new fact in electricity, and so states in his paper. He referred to Prof. George F. Barker, Professor of Physics in University of Pennsylvania, who came to the conclusion that it was caused by induction; a mild induced current causing the contraction. It was very clear that it did not come from reflex action, because if the nerve trunk on the healthy side was excited by a strong galvanic current, no contraction was produced on the diseased side. The error was in supposing that there was induction. Prof. Barker's experiment consisted in taking a shallow glass dish, eight inches in diameter, into which was poured some salt water. On one side of the dish he placed the ends of two platinum wires, some inches apart, and connected them to a Thomson's galvanometer. On the opposite side of the dish he placed two copper wires, about six inches apart, these being connected with a single cell of the battery. When the battery wire was introduced, there would be a marked deflection of the galvanometer needle; when re-

moved, there would still be deflection, but in the opposite direction. These observations are correct; I have many times made these observations during the past three years. Now, the question arises, what is it that causes a deflection of the galvanometer needle? Prof. Barker supposed, that because the galvanometer needle moved when the current was closed as the battery wire was placed in the dish, therefore it was an induced current. It is not an induced, but a diffused galvanic current. How far this diffusion extends, depends upon the nature of the conducting fluid. As I concluded from experiments made some years ago, this diffusion extends to great distances, two feet or more, so as to affect a galvanoscopic frog, or Thomson galvanometer. I made experiments illustrating the diffusion of electricity at the time that I was repeating the experiments of Hitzig and Ferrier on the brain, and also when experimenting with Edison on his alleged new force.

Now comes the point where Prof. Barker made his mistake: when you interrupt a current, you cause the diffusion to be more extended than when the current is continuous. And this is in brief the explanation of what has been observed. The observation of Dr. Mitchell is correct, but the explanation which he and Prof. Barker give, is incorrect. It is the ordinary diffused *galvanic*, and *not* any induced current that causes the contraction across the median line of the body, and the interest of the observation that it illustrates how very feeble galvanic currents may cause a paralyzed muscle to contract.

REMARKS UPON DR. BEARD'S PAPER.

Dr. Mason.—I did not read Dr. Mitchell's paper, but if I get, from Dr. Beard's remarks, a correct statement of the facts, I would say that Dr. Beard's explanation is perfectly satisfactory, and I am very much surprised that there could have been any doubt upon the subject, because it is so exceedingly elementary.

Dr. Seguin.—I had observed the reaction previous to the publishing of Dr. Mitchell's paper, and had not thought much of it, except that it was due to the transmission of the galvanic current which was being used at the time, and had not considered the matter worthy of investigation.

Dr. Seguin.—I have a case of double facial paralysis of

Bell's form, which developed within the course of two days, and though the patient had been paralyzed for ten days, on Saturday last there was an exceedingly well marked increase of excitability to both currents, in the nerve and muscles.

Dr. Putnam to Dr. Seguin.—Is there anything peculiar in the character of the reaction, in your case?

Dr. Seguin.—Nothing, sir.

There being no further remarks upon Dr. Beard's paper, Dr. Spitzka exhibited an interesting case, the physical signs of which were obscure. There was clonic spasm, involving every muscle supplied by the spinal accessory nerve. There had been considerable doubt as to the condition of the larynx. Upon making a careful examination, the left adductor cord was found to be paralyzed. There was a peculiar tone to his voice. The patient had been sick for six years. The longer he stands the worse the spasm becomes.

Dr. J. J. Putnam called the attention of the Association to the galvanometer of Dr. Mann, patented by Sprague, of Manchester. The peculiarity of this instrument was, that the divisions of the scale corresponded to units of quantity of electricity, instead of being merely arbitrary markings, as is usual. He spoke of the manifest advantage in the direction of accuracy by expressing the strength of current used in experiments and in therapeutics, in terms of the deviation of such a galvanometer as this, rather than by the number of cells, etc.

Dr. Seguin made an objection to the instrument, because there was no means of arresting the needle, and called attention to the fact, that after taking one observation with such an instrument, considerable time would necessarily elapse before a second observation could be made.

He thought that all scientific observers should use galvanometers of the same kind, and thus much confusion would be avoided. To bring about this, manufacturers should be induced to supply the trade with galvanometers whose units of registry were the same.

Dr. Putnam remarked that any galvanometer could have attached to it a calculated scale with divisions indicating units of electricity.

FIRST DAY—EVENING SESSION.

The Association was called to order at 9 p. m. by President Webber.

REPORT OF THE COUNCIL.

Dr. Seguin reported that the Council recommended that nominations of associate members be postponed.

The Council recommended for active membership Dr. L. C. Gray, of Brooklyn, N. Y., whose paper had been examined and approved. A vote by ballot resulted in his unanimous election.

The first paper of the evening was read by Dr. S. G. Webber, of Boston, and entitled

A SUGGESTION AS TO THE NOMENCLATURE OF INFLAMMATORY AFFECTIONS OF THE SPINAL CORD.

The names now in use were, as a rule, not expressive of the pathological changes which cause the symptoms. Fox, Leyden, Lange, and Erb recognize only interstitial myelitis. In so-called sclerosis the connective tissue is primarily and chiefly affected, the nerve fibres secondarily. Owing to the different arrangement of the connective tissue in different parts of the cord, the pathological changes do not advance the same, and are not distributed alike in all regions of the cord. So-called locomotor ataxia is considered by Charcot a parenchymatous inflammation. The nerve fibres are primarily affected, and the interstitial tissue secondarily; it is a chronic parenchymatous myelitis. In acute myelitis, generally all tissues are so seriously affected that there is no possibility of drawing a distinction; yet cases of somewhat protracted duration are found where a distinction may be seen after death, at least. Two cases of acute interstitial myelitis previously reported were referred to, wherein the nerve fibres around the grey matter were not affected; but a short distance from the grey matter they underwent degeneration. The interstitial tissue had undergone little or no change.

The next paper was read by Dr. A. D. Rockwell, of New York, the title of which was

REMARKS BASED ON A CASE OF TOTAL BLINDNESS, WITH SUDDEN RECOVERY OF SIGHT, FOLLOWING BRAIN DISEASE.

The infrequency with which we meet with serious impairment of the function of nerves of special sense in cases of hemiplegia, following brain lesion, rendered the subject perhaps of secondary importance, but as every deviation from the common symptoms of any disease, materially aided in its comprehension, the Doctor proposed to glance very briefly at what he considered the more interesting features connected with this topic. His remarks were suggested by a single case in which sight was suddenly lost and as suddenly regained. The fact that complete unilateral paralysis never follows cerebral lesion, has been thought to disprove the suggestion generally accepted, that the will in calling into activity the muscles of the opposite side, acts through the corpus striatum; but the hypothesis of the connection of spinal nuclei by commissural fibres that has been advanced to harmonize this suggestion with the fact that bilaterally acting muscles are exempt from paralysis following lesions in and around the corpus striatum, is ingenious and plausible.

When we come to the consideration of sensory phenomena, equally interesting facts are observed. As a matter of fact we know that not only do ordinary lesions of one hemisphere fail to impair sight, but that almost complete destruction of one hemisphere may leave these functions intact, through the power of the opposite hemisphere to take cognizance of visual impressions. There are probably no exceptions to this exemption in damage to the hemispheres, as regards hearing, since the nuclei of the two auditory nerves lie near the junction of the pons with the medulla.

The determining causes of hemiplegia, which at the same time may occasion defects or complete loss of sight, are, first, where the superior peduncles which are closely related to the visual centres, suffer damage either by hemorrhage or occlusion of vessels. Second, when with a thrombosis in the carotid and middle cerebral arteries of one side, the obstruction is prolonged into the ophthalmic artery. Third, when one of the posterior cerebral arteries is obstructed by a thrombosis. Fourth, the optic tract may be compressed by hemorrhage

into the substance of the crus cerebri or lower part of the sphenoidal lobe.

The case which the Doctor reported was not recent, but came under his notice in the autumn of 1866. The patient, a middle aged gentleman, had some years previous suffered from a convulsive seizure which was supposed to be epileptic in character, and this at long intervals had been repeated. Two months before he came under Dr. Rockwell's notice he had an epileptiform attack resulting in incomplete paralysis of one side of the body and total blindness. For two months after the attack, while the arm had almost wholly regained its normal power, the strength of the leg was still impaired to a considerable degree.

A powerful induction current was applied simultaneously to both eyes of the patient for about two minutes, resulting in an immediate and permanent restoration of sight. Judging from the reports of other cases and the probability of the purely functional or indirect cause of the blindness, recovery would probably have taken place spontaneously, and was only hastened by the reflex stimulation of the electricity.

It is notorious that the most profound derangements of the functional activity of the nerve centres may exist, and yet by none of our advanced methods of investigation has it been possible to discover any appreciable lesion; and until we have more accurate knowledge in this direction, the theory of reflex influence upon which we must fall back for explanation, although beyond the sphere of absolute verification, is at all events plausible, and to a certain extent, rational.

REMARKS UPON DR. ROCKWELL'S PAPER.

Dr. Putnam.—It is certain that loss of sensibility may be restored, although there is an organic lesion which is undoubtedly the cause of it, and as blindness is tolerably easily obtained in animals by injury to the hemispheres, it would seem to me not out of the question, that it was a lesion in the brain.

Dr. Webber.—Would not the complete recovery which took place exclude embolism in the retina of the eye?

Dr. Putnam.—Was hemiplegia permanent?

Dr. Rockwell.—It was so far as I know.

Dr. Spitzka.—I do not believe that any cranial nerve is subject to so much variation as the optic. There are several cases on record where there was no decussation at all. One of these cases is indorsed by the great anatomist Vesalius.

Dr. Seguin.—Was there blindness in both eyes?

Dr. Rockwell.—Yes sir, blindness existed in both eyes.

The next paper was read by Dr. E. C. Seguin, of New York, on

CERVICAL PARAPLEGIA.

Three cases were related, all presenting degrees of atrophic paralysis in both hands and arms, with more or less anæsthesia. In one case there was paralysis of the ciliary sympathetic fibres in one eye, on the same side as the greater wasting. In Case I., there were symptoms indicating progressive disease, such as a broad constriction feeling about the thorax and great weakness in the legs. This case was treated with the satisfactory result of relieving the last named symptoms and limiting the disease in the hands. The patient was seen a few months ago (four years after treatment) and no progress had been made by the affection. Case II., of moderate severity, and probably syphilitic, was entirely cured after several months' treatment. Case III. has been under observation only a few weeks.

REMARKS UPON DR. SEGUIN'S PAPER.

Dr. Putnam.—I would doubt the therapeutic value of the paper in reference to the first case, for I had a case similar to it in which there was great improvement, although there was almost no treatment. There were symptoms of hemiplegia. Recovery was complete, except from the atrophy of the muscles of the arm, in which there was no improvement.

Dr. Seguin.—There was no improvement in the hands in my Case I., but the relief from apparently impending paraplegia was very striking.

Dr. Beard.—Was there any attempt to differentiate the effect of the remedies used?

Dr. Seguin.—Not in the first case, for the records were not very scientifically made.

Dr. Putnam.—The parts which are not very severely af-

fected are likely to recover, and might this not be fairly expected in Dr. Segnin's case, that it would pass away? I would not run the risk of no treatment, though I have great faith in spontaneous recovery.

Dr. Spitzka.—Do you think syphilitic cases will spontaneously get well?

Dr. Segnin.—I should rather doubt it. I should like to hear an expression of opinion as to the frequency of these cases.

Dr. Webber.—I cannot recollect any cases where the paralysis ceased, but I have had cases where the paralysis began and the patient died, but have had no autopsies. One in which the trophic part of the spinal cord was not affected, there was no diminution except in one muscle of the leg, and that was doubtful.

There being no further remarks upon Dr. Segnin's paper, Dr. Geo. M. Beard exhibited a simple apparatus, devised by one of his patients, for the relief of writer's cramp. The gentleman who invented it could not write his own name, but now, after the use of this instrument and electricity, was able to write ten hours per day. The great factor in the production of writer's cramp is the pressure upon the pen-holder, but in this instrument the evil referred to is wholly obviated.

Dr. Putnam showed a new apparatus, resembling "Barwell's Elastic Muscle," for giving support to the hand in cases of wrist-drop.

THURSDAY, JUNE 20TH—AFTERNOON SESSION.

Meeting was called to order by President Webber, at 2.30 p. m.

Dr. Edes read the report of the nominating committee recommending Dr. F. T. Miles, of Baltimore, for President; Dr. S. G. Webber, of Boston, for Vice-President; Dr. E. C. Segnin, as Recording Secretary and Treasurer; Dr. J. J. Mason, and Dr. F. P. Kinnicut, of New York, as Council. Upon motion, the report was accepted.

A check for $250 was received from Dr. Hammond and handed over to the successful competitor for his prize, Dr. Spitzka.

Upon motion of Dr. Shaw, the Council was requested to confer with Dr. Jewell, and determine the conditions upon which the Jewell prize would be awarded.

Dr. Putnam referred to the publication of the papers of the Association, and called attention to several objections to the method now adopted. He urged that some means be determined upon whereby a cheaper publication and a wider circulation of the Association's papers could be insured. After some discussion it was decided, upon motion of Dr. Hammond, to refer the whole matter to the Council, with power to take final action. By permission of the Council, members of the Association can publish their papers where they choose.

Dr. McBride made the report of the auditing committee, which was accepted.

The next business of the Association was the reading of papers, the first being by Dr. N. B. Emerson, of New York.

A CASE OF PROBABLE ANEURISMAL TUMORS OF THE BRAIN, WITH SPECIMENS AND DRAWINGS.

Dr. Emerson presented specimens and drawings of small tumors from the brain of a man who died in the Hoboken Church Hospital. The patient, aged 46 years, seaman, complained of a racking pain located in the forehead and occiput, violent exacerbation at night, with delirious attacks; memory was greatly impaired, so that a history was not obtained. His wife stated, at the time of his death, that he had suffered from convulsive attacks for twelve months. None were observed while the patient was in the hospital until the very last. He several times urinated in bed while asleep. His walk was feeble, but there was no paralysis, and sensibility was unchanged. While in hospital, subcutaneous hemorrhages appeared over face and neck, of irregular shape and size. Mucous membranes were free from hemorrhage and pale, so far as visible. Sight extremely poor, no measurements; ophthalmoscopic examination showed atrophy of the nerve, and retinal hemorrhages.

Death occurred suddenly during a delirious attack, and was immediately preceded by convulsions. Autopsy revealed pachymeningitis across the track of each middle meningeal artery, and chronic inflammation of the meninges, especially at the base of the brain. After hardening in potassium bichromate, the small tumors above mentioned were found more frequently in the cortical portion of the cerebrum and attached to the meninges at the base of the brain. In order to discover the locality of these little bodies, Dr. Emerson probed the brain in every direction with a fine sharp-pointed needle. The moment one of them was touched, its presence was indicated by the resistance offered. By this method, which he demonstrated before the Association on the brain in question, it is possible to discover a deep-seated tumor of hard consistency without in the least damaging the brain itself. The diameter of the tumors varied from 2.5 to 4.5 mm., and were enveloped in a capsule of proliferating connective tissue. Sections of tumors, stained and mounted, showed centrally, amorphous matter and fat granules; next, a zone of granular matter with traces of fibrils, also leucocytes; next, a zone of proliferating tissue with large numbers of nuclei or leucocytes in groups at the periphery.

Dr. Emerson was at first inclined to look upon these tumors as gummata, but at length came to look upon them as old healed aneurisms.

Dr. Emerson expressed his obligations to Dr. Wm. R. Fisher, of Hoboken, and Dr. O. D. Carpenter, of Jersey City, to whom he was indebted for the privilege of using the case.

REMARKS UPON DR. EMERSON'S PAPER.

Dr. Spitzka.—About a year and a half ago, I examined some gummatous tumors, not displacing the brain substance, but replacing it. While most of them looked similar to the tumors we have just seen, there were some that extended through the two first layers of the cortex beyond the blood-vessels, which I preferred to consider as miliary myxoma of a syphilitic nature. There was one of these near the origin of the third nerve.

Dr. Seguin.—I would state that I am disposed to look upon

the tumors which Dr. Emerson has presented, as aneurismal, for the following reasons: The central portion of the tumors looks like thrombus undergoing fatty degeneration, the capsule resembles changed and thickened arteries which I have in my possession. I fail to see in these specimens any of the young cell formation which is so characteristic of gummata; also the absence of continuity between the tumors and the normal brain tissue; their size is, however, rather greater than that of ordinary miliary aneurisms.

In this connection, perhaps, I may be permitted to show some miliary aneurisms prepared by Dr. Amidon, of the New York Hospital. They show very beautifully the peri-arterial changes, and the fatty degeneration of the muscular coat. Dr. Amidon has devised a method of obtaining these aneurisms, which is simpler and much more rapid than Charcot's; he disintegrates the nervous tissue by a stream of hot water.

Dr. Spitzka.—As no gummata are known to occur centrally, I have always thought they proceeded from the meninges. I can exclude aneurism in my case, for I have found all the evidences of syphilis.

Dr. Emerson.—In looking at one of these specimens I found along its edge what appeared to be little extravasations, but I did not find any in the brain. The heart, in this case, was opened, but I could not say that it was examined.

Dr. Edes.—I have been struck with the resemblance of this drawing to some specimens of endarteritis which I have, without the same contents. The contents would, of course, be referred to the clot. I must say that I am in favor of aneurism, for it has been shown that endarteritis, occurring in a syphilitic patient, is not necessarily syphilitic, because other things give rise to it.

Dr. Spitzka.—In the case referred to by Dr. Edes, was there disease of the adventitia?

Dr. Edes.—I could not say as to that, positively.

Dr. Edes.—I have at home a very pretty specimen of aneurism, taken from the pons, which in some respects resembles the tumor here upon the slide, and also resembles somewhat that which is mounted in wax. The brain from which I took the specimen contained several. The parts around the aneur-

ism were stained so as to be easily recognized by the naked eye alone, the thick walls of the vessel could be seen, and at the dilatation, where the blood had broken through, the thinness of the walls of the perivascular sheath of the adventitia was easily recognized. The size of the tumor was as large as those which Dr. Emerson has presented, or larger.

DEMONSTRATION OF THE BRAIN AND CORD OF MENOBRANCHUS.

Dr. E. C. Spitzka exhibited specimens of the spinal cord, medulla oblongata, and hemispheres of the menobranchus, which showed the immense size of the nerve-cell nuclei, and the peculiar arrangement of the fibrils constituting the axis cylinder, and other processes around the latter, giving the appearance as if a nucleus had been fortuitously entrapped in their meshes. The gradual transition of the epithelium of the central canal to nerve cells was clear and distinct. Of the distal nerve cells, one had a sharply-marked axis cylinder process extending to the periphery of the cord at the point of egress of the anterior roots. The brain cortex showed an embryonic structure, and the relations of the epithelium of the choroid plexus and tela choroidea were unusually distinct and interesting.

REMARKS UPON DR. SPITZKA'S PAPER.

Dr. Edes to Dr. Spitzka.—Have you been able to trace the processes from one cell to another cell?

Dr. Spitzka.—In one case, in the human spinal cord, I think I have unquestionably traced an anastomosis. Also in the sea-turtle I think I have an appearance of the same kind.

Dr. Seguin.—While studying in Bonn I found what I supposed to be an anastomosis, but upon looking at it with a one-fifteenth inch immersion lens I found the appearance due to superposition of the processes.

Dr. Putnam.—What is the character of the nerve tissue cells?

Dr. Spitzka.—From their position I consider the thick congregation of cells as the germ cells of higher animals, and the scattered cells as connective tissue cells. As everything in the menobranchus is embryonic, you cannot say which is which.

Dr. Seguin.—I would like to ask Dr. Mason if, in his researches upon the spinal cord of the alligator, he has found anything peculiar?

Dr. Mason.—As yet I have not had an opportunity of studying the sections which I have prepared.

Dr. Seguin called attention to the history of an anomalous case of sclerosis. The patient had as symptoms persistent numbness in hands, forearms, and legs. No other symptoms except on two occasions, an attack of fulminating pain. Upon post-mortem examination he found sclerosis in the posterior columns, chiefly in the columns of Goll, but also involving a part of the radicular zones. Sections from different points in the spinal cord were exhibited.

The next paper was read by Dr. S. G. Webber on

A CASE OF PROBABLE SYPHILITIC DISEASE OF THE SPINAL CORD, WITH SPECIMENS.

The case was interesting on account of the rare pathological changes. The cord was sent to Dr. Webber from the country, and there was no notice of changes elsewhere. Patient had syphilitic history, but no secondary symptoms; no venereal excess; no injury to back or head; lived in good quarters; four or five weeks before death he experienced numbness in the side of his nose, face, and left thumb; about the same time had pains in back, and both arms and legs, which became so severe that in two weeks he was obliged to take to his bed; had retention of urine, constipation of the bowels, no disturbance of eye-sight, but ptosis on the right side; eyes, pupils, and facial muscles readily responded to the will; motion of arms, hands, and legs possible; sensation in legs and hands rather diminished; no prickling sensation.

The greatest change in the cord was found just below the cervical enlargement. The membranes opposite the lateral columns were thickened and adherent; central grey substance was deformed; centre of the cord was occupied by a mass of diseased tissue, in which were seen numerous vessels with extravasated blood pigment and blood corpuscles, many nuclei and much fibrous tissue; also many enlarged spider cells, which seemed to lie near the root of the anterior cornua. Below this a section showed the most changes in the right lateral col-

nmus, the morbid changes being similar, except an increase in the number of spider cells.

Above first section in cervical enlargement the chief changes were, increased number of enlarged spider cells in the anterior cornua, and degeneration of the posterior columns. Posterior columns were also affected in the dorsal region, below the part most diseased. Hence, changes in the posterior columns may have been an independent lesion. The change consisted of an enlargement and increase of the spider cells; the neuroglia fibres were increased, interlaced and wavy; nuclei were increased, the nerve fibres fewer, and the walls of vessels thickened.

The case is of interest for the enormous increase of spider cells. The development of new-formed tissue was at the expense of the nervous fibres and cells, which seemed to have been pushed aside or supplanted. The new-formed tissues may be considered to form a tumor composed of the normal interstitial tissue; that is, it is a variety of glioma.

Dr. Webber exhibited several sections of the cord of the above case.

REMARKS UPON DR. WEBBER'S CASE.

Dr. Seguin.—I would like to ask if Dr. Webber has any explanation of the face symptoms in his case?

Dr. Webber.—The specimen was sent to me, and there was no history of a lesion elsewhere.

Dr. Seguin.—The question of spider cells is one which is attracting considerable attention at the present time. Dr. Shaw and I have made some observations in this direction. He found them in making examination of specimens of central myelitis and disseminated sclerosis, and I have demonstrated them in the lateral columns in amyotrophic lateral sclerosis and in sclerosed posterior columns. The question in Dr. Shaw's mind and my own, is, how far can we look upon these cells as evidence of abnormal cellular nutrition in contradistinction to nervous degeneration?

Dr. Webber.—In the drawing from the case of sclerosis, one of the spider cells is seen connecting with the artery or at least with a blood-vessel. I have found that quite a common

connection, the processes of spider cells running into the walls of the artery and connecting with a sort of infundibulum. In one case I saw one of the processes of a spider cell take a circuitous course to the artery, while another took a direct course to the same artery.

Dr. Seguin.—I should like, Mr. President, your views as to whether spider cells are to be relied upon as evidences of inflammation.

Dr. Webber.—They are part of the normal constituents of the cord, and can be seen in almost any section, but of course the nerve fibres obscure their demonstration. A spider cell growing in a group of nerve fibres, which are transversely cut across, very soon loses its processes in the septa separating the nerve fibres one from the other, and it is not always easy to make out the cells from which these processes arise, these cells having nuclei and sometimes nucleoli, and sometimes I have seen two or three little fibres radiating from a nucleus. Being the normal constituents of the cord, like all other constituents of the neuroglia, when an inflammation is set up these spider cells enlarge, and perhaps imbibe carmine more readily, so that the processes given off by them are more readily seen. Sometimes these spider cells and their processes become very strongly tinted with carmine. After the inflammatory process has gone on for a length of time and has become quite chronic, a change takes place in these spider cells, and instead of being more deeply tinted by carmine, they become less strongly stained and it is sometimes difficult to see them. They then become reduced many times, merely to a nucleus that is pretty large. Around this nucleus, very early, can be seen the outline of the cell, which, having taken no carmine whatever, is translucent and without color. This I have looked upon as the stage of degeneration or atrophy of these spider cells. The interstitial tissue becomes condensed and solidified in the last stage of the disease. In the drawing which I presented, I think there was one of these spider cells which had passed through this degenerative process.

I have examined for nuclei very carefully in cases of so-called sclerosis, and have found that it was possible, in a very large majority of cases, to trace the outline of the spider cell.

So I think it is fair to conclude, that the nuclei of these cells are merely the nuclei of the spider cells, and that, owing to non-coloration by carmine or other causes, we cannot trace the outline of the spider cells. Taking this view of it, the above described changes are merely changes in the normal structures of the cord, and may be considered inflammatory.

Dr. Seguin.—Dr. Webber's views are in accord with my own, but I wish to call attention to the cloudy appearance of these cells when they become enormously enlarged and the number of processes seem to be larger than normal.

Dr. Webber.—This appearance can be seen in one of the specimens now under the microscope. The cell is in rather a thick portion of the cord. I would add, that it is my opinion, that these cells sometimes undergo fatty degeneration and are absorbed.

Dr. Shaw.—I think that evidence of the last statement which Dr. Webber made may be seen in disseminated sclerosis. I think these cells do undergo degeneration. It has appeared to me, that where specimens were kept in alcohol, the cells could not be seen.

Dr. Webber.—I have been unable to satisfactorily observe specimens freshly placed in Canada balsam. It is only after the lapse of a considerable length of time and the specimen has become thoroughly permeated with the mounting fluid, that study of these cells is followed by satisfactory results.

There was another point to which I neglected to call attention. Spider cells seem to undergo a great many changes. In a swollen condition the cells seem to be filled with some material. In such, I have seen groups of from two to six nuclei, as if white blood corpuscles had migrated into the cells, and accumulated there. I found one cell in which there were five or six nuclei grouped together in one end, but it is very common to find cells with two large nuclei, the rest of the cell having the appearance as though it had been filled with a fluid which had become solidified through the agency of the reagents.

Dr. Shaw.—I would like to ask Dr. Webber, if, in those cells where there were a number of nuclei, it was not due to division of the original nucleus? In drawings from my case

you will see a contraction in the nucleus as if it was about to divide.

Dr. Webber.—Yes, it is quite possible that is the case.

Next followed the reading of a paper by Geo. M. Beard, M. D., of New York.

HOW TO EXPERIMENT ON LIVING HUMAN BEINGS.

Mr. John Stuart Mill, in the preface to his work on, "Logic and the Principles of Evidence" observed, that, while we may instinctively avoid errors in the search for truth, yet it is an advantage not to be dispensed with, to have a rational understanding of the philosophy of reasoning, so that we shall not be forced to depend on dumb instinct alone.

In experimenting with living human beings there are six sources of error which physiologists and physicians guard against instinctively, but which ought to be and can be reduced to a positive statement. These elements of error come from the nervous system, their study therefore belongs to neurology. It is because of the backwardness of neurology that this subject has thus far been passed by. Dr. Beard had already called attention to these six sources of error in experimenting upon living human beings, very briefly, in an article on the physiology of mind reading. But in his remarks before the Association he proposed to discuss the subject in greater detail. He found in making experiments upon living human beings for the purpose of studying the physiology of mind reading, and also in making experiments relating to the action of mind on body, that this whole matter was as unexplored as the north pole, and that it was necessary to hew one's way through infinite obstructions at every step. The elements of error in experimenting with animate objects were indicated by Bacon, under the strange headings, "Idols of the tribe, of the den, of the forum, of the theatre," and later writers upon evidence and principles of science have repeated or assumed the Baconian formulas, but the special elements of error in experimenting with *living* human beings, have thus far escaped conscious exhaustive analysis. The six sources of error are as follows:

First Source of Error.—The phenomena of the involuntary life in both the experimenter and the subject experimented on.

Under this head are embraced trance, with all its symptoms and all the interactions of mind and body that are below the plane of volition, or of consciousness, or of both. Without a knowledge of this side of physiology, scientific experimenting with living human beings is impossible.

Second Source of Error.—Unconscious deception on the part of the subject experimented on. This element of error slips into all the ordinary experiments with new remedies and supposed new forces in the animal body, thus corrupting science at its very sources. The neglect of this element of error, even although all the other errors were guarded against, would vitiate the utility of such experiments as those of the committees of the French Academy on clairvoyance and mesmerism. In regard to guarding against these two sources of error, two things are necessary for the experimenter: First, a thorough knowledge of the involuntary life; second, *the subject experimented on must always be deceived.* The three methods of deceiving the subject, by which alone the element of error from mind acting upon body can be eliminated, are these: First, by doing something when the subject experimented on believes we are doing nothing. Second, by doing nothing when the subject believes we are doing something. Third, by doing something different from what the subject believes is being done.

Third Source of Error.—Intentional deception on the part of the subject. The methods of deception already described suffice to guard against all deception, intentional or unintentional.

Fourth Source of Error.—Unintentional collusion of third parties. By third parties is meant witnesses, audience, bystanders, seen or unseen. The best illustration of error from this source, is the aid which audiences give to a performer, by their silence when the mind reader wanders from the object for which he is searching, and by their murmur or applause when he nears or reaches it. This is quite analogous to the cry, "hot!" or "cold!" in the game of "blind man's buff." Dr. Beard, in his experiments in mind reading, found it necessary to send out all witnesses, or insist upon their being perfectly silent or motionless.

Fifth Source of Error.—Intentional collusion of third parties. Under this head come the aid that assistants give in experiments like those of Houdin and Heller.

Sixth Source of Error.—Chance and coincidence. The experiments during the past year of Charcot with metalloscopy and metal therapeutics, have given special interest to this subject. Admitting all the facts which Charcot claims, considered as facts alone, it is clear from his own statement that he did not consider or allow for these six sources of error. He did not prove whether the results obtained by putting metals on the bodies of hysterical patients, were subjective or objective, but in his lecture on metal therapeutics assumed that the question was settled and that the results were objective. His experiments were, therefore, of no value scientifically. If it should be proved hereafter by Charcot himself, or by others, that the application of metals, gold, silver, copper, have a real objective effect with regard to the mind of the patient, it is none the less true that he has not thus far proved it, for results far more remarkable Dr. Beard stated he had obtained in experiments with living human beings, by the mind acting upon the body alone. If it should be proved hereafter that these results are objective, another series of experiments would be needed to determine whether they were the result of pressure or of absorption of feeble currents of electricity generated by the metals.

From the reduction of this subject to a science, two results must follow: First, there will be more precision in the results of our experiments with new remedies and in modes of treatment. Second, men of ordinary ability can succeed in experimenting with living human beings where now men of great genius fail. With these six sources of error before them and fully understood, any medical student could make the experiments of Charcot, and settle for all time the question at issue.

REMARKS UPON DR. BEARD'S PAPER.

Dr. Mason.—It has occurred to me, that to make Dr. Beard's paper strong and precise, the points should be given in which Dr. Charcot has erred. I would like to know if Dr. Charcot has ever claimed curative effect in these metals.

Dr. Kinnicut.—Dr. Charcot expresses himself as a supporter of that view. In his lectures delivered last autumn and published, he is reported to believe in the efficacy of these metals, as shown by cases in his own hospital, but he gives no theory so far as I know, except to claim that it is not due to expectation and attention. If I remember rightly, all of his cases were not hospital cases. One occurred in private practice, and he states that all possible sources of error had been removed.

Dr. Seguin.—I think Dr. Beard's paper is very interesting and a valuable contribution. I think it will help us in our very blind and groping way of testing medicines. I hope to see a reform obtained by bearing in mind these sources of error, yet in private practice it is becoming more and more difficult, because our patients are becoming more and more clamorous to know what medicine is being used, and what is expected of it.

Dr. Putnam.—I saw the experiments of Dr. Charcot performed, and though I was at that time unable to discover any source of error, yet I am strongly inclined to agree with Dr. Beard, and I must say the impression made upon me by Dr. Charcot was, that he was wanting in the necessary qualities to successfully perform experiments of that kind.

The next paper was by Dr. E. C. Seguin, entitled

AMYOTROPHIC LATERAL SCLEROSIS. SPECIMENS.

The patient had presented, in the course of a two years' illness, all the classical symptoms of the disease, viz.: atrophic paralysis of the hands and forearms, contracture of all the limbs, signs of labio-glosso-pharyngeal paralysis. The post-mortem examination showed no lesion visible to the naked eye, except smallness of some anterior rootlets; but the study of numerous microscopic sections of the spinal cord and medulla oblongata showed the following lesions: First, a focus of disease in the cervical enlargement, consisting in atrophy of the ganglion cells of the anterior horns, especially in the lower part of the enlargement, and sclerosis in the postero-lateral column. Second. Sclerosis of the postero-lateral columns throughout the rest of the spinal cord above and below the cer-

vical enlargement. Third. Destruction of nearly all the ganglion cells in the nuclei of the hypoglossal nerves. The duties of Secretary to the Association had prevented the completion of the study of the sections, so that many points required elucidation. Dr. Seguin was, however, inclined to agree with Prof. Flechsig in locating the sclerosis found in this disease in the coarser pyramidal columns, and not, as Charcot expresses it, in the lateral columns.

Dr. Webber.—I would like to ask whether the lesion of the white columns in these cases affected the interstitial or nerve fibres first?

Dr. Seguin.—Probably, the interstitial tissue. I was very desirous, and am glad to have had the significance of enlarged spider cells explained, before I read my paper, for that very reason. I found, and can demonstrate in these specimens, spider cells in the sclerosed columns.

There being no further discussion the Association adjourned to meet again in the evening.

SECOND DAY—EVENING SESSION.

The Association was called to order by President Webber at 9:30 p. m., and there being no business to transact, Dr. Landon Carter Gray, of Brooklyn, proceeded at once to read his paper on

CEREBRAL THERMOMETRY.

The paper contained the results of an investigation of the cerebral temperatures of one hundred and two individuals. The same points for placing the thermometers were adopted as in the researches of M. Paul Broca, which were reported in Aug., 1877, to l'Association Francaise pour l'Avancement des Sciences, but which only embraced twelve persons. To these points Dr. Gray has given the name of stations. They are situated as follows: One on each side somewhat back and above the commencement of the external angular process of the frontal bone, being the frontal stations; one on each side just above the ear, being the parietal stations; and one on

each side of the occiput, being the occipital stations. In addition, Dr. Gray ascertained, by means of certain measurements which are detailed in his paper, the distance of the fissure of Rolando at its vertical termination from the furrow formed by the junction of the nasal bones with the nasal process of the frontal. This distance was, on the average, 6¾ inches. Around the fissure of Rolando are the four vertical stations. Seguin's surface thermometers, made especially with a view to obviating all error from any accidental pressure upon the bulb, were employed; and they were held in position by a head-gear made of ribbon or soft morocco. The conclusions arrived at by Dr. Gray were:

"1. The average temperature of the left frontal station is 94° 36′, the right being 93° 71′.

"2. The average temperature of the left parietal station is 94° 44′, the right being 93° 59′.

"3. The average temperature of the left occipital station is 92° 66′, the right being 91° 94′.

"4. The average temperature of the left side of the head is 93° 83′, the right being 92° 92′.

"5. The average temperature of the whole head, exclusive of the vertex, is 93° 51′.

"6. The average temperature of the motor region of the vertex is 91° 67′.

"7. The average temperature of the whole head, inclusive of the vertex, is 92° 66′.

"8. If there be an alteration of temperature at any of the lateral stations of more than one and a half degrees above or below the average temperature of such station, this fact will justify a *suspicion* of abnormal change at that point.

"9. If there be an alteration of temperature at any of the lateral stations of more than two degrees above or below the average of such station, this fact will constitute *strong evidence* of the existence at this station of abnormal change.

"10. In proportion as the alteration of temperature at any individual station is increased or decreased beyond the figures just mentioned, in exact proportion will the strength of the evidence be increased, until, the maximum or minimum having been passed, the evidence will become almost conclusive.

"11. Should it so happen that such elevation of temperature above the average should be at any lateral stations on the right, causing a rise at this point beyond the average temperature at the corresponding station on the left, this would strengthen the suspicion or the evidence.

"12. These remarks apply with equal force to the average for the whole of either side, as well as to the average for the whole head.

"13. It is necessary to the validity of these conclusions that the contemporaneous bodily temperature should be normal, or that there should be a marked disproportion between it and the cerebral temperatures."

Dr. Gray detailed a case of glioma of the brain, which he had been able to locate during life at the side of the cerebrum, when the symptoms pointed to an affection of the base, and claims that it is the first case on record in which an intra-cranial morbid growth had been localized during life by a thermometer, and the diagnosis verified by a post-mortem. Dr. Gray spoke of other cases, which were, however, not followed by a post-mortem, and the publication of which he therefore reserves. He is now engaged upon the study of the pathological aspect of the question, but his material is not yet ready for publication.

REMARKS UPON DR. GRAY'S PAPER.

Dr. Webber.—This paper seems to be opening up a new line of investigation, especially the pathological part of it, and the results so far have been carefully worked out. Should further trial result as satisfactorily as in the case reported, this means of diagnosis will be of great practical importance, especially in doubtful cases. I had a case a month ago, the symptoms of which caused me to suspect tumor of the brain, just commencing, and I can imagine that a thermometrical observation, such as Dr. Gray has described, might be valuable in settling the diagnosis, should it be proven always to be reliable. It had seemed to me rather curious that the change in temperature should be so localized when the brain is not immediately connected with the skull or the scalp. Has Dr. Gray any explanation of that?

Dr. Gray.—I should think the thermometer would indicate largely the temperature of the cerebrum itself, seeing that it is only covered by the scalp and a thin layer of bone, the bone being thinnest at the three stations selected for observation.

Dr. Shaw.—There is one great source of error in the use of surface thermometers, namely that any pressure upon the bulb will cause the mercury to rise very perceptibly.

Dr. Gray.—I stated in my paper that I had a great deal of trouble from this source, but that it was possible to make a thermometer with a bulb three-eighths of an inch across which would obviate this source of error. A safe way is to shake the mercury down ten or twelve degrees below the normal temperature before taking an observation. This was the method which I adopted in my observations, and by so doing I consider that I eliminated the error from pressure upon the bulb.

Dr. Shaw.—I am of the same opinion as Dr. Webber. I can hardly see how we can have such localized temperatures outside.

Dr. Gray.—We can only arrive at conclusions, by exclusion. The superficial parts do not vary to the extent indicated by the thermometer, the variation then can only be due to the change in the temperature of the parts beneath.

Dr. Seguin.—It seems to me that Dr. Gray has largely obviated the error which has crept into former observations of this kind, by making it a rule *not* to consider the temperature as indicating a pathological condition of the brain, unless it is more than a degree and a half above or below the normal. I regret that Dr. Hammond is not present this evening, otherwise it would be interesting to go into a general discussion of cerebral thermometry. I think Dr. Gray has demonstrated the fact that delicate thermometers are not of great value in cerebral thermometry.

Dr. Gray.—I should have given Dr. Hammond credit for his observation had I known of it before I submitted my paper.

Dr. Beard.—I was not able to judge from the paper whether any observations were made upon the effect of emotional disturbances.

Dr. Gray.—My object has simply been to obtain something which would be of diagnostic value, as is the ophthalmoscope and the laryngoscope.

Dr. A. McL. Hamilton.—It has occurred to me that considerable deviation may arise from a functional condition of the scalp.

Dr. Seguin.—I would suggest the probable usefulness of a series of experiments upon animals, whereby the temperature of the scalp and that of the brain could be determined at the same time, the temperature of the brain being taken through perforations in the skull.

Dr. Webber.—The results thus obtained would be only approximately accurate, for exposure might mutilate the brain and produce an inflammatory condition of the scalp also, or a disturbance in the circulation of the scalp which would interfere.

Dr. Gray.—It seems to me, though it might gratify a curiosity to know what the actual temperature of the brain is, there would be nothing of practical importance gained, for, by making the observations in the way I have suggested, the same conditions are met with in every person, and though these observations are only relative, yet they are sufficiently accurate for diagnostic purposes. It would seem quite certain that morbid changes in the skull would cause a rise in temperature.

Dr. Webber.—The case reported by Dr. Gray is of great value, and it is to be hoped that many other cases will receive like attention. I have noticed in my own case, that, after mental labor the vertex is much hotter than the sides of my head, and almost always it is accompanied with a feeling of discomfort. I have looked upon this as a rise in the temperature of the scalp, but as to its origin, I do not know.

Dr. Kinnicut.—I was about to remark that the same condition exists in other parts of the body after mental labor, as for instance in the mouth a rise in temperature of seven-tenths of a degree, so that perhaps the condition observed in the scalp is only part and parcel of what takes place elsewhere.

Dr. Beard.—I have never been able to answer that question to myself satisfactorily. You get a rise of temperature not only after mental, but after physical exertion, and sometimes

where there is no exertion at all. Sometimes I have thought it was brought about through the nerves. In the hand, and in other parts of the body, where there is no mental disturbance, there are variations in the temperature. When we look at a great orator in the midst of a speech, we notice an enlargement of the veins of the face, and the question arises, what relation has the cerebral circulation with the circulation we see.

Dr. Emerson.—The fact that there are local disturbances in the temperature of the body does not favor the theory which Dr. Gray has advanced.

There being no further discussion, Dr. F. P. Kinnicut, of New York, read the history of a case of

GENERAL DOUBLE ATHETOSIS.

E. D., æt. 14; the patient is of average size for his age. General physical development good. Intelligent countenance and apparently no marked mental impairment. Marked and typical athetosis of hands, arms, cervical and facial muscles. The head is constantly being turned from one side to the other, rather more frequently and to a greater degree to the right, but without any regularity as to direction. The athetoid movements of the facial muscles are *particularly* marked, causing strange grimaces on the patient's attempting to speak. The sterno-mastoid muscles of the two sides are equal in size, both apparently being somewhat hypertrophied. The movements of the fingers, hands and arms are not "jerky," they are vermicular, irregular, semi-tonic and exist to an extreme degree. Hypertrophy of the muscles of the hands is doubtful; there is apparently some of the muscles of the arms.

Inco-ordination of legs exists, but in a slight degree. The speech is affected, being "jerky," slow, apparently attended with much effort and causing a marked increase in the athetoid movements of all the affected parts.

By drawing the patient's attention, the movements are somewhat controlled. There is absolutely no paralysis and no disturbances of sensation.

The characteristics of double or general athetosis as distinguishing it from hemi-athetosis, would seem to be as follows:

1. An affection congenital in nature, or dating from earliest infancy; in other words a *primary* affection.

2. An athetosis bilateral in character and always involving the muscles of the neck and face.

3. The absence of all paralysis and of all sensory disturbances.

4. The existence of a greater or less degree of imbecility in the majority of the cases thus far observed.

REMARKS UPON DR. KINNICUT'S PAPER.

Dr. Hamilton.—Within the past few weeks I have seen, I think, an unique case of hemi-athetosis, the history of which was taken by the House Physician in the Epileptic and Paralytic Hospital. The peculiarity of the case is the entire absence of mental disease, and his age, thirteen years.

Dr. Seguin.—I fail to see why the occurrence of athetosis is not a sign of brain disease. Disease of the cerebellum gives rise to disorders of locomotion without paralysis. Cases of sclerosis occur without any marked paralysis. I saw a man this morning in my office, who for several months has had tension of the muscles of the right leg and arm, and he has a tremor which is not present when he is quiet. In this case Prof. Westphal corroborated my diagnosis of sclerosis of the left hemisphere of the brain. I simply make these remarks in support of the view that choreic movements may result from brain disease.

Dr. Hamilton.—I think the fact remains, that in the majority of the cases of athetosis, they follow some brain disease, or are connected with some undeveloped condition of the brain, such as idiocy.

Dr. Seguin.—Still Dr. Hammond's original case was not preceded by paralysis.

Dr. Kinnicut.—It was preceded by epilepsy. An analysis of twenty-seven cases, recently made, gave only four cases in which there was not paralysis, two of which were Hammond's cases.

Dr. Webber.—I have myself, a short time since, seen a case of athetosis beginning in infancy, at the age of fourteen months, the first symptom of which was a convulsive attack. The

patient told me that from that time until the present (she is now twenty-one years old), there has been a constant motion of the left hand when she is awake, unless interrupted by placing her hand in some position. I looked up this subject and found thirty-three cases, of which about the same number were affected upon the right side as upon the left side. In a large proportion of these cases, where the age was given, the disease began in infancy. In two cases, autopsy showed hemorrhage into the corpus striatum. There was softening around the hemorrhage. The locality of all these lesions was the lenticular nucleus, sometimes towards its posterior end, and the internal capsule was also affected. It is interesting to notice that Pitres has lately discovered white bundles extending through the internal capsule, irritation of which produces the same movements in the limbs as irritation of the cortex. The view which they take of it is, that the nerves of motion pass through the basis cerebri, and in the internal capsule are in bundles which are separate from one another. If this be so, we can see how irritation in one neighborhood may be communicated to other fibres, and thus keep up a slow motion. Charcot has, I believe, reported three autopsies where the lesion was in the optic thalamus. The cases of infantile origin, such as the one I have seen, such as Dr. Jewell reported last year, such as Dr. Seguin has referred to in the transactions of this Association, such as Dr. Berger, of Breslau, reports, in all of these cases there is a lesion of the brain substance, which I suppose is not yet fully demonstrated, but which seems to be somewhat of the nature of sclerosis. I had an opportunity of examining one case after death. It was not a case of athetosis, but one of those infantile cases followed by motor disorder. The part affected was the sphenoidal lobe around the inferior cornu of the lateral ventricle. The examination was not very satisfactory, but the gross appearance was more satisfactory than the microscopical examination. The specimen had been placed in alcohol. I suppose in those cases of infantile origin there is some such change as I have described. I doubt if there is always a hemorrhage. In other cases occurring in infancy, resulting in motor disturbances, I have found a slight tremor, which would connect these together, as a chain

of cases of varying peculiarities which shade into each other, and which enable us to judge that the lesion is very much alike in all cases.

Dr. Kinnicut to Dr. Webber.—Do you regard the localization as more important than the lesion? Why is not that the all-important point?

Dr. Webber.—That is just what I meant to have said. I think there is probably an interstitial change which produces an irritation at that point. In the post-hemiplegic cases which I have seen, which are few, the motions were not at all alike. In none that I have seen, and in the reports which I have read, is there a resemblance to the disease we call chorea. The movements are involuntary, occurring without any effort of the will, and without any object in view. The movements in chorea are, I think, entirely different from those in post-hemiplegic cases, and the name of post-hemiplegic chorea should be discarded.

Dr. Seguin.—In my experience I have thought I have made out considerable distinction between the two conditions. The movements in the athetoid condition in the child are never jerking, but are movements of flexion and extension, almost rhythmically, almost vermicular. In my two cases of post-hemiplegic chorea there was nothing of the kind, the movements were jerking, and there were also certain actions which resembled the movements of ataxia; they were certainly remote from true athetosis. In my own mind I drew a plan of forms of spasm running all the way from chorea, and presenting a great many varieties, including ataxic movements, and looked upon those post-hemiplegic cases as more resembling chorea than any form of athetosis.

Dr. Webber.—I do not doubt that many cases of post-hemiplegic chorea have this character. I mean to say that cases can be found where they shade off into each other, and seem to belong to the same class. At one end of this series there is great motion, and again at the other end the movements run into those of athetosis.

Dr. Seguin.—Prof. Charcot is disposed to make the choreic group include all of this group, taking the same view of chorea as did Trousseau, making it into an immense family with

genera. I would add that there is one radical difference between athetoid movements and chorea: In athetoid movements the fingers chiefly participate, while in chorea we get movements of the whole face, or the whole side of the body.

Dr. Kinnicut.—It would seem that only in this double form are the muscles of the extremities affected. In the bilateral form the muscles of the face have been affected.

There being no further remarks, Dr. V. P. Gibney, of New York, proceeded to read a paper entitled

CERVICAL PARAPLEGIA,

with a request that a diagnosis as to lesion be given by the Association. The following abstract contains the main points in the case.

A female child, aged eight years, was first seen June, 1876, at which time a plaster of Paris jacket and head-support was worn, having been first applied in January of the same year, the supposition being that she had caries of the cervical spine. The disease dated from October, 1874, at which time she had a strain by catching her head under the round of a chair. Nothing marked occurred from that time until she was placed under treatment, save an inability to rotate the head completely. When the plaster of Paris jacket was removed the opisthotonus was marked, and no deformity of the spine could be discovered, though other signs pointed to cervical caries. By Aug. 22d, of the same year, the opisthotonus was entirely relieved, and motion of the neck nearly, if not quite perfect, and the diagnosis of caries was abandoned. A month later there was cervical tenderness without deformity. Mechanical appliances were discontinued. A certain degree of stiffness remained from this time until February, 1878, without other marked signs, the child enjoying good health meantime. In February there was induration over the trapezii, and marked tenderness and pain upon any motion of the head. The above symptoms and signs increased, and the patient soon lost the use of the upper extremities. The deltoids became atrophied, and loss of flesh followed rapidly the loss of power in the lower extremities. Treatment was again begun with ergot and iodide of potassium, a drachm of the former three times daily,

and eight grains of the latter four times daily, which was followed within a few days by rapid improvement, and in a week by apparently perfect recovery. The patient was removed from the hospital March 29th, in this condition. June 20th, 1878, patient was found with absolute loss of power in upper extremities, little or no anæsthesia, considerable power in lower extremities, former degree of torticollis, no pupillary disturbances, sphincters of bladder and rectum normal, no headache, no spinal deformity, but tenderness in the cervical and upper dorsal region; greatly emaciated, and suffering from bad hygiene.

REMARKS UPON DR. GIBNEY'S PAPER.

Dr. Emerson to Dr. Gibney.—How about reflex action?

Dr. Gibney.—Tickling the feet caused scarcely any reflex action.

Dr. Seguin.—If the case did not have a history of an injury I would be disposed to look upon it as a case of myelitis of the anterior horns. I think that is present; perhaps something else.

Dr. Webber.—Would not pressure upon the nerve explain the symptoms?

Dr. Emerson to Dr. Gibney.—Were there any eye symptoms?

Dr. Gibney.—No, sir.

Dr. Seguin.—Cervical pachymeningitis would be thought of, but that would not begin with neck symptoms. A lesion of the nerve roots would have to extend from the atlas to the seventh cervical vertebra. Has the phrenic nerve escaped?

Dr. Gibney.—Yes, sir.

Dr. Seguin.—Is the sterno-mastoid paralyzed? I would be disposed to think part of the trouble due to myelitis of the anterior horns.

Dr. Shaw.—It seems to me that pain upon exertion or motion does not look like myelitis.

Dr. Gibney.—I regret that I did not make a thorough ophthalmoscopic examination.

Dr. Seguin to Dr. Gibney.—Is the heart's action disturbed?

Dr. Gibney.—No, sir.

Dr. Emerson.—There is a case of a physician in this city

who received a severe injury in the neck, below the third cervical vertebra, and as far down as the first or second dorsal vertebræ, by diving into shallow water. He was laid up, and for more than a year had pain upon rotation of the head. There were no eye symptoms, no retardation of the heart's action. By the use of counter-irritation over the spine the pain was entirely removed. There was no paralysis.

FRIDAY, JUNE 21ST—AFTERNOON SESSION.

The Association was called to order at 2.30 p. m., and upon motion, Dr. E. C. Spitzka, in the absence of President Webber, acted as presiding officer.

Dr. Seguin gave notice of an amendment to the constitution of the Association, whereby all matters of business shall be attended to by the Council, subject to the control of the Association.

Dr. J. J. Putnam, of Boston, proceeded at once to read his paper upon

THE INTRA-OCULAR CIRCULATION.

The paper consisted of a brief notice of some experiments by Dr. O. F. Wordsworth and Dr. J. J. Putnam, originally undertaken with a view of studying the effects of mechanical compression of the veins of the neck, upon the circulation within the eye, but bearing also upon the question of the cause or the venous pulse, the rhythmical activity of the vasomotor centres, and the action of nitrite of amyl.

The only general conclusion which Drs. Wordsworth and Putnam would draw from the results of their experiments, which are preliminary only, is that mechanical causes do not readily, if at all, induce an appreciable arterial congestion of the fundus oculi, and that the degree of venous fullness so produced is not such as to justify, if seen in an eye examined for the first time, the belief that it is pathological. They are led, therefore, to doubt whether such a condition as congestion of the retina unassociated with inflammation of greater or less degree, in which case the term congestion would be, properly speaking, a misnomer, is ever met with.

The authors of the above observations, while acknowledging the fewness of their experiments and their want of accord with those of other observers, still feel that their work was done with such care and precision as to incline them not to doubt their accuracy on account of the objection stated, and they are convinced, that in no department of science have errors of observation played a larger share than in the ophthalmoscopic study of the fundus oculi.

REMARKS UPON DR. PUTNAM'S PAPER.

Dr. Shaw.—Dr. Mathewson, of Brooklyn, examined my eye while I took nitrite of amyl. There was no change in the retina, and when I took quinine, there was no change in the retina or disk. We concluded that there was no dependence to be placed upon the appearance of the eye in cerebral anæmia and hyperæmia.

Dr. Gray.—The experiments to which Dr. Shaw alludes are valuable in certain respects, yet I hardly think they warrant the conclusions arrived at, for the first factor necessary to that argument remains unproven, namely, why is it that quinine produces hyperæmia or anæmia of the brain?

Dr. Seguin.—Unfortunately one of our members, who is himself much interested in this subject, is not with us this evening. As a matter of clinical experience I have arrived at the conclusion, that there is no great value to be placed upon this observation, partly because it is almost impossible to determine exactly between the normal and abnormal circulation, especially when only having seen the patient once. I have frequently seen cases where the retinas had previously been said to be congested, but in which I could find nothing that looked like hyperæmia. I am under the impression that the ophthalmoscope has been overrated in this respect. It would be very desirable, of course, if we could determine the difference between normal and abnormal vascular states within the eye.

Dr. Shaw.—It has always appeared to me a very difficult matter to determine a change in the intra-ocular circulation.

Dr. Putnam.—It is with great difficulty that changes in the circulation of the eye are brought about. There is an inelastic incasement, and it may be taken presumptively that changes

in the circulation could not be readily seen. The blood enters the eye under peculiar circumstances, and spreads itself rapidly through the arterial tree, for the reason that the capsule does not stretch at all readily. I am sure Dr. Wordsworth considers it difficult indeed, when the disk is quite red, to say from one examination, that the condition observed is abnormal; the physiological limits allow of great variation.

Dr. Seguin.—In this relation, it might not be out of the way to call attention to the fact, that the circulations of the retina, and the nerve itself, are independent. It seems to me desirable to state, independently in our notice of cases, the appearance of the retina and the disk; to state whether the retinal vessels are straight, or twisted, or over full. In some accounts it is simply stated, that the fundus of the eye was congested—a very crude formula, in my opinion.

Dr. Gray.—It seems to me the thing to be proven is, that the circulation of the conjunctiva, brain and fundus are alike.

Dr. Spitzka.—There is one disease which is associated with congestive attacks, namely, paralysis of the insane, and that has been the subject of ophthalmoscopic investigation. Klein, of Vienna, and Clifford Allbutt, of England, agree in frequently finding lesions in the eye. In the patient whom I exhibited before the Association the other day, the veins are tortuous and rather broad, the arteries being of normal calibre.

Dr. Putnam.—I would like to ask if any observations have been made upon the condition of the large veins of the retina? If the large arteries are full, we would expect to see the veins full also. The question was, whether it is possible or easy to have arterial congestion where there was no irritation inside sufficient to cause an increased tension.

Dr. Shaw.—I am of the opinion that the case which Dr. Putnam has been speaking of does not come under the same head as those of paresis.

DR. BEARD'S VERBAL COMMUNICATION.

The discussion having closed, Dr. George M. Beard made brief remarks upon a case of agoraphobia occurring in a lady whom he had under his care. The peculiarity of this case was her inability to go to church, though previous to an attack of

unpleasant head-symptoms while at church, she regularly attended church. The plan which Dr. Beard adopted for the relief of this symptom, was to build up and improve the general condition of the patient, after which she was able to attend church as before.

The second point which Dr. Beard brought out was in reference to the treatment of fibroid tumors by electricity. He thought it could be negatively proven, that the decrease in the size of the tumors, while under treatment with electricity, was brought about by an effect upon the nervous system.

Dr. Beard then made some remarks upon the "Jumpers, or jumping Frenchmen," who are to be found in the northern part of Maine, among the French Canadians and lumbermen of that region, and who have for a long time been known for jumping or striking on sudden excitation. It is a psychological and not a pathological state. It is not a nervous disease. It is the result of a psychical condition among a people of a low order of intellectual development. If one of these persons be suddenly told to strike another, he strikes right out, even if it be his own mother whom he hits. If he have an ax in his hand it makes no difference. If one be sitting by the edge of a pond of water and is struck suddenly, he will jump into the water, even at the risk of drowning. They will strike against a hot stove, or against a hard object. They seem to have no inhibitory power after sudden excitation. They are unable to speak English well, but if suddenly addressed in English, they respond automatically in English. It is a remarkable illustration of the involuntary life. Dr. Beard's attention was called to these people during the past year, and he has investigated the subject through various sources of evidence, but this summer he expects to do it more in detail in Maine. It is not an epidemic or a disease, but a fixed psychologic state, a survival of the habits of the middle ages. Very large numbers of these people are thus affected.

There being no remarks upon Dr. Beard's communication, Dr. Seguin read the next paper, which was by Dr. H. D. Schmidt, of New Orleans. The paper was illustrated by a box of microscopic specimens which accompanied it.

ON THE STRUCTURE AND FUNCTION OF THE GANGLIONIC BODIES OF THE CEREBRO-SPINAL AXIS.

To form a correct idea of the structure of the different nervous tissues, we must bear in mind that the whole nervous apparatus consists, in its central as well as its peripheral parts, of only three essentially nervous anatomical elements. These are fibrils, nuclei and granules, and, moreover, a certain intermediate so-called "inter-cellular" substance, by means of which granules linearly arranged in rows are united to each other and transformed into fibrils. The only difference existing between the individual nervous tissues consists in the morphological arrangement and mutual relations of these elements. The nerve medulla and tubular membrane of the dark-bordered nerve fibres, though they may serve some special purpose, must be regarded as non-essential to the liberation and propagation of nervous energy.

The axis cylinders of the dark-bordered nerve fibres are but the direct continuations of the subordinate branches of the processes of the ganglionic bodies. All axis cylinders consist of a number of individual nervous fibrils, which, united by a homogeneous inter-fibrillous substance, are surrounded by a delicate, tubular sheath. The individual fibrils of the axis cylinders are not *smooth* fibrils, but present a knotted appearance, which indicates their *original anatomical composition of elementary granules*, and can be recognized on all axis cylinders.

In passing from the axis cylinders towards the centre, we of course arrive at the processes of the ganglionic bodies; and as the axis cylinders represent nothing else but the subordinate branches of the processes, it becomes obvious that both are very probably identical in structure. In the mutual relations of these elements, however, a slight difference is already observed, which consists in the individual fibrils not being applied to each other as closely in the processes as in the axis cylinders. Hence, in the processes the individuality and the granulous character of the fibrils becomes more apparent. Finally, upon the ganglionic body itself, the granules, of which the fibres are originally composed, are most distinct;

but the individual fibrillæ cannot always be traced, for the longitudinal furrows have not proportionately increased in width. The best method of making preparations is that proposed by Gerlach.

From demonstrations which Dr. Schmidt gave, he drew the conclusion that the ganglionic body, or cell, is formed by a smaller or larger number of nervous fibrillæ, mostly derived from the axis cylinders of the nerve fibres, embracing an independent body, known as the nucleus. In embracing the nucleus, the fibrillæ of the different processes not only decussate, but probably also interlace with each other, particularly those in the immediate vicinity. While surrounding the nucleus, the fibrillæ form no direct communication with it, but remain isolated.

The nucleus of the ganglionic body differs from the nuclei of other tissues in the complexity of its construction. This complex structure was described in detail, and the statement made that all ganglionic bodies of the cerebro-spinal axis are constructed in the same manner. Dr. Schmidt gave a minute account of his researches in regard to the termination of the finest subdivisions of the processes in the grey matter, which were conducted, without regard to trouble or pains, for the purpose of coming to some definite conclusion. The results which he obtained from these examinations, made on specimens of grey matter of the spinal marrow, conscientiously prepared after Gerlach's own formula, were the same as he obtained from his examinations of uncolored chromic acid preparations, a number of years ago; and as these preparations were made with the greatest care and accuracy, and examined by superior objectives, Dr. Schmidt feels conscientiously bound to deny the existence of a fine nervous net-work in the grey matter of the spinal marrow, such as Gerlach has described, in which the finest ramifications of the processes of the ganglionic bodies terminate, and from which nerve fibres arise. As Gerlach's drawing of his net-work in Stricker's "Handbuch der Gewebelehre, etc." resembles, in some measure, the apparent net-work of the neuroglia, Dr. Schmidt is inclined to think that he mistook the latter for the former. The question now arises, in what manner do the ultimate

ramifications of the processes in question terminate? Dr. Schmidt now suggests, as he has done once before, that most probably a communication may exist in the form of anastomoses, though he cannot adduce sufficient evidence to support this suggestion.

Dr. Schmidt stated that in all cases we meet with fibrillæ surrounding a nucleus, and in order to show that this is a rule in the construction of ganglionic bodies, he described the manner in which these bodies are formed in the cerebro-spinal axis of the *Amphiuma tridactylum*, already known for the enormous size of its colored blood corpuscles. From facts presented, he concluded that all ganglionic bodies are composed of two elements anatomically and functionally distinct from each other. The one which is represented by fine nervous fibrillæ undoubtedly serves to conduct nervous energy liberated by stimuli applied at the peripheral surface, or emanating from a centre. The function of the other element, the nucleus, is more difficult to determine, but is probably related to the liberation of potential energy residing in it. That the integrity of the tissue depends upon the integrity of its nuclei is a pathological fact. And it is for this reason that Dr. Schmidt ventures to direct the attention of neurologists to the condition of the nuclei of the ganglionic bodies in diseased nervous tissues constantly met with in organic diseases of the nervous system. He considers the ganglionic body as an organ destined for the generation and the distribution of nervous energy, but incapable of receiving sentient or conscious impressions, and states that it is obvious that impressions can only result in consciousness in those nervous elements which represent the terminus, as well as the beginning of the nervous apparatus in the cortical layer of the brain. He states, further, that to suppose the nucleus is the particular receiving organ, is not logical, for the reason that the nervous fibrillæ carrying the stimulus to the centres, do not terminate in the nucleus, but in a delicate net-work which extends throughout the whole cortical layer. It would appear more probable that the stimulus coming from the sentient surface of the periphery, after being increased in strength by an additional amount of liberated potential energy from the true nervous cell, viz., the nucleus

of the ganglionic cell, passes along the nervous fibrillæ surrounding this body without interruption, and finally arrives in the terminal nervous net-work, to be, in proportion to its strength or intensity, diffused throughout its delicate meshes, and moreover to the granular substance itself.

Judging from the arrangement of the terminal elements in the grey matter of the spinal marrow, medulla oblongata, and other nerve centres, being analogous to that of those elements in the cortical layer of the brain, it seems reasonable to suppose that these parts also should be the seat of a limited amount of consciousness, which, though insufficient for intellectual manifestations, may suffice to give rise to those well-known reflex and automatic actions.

REMARKS UPON DR. SCHMIDT'S PAPER.

Dr. Spitzka.—I am agreeably surprised to find the results which I laid before the Association, yesterday, almost simultaneously confirmed by Dr. Schmidt. Although his researches were made on the amphiuma, and mine on the menobranchus, yet zoologically closely allied as these amphibians are, it was to be expected that both should present the same or similar histological peculiarities; and in a short preliminary communication in Dr. Jewell's journal, I have ventured to predicate the largely nucleated nerve cell, and the peculiar arrangement of the fibrils for the whole group of proteans and all amphibians with permanent external or internal gills.

In some points I must differ from Dr. Schmidt. He states that the amphiuma has numerous nerve cells scattered among the fibres of the white columns. From the analogous cases of the siren and menobranchus, I am inclined to believe that he has possibly mistaken free nuclei, analogous to the so-called free nuclei found scattered in the white substance of higher animals, for nerve cells. As regards Dr. Schmidt's views on the brain, when he states that the cortex is poor in cells, and seems to consider as the cortex the layer just outside of the thickly crowded free nuclei which border on the ventricular epithelium, I must again differ with him. In a series of preparations of hemispheric sections, beginning with the urodela and passing to the anoura, then the chelonia, and further on in

the animal scale, one can trace a perfect gradation of the free nuclei zone into the cellular layer of the cortex. As the animal develops, the nerve fibres accumulate between the free nuclei and the epithelium, and as this mass grows it entirely separates the free nuclei (metamorphosing, as some of them do, into pyramidal cells) from the mother-epithelium. This relation is well seen in turtles. The thickening of the ventricular floor, which is the rudimentary trace of a corpus striatum, is very feebly marked in the menobranchus and siren; it is comparatively better developed in the salamander and the frog. In the lower amphibia its cells are perfectly continuous with the free nuclei, and become separated therefrom in higher animals by the development of white substance. Dr. Schmidt styles the columnar arranged cells around the central canal, nerve cells. I have found that the extremity turned towards the free lumen had an epithelial fringe, the other end was connected with fibrils, and have had positive proof of the correctness of Stilling's view, that the epithelia were in reality of a nervous nature, of which the turtle's optic lobes give good evidence.

There being no further remarks upon Dr. Schmidt's paper, Dr. E. C. Spitzka read the last paper of the session, entitled

THE SIGNIFICATION OF NEGATIVE PATHOLOGICAL EVIDENCE IN INSANITY.

The result of experimental and pathological researches having demonstrated the close relation between brain and mind, some investigators were induced to defend the dictum, that all insanity was due to palpable cerebral disease. Although this view was in part erroneous, it still constituted a great step in advance of the psychological abstractions which, until then, had usurped the field of mental pathology. It stimulated not only alienists, but also general pathologists, to devote more attention to a neglected field, and if it has done no other service, has accomplished this much, that more elaborate methods of diagnosis, and sounder principles of treatment, are gradually becoming adopted in the asylums of most civilized countries. Several of the hitherto imperfectly recognized forms of insanity have received a clear demarcation by being proven to be associated with constant and characteristic pathological processes.

The purely symptomatic forms, as acute mania and melancholia, are not accompanied by any histological change. Vascular anomalies are however present, and thus serve to reduce these forms also to a symptomatic basis. It must not be supposed, however, that anæmia is equivalent to melancholia, and hyperæmia to mania, but that the conditions must be associated with other impalpable, perhaps biochemical anomalies, while the vasomotor relaxation of the one, and the vasomotor hyperkineses of the other, are as little referable to any known somatic basis, as the vasomotor hyperæsthesia of epilepsy. In one form of melancholia, even the subtle element of biochemical change may be wanting, and purely dynamic factors be the basis; it is that complete atonicity which follows a strong sudden emotion, as fright or joy, and which is comparable to the anæsthesia of the retina which follows exposure to a strong light.

The view that all insanity was due to palpable cerebral disease, was soon abandoned by European investigators, in consequence of these and similar observations. Its revival by those in this country, who, from their official connection with asylums claim to be considered alienists, is based on such imperfect observation and crude logic, that equally with our transatlantic cotemporaries, we can afford to consider a serious consideration or refutation thereof superfluous.

Let us select, as a paradigma of those insanities which present neither vasomotor nor structural abnormalities, that intellectual form of chronic mania which the Germans term *Paranoia* or *Verrücktheit*. A priori we need not look for any changes in a form of insanity whose main symptom is a delusion, analogous to normal (though perverted) thought in its component elements and logic. The delusions of the intellectual chronic maniac are widely different from the delusions of paresis and senile dementia.

A case was given, illustrating the fact to which Meynert alone has called special attention, that the cerebral structures may be completely healthy in this disease; the examination was thorough, specimens from every convolution preserved as quadruplicates immersed in different preservative media, and examined also in the fresh state, being carefully studied. All

the structures, both of the cortex, ganglia and medullary tracts were found ideally perfect, as well as the tutamina cerebri. Not even that accumulation of granules in the adventitia, and pigmentation in the base of the pyramidal nerve cells, frequently found in sane subjects, was present, probably for the reason that the sane subjects examined had died at a more advanced age than this patient. According to some pathologists, these appearances would, if present, have been chronicled as lesions of insanity, with how little reason is obvious. In order to show that such changes, especially in that extreme degree which is found with senility, have no real relation to the symptoms of chronic mania, the speaker related another case of a chronic maniac who had died at the advanced age of eighty, in whom moderate but general pigmentation of the cortical cells, and intense pigmentation of the nerve cells of the medulla, besides several small cysts, were found. These appearances are accompaniments of the senile state, as well in the previously sane as in the insane, and they affect previously contracted delusions, just as they weaken normal ideas, rendering them less original and energetic.

From these and several similar negative observations, we could learn the following lessons: First. That the changes to which the sane brain is subject should be carefully studied in order that we may be better able to exclude the possibility of their bearing a specific relation to insanity. Having done this, we should enquire whether a given change, which qualitatively does not necessarily cause insanity, may not produce alienation quantitatively. Second. That not every lesion found in advanced insanity stands in a causal and determining relation thereto; aside from the element of accident, we must bear in mind that such lesion may be the result of fluxionary hyperæmias oft repeated (as in querulant delusions and recurrent mania) as well as of the original pathological process with which the insanity may have started. For example, to infer that because one or two pareties out of a hundred have miliary aneurisms, miliary aneurisms are the lesion determining the symptoms of paresis, would be as absurd as to conclude that the extreme degree of cerebral congestive hyperæmia found in a patient who dies comatose and delirious from being confined

in an Utica crib, was the cause of his previous, often very harmless, insanity. The third lesson taught is, that debarred from finding the pathogenesis of all forms of insanity on a purely histological basis, we must look to other solutions of the problem. Although the chemical changes occurring in the brain at the moment of death are presumably so much more intense than the delicate biochemical modifications which, compatible with life produced functional under or over-excitation, and may, therefore, blot them out almost entirely, yet in extreme cases something may still be elucidated.

A fourth lesson is, that in endeavoring to obtain an insight into the mechanism of acute mental symptoms, we can only take into account the appearances found in the bodies of those who presented these symptoms immediately before death. If a patient who has suffered for several months from melancholia, dies in the delirium of a fever, we cannot expect to find anything in his brain pointing to the causation of the symptom melancholia, but rather to delirium.

Still another lesson is, that inasmuch as in intellectual insanity the abnormal mental processes though perverted are yet analogous to normal mental processes, we shall look to neither a morbid histological, vasomotor, or biochemical change. In these cases, asymmetries of the hemispheres, incongruities of development in the peduncular tracts, heterotopia and disproportionate development of the centres on one and the same side, have been found by Muhr, Betz, Zenker and myself. Although found in but a few cases, yet the temptation to judge from the condition found in them, to less palpable but similar ones, in all or at least most cases of chronic mania, is a great one, when we recollect how pre-eminently hereditary this disease is, and how logically correct it is to seek for incongruities in the anatomical associations, as the basis of faulty psycho-physiological action.

Before closing, I would refer to an error which has crept into the etiology of reflex insanity. Many alienists would unquestionably style a patient who, having developed a delusion, that he has the devil in his belly, is found to have extensive adhesions binding down the intestines, a case of reflex insanity. Now as at least in one of these cases I have been able to trace

the insanity further back than the peritonitis, and the subjective abdominal sensation had obviously only that accidental relation to the patient's insanity which every external circumstance, even outside the patient's body, might have in causing special delusions.

However, with all the negative pathological evidences in insanity, we find only confirmations of the conclusion that all abnormal, like all normal cerebration, has its immediate seat in the brain.

The true import of this negative evidence is to point out that the search for the somatic basis of insanity should not be a one-sided one, and that a rational psycho-pathology cannot afford to neglect any of the methods offered by anatomy, histology, chemistry, embryology or physics. The further we will be able to carry our reasoning by exclusion even, the more distinctly will the proper lines of research to be followed, be appreciated.

There being no remarks upon the above paper, and no further business, the Association adjourned, to meet in New York on the third Wednesday in June, 1879.

Neurological Correspondence.

By Geo. W. Wells, A. M., M. D.,
of New York City.

New York, June 25, 1878.

Messrs. Editors:—I have the honor to transmit herewith a resume of transactions in my peculiar field of inquiry—being in some cases a continuation of subjects heretofore considered, but which, from their importance, are worthy of preservation. We would call especial attention to the remarks of Mr. Rokenbaugh on "Testamentary Capacity," and the debate upon the same by leading members of the medical and legal professions; the blood-curdling debate upon Dr. Packard's paper proposing another method of executing criminals; and Mr. Eller's valuable remarks upon "Medical Jurisprudence of the Romans, etc."*

THE MEDICO-LEGAL SOCIETY.

At a meeting of the Medico-Legal Society, held May 1st, Mr. Henry S. Rokenbaugh, of the New York Bar, read a paper entitled

TESTAMENTARY CAPACITY AND THE MENTAL CONDITIONS AFFECTING IT.

He said: The subject under consideration this evening, we may divide into two general classes.

I. The soundness of mind requisite to make a valid will, and

II. How this testamentary capacity is affected by mental conditions.

As to the first, the degree of soundness of mind requisite to make a valid will, the statute relative to wills of *real property* declares that "all persons except idiots, persons of unsound mind, and infants," may devise real estate (2 R. S. 38, § 58; Laws 1867, chap. 782, § 3); and that relative to wills of *personal property*, that every person of a certain age or upwards, "of sound mind and memory, and no others, may give and

* With the exception of the first named, these are omitted in the present number for want of space.

bequeath " personal estate (2 R. S. 60, § 21; Laws 1867, chap. 782, § 3).

Though the language is different in the two cases, the condition of the testator's mind is in each case recognized, and the principle remains the same. The courts, too, from time to time have laid down rules for our guidance. It was formerly held in the case of Stewart *vs.* Lispenard (26 Wend. 255) that " imbecility of mind in a testator will not avoid his last will and testament," and a person, " be his understanding ever so weak," is competent to make a will.

This doctrine, however, was criticized and overruled in the celebrated case of Delafield *vs.* Parish, decided by the Court of Appeals in June, 1862, wherein the learned Chief Justice, the Hon. Henry E. Davies, in a very able and exhaustive opinion expressed his disapproval of the law as enunciated in the Lispenard case, and defined accurately the degree of mental soundness necessary to constitute a valid will. It was held in this case, that " in law the only standard as to mental capacity, in all who are not idiots or lunatics, is found in the fact, whether the testator was *compos mentis* or *non compos mentis*, as those terms are used in their fixed legal meaning. Such being the rule, the question in every case (of probate) is, had the testator, as *compos mentis*, capacity to make a will, not had he capacity to make the will produced. If *compos mentis* he can make any will however complicated; if *non compos mentis* he can make no will, not the simplest." It is therefore essential that the testator should have sufficient capacity to comprehend perfectly the condition of his property, his relations to the persons who were or might have been the objects of his bounty, the scope and bearings of the provisions of his will, and a memory of an activity sufficient to collect in his mind, without prompting, the particulars or elements of the business to be transacted, and to retain them in his mind for a period sufficient to perceive, at least, their obvious relation to each other, and be able to form some rational judgment with relation to them.

The doctrine herein laid down has been cited with approval by our courts to the present day, and well establishes the law on this subject.

Van Guysling *vs.* Van Kuren, 35 N. Y., 70.
Tyler *vs.* Gardner, 35 N. Y., 559.

Some eminent jurists, however, believe that *any* insane delusion, though unconnected with the subject matter of the will, destroys testamentary capacity *in toto*. Though this theory classes among its supporters as strong a name as Lord Brougham's, yet we cannot adopt it, for should we do so, we would be asserting a theory which it would be impossible to maintain. Was Dr. Johnson insane because he believed he heard his deceased mother's voice crying "Samuel?" Was Lord Castlereagh insane because one night, upon retiring to his room and extinguishing the light, he believed he perceived the figure of a young and beautiful child, and arose and followed it until it nestled in the arch of the great chimney, and at last sunk beneath the fire-board? Or was our late President Lincoln insane because, as his biographer, Mr. Lamon, says, "he lived constantly in the serious conviction that he was himself the subject of a special decree, made by some unknown and mysterious power, for which he had no name?" or because "he had great faith in the virtues of the 'mad-stone,' although he could give no reason for it, and confessed it looked like superstition?"

Should we, therefore, assert that *any* insane delusion incapacitates the making of a valid will, we would certainly be doing a great injustice to a great class of our fellow-creatures.

In accordance, then, with the tenor of the great body of American decisions and the weight of authority, the better opinion seems to be, that where the provisions of the will are entirely unconnected with the particular delusion, such will is valid. The Bonard will, which was offered for probate in the Surrogate's Court, of this city, in the year 1872, was an instance where this question was directly presented to the Court. Louis Bonard, the testator, devised and bequeathed his entire estate, amounting to about $150,000, to the American Society for the Prevention of Cruelty to Animals, believing that the souls of men, after death, passed into animals, and by leaving his money to the Society, he would consequently insure his own future existence. His heirs contested the will, alleging, among other matters, that the instrument propounded for

probate was not the free, unconstrained and voluntary act of the decedent; that he "was not of sound mind, memory and understanding, but on the contrary, that his mind was unsound, his memory impaired, and his understanding weakened by sickness and various other causes." It did not appear on the probate, however, that the testator made any declaration of his peculiar ideas in connection with his intended testamentary disposition. In pronouncing in favor of the will, the Hon. Robt. C. Hutchings, then Surrogate, held "that these opinions were not evidence of insanity or insane delusion, even though the testamentary intention might not otherwise, than for the alleged delusion, have been entertained." From the decision of the Surrogate, no appeal was taken.*

The present rule, therefore, as to the degree of mental capacity necessary to make a valid will, may be briefly stated as follows: The testator must be of sound mind and memory; he must be capable of disposing of his property with sense and judgment, both in reference to its situation and amount, as well as to the relative claims of those persons who are, or might be, the objects of his bounty. Let us then pass on to the second, and by far the more interesting general division of our subject, and see,

II. How this testamentary capacity is affected by mental conditions, considering the same as far as our limited time will permit. In pursuing this line of our inquiry, we may, too, be the better able to understand the former.

Testamentary capacity is affected by imbecility, delusions, monomania, or hallucinations, intoxication, lucid intervals, undue influence or fraud, and presumptions arising from the character of the act itself, the age of the testator, and such bodily infirmities as deafness, dumbness, or blindness.

Of imbecility, we need but say a word. Under it are classified lunatics, idiots and insane persons. There can be no

* This interesting case is but briefly alluded to, as it has been already fully reviewed in a very able paper heretofore read before this Society. Nor will we here refer to other cases illustrating singular beliefs, as they will be more properly considered in connection with another division of our subject.

doubt of the incapability of this class of persons to make a valid will, as our association with them plainly teaches us. As the test of mental capacity has heretofore been presented, we will now take up the topic of delusions, monomania and hallucinations.

Although, as we have seen, one laboring under a delusion can make a valid will, if its provisions are entirely unconnected with the particular delusion, yet there are cases in which the delusion is such as to amount to a general unsoundness of mind, and the person is entirely deprived of testamentary capacity. A nice degree of distinction must necessarily be observed by the courts in thus determining the competency of a testator. Medical testimony is often required; and, even with the opinion of experts and witnesses familiar with the decedent's ways and habits, the courts have in many instances been unable conclusively to pass upon the exact capacity of the testator whose will for probate comes up before them.

The will of Anne Thwaites, in the case of Smith *vs.* Tibbett (Law Rep., 1 Prob. and Div., p. 398), though not absurd in its provisions, offers us a remarkable instance of religious mental delusion. The deceased was a widow with a fortune of £500,000. She had made several wills and codicils since the death of her husband, some years previously, in all of which preceding wills and codicils a certain family of Smiths was left the bulk of her fortune. By the will propounded for probate, she left legacies amounting to £45,000 to the family of her husband; legacies of the same amount to the family of her sister, Mrs. Tibbett, besides various sums to other persons and charities. The residue of her property she left to one John Smith and one Samuel Smith. The will was contested by the defendant, Mrs. Tibbett (the sister of the testator), for a number of reasons, among others alleging the decedent was not of sound mind and memory at the time of the alleged execution.

On the probate it was shown that the deceased, for more than thirty years preceding her death, exhibited most prominent signs of monomania. She believed she had constant and direct communication with God; that she was the Holy Ghost, and that Dr. Smith was the Father; that with some other per-

son—not always the same—they constituted the Trinity. She believed, at another time, she "was above God seven degrees," and that epidemic diseases came through her agency. "God said, 'Turn on the screw,' and the cholera came. 'Turn it again,' and it ceased." In 1832, she had a severe illness, during which time she suffered much. After being restored to health and strength, she said it was at this period that she was born anew—"with new body, bones, nerves and blood." She thought she was about to give birth to the Savior, for which event she provided baby-linen. At another time, she said she was the bride of Christ; at another, the Virgin Mary. Lastly, she believed that the great final judgment would take place in her own drawing-room, and that she would sit there in judgment on her fellow-creatures with the Creator.

Time will not permit us further to consider this interesting case, to picture the sumptuous appointments of the judgment chamber or describe the magnificent toilet and diamonds with which she intended to decorate her person on the august occasion. Sir J. P. Wilde, in pronouncing against the will asks, "if any sane person ever conceived or fostered such ideas as these? Some of them, such as being the Bride of Christ, that there was a mystical and spiritual connection between herself and Dr. Smith, which enabled him to read her thoughts, that she was born anew in some mysterious sense, beyond a merely spiritual regeneration. I can conceive it just possible to have been bred in a mind not very highly educated or robust, acted upon by an excitable, nervous, enthusiastic temperament. But will anything we know of the natural and healthy working of the human mind, extend to the conception of a false identity, such as that involved in her being one of the Holy Trinity, or the Virgin Mary?

"Or still further, to her assumption of the Divine attributes, in the preposterous expectation that she would sit in judgment on the rest of mankind, culminating as it did in the puerile bathos of the London drawing-room, with its velvet and silk, as the scene of her future glory.

"Religious and fanatical enthusiasm will account for much, but did it ever stretch so far and stoop so low?"

Another man, otherwise intelligent, insisted on his being

an umbrella, and always stood himself up in a corner when he entered the house; while a third considered himself a coffee-pot, and was continually going about, with one arm akimbo and the other representing a spout, asking to be "poured out."

As other cases of great interest on this subject, we may refer to

Stanton *vs.* Wetherwax, 16 Barb. 259.
Waring *vs.* Waring, 10 Jurist.
Dew *vs.* Clark, 3 Addams, 79.
Thompson *vs.* Thompson, 21 Barb. 107.

Akin to the subject of delusion in the execution of wills is that of *eccentricity*. The two, however, are vastly different and a clear distinction can be drawn. The will of one made while laboring under delusion, we find to be wholly different from what it would otherwise have been at any other period; while eccentricities in the will of an eccentric are naturally to be expected. An interesting case for determination came up in Paris in 1864. Machado, a wealthy Portuguese died in 1861, having made a will with seventy-one codicils, one of which read as follows: "I leave for the Athenæum of Paris 10,000 francs, the interest of which at five per cent., will be 500 francs. Half of this interest to be paid to a Professor of Natural History, who shall lecture on the colors and patterns of dresses and on the characters of animals." Another codicil was in these words: "My funeral shall take place at 3 p. m., the hour at which the rooks of the Louvre come home to dinner." As such directions carried absurdity on their face, the will was contested on the ground of the insanity of the testator. It appeared on the hearing that the deceased had entertained the most peculiar and strange desires. He wished upon his death that stuffed specimens of birds should be placed in his coffin, that his servant should carry a favorite one to the ceremony, and that certain others should be let loose on the day of his funeral. During his life he erected his own tombstone, on which was engraved a sun, a bird, an ox and a dog. Above them was cut an egg and beneath, the inscription, "Here reposes the author of the 'Theory of Likeness.'" The will of this singular man was admitted to probate, the

court saying that these peculiarities were but the absurdities of a vain man, and the deceased having managed his affairs cautiously and prudently during his lifetime. The will of a Mr. Stott, referred to in the 2d volume of Taylor's Principles of Medical Jurisprudence, is an interesting case, where the testator fancied he could deliver pregnant women by means of electricity, and proposed to the wife of a neighboring baker to bring about her *accouchement* by a number of wires connected with an electrical machine.

Though electricity, it seems, has since been used to aid parturition, it has been under circumstances very different from those which gave rise to the absurd idea in the case just cited. This will was not sustained. Morgan *vs.* Boys is another instance of a testator's peculiar notions, where, in his will, he directed that his executors should "cause some parts of his bowels to be converted into fiddle-strings—and others should be sublimed into smelling-salts, and that the remainder of his body should be vitrified into lenses for optical purposes." Attached to his will was a letter in which he says: "The world may think this to be done in a spirit of singularity or whim, but I have a moral aversion to funeral pomp, and I wish my body to be converted into purposes useful to mankind." Eccentricity only being shown, *not insanity*, the will was held a valid one.

Very many curious cases of eccentricity—eccentricity even bordering on insanity—are to be found in the books. Sometimes the decedent has evinced a strong propensity for animals, as is shown in the case of Yglisias *vs.* Dyke (Prerog. Court, May, 1852), where fourteen dogs of both sexes were kept in kennels in the drawing-room—two sleeping in the room of the testatrix, while a third—a blind one—she took to bed with her. In another case, cats were the peculiar hobby, these animals being fed at regular hours, and provided with plates and napkins.

Another old lady kept her room full of parrots; another of monkeys. But we must not devote too much time to this amusing though interesting branch of our subject. Let us be careful, however, and not confound eccentricity with insanity; for Cicero says: "Nihil tam absurdi dici potest quod

non dicatur ab aliquo philosophorum" (Cicero de Divinatione II., 58). There is nothing so absurd but that some philosopher may be found by whom it is affirmed.

Redfield, in his excellent "Treatise on the Law of Wills," draws a line clear and distinct between eccentricity and delusion when he says, "The *eccentric* man is aware of his peculiarity, and persists in his course from choice, and in defiance of popular sentiment; while the *monomaniac* verily believes he is acting in conformity to the most wise and judicious counsels, and often seems to have lost all control over his voluntary powers, and to be the dupe and victim of some demon like that of Socrates."

The next point we will consider is the effect of intoxication. Shelford, in his "Treatise on Lunacy," tells us that "drunkenness, to such an extent as to render the party unconscious of what he is engaged in, or drunk even to a slight degree, when its effect is to render a party subject to the influence of others, avoids a will, though the mere fact of the testator being at the time under the influence of liquor, will not suffice, unless consequent disability be proved" This is the rule laid down by our courts, and reference to but one well-established case will suffice as an illustration, that of Peck *vs.* Cary, reported in the 27 N. Y. 9.

Robert L. Peck died at New London, Conn., in the year 1859, leaving a last will and testament which was admitted to probate by the Surrogate of New York County, though contested on the ground that the testator was intoxicated at the time of its execution. An appeal from this decision being taken, the Court of Appeals, in rendering final judgment, held the will a valid one; and Chief-Justice Denis, in his able opinion, says: "It is not to be understood that a will made by one who is at the time under the influence of intoxicating liquor, is for that reason void. Intoxication is said to be temporary insanity. The brain is, at the time, incapable of performing its proper functions; but that species of derangement ceases when the exciting cause is removed, and sobriety brings with it a return of reason. In order to avoid a will made by an intemperate person, it must be proved that he was so excited by liquor, or so conducted himself during the particular act,

as to be, at the moment, legally disqualified from giving effect to it (Shelford on Lunacy, 276). Incapacity arising from intoxication differs from ordinary lunacy in this, that the effects of drunkenness only subsist while the cause, the excitement, visibly lasts. (See also, on this subject, Gardner *vs.* Gardner, 22 Wend., 526; Burrett *vs.* Silliman, 16 Barb. 198, 1 Redf., 454; Lowe *vs.* Williamson, 1 Green, Ch. 85.)"

We next come to the consideration of a will made during a lucid interval of a testator's mind. Though it is generally admitted a person, heretofore insane, can make a valid will during such an interval, yet such intervals, we find, are of very rare occurrence. The law supposes the probability of such a period, though its existence in a particular case must be shown. It is not necessary that the highest degree of reasoning power should prevail; for were this the case, persons who have never been insane would in many instances be incapacitated, and a large portion of the human family entirely deprived of the disposition of their estates. It is only necessary that the testator possess a *disposing* mind (Holyland ex-parte, 11 Vesey, 11).

When the mind of one is so afflicted that he is incapable of acting rationally in the ordinary affairs of life, and incapable of understanding the effect and consequences of his acts, he is rendered incompetent to make a will. The law presumes that every man is sane, though, if general lunacy be once established, the party holding the will to be executed during a lucid interval, will be under the necessity of showing, not only that there was a cessation of the violent symptoms of the disorder, but a restoration of the faculties of the mind sufficient to enable the party to judge soundly of the act.

Sir William Wynne, in Cartwright *vs.* Cartwright, (1 Phill., 90) remarks that "the strongest and best proof that can arise as to a lucid interval is that which arises from the act itself. * * * * If it can be proved and established that it is a rational act, rationally done, the whole thing is proved."

Coghlan's case, reported in the *Law Times* of July 6, 1844, is an instance where the will of a lunatic, actually confined in an asylum, was allowed, as having been executed during a lucid interval, on the ground that he was suffering from "no

disorder at the time, though he was afflicted with a distemper of mind to a very great degree, and the will was consistent with his intentions when of capacity."

The case of Chambers *vs.* The Queen's Proctor is an instance where a testator committed suicide the day after he executed his will. Insanity was shown to have existed about a year before, and for three days preceding the *factum* the deceased had been laboring under insane delusions. Sir Herbert Jenner, in his opinion, says: "What is the Court to do, in order to see whether the act of the deceased is a valid act? It must look to the manner in which the act was done to satisfy itself whether a lucid interval is established. It cannot be contended that the delusion was fixed and of long duration; and if done during a lucid interval the act will be valid, notwithstanding *previous and subsequent insanity.*"

The case of Edwards *vs.* Edwards (Prerog. Court, Feb., 1854) might also be cited as an illustration of this principle. Here it was shown the testator had committed suicide three days after the execution of his will, and although it was also proved that the deceased was of eccentric habits, almost amounting to insanity, his will was pronounced valid and admitted to probate.*

Undue influence and fraud are also elements we must consider in determining testamentary capacity. Daily do we find persons of sound and disposing mind and memory, worn out by a long and painful illness, falling victims to the scheming and unscrupulous. Yet the law is vigilant, and strives in all ways and by every safeguard to provide against such imposition. We do not here refer to that influence arising from a sense of gratitude, affection, or esteem, for such an influence will not avoid a will, but rather that exercised by coercion, imposition and fraud. Chief Justice Lowrie, in his opinion in the case of Dean *vs.* Negley (41 Penn. St. Rep., 312), distinctly says, that "lawful influence, such as that arising from legitimate family and social relations, must be allowed to produce its natural results, even in influencing last wills. How-

* Many other cases might be cited to show that suicide is not deemed as proof of insanity. See paper Judge Palmer read at last (April) meeting.

ever great the influence thus generated may be, it has no taint of unlawfulness in it, and there can be no presumption of its actual unlawful existence, merely from the fact that it is known to have existed and that it has manifestly operated on the testator's mind, as a reason for his testamentary dispositions. Such influences are naturally very unequal, and naturally productive of inequalities in testamentary dispositions; and as they are lawful in general, and the law cannot criticize and measure them so as to attribute to them their proper effect, no will can be condemned because the existence of such an influence is proved, and because the will contains in itself proof of its effect. It is only when such influence is unduly exerted over the very act of devising, so as to prevent the will from being truly the act of the testator, that the law condemns it as a vicious element of the testamentary act." The free agency of the testator must not be destroyed or overcome, there must be no constraint operating on his mind at the time the will is made. (See, also, 3 Bradf. 320, Clapp *vs.* Fullerton, 34 N. Y., 190, 4 Abb. Ct. App., 191.) Undue influence, fraud and duress, are in many instances difficult to be determined, and courts look with jealousy at all the circumstances, the condition of the testator and the relation of the parties. One offering an instrument for probate, as a last will and testament, must show satisfactorily that it is the last will of the alleged testator; but if fraud is alleged to have been practiced, the charge *must be established*, that it was *actually and successfully practiced*, for it is not enough to show that an *attempt* was made to deceive the testator by *suppressio veri* or by *suggestio falsi*."

"It is not the duty of the Court to strain after probate," Judge Davies said, in the case of Delafield *vs.* Parish, already referred to, "nor in any case to grant it where grave doubts remain unremoved, and great difficulties oppose themselves to so doing."

Undue influence is not often directly proved, and may be inferred from circumstances (Marvin *vs.* Marvin, 3 Abb. Ct. App., 192). Thus we find wills have been set aside when a daughter was constantly with a testatrix during her last illness, and received the bulk of her property to the exclusion of other

relatives (Tyler *vs.* Gardiner, 35 N. Y., 559); where there was over-importunity of friends standing in confidential relations (Bates *vs.* Bates, 27 Iowa, 110); where a husband was the principal beneficiary under a will made by his wife (Marsh *vs.* Tyrrell, 2 Haggard, 87, 110); where a will is made in favor of a medical attendant to the exclusion of relatives (Prerog. Court, July, 1844); and where the will is in the handwriting of the principal beneficiary, or drawn up at her suggestion by counsel procured by her (Delafield *vs.* Parish, supra).

Declarations or previously expressed intentions of a testator, differing from the provisions of his will, form other evidence, tending to show that a will was executed under the influence of fraud or duress, and must be overcome by the most satisfactory testimony that the testator understood its provisions, and acted freely (Lee *vs.* Dill, 11 Abb. Pr. 214; O'Neil *vs.* Murray, 4 Bradf. 311).

So, too, secrecy and contrivance may, at times, become badges of fraud, "but when such circumstances can be clearly traced to the mind of the testator himself, they cannot be received as having any tendency to impeach his testamentary dispositions" (Coffin *vs.* Coffin, 23 N. Y., 9).

We need not discuss this branch of our subject further. The case of Rollwagen *vs.* Rollwagen, recently decided in our highest court (63 N. Y., 504), is of itself quite an *epitome* of the law of duress, undue influence, and fraud, and though still fresh in our minds, we regret that time will not permit a recapitulation of it here.

We come, in conclusion, to some of the *presumptions* which the law recognizes as tending to establish or disprove the validity of wills.

By the Roman law, no person who lacked some of the principal senses could make a will. Blackstone says of those born deaf, dumb, or blind, that, "as they have always wanted the common inlets of understanding, they are incapable of having *animum testandi*, and their testaments are therefore void." This rule, however, was subsequently somewhat relaxed, and persons were allowed to make a will where the defects were not congenital, and the testator was possessed of sufficient capacity (Cod. Lib. VI., Tit. 22, § 10). A blind man

might make a nuncupative will by declaring the same before seven witnesses, but he could not make a testament in writing, unless it was read to him, and acknowledged by him, before witnesses. That the will should be read to the testator is no longer necessary, though preferable; and our statute is at present satisfied, if the testator subscribe the will at its end, in the presence of two witnesses, or acknowledge such subscription, declare it as his will, and request the witnesses to sign the same, which they shall do in his presence, and in the presence of each other. Yet even a will executed in this manner, and complying with all the forms of law, may be impeached, for it must also be shown that the testator was in every way cognizant of his act, and that his mind accompanied the will. When the testator is deaf and dumb from infancy, an inquiry is necessary as to the degree of development of his mental and moral powers. If insufficient intelligence is shown, the testamentary act is void, but if the testator possess sufficient mind (which may be determined by questions propounded and answers given by signs or in writing) his testamentary act is valid (Moore *vs.* Moore, 2 Bradf. 261; Weir *vs.* Fitzgerald, 2 id. 42; Oliver *vs.* Berry, 53 Me. 206; Reynolds *vs.* Reynolds, 1 Spear, 756).

Another matter to be taken into consideration in determining the capacity of a testator is that of *old age*. "Great age, alone," says Surrogate Bradford, "does not constitute testamentary disqualification, but, on the contrary, it calls for protection and aid to further its wishes, when a mind capable of acting rationally and a memory sufficient in essentials are shown to have existed" (Maverick *vs.* Reynolds, 2 Bradf. 360). Yet there are many cases to be found in the books where the minds of persons of advanced age have become so impaired and enfeebled that their wills have been set aside. As an instance of this the case of Dumond *vs.* Kiff (7 Lansing, 465), may be referred to, where the testator died at the age of eighty-seven, having executed a will in his eighty-first year. Upon the instrument being offered for probate, it was shown that the testator failed to know his own children, and frequently inquired how many he had.

"A person's mind in extreme old age," says an eminent

jurist, "may be quite intellectual, his understanding of business clear, his competency to converse upon and transact such undoubted, and his bodily strength good; but there may grow upon him such a fear and dread of relatives and servants, who may have surrounded him, and on whom he may have become perfectly dependent, that his nervous system is wholly overcome, and he becomes a mere child and tool in the hands of those about him, so that he has no power to exert his mind in opposition to their wishes, or to resist their importunities. His mind is enslaved by fears and doubts, and a feeling of helplessness, so that, to that extent and in matters in which he may be moved by them, he readily is facile and imbecile. This state of things seems to be easily brought on in old age, when the faculties are otherwise entire, and the bodily strength considerable."

Great caution, too, is necessary in determining the *nature* of the influence exerted upon aged persons, as a ground for setting aside a will. Mere opinions of witnesses should have no weight; facts, rather, should be proved. We have already seen how the influence acquired over a testator by kind offices, unconnected with any fraud or contrivance is a natural one, and can never *alone* be good ground for impeaching a will. Nor can we expect to find in one of advanced years that strength of mind he once possessed. The memory necessarily becomes, to some extent, impaired and enfeebled, and to deprive one of the control of his property because of this fact, would not only be unjust, but would be robbing him of the only means he has left of doing good in the world, and providing for those he holds near and dear.

Courts, therefore, also exercised great caution that mere old age and its attendant infirmities are not made the pretext for establishing that unsoundness of mind which will deprive one of his liberty and property.

Another presumption as to the validity of a will may be deduced from the *character of the act* itself. A will making a just distribution of an estate is, *per se*, strong evidence of testamentary capacity, while a will unnatural in its character is presumptive evidence to the contrary. The case of Barton, determined by the Ecclesiastical Court of England, in 1840, is

a case where the testator, laboring under the delusion that he could retain control of his property after death, made himself his sole legatee and executor. Such a will, of course, was the product of a diseased mind, and on its face invalid. A testator, however, has the right to make an unreasonable and injudicious disposition of his property, even to the exclusion of his own relatives, yet, "unless the will on its face," says Judge Whelsley, in Boylan *vs.* Meeker (4 Dutcher, (N. J.,) 274), "carries clear marks of being the product of a diseased mind, its injustice, its unreasonableness ought not to be the foundation of a verdict against it."

We have thus briefly shown the degree of intelligence necessary to constitute testamentary capacity, and considered the conditions of the mind affecting it. As we are oftentimes liable to err in a hasty judgment of the testamentary act, we should at all times inquire into all the conditions surrounding it. Should physicians bear in mind that when asked to witness a will, they also bear testimony as to the soundness of mind of the testator, and lawyers in drawing a will assure themselves that it is not the expression of a sudden or capricious whim, the bulwarks against artifice and fraud would be strengthened, and our courts relieved of many and perplexing questions. Yet even then we may expect difficulties to arise, "so long as human nature is the mysterious phenomenon that it is, and the empires of reason and unreason border so closely on each other."

The following interesting debate ensued:

Hon. Henry E. Davies stated that the subject of testamentary capacity had been one of very grave consideration and the most elaborate discussion by courts and jurists ever since the statute of 32d Henry VIII., which authorized persons to make wills in writing. Before that statute, testamentary disposition of property was unknown; and he thought the lecturer had clearly shown that the principles which now govern courts in adjudicating upon wills have been very much simplified, and are now very clearly and accurately understood.

In the first place, a man owning property, or an individual in possession of money, has no inherent or natural right to

make any disposition of that property, to take effect after his death. While the legislature has determined that it is in the power of an individual to make disposition of his property after his personal control over it ceases, in the form of a testament or will, to take effect after his death, yet to guard against fraud—to guard against a disposition of property not intended by the person disposing of the property—it has thrown around such act many safeguards: first, in reference to the factum of the will, as to the form in which the thing is to be done; second, that the person who is making the will should be of sound and disposing mind and memory at the time.

The cases which have been referred to by the lecturer are only a few of those running through the history of wills, for all the adjudications which have taken place in reference to them are, after all, only indices of what must eventually be the final result. Was the person making the will of a sound and disposing mind and memory at the time the will was executed? It has to come back to that finally.

Now the cases which have been referred to are, many of them, full of intrinsic interest. The Parish will case, which has been spoken of, was one of the most notable which, perhaps, ever came up in the history of judicial administration.

In June, 1849, Mr. Parish was stricken with paralysis and became, as the speaker thought the testimony proved, utterly incapable of the expression of his wishes, or of indicating them intelligently to persons around him. Codicils were made to a will made in 1842, while he was in this state of mind; and these came before the courts for adjudication. Judge Bradford, one of the most able men on testamentary law which this country has ever produced, had the advantage of a very learned discussion before him by the ablest counsel of the New York Bar, and he rejected the codicils to the will, except the first. There were some peculiar reasons why that should have been admitted to probate.

The case went through various courts, and was argued in the Court of Appeals twice, and the result at which that court arrived has been alluded to by the lecturer of this evening. He thought he might with propriety state, that the judgment of

that court is generally received by the profession as expressing truthfully the law upon the subject of wills. The very able and exhaustive medical testimony, which was comprised in an octavo volume of 700 or 800 pages, greatly aided the court in arriving at a just and correct decision. The legal profession have been generally benefited by medical testimony in cases of this kind, for it was peculiarly the province of the medical profession to study the operations of the human mind, and so cognizant are they of its functions and its operations that they are the first to notice its decay and aberrations.

The case alluded to by the lecturer of Mrs. Thwaite's will was a most extraordinary one. It was a case in which the person making the will was under a delusion, and the lecturer has very well said, that to set aside a will made by a person laboring under a delusion, it is necessary that that delusion should be connected with and predicated upon the testamentary disposition; because it will strike every person's mind in a moment, that one may be under a delusion on particular subjects, and yet such a delusion would have no influence whatever upon the testamentary disposition of property. On the other hand, the disposition made may be the result of a particular delusion, and therefore you see immediately, that the disposition of the property is not the act of a sound disposing mind and memory.

Now, the case of Mrs. Thwaite was extraordinary in that regard. She had a physician, a Doctor Smith, to whom she had become very much attached, in a proper way, and had come greatly under his influence, also not improperly, but he indulged her in her whims and led her on in the harboring of her absurd delusions. At his suggestion, his brother was employed as her agent to manage her estates, and thus you see he got a controlling influence over her affairs, and thus would he be able to operate upon a person whose mind was diseased and disorganized. Her peculiar delusion was, that she was to give birth to a second Saviour, and she went to extravagant expense in preparation for the expected event. Under the circumstances it will be readily understood how easily her mind could be influenced by and operated upon by those around her.

This will was very properly set aside as entirely improper,

and as a will of one who was not of sound disposing mind and memory.

Now, we have another case in this State, where a prominent merchant of this city was subject to almost as absurd a delusion as that of Mrs. Thwaite. His name was Abraham G. Thompson. He was a firm believer in the story that the treasure of the late Capt. Kidd had been sunk in the North River, at the foot of the Dunderberg Mountains, opposite Peekskill, and he expended a very large amount of money in fitting up and sending a vessel to engage in the search for it. The vessel was anchored in the North River for a long time in the endeavor to raise the treasure from the bottom of the river. He was a firm believer in that absurdity, and he proved his faith by his works. Of course, it was all a delusion, a vanity of vanities, and yet, when he made his will, it was apparently a reasonable and sensible will. Nevertheless, it was attacked on the ground that he was the subject of a delusion, but not because the delusion was connected with, or operated upon, the disposition of the property. It was held that he was competent, and the will was admitted to probate.

That, he thought, was one of the few cases where delusion of the testator has been held *not* to affect testamentary property. Then the other cases of fraud in disposition of property and undue influence, are familiar to all jurists, and ought to be reasons for setting aside a will when it is proved and established that the will has been procured by any of these influences.

Another very striking cause for setting aside a will, and a great cause for suspicion, is when the individual becomes the writer of the will and the recipient of its benefits.

The law always attaches its stigma to, and is very astute in looking out for, reasons why such a will should not be established; and the parties in interest have got to prove, beyond all peradventure, that the will was the will of the person making it, and not influenced or suggested by "qui se scripit hæredeme." By the civil law, such a will was rendered void, and though this rule of the civil law has not been adopted in our courts, yet they do demand satisfactory proof in such cases, that the party executing the will clearly understood and freely

intended to make that disposition of his property which the instrument purports to direct.

The lecture to which we have listened with so much interest this evening, evinces great research, and a careful examination of the cases and principles governing testamentary capacity. It is a valuable contribution to the study of the law of testamentary disposition, and is highly creditable to the learning, industry and legal acquirements of the lecturer. It is worthy of a place among the archives of the Society, and should be published with its proceedings.

REMARKS OF PROF. W. A. HAMMOND, M. D.

There were only one or two points in the paper of Mr. Rokenbaugh upon which he would like to remark.

In the first place, it appeared to him that a sufficient distinction had not been made between the different kinds of delusion. It is not every delusion that is an evidence of insanity. He had the honor of being one of the experts in the Bonard will case, and he thought he was the first to make the point that a delusion, to be evidence of insanity, must relate to matters of fact, and that we do not know in fact whether a delusion, as regards faith, is a delusion or not.

Take the case of Thompson, which has been alluded to by the learned gentleman who last spoke. He thought the action of the court in that case was correct, for nobody knows to this day whether his belief was a delusion or not—whether Captain Kidd's treasures are at the foot of the Dunderberg Mountains or not. His delusion was solely as regards a matter of faith, and not of fact, therefore the will was properly sustained. Suppose he had found the treasure, his belief would not have been a delusion then! He was sure that very much greater delusions than Thompson's have been proved to be facts upon further investigation.

Now, Bonard believed the doctrine of transmigration of souls, and it was attempted to set aside his will on account of that delusion. He said, on the trial, that the doctrine of transmigration of souls is a religious belief—it could not be proved or disproved—it was a matter of faith. He also said that if Mr. Bonard had had a delusion in regard to a matter of fact—

whether it bore upon the matter of his will or not, he should not have believed that he was competent—but no matter what his belief was as regards a matter of faith, it is not evidence of insanity.

Look at the religious beliefs of the world, how many and diverse they are! We do not call Spiritualists lunatics, because that question is a mere matter of faith. But if a man goes through the world thinking himself a coffee pot, that man is suffering under a delusion in regard to a matter of fact, and the speaker would be in favor of setting aside his will. How perverted must be a person's intellect who is brought to imagine himself a coffee pot! He did not think that man should be allowed to make a will, or that his will should be sustained.

The essayist failed to draw a sufficient distinction between delusion and insane delusion. Delusion, simple, is false belief, and all false beliefs are not evidences of insanity, and should not incapacitate the making of a will even as regards matters of fact; but insane delusions are altogether different. If a man holds a false belief in regard to matters of fact, out of which he cannot be reasoned, then he has an insane delusion and is incapable of making a will.

Then, again, the essayist was somewhat mistaken in asserting that sane acts are always evidence of sanity. He thought alienists hold that they are not necessarily so, because if all sane acts were evidence of sanity, two-thirds of all the lunatics should be discharged. It is a sane act to get away from fire when it burns. It is a sane act for a lunatic to eat his dinner, or to poke a fire with a poker. It requires a continuity of sane acts to prove sanity, and if the sane acts are interrupted by insane acts, that man is insane for the time being; if the insane acts continue he is continually insane. He did not think the legal profession goes into the science of this matter, and he was not in favor of having it different.

Every man who is a little aberrant, in the minds of some alienists, is insane altogether. Some go so far as to say that a man committing a crime is insane. The commission of crime is not an evidence of insanity. He is in favor of punishing insane people just as he would a tiger who went about

destroying people. If a lunatic had a homicidal mania he would hang him. He thought that the protection of society requires that the law and science of insanity should not coincide, and he was happy to find that they did not.

Regarding lucid intervals, he remarked that he was not so sure that there was any such thing. A man is either insane or he is not. A lucid interval, so called, is, in the vast majority of cases, probably a simple remission in the violence of insanity, and is not evidence of the sanity of the patient by any means.

The subject of delusion is one which it is particularly well to be considered. Those which relate to matters of faith, those which relate to matters of fact, those which are simple delusions, as well as those which are insane delusions.

REMARKS OF AUSTIN ABBOTT, ESQ.

Mr. Abbott said that the essayist had brought before the society in an interesting way, what is one of the most important characteristics of the present law of insanity; and that is, that to a certain degree, the law considers the question of mental competency with reference to the nature of the act which has been performed.

In old times—and the books are full of the records of it—the question was one of apparent, obvious incompetency—"an idiot or person furious" is the language of the old books; the insanity recognized by the law was a total want of reason, a manifest state of brutishness, which appeared in the demeanor, the talk, etc;—and the adjudications seem to have been confined strictly to this class. In modern times the physicians have shown very different elements in the case. And the law, following science as it always does, though often slowly and at a distance, now recognizes occult insanity. But we look at it differently. The physician looks at mind, if he might be permitted to use the figure, as if he were surveying the apparatus which is to do the act: he inquires whether there is anything defective in it as a mechanism (so to speak), and considered as an entirety. The law on the other hand looks at the act which has been done, and inquires, is there anything in the character of this class of acts which that mind, defect-

ive though it may be, is not competent to perform or responsible for performing? This difference between the medical and legal aspects of insanity, he suggested, is the cause and the explanation of a great deal of the controversy which has prevailed on this subject. One of the great services which Delafield *vs.* Parish has done to our jurisprudence has been in settling the legal doctrine in this respect. The Bench have always adhered to the view that the test of a man's competency, and the legal test of insanity, is his being "*non compos;*" but if you look into the past down to about sixty years ago, it will be seen that the lawyer's idea of insanity has been a man without a mind, not a man of deranged or defective mind merely; but in a legal sense, *without a mind*. And as you go back in the history of the law, that test becomes stronger and stronger, until you reach the time when, if a man could not count twenty, could not recognize his parents, &c., or was like a wild beast, he was regarded as non compos.

Now the case of Delafield *vs.* Parish excluded the idea that insanity, to be dealt with by the law, was an entire absence of mentality, and established this test, that the power of the mind is to be considered in reference to the character of the act to be done; and no matter what may be the source or nature of the mental defect, if it is one which leaves no adequate free power for that act, the act is defective.

Now it is curious to observe the application of that principle among cases which have arisen since the courts have begun to act upon it. Quite a number of classes of cases have already been recognized by the law, and it has come to be acknowledged that the same rule is to apply to every one of them, viz., that unsoundness of mind, in the general or medical sense, does not necessarily amount to incompetency, in the legal sense.

When it is objected that a man is not competent to testify because he is insane, the question is no longer whether his mind is defective, but whether he has that kind and degree of insanity which would prevent him from observing the facts, or from giving us a narration of them. If the contention is that a partnership ought to be dissolved because a member of it has become insane, the question is of a different character;

viz., whether he is so insane that he is no longer able to perform his functions under those articles of copartnership—an entirely different thing. He may be competent for one purpose, and not for another. When the question is whether he is to be punished for an offence, the issue is again entirely different—*i. e.*, whether he knew what was right or wrong in respect to this act at the time of the commission of the deed, and whether he had the power of self-control or not. This question as to the power of control, however, is still among lawyers a mooted point. If the question at issue is one of ordinary contract, the inquiry is not whether his act was of such a nature as to cancel a partnership, disqualify a witness, or excuse a crime; but rather the inquiry will be in relation to the competency for the kind of act in question. Thus, however, the question may arise in its different forms, as it constantly does in legal practice; then it will resolve itself in this, Was the man competent to perform acts of this class? If not, the act is defective, no matter how capable he may have been in other respects. If the man was capable to perform, freely and intelligently, acts of that character, his act is not impaired, no matter how defective the mind may have been in other respects.

It appeared to him that these principles are fully supported by the present state of the law of insanity, although there are many cases in the books which do not concede this position, and apparently insist on the idea of a total lack of mental power.

There is a class of cases where the jury have found a man to be of unsound mind, and incapable of managing his affairs, upon which the law pronounces him generally incompetent.

There was a case recently—say about the year 1870—in the records of the Supreme Court of the United States (Dexter *vs.* Hall, 15 Wall., 9), which goes on this idea, that if a man is found to be a lunatic, nothing that he does can be valid, because, as the syllogism goes, to perform any civil act, mind is necessary; a man who is a lunatic has no mind; therefore, there is no act, in a legal sense. That doctrine, although still sound as to the general effect of an inquisition, has been wholly exploded in other respects, as the essayist has shown.

Thus in the case of Banks *vs.* Goodfellow, it was held by Chief Justice Cockburn (L. R. 5 Q. B.; S. C., Ewell's Lead. Cases, 677 n.), that although a single delusion which affects the testamentary act is sufficient to avoid it—an insane delusion which does not affect the testamentary act, and was not connected with it, does not avoid it.

Prof. Hammond remarked that a man may have a delusion which apparently did not affect the will—was not connected with it; but how do you know that? The mere fact of his believing himself to be George Washington would put him in the place, for the time being, of that illustrious personage, and he might do a great many acts connected with that situation; and if he should make a will, it should be set aside, for it may be shown that it started from a delusion as regards a matter of fact.

There are always secondary delusions in a man's mind, to which he does not give any vent. It is only the primary one that comes out. If a man, therefore, is shown to have had an insane delusion in regard to a matter of fact, that man ought to be rendered incapable of making a will.

MR. MAX F. ELLER

Had listened with great pleasure to the paper of the evening, and all the more pleasure because it demonstrates clearly the great influence which the advances of medical science, and especially those in reference to diseases of the mind, have had upon the rules of law as to testamentary capacity.

The influence has been of a restrictive kind. While formerly we have been apt to reject the will of a person supposed to be insane, who suffered from a delusion, or even from an illusion, yet we have lately arrived at that point where we inquire into the particular circumstances surrounding each case, and we have allowed the science of medicine to narrow the question down in the same degree that the medical side of the question has expanded. Not long since it was, so to speak, an epidemic to attack every will, and that epidemic, for that matter, is raging still. Because a man makes a will which is distasteful to some of the relatives and friends, therefore he must be insane. Now medical science has enlightened the legal pro-

fession. The physician is called in and testifies as to his knowledge of the diseases of the mind in relation to the case, and dispels that theory the same as the law has called in medical experts in cases of homicide where it was contended that because one man killed another, therefore he was insane. Because a murder was committed, that is, *eo ipso*, evidence that the man who committed the murder was suffering from temporary insanity. It is well known that there were a great many cases in which the defense was insanity on the ground of temporary aberration of the mind.

While it cannot be denied that there may be such cases, yet it would be equally dangerous to lay down a rule that because a man was a murderer, he must be suffering from temporary insanity, as to lay down a rule that because a man makes a provision in his will which does not coincide with the expectations and anticipations of his friends, or because it shows some eccentricities, or diverts the property from the channels which they expected, therefore he must be insane.

Such papers as this one by Mr. Rokenbaugh are an evidence of the appreciation which the legal and medical professions have of the importance of working together in these branches of their respective sciences, and thus the interests of both are advanced.

REMARKS OF JACOB F. MILLER, ESQ.

Mr. Miller arose not to discuss the paper of the evening, or to add to the interesting and instructive remarks which have been made respecting it, but rather to consider a question propounded by Dr. Hammond. He seems to think that if a person was partially insane his will should be invalidated. He asks, suppose a person thought himself to be George Washington, and while holding such an erroneous belief should make a will, how can you show that his belief that he was George Washington did not enter into and affect the provisions of his will? The law would put the case in exactly the opposite way. How can you show that his belief that he was George Washington did enter into his will? If the connection between his belief and the provisions of the will did not appear upon the face of the will, the presumption of law would be against such connection. What is not proved does not ex-

ist in law. The connection not appearing, the law presumes, in the absence of proof, that there is none. The burden of proof lies upon the person affirming such a connection to show it, and not upon the preferments of the will.

The view taken by Dr. Hammond was held by Lord Brougham, who maintained that the mind was one and indivisible: whatever affected it in one part affected it in all its parts, therefore a person insane upon any one subject, must be insane on every other, and hence incapacitated to make a will. This has not been, and is not now the American doctrine. Nor is it the English doctrine any longer. A few years ago the judgment of Lord Brougham was reviewed and held to be unsound. The doctrine of the law upon this subject is now the same in both countries. Now unless it can be shown that the unsoundness of mind in any one direction entered into the will and moulded its provisions, the will would stand.

MR. D. S. RIDDLE

Said that the old rule of the courts upon the testamentary capacity was that any person of however low degree of mental power could make a valid will. If he had mind enough to distinguish a man from a tree or a house from a horse he had testamentary capacity.

This rule was simply absurd. The Court of Appeals in this State formulated a new rule in the Parish will case, Judge Davies delivering the opinion. It is that the testator must have sufficient capacity to comprehend perfectly the condition of his property, his relations to the persons who were or should or might have been the objects of his bounty, and the scope and bearing of the provisions of his will. He must have sufficient active memory to collect in his mind, without prompting, the particulars or elements of the business to be transacted, and to hold them in his mind a sufficient length of time to perceive at least their obvious relations to each other, and to be able to form some rational judgment in relation to them.

He knew of no better rule or one more intelligible or practical upon the subject of testamentary capacity than this one.

Reviews and Bibliographical Notices.

I.—RECENT WORKS ON DISEASES OF THE NERVOUS SYSTEM.

I. Nervous Diseases; their Description and Treatment. By Allan McLane Hamilton, M. D., Fellow of the New York Academy of Medicine, etc. With fifty-three illustrations. Philadelphia: Henry C. Lea, 1878. 512 pages. Chicago: Jansen, McClurg & Co.

II. Diseases of the Nervous System; their Prevalence and Pathology. By Julius Althaus, M. D., M. R. C. P., London, Senior Physician to the Hospital for Epilepsy and Paralysis, Regent's Park, etc., etc. New York: G. P. Putnam's Sons, 1878. 366 pages, 8vo. Chicago: Jansen, McClurg & Co.

III. Lectures on Diseases of the Nervous System, Delivered at Guy's Hospital. By Samuel Wilks, M. D., F. R. S. Philadelphia: Lindsay & Blakiston, 1878. 472 pages. Chicago: Jansen, McClurg & Co.

For a long time, the works of writers like Marshall Hall, Romberg, Ollivier and Calmeil, exhausted the list of important publications in regard to diseases of the nervous system. But few practitioners, however, met with these works, contenting themselves with the meagre and often confused chapters on nervous diseases, found in manuals on the practice of medicine and surgery. As time passed on, other works were published, such as those of Brown-Séquard, Sandras, Trousseau, Schroeder Van Der Kolk, Hasse, Leubuscher, Benedikt, Rosenthal, Handfield Jones, the composite volume on nervous diseases in Reynolds' "System of Medicine," Gintrac, Hammond, etc., some of them of great value, until, at this present moment, the interest of the profession is deeply aroused in regard to the nervous system and its diseases, if one may infer anything from the phenomenal supply of new editions of older, and the rapid supply of new works in this important domain of medicine. Quite recently we have the excellent manual of Rosenthal, now translated into the French language, with an introduction from Prof. Charcot, Eulenberg's admirable work, and that of Hammond, in this country, and the really remarkable volumes composing part of Ziemssen's *Handbuch der Spec. Pathologie u. Therapie*, and the separate works of such writers as Erb, Huguenin, Nothnagel,

Charcot and his pupils, Vulpian, Poincarè, and a host of others in different languages, in regard to various aspects of a study of the nervous system, in health and disease. Finally, among the latest productions in this prolific field, are the three works whose titles stand at the head of this notice. They are all brief, and by authors of more or less repute in neurological medicine.

The real justification for the production of a new book, on, let us say, *Nervous diseases*, must lie either in something new or valuable in matter, or the method of treatment of such subjects. A book which can claim nothing on either score, must be superfluous, to say the least.

The principal object Dr. Hamilton seems to aim at, in the production of his work, is stated in this extract from the preface. He says: "Should the satisfaction be ever accorded me of knowing that I have made the subjects of diagnosis and treatment of nervous diseases *more simple* to my readers than they now are, I will be amply rewarded for the task I have undertaken." By this standard, therefore, perhaps, should the work be judged. It is divided into eighteen chapters, beginning with "Diseases of the Cerebral Meninges," working out from thence to "Diseases of the Peripheral Nerves." Now, what has been done by Dr. Hamilton to render "more simple," that is, more clear and comprehensible than former writers have done, the "Diagnosis and Treatment of nervous diseases?"

One of the chief requisites on the part of an author, in such a case as this, is the power of *natural, clear analysis*. This insures that things apparently unlike, but really alike, however widely separated from each other by false or artificial methods, may be grouped under their natural heads, while phenomena, apparently alike, but really unlike, may be placed in their proper relations, in a truly simple and philosophical scheme, so far as the present imperfect state of science renders such a scheme possible. Healthy, natural analysis, and hand in hand with it, simple, judicious classification of the sum total of the accredited data of a science, is indispensable in the attempt to render knowledge "simple."

This quality is certainly lacking in Dr. Hamilton's book. Thus, for example, he says, on the first page, "One of the greatest misfortunes that can happen to the student, is the possession of a large accumulation of *badly-arranged facts*, which are stored away in the brain like odds and ends in a garret." This sounds well. But immediately afterwards, on the same page, the author, unconsciously to himself, of course, shows how slight a practical hold on his mind such excellent cautions have, when he lays down a "plan which will enable us to avoid confusion" in the clinical examination of a patient. The chief distinct heads in the plan are as follows: "Examination of the patient," "Symptomatology," "Tremor," "Inco-ordination of the upper and lower extremities," "Vertigo," "Sensation," "Psychical disorder," "Miscellaneous," "Exciting cause," "Diagnosis," "Treatment." But what principle of analysis could have guided the writer when he separates

any of the other groups from the first one, or "Examination of the patient," or why set up this first head at all? Are "tremor," "inco-ordination," "vertigo," etc., not symptoms? If they are, why exclude them from the category "symptomatology?" Under this latter head, certain disorders of "motility" are embraced, but no reasons appear why symptoms in the sensory, psychical, or other spheres of nervous action are not included under this comprehensive head. Under the category "motility," many of its plain disorders are omitted to be mentioned elsewhere, such as "tremor," "vertigo," "inco-ordination," etc., under separate heads, while under the head of "*Disorders of the Special Senses*," we have included such at least partial affections of "motility" as "nystagmus," "strabismus," "conjugate deviation," "ptosis," "diplopia," "ataxia," "clumsy speech," and, under this same head, complex, ill-defined, states such as "discharge," "aphasia," and "loss of speech." On what intelligible principle many of these affections could be excluded from the category of disorders of "motility," and subordinated to those of the "special senses," we are utterly at a loss to determine. The author says, "This list, though imperfect, will, I think, enable the observer to pursue *a systematic course* in examining his patient." But we must beg leave to differ from him. The one who adopts this list as a guide in making clinical examinations, may "get over the ground," but in a very *un*-systematic way.

Then, again, turn to page 114, and find apparently under the head of *subjective* symptoms, "vomiting," and among *objective* symptoms, "hallucinations of hearing," "visual hallucinations," "delusions," "sensations as if falling through the bed," etc. Such kind of mechanical, rather than logical and scientific analysis, rather plainly shows that the author has in some measure fallen into the "misfortune," as he himself very properly calls it, of possessing a "store of badly arranged facts." A comparative lack of fine analytic discrimination, is one of the most marked features of the book, for it prevails consistently from beginning to end.

Besides looseness in analysis, there are evidences of undue haste, or lack of care on the part of the author, in preparing his work, and in conducting it through the press. Thus we find too many well known proper names mis-spelled, such as "Dieters," for "Deiters;" "Northnagle," for "Nothnagel;" "Gastrowitz," for "Jastrowitz;" "Vesseyer," for "Veyssiere;" "Carvaille," for "Carville;" "Kausmall," for "Kussmaul;" "Parnum," for "Panum;" "Ammidown," for "Amidon;" "Camleil," for "Calmeil;" "Volekmann," for "Volkmann;" "Dumaschino," for "Damaschino;" "Corneil," for "Cornil;" "Desjerine," for "Dejerine;" "Subetski," for "Lubelski," and many others.

Then, again, there are various inaccurate, or vague statements, such as the following: "important *sensory* ganglia at the base of the brain, such as the *corpus striatum*" (p. 85). Or in the following extracts from the beginning of the chapter on cerebral

hyperæmia: "Two forms of cerebral hyperæmia have been recognized by the majority of medical writers, one of them which is *active*, and connected with *forcible* arterial fluxion." What can be the real meaning of the word "forcible," in this connection? On the same page, in speaking of the two kinds of hyperæmia, active and passive—or, as Dr. Hamilton prefers to call them, sthenic and asthenic—he says, "these differ only in their manner of appearance. In one they are early and sthenic expressions, *and are produced by rapidly exerted and violent force*, and in the other their advent is more slow, as they appear to be produced *by a sluggish force or tardy impairment of cell function*," etc. This is certainly vague and loose writing.

There is also traceable through the book, a harmless vein of assumption, and we had almost said presumption, which is somewhat difficult to characterize, but can be best appreciated from an example: He says in a note (p. 285), "I have no doubt *some of my American readers* have witnessed the performance of a tight-rope walker," etc. The implications in this note may be fairly regarded as not only uncalled-for, but in a sense invidious and disparaging. Not only so, but the author deploys something more than his hope—an expectation—that he is to have a circle of foreign readers, and we earnestly hope he may have it. Candid American readers will possibly acknowledge their inferiority in point of knowledge to foreign ones, but will probably not relish gratuitous reminders of the same. Such a tone as is revealed in this passage, however much it misrepresents the conscious spirit of the author, will render his work in some degree distasteful to discriminating readers.

The discussions of nervous disease are by no means as full, nor so judicious, as we had expected in view of the practical aim of the work, and the pictures of disease are not drawn after the best models. Diagnostic parallels are not always clear enough to be of service to the physician, and fall far below those presented in many other accessible works. In practical matters, the author is often indecisive and too general, notwithstanding the array of prescriptions found at the end of the volume. The work is especially defective in the portions devoted to "pathology." Hardly a disease is discussed, as regards pathology, with the degree of clearness and decision rendered possible by the present state of knowledge. Notwithstanding quite a range of literary references, the author would appear not to have availed himself of some of the most valuable contributions to neurological literature. As an example, we find no reference is made to Leyden's remarkable work on the spinal cord, with which the author must be acquainted, though he gives no signs of it. Throughout, evidence is afforded of a want of careful, if not of wide reading, and of mature thought. But for all this, we have a most sincere respect for the aim and labor of the author, and would not take our leave of the volume without expressing our belief that, even in the midst of better works on the same

subject, it will be useful to the general practitioner, for while it contains nothing striking or novel, and is apparently not written in full view of existing knowledge, yet it abounds in practical matter of value within moderate limits, and in presenting which, the publishers have left nothing on their part to be desired.

The work of Dr. Althaus, the second on our list, is novel both in its plan and execution. It cannot be considered a text-book, for while it gives the pathology and symptomatology of the various diseases, a large class of nervous affections are entirely unconsidered, and nothing whatever is said of the treatment of any of them. The author has adopted a rather peculiar plan of analyzing statistics to obtain data as to the prevalence of nervous diseases, their comparative frequency in races, sexes, and regions. As he says in his brief preface, he has "endeavored to elucidate the part played by diseases of the nervous system in national pathology, and to show the laws to which their occurrence and fatality are subject." And he says, in effect, in his opening remarks, that his purpose has been to arrive at definite conclusions in regard to such questions as the following: "Do nervous diseases occur in a certain definite proportion, or do they vary perceptibly from time to time? Are paralysis and insanity really more frequent nowadays than formerly? Are large towns more suitable for their development than the country? Is the Anglo-Saxon race more liable to their invasion than other races?" The general interest in these questions, and the fact that this is almost, if not quite, the only systematic attempt of the kind at their solution, give to his work a special importance, and will insure it attention that it might not otherwise obtain.

In pursuance of his task of answering these questions, Dr. Althaus has carefully analyzed the annual mortality reports of the Registrar General of England and Wales. As he himself states, it is the reports on *disease* and death, but the first very important particulars are wanting, and the conclusions are derived entirely from the mortality statistics. That these cannot fully serve to show the prevalence of nervous disease is, we think, sufficiently evident, and this is the principal general criticism we have to make on our author's method. The very large class of nervous affections that never, or, at least, very rarely appear as causes of death in mortality reports are, necessarily, left out of the account, yet they make up a very large proportion of the general division of nervous diseases, and in estimating the prevalence of the latter the results are to a very great extent vitiated by this deficiency. Neurasthenia with all its complications, and hysteria with its manifold symptoms, may be increasingly prevalent, and yet neither of them make any figure in such statistics as are usually kept, and we have considerable doubt as to the value of any estimates of the frequency of such diseases as epilepsy and chorea, based on the mortality returns of any country, no matter how carefully and accurately they have

been compiled. Indeed, it is not, in our opinion, going any too far to say that, with the exception of a few almost universally fatal affections like hydrophobia and one or two others, this plan of estimating their prevalence by mortality statistics must necessarily be inaccurate. The preliminary clinical investigations to accurately determine the ratio of fatality to the whole number of cases are wanting in the case of nearly every nervous disease, to say nothing of the unskillfulness of the average practitioner in the diagnosis of these diseases, and the unscientific terminology which is even now prevalent and to a certain extent recognized in the author's work. To illustrate this point we may use a quotation from Dr. Althaus himself, a passage indeed, in which he speaks of the advantages of his method. After mentioning the uncertainty that prevails in regard to these questions, and declaring that it is mainly due to the limited clinical field from which most authors have derived their statistics, he says: "Even where such researches have extended over a series of years, erroneous conclusions have been arrived at. Thus, M. Falret, for many years physician to La Salpétrière, ascertained that of 2,297 cases of apoplexy, 1,660 occurred in males, and only 637 in females; and Dr. Hammond, of New York, found the proportion to be 153 male and 76 female cases in a total of 229. These numbers have led those authors to confidently express the opinion that apoplexy, or cerebral hemorrhage, is more common amongst men than amongst women, and yet it is shown by my researches, which comprise not 229, as Dr. Hammond's, or 2,000, like M. Falret's, but nearly a quarter of a million cases, that women, in England at least, are rather more liable to die of apoplexy than men, and this not only for a year or two, but absolutely in the whole period over which the investigation extends. Unless we, therefore, assume that deaths from apoplexy occur in France and the United States in a very different proportion from what they do in England, we are led to the conclusion that the two physicians just named, accidentally happened to come across more cases of males than females in the limited field of observation which was at their disposal; and that their deductions must therefore be devoid of value." The principal misapprehension of Dr. Althaus in this passage is that M. Falret and Dr. Hammond, either lost all their cases, or were using only mortuary statistics like himself. Moreover, when we consider the loose usage of the term apoplexy (which ought, in our opinion, to be expunged from our nosology) in death returns—when we consider that it includes the most opposite pathological conditions, hyperæmia, anæmia, blood poisoning, etc., to say nothing of other mistakes in diagnosis of the numerous reporters, we are inclined to consider even the limited clinical experience of one accurate observer of extensive opportunities, to be of more value in the positive decision of such questions than whole volumes of mere statistics of mortality loosely reported, and showing only, at the best, a fraction of the cases that actually occur. We are cer-

tainly very far from considering the deductions of such men as Falret and Hammond as "devoid of value."

Apart from this persistent confounding of clinical with mortuary statistics, Dr. Althaus has furnished in this work some very useful discussions, and formulated his results in a series of propositions which we cannot do better than reproduce here, *verbatim*. They need, of course, to be accepted with the mental reservation that they apply only to fatal cases, the proportion of which to the whole number occurring is far from being satisfactorily ascertained in the majority of these diseases. They are as follows :

I. The rate at which diseases of the nervous system prove fatal to the population of this country is a steady one, and subject to a definite law, to which there are not any or only apparent exceptions. This rate does not appear to vary perceptibly from time to time, and amounts to about twelve per cent. of the entire mortality from all causes.

II. Diseases of the nervous system occupy the fourth rank amongst the maladies destructive of human life, being only surpassed in fatality by zymotic, tubercular, and respiratory diseases.

III. Nervous diseases are not, as is commonly asserted, more frequent, but, on the contrary, less numerous in large towns than in the country, and it is probable that their occurrence is largely influenced by race.

IV. Sex has a powerful influence on the production of nervous diseases; for although in this country the population of females exceeds that of males, the deaths of males from nervous affections preponderate constantly over those of females, the male death-rate being 12.94 and the female 11.62 per cent.

V. Age has even a more powerful influence on the production of nervous diseases than sex; for these maladies attain an immense maximum in the first year of life, owing to the great prevalence of infantile convulsions. They are much less frequent in youth and middle age, and attain a second maximum in old age, that is, after seventy, owing to the prevalence of apoplexy and paralysis; but the second maximum amounts to only about the tenth part of the first maximum attained during infant life.

The first two of these propositions are legitimate deductions from the data employed, but as much cannot be said of the rest, for the reasons already given. The very insufficient nature of the classification adopted by the Registrar General, the defects of which are remarked by our author himself, ought to show how little reliance is sometimes to be placed on these, even as merely mortuary statistics: the confusion would be still worse confounded if they had a larger scope. The only satisfaction is in the hope that as many errors are made in one direction as in another, and that the fatal law of averages brings the results out about right. The error we charge the author with here, is in trying to extend the application of his results and deduce the facts as to the

prevalence of nervous diseases in general from the analysis of mortality reports, which necessarily cannot include the non-fatal cases, which by far constitute the majority of the whole. The error goes through the whole volume, and we shall notice it again in a few places, when speaking of Dr. Althaus' remarks on certain special disorders.

A chapter on general nervous physiology, which immediately follows the introductory one, is, on the whole, very good, though brief. The author's position in regard to the question of the functions of the brain, in regard to which there is so much dispute, seems to us the correct one, and his remarks in regard to the value of the older and contradictory observations, on which Dr. Brown-Séquard has put so much stress, are eminently judicious. He follows Ferrier, in the main, in his statements on this subject, though noticing briefly the views of other authors.

In the succeeding chapters on special pathology, Dr. Althaus follows, according to the plan of his book, the classification of the British Registrar General, including, under such heads as convulsions, apoplexy, paralysis, and even under so vague a term as "disease," a very large proportion of the affections of the nervous system. He, however, analyzes these pathologically, as he proceeds, and this part of his work appears to be, on the whole, well up to the present state of neurological science; at least, so far as the pathology and symptoms of the various disorders are concerned. Dr. Althaus is, in the main, a safe pathologist, and his remarks, though brief, appear to be generally in accordance with what seem to us to be the correct views. He sometimes groups, however, a number of clinically or anatomically distinct diseases, in a few general remarks, under some one comprehensive head; thus we find no mention whatever of several spinal diseases, which are all perhaps included under the one head of myelitis, and none of the usually recognized varieties of insanity are noticed separately from the brief treatment of the general subject, except general paralysis, which indeed is perhaps the only well anatomically defined species of the whole class. The scope of the book, however, is not such as to render it in any way exhaustive in this respect.

The statistical analysis runs through all the work. The plan is followed of giving, in tabular form, at the beginning of the remarks on each disease, or class of diseases, recognized in the Registrar General's reports, a comparative statement of the percentages of deaths, males and females, through a series of years, and a diagram illustrating the mortality curve at different ages of the individual. It is the deductions from these analyses that call most for criticism, for it is in these that the author most exhibits his incautious generalizations and hasty conclusions. A few instances will suffice to indicate what is alluded to. We would refer to such passages as that on page 86, where, after comparison of Dr. Lidell's statistics of apoplexy in New York city with the English registration reports, the author says: "It

is, however, to be remarked that, while for New York the difference between the seventh and eighth decennium is considerable, it is very slight for England and Wales, which probably means that longevity is greater in England than in the States." The only comment we are inclined to make on this passage is, that such a conclusion, drawn as it is from a comparison of a limited number of cases in the single city of New York, and extending over three years, with those of the whole of England and Wales for a single year, seems altogether gratuitous. Of the same character is the presumption, based on the comparative number of deaths from delirium tremens in London and in Wales, that the use of alcoholic drinks may secure immunity from nervous disorders; an utterly inconsequent conclusion. We have not the space to go into details; though much more might be said in regard to these and other passages to which we might take exceptions; the instances we have given are sufficient to point our general remarks.

To recapitulate our objections to Dr. Althaus' work: He has endeavored by the analysis of mere mortality statistics, compiled according to an imperfect classification, and subject to all the errors that can arise from this, as well as the confusion of terms and faulty observation and diagnosis of unscientific practitioners, to decide as to the prevalence, both actual and comparative, of nervous diseases, their variations in regions and races, apparently ignoring all the time the fact that mortality statistics are not clinical statistics, and that the relation between the two is very far from being so definite, in the case of the large class of nervous disorders, that we can determine the prevalence of nervous diseases by the number of deaths attributed to them. A very large number of these disorders never cause death, and some even of the more formidable ones are so seldom its direct cause, that they play no proportionate part in registration statistics. Dr. Althaus' third, fourth and fifth propositions, are therefore not legitimate deductions; his facts were too few and imperfect to warrant them.

On the other hand, it must be admitted that he has produced a work interesting in its style and subject, novel in method, and well worthy of a place in the physician's library; and as a discussion of statistics, it is, with all its drawbacks, a real contribution to medical literature.

Dr. Wilks' work is of a different class from either of the others. Though in the form of lectures, which seems to us in some respects, perhaps, less advantageous in a text-book, and entirely without illustrations, it creates a very favorable impression upon careful perusal. Dr. Wilks seems to be a very well informed man, but also a thoroughly practical one, and his lectures give evidence that he is an able clinical teacher. In this book he treats of all, or very nearly all, the disorders generally classed as nervous diseases, and one or two like cerebro-spinal meningitis and

metallic paralyses, which are not always included in this category. These are treated throughout in a practical, sensible way, and illustrated by cases, generally very briefly, but, on the whole, satisfactorily reported. To these he has added one or two clinical varieties not usually recognized as separate forms of disease, at least in this country, such, for example, as alcoholic paraplegia, and, indeed, we may say that very little is omitted. One notable omission, however, occurs. No mention is made of Grave's disease or exophthalmic goitre. He is not strong in his discussions of the pathology of the various affections of the nervous system, but is usually on the safe side, and very ready to admit ignorance in many cases. This, in fact, may perhaps be carried a little too far by our author. The physiological statements are judicious, as far as they go, and he is, we judge, a very safe authority for the student to follow in this regard. As to the classification of nervous diseases, no very ambitious attempt is made—no special remarks at all are given to this subject. Dr. Wilks retains, in his nomenclature, a few old-fashioned, and, we think, objectionable terms, such as apoplexy and arachnitis. Of the first of these we have already spoken in our remarks on Dr. Althaus' work, and we need say no more here; the second is objectionable, as conveying a false and obsolete pathological idea, which fact, however, our author does not appear to recognize. The therapeutics of the different disorders is sometimes, it seems to us, a little slighted; there appears to be an undue skepticism now and then; thus, at the close of the book, in some pages devoted to some general remarks on remedies, opinions are expressed in regard to the value of opium, belladonna and strychnia, in nervous affections, with which, especially as regards the last named remedy, we certainly cannot agree.

In conclusion, we can cordially recommend this work as a convenient, practical and sensible text-book, which can be safely taken as a guide by the student.

II.—ADAMKIEWICZ: THE SWEAT NERVES.

DIE SECRETION DES SCHWEISSES—Eine bilateral-symmmetrische Nervenfunction. Nach Untersuchungen am Menschen und an Thieren. Dargestellt von Dr. Albert Adamkiewicz. Berlin, 1878. (*The Secretion of Sweat, etc.*)

Among the different animal secretions some are due solely to filtration of the fluid portion of the blood through the tissue of the glands; such are the secretion of urine, bile and milk. Other secretions, however, depend on an activity of the living gland-tissue, which may be excited by stimulation of its nerves, and which will continue to act even after cessation of the circulation. So far only the salivary and lachrymal glands have been proven to belong to the latter category. The researches of our author have now included the sweat glands as well in this class. (Independently of Adamkiewicz, Luchsinger has arrived at the same results at the same time.)

Proceeding from the common experience, that muscular movements are followed by sweating, he galvanized various motor nerves in man. As a result, sweat drops appeared at once on the corresponding portion of the skin, and continued to be formed for a short time after interrupting the stimulation. The sweating region is the portion of the skin over the contracting muscles, but this is always accompanied by sweating of the symmetrical spot on the other side of the body. The perspiration is independent of the circulation, as it occurs equally intensely, and just as soon after compression of the main artery, for instance, the femoral. It can be induced as well by direct galvanization of the muscles as by their *voluntary* contraction. It must be mentioned, however, that Adamkiewicz selected as subjects persons with a disposition to sweat.

To obtain a reflex perspiration he made use of the faradic brush. Its application is speedily followed by sweating of the irritated spot and its mate on the other side. In persons predisposed to sweating, the perspiration, however, is induced soon in all those parts which sweat most readily, normally, such as the palms and soles. This result he expresses in the law, "that the secretion of sweat occurs in man always symmetrically on both sides, and independently of the place of stimulation." The same law holds good when thermic stimulation is resorted to. The contact of a substance some 6–8° C. warmer than the body produces bilateral sweating on those spots most liable to perspiration, such as the soles of the feet, even when the warm body is applied in another locality. A cold substance, on the other hand, will not induce, but rather check, perspiration. Cold, however, is a powerful stimulus to unstriated muscles, and the contraction of the fibres surrounding the sweat glands can force out their contents.

Extending his experiments to animals, the author showed that in cats the paws will sweat on stimulating the sciatic nerve, even after death or removal of the member, which proves the independence of perspiration from the circulation. On stimulating the undivided nerve, a very slight sweating is also noticeable on the paw of the other side. Evidently the tendency to symmetrical bilateral sweating is less developed in the cat than in man. The sweat nerves retain their irritability longer than the motor nerves; at a time after death, when stimulation of the sciatic nerves has ceased to produce muscular contraction, the appearance of drops of sweat is still often noticeable. The perspiration of the head and neck caused by section of the sympathetic nerve in the horse has usually been attributed to paralysis of that nerve. It ceases, however, about six hours after the operation, and does not return again. There is hence more cause to attribute it to the traumatic irritation of the sympathetic nerve.

As regards the sweat centres in the cord, Adamkiewicz found that perspiration of the hind paws can continue after division of the spinal cord at the level of the first lumbar vertebra. Removal of the lumbar cord, however, prohibits all further spontaneous sweating of the hind paws. After section of the cord at the level of the first lumbar vertebra stimulation of the central end of the divided sciatic nerve causes a reflex sweating of the other leg. On shortening the lumbar cord by successive slices, it was found that the spinal centres of the sweat nerves of the hind paws are situated in the portion between the first and fourth lumbar vertebræ. This coincides with the centres of the motor nerves of the leg; similarly it was found that the centres of sweat nerves and of the motor nerves of the anterior extremity coincided in their position. The sweat nerves leave the cord with the anterior roots, since reflex perspiration can still be obtained after division of all posterior roots of the lumbar fragment. Histologically we are to infer that the sweat centres exist in the anterior cornua. This supposition was confirmed by a case of poliomyelitis anterior acuta, in which the motor paresis was accompanied by paralysis of the sweat nerves.

After removal of the lumbar fragment of the cord, stimulation of the brachial plexus will still produce sweating of the hind paws. This is due to the influence of fibres joining the sciatic nerve, and coming from the sympathetic. Their origin is found in that portion of the cord included between the tenth and thirteenth dorsal vertebræ (of the cat).

Finally, it was shown that the common centre of all sweat nerves exists in the medulla oblongata.

Thus far the experimental part of this work, the results of which are in unison with the independent, though contemporaneous researches of Luchsinger. The remainder of the *brochure* is devoted to some reflections on the bilateral symmetry in the innervation of sweat glands and muscles. H. G.

SHORTER NOTICES.

I. Physics of Infectious Diseases. Comprehending a Discussion of certain Physical Phenomena in connection with the Acute Infectious Diseases. By C. A. Logan, A. M., M. D. Chicago: Jansen, McClurg & Co., 1878. 212 pages.

II. A Manual of Operative Surgery. By Lewis A. Stimson, B. A., M. D. With three hundred and thirty-two Illustrations. Philadelphia: Henry C. Lea, 1878. 477 pages. Chicago: Jansen, McClurg & Co.

III. Handbook of Ophthalmology. By Prof. C. Schweigger, of the University of Berlin. Translated from the Third German Edition by Porter Farley, M. D. With Diagrams and other Illustrations. Philadelphia: J. B. Lippincott & Co., 1878. 555 pages. Chicago: Jansen, McClurg & Co.

IV. Congenital Occlusion and Dilatation of Lymph-Channels. By Samuel C. Busey, M. D. New York: Wm. Wood & Co., 1878. 187 pages. Chicago: Jansen, McClurg & Co.

V. A Manual of Nursing. Prepared for the Training School for Nurses, attached to Bellevue Hospital. New York: G. P. Putnam's Sons, 1878. 143 pages. Chicago: Jansen, McClurg & Co.

VI. Sixth Annual Report of the Secretary of State of the State of Michigan, Relating to the Registry and Return of Births, Marriages and Deaths for the year 1872. By authority. Lansing, 1877. 464 pages.

VII. Algesimetrie. Eine neue einfache Methode zur Pruefung der Hautsensibilitaet, von Dr. Friedrich Björnstrom. Upsala, 1877. (*Algesimetry. A new simple method for testing the cutaneous sensibility, etc.*)

1. The author of this work, during an extended residence in an official capacity in South America (as U. S. Minister to the Republic of Chili), had his attention called to certain peculiar phenomena of disease in that region; a complete or partial immunity from some of the infectious diseases so deadly elsewhere, and a modified condition of others. He was led to connect these facts with certain other facts of the physical features of the country, and some generally observed analogous phenomena in other parts of the world, and by a course of reasoning on these, he arrives at the following generalizations as to the acute infectious diseases:

First. "That each one of the class possesses an original habitat or indigenous locality.

Second. "Then when one of the type is developed in a locality foreign to the disease, it occurs from a transportation, not of the original cause, but of the representative results of the disease.

Third. "That the original producing agent may be of animal or vegetable character, coming immediately from animalcular or micro-phytic forms, or directly or indirectly from the lower animal creation; and it would appear not impossible, that a contagion of specific character might be wholly inorganic in constitution.

Fourth. "That the contagion of a type disease in man, does not consist of vegetable germs, but is a product of a preceding type-action, and is representative of that action in its atomic construction, and possesses the property, under vital and physical laws, of inducing the typical action in a healthy system.

Fifth. "The atmospheric aridity to the extent existing upon the South Pacific coast, does not destroy phyto-germ life, and hence cannot be the cause of the exemption of that coast from the acute infectious diseases; nor can it be said to decompose either animal or vegetable germs; but, upon the contrary, is rather preservative of them.

Sixth. "That violent electric energy, as an agent powerfully operative in inducing energic transmutation, directly decomposes the infectious molecule by chemical rearrangement or breaking up of its atoms."

These conclusions may or may not be accepted by our readers; we cannot ourselves fully appreciate all of them or follow the author in all his reasoning which leads to them. The facts and the observations in medical geography on which they are based are suggestive, however, and we hope that Dr. Logan will give some time more fully their details. Some criticism might be made of the author's physical theories and of his style, which appears to us sometimes rather strained. But we leave the work in these respects to the judgment of the individual reader.

II. This is, it seems to us, a very excellent condensed manual of operative surgery, and will be, we think, a safe and convenient guide, not superseding, but supplementing the larger works on the same subject. The text is made very easy to understand by the profuseness of the illustrations. We predict for it a very extensive circulation in the profession.

III. An American translation of a work by so respectable an authority as Prof. Schweigger, which has already attained its third edition in its own country, must, of necessity, be an addition to our medical literature, provided that the translator has done his work well. In the present instance, this appears, with the examination we have been able to give it, to be very fairly done. To those who desire a convenient, scientific manual of ophthalmology, well up to the more recent advances in the department, we can safely recommend its purchase.

IV. The readers of the *American Journal of Obstetrics* will be familiar with most of the illustrations and text of the present volume. It is, in fact, a republication of a rather notable series of papers in that journal. The author having had a well-marked case of congenital lymphangiectic elephantiasis in his practice, was led to look up the subject and utilize, as few Washington physicians appear to do, the opportunities afforded by the National Medical Library, and has collected in medical literature nearly ninety similar and allied cases, which he here discusses in this profusely illustrated monograph. The whole he classes under the general head of dilatation and occlusion of lymph-channels. There is not a great deal that is original in the volume, nor is there any pretense of it, but the author has shown a very commendable industry, and has made a very useful compilation.

V. This little volume will find a welcome in many quarters, and, we doubt not, supply a great need. It will be useful to the general practitioners, many of whom, we have a suspicion, might find in its pages some practical hints that would be information to them. On the other hand, the general circulation of such a book as this among the laity would be of the greatest advantage. We might say with more truth than it is usually said that "no well-regulated family can afford to be without it."

VI. Michigan is almost, if not quite, exceptional among the Northwestern States in having a bureau of vital statistics, which publishes elaborate annual reports. The very respectable volume before us seems to leave little to be asked for from its able compiler, Dr. H. B. Baker. He has very fully worked up the various statistics of births, marriages, and deaths, for the State of Michigan for the year 1872, giving very numerous and elaborate tables, and diagrams illustrating the curves of births, deaths and marriages at different periods, with full discussions of the same. Some of these, such as those on the relative birth-rate among foreign and native-born residents are of interest in connection with statements that have been made in regard to this point by Eastern medical statisticians.

We see in his tables of deaths, classified according to causes, that the number of those from nervous disorders exceed in number those from any other class, except those that are properly called zymotic, a proportion somewhat unusual we think. As in all registration statistics, the classification might, and ought to be, improved.

Altogether, this volume is a credit to the State under whose auspices it appears, and to the compiler, Dr. Baker. We hope that we may in the near future see equally valuable contributions from other States, which at present furnish none.

VII. Recognizing the various errors incident to the previous methods of æsthesiometry, the author presents us with two new means of testing the tactile sense. With E. H. Weber and the

later authors he divides the tactile sense into (1) sense of pressure, (2) sense of temperature, and (3) sense of space, either of which is distinct from pain. The ordinary (Sieveking's) æsthesiometer measures only the third variety, while Björnstrom's new instrument shows plainly the border line between the sense of pressure and pain. It may be described as a broad forceps, between the branches of which a fold of skin is pinched, while the force with which it contracts is shown by an index. The force, just sufficient to be recognized as pain beyond mere pressure is registered in kilogrammes. It was thus found that a pressure of one-half to one (kilogrammes) is just felt painfully by a fold of skin from the infraorbital region. As a scale of sensibility we should mention the following regions: the forehead, temporal region, lips, skin of face, eyelids, auricles, the latter perceiving as pain only a pressure of five kilogrammes or more. Without following his details, suffice it to state, that B. found the trunk and extremities no more and in general less sensitive than the skin of the face. The skin from the region of the elbow was found least sensitive, reacting but to a pressure of twelve to fifteen kilos. Considerable variations were found in different persons. It seems to us however, that a method which detects no differences finer than one-half to one pound of pressure, and the range of which does not exceed thirty pounds, is comparatively crude. Still B. claims to have detected with it unilateral alterations of sensibility in a number of morbid processes, when other methods failed in showing any difference between the two sides. But the large number of cases of disease adduced possesses only casuistic interest.

A second method described by the author is intended to register the rapidity with which a tactile or painful sensation is perceived: A T-shaped rubber tube is connected with a Marey's polygraph. One branch of the tube is compressed by a needle, when the latter is forced into the skin, while the patient is to bite on the other tube when the prick is felt. The two successive movements communicated to the air in and the membrane of the polygraphic drum are registered on a cylinder revolving with a known velocity. Several cases of retardation of sensibility from disease of the central nervous system are cited as proof of the efficiency of the method. H. G.

Editorial Department.

IN the July number of the *Journal of Mental Science* is a rather extended, and upon the whole favorable, notice of an article which appeared in the April number of this journal, by Dr. Spitzka, of New York, entitled "Reform in the Scientific Study of Psychiatry." In the course of the above mentioned notice it is said, "the ability of the article is unquestionable, and its vigor almost excessive, but its personalities and spirit are certainly not becoming in one member of a profession, towards other members of the same profession, the aims of many of whom are no doubt as high, and their conduct as honest, as his own. It is certainly a pity that the mode of American political vituperation and its intemperance of language should be allowed entrance into the literature of the mild and merciful profession of medicine." And yet it is said, on the same page, that "there is much truth in what Dr. Spitzka says, and we think our American brethren would do well to take heed to this and many other indications that a more liberal and open mode of conducting their asylums, and managing their Association, is required."

But Dr. Spitzka himself admits his language as harsh, to say the least. He says, "In dealing with the flagrant and obvious short-comings of medical officers, I have employed terms which it is customary to censure as being unnecessarily harsh. . . . When I have used the terms, ignorance, charlatanism, insincerity and neglect, I have employed them because no other words could characterize so aptly the condition to which I found it necessary to allude in the course of this enquiry. I have used these terms deliberately, and shall present, if challenged, the detailed proof, which I have hitherto not given, as it would have necessitated that *mention of names* which it has

been my chief purpose to avoid." This extract shows not only that the author was not insensible to the view which might be fairly taken, by dispassionate readers, of his hot and vigorous criticisms—which we believe to have been substantially deserved—but it shows also in part his reasons for adopting the style which in some measure characterizes his paper. Our principal objects in writing these lines, are to join in condemning as a rule all personalities and vituperative language, especially in medical writings, and also to express the opinion, that to describe Dr. Spitzka's paper as *characterized* by personalities and by the employment of the vituperative language of American or other politics, is to do him, if not ourselves, as the publishers of his paper, a certain measure of injustice. It contains really but few personalities, being directed chiefly against the shortcomings of a class, rather than separate individuals. Moreover, while the language is hot, and at times even intemperate, it cannot be fairly spoken of as vituperative. It is a style of writing more common we believe in Germany than in America, which former country we believe the *Journal of Mental Science* to be in error in giving as the fatherland of our contributor. But the final object we have had in writing this has been, to call attention to an instance in which personalities and vituperation reach a hight hitherto unknown in respectable medical literature in this country. We now refer to the paper by Dr. Grissom, of North Carolina, on "True and False Experts," before the "Association of Superintendents of Hospitals for the Insane," at its annual meeting at Washington, since published entire in the *American Journal of Insanity*. Many of our readers must have seen this remarkable paper, for a reprint has been rather widely circulated.

It contains an attack of the most personal and offensive description upon one of the associate editors of this journal, against which attack one of the members of the Association (Dr. Wallace), in a speech remarkable for manliness and vigor, said, "for one I claim the right to protest against it with all the emphasis of which I am capable." In the conclusion of his speech against the personalities of Dr. Grissom, he says, "I beg to conclude with a resolution, *to which I do not expect a second, and seconded, not a single vote, except my own:*

"*Resolved*, That the members of the Association of Superintendents of American Institutions for the Insane, listened to the personal attack upon a professional brother of New York, by one of their number, Dr. Eugene Grissom, of North Carolina, with unmixed regret, as out of place, and as calculated to do no good, but, on the other hand, to stir up strife and confusion, and therefore contrary to the spirit of science and the benevolent, humane work, in which they are engaged."

So far as the record goes to show, the prediction of the author of the resolution was fulfilled, in that his resolution was not seconded, and did not receive a single vote, but, on the contrary, it is said, outside of the record, to have been received with marked approval, the motion above quoted was promptly laid on the table, and the offensive paper is now published as the leading article in the *Journal of Insanity*, without a word of editorial comment.

It must be remembered, that the article of Dr. Spitzka was directed as a criticism, not so much against persons, as against the character of their scientific labors, and the alleged mismanagement of great *public institutions* by *public officers* of the same. But this attack of Dr. Grissom on Dr. Hammond, is *purely personal*. It aggravates the case when it is known that he had no personal acquaintance whatever with Dr. Hammond, and had never in any way, so it would seem, received an injury or any sort of notice from him.

Dr. Hammond is described by Dr. Grissom, as "lost to conscience and honor," as "a Bombastes Furioso of false experts," as "a modern Spartan," a "Titus Oates," as "the Cagliostro of to-day," "a moral monster, whose baleful eyes glare with delusive light, whose bowels are but bags of gold, to feed which, spider-like, he casts his loathsome arms about a helpless prey," and many other passages, in which, among other things, the private religious beliefs of Dr. Hammond are canvassed in a disparaging and unwarrantable manner.

We do not deem any comment necessary, especially since Dr. Hammond has taken the case in his own hands, in an "open letter" to Dr. Grissom. But we wish to call the attention of the *Journal of Mental Science* to this flagrant specimen of personalities and vituperation, which, notwithstanding their

silence, at their meeting and in their journal, we cannot believe met with the approval of all those who heard the paper of Dr. Grissom. We hope the time will be long before we may have to notice such a venomous personal attack, really on impersonal grounds, by one professional gentleman of standing on another.

Under the circumstances, we could scarcely avoid, in justice to ourselves, noticing the cases on which we have commented, for various reasons, and among them that of improving a chance to set the seal of our condemnation on offensive personalities and abusive, intemperate language anywhere, especially in medical writings.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM

Functions of the Brain.—In our last issue we gave an abstract of a former communication by Dr. Hermann Munk, on the functions of the brain, presented to the Berlin Physiological Society, March 23d, 1877. Another communication, giving the further results of his investigations, was presented in July of the same year. Now in the proceedings of the Society printed in the physiological part of *Reichert and DuBois Reymond's Archiv*, we find a third report, made at the session of March 15th of the present year, of which the following are the principal points:

After restating the points observed up to the date of his former communication (see April number of this Journal, p. 391) as to the cortical localization of the visual and auditory perceptions, the psychic blindness and deafness produced by the destruction of these centres, and the gradual return of the senses, before detailed, with his explanation of this cortical blindess and deafness, as he calls it, he gives the results of similar experiments on the brain of the monkey. The visual cortical sphere of the ape is, according to him, the cortex of the occipital lobes, and not the angular gyrus, as Ferrier has it. In fact, Munk rejects all of Ferrier's localizations of the special senses. He finds that extirpation of a limited tract of the cortex in this region produces limited disturbances of vision, the animal appears to have lost the visual impressions for certain objects, while still retaining those for others. The animal rubs his eyes and protects them just like a man who sees indistinctly. If the whole cortex of the convex surface of one posterior lobe is destroyed, the animal becomes hemiopic; he is cortically blind in the same half of both retinæ. For example, if the extirpation is practiced on the left hemisphere he recognizes nothing projected on the left halves of his two retinæ, but perceives objects with the right half of the same. This is indisputably shown by testing both eyes, but the animal soon learns to correct his sight by moving his head, etc., in looking at objects.

If the cortical surfaces of both posterior lobes are extirpated, the monkey becomes absolutely blind, and from being, as usual, a lively, active animal, becomes perfectly quiet and apathetic, and if stirred up to move, stumbles over every obstacle. After a time, sight partially recovers so as to permit him to go about slowly without falling. Further improvement only takes place if certain portions of the cortex still remain untouched on the edge of the convexity. But in these cases it is interesting to notice a hemiopia corresponding to the side which is most completely destroyed. In this point this animal differs from the dog, in whom a hemiopia could not be detected.

As regards the auditory tract of the cortex, Munk had, in this communi-

cation, little or nothing to add to what he had previously reported. The dogs usually recovered their hearing; a few times lasting deafness was observed after extirpation of the whole upper portion of the cortex of both temporal lobes.

Still another section of the dog's brain, that which includes the region of Fritsch and Hitzig's researches, Munk considers as the general sensory region of the cortex, not merely that pertaining to cutaneous sensation, but also the general, bodily sensations. He finds in this region separate centres (1) for the head, (2) for the fore limb, and (3) for the hind limb. Extirpation of these parts causes loss of tactual and muscular sense: partial if the injury is partial, and complete if it is complete. Hence, we have loss of psychic perceptions of these sensations of motion or tact, and hence psychic motor or sensory paralysis of the corresponding members of the side of the body opposite the lesion. The motor centres proper are, in his view, in the basal ganglia and cord, and these cortical centres are those in which the perceptions and conceptions of movements take place. The author's difference in opinion with those who speak of psycho-motor centres appears to be a psychological one,—he has made his explanation of the phenomena of cerebral localization agree with a purely reflex theory, admitting no original will incitations to motion from the cortical centres. His assumption in this respect is not altogether consistent with his skepticism in regard to the theories of others.

The Perception of Sensation.—At the session of the Soc. de Biologie, May 4 (rep. in *Le Progrès Médical*), M. Brown-Séquard reported that he had made a large number of experiments with the compass of Weber on the sensibility of regions situated near the median line of the body. He found that the distance at which the two points gave the sensation of two contacts is not much less here than in other parts of the body. On the back the difference is imperceptible, in front it is a little more marked. This fact is difficult to explain by those admitting two perceptive centres, since, if actually each hemisphere is a centre, two points, one on each side of the median line, should be distinguished, whatever might be their distance apart, since the sensation is transmitted by the nerves to the two halves of the cerebrum. When the compass points are placed vertically it is generally the upper one which is best felt. M. Brown-Séquard, moreover, had found that in certain cases of hyperæsthesia the two points were distinguished, even when they were brought close together.

The Chorda Tympani.—At the session of the Soc. de Biologie, March 30 (rep. in *Gaz. des Hopitaux*, No. 41), M. Mathias Duval gave a communication on the central origin of the chorda tympani nerve. According to him, the chorda tympani or intermediate nerve of Wrisberg is an erratic branch of the glosso-pharyngeal nerve, as both nerves have the same nucleus of origin. He added that it was a true vaso-motor. This, if true, will serve to throw light also on the presumed gustatory function of this nerve.

Spinal Respiratory Centres.—B. F. Lautenbach, in an article in the *Philadelphia Med. Times*, May 11, disputes the opinion that the respiratory

movements depend upon a circumscribed medullary centre alone, holding that there are other lower centres situated in the spinal cord that may still maintain respiration after the destruction of that in the medulla. In various experiments he had destroyed the medulla, and in some instances he had observed that the respiratory movements still continued for a while, or an ineffectual effort at respiration was still seen. More recently he destroyed the whole medulla above the calamus in two young dogs, and saw the respiratory movements of the chest and diaphragm continue for fifteen minutes after the operation. At the autopsies complete destruction of the medulla was found in each case. In these animals, however, the connection between the hemispheres and the pons had been severed several days previously, and this may have, Dr. L. says, in some manner influenced the result; but the persistence of respiration must, nevertheless, be admitted after destruction of the medulla.

Still subsequent experiments put the matter beyond doubt. He was able to find that in normal young dogs and cats respiration still continued after destruction of the medulla. The animals had to be young, and the operation performed with very sharp instruments. If the animal was older, or the operation too rapidly performed, the experiment failed. But in these cases, even, respiration could be reflexly excited by irritation of the anterior crural nerves.

Dr. Lautenbach also severed the upper cervical cord to cut off the connection between the phrenic and intercostal nerves and the medulla, and found that respiration frequently continued in young mammals after such section. The same was the case in several cases when the vagi had been cut previous to the section of the cervical cord.

Dr Lautenbach gives a resumé of the literature of this subject, and finds that Brachet had noticed this last phenomenon as early as 1835, and that analogous observations had been made by P. Rokitansky (*Wiener Med. Jahrb.*, 1874). He comes to the following conclusion, stated in his own words:

"Besides the respiratory centre or centres in the medulla oblongata, there exist in the spinal cord nervous mechanisms, which may keep up the respiratory movements after the destruction of the former."

Physiological and Pathological Effects of Section of the Auditory Nerves and of the Semicircular Canals.—At the meeting of the Soc. de Biologie, Apr. 6 (rep. in *Gaz. des Hopitaux*), M. Brown-Séquard presented some very large American frogs, on which he made his first experiments, in 1852, on the effects of section of the auditory nerve and the semicircular canals. He exhibited two frogs; in the first, he had injured the semicircular canals, and no paralytic phenomena were presented; in the second, he had operated on the auditory nerve, and it showed a contracture on the side opposite to the lesion; the member was not only contracted, but there was also paralysis of pronation, and reflex phenomena were lacking.

He also announced that he had an important communication in regard to cerebral localizations; but for the time merely would state, that from his

numerous researches it resulted that there was no single part of the encephalon, injury of which did not produce disorders of motility or of sensibility in the arm.

Researches on the Vertebral Nerve.—At the session of the Soc. de Biologie, April 27 (rep. in *Le Progrès Médical*), M. Francois Franck communicated the result of some recent researches of his on the vertebral nerve. He stated that we generally describe the filaments that leave the superior extremity of the first thoracic ganglion, and accompany the vertebral artery in the foramina of the apophyses of the cervical vertebra, as efferent nerves from that ganglion. He now presented results of researches in comparative anatomy and vivisections, tending to show that these vertebral fibres form not only the efferent branches of the first thoracic ganglion, but also, and more especially, they constitute the *cervical roots* of the thoracic portion of the sympathetic.

Of two filaments that leave the superior extremity of the first thoracic ganglion, one, the most external, anastomoses with the last mixed cervical nerve, the other, internally placed, joins successively the cervical nerves from above downward to the third inclusive, sometimes also the fourth. It is by its anastomosing fibres that the cord furnishes the sympathetic roots to the first thoracic ganglion.

Operating on the two united branches of the vertebral nerve, between the skull and the second cervical vertebra, just as we operate on any nerve to discover its origin and distribution, M. Franck obtained modifications of the cardiac rhythm and in the hepatic circulation.

The excitation of the ganglionary ends of the vertebral nerve, well isolated, produced acceleration of the beats of the heart, enabling us to assimilate this nerve to the dorsal roots of the first thoracic ganglion, furnishing, as we are aware, the cardiac accelerator nerves. These nerves therefore also come from the cord by way of the vertebral nerve. We can thus explain more easily the results of Bezold's experiment, which showed an acceleration of the heart following the excitation of the lower portion of the cord divided in the lower part of the neck.

Excitation of the superior portion of the vertebral nerve produces, like that of the superior portion of the divided cervical sympathetic, but in a lesser degree, a pupillary dilatation. M. A. Voisin had already emitted the opinion that not all the pupillary fibres were contained in the sympathetic, but that a certain number of them followed the vertebral artery. Experiments thus demonstrate that this is the case. Besides these pupillary effects, excitation of the superior portion of the vertebral nerve causes modifications of the intracranial circulation, which will be treated in a future communication by M. Franck.

These experiments demonstrate that there are in the nerves accompanying the vertebral artery ascending fibres (iridean and vascular) and descending fibres (cardiac accelerators). But one other effect is produced by section of the vertebral nerve, and one to which attention has been already drawn by Pavy, and Cyon and Aladoff, that is the production of diabetes. This result of the section of the vertebral nerve has been contested by Eckhard, and was

not met with by Prof. Vulpian. M. Franck has re-performed these experiments and found, following the section of the vertebral nerve, the urine sometimes charged with sugar, sometimes perfectly normal; variations in results which are ample to account for the disaccord of the above-mentioned physiologists. But if, instead of looking for sugar in the urine, comparative analyses are made of the blood previous to the operation, after the exposure of the nerve, and after its section, we find that in all cases the proportion of sugar in the blood is increased after the division of the nerve; that it passes into the urine if the augmentation is sufficient; it does not do so if the proportion of sugar does not pass above the figure indicated by Claude Bernard, *i. e.*, 1-1000 in the arterial blood. There is always *hyperglycemia* and sometimes *glycosuria*. M. Franck gives the figures of the proportion of sugar in some analyses of the blood of the subhepatic veins and the femoral arteries, collected simultaneously, according to the method of Claude Bernard.

The mechanism of this superabundance of urine will be discussed in future papers, but M. Franck offers provisionally the following explanation: The section of the vertebral nerve interrupts the continuity of a certain number of vaso-motor nerves coming from the spinal cord and destined for the branches of the hepatic vessels. This hypothesis rests on the following experiments: If we introduce a canula of a manometer filled with serum, to which sulphate of soda has been added, into the visceral extremity of the hepatic artery, and then excite the nervous plexus accompanying the artery, we see a rise of the liquid in the free branch of the manometer; there is therefore produced an intra-hepatic vascular contraction.

If we repeat the experiment, exciting, not the hepatic plexus, but the inferior ganglionary portion of the divided vertebral artery, we observe the same phenomena, increase of pressure in the manometer and hepatic vascular contraction. The vertebral nerve, therefore, appears to contain the vaso-motor fibres for the liver. M. Franck believes that the hyperglycemia observed after the section of this nerve depends solely upon a super-irrigation of the liver, and a more abundant diffusion of sugar in the circulation. In future communications, M. Franck will detail these researches, which were begun several years ago (*Comptes Rendus* of the laboratory of Prof. Marey, 1875, and Thesé de Paris), and of which he at present only gives the general results.

The Development of Nerves.—Dr. A. M. Marshall is continuing his careful researches into the earliest stages of nerves in vertebrate embryos. He has recently published, in the *Quarterly Journal of Microscopical Science*, some of his latest results, obtained from embryos of the common fowl, treated with picric acid. He describes a distinct neural ridge at the top of the cerebro-spinal tube in the middle cerebral track, before it has even closed in, the embryo being barely two days old in development. This ridge afterwards becomes continuous along the whole brain and great part of the spinal cord, and many of the nerves undoubtedly arise from it. It appears in the highest degree probable that the olfactory nerve originates from the anterior part of this ridge; but Dr. Marshall is quite certain that there is no special olfactory vesicle in the chick. This is directly contrary to the received

teaching, which speaks of an olfactory lobe in the brain, and does not compare the olfactory with other nerves. Dr. Marshall believes that the common olfactory nerve is really the nerve of the anterior cranial segment. The third nerve is for the first time developmentally traced by Dr. Marshall, and he finds it to be a strictly segmental nerve. The seventh and eighth nerves (facial and auditory) are shown to have a common origin; the auditory is really a branch of the facial. The history of the vagus nerve (pneumogastric) is regarded as suggesting very strongly that it is equivalent to several spinal nerves, and not merely to one.—*Nature*, Mar. 14.

Head Measurements.—A few days ago MM. Cliquet and Lacassagne communicated to the Society of Hygiene a memoir upon the influence of intellectual work upon the formation and development of the head. The researches of these authors are of particular interest because of the measurements of the heads of physicians, and the results obtained are so complimentary to our profession that we cannot deny ourselves the pleasure of presenting them to our readers. The examinations were made on 190 doctors, 130 soldiers who could read and write, and 90 soldiers who were illiterate.

The differences in favor of the doctors over the latter were in the longitudinal diameter one and one-half inches; anterior diameter two inches; posterior diameter one-half inch. Moreover, the asymmetry of the two sides, which is constant, is not the same in all cases. In the heads of educated persons the left half of the frontal region is most developed; in the uneducated it is the right half of the occipital region. The latter being the seat of the passions is always more developed in the ignorant than in the educated.

The conclusions arrived at by the authors are as follows:

1. The head is more developed in persons who have indulged in brain-work than in those whose intelligence has remained inactive.

2. In the educated the frontal region is relatively more developed than the occipital region, and if the difference between these two regions exists in favor of the latter it is very slight, whereas in the former it is considerable.

Parchappe had already come to similar conclusions from measuring the heads of ten laborers and ten well educated men, and in 1861 M. Broca brought forward in support of this theory his authority and numerous examples of autopsies of great men. It is well known that Cuvier's brain was very heavy, and that the physicians who examined Pascal observed a prodigious development of the cerebellum. The observations of MM. Cliquet and Lacassagne are in accordance with those published some time ago by Delaunay. We ought to presume that the development of the head will be synchronous with that of the human intelligence. It is not logical to believe that in these times, when intelligence asserts unquestionable authority in the struggle for existence, there will be found by heredity and natural selection a race possessing large heads and occupying a high intellectual standard of which our grandchildren will be the fortunate representatives

Unluckily a natural fatality rests upon this development of the brain in the capacity of the female pelvis. Until accoucheurs have found a solution

to the problem of the camel and the needle, the development of the pelvis ought to be encouraged, for upon it depends, perhaps, the superiority of our progeny.—*Clinic*, May 18.

Nerve Irritability.—S. Tschiriew (*Arch. f. Anat. u. Phys.*, Phys. Abth., 1877, p. 489) concludes from his experiments, that: Nerve and muscle are excitable by electric currents, traversing them either in a longitudinal or a transverse direction.

The difference in the excitability of the *nerve* varies very little according to the direction of the current. The striated *muscle* can be excited by a feebler current passing through it transversely, than when its direction is parallel to the muscular fibres.

The Accelerator Nerve.—In a series of experiments performed at the Leipzig Laboratory, N. Baxt (*Arch. fuer Anat. u. Phys.*, Phys. Abth., 1877, p. 521) studied the reaction of the heart to maximal excitations of the accelerator nerve, of unequal duration. The dogs were curarized, artificial respiration was maintained, both vagi divided, and the accelerator nerve placed on immovable electrodes in connection with a Du Bois Reymond's induction-coil. The least intensity of a current producing maximal irritation was determined and the nerve excited at intervals of two or more minutes. The results were characterized by an astonishing regularity and precision.

1. The pulse rate following maximal irritation of the accelerator nerve is the same, whatever be the duration of the exciting current. The heart performed the same number of pulsations per second, whether the nerve was irritated during two seconds only, or during thirty-six seconds.

2. The pulse rate after excitation does not depend on the rapidity of the pulse before excitation. After excitation of the accelerator nerve the heart of a dog will always pulsate at a certain rate peculiar to that animal, independently whether the pulse was slow or fast before the excitation commenced.

3. The heart gains its greatest rapidity of pulsation within eight to eighteen seconds after the beginning of the excitation, independently of the duration of the stimulus and of the previous pulse rate. If the current lasts only one second, the pulse will still increase in frequency during the following eight seconds, until the maximal speed is obtained; if the stimulus continues during one minute, the heart will beat no faster at the end of that time, than it did at the end of the first ten seconds. If, therefore, the pulse is slow at the beginning of the excitation, it increases in frequency rapidly until it reaches its maximum rate (peculiar to each heart) at the end of eight to ten seconds; if, on the other hand, the pulse be frequent in the beginning, the increase until the maximum is attained is less, and still spread over the same time—provided the vagus is not excited also.

4. The maximal frequency of the pulse is always maintained for the same length of time (about ten seconds) *after* the cessation of the current, independently of the duration of the stimulus. But the *entire* length of time during which the heart beats with maximal frequency does depend on the duration of the nerve excitation; since when the heart has once reached its

maximal rapidity, it maintains this rate as long as the nerve is excited (and about ten seconds longer, *i. e.*, the after-effect of the excitation).

The entire time, therefore, during which the heart pulsates with greatest rapidity is made up of the duration of the excitation plus the time of the after-effect (ten seconds) *minus* the time necessary to attain the maximum rate (eight to ten seconds). It is hence possible to calculate the numerical result of an excitation of the nerve of any duration after having once learned the maximal rate of frequency of the individual heart. The figures furnished by Baxt of the calculation and its experimental confirmation prove the truth of this statement.

6. The length of time required by the heart in order to return from its maximum rate to its original frequency depends to a certain extent on the duration of the excitation. This statement as well as the former ones refers strictly only to the nerve and heart not fatigued by too long-continued excitation. G.

Respiratory Centre.—In a very long article, mostly of a theorizing nature, R. Burkart (*Pflueger's Arch.*, Bd. 16, Heft. IX. and X., p. 427) produces a number of original experiments in connection with each division of his subject. In the first place he repeated the experiments of Rosenbach, who claimed that after severe hemorrhage, a state of apnœa could be more easily induced and longer maintained by vigorous artificial inflation than in the normal animal. Burkart modifies this statement, since in his experiments this facilitation of the apnœic state, when it existed at all, lasted only a short time, and within ten minutes after a hemorrhage sufficient to cause spasms, the apnœa was more difficult to obtain than normally, if at all possible. The latter result is the one to be expected, since the diminished quantity of hemoglobine cannot serve to store up as large a stock of oxygen as is required for the existence of apnœa. The short time, however, during which the production of apnœa is facilitated, Burkart accounts for by the syncope of the rabbit after the hemorrhage, lessening the demand for oxygen.

A second series of experiments were confirmatory of Rosenthal's views as to the existence of inspiratory and expiratory fibres in the trunk of the pneumogastric nerve. The vagus contains mostly fibres, the centripetal excitation of which cause tetanic inspiration. The superior laryngeal nerve contains, on the other hand, fibres whose influence produces expiratory movements. Besides these two sets, fibres are also found in the trunk of the vagus below the branching of superior laryngeal nerve, which can produce *expiratory* movements, but it seems that they communicate with the expiratory centre only indirectly through the cerebral hemispheres. Destruction of the hemispheres or their paralysis by narcotics prevents completely the action of the last-named fibres. Their action can be most readily shown by exciting the trunk of the vagus continuously for some time with currents of moderate intensity. At first inspiratory movements result. It seems, however, that the inspiratory fibres are more exhaustible than the expiratory nerves, since after fifteen to twenty minutes, the excitation of the vagus will be responded to by *expiratory* movements, especially if the vagus of the other side has been divided.

Finally Burkart tested experimentally the hypothesis, whether the influence of the vagus on the respiratory centre can be accounted for by its supposed action on the vessels of that centre. In other words, whether the vagus is the vaso-motor nerve of the medulla oblongata. The results overthrow completely that hypothesis, since excitation of the central end of the divided vagus in the frog will produce inspiratory efforts, even after the circulation has been stopped by extirpation of the heart, or the blood removed and its place supplied by a solution of salt. G.

THE FUNCTIONS OF THE SPINAL CORD.—B. Luchsinger (*Plueger's Archiv*, B. 16, Heft IX and X).

In a preliminary historical review L. cites the different recent experiments of Goltz, Schlesinger, and others, which have successfully overthrown the older notions, that the spinal cord acted the part of a conductor merely. On the contrary, these recent investigations have proven that a number of centres of vaso-motor and ordinary motor nerves exist in the cord. Luchsinger's own experiments have further established the existence of centres of sweat nerves. The excitability of these centres, however, to the ordinary irritants, as venous blood, convulsive poisons, etc., has usually been denied mainly from the want of reaction of a segment of the cord severed from the medulla. This inertia, however, of the severed segment is due to the shock following the section of the cord.

The following elegant experiment shows the fallacy of the older view. In a chloroformed cat the two subclavian arteries are ligated in order to avoid anastomotic blood supply to the cord. If hereupon the descending aorta itself is closed with a ligature, the posterior half of the animal is thrown into violent spasm comparable to the general spasm following the closure of the carotid and vertebral arteries. These spasms finally pass into paralysis of the cord. During all this time the anterior half of the cat remains perfectly quiet.

While dyspnœa produces in the animal with uninjured nervous system an enormous rise of blood pressure, this result had been no longer observed by previous experimenters after severance of the cord from the medulla. Schlesinger, perhaps, alone, did see such a result, but only in rabbits poisoned with strychnia after that operation. In order to avoid the shock of the cord resulting from its division, S. Mayer and Luchsinger attempted, independently of each other, to eliminate the medulla oblongata without division, but by anæmia produced by ligature of the two carotid and the two vertebral arteries. Luchsinger proceeded as follows: The animals, mostly cats, were chloroformed, tracheotomy was performed, both vagi and sympathetic nerves were divided, and artificial respiration was commenced. This was followed by ligature of all four arteries going to the head. In some cases the natural respiratory movements did not entirely cease after the anæmic spasms had come to an end. The death of the medulla oblongata could be hastened in such cases by a temporary asphyxia. Usually, however, Luchsinger put an end to all respiratory movements by severing subsequently the cord from the medulla with scissors. Curare was next administered to exclude all muscular movements. In an animal thus prepared,

the kymographion will show a steady blood pressure varying from twenty to forty millimetres of mercury. Irritation of a sensory nerve has but little effect on the blood pressure under such conditions. On the other hand, the cessation of respiration produces soon a slackening of the pulse (stimulation of the periphery of the vagus), followed by a slight increase of blood pressure. During the continuance of the asphyxia the arterial tension falls again, but as soon as artificial respiration is recommenced it rises considerably above its original height.

It might be questioned whether this rise in blood pressure is really due to stimulation of the vaso-motor centres in the cord, or to a contraction of the arterioles induced by the irritation of their walls by venous blood. That the blood-vessels themselves are irritable and may be stimulated to contraction by dyspnœic blood has been fully proven by Mosser on the vessels of the excised kidney. In order to eliminate the vaso-motor centres in the cord, Luchsinger closed the aorta in animals prepared as above, until the excitability of the cord was destroyed. After reopening the aorta dyspnœa was induced, without, however, affecting in any manner the blood pressure, thus proving the centres in the cord are responsible for the dyspnœic rise of blood pressure.

Among poisons picrotoxin has hitherto had the reputation of exciting only the general spasm centre in the medulla. Nevertheless L. found convulsions easily produced by this agent in the muscles supplied from segment of the cord separated from the medulla. It is, however, necessary to wait a few days after division of the cord, until the shock of the cord due to the operation has passed off. In new-born or very young animals there is, however, but very little shock produced, and in those cases the convulsions of picrotoxin appear at once. Still the cord of frogs will apparently not react on this drug during winter, although the experiment succeeds with ease during the warm season. But this is only due to the diminution of the excitability of the cord by a low temperature. If a winter frog whose spinal cord has been divided is put for a short time in warm water, and hereupon poisoned with picrotoxin, the convulsions set in at once in both the anterior and posterior parts of the animal.

Picrotoxin excites likewise the vaso-motor centres, as well as the sweat centres, both in the medulla and in the spinal cord. After a high division of the latter, an injection of that agent will still produce an enormous perspiration on the hind paws, as well as a considerable increase in blood pressure.

Luchsinger's experiments prove therefore satisfactorily that there exists not only a general centre for muscular movements, for the vaso-motor and sweat nerves in the medulla oblongata, but that minor centres are also dispersed throughout the entire length of the cord; that these centres can act independently after division of the cord, and that they are excitable by the same agents as the centre in the medulla oblongata. G.

Sweat Nerves.—Continuing his previous experiments, Luchsinger (*Pflueger's Archiv*, 16 Bd., Heft IX. and X.) has traced the course of the nerves supplying the sweat glands of the fore paw of the cat. The result

of direct irritation of the nerves was always corroborated by their division, after which irritation of the cord by asphyxia failed to induce perspiration in the parts supplied by them. The fibres in question leave the cord about the level of the third to sixth dorsal roots, enter the sympathetic, and pass through it into the ganglion stellatum, thence they pass into brachial plexus and follow the course of the median and ulnar nerves. The trunk of the former supplies the inner half of the paw, while the latter innervates the external half.

The following are the titles of some recent papers in periodical medical literature in regard to the Anatomy and Physiology of the Nervous System : Stefani, On the Physiology of the Cerebellum, *Archivio Italieno*, July ; Putnam Jacobi, Sphymographic Experiments on a Human Brain, exposed by an opening in the Cranium, *Am. Jour. Med. Sci.*, July ; Bevan Lewis, On the Comparative Structure of the Cortex Cerebri, *Brain*, April ; Crochley Clapham, On Skull Mapping, *Ibid* ; Marc Sée, The Communication of the Ventricular Cavities of the Brain with the Sub-Arachnoid Spaces, *Revue Mensuelle*, June ; Eulenberg and Guttmann, Physiology and Pathology of the Sympathetic System of Nerves, translated by A. Napier, M. D., *Jour. of Ment. Sci*, July ; Jewell, On Dr. Brown-Séquard and his Recent Lectures, *Med. Jour. and Exam.*, June and July ; R. M. Smith, Note on the Effects of Pressure on the Irritability of Nerve Trunks, *Phil. Med. Times*, July 6 ; Wight, Some Measurements of Skulls and Heads, showing that Development, Growth and Education increase the volume of the anterior parts of the Cerebrum, indicated by the Masto-Frontal Angle, *Proc. Med. Soc. Co. of Kings*, May.

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

An Early Symptom of Tabes Dorsalis.—Westphal, *Berliner klin. Wochenschr.*, 1878, No. 1 (abstr. in *St. Petersb. med. Wochenschr.*). The lack of the "knee or lower thigh symptom" discovered by the author in 1871, is according to his latest careful observations characteristic of tabes, and a very valuable symptom in the earliest stage of the disorder. He has almost invariably observed that the failure of this phenomenon before the development of ataxia and the decrease of sensibility, and in many such cases where, from its failure in conjunction with vague pains indistinguishable from neuralgia, he had diagnosed locomotor ataxia, the diagnosis was confirmed by the ophthalmoscopic examination—advanced white atrophy of the optic nerve, and variations of the pupil. A combination of occasional pains in the lower extremities, together with failure of the knee sinew reflex, appears to indicate incipient locomotor ataxy, but the occurrence of such pains with the continuance of the knee reflex is not so certain an indication that these pains are not tabetic. The author lays especial stress upon this

symptom in certain cases of hypochondria, in which, as in tabes, there are troublesome pains, vesical weakness, impotence, etc., and which are there distinguished with difficulty from incipient tabes. If the knee phenomenon does not appear in such cases then we may infer that the trouble is locomotor ataxy.

Apart from its physiological, pathological, prognostic and diagnostic importance, this symptom may be of significance as regards the treatment, since it enables us to recognize in its earliest stages a disease, which in its later development is almost incurable, and therefore may help us to combat it more successfully.

Hyperæsthesia of the Joints.—Koch, *Berl. klin. Wochenschr.*, Apr. 27 (rep. of meeting of German Surgical Society in Berlin, 7th Congr.).

The author showed that, with the occurrence of hyperæsthesia of the skin the results of hemisection of the cord are not exhausted; that there is besides a pronounced increase of sensibility in the fasciæ, the periosteum and especially of the joints. He further remarked, previous to showing how this accounts for a part of the articular neuroses, that the section must be at the level of the third lumbar vertebra if the hyperæsthesia involves the lower extremities; when the body and upper extremities are involved the section must be as high as the sixth cervical vertebra; and finally, that the same phenomena as are produced by hemisection of the cord are also caused if only the cerebellar lateral tracts of Flechsig (situated on the inner side of the lateral columns of the cord) are injured. Moreover, we are able to recognize at least the skin and joint hyperæsthesia separately if only definite points are implicated within the cerebellar lateral tracts.

The most various methods of research on the great nerve stems do not enable us to produce anything like this hyperæsthesia. The fibres whose course through the cord renders possible the disturbance of sensibility, do not cross until at the level of the medulla, and leave the cord at the sixth cervical vertebra to go to the upper extremities; and those for the lower limbs begin to leave about the level of the first lumbar vertebra.

Considering the absolutely similar arrangement of the lateral cerebellar tracts in man and the higher mammals, it is worth while to examine whether there does not occur in man articular neuralgic affections, like those experimentally produced in animals, affections that are principally characterized by the appearance in an apparently sound joint, of sudden intense neuralgic pain when it is moved, the pain goes away when the cause of irritation disappears. The author mentioned in the further characterization of this affection, the probability that it is, as a rule at least, multiple and unilateral, and mentioned the reasons which speak against the involvement of symmetrical joints, and recalled the fact that simultaneous anomalies of sensibility of the skin and periosteum may be considered. He then cited cases from the literature which confirm the clinical bearings of the experimental disease, and suggested that possibly a large part of what are considered as neuralgias of the joints might be thus explained, but for the difficulty of distinguishing in an animal a condition of the sensibility of the joint in rest and the presence of painful points in the same. In this regard we must look for help to

the clinical analysis of cases in man in which the cord or medulla has been wounded unilaterally. In all cases we can now already distinguish articular hyperæsthesia from neuralgia of the joints in a special sense of the word. The author sketched the differential diagnosis of these two affections, and showed how on the basis of experiment, a whole series of well known brain and cord regions, the interruption of continuity of which, whether traumatic or inflammatory in its cause, or only temporary, must be accompanied with the above sketched phenomena. Experimental evidence is still wanting as to whether purely inflammatory alterations or partial section of peripheral nerves are to be considered as etiological factors of articular hyperæsthesia.

As regards therapeutics it may be said, that by a strong electrical irritation of the skin it is possible to immediately relieve the condition of hyperæsthesia of the joints. Still it returns again after the application, and it is uncertain whether repetition of the treatment really quickens the cure. Koch observed some results from the introduction of a watery saponin solution into the joint; narcotics were absolutely worthless.

Experimental details serving to elucidate the condition closed the paper.

Esmarch, after the reading of the paper, testified as to the value of Koch's experiments, and thought that the central disorder which accompanied neuralgia in man must, in consideration of the usually resulting cure, be of a not very serious, and perhaps only vaso-motor. This helps to understand the temporary œdema sometimes noticed in the articulations.

Ophthalmoplegia Interna.—At the meeting of the Royal Medical and Chirurgical Society, April 9 (rep. in *Brit. Med. Journ.*), Dr. Jonathan Hutchinson read a paper on a group of symptoms (ophthalmoplegia interna) indicative of disease of the lenticular ganglion. After briefly alluding to the anatomy of the lenticular ganglion, the author stated that its distribution by disease might be expected to be followed by paralysis of three distinct muscular structures, the dilator of the pupil, the constrictor of the pupil, and the ciliary muscle. Under such circumstances, the pupil would become motionless, and the patient losing the power of accommodation, would be unable to read with spectacles. For this condition, involving paralysis of all the muscular structures within the eyeball, the term ophthalmoplegia interna was proposed, in contradistinction for cases in which all or most of the muscles moving the eyeball were involved. The author expressed his belief that examples of both these conditions were met with occasionally in practice, and that the features of each were peculiar and of great interest to neuropathologists. His conjecture was that when ophthalmoplegia interna existed alone, that is, unattended by paralysis or defect of any of the external muscles of the eyeball, that the disease was in all probability in the lenticular ganglion itself. In venturing upon this diagnosis, as great importance was to be attached to the absence of some symptoms as to the presence of others. If disease existed implicating the nucleus or any part of the trunk of the third nerve, and thus paralyzing the constrictor of the pupil, there must necessarily be defect of some of the external muscles of the eyeball. The paper next proceeded to narrate the details of eight cases in which

the condition referred to was present. None had afforded an opportunity for dissection; and the diagnosis for the present must, therefore, be held to be conjectural only. It was observed, however, that the cases bore a very close resemblance to each other. In none of them was the patient seriously ill, and in but two were there definite indications of implication of other parts of the nervous system. In none did the disease of the nervous system, whilst the patient was under observation, extend; a fact which might, it was suggested, be in part accounted for by the fact that specifics were used in all. Of the eight cases, in five both eyes were affected. It appeared highly probable that syphilis was in most the remote cause. In three out of the eight, there was no history of syphilis, but in none of these were the facts conclusive as to the negative. All the patients were of an age at which syphilitic affections of the nervous system are common. The oldest was forty-four; the youngest was twenty-seven. In one case, the author had himself attended the patient for severe syphilis four years before the eye symptoms began. Attention was especially asked to the fact that in many cases the paralysis of the iris preceded that of the ciliary muscle, and almost always in excess of it; and further that, under treatment, the ciliary muscle might regain its power whilst the iridoplegia persisted. In no single case was the failure of the accommodation the first symptom. A suggestion was made as to the possible association of this group of symptoms with the early stage of locomotor ataxy, especially with that form of it which appears to be connected with syphilis.

Differential Diagnosis of Disseminated Cerebro Spinal Affections.—Charcot. *Gaz. des Hopitaux*, No. 15, Feb. 5.

To facilitate the diagnosis of the four principal groups of multilocular cerebro-spinal affections, I have thought it well to put in tabular form the various symptoms of each, indicating the relative frequency of the cardinal symptoms and their value. In the following table the italics designate those symptoms to which I attach the most importance.

The other symptoms are as indicated, but less frequent. For example, spasmodic paraplegia and muscular atrophy in multilocular sclerosis, vesical paresis in general paralysis, the embarrassment of speech in locomotor ataxia, etc.

We should be guided principally by the characteristic symptoms, so to speak. Thus, if we observe in a patient ataxia with nystagmus, we must look for disseminated sclerosis, and not for locomotor ataxia, since nystagmus is a valuable symptom of multilocular sclerosis. Also spasmodic paraplegia (recognized by the continuous movements of trepidation produced by a tap on the sole of the foot) is much more characteristic of syphilitic disorder than of multilocular sclerosis, especially if it is accompanied with fixed pain, which always indicates a phenomenon of compression, e. g., the paraplegia consecutive to Pott's disease.

It is unnecessary to add, that this table is intended to show clearly only what we think to be the present state of our knowledge, but that it may be modified in some details by future discoveries. Nevertheless, in its principal points it appears to me only to contain actual facts acquired by medical experience.

Disseminated Cerebro-Spinal Affections.

	Tabetic Series.	Multilocular Series.	Disseminated Syphilitic Disease.	General Paralysis.
Cerebral Symptoms.	Epileptiform Apoplectic Attacks.	*Epileptiform Apoplectic Attacks.*	Epileptiform Attacks.	*Epileptiform Apoplectic Attacks.*
	Vertigos.	*Vertigos.*	*Vertigos.*	*Vertigos.*
	Intellectual Disorders.	*Intellectual Disorders.*	Intellectual Disorders. Aphasia.	*Intellectual Disorders.*
	Diplopia Strabismus.	*Diplopia Nystagmus.*	*Amblyopia Optic Neuritis.*	Diplopia. Amblyopia.
	Amaurosis.	Amblyopia. White Atrophy.	*Diplopia.*	*Inequality of the Pupils.*
	Inequality of Pupils			
	Facial Anæsthesia.		*Headache. Fixed Pain.*	Headache.
	Deafness.			
	Aural Vertigo.			
	Laryngismus.			
	Embarrassment of Speech.	*Embarrassment of Speech.*		*Embarrassment of Speech.*
		Troubles of Deglutition.		
		Paralysis of Pneumogastrics.	Total Facial Paralysis.	
Visceral Symptoms.	*Gastric Crises.*	*Gastric Crises.*	Crises. Non-nervous.	
	Nephritic Crises.			
	Vesical Crises. Paresis of the Bladder.			Paresis of Bladder.
	Cystitis.			
Spinal Symptoms.	Sensation of Cord around Body.	Fulgurant Pains.	*Pseudo-neural Pains.*	*Fulgurant Pains.*
	Hyperæsthesia.			
	Anæsthesia.	Patches.	*Spinal Hemianæsthesia*	*Formications.*
	Inco-ordinated Movements.	Inco-ordination.		*Inco-ordination.*
	Contractures and Tremors.	*Special Tremor.*	*Spasmodic Paraplegia in the form of Hemiparaplegia.*	*Paresis Tremor.*
		Spasmodic Paraplegia.		*Special Tremor of the Hand.*
Trophic Symptoms.	Bedsores.	Bedsores.		Bedsores.
	Arthropathies	Arthropathies.		
	Fractures.			
	Muscular Atrophy.	Muscular Atrophy.		Muscular Atrophy.

Muscular Spasm of the Facial Muscles.—At the session of the Soc. de Biologie, Mar. 16 (rep. in the *Gaz. des Hopitaux*), M Krishaber reported a case of a man in whom there existed a condition of muscular spasm of the facial muscles, rendering it extremely difficult for him to masticate his food or to talk. Whenever he opened his mouth, certain muscles of his face and neck were spasmodically affected, and continued so until it was closed again; this spasm was bilateral, but a certain predominance of the muscles of the right side was observed. The zygomatici, the orbicularis, the eyelids, the nasal muscles, in short, all those of the upper part of the face were unaffected, while those below were very much distorted. The line of demarcation was the mouth itself. The muscles of the tongue, of the floor of the mouth, and the velum palati were uninvolved. The patient could not feed himself, and was condemned to absolute silence. M. Krishaber had not been able to follow up the patient, and could not give the further particulars as to the outcome of this strange affection.

Trophic Disorders Following Experimental Lesions of the Sciatic.—M. Brown-Séquard, at a recent meeting of the Soc. de Biologie, April 27 (rep. in *Le Progrès Médical*), stated that he had observed, in a number of Guinea pigs, the sciatic nerves of which had been divided, a number of hitherto undescribed phenomena, such as extreme agitation, cries, rotation, transient cataleptic rigidity. In all of them, moreover, the characteristics of very pronounced spinal epilepsy, persistent after the first symptoms, which may be attributed to a spinal meningitis, have more or less rapidly disappeared. In one of these Guinea pigs, the paw of the side opposite the divided nerve is now the seat of a notable atrophy, that of the injured nerve being intact. This fact counts against the classic theory of trophic disorders. Moreover, M. Brown-Séquard maintained that the so-called trophic disorders are often due to the animal's biting the paw in its convulsive movements.

General Paralysis.—We condense the following from the *Gaz. des Hopitaux*, May 4th.

General paralysis of the insane is not a paralysis, but an ataxia. M. Bouillaud remarked this in 1846, and his statement has been repeated since then by Marcé and Jaccoud. Dr. J Christian, physician in chief of the asylum of Maréville, seeks to demonstrate this anew in a memoir before us. The elements of true paralysis do not exist in what is commonly known as general paralysis; no matter how long it exists, we never observe (except in fortuitous complications) an absolute veritable paralysis. As the disease progresses and the intelligence fails, the patient becomes less and less able to move, but, even to the last, there are movements, feeble, to be sure, irregular and inco ordinated, but still possible and voluntary. Such is the position maintained by M. Christian, and in which he is supp orted by all alienists.

The physician of Maréville proceeded to experimentally establish this fact in the following manner: he made use of dynamometrical tests on twenty-two paralytics; nine being between thirty and forty years of age, nine

between forty and fifty, and four only over fifty years of age. In all the disease was of long standing.

The important point was to ascertain what is the difference in muscular power in the same patient at different stages of the disease. In order to have a point of comparison, M. Christian conceived the idea of weighing his paralytics every time the dynamometer test was made, the variations of weight to mark the progress of the disease. He noticed that, in general, as the bodily weight augmented the force of pressure also increased, but occasionally the reverse occurred, so that it was not possible to establish the exact relation between the variation of the weight and that of the muscular power.

Taking 50 as a mean of muscular force, out of the twenty-two patients, five exceeded this figure, the highest being a man 41 years old, who gave 85 at the first examination, and at subsequent ones oscillated between 80 and 60; mean 70. Six patients were below 20, the minimum being 10, observed once only at the end of the disease, in a man 37 years of age, who at the beginning gave 25, and as an average, 16 to 18. The means of the other patients were, in one 17, one 19, in two 26, one each of 32, 35, 36, 37, 38, three each of 38 and 40, two 45, and one each of 46, 47, 48, 52, 54 and 55.

With all these variations it will be seen in no case was there absolute paralysis, and in four cases the average was above the normal. Even at the very last there was still ability to voluntarily contract the muscles.

Acute Pneumonia as a Neurosis.—M. Ch. Fernet, *La France Médicale*, Nos. 23 and 24, advances the theory that acute pneumonia is a herpes of the lung due to neuritis of the pneumogastric nerve. In this idea that pneumonia is a herpes, he follows MM. Parrot and Lagout, who, in their respective memoirs on herpetic fever (*Gaz. Hebdom.*, 1871), and on herpes labialis (*Memoires de la Soc. Méd. des Hopitaux*, 1873), have advanced the same opinion. They supported it on the analogy of facts of internal herpes of the mucous membranes, such as the herpetic sore throat recognized by Trousseau, Gubler, Lasègue and others, the resemblance in certain essential points of the symptoms of these affections and those of acute pneumonia, the coincidence of labial herpes in both these diseases, etc. That the skin remains generally unaffected, is a fact they explain by the greater resisting power of the cutaneous surface to the morbid manifestation, and that the herpetic process exhausts itself on the internal organs.

The nature of pneumonia as a neurosis (herpes being considered as such) being thus assumed, the intermediary nerves must be sought. Our author thinks, and here he follows the analogies suggested by others, that zona of the face is a neuritis of the trigeminus, that herpetic angina is neuritis of the same nerve, or rather, perhaps, of the glosso-pharyngeal, that pneumonia is due to a neuritis of the pneumogastric. In support of this view, he cites three cases of post-mortem of cases of acute unilateral pneumonia, in which there were evidences of decided irritative lesions of the vagus on the affected side, reddening, enlargement and congestion. In one case in which there was a clinical history of diaphragmatic pleurisy, the phrenic nerve of the same side (right) was affected. The results of microscopic examinations

were merely negative. These facts seem to give some plausibility to his views, which, nevertheless, we think will hardly yet meet with universal acceptance.

Changes of the Sympathetic in Diabetes—The following are the results of the examinations of the sympathetic in five autopsies of diabetic patients, made by Dr. Stanislaus John Poniklo, of Cracow, and published in the London *Lancet* (Am. Rep.), May, 1878. We quote his own words:

1. "In all the cases I found an increased amount of connective tissue in the ganglia, especially in the upper and lower cervical ganglia. This change consisted partly in an hypertrophy of the connective stroma, normally occurring in the sympathetic ganglia, and partly in the accumulation of newly-formed connective tissue between the nervous elements, a change that was beautifully seen in transverse sections of the ganglia. In some sections the amount of connective tissue was so great it was only with considerable difficulty that rudiments of nervous tissue could be detected. The capsules of the nerve cells appeared to be thickened, having numerous granules adhering to them. In some parts numerous cells, partly round, partly fusiform or elongated, were deposited between the fibres of connective tissue, this cellular infiltration being most marked around the blood-vessels.

2. "In one case I found abnormal dilatation of blood-vessels in the solar ganglion, the vessels being tortuous and distended with corpuscles. In some of the microscopical preparations the dilated vessels covered nearly one-half the field. There were also irregular accumulations of red corpuscles, the result of small haemorrhages.

3. "The most interesting change was that presented by the nervous elements themselves. The nerve cells, the capsules of which were thickened as above described, appeared to be of smaller size than normal, their protoplasm was more homogeneous, especially in those parts of the ganglia where the cellular infiltration predominated. In one case, that of a young subject, there was an increased amount of dark-yellow or brown granular pigment in the nerve cells. Turning now to the nerve fibres, it may be mentioned that in the normal ganglia of the sympathetic, in addition to bundles of grey non-medullated fibres, there occur a fairly large number of *white medullated* fibres, arranged partly in separate bundles enclosed within a distinct sheath of areolar tissue, and partly intermixed in the bundles of the non-medullated fibres. I did not find any bundles of medullated fibres in the sections of ganglia from these cases of diabetes, but here and there isolated and extremely atrophied white fibres could be distinguished in the midst of the grey, which were preserved in the preparations. These last-named fasciculi were even much atrophied, the bundles being small, and the individual fibres wasted, and in parts almost concealed by cellular infiltration. The changes in the connective stroma of the ganglia above described sufficiently account for the atrophy of the nerve fibres and cells, an effect very frequently observed in other parts of the nervous system—e. g., the brain and spinal cord—where there is hypertrophy of the connective tissue. It must, however, be reserved for future investigation to ascertain whether this atrophy of the nerve fibres does not extend higher up in a centripetal direction.

"The above-mentioned changes were met with, to a more or less extent, in each of the five cases examined, and they presented an excellent illustration of extremely advanced degeneration of the cervical sympathetic ganglia.

"The results of this examination of the sympathetic nerve in diabetes, although based on a small number of cases, are in perfect accordance with physiological experimentation, for Cyon and Aladoff have shown that excision of the cervical ganglia is immediately followed by diabetes. So extensive a degeneration of the ganglia as that above described may be considered as equivalent to their excision."

The researches of M. Franck, given elsewhere in the Periscope of this issue, are of interest in this connection.

Neuritis.—Ottomar Rosenbach, *Arch. f. exp. Path.*, VIII., 223 (*St. Petersburger med. Wochenschr.*), has repeated in Cohnheim's laboratory, Tiesler and Feinberg's experiments of producing artificial inflammation of a nerve, and watching for its extension. He employed the sciatic and vagi nerves of rabbits, operating in part with caustics, and in part by transfixing the nerve, but found nothing but local alterations to follow the lesion; as regards the question of *neuritis migrans* also the results were entirely negative, no disseminated inflamed patches being found elsewhere in the course of the nerve, or in the cord.

Psychic Epilepsy.—J. Weiss, *Psychiatrische Studien aus der Klinik von Prof. Leidesdorf*, Wien, 1877. *Centralbl. f. d. med. Wissensch.*, No. 15. Psychic epilepsy is a special form of disorder, running a typical course, convulsive attacks being replaced by psychoses, which stand in no relation to convulsions, and have nothing to do with them. The mental disorder is characterized by a sudden onset without incubation; it speedily or within a few hours reaches its acme, then quickly disappears, and is followed by complete psychic restoration, and by its periodic recurrence in the same form, without any production of a secondary psychosis or notable mental weakness. Four cases are given. One denied ever having had any convulsive attacks. He suffered from frequent attacks of insanity, lasting three or four days, was in this condition highly demented, felt a short aura, and was, after this period, perfectly normal again, but with no recollection of what had happened during the attack. The second of the patients had suffered for thirty years from vertigo, pain and confusion of mind. At present are observed spells of running frantically around, behaving violently, crying "fire," etc. Duration, one to two days. In the third the patient was regularly, two or three days before menstruation, at first melancholic, then fully demented, and again, after two or three days, the patient is well. The fourth, a boy, had sometimes several times a day, and sometimes not oftener than once in eight to fourteen days, an attack of pain of short duration, and during the time there appears to him a figure that it seems to him he must kill—consciousness returns again in one or two minutes. In this case, three months later, after a first short attack of stupor, there occurred actual epileptic attacks, with tonic and clonic convulsions. The author explains this as a metamorphosis of the earlier psychosis to epilepsy with convulsive attacks.

A further paper on this subject, with detailed accounts of two cases, by the same author, is published in the *Allg. Zeitschr. f. Psychiatrie*, XXX., 1.

THE DIAGNOSIS OF EPILEPSY AND HYSTERO-EPILEPSY.—In a lecture at the Salpêtrière, M. Charcot thus gives the differential diagnosis between true epilepsy, the greater epilepsy, as he calls it, and hystero-epilepsy.

The following three phases are known of the former:

1. An aura of short duration, not giving the patient time to prepare for the attack.

2. A second phase comprising the initial cry, the period of tonic contractions, that of clonic movements, then stertor.

3. A third phase, lacking in many, we may say the majority of cases, which consists in what we have called the post epileptic delirium, a period of violent impulse or maniacal agitation.

Hystero-epilepsy has an entirely different symptomatology.

1. The aura is often extremely prolonged: it lasts one, two, or several days. The patient has abundant time to prepare for the attack. The sensation mounts slowly from the abdomen toward the head, making stoppages by the way. It may be compared to a knotted cord.

2. Arrived at the *epileptoid* stage. There is first a prolonged, modulated cry, we may say in one case like the whistle of a locomotive. Then comes on a period with ataxic contractions, extension of the limbs, turning of the head, then one of clonic movements and finally a light degree of stertor.

The above may be compared with epilepsy. What follows appertains to hysteria proper, and we observe, moreover, as it were, a pause between the two acts of the drama.

3. The patient is then seized with bizarre contortions, which correspond with nothing, and produce no delirium. The body rises in the bed, falls and rises again, curves in the arc of a circle, of which the head and feet form the two extremities. All these gymnastics are as incoherent, disordered, and fantastic as possible.

4. This gradually disappears to give place to the phase of *emotional attitudes*. Here the gestures, positions, violent as they may be, are governed by some sentiment and idea, the sentiment and the idea delirious. The attitude expresses, in turn, the most various thoughts, the look and the speech indicate within a few moments fear, anger, shame, tenderness, pleasure; many patients are plunged into a sort of ecstasy, the eyes are directed toward heaven, the hands clasped, the visage illuminated. Some express by the *pose*, the special form of delirium, long before they express it in words. A young German woman, long in the hospital, remained two hours immobile and silent, the arms extended as if crucified. When she rose she intoned a choral of Luther, or some other religious poetry.

It may be questioned in this connection whether, in cases of this kind, it is rather the delirious idea that suggests the attitude, or the attitude that suggests the idea.

5. Finally, there appears a phase corresponding very nearly to the post-epileptic delirium of epilepsy, though it is not altogether comparable in the

two cases. In the hysterical patient illusions and hallucinations predominate. She sees animals—cats, dogs, rats, and vipers. Each patient has her own favorite hallucinations. The illusions are in regard to persons about her. She ordinarily takes the physician or *interne* for some one of her acquaintances, and, according to the case, treats him with courtesy or abuse, employs contumely or words of affection

Such are the five phases of the hystero-epileptic attack, which cannot be reasonably confounded with the true disease. Some of these phases may be lacking, but we may rest assured that their order will not be other than here described. (*Tribune Médical.*) *Gazette des Hopitaux.*

The Association of Obscure Abscess of the Liver with Hypochondria.—Dr. W. A. Hammond, *St. Louis Clin. Record*, June, calls attention to the possibility of abscess existing in the liver, unrevealed by special hepatic symptoms, and giving rise to very considerable gastric derangement, and cerebral and emotional disturbances, which latter may amount to serious hypochondria, verging on insanity. The cerebral symptoms, in the five cases narrated, were headache, insomnia, vertigo, and great mental depression. He was led to examine the liver, and in most of the cases detected fluctuation. The abscesses were emptied by the aspirator introduced through the intercostal spaces, and the results were, in all five cases, satisfactory as to the removal of the cerebral symptoms, after the evacuation of a variable quantity of pus. In one other case, instead of an abscess, a hydatid cyst was discovered by the needle. Dr. Hammond deduces from these facts the following conclusions:

1. That hepatic abscesses are probably much more common with us than is generally supposed.

2. That they may exist without any local symptoms, or such general disturbance of the system as is commonly regarded as indicating their presence.

3. That they may be associated with hypochondria and other evidences of cerebral disturbance.

4. That they should be opened at the earliest possible moment, and without waiting for adhesions to form between the liver and the abdominal wall.

5. That the proper place for performing the operation of aspiration is in one of the intercostal spaces. This point is strongly insisted upon by Dr. Davis, in his memoir.

6. That the operation by aspiration is free from danger. Dr. Davis never saw any ill consequences from it, and Dr. Jimenez, of Mexico, states (*Clinica Medica*, Appendice a las lecciones sobre los Abscessos, del Higado, Mexico, 1866, p. 4), that of the hundreds of times he has punctured the liver through the intercostal spaces for abscess, he has never once seen the operation followed by peritonitis. In a very admirable paper, Dr. Tauszky, of New York (*Med. Record*, April 20, 1878), expresses the same opinion.

7. That in all cases of hypochondria or melancholia, the liver should be carefully explored, and that even if no fluctuation be detected, or any other

sign of abscess be discovered, aspiration, being a harmless operation, should be performed.

8. That if pus be evacuated, the operation may be expected to be followed by a cure of the mental disorder, as well as by the preservation of the life of the patient from the probably fatal consequences of hepatic abscess.

9. That if no abscess be found, the patient will at least be no worse off than he was before.

The paper alluded to above, of Dr. J. C. Davis, appeared simultaneously with Dr. Hammond's publication in the *N. Y. Med. Journal* for June of the present year.

Pathology of Nerves.—Dr. G. Tizzoni publishes, in a short note in the *Centralblatt*, No. 13, 1878, the following conclusions from his histological researches. In order to render the contents of the sheath of the nerve plainly visible, he removed the myeline by dissolving it with chloroform.

1. In normal nerves and also in fibres recently divided or irritated, a fine *horny* reticulum is found supporting the medullary investment. The same has been demonstrated by Kuehne with another method.

2. After division of a nerve, degeneration of the axis cylinder and myeline occurs, both in the central and the peripheral portion; faster, however, in the latter, and extending to the last intramuscular terminations.

3. This degeneration commences always at the annular constrictions of Ranvier, and proceeds thence towards the nucleus in the centre of the interannular portion.

4. The changes in the myeline consist at first in the formation of large oval drops, separated from each other and corresponding to the segments of the medullary sheath described by Schmidt and Lantermann. Subsequently these drops divide still further, and give rise to a varicose appearance of the nerve fibre.

5. The main factor in the destruction of the medullary sheath is the penetration of leucocytes into the interior of the fibre. The protoplasm of these cells absorbs the myeline globules, and either destroys or transforms them. To all appearance, a part of the leucocytes leave again the fibre, carrying with them their spoils, while another portion of those cells undergoes annihilation in the nerve fibre.

6. This immigration of cells occurs not only at the cut surface of the nerve, but probably also by diapedesis, and through openings in the neurilemma, since it can be observed after ligature of the nerve or injection of irritants, without opening the neurilemma.

7. The immigration is followed also by destruction of the horny reticulum.

8. The degeneration of the axis cylinder consists in multiple segmentation, which is always preceded by a thinning of the axis cylinder at the subsequent line of division. More rarely granular degeneration and formation of vacuoles is observable in the axis cylinder.

9. Some of the fragments of the axis cylinder can be absorbed and destroyed by the immigrated cells.

10. After destruction of the axis cylinder and myeline, the fibre appears shrunken on account of the hypertrophy of the endoneurium, the longitudinal fibres of which border on the neurilemma. Within the neurilemma, numerous oval nuclei are arranged in rows.

11. After section and other modes of irritation of nerves, the nuclei of the nerve multiply, and the protoplasm surrounding them is augmented. Tizzoni also saw the nuclei divide into two, three or more pieces by transverse lines of segmentation.

Further details are reserved for a larger publication on the pathology of the nervous system. G.

The following are a few of the most recently published papers on the Pathology of the Nervous System and Mind :

Gray, Spasmodic Spinal Paralysis, *Proc. Med. Soc. Co. of Kings*, July; Fileine, Mellituria from Irritation of the Depressor Nerves in Rabbits, *Centralbl. f. d. Med. Wissensch.*, No. 18; Baumgarten, Hemiopia after Disease of the Occipital Cortex, *Ibid*, No. 21; Eisenlohr, On the Pathological Anatomy of Sclerosis of the Posterior Columns, *Centralbl. f. Nervenheilk.*, No. 6; Weber, The Nature of Hydrophobia, *Phil. Med. Times*, June 8; Morelli, Pathological Cases Bearing on the Question of the Existence of Cortical Motor Centres, *Lo Sperimentale*, June; Doutrebente, The Different Kinds of Remissions Occurring in the Course of Progressive General Paralysis, *Ann. Med. Psychol.*, May; Jacobs, Mania Epileptica, *Deutsche Med. Wochenschr.*, June 1; Seguin, Lectures on the Localization of Spinal and Cerebral Diseases, *N. Y. Med. Record* (cont. art.); Robertson, Observations on Some Points in Cerebral Pathology and Percussion of the Skull, *Jour. Ment. Sci.*, July; Gowers, On Some Symptoms of Organic Brain Disease, *Brain*, April; Duret, On the Role of the Dura Mater and its Nerves in Cerebral Traumatism, *Ibid*; Mitchell, On a Rare Vaso-Motor Neurosis of the Extremities, and on the Maladies with which it may be Confounded, *Am. Jour. of Med. Sci.*, July.

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c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

Morphia.—Dr. H. Gibbons, Sr., *Pacific Med. and Surg. Journal*, June, lays down the following rules in regard to the hypodermic administration of morphia :

1. Avoid it in congestion and inflammatory conditions of the brain.

2. Avoid it in pulmonary congestion, and where dyspnœa is not the result of spasm.

3. Avoid it in acute inflammatory affections of the heart and pericardium.

4. Avoid it in high febrile excitement.

5. Avoid puncturing a vein. The effect of introducing morphia in a vein is instantaneous. It is possible that the evil results were due in some

cases to the entrance of the narcotic into a small vein, so small and deep as to escape observation.

6. Avoid a deep puncture, unless there is a special purpose to be accomplished by depositing the narcotic deep in the tissues.

7. Introduce the liquid slowly, and not by sudden projection, as is sometimes done. Spend four or five seconds in driving the piston home.

8. Require the patient to lie down and remain quiet after the operation. There is some risk in moving about, independently of the nausea which follows. The syringe should be used in the patient's chamber, and not in the doctor's office.

I may add, in regard to the appliance in cardiac cases, that according to my experience, there is no condition of human suffering to which it is more applicable than in the agonizing orthopnœa of cardiac asthma. In fact, it is the remedy *par excellence* for the paroxysm of spasmodic asthma, from whatever cause.

Strychnia.—Dr. W. A. Hammond, *St. Louis Clinical Record*, May, recommends in facial paralysis induced by cold, in conjunction with other measures, such as electricity, elastic support, etc., the energetic use of strychnia, employing a solution of one grain to the ounce of water, and beginning the administration with ten minims three times a day, increasing one minim each day until the patient experiences the physiological effects of the remedy in cramps in the limbs or jaw. Then he commences again, if necessary, with the initial dose of ten minims (1-48 gr.). He gives two cases illustrating the success of this method of treatment.

Warm Baths in Tetanus.—At the session of the Soc. Méd. des Hopitaux, Jan. 11 (rep. in *L' Union Médicale*), M. Fereol read a report by Dr. Dionis de Carrieres (of Auxerre), of a case of tetanus cured by the use of warm baths prolonged for from two to six hours. This observation supplements two similar ones reported by M. Blachez in the *Gaz. Hebdomadaire* of Jan. 4th, and one case of traumatic tetanus in a German soldier cured by the same means during the Franco-Prussian war, at the same hospital of Auxerre. It was the reminiscence of this case that suggested the treatment in that of Dr. de Carrieres.

Electricity.—Dr. A. D. Rockwell, *N. Y. Med. Record*, April 6, recommends the use of the methods of central galvanization and general faradization in epilepsy, together with the medicinal treatment, and claims good results in his own experience. Three cases are narrated in which this treatment was employed, and with apparent benefit.

He applies the galvanic current to the brain and sympathetic for not over five minutes, and the alternating *seances* of general faradization are not continued over from five to ten minutes. Large sponges should be employed in preference to smaller ones, which cause a disagreeable concentration of the current.

Electricity in Incontinence of Urine in Children.—Dr. Arthur Kemble, *Boston Med. and Surg. Journal*, April 25, in the cases of atony of

the sphincter vesicæ, which are the result of long-continued morbid sensibility of the neck of the bladder, and accompanied with incontinence of urine, uses electrization (faradization) of the sphincter by means of conical-pointed electrodes introduced into the bladder, and then withdrawn just so far that its enlarged extremity is grasped by the muscle. The other button-shaped electrode may be placed on the pubis, and a mild current from a pocket Gaiffe battery is sufficient for the purpose. In the cases in which he has employed this treatment he has seen decided benefit result.

Physostigma.—Sidney Ringer, *Lancet* (Am. repr.), March, gives an account of four cases of paraplegia, two of locomotor ataxy, and one of writer's cramp, which were treated with small doses of physostigma, one-tenth of a grain, or less of the extract, taken at frequent intervals. Improvement was observed in three of the cases of paraplegia, though two of these afterwards relapsed; both the cases of ataxy and the case of writer's cramp were cured. In spite of the comparatively poor results, Dr. Ringer thinks that physostigma will be found useful in some forms of paraplegia, before the disease has had time to disorganize the cord.

Nitrite of Amyl.—Dr. W. S. Forbes, *Hays' Jour.*, April, reports two cases of hydrophobia in which great relief was afforded by inhalations of nitrite of amyl. The patients were enabled to eat and drink with comfort, and enjoyed refreshing sleep after the inhalations. Death, however, finally occurred during convulsions. The doses used were large—twenty-four drops to a teaspoonful at one administration.

Bromide of Ammonium.—The following are the conclusions deduced by Dr. L. Brechemin, *Phil. Med. Times*, March 30, from his experiments on the physiological action of bromide of ammonium, performed mostly on frogs, but also, to some extent, on rabbits and pigeons:

1. The convulsions produced by poisonous doses of bromide of ammonium are spinal.

2. Bromide of ammonium produces paralysis of the receptive tract of the spinal cord—that is, of the part which receives and transmits impressions—and of the peripheral ends of the afferent or sensory nerves.

3. Death is produced by asphyxia.

4. The action of bromide of ammonium on the nervous system is therefore identical with that of bromide of potassium.

Magnets and Static Electricity in Hysterical Anæsthesia.—At the Soc. de Biologie, March 16 (rep. in *Gaz. des Hopitaux*, No. 33), M. Romain Vigouroux gave an account of some experiments performed at the Salpêtrière by invitation of M. Charcot. The following are the principal results: A steel bar magnet, held at a distance of many millimetres from an anæsthetic part, recalled the sensibility in a time rarely exceeding ten

minutes. It makes no difference whether both poles or either one of them is presented, the result is the same.

As a counter proof, if we present the centre of the bar no effect is produced.

This effect has been noted as well in cutaneous anæsthesia as in that of the organs of sense.

The effects observed have a great analogy to those from the metallic applications of M. Burq (increase of muscular power, return of sensibility).

It appears probable that magnets, which played a great part in the therapeutics of the last century, should be rehabilitated with more precise indications.

M. Romain Vigouroux next reported two experiments upon patients with static electricity, using the dielectric machine of Carré. The patients having been placed on an isolated stool, under the influence of moderate discharges, the sensibility was seen to return and become generalized more rapidly than by any other means. This goes to confirm the *rôle* attributed by M. Vigouroux to the electric tension in the phenomena produced in hysterical patients by metallic applications, continued currents, etc. (*Gaz. des Hop.*, Nov. 17, 1877.)

Ergot.—At the session of the Soc. de Biologie, March 2 (rep. in *Gaz. des Hopitaux*), M. Laborde stated that he had made a series of experiments with ergotine from various sources, especially the ergotine of Bonjean, the ergotine of Tanret, the ergotine of Wiggers, that of Yvon, etc. If we inject into the ear of a rabbit a gramme of Bonjean ergotine, or a centigramme of ergotine of Tanret (this latter being much the most active), we see at once the commencement of an anæmia in that ear, reducing the vessels to their minimum calibre, and we find the temperature of the ear decreased at least four degrees centigrade. If the same injection is made into a rabbit's ear after section of the great sympathetic, the same phenomenon is produced. It is, therefore, on the muscular fibre of the vessel itself, rather than on the nervous system, that ergot exerts its action.

Antagonism of Aconite and Digitalis.—The following is the general summary of a recent memoir by Dr. J. Milner Fothergill, on the antagonism of aconite and digitalis, concluded in the *British Medical Journal*, May 4:

1. The minimum lethal dose of Morson's aconitine for a Guinea pig of 1½ lbs. weight is 1-1200th of a grain; for a rabbit of 1½ lbs. weight, 1-400th of a grain; for a 3 lb. rabbit, 1-300th of a grain; the difference being due, probably, rather to age than to size. The younger rabbit took a comparatively larger dose.

2. The minimum lethal dose of digitaline (Smith's) is nearly one grain to the pound weight in the rabbit.

3. The simultaneous administration of digitaline and aconitine affords no protection.

4. If digitalis be given in sufficient dose and a sufficient time (from five to nine hours) before a lethal dose of aconitine is administered, a distinct protection is afforded.

5. When the digitaline is given at a less interval than five hours, the toxic power of aconitine is intensified.

6. Small doses of atropine given before lethal doses of aconitine exercise a decided protective influence.

7. Small doses of aconitine do not appear to exercise any influence over lethal doses of atropine.

8. Atropine in four-grain doses effectually antagonizes lethal doses of aconitine, up to sixteen minutes after the administration of the latter,

9. Atropine exercises no like influence over lethal doses of chloral.

10. Strychnia exercises a potent influence over lethal doses of aconitine, but the experiments are too few to warrant very decided conclusions.

NICOTINE.—The following are the general results of an extended series of experiments by M. Albert René, of Nancy, as reported in the *Gaz. des Hopitaux*, No. 52.

The experiments were principally directed to the following points: The routes of absorption, the action on the muscles, the nervous system (motor and sensory nerves, and nerve centres), the respiration and the blood.

Nicotine may enter the system through all the ordinary methods of absorption, the gastric and intestinal mucous surfaces, the conjunctivæ, by hypodermic, intravenous, or intraglandular injections, and by the respiratory passages, and the characteristic symptoms of poisoning are observed in all. By the mouth its action is more rapid than by simple hypodermic injection, but intravenous injection produces very sudden results.

The toxic dose varies somewhat from unappreciable causes. Generally it is not exactly proportional to the weight of the animal. The most important point, considering the rapidity of its elimination, is the absolute amount of the poison introduced into the system at a given moment, and this quick elimination explains to a certain degree the tolerance of the substance often noticed.

The direct effect of nicotine is to destroy or diminish the muscular contractility. The poisoned muscle loses in a few moments, more or less completely, the power of contracting, under the influence of the galvanic current, as well as from nervous stimulus; the nerves themselves are, moreover, very rapidly paralyzed. M. René remarks that the muscular paralysis is not due to that of the motor nerves, but is a true paralysis of the muscular fibre due to the direct local action of the nicotine. Nervous motricity is destroyed at the commencement of the poisoning, even before the loss of muscular contractility.

Nicotine acts also on the sensibility; it enfeebles or destroys it.

But it is the central nervous system that is first affected by this agent, which acts at once from it on all parts of the motor nerves. Convulsions are the first indications of excitation of the nerve centres. They consist in a general tremor, analogous to that of an intense chill. It is a veritable vibration of muscular waves; a tremor of each muscle, or better, of each muscular fibre. In place of the fusion of contractions, which results in a contraction of all the muscle, there is a tetanic succession of all the contractions that ought to blend in a single one.

The excitation of the cord at the commencement of the poisoning, especially by small doses, and giving rise to an exaggeration of reflex power,

speedily disappears, to be replaced by paralysis. The period of excitation and convulsions is very short. The reflex power of the cord lasts a little longer, then it, like the sensibility, the excito-motor faculty of the nerves, and the muscular contractility, is also abolished.

The initial excitation of the nerve centres by nicotine extends also to the respiratory centre in the medulla. Respiration is accelerated by small doses, and becomes slower, again, after a longer or shorter time of excitation. In becoming slower it also becomes fuller and stronger. After a few moments a convulsive cramp occurs at the end of expiration, and in this the animal may succumb. It is ordinarily the expiration that is more troubled than the inspiration. The retardation of the respiration augments progressively.

If the animal does not die during the expiratory convulsion, it succumbs finally to arrest of the respiration and the circulation due to the general depression of these systems.

In the autopsies, M. René observed only occasional nuclei of congestion in the lungs and hyperæmia of the brain, and especially of the cerebellum. These authorize no conclusion as to the mode of action of the poison.

Finally, it follows from M. René's experiments, that nicotine has no effect on fermentation, on artificial digestion, or on putrefaction, and that there is no antidote except elimination.

The action on the heart and vessels is to be taken up hereafter.

The Chinoline and Pyridine Bases.—Dr. J. G. McKendrick, *British Medical Journal*, May 4, published a report of a series of investigations undertaken to determine, (1) the physiological action of the various members of the pyridine and chinoline series of bases; (2) whether there was any difference between the action of these bases derived from cinchonine and the isomeric ones derived from tar; (3) whether, if so, how, both as regards extent and character, the physiological action of these bases differed from that of the original alkaloidal bodies; (4) whether the action of the methyl and ethyl compounds of the substances was different from that of the substances themselves; and (5) whether any facts could be ascertained generally which would throw light on the interesting problem of the relation between the chemical constitution of active substances and their physiological action.

After giving a statement of the chemical relations of the series, he gives in detail his experiments, which were chiefly made upon rabbits, though other animals up to man were also employed. The physiological action seemed, however, to be similar on all. These investigations have been extended over four or five years, and a summary of the conclusions reached at an early period of the research was published in this journal for October, 1875. The following is the latest summary contained in the present report:

1. There is a marked gradation in the extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose, however, becomes reduced as we rise from the lower to the higher.

The higher members of the pyridine series resemble, in physiological action, the lower members of the chinoline series, except (1) that the former

are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one-half that of the chinolines.

3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the spinal cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first as irritants, causing violent convulsions, and afterwards producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appears to be so far inactive as not to be excited by pinching or pricking, it may be readily roused to action by strychnine. These conclusions apply especially to rabbits; in animals of higher cerebral development, such as dogs, monkeys, and man, nausea and vomiting are produced in addition.

4. On comparing the action of such bases as $C_9 H_7 N$. (chinoline) with $C_9 H_{13} N$. (parvoline), or $C_8 H_{11} N$. (collidine), with $C_8 H_{15} N$. (conia from hemlock), or $C_{10} H_8 N_2$ (dipyridine) with $C_{10} H_{14} N_2$ (nicotine from tobacco), it is to be observed that, apart from differences in chemical structure, the physiological activity is greater in those bases containing the larger amount of hydrogen.

5. Those artificial bases which approximately approach the percentage composition of natural basis are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases; but the kind of action is the same in both cases.

6. When the bases of the pyridine series are doubled by condensation, producing polymeric compounds such as dipyridine, dipicoline, etc., they not only become more active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids having an approximately similar chemical composition.

7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life, in lethal doses, either by exhaustive convulsions or by gradual paralysis of the centres of respiration, thus causing asphyxia.

8. There is no immediate action on the sympathetic system of nerves, although there is probably a secondary action, because after large doses the vaso-motor centre, in common with other centres, becomes involved.

9. There is no appreciable difference between the physiological action of the bases derived from cinchona and those derived from tar.

10. The union of methyl, ethyl, amyl and allyl with chinoline did not entirely change its characteristic mode of action, but their presence caused, along with stupor, a tendency to spasm and convulsion. Also, in the case of the pyridine and picoline addition-compounds, increase in molecular complexity and weight, while it does not entirely change the mode of action of the simpler compound, is always attended by a tendency to spasm and convulsion, probably arising from irritation of cerebral motor centres.

11. The nature of the acid with which these compounds were combined did not appear materially to affect the physiological action, which apparently depended on the nature of the base. As a rule, bromides and iodides were more potent than chlorides or sulphates. No difference of effect could

be observed between the iodine and bromine compounds of the addition-derivatives.

12. Dicarbopyredenic acid ($C_7 H_5 NO_4$), the only substance containing oxygen we have examined, is remarkable for rapidity and potency of physiological action; it also acts rapidly on the vaso-motor centres. In containing oxygen it has a nearer resemblance in percentage composition to several alkaloids than any of the chinoline or pyridine compounds.

13. Substances having the same percentage chemical composition, but a different chemical structure, may have different physiological effects. Contrast lepidine with methyl-chinoline, dispoline with ethyl-chinoline, isoline with amyl-chinoline, and quinine with cinchonine.

14. Substances which have an action on the cerebral portion of the nervous system will act on animals differently, according to the degree of their cerebral development. Thus, chinoline causes in the dog, cat, and monkey, excitement and nausea and vomiting antecedent to stupor and anæsthesia; while in the rabbit none of these phenomena occur.

It is understood that several of these conclusions are merely tentative, and may require to be modified on future investigation.

Hypophosphite of Zinc.—R. W. Gardner, *N. Y. Med. Record*, proposes the use of hypophosphite of zinc as a substitute for the phosphide of zinc in therapeutics. Its advantages he claims are its containing phosphorus in the state of the protoxide, in which condition it is "rendered non-irritant, admitting the use of such quantities as to fully meet all indications for either phosphorus or zinc, and that being readily soluble it is readily assimilated." He recommends it in the form of syrup, and says that it has been therapeutically tested with very satisfactory results.

As to the advantages of this preparation which are claimed by the author over phosphide of zinc, that it contains a larger proportion of phosphorus, we are somewhat in doubt, as it appears to us that enough for all practical purposes of such an active agent may be administered in the latter combination. Still we shall wait to hear of further therapeutical investigations in regard to the hypophosphite.

Physiological Action of Lycoctonia, Napellina and Aconitia.—The following conclusions in regard to the physiological actions of lycoctonia, napellina and aconitia, are given at the conclusion of a lengthy paper by J. Munro Murray, Asst. Surg., U. S. N., published in *Philadelphia Med. Times*, May 11th and 25th:

ACONITIA.

1. Aconitia is a powerful toxic agent.
2. It kills mainly through the respiratory apparatus.
3. It paralyzes the motor nerves.
4. It paralyzes, first the sensory nerve ends, then the nerve trunks and spinal sensory ganglia, and does not affect the striated muscles.
5. It reduces the arterial tension and pulse, with a decided primary rise of the former.

6. The pulse and pressure changes are due to an action on the intra-cardiac nervous apparatus.
7. The vagi are paralyzed by small doses.
8. The delirium cordis is due to a change in the nervous mechanism of the heart.
9. It paralyzes the heart of batrachians.

NAPELLINA.

1. Is a weaker toxicant than aconitia.
2. Same as aconitia.
3. Same as aconitia.
4. Same as aconitia.
5. Same as aconitia.
6. Same as aconitia.
7. The vagi are paralyzed by medium doses.
8. Same as aconitia.
9. It does not poison the heart of batrachians.

LYCOCTONIA.

1. Lycoctonia is a weaker toxicant than either aconitia or napellina.
2. The same as aconitia.
3. The same as aconitia.
4. It does not affect sensory nerves, spinal cord, or striated muscles.
5. It reduces arterial tension and pulse without any primary rise.
6. ——.
7. The vagi are only paralyzed by large doses.
8. Same as aconitia.
9. It does not paralyze the heart of batrachians.

The following are some of the articles recently published on the Therapeutics of the Nervous System and Mind:

OHMS, On Muriate of Pilocarpine, *St. Petersburg Med. Wochenschrift*, No. 6; GUAITA, Metallotherapy and its Application in Diseases of Children, *Lo Sperimentale*, April; MACKENZIE, On Some Indications for the Use of Digitalis in Acute Mental Disorders, *Journ. of Ment. Sci.*, July; LENTE, Treatment of the Vomiting of Pregnancy, *N. Y. Med. Record*, June 22; BEARD, The Nature and Treatment of Inebriety, *Quarterly Journ. of Inebriety*, June; MOSLER, On the Local Treatment of the Meningeal Affections in Acute Rheumatism, *Deutsche Med. Wochenschr.*, Nos. 23 and 24; EULENBERG, On Metallotherapie, *Ibid*, No. 25; WEBER, Belladonna as a Stimulant to the Circulatory System, *Philadelphia Med. Times*, July 6.

BOOKS, ETC., RECEIVED.

Fowne's Manual of Chemistry. Theoretical and Practical. Revised and corrected by Henry Watts, B. A., F. R. S. A New American from the Twelfth English Edition. Edited by Robert Bridges, M. D. With One Hundred and Seventy-seven Illustrations. Philadelphia, 1878: Henry C. Lea. Chicago: Jansen, McClurg & Co. 1027 pages.

The Atlantic Islands as Resorts of Health and Pleasure. By S. G. W. Benjamin. Illustrated. New York: Harper & Bros., 1878. 274 pages. Chicago: Jansen, McClurg & Co.

Insanity in Ancient and Modern Life, with Chapters on its Prevention. By Daniel Hack Tuke, M. D. London: MacMillan & Co., 1878.

The Perception of Color. By G. Stanley Hall. (From the Proceedings of the American Academy of Arts and Sciences, Vol. XIII.)

Vascular Tumors of the Female Urethra. With the Description of a Speculum Devised to Facilitate their Removal. By A. Reeves Jackson, A. M., M. D. Chicago, Ill. Reprint from Volume II., Gynecological Transactions, 1878.

Neuralgia and its Modern Therapeusis. By James B. Baird, M. D., Atlanta. Reprint from the Transactions of the Medical Association of Georgia.

The Present Status of the Pathology of Consumption and Tuberculosis. By J. Hilgard Tyndale, M. D., of New York City. New York, 1878. 93 pages.

Case of Dr. John W. Finley, with Autopsy of the Brain. Read before the Iowa State Medical Society, Jan. 30, 1878. By Walter Hay, M. D., of Dubuque, Iowa.

Observations in Practice, Surgery, Gynecology, and especially Obstetrics. By George B. Walker, M. D. Read before the Indiana, Illinois and Kentucky Tri-State Medical Society in Evansville, Oct. 17, 1877. Reprinted from the Chicago Medical Journal and Examiner, February and March, 1878. Chicago, 1878.

Scarlatina in Chicago. Particularly the Epidemic of 1876–7. By Chas. W. Earle, M. D. Read before the Illinois State Medical Society.

Medicinal Plants Indigenous in Michigan. By A. B. Lyons, M. D. Read before the Detroit Academy of Medicine, Nov. 27, 1877. (Reprinted from the Detroit Lancet, February and March, 1878.)

On the Immunity of Certain Mothers of Children Affected with Hereditary Syphilis. A paper read at the First Annual Meeting of the American Dermatological Association, Niagara Falls, New York, Sept. 8, 1877. By James Nevins Hyde, A. M., M. D. Reprinted from the "Archives of Dermatology," April, 1878.

Trephining the Skull in a Case of Idiocy, with Remarks. By William Fuller, M. D. (Read before the Medico-Chirurgical Society of Montreal, 1878.)

The Treatment of the Insane. By Dr. Nathan Allen, of Lowell, Mass. Read at the meeting of the American Social Science Association. Saratoga, Sept. 6, 1876. Albany, 1876.

Die Heilkrafte der sogenannten indifferenten Thermen, insbesondere bei Krankheiten des Nervensystems. Historisch-kritische vorträge in Collegenkreise von Dr. Wilh. Theodor v. Renz. Allgemeine Theil I—Die Wärme, Affinität und Elektricität der indifferenten Thermen und Badformen in theoretischer und praktischer Beziehung. Tubingen, 1878.

Electricity in Medicine. By F. T. Miles, M. D. (Reprint from Maryland Medical Journal for March and April, 1878.)

Is Modern Education Exerting an Evil Influence upon the Eyesight of our Children? By A. W. Calhoun, M. D. (Reprint from the Atlanta Medical and Surgical Journal.) Atlanta, 1878.

The Codes of Medical Ethics. (American Medical Association, American Institute of Homœopathy, National Eclectic Society.) Arranged and compiled by C. Henri Leonard, A. M., M. D. Detroit, Mich., 1878.

Amputations and Excisions of Cervix Uteri; their Indications and Methods. By J. Byrne, M. D., M. R. C. S. E. (Reprint from Vol. II., Gynecological Transactions, 1878.)

Sammlung Klinischer Vortraege, R. Volkmann. No. 103. Ueber Neuritis in diagnostischer und pathologischer Beziehung. Von H. Nothnagel, 1876. No. 108. Die Ursachen und Anfangssymptome der psychischen Krankheiten. Von Ewald Hecker, 1877. No. 112. Ueber den heutigen Stand der Frage von der Localisation im Grosshirn. Von Edward Hitzig, 1877.

Certain Symptoms of Nervous Exhaustion. By George M. Beard, M. D. (Reprint from Virginia Medical Monthly, June, 1878.)

True and False Experts. By Eugene Grissom, M. D., LL. D. (From the American Journal of Insanity for July, 1878.)

An Open Letter to Eugene Grissom, M. D., LL. D., etc. From William A. Hammond, M. D., of New York.

Trastornos del Aparato de la Vision en las Fiebres Paludeas y Accidentes a que puede dar lugar su ineludible tratemiento por el Sulfato de Quinina. Por el Dr. D. Juan Santos Fernandez. Habana, 1877.

The Theory and Practice of Non-Restraint in the Treatment of the Insane. By W. Lauder Lindsay, M. D., F. R. S. E. (Reprinted from the Edinburg Medical Journal, for April and June, 1878.)

Transactions of the Iowa State Medical Society, 1877–78. Volume III. Des Moines, 1878. 196 pages.

The Respiration of Compressed and Rarefied Air in Pulmonary Diseases. By F. H. Davis, M. D., Chicago. (Reprinted from the Chicago Medical Journal and Examiner, Oct. 1877.)

Fluid Extracts by Repercolation. By Edward R. Squibb, M. D., Brooklyn, N. Y. (Reprinted from the American Journal of Pharmacy, for May 1878). With additions by the author.

Note on Hydrobromic acid. By Edward R. Squibb, M. D., of Brooklyn, N. Y. (Republished from the Transactions of the Medical Society of the State of New York, for 1878.)

Artificial Perforation of the Membrana Tympani. By Lawrence Turnbull, M. D., Philadelphia, 1877.

On the Several Methods of Educating, and on the Selection of Proper Schools for the Deaf and Dumb.

On Bathing, Swimming, and Diving as Causes of Aural Disease. By Lawrence Turnbull, M. D. (Extracted from the Transactions of the International Medical Congress, Philadelphia, Sept., 1876.)

How to save the Perineum —A New Use of the Obstetric Forceps.—An Improved Instrument. By Edw. Warren Sawyer, M. D. (Reprinted from the Chicago Medical Journal and Examiner, May, 1878.)

Amputation of Cervix Uteri. By W. H. Wathen, M. D. (Reprint from May No. Richmond and Louisville Medical Journal.)

On Destructive Lesions of the Cortical Layer of Cerebrum. By H. D. Schmidt, M. D. (In New Orleans Medical and Surgical Journal, March, 1878).

Second Annual Report of the State Board of Health of the State of Wisconsin. For the year ending Dec. 31, 1877. Madison, 1878.

Annual Report of the Trustees and Superintendent of the State Lunatic Hospital of Pennsylvania. Harrisburg, 1877.

Fourteenth Annual Report of the Board of State Charities, of Massachusetts, January, 1878. Boston, 1878.

Twenty-fifth Annual Report of the Pennsylvania Training School for Feeble-Minded Children, Media, Delaware Co., 1877.

Reports of the Trustees and Superintendent of the Butler Hospital for the Insane. Presented to the Corporation at their Annual Meeting, January 23, 1878.

Thirty-fifth Annual Report of the Managers of the State Lunatic Asylum, Utica, N. Y. For the Year 1877. Transmitted to the Legislature, Jan. 14, 1878.

The Fifty-fourth Annual Report of the Officers of the Retreat for the Insane, at Hartford, Conn., April, 1878. Hartford, 1878.

Eighteenth Annual Report of the Medical Superintendent of the State Asylum for Insane Criminals, Auburn, N. Y. For the Year ending Sept. 30, 1877.

Report of the Department of Health of the City of Chicago, for the Year 1877. Chicago, 1878.

Fifth Annual Report of the Managers of the New York State Inebriate Asylum, at Binghamton, for the Year 1877. Transmitted to the Legislature, Jan. 17, 1878.

Thirty-ninth Annual Report of the Board of Trustees and Officers of the Columbus Hospital for the Insane (formerly the Central Ohio Lunatic Asylum) to the Governor of the State of Ohio, for the Year 1877.

Twenty-first Annual Report of the Trustees and Superintendent of the Ohio State Asylum for the Education of Idiotic and Imbecile Youth, to the Governor of the State of Ohio, for the Year 1877. Columbus, 1878.

Eleventh Annual Report of the Board of Trustees and Officers of the Minnesota Hospital for Insane (located at St. Peter), to the Governor of the State of Minnesota, for the Fiscal Year ending Nov. 30, 1877. Transmitted to the Legislature of the Twentieth Annual Session, 1878. Minneapolis, 1878.

Thirteenth Annual Report of the Chicago Hospital for Women and Children, Corner Paulina and West Adams Streets, for the Year ending March 1, 1878. Chicago, 1878.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie, Physiologie, und Wissenschaftl. Medicin.
Archiv fuer Path. Anatomie, Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
Australian Practitioner.
Brain.
British Medical Journal.
Bulletin Générale de Thérapeutique.
Centralblatt f. d. Med. Wissenschaften.
Centralblatt f. d. Nervenheilk., Psychiatrie, etc.
Cronica Med. Quirurg. de la Habana.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Frenocomia di Reggio.
Gazetta Medica de Roma.
Gazette des Hopitaux
Gazette Médicale de Bordeaux.
Glasgow Medical Journal.
Hygeia.
Hospitals Tidende.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Mind.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Rivista Sperimentale di Freniatria e de Medicina Legale.
Revue Médicale du Nord-Est.
Revue Mensuelle de Médicine et de Chirurgie.
Schmidt's Jahrbuecher der In- und Auslandischen Gesammten Medicin
St. Petersburger Med. Wochenschrift.
The Practitioner.
Upsala Lakareforenings Forehandlinger.

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 Medical Weekly.
American Practitioner.
Archives of Dermatology.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Buffalo Medical Journal.
Canada Medical Record.
Canadian Journal of Medical Sciences.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Lancet.
Maryland Medical Journal.
Medical Brief.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Michigan Medical News.
Nashville Journal of Medicine.
New Remedies.
New York Medical Journal.
Ohio Medical and Surgical Journal.
Pacific Medical and Surgical Journal.
Proceedings of Med. Society of the County of Kings, N. Y.
Pharmacist.
Philadelphia Medical Times.
Quarterly Journal of Inebriety.
Richmond and Louisville Medical Journal.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.

THE

JOURNAL

OF

Nervous and Mental Disease.

Vol. V. OCTOBER, 1878. No. 4.

Original Articles, Selections and Translations.

ART. I.—ON THE ANATOMICAL AND PHYSIOLOGICAL EFFECTS OF STRYCHNIA ON THE BRAIN, SPINAL CORD AND NERVES.

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IT is with considerable misgiving that I undertake to describe anatomical changes produced in the nervous system of the economy by the drug strychnia. Many have tried, but thus far in vain, to discover some effect that they could state positively was produced by strychnia. The trouble is, that pathological changes are met with, but it is hard to tell if they are simply post-mortem, or if they are produced by strychnia. While an ordinary practitioner should be able to determine with the microscope the various kinds of abnormal growths, yet it requires years devoted to the one subject, with constant practice, to recognize any changes produced in nerve substance itself. Again, the effects of the various reagents used in preparing the specimens may mislead one, and make him think he sees things he does not see. So, altogether, the subject is fraught with difficulties; and I will be only too much pleased if I can succeed in throwing even the shadow of a light

on the obscure problem. I claim, at least, that the sections were prepared with the utmost care; that all parts of the nervous system have been examined, and examined closely; and that, in *every case*, sections have been made of both the normal tissue and the strychnized tissue, taken from precisely the same spot in two different animals, and the two carefully *compared*.

As just stated, in each case two rabbits were used, both in good health, one of which was killed by cutting the medulla spinalis between the occiput and the atlas, and the other killed by hypodermic injections of strychniæ acetas in the leg.

Post-mortem examination with the naked eye in the strychnized rabbits showed excessive congestion. So great was this that, in one instance, on showing to a friend the specimens I had obtained, and which I had suspended in a bottle, he said, "How did you inject these specimens?"—so certain was he that I *had* injected them; and this ran throughout the whole brain and cord, and was not confined to the membranes; indeed, the latter had been removed in the case mentioned above.

On removing the skull-cap and the processes of the vertebræ, the sight was really sickening. I can express it by no other word; and the astonishing thing is that the animal could have lived as long as it did—great swollen vessels standing out, gorged, and looking, under the microscope, like twisted cords, the blood with difficulty held in by the thin walls of the small arteries and capillaries, and threatening to break through its investments.

Of change in the nerve substance proper, I have little to say.

(1) The sciatics were used to examine the effects of the drug on the *nerves* in their course, being carefully teazed out. The cells themselves showed no change whatever in any case, the normal and the strychnized specimens side by side having the same appearance. The only thing noted was a change, real or apparent, in the axis cylinder. It came out more distinctly in the normal than in the strychnized specimen. The white substance of Schwann appeared perfectly normal.

(2) The brain proper showed no great lesion, except that in the grey substance there was apparently granular disintegration, in some places quite general, and in others, here and there,

in little spots. This, too, was apparent in the cord, extending into the white substance. This is all that could be noted in any case. The question now arises, Is this simply a post-mortem change, or is it due to the strychnia? I think it is a *secondary* result of the strychnia, due to the intense vascular engorgement. This is justifiable, because both the normal and the poisoned nerve were treated in every way the same, both in respect to time, and condition, and the reagents in which they were placed; so that they ought to have had the same appearance if the strychnia had not produced any effect. At the same time, I do not think this is a primary change, due to the strychnia, but a secondary one, due probably to the vascular changes, and that, if there were another drug producing the same effects physiologically, it would also produce the same anatomical changes. To any one who has examined the brain and cord of an animal killed by strychnia, the vascular changes are seen to be sufficient to cause death. The nerve-cells throughout the brain and cord are squeezed and pushed, and their functions must be disturbed; the relations of the blood and the cephalo-rachidian fluid fluctuate violently. Many of the symptoms are the same as in compression of the human brain. This fact seems to have escaped notice hitherto. The pupil is widely dilated, the pulse is slowed, there is vaso-motor paralysis, yet the temperature is little changed in either case. And now, in addition to all these sufficient causes of death, no one seems to have remembered (that I am aware of) that the *respiratory centre* is affected; that the granular disintegration that has been noticed occurs. I propose to show hereafter that this little spot is more especially affected, and through it, of course, the respiratory and cardiac movements. The vascular engorgement of the rest of the nervous substance would tend to produce paralysis, but not necessarily death; while the vascular engorgement of this little tract would produce certain death, unless the action of the lungs and heart were kept up by artificial means.* This explanation accords exactly with the physiological action of the drug, as detailed below. I think I prove conclusively that the nerves in their course are not affected by this drug, and so we find few changes. Now, this

*Vide page 648, *et seq.*

is as it should be, for the nerves in their course do not suffer the compression that the brain and cord do: in fact we might disregard the infinitesimal quantity, and say they do not suffer at all. The paralysis all over the body is purely spinal; the nerves themselves respond perfectly to any stimulation. So, to my mind, this explanation is conclusive, and I do not think we need or ought to look for further changes in the nerve substance than would be produced by the vascular engorgement, remembering the symptom of compression of the human brain, and the great effect this engorgement has on the respiratory and cardiac centres.

1.

It would be a work of supererogation for me to enumerate the great physiologists who have experimented on the action of strychnia. The conditions of this paper do not require it. I trust I may be pardoned, however, if I call to mind the names of Majendie in 1809, Stannius in 1837, and Van Deen in 1839. Then follows the noble work of the lamented M. Claude Bernard, and M. Brown-Séquard's experiments, begun in 1849, and continued at intervals ever since. In 1858 Koelliker published his experiments, and in 1860 Martin-Magron and Buisson published their interesting paper in Brown-Séquard's *Journal de la Physiologie*. Then follows Vulpian, and many others—names that it would be a pleasure to write did time and space permit.

With regard to the action of strychnia on the motor nerves, I must be more explicit, as the results have apparently been so heterogeneous in nature. MM. Mueller, Matteucci, Morean, Ambrosoli, Wittich and Martin-Magron and Buisson asserting that the poison effectually paralyzes the motor nerves, while MM. Bernard, Koelliker, Pelikan, Valentin, and Vulpian, in quite as positive a manner, assert that it does not act on the motor nerves in their course, or influence them in the least after once leaving the spinal cord. We will shortly narrate (in their proper sequence) some experiments that we hope will tend to clear up these doubts, and place the matter in a way of being definitely settled.

I will now narrate some of my own experiments on the ner-

vous system. The strychnia that I used was prepared by Messrs. Powers & Weightman, of Philadelphia. The ordinary tests were applied, and it was found to be *perfectly pure.* In each cubic centimetre of distilled water there was dissolved by the aid of acetic acid, one hundredth of a gramme of strychnia, and the solution gave a neutral reaction with litmus paper. For the purpose of electric irritation, I made use of one Grove cell, eighty millimetres high, and fifty-five millimetres in diameter. I also used DuBois Reymond's induction apparatus, with Helmholtz's modification for equalizing the opening and closing currents, in connection with DuBois' key. The induction apparatus was graduated by means of a scale, marking the fractions of centimetres, 0 representing the condition of the apparatus when the secondary coil was pushed completely over the primary coil, hence representing the strongest position of the apparatus.

From the salient phenomena of poisoning by strychnia, so well known to all, we are justified in assuming that the alkaloid exerts its peculiar power either on the cerebrum, on the efferent nerves, on the afferent nerves, or on the gray matter of the spinal cord. We will consider each in turn. Before proceeding to examine these points *seriatim*, however, let me narrate a few experiments I have made in connection with the general action of the alkaloids on guinea-pigs, pigeons, turtles and snakes.

Guinea Pig. At 4:00 P. M., .005 gramme of the acetate of strychnia was injected hypodermically into the leg. At 4:1 short, rapid breathing and trembling. At 4:2:30 rolls over in a general and severe convulsion. At 4:3:30 sensation increased; tetanus continues. At 4:4 retches; frequent but short convulsions. At 4:7 dead. Chest opened at 4:14; ventricles beat 26 per minute; auricles beat 50 per minute. Autopsy—Intense congestion of all the organs; right ventricle full of blood, but left ventricle empty.

Pigeon. At 3:58 P. M. received .0025 gramme of strychnia hypodermically in the leg. At 3:59 excessive tetanus. At 4:00 tetanus frequent and prolonged. At 4:1 no cessation; 1:2:15 dead. At 4:13 the sciatic nerve responded with the coil marking 0.

Snake. At 1:15 P. M. the cord was cut below the medulla oblongata, and .01 gramme of strychnia was injected into the lower segment of the body. Rapid tetanus of an exquisite character occurred. As the poison gradually coursed down the spine, the tetanoid phenomena were manifested at different points of the body. Sometimes a space of a foot in length would form a firm arch, standing upright on the table.

Vulpian says that he has produced an effect by introducing strychnia under the skin of an earth-worm, but that it was merely local. On crabs it has hardly any action. He introduced as much as ten centigrammes of strychnia, sometimes into the tissues, and sometimes into the lungs, without producing the least toxic effect.*

I procured a turtle, and injected as much as a tenth of a gramme. No characteristic phenomena of poisoning were produced. On opening the turtle the next day, its heart was found still beating.

But to return. We are to consider whether strychnia exerts its peculiar power on the cerebrum, on the afferent nerves, on the efferent nerves, or on the grey matter of the spinal cord. That the convulsions are not cerebral is shown by the following experiment, one of fifteen that I performed in this connection: The encephalon was removed by cutting through the medulla spinalis, on a line with the posterior edge of the tympana. The convulsions not only occur, but they are much more violent after cutting the cord, because we remove the influence of the centre for the inhibition of reflex action, situated, according to Setschenow, in the corpora quadrigemina and thalami optici.

Frog. Weight 13 grammes. At 3:7 P. M. receives .005 gramme of strychnia, injected into the lungs. At 3:7:30 general tetanus ensues. At 3:11 the cord was cut, but prolonged tetanus remained until 3:21:30, when the frog was dead.

Frog. Weight 14 grammes. At 3:18 P. M. .005 gramme of strychnia was injected into the lungs. At 3:18:15 general tetanus ensues. At 3:20 ablation of the cerebrum. The tetanus continued unabated till 3:25, when the observation was

Leçons sur la Physiologie, etc., page 448.

suspended. That the poison does not affect the peripheral terminations of the sensory or motor nerves, may be seen, as in the following experiments, seven in number, of which I relate two as examples: I cut off the access of the poison entirely from one leg, by tying all the arteries and veins on that side. In spite of this operation, the convulsions took place with as great force as if it had not been performed.

Frog. Weight 20 grammes. Right iliac artery and veins tied. At 11:13 A. M. .005 gramme of strychnia was injected into the lungs. Hops away. At 11:15 general tetanus, and opisthotonos at 11:15:30. The convulsions show no preference for any part of the body, being as strongly marked in the right leg as anywhere else. At 11:16 the same phenomena show themselves. At 11:17 the rigidity is so great that the frog can be made to stand up on either hind leg. At 11:19 the muscles of the right side of the body are hard and tense, the gastrocnemius standing out prominently. At 11:35 dead.

Frog. Weight 22 grammes. Right iliac artery and veins tied. .005 gramme of strychnia is injected into the lungs at 11.48 A. M. At 11:50 convulsions. At 11:53 spontaneous tetanus all over the body, including the right leg. At 12:1 the tetanus is almost as marked in the right leg as anywhere else in the body. At 12:15 dead.

In order to determine whether the poison could affect either the sensory or motor nerves during their course, after leaving the spinal cord, I cut the cord below the origin of the brachial plexus, but left all the nerves going to the posterior extremities intact. I then severed all the spinal branches of the aorta below the point of section, and on administering strychnia, I found that no tetanus whatever occurred, there was no hyperæsthesia (that is to say, no more than would be due to the removal of Setschenow's centre), but perfectly normal reflex action remained. The minutiæ of two of these experiments (six in number) are detailed as follows:

Frog. Weight 20 grammes. Medulla spinalis cut below the origin of the nerves of anterior extremities, and the spinal branches of the aorta severed. At 11:31 A. M. .005 gramme of strychnia was injected into the lungs. At 11:32 great irritability was shown in the fore part of the body, but none in the

posterior extremities. At 11:33 tetanus in front, but no movement of the hind legs. Indeed, tetanus did not occur at any time in the hind legs. The frog was dead at 12:10 P. M.

Frog. Weight 25 grammes. Cord cut below the brachial plexus, and spinal branches of the aorta severed. At 12:11 P. M. .005 gramme of strychnia was injected into the lungs. At 12:12 heightened sensibility in front legs and head, but only normal reflex action in hind legs. At 12:14 violent convulsions in upper part of the body, but not even a tremor in the hind legs, the quadriceps flexors and gastrocnemii soft and flaccid. At 12:16 the tetanus became so violent in the fore part of the body, that it lasted continuously for two minutes, the eyes being closed and sunken in the head, and the head itself twisted violently backwards, but the hind legs remained unaffected. At 12:25, dead.

The experiments that I now relate prove conclusively that the grey matter of the spinal cord must be the seat of the tetanus-producing action of strychnia. In these cases the poison was cut off from the posterior extremities, but was allowed free access to the spinal cord, and, as a result, general tetanus was induced. I made five experiments under this head, of which I relate two as examples:

Frog. Weight 67 grammes. The aorta was tied above its bifurcation, and at 6:44 P. M. .005 gramme of strychnia was injected under the skin of the lower jaw. At 6:46 tetanus occurs over the whole body, without discrimination. At 6:48 the cord was cut, but general tetanus still continues, till, at 7:15, the animal dies.

Green Frog. Weight 103 grammes. Aorta tied above its bifurcation. At 12:13 P. M. .005 gramme of strychnia was injected under the skin of the back. At 12:17, general tetanus occurred over the whole body. At 12:18, emprosthotonos. General convulsions occurred till the observation was suspended.

The question has been agitated for some time as to whether the motor nerves are paralyzed in poisoning with strychnia. Many eminent names are arrayed on each side, as shown above, and it is difficult to explain how it happens that such

contrary results could be obtained as those at present on record. At first I thought that perhaps the difficulty lay in the species of frogs, as some experimenters used the *Rana temporaria*, and some the *Rana esculenta*; but I procured animals of both these species, and have obtained exactly the same results in each. Perhaps it was due to the means of irritation, as many of them used Pulvermacher's apparatus, which is by no means constant, or, perhaps, the nerves were not handled carefully enough, and thus sustained a traumatic injury in dissecting them. Again, if the nerve was raised on a thread, the irritation might have been applied to the proximal and not to the distal end. But these are only surmises. Should any of them be true, the fault doubtless must be laid to an oversight on the part of the experimenters, and not to a positive error. Again (and this, perhaps, is the more likely), the strychnia may not have been pure. Few, if any, of these observers mention having tested their strychnia, I believe. Now, we all know that the strychnia of commerce contains more or less *brucia*, and this may paralyze the motor nerves, though strychnia does not. I have instituted a series of experiments with *brucia*, which may yield an explanation of this difference of results. Many are the charges that have been brought against those who asserted the non-paralyzation of the motor nerves. It has been said that they did not give large enough doses of the poison, or that they tested the nerves too soon. But both of these charges must be unfounded, as I have given as much as one-tenth of a grammie (1.54 grains) to a frog weighing only eighteen and a half grammes, and have tested the sciatic an hour after the animal was dead and the heart ceased beating, and have found it still capable of responding. Great care was observed in the following experiments, that the current should not spread to the muscle, by raising the nerve high up on a silk thread. The strength of the current was also accurately noted by means of DuBois' graded coil, with which a platinum zinc cell was used. To determine this point I have made thirty-seven (37) experiments, of which I relate a few. The notes of all the others, which are before me, exactly accord with these. In each case the frog was weighed, so that a comparison might be made

between the weight of the frog and the amount of the strychnia injected.

Green Frog. Weight 120 grammes. At 11:50 A. M. .005 gramme of strychnia was injected into the lungs. At 12:30 .01 gramme more was injected; same dose repeated at 12:32, and again at 12:34. Some time after the heart had ceased beating the sciatic was irritable. At 2:15 the left sciatic responded at 11 centimetres of DuBois' coil, and at 2:52 it did the same. At 2:55 the right sciatic responded at 13 centimetres, and again at 4:18 with the same strength of the current. At 4:18 the left sciatic responded at 11 centimetres. At 6:17 the left responded at 11 centimetres, and the right sciatic at 8.5 centimetres.

Frog. Weight 15 grammes. At 4:48 P. M. .01 gramme of strychnia was injected under the skin of the lower jaw. At 4:48:30 general tetanus. At 4:53, both sciatics irritable at the weakest current. At 5:00, dead—both sciatics respond to the weakest current. At 5:30 both sciatics respond to 4 centimetres.

Frog. Weight 14 grammes. At 10:44 A. M. received .05 gramme of strychnia into the lungs. At 10:44:30 tetanus. At 10:52 dead. At 10:53 the sciatics respond at 14.5 centimetres, DuBois' coil. At 11:53 the sciatic responds at 15 centimetres. At 11:20, sciatic nerve responds at 7 centimetres. At 11:30, sciatic responds with secondary coil pushed over primary coil. At 12 M. the same phenomena.

Frog. Weight 18.5 grammes. At 10:49 A. M. 1 gramme of strychnia was injected into the lungs. At 10:49:10, tetanus. At 10:58, heart stopped. At 10:59, sciatic responds at 14.5 centimetres. At 11:02, the same. At 11:19, sciatic responds at 7 centimetres. At 11:30, sciatic responds with secondary coil pushed over primary coil. At 11:52 and 12 M. the same phenomena were observed.

On the authority of the large number, and the universally constant results of these experiments, I think we are not presumptuous in asserting our conviction that strychnia does not paralyze the motor nerves. It must be remembered that if a galvanic irritation is applied too frequently, the neurility of a nerve will be destroyed, and you will fail to elicit a response,

independently of any poison. This, too, may be another reason that so many experimenters have decided the converse of our proposition.

Dr. Sigmund Mayer,* Assistant to the Physiological Institute at Prague, and Dr. Wilhelm Schlesinger, are the only two who have given us any definite information as to the effects of strychnia on the blood-pressure. Mayer used the nitrate, having dissolved .0008 grm. in every cubic centimetre of distilled water. He always gave tinct. opii before operating. He gave two cubic centimetres of his solution by means of the jugular vein, and noticed in thirty seconds a rise in the pressure, and a more frequent pulse. He reasoned that the rise was due, either to the opposition to the blood current, produced by contraction of the muscles, or else to the action of the carbonic acid on the nerve centres in the heart. He thinks that it is not due to any action of the muscles, nor to any "besoin de respirer," because he obtained the same results with animals rendered quiet by curare, and while using artificial respiration. But no change in the pulse was produced under these circumstances, hence the pressure and pulse are mutually independent. In very large doses the pressure did not rise. The rise, therefore, is due to an irritation of the vaso-motor centre, and a consequent contraction of the arteries. In large doses the vaso-motor centre is paralyzed. He says that after cutting the cord, either no rise, or only a very small one took place. Irritation of the cut ends of the crural nerves, produced a marked rise, according to the force of the current, from which he reasons that the ganglia between the sensory and vaso-motor nerves are not altered.

The pulse he found inconstant: if the pneumogastrics were cut, the pulse was not slowed. The pulse sometimes rose in connection with the tetanus, especially in non-curarized animals. He says that strychnia not only can excite the vaso-motor centre, but also the origin of the inhibitory fibres of the pneumogastrics. He finally concludes that the slowing of the pulse is only due to a want of oxygen, which reacts on the central ends of the pneumogastrics, for he could not obtain it if he used artificial respiration, though the pressure still re-

* *Stricker's Jahrb.*, 1872, 111.

mained far above normal. Hering* thinks that the quickening of the pulse, where it occurs, is due to apnœa (apnoë) produced by the frequent inspirations.

Dr. Schlesinger† published his article in 1874, in which he agrees with Mayer as to some of the main points above related. He says the rise in pressure is not due solely to the influence of a vaso-motor centre, situated in the medulla oblongata, but in a great measure is due to vaso-motor centres in the spinal cord, at present only determined for strychnia. On cutting the cervical cord, as is acknowledged by all, the pressure falls, but on exhibiting strychnia, a considerable rise in the blood-pressure is called forth. This rise, he says, both absolutely and relatively, exceeds the rise with the cord intact. Irritation which, after separation of the brain from the cord, no longer exerts an influence on the blood-pressure, after the injection of strychnia causes a marked rise. This result holds good for curarized animals, in whom the sympathetics and pneumogastrics have been cut. On consulting his results, I find that the highest rise he obtained was from fifteen millimetres, after cutting the cord, to one hundred and fifty millimetres after injecting strychnia, and the lowest rise was from twenty to fifty. He made all his experiments on rabbits, and used either the crural or the sciatic nerve for the purpose of irritation. He concludes that, first, vaso-motor centres exist below the fourth ventricle (*Rautengrube*) for strychnia at least, which the latter throws into inordinate action, whereas, otherwise they are quiet; second, strychnia by arrest of the respiration, in most cases shows a rise in pressure; third, on irritating a spinal nerve, a rise in pressure is induced, after cutting the cord.

I will now relate some of my own experiments with regard to the influence of strychnia on the nerves that govern the circulatory system. Upon warm-blooded animals, these amounted to twenty-three in number, of which I only detail two or three under each division. I used the same solution as before, namely, .01 gramme of the poison, dissolved in one

* *Stricker's Jahrbucher* for 1872

† *Stricker's Jahrbucher*, 1874, page 20

cubic centimetre of distilled water, with the aid of acetic acid, the solution being absolutely neutral. I also used DuBois-Reymond's induction apparatus, with Helmholtz's modification, for the purpose of irritation, as described above. In order to maintain artificial respiration, an apparatus was employed on the principle of Springel's blower. A metronome, attached to a Grove battery, by breaking and renewing the current at regular intervals caused a weight to fall on the tubing, thus imitating normal rhythmic respiration. The pulse and pressure were noted on the drum of Ludwig's registering apparatus, which was connected with his mercurial manometer. Throughout the tubing of the manometer, a saturated solution of bi-carbonate of soda was passed to keep the blood from clotting. The time was registered by means of a pen, connected with an electro-magnet; to this was joined Bowditch's clock, which, by breaking and renewing the current every second, caused the pen to inscribe the latter interval of time upon the drum. The strychnia was injected by means of a canula, placed in the external jugular vein, to which a hypodermic syringe accurately fitted. Great care was observed that no air entered the vein. The poison was in all cases injected toward the heart. The kymographion was connected to the external carotid artery by means of a canula bound in the artery. As the tetanic convulsions would influence the circulation, curare was used in just sufficient quantity to prevent any muscular movements.

Ex.—*Rabbit*, curarized. Tracheotomy; canula in right jugular. Carotid used for observation. *All nerves intact.* Artificial respiration, &c.

TIME.	PULSE.	PRESSURE.	TIME.	PULSE.	PRESSURE.
9:48:0	76	133	9:49:45	30	122
.01 gram. strychnia injected.			9:50:45	36	126
9:48:15	44	154	9:53:0	47	130
9:48:30	21	128	9:54:15	50	136
9:48:45	27	120	10:0:0	33	91
9:49:0	34	152			

I omitted saying above that I have made twenty-three (23) successful experiments with warm-blooded animals (rabbits, cats, and dogs) on the nerves governing the circulatory system

TIME.	PULSE.	PRESSURE.
10:5:15	36	89
10:9:15	34	100
.005 gram. strychniæ acetas injected.		
10:9:30	37	96
10:10:0	44	52
10:11:15	46	70
10:17:0	47	76
10:20:38	44	64
Observation suspended.		

Ex.—*Cat*, curarized. Tracheotomy; canula in left jugular; carotid used for observation. *All nerves intact*; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
12:26:45	52	86
.005 gram. strychniæ acetas injected.		
12:27:0	51	86
12:27:15	49	86
12:27:30	51	148
12:27:45	49	144
12:28:0	49	128
12:28:15	49	107
12:28:30	48	96
12:28:45	48	90
12:29:0	47	86
12:29:15	46	86
12:29:45	47	86
12:39:45	46	80
.005 gramme of strychniæ acetas injected.		
12:40:0	47	80
12:40:15	47	84
12:40:30	47	94.5
12 41:0	46	91
12:57:15	44	66
12:58:0	43	64
.01 gram. strychniæ acetas injected.		
12:58:15	36	59
12:58:30	36	50
12:59:0	30	40
1:0:0	25	23
1:4:0	26	36
1:5:0	25	38
1:25:0	21	27
1:29:45	14	20
Observation suspended.		

These experiments are simply given as examples. All the others are in perfect accord with them. They show that the general action of strychnia is to slow the pulse, and, by irritating the vaso-motor centre, to cause a contraction of the capillaries and small arteries, and thus to increase the arterial pressure. Following this irritation, if small doses are given there is a steady fall in pressure, until the animal dies (if the dose has been a lethal one). If large doses are given, there is a primary paralysis of the vaso-motor centre or centres, and the pressure constantly falls immediately. The fall in pressure cannot be due to a weakened heart, since strychnia does not affect the muscular system, as proved by Vulpian* and confirmed by many experiments that I myself have made. To what is the decrease of the pulse frequency, then, due? It

*Archives de Physiologie, etc., p. 118.

may be due either (*a*) to a central, or (*b*) to a peripheral stimulation of the pneumogastries, or (*c*) to action on the excito motor ganglia of the heart itself. It will be remarked that sometimes, on injecting the poison, the pressure does not rise, or rises only very slightly after having given several doses previously. This, I think, is due to the fact that the nerve centres have lost, to a great degree, their power of responding. The animal's heart still beats, but practically it is dead.

In order to determine whether the phenomena above stated were due to a central stimulation of the vagi, I cut both these nerves in the neck, thus entirely removing their central action, but leaving their peripheral action free to exercise its power.

Large rabbit, curarized. Tracheotomy; canula in right jugular; carotid used for observation. *Both vagi cut*; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
12:00	67	120
.005 gram. strychniæ acetas injected.		
12:0:15	63	145
12:0:30	46	200
1:0:45	50	182
12:1:15	50	170
12:2:4	60	160
12:3:15	47	100
A clot occurring in the carotid canula, some time was consumed in removing it.		
12:23:15	44	82
12:23:42	44	76
12:25:12	47	90
.005 gramme strychnia injected.		
12:25:30	46	89
12:25:45	44	74
12:26:0	46	39
12:26:15	34	32
12:26:45	34	26
12:27:15	32	26
12:27:30	30	26
12:28:30	30	26
Observation suspended.		

Little kitten, curarized. *Both pneumogastries cut in neck*; tracheotomy; artificial respiration; canula in left jugular; carotid used for observation.

TIME.	PULSE.	PRESSURE.
5:6:15	54	118
.0025 gramme strychnia injected.		
5:6:30	53	98
5:6:45	46	69
5:7:0	22	48
5:7:15	17	44
5:7:30	31	38
5:7:45	29	37
5:8:0	28	34
5:8:15	26	30
5:10:15	17	16
5:10:30	16	14
5:10:45	15	14
5:11:0	14	12
Observation suspended.		

The same result as to both pulse and pressure follows; hence strychnia does not decrease the pulse through central

stimulation of the vagi. Now, it is well known that atropia has the power of paralyzing the ganglia at the peripheral ends of the pneumogastrics, and thus removing their inhibitory action on the heart. I administered a solution of the sulphate of atropia by means of the jugular vein, in just sufficient quantity to effect this paralysis. To determine when I had attained this result, I applied a strong induction current to the course of the pneumogastrics. When these had no effect on the cardiac action, it was evidence of the paralysis of these ganglia.

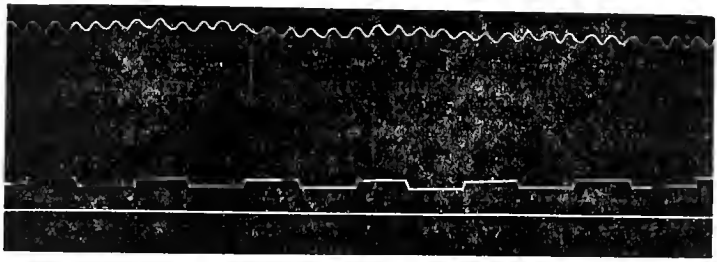
Rabbit. Canula in left jugular; carotid used for observation; pneumogastrics paralyzed by atropia, as tested by strong currents; curare; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
5:32:0	74	108
.00125 gramme strychnia injected.		
5:32:15	55	126
5:32:30	54	124
5:33:0	54	120
5:33:30	54	117
5:34:0	52	112
5:34:30	50	108
5:35:0	53	102
5:36:0	53	102
5:37:0	54	100
5:38:0	53	101
5:39:0	47	94
.00125 gramme strychnia injected.		
5:39:15	43	88
5:39:45	45	80
5:40:30	44	70
5:41:30	46	72
5:42:30	45	74
Observation suspended.		

Rabbit. Canula in left jugular; carotid used for observation; pneumogastrics paralyzed by atropia, as tested by strong currents; curare; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
4:45:30	59	80
.0025 gramme strychnia injected.		
4:45:45	57	92
4:46:0	51	94
4:46:30	51	94
4:47:15	47	82
4:48:0	49	75
4:49:0	48	78
4:50:0	49	78
4:51:30	51	83
.0025 gramme strychnia injected.		
4:51:45	50	74
4:52:0	46	50
4:53:0	44	46
4:53:45	45	58
4:54:30	45	60
4:56:0	47	70
4:59:30	47	78
.0025 gramme strychnia injected.		
4:59:45	46	64
5:0:0	41	44
5:1:0	42	46
5:2:15	42	54
5:11:15	41	50
5:15:0	41	45
5:18:0	38	36
Observation suspended.		

These experiments show that strychnia does not exert its influence through any stimulation of the regulator ganglia. Therefore, by a process of exclusion, negatively, it must affect the excito-motor ganglia of the heart. I now propose to prove this positively in the case of mammals, and later, also, in the case of frogs. I cut the pneumogastrics, the sympathetics and the depressor nerves of Ludwig, in the neck, and placed the heart beyond the control of the acceleraotrs and the vaso-motor centre in the medulla oblongata, by severing the spinal cord just below the medulla. It will be observed that the pulse is slowed steadily; hence, I think, proving conclusively that the seat of the decreased pulse-frequency is in the ganglia of the heart.



NOTE.—It may not be amiss for me to state the mode in which these results were recorded on the drum, and the method pursued in counting them. In order to do this the more clearly, I insert a specimen of one of the tracings. The drum is covered with smoked paper, and, when the experiment is finished, this paper is cut off and passed through a simple varnish made of shellac and alcohol in proper proportions. The broken line is that which was recorded by the pen which marked the time, each break denoting one second. The straight line is the abscissa, and the highest line, of course, marks both the pulse and the arterial tension. The mode of procedure is to mark off a certain number of seconds; then to erect ordinates at the beginning and end of said time, and count the pulse between them; then, from the abscissa and on the ordinates, to measure the height of the blood tension in millimetres—of course doubling the result, since the manometer consists of a tube, U-shaped, having two columns of mercury. When the tension was very uneven, I erected several ordinates, and then made an average of the different results.

Cat, curarized. *Cord cut* between the atlas and occiput (verified by post-mortem examination). Hemorrhage from the vertebral arteries checked by bovista. *All the cardiac nerves in the neck cut;* cannula in right jugular; carotid used

for observation. The sciatic nerve prepared. DuBois-Reymond's induction coil used for irritation. Tracheotomy; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
4:55:30	54	18
.0025 gramme strychnia injected.		
4:55:45	54	16
4:56:0	46	15
4:56:15	46	16
4:56:45	43	20
Irritation at 0 for 7″.		
4:57:0	43	22
4:57:15	43	27
4:57:30	43	27
4:58:0	40	25
Irritation at 0 for 15″.		
4:58:15	40	23
4:58:30	40	21
Observation suspended.		

Cat. *Cord cut* between the atlas and occiput (verified by post-mortem). Hemorrhage checked with bovista. *Vagi, sympathetics and depressors of Ludwig cut* in the neck. Sciatic prepared; curare; tracheotomy; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
4:56:0	53	58
.0025 gramme strychnia injected.		
4:56:15	53	58
4:56:30	47	64
4:56:45	48	90.5
4:57:0	48	114
4.57:15	47	111.5
4:57:30	46	97
4:58:15	45	77
4:59:0	44	70
5:1:30	50	78
5:2:30	52	81
5:3:30	52	81
.0025 gramme strychnia injected.		
5:4:15	53	84
5:4:30	54	88
5:4:45	46	79
5:5:45	43	70
5:6:45	45	62
5:7:15	46	62.5
.0025 gramme strychnia injected.		
5:7:30	45	65
5:7:45	46	70
5:9:15	41	64
5:9:45	41	62
5:11:0	41	56
5:13:45	43	55
5:14:45	48	63
5:25:30	46	56
Irritation of sciatic at 1 centimetre.		
5:25:45	45	56
5:26:0	43	57
5:26:45	43	54
Irritation at 0 for 5″.		
5:27:0	43	58
5:27:15	44	64
5:27:30	46	70
5:33:0	37	66
5:35:0	38	54
5:36:30	26	46
Irritation at 0 for 5″.		
5:36:45	29	56
5:37:0	29	50
5:41:15	31	48
5:42:0	32	48
5:43:0	23	48
5:44:0	23	46
.01 gramme strychnia injected.		
5:44:15	25	47
5:44:30	22	42
5:45:15	20	44
5:46:15	23	48
5:46:45	23	46
5:47:0	23	44
Observation suspended.		

Cat, curarized. *Cord cut* between atlas and occiput (verified by post-mortem). Bleeding checked by bovista. *All the cardiac nerves in the neck cut.* Canula in right jugular; carotid used for observation; sciatic prepared, etc.; tracheotomy; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
4:5:45	51	34
.0025 gramme strychnia injected.		
4:6:0	51	30
4:6:30	55	36
4:6:45	48	66
4:7:0	45	58
4:7:15	44	58
4:7:30	43	45
4:8:0	40	38
4:8:30	39	29
4:8:45	37	25
4:15:45	30	20
Sciatic irritated at 1 cent. for 29″.		
4:16:0	30	20
4:16:15	33	20
4:16:30	36	20
Observation suspended.		

Cat. *All cardiac nerves in neck cut.* *Cord cut* between the atlas and occiput (verified by post-mortem). Curare; artificial respiration; tracheotomy, etc.

TIME.	PULSE.	PRESSURE.
4:11:0	62	54
.001 gramme strychnia injected.		
4:11:15	59	54
4:11:30	55	54
4:14:48	45	22
.001 gramme strychnia injected		
4:15:55	48	22
4:15:10	38	22
4:15:40	42	36
4:16:55	42	26
4:17:10	43	26
4:17:55	43	22
Observation suspended.		

Cat. *Cord cut* between atlas and occiput (verified by post-mortem). Hemorrhage stanched by bovista; canula in right jugular; carotid used for observation. *All cardiac nerves in the neck cut.* Sciatic nerve prepared; DuBois-Reymond's induction coil used for irritation; tracheotomy; artificial respiration, etc.

TIME.	PULSE.	PRESSURE.
4:20:15	42	44
.0025 gramme strychnia injected.		
4:20:30	42	45
4:20:45	41	47.5
4:21:15	39	44
4:21:45	38	35
4:22:0	38	32
4:22:45	37	29
4:23:0	37	30
Irritation at 0 for 15″.		
4:23:15	36	45.5
4:23:30	39	31
4:24:0	40	31
4:24:45	41	29
4:27:30	44	36
4:28:15	44	39
4:29:15	44	37
4:30:15	42	38
Irritation at 0 for 15″.		
4:30:30	44	38

TIME.	PULSE.	PRESSURE.
4:31:0	44	39.5
4:33:15	42	42
4:34:0	44	42
4:34:15	45	42
4:39:15	44	38
4:40:30	44	34
Irritation at 0 for 15".		
4:41:0	44	38
Observation suspended.		

From the last series of experiments, and, indeed, from all others that I have related, I think we are warranted in concluding that Mayer was wrong in his theory that the slowing of the pulse is due to the presence of carbonic acid, irritating the roots of the inhibitory fibres of the vagi; and for this reason: In the experiments above narrated, I have invariably observed this slowing of the pulse, and yet, in all of them, I have used artificial respiration, by this means inducing a state of apnœa, and not allowing the carbonic acid to collect—nay, rather, causing an excess of oxygen.

But it will be seen at a glance that these experiments show more. They were also undertaken to prove or disprove the correctness of Schlesinger's theory. It will be remembered that Schlesinger said, that there exist in the cord vaso-motor centres for strychnia, which are habitually in a state of quietude; but that if the poison be administered, they are thrown into violent action. He said that, after cutting the cord, and severing all the cardiac nerves in the neck, if he administered strychnia and then applied a galvanic irritation to a sensory nerve, that he could obtain a higher rise of arterial pressure than he could if he administered strychnia, with all the animal's cardiac nerves and cord uninjured. I confess I cannot obtain identical results. I have a very marked rise on irritating a sensory nerve, but nothing to compare with what Schlesinger says he obtained. From my own experiments I am led to believe that he is right in his theory, and our results differ only in degree, not in kind. With all respect, I would venture to suggest that perhaps he did not have his cord entirely severed. If even a very few fibres are left, they are sufficient to convey the irritation to the medulla oblongata. I have had evident proof of this in many experiments. I have been almost certain that the cord was entirely severed, and have obtained the great rise that Schlesinger records, but on making an examination post-mortem, I have found some

fibres still intact. Indeed it is by no means an easy surgical operation, working in the dark as one must, to strike the exact line between the atlas and the occiput, and then to make a clean cut of the whole cord and rapidly to insert the bovista, so that the animal may not lose any blood, so valuable here where a very severe shock has to be sustained by the animal; so that the most skillful operator will often fail in accomplishing his object, and need not blush at his failure either.

Dr. Carl Heinemann* has made some very interesting experiments on the influence of strychnia on the frog's heart. He concludes that the heart is slowed, and that the tetanus and the decrement of the pulse are mutually independent; also that the vagi are not paralyzed, since he could cause immediate diastolic arrest, by applying a galvanic stimulation. Mayer also found that the vagi are not paralyzed.

I now relate some of my own experiments on this subject, from which I conclude that the heart is slowed, and that the vagi are not paralyzed. My method consisted in raising the vagi on silk threads, and observing at what strength the induced current would slow the heart. Then I gave the poison. Now if the vagi are paralyzed, the same strength of current, or even the strongest current, should not have any effect on the heart beats; but, on the other hand, if they are not affected, the same phenomena should be presented as before giving the poison—that is to say, the same phenomena with regard to the cardiac pulsations. The current was prevented from spreading, by separating the nerves completely from any connection with the surrounding tissue.

Green Frog. Weight 90 grammes. Chest opened and vagi raised on silk thread. At 4:53 P. M., the heart beats 11 in 15 seconds. At 4:54 received .01 gramme strychnia injected into the abdomen. At 4:54:30 tetanus. At 4:56 heart beats 7 in 15 seconds. At 4:57, irritation of the right vagus slows the heart with DuBois' coil marking five centimetres. At 5:4 the heart beats 7 in 30 seconds. At 5:5 irritation at 5 centimetres slows the heart. At 5:12 the heart beats 4 in 30 sec-

* Beitrage zur Physiologie des Herzens. *Virchow's Archiv*, Bd. 33, pp. 394—410.

onds. At 5:13 irritation at 4 centimetres stops the heart. At 5:17, frog dead. At 5:19 the sciatic responds at 12 centimetres; at 5:25, thrusting probe down the spine elicits a marked response.

Green Frog. Weight 95 grammes. At 4:4 P. M., the heart was beating 15 times in 30 seconds. At 4:5, .005 gramme strychnia was injected into the abdomen. At 4:6:30, the heart beats 9 times in 30 seconds. At 4:8, the right vagus was irritated at 5 centimetres, DuBois' coil, and slowed the heart considerably. At 4:8:30, opisthotonos. At 4:9, .005 gramme of strychnia was injected into the abdomen. At 4:10, irritation of right vagus slows the heart. At 4:14, heart beats 4 in 30 seconds. At 4:16, irritation at 5 centimetres slows the heart. At 4:18, heart beats 4 in 30 seconds; at 4:19, irritation at 5 centimetres slows the heart one-half. At 4:26, heart beats 6 in one minute; irritation at 5 centimetres causes the heart to beat once in 15 seconds. At 5:2 heart beats 2 in 15 seconds; irritation at 5 centimetres makes the heart beat 2 in 20 seconds. At 5:22, all motion of heart and muscles had ceased. At 5:24 the sciatic nerve responds at 7.9 centimetres.

I made also some similar experiments on warm-blooded animals, one of which I now relate. The observations were made with Middledorpff's needle. This is a long slender silver needle which is thrust into the ventricle, using the walls of the thorax as a fulcrum. At each systole of the heart, of course the needle will move, and as it is of the nature of a lever of the first class, the movement of the heart will be greatly exaggerated, and may be counted with perfect ease.

Little Rabbit. Tracheotomy. Canula in jugular vein. Vagus prepared. DuBois' induction coil used for irritation. Middledorpff's needle thrust into the heart. At 5:34 P. M. the heart was slowed by irritating the vagus with the coil marking 10 centimetres. At 5:38, .005 gramme of strychnia was injected into the jugular vein. At 5:48, the heart was slowed by irritation of the vagus, with the coil at two centimetres. At 5:58, the same. At 5:59, the heart was considerably slowed by injecting .005 gramme of strychnia. The chest was now opened. At 6:00 the heart was slowed by irritation of the vagus, with the coil marking two centimetres. At 6:5 the

same. At 6:40 the heart was slowed at one centimetre. Observation suspended.

In order to satisfy myself more fully of the correctness of my results, I performed what the Germans call Goltz's *Klopfversuch*. Of course the sensory nerves in and around the abdomen can convey an impression to the pneumogastries; and if the nerves are not hampered in their action, they will reply by slowing the heart. I laid open the abdomen, and then by striking a sharp, moderately quick blow on its contents, I produced at times a slowing of the heart, and at times a complete stoppage of the pulsations of that organ, thus proving the correctness of my former observations, namely, that the pneumogastrics are not paralyzed by strychnia.

Green Frog. Weight 103 grammes. At 12:13 P. M., .005 grammes of strychnia was injected under the skin of the back. At 12:29 the heart beat 12 times in 30 seconds. The *Klopfversuch* was then performed At 12:30, the heart beat nine times in 30 seconds. At 12:52, the heart beat nine times in 30 seconds; blow struck; at 12:53, heart beats five in 30 seconds. After death the sciatic nerve responds at 15 centimetres.

Dr. J. Steiner,* Assistant at the Physiological Institute at Halle, has also studied the action of strychnia on the frog's heart. He observed that when a one-third per cent. solution of the nitrate of strychnia was placed on the anterior side of the heart, the reduction of the heart beats was very small, but when placed on the posterior side of the heart the decrement was nearly one-half. He found the same result followed when he had previously given the frog atropia, inferring therefrom that the slowing was not due to irritation of the peripheral ends of the vagi. He then put the sinus venosus, separated from the action of the heart, into his solution of strychnia. In a short time it stopped, and was mechanically inexcitable. He then placed the auricles and ventricles, but without the sinus venosus, into the solution of strychnia, and found movements to continue for a long time; after they had stopped a

* Zur Innervation des Froschherzens. *Reichert u. DuBois' Archiv*, 1874, No. 4, p. 474.

mechanical irritation caused continuous beats for half an hour. He infers that the point of attack (*Angriffspunkt*) of strychnia is in the ganglia situated in the sinus venosus, and that it does not affect the ganglia in the septum or the auriculo-ventricular groove. I have repeated these experiments of Steiner in ten different instances, and have arrived at the same conclusion. In order not to anticipate my results, and to follow out a preconceived logical sequence, I refer to page 644, where an example of these experiments will be found detailed.

Let me now relate, in order, some of the experiments I have made on the influence of strychnia upon the cardio-nervous apparatus. These in number amount to twenty-four.

The action of strychnia is to slow the heart so palpably that I only relate one of my experiments in proof of this fact alone. Reference to very many other experiments, that were performed to prove other physiological phenomena, will give evidence of the same thing.

Frog. Weight 15 grammes. Sternum removed. At 4:31 P. M., heart beat 14 in 15 seconds. At 4:33, .005 gramme of strychnia was injected under the skin of the lower jaw. At 4:34 heart beat 2 in 15 seconds. At 4:35 heart beat 7 in 15 seconds. At 4:36, opisthotonos. At 4:37, heart beat 3 in 15 seconds. At 4:40 it beat 4 in 40 seconds. At 4:42, 4 in 15 seconds. At 4:48, 5:0, and 5:20 the same, and at 5:22 it beat 3 in 15 seconds, when the observation was suspended.

(It is to be noted that in all my experiments the heart has invariably stopped in diastole.)

Pursuing the same system as I had done before, I first endeavored to ascertain whether this pulse-slowing was due to a stimulation of the central ends of the vagi. I cut the said nerves on both sides, and after doing so I counted the heart beats; I then gave the poison and again counted the heart beats, and, as the following experiments show, observed a very decided slowing of the pulse; the heart appeared as if it was tired of beating, to use a familiar phrase; there never seemed to be a full energetic systole, and long diastolic pauses occurred. These results prove, then, that the phenomenon under consideration at present is not due to any hyper-excitability, or increased action of the central ends of the vagi.

Green Frog. Weight 140 grammes. Chest opened and both pneumogastrics cut. Heart beat 16 in 30 seconds. At 5:29 P. M., .01 gramme of strychnia was injected into the lungs. At 5:32, the heart beat 10 in 30 seconds. At 5:34 opisthotonos occurred. At 5:35 the heart beat 8 in 30 seconds. At 5:45 it beat 5 in 60 seconds; at 5:50, it beat 2 in 30 seconds; at 5:54, 3 in 60 seconds, and the observation was suspended.

Green Frog. Weight 142 grammes. Both pneumogastrics cut. Heart beat 12 in 15 seconds. At 4:40 P. M., .005 gramme of strychnia was injected into the lungs. At 4:44 tetanus. At 4:45, heart beat 5 in 15 seconds. At 4:48 the heart beat 6 in 30 seconds. At 4:49 opisthotonos. At 4:50 the heart beat 4 in 30 seconds; at 4:52 it beat 7 in 60 seconds; at 5:5 it beat 3 in 30 seconds and at 5:34, 4 in 60 seconds. Observation suspended.

Again, the pulse-slowing might be due to a stimulation of the peripheral ends of the pneumogastrics; for the purpose of determining this point, either positively or negatively, the annexed experiments were undertaken, with the effect of proving that the action of the poison is not located in this part of the cardio-nervous system. Here I paralyzed the peripheral ends of the vagi by the administration of atropia.

Large Green Frog. Weight about 150 grammes. Vagi paralyzed by atropia, as tested by strong currents (.01 gramme of atropia administered). At 11:50 A. M. the heart beat 14 in 30 seconds. At 11:51, .005 grammes of strychnia was injected. At 11:53, tetanus. At 11:54, heart beat 8 in 30 seconds; at 11:55, opisthotonos. At 11:56 heart beat 7 in 30 seconds. At 11:58, heart beat 5 in 30 seconds. At 12:03, heart beat 4 in 30 seconds. At 12:05, heart beat 3 in 30 seconds. At 12:07, heart beat 3 in 30 seconds. Observation suspended.

Green Frog atropinized; vagi tested as before. At 4:51, heart beat 13 times in 15 seconds. At 4:52, .0025 gramme of strychnia was injected into the abdomen. At 4:53, tetanus. At 4:55, heart beat 7 in 15 seconds; 4:57, heart beat 6 in 15 seconds; at 5:00, heart beat 3 in 15 seconds. Observation suspended.

To briefly recapitulate then, we have proved that strychnia slows the pulse: that this decrement is not due to any excitation of either the central or peripheral ends of the pneumogastrics: and we know it is not due to any action of the poison on the muscular structure. The inference then is that this phenomenon takes place through action on the cardiac nerve centres proper. This is proven positively by the experiments I now relate. These results also prove that the exact situation of these cardiac ganglia that are affected by the poison can be isolated to the sinus venosus, and that the centres situated in the auriculo-ventricular groove are not at all influenced by the drug.

Green Frog. Heart removed from the body, and the sinus from the ventricles, and observed.

I. At 4:48, heart beat 13 in 15 seconds. At 4:49, the sinus was placed in a solution of strychniæ acetas, at a temperature of 65° F., and the beats decreased to 7 in 15 seconds. At 5 P. M., the sinus beat 2 in 15 seconds. At 5:1:30, it had stopped beating and was mechanically unirritable.

II. At 4:49 the ventricles were placed in another dish containing strychnia of the same strength and temperature as in I. At 5:00, ventricles beat 10 in 15 seconds. At 5:1:30, ventricles beat 8 in 15 seconds. At 5:5, they beat 7 in 15 seconds. At 5:20, stopped beating, but mechanically irritable.

Frog. I. The sinus venosus was cut from the heart, and observed to beat 60 times a minute. At 12:24 P. M., it was laid in a solution of the acetate of strychnia. At 12:29 it beat 20 per minute, at 12:30 it beat 16 per minute. It could not be mechanically irritated to contract at 12:40.

II. At 12:24 the remainder of the heart was also laid in a solution of strychnia; it beat 28 per minute at 12:25, and 16 per minute at 12:29. At 12:40, it was mechanically irritable.

Large Frog. Heart removed. I. Sinus cut from the rest of the heart at 12:55 P. M., when it beat 52 per minute. At 12:56 it was placed in the solution of strychnia. At 12:57 it beat 48 per minute. At 12:59, it beat 44 per minute. At 1:0 it beat 32 per minute. At 1:4, it could not be mechanically irritated to contract.

II. At 12:56 the rest of the heart was placed in the stryeh-

nia solution; it beat very regularly for some time and then stopped. For a long time it could be excited to contract by mechanical stimulation.

It is to be remembered that in all my experiments under this head, of which the above serve as examples, that great care was taken in removing the heart itself, and the sinus removed from the rest of the heart; and also that the temperature of the strychnia solution was made exactly the same as that of the water from which the frogs had been taken.

I now propose to discuss what effect strychnia has on the nerves that govern the function of respiration.

In 1866 Leube* thought that perhaps the fatal effects of strychnia poisoning were produced by some action on the lungs, and that if an increased supply of oxygen was given to the animal, the working of the poison could be prevented. He concluded, from an elaborate series of experiments, that artificial respiration was very beneficial, and in some instances could save, even when the lethal dose was exceeded by three milligrammes. A little later, in 1868, Uspensky† confirmed Leube's results with especial reference to brucia. Rosenthal‡ followed, and agreed with Leube. In the year 1872, M. Brown-Séquard obtained the same results as Leube and Rosenthal, but explained the *modus operandi* differently. He thinks that the beneficial action ought to be ascribed to an irritation of the pulmonary branches of the pneumogastrics, thus exciting a reflex inhibitory action. Carbonic acid irritates these more powerfully than air, and thus has a more powerful inhibitory action. So he recommends the forcible inflation of carbonic acid. Its action is that of a simple irritant, and it produces or arrests convulsions, according to the part of the nervous system on which it acts.§ Dr. Rossbach was unable to obtain this effect on the strychnia convulsions, and, at his suggestion, Dr. Jochelsohn‖ began a series of ex-

* *Archiv f. Anatomie, etc.*, s. 632, 1866.
† *Archiv f. Anatomie*, s. 523, 1868.
‡ *Archiv f. Anatomie*, s. 201, 1865.
§ *Arch. de Phys.*, 1872, p. 204, *et seq.*
‖ *Lond. Med. Record*, April 30, 1873
¶ *Pharmakol. Untersuch.*, 1873, p. 93, *et seq.*

periments. His conclusions were, that in no case was it beneficial, and in some positively injurious. He says: "Die kunstliche Respiration weder einen Einfluss auf die erhaltung der Thiere, die mit Strychnin vergiftet sind, noch auf die Intensitaet und Dauer der Strychnin Kraempfe auszuueben im Stande ist, sondern das die Leben der Thiere nur um 3–4 Stunden verlaengern kann."*

In January, 1874, Dr. Wilhelm Filehne† worked up this subject, and has given us a valuable paper. From numerous experiments of his own, and from those of all others except Jochelsohn, he assumes that artificial respiration has the power of arresting the convulsions, but seeks to find the *modus medendi*. He proves that Brown-Séquard and those of his school are wrong in supposing it to be due to any influence transmitted through the pneumogastrics, for he cut the latter nerves, and obtained the same results, or at least preserved the life of the animal for thirty-six hours. Doses that were lethal in animals with the vagi cut, were powerless in animals with the vagi intact, if artificial respiration was used. "When the period has come when the heart begins to tire, an important disturbance of the circulation begins. In vain a stronger and more energetic inflation (*Einblasung*) can only force a little blood to the breathing centre: the blood supply is not sufficient to affect the breathing centre—that is to say, to produce apnœa, and to strongly irritate the motor centre. The animal breathes spontaneously, becomes later tetanic, and finally dies through lameness of the heart's action."‡ M. Vulpian§ has no faith in the influence of artificial respiration. He apparently, however, bases his opinion on one experiment; at least he does not mention having made any more. He gave four milligrammes of strychnia, and applied artificial respiration in twelve minutes, and did not note any decrease in the number of spasms. The animal died in a couple of hours.

I will now relate the experiments I have made to determine the effect of strychnia on the nerves governing the respiratory

* *Loc. cit.* p. 96.

† *Archiv f. Anat.*, 1874, p. 361, *et seq.*

‡ *Loc. cit.* p. 373.

§ *Lecons sur l'appareil vaso-moteur*, a Vulpian, Paris, 1875, p. 689, *et seq.*

movements. These were *fourteen* in number, and were performed on rabbits. The animal was simply tied in Czermak's rabbit-holder, tracheotomy performed. A T-shaped tube was introduced into the trachea, and a rubber tube then connected with this tracheal canula. On the distal end of the tube was placed a small, air-tight drum, connected on its upper surface with a long, light pen, after the manner of a lever of the first class. As the animal respired—by the contraction of the drum—the upper surface of the drum was drawn down, and the point of the pen was elevated, and the contrary, of course, took place when the animal made an expiration. The pen inscribed the result on Ludwig's registering apparatus.

Rabbit. Left jugular prepared; tracheotomy. At 12:22:30 P. M. there were 11 respirations in 15 seconds. At 12:23:0, .0015 gramme of strychnia was injected toward the heart. At 12:23:15 there were 8 respirations in 15 seconds. At 12:26:30 there were 3 respirations in 15 seconds, and the same at 12:26:45. At 12:27:0 there were 6 respirations in 15 seconds, and at 12.27:15 tetanic struggles occurred, and the attempts at respiration were almost ineffectual. At 12:27:30 there were 7 respirations in 15 seconds. At 12:27:45 there were 6 respirations in 15 seconds. At 12:28:0 there were 5 respirations in 15 seconds. At 12:28:15 there were 4 respirations in 15 seconds. At 12:28:45 there were 4 respirations in 15 seconds. At 12:29:0 there were 5 respirations in 15 seconds. At 12:30:45 there were 3 respirations in 15 seconds. At 12:31:0 there were 6 respirations in 15 seconds. At 12:31:15 there were 5 respirations in 15 seconds. At 12:31:30 opisthotonos occurred, and for a period of 15 seconds no respiratory movements took place. At 12:32:0 there were two short inspirations, and then one expiration, and the animal was dead. The details of this experiment sufficiently illustrate all the rest of the series.

From these experiments it will be seen that the action of strychnia is to decrease the number of respiratory movements. My theory with regard to this result is, that at the very beginning the heart beats rapidly, and the respirations are accordingly quickened—that is, for the first few seconds only; but soon the heart loses its power by the action of the poison

on the cardiac excito-motor ganglia, and consequently less blood flows to the respiratory centre, and, as a result, it does not fully perform its function, and the respirations are less frequent. When the respiratory centre becomes paralyzed, as it does later, or if enormous doses of the poison are given, then the phenomena of inspiration and expiration are stopped immediately. Of course this is only true where the dose has been toxic, but not lethal, or when artificial respiration has been employed: when the latter is not used, a large part of the effect is undoubtedly due to the tetanic contraction of the muscles of respiration. At every available lull in the spasms the animal appears to take in deep inspiration, thus seeming to desire to induce, by natural means, the condition we afterwards induced by artificial means, to-wit: a state of apnœa.

The objection may be urged that the slowing of the respirations is due to some action on the pneumogastrics. In order to meet this objection, I cut these nerves, and observed the same effect, as seen below. This might have been presupposed from our former conclusions as to the negative effect of the poison on the vagi, which we reached in our experiments as to the influence of the poison on the nerves governing the circulation.

Rabbit. Pneumogastrics cut; tracheotomy, etc. At 6:0:15 there were 18 respirations in 15 seconds; .00125 gramme strychnia was injected hypodermically. At 6:0:30 there were 14 respirations in 15 seconds. At 6.1:0 there were 13 respirations in 15 seconds. At 6:2:0 there were 15 respirations in 15 seconds; nystagmus very marked; pupil dilated. At 6:3:0 there were 14 respirations in 15 seconds. At 6:4:0 there were 16 respirations in 15 seconds. At 6:5:0 there were 15 respirations in 15 seconds. At 6:6:0 there were 16 respirations in 15 seconds. At 6:9:0 there were 16 respirations in 15 seconds. At 6:10:0 there were 14 respirations in 15 seconds. Observation suspended.

Having proceeded thus far in our experimentation, it is incumbent upon us to take up the consideration of the advantage or disadvantage of artificial respiration in saving the lives of animals poisoned with strychnia. Here, even more than anywhere else in our discussion, we have the two radi-

cal views presented to us; on the one hand Filehne asserting the wonderful power of artificial respiration in cases of strychnia poisoning, and, on the other hand, Jochelsohn's announcement that artificial respiration is perfectly useless, nay, indeed, injurious in such cases. In order to meet every point of dispute between these gentlemen, I obtained all my rabbits from the same stock, born and nourished in the same burrow, of nearly the same age and size, and, in a majority of cases, of the same litter. During our experiments they were all subjected to precisely the same influences, the same degree of heat and cold, the same food, etc. I never used an animal twice for experimentation. I first made myself thoroughly conversant with the different doses that were harmless, toxic and lethal in these rabbits, and then proceeded as narrated below.

Reference to page 631 will show by what means artificial respiration was maintained in these experiments.

I made *twelve* experiments to obtain these results, of which I narrate several.

Rabbit. Weight 1240 grammes. Tracheotomy. At 4:12 P. M., .00125 gramme of strychnia was injected hypodermically. At 4:14 convulsions occurred, and nystagmus was marked. At 4:20, opisthotonos did not again occur, but there were very violent convulsions. At 4:26 the severity of the convulsions decreased. At 4:34 the spasms became more intense. At 4:45 the convulsions had abated considerably. Artificial respiration was now stopped. Up to this time there were almost constant slight convulsive movements, but on stopping the artificial respiration, these stopped also, except occasional jerks on irritation. The animal now began voluntary respiratory movements; the respirations were quick, shallow and mostly diaphragmatic. At 5:6 there were only occasional contractions of single muscles. At 5:10 the respirations numbered 80 per minute. At 6:20 the breathing was regular and quiet, and assumed more of the costal type. At 10 A. M. the following day, the animal was quite lively, and ate voraciously.

Rabbit. Weight 1256 grammes. Tracheotomy. At 4 P. M. three (3) milligrammes of strychnia was injected hypodermically. At 4:1:30 tetanus. At 4:2 artificial respiration was

begun. The convulsions now gradually lessened to a mere heightened excitability and tremulousness, except at 4:30, when they became again marked on irritation: occasional attacks of trismus also occurred. At 4:34 artificial respiration was suspended, and when the animal had recovered from its apnœa, it breathed voluntarily and regularly at the rate of 56 respirations per minute. At 4:45 the artificial respiration was renewed: at first it appeared to increase the excitability, but soon the animal became perfectly quiet. At 5 P. M. it was difficult to induce convulsions, even by strong mechanical irritation. At 5:15 tremulousness in the hind legs; no convulsions. At 5:40 the artificial respiration was permanently suspended, and the animal released; no convulsions occurred: the animal rose, ran around the room, and seemed quite lively.

Rabbit. Weight 1250.5 grammes. Tracheotomy. At 4 P. M. seven (7) milligrammes of strychnia was injected hypodermically. At 4:10 tetanus occurred, followed by opisthotonos. Artificial respiration was now applied. The violent convulsions still continued for a few minutes, but when a full state of apnœa was attained, they became intermittent, and only occurred occasionally. Almost continuous trismus was observed. At 4:11 a violent convulsion; at 4:14 another. At 4:26 they had subsided to mere tetanic jerks. At 4:30, on stopping the artificial respiration, as soon as the animal had come out of its state of apnœa, violent and continued convulsions occurred. The artificial respiration was renewed immediately. At 5:0 no convulsions occurred, unless excited by external violence. At 5:45 tries to get away. At 6:0 the spasms were few, and occurring at long intervals. At 6:2 artificial respiration was suspended; decreased amount of irritability; on recovering itself from apnœa, the animal made voluntary respirations at the rate of 52 per minute. At 6:5, artificial respiration being again renewed, the rabbit again became convulsed in a moderate degree. At 6:15 artificial respiration was suspended entirely, and with only a few slight twitches. At 9:00 P. M. the animal was still living, but died during the night.

Rabbit. Weight 1271 grammes. Tracheotomy. At 3:40

P. M. three (3) milligrammes of strychnia was injected hypodermically. At 3:45 opisthotonos. Artificial respiration was now begun. At 3:49 violent convulsions. At 3:56 continued trismus occurs. At 4:0 convulsive waves sweep over the whole body. At 4:15 the convulsions are not so grave; it is rather a continued trembling. At 4:50 the convulsions are slight. At 5:00 the animal is very quiet; no convulsions occur unless mechanically excited. At 5:15 spasmodic contractions of the several muscles occur. At 5:40 no more convulsions; the animal tries to get away. At 5:42 the artificial respiration was stopped, and, on voluntary respiration taking place, the number of movements was found to be 100 per minute. The artificial respiration was then renewed at 5:45, and continued till 6 P. M., when it was stopped permanently. After this stoppage, the animal evinced only a heightened reflex sensibility, by giving occasional slight jerks on irritation. It died the next day at 9 A. M.

Rabbit. Weight 1251 grammes. Tracheotomy. At 3:35 P. M., there was .002 gramme of strychnia injected hypodermically. Tetanic signs rapidly appearing, artificial respiration was immediately begun. At 3:38 slight tetanus. At 3:40 opisthotonos. At 3:50 long-continued, almost incessant, trismus was noted. At 4:15 only occasional convulsive spasms took place. At 4:30 artificial respiration was stopped, but as no voluntary respiration took place, it was renewed. At 5:00 the heart was beating very rapidly, and on the animal making one or two attempts at voluntary respiration, the artificial respiration was stopped.

Rabbit. Weight 1326.5 grammes. Tracheotomy. At 2:25 P. M., .0025 gramme of strychnia was injected subcutaneously. At 2:27 tetanus occurred. Artificial respiration was now applied. At 3:28 opisthotonos; nystagmus marked. Violent convulsions continued, and at 3 the animal was dead.

Rabbit. Weight 1269.5 grammes. Tracheotomy. At 3:42 P. M., .0025 gramme strychnia was injected hypodermically. At 3:42, on the appearance of tetanus, artificial respiration was begun. At 3:46, pleurosthotonos; this passed off and was followed by single spasms, following each other at not very long intervals. At 3:48, trismus was marked. At 3:52,

alone remained. At 4, the artificial respiration was stopped and the animal lived for some time.

Rabbit. Weight 1315 grammes. At 3:28 P. M., .002 gramme strychnia was injected subcutaneously. At 3:30, tetanus occurred and artificial respiration was begun. At 3:35 a spasm occurred. At 3:36 convulsions occurred on the least irritation; the eyes were forcibly closed, the lids were raised with difficulty and nystagmus was extreme. At 3:44 excessive opisthotonos occurred. At 4:45, the animal was very quiet; at 5:13, the artificial respiration was stopped and voluntary, but quick respiratory movements ensued. A very slight spasm now occurred, and the artificial respiration was reapplied. Not a movement now occurred. At 5:25, the artificial respiration was permanently stopped and calm voluntary respirations took place, numbering 82 in the minute. The animal now lay perfectly quiet and did not even show heightened sensibility when touched. At 6, it was untied and presented no signs of tetanus whatever.

Now, I think these experiments plainly show that the middle way is not only safest, but also best. There can be no doubt that artificial respiration has a truly wonderful power in lessening the convulsions of strychnia-poisoning, and in prolonging the life of the animal, not for two or three hours, but for two or three days; and at the end of that time the animal may have died from other causes, tracheotomy for instance. Our own experiments will show, and any rational observer will acknowledge, that if you inject subcutaneously in a rabbit over two milligrammes of strychnia, the rabbit will rapidly die; now, by means of artificial respiration I have prolonged the life of the animal until the next day, nay far longer, and, what is more, the animal for many hours before its death has seemed very lively, eaten heartily, and not presented one sign of convulsions. Again, three milligrammes is an enormous dose of strychnia for a little rabbit, and should kill in a very few minutes, but by means of artificial respiration its life has been prolonged, not, as Jochelsohn says, for two or three hours, but for from twenty-four to thirty-six hours. Still further, a reference to the experiments before narrated, will show what truly frightful doses of the poison

have been given and yet have not proved fatal: this can be explained no other way than through the influence of artificial respiration.

Now, on the other hand, I have never had an animal live over forty-eight hours after receiving two or three milligrammes of the poison, with two exceptions; and it is to be noted that if, during the later stages of the convulsions, I stopped the artificial respiration for a minute or two (not more) the animal would lie still, but on again joining the respiratory tube to the tracheal canula, quite violent convulsions would ensue, but these, in time, would pass off rapidly and leave the animal in its previous quiescent state. So I say, that in toxic and even lethal doses, artificial respiration is life-saving, and, that in large doses its power is great in postponing the fatal issue, and that, perhaps, in the cases where death has taken place, it has resulted from other causes, and not from the strychnia; but that it possesses the almost miraculous power ascribed to it by Filehne, of saving life in every case, and under all circumstances, is going farther, in my judgment, than the facts of the case will warrant.

Is this good influence of artificial respiration due reflexly to an irritation of the pneumogastrics, or to a super-oxygenation of the blood? The method I have employed in deciding this question, is to take two rabbits, equal in every respect, cut the pneumogastric nerves in both, and inject the same dose of strychnia in both; then to establish in one a state of apnœa through artificial respiration, and to leave the other to its own resources. Now this cuts off all influence of the pneumogastrics, and so both rabbits should stand an even chance of life, or rather, I should say, of death, if the artificial respiration exerts its influence through reflex stimulation of the pneumogastrics as Brown-Séquard thinks; but on the other hand, if it is due to super-oxygenation of the blood, the animal that has artificial respiration should be saved and the other should die. As the experiments narrated below show, this latter circumstance takes place, hence excluding Brown-Séquard's theory. It has been my experience, that even after the pneumogastrics are divided, a state of apnœa can easily be induced and voluntary respiratory movements stopped by artificial respiration.

provided the insufflation be strong enough, and I cannot understand how it is possible that Brown-Séquard should state that such is not the case. Therefore it seems rational to me to suppose that the real and only influence of this artificial respiration is to keep the blood of the rabbit oxygenated, and the wheels of the animal economy in motion until the poison can be eliminated, which elimination takes place through the ordinary excretory channels, and not solely through the lungs.

(A.) *Rabbit.* Weight 1177 grammes. Both pneumogastrics divided at 10:12 A. M. At 10:15, .00125 gramme of strychnia was injected subcutaneously. At 10:24 tetanus. At 10:27, nystagmus marked. At 10:28 tetanus very pronounced. At 10:30, opisthotonos for fifteen seconds. At 10:33, opisthotonos for half a minute—death.

(B.) *Rabbit.* Weight 1165 grammes. Of the same stock, etc., as (A). Both pneumogastrics divided at 12:15 P. M. At 12:17, .00125 gramme of strychnia was injected hypodermically. At 12:19, artificial respiration was begun. At 12:22, heightened sensibility. At 12:24, tetanus. At 12:25, opisthotonos of short duration. At 12:26, nystagmus marked—violent convulsions now ensue. At 12:30, trismus was noted; convulsions have now almost entirely disappeared, except the trismus and a slight trembling of the thoracic muscles. At 1:5 general trembling; no marked convulsions appear. At 1:12 the trembling has disappeared except at the upper part of the sternum, and near the larynx. At 1:35, the animal was very quiet; not a sign of tetanus; at 1:45 it still remains quiet, and appears to attempt voluntary respiration. At 3, the heart is beating strongly and rapidly. At 3:5, the artificial respiration is stopped, the animal not showing signs of even heightened sensibility; the breathing is regular, and of course, slow, on account of the influence of the vagi being removed; the number of respirations is thirty-two per minute.

Having now examined the anatomical and physiological effects of strychnia on the brain, spinal cord and nerves, the question naturally arises: Has the time we have spent in these experiments been of any real practical value, and do our deductions show any results that will, in the future, lead to a more philosophical use of strychnia, or allow us to draw

any parallel between its effects and the allied ones of traumatic tetanus, or the convulsions of epilepsy? This we will leave others to decide. It is necessary for us to know the effects our medicines are going to produce in disease, before we can conscientiously and willingly administer them. Empirical knowledge is good, and undoubtedly the foundation of our great science was laid in empiricism, in many instances; *a priori*, it is impossible to treat disease otherwise than empirically; yet it seems to me that we should use every means in our power to build up and adorn the lofty structure with all the details of philosophical research; provided, always, that such details be philosophic and yet practical. Unless we do this, as far as in us lies, we are, in a measure, looking through a glass darkly, and not face to face.

What conclusions are we, then, warranted in drawing in regard to the effects of strychnia?

1. Strychnia produces no appreciable primary lesion of the nerve substance proper; that secondary lesions are produced—granular disintegration—by the engorgement of the vascular system, and that this is more marked in the brain and cord than in the nerves.

2. That the convulsions of strychnia are not cerebral, and that they are much more severe after the ablation of the cerebrum, owing to the removal of Setschenow's ganglia.

3. Strychnia does not affect either the sensory or motor nerves at their periphery.

4. Both sensory and motor nerves, in their course, are unaffected by strychnia.

5. The tetanus-producing power of strychnia has its only action in the gray matter of the spinal cord.

6. In small doses, the primary action of strychnia is to excite the vaso-motor centre, causing thus a rise in the arterial pressure, and secondarily to paralyze this centre, and hence to supplement this rise by a fall.

7. In large doses, the vaso-motor centre is immediately paralyzed.

8. The slowing of the pulse, produced by the exhibition of strychnia to both warm and cold-blooded animals, is, in neither case, produced by any action on the central or per-

ipheral ends of the pneumogastrics; but, in warm-blooded animals, is due to action on the excito-motor ganglia of the heart, and in cold-blooded animals is due to action on the ganglia situated in the sinus venosus.

9. The main vaso-motor centre for strychnia is situated in the medulla oblongata, but simpler centres exist in the spinal cord.

10. The pneumogastric nerves are *not* paralyzed by strychnia, in either warm or cold-blooded animals.

11. Strychnia decreases the number of respiratory movements; at first from too little blood, and afterwards from too much blood flowing to the respiratory centres.

12. The decrease is not due to any action of the pneumogastrics.

13. Artificial respiration always moderates, and sometimes stops, the spasms; and this power is due to a maintenance of the oxygenation of the blood until the poison can be eliminated, and is not due to a reflex stimulation of the pneumogastrics.

ART. II.—THE DIAGNOSTIC SIGNIFICANCE OF THE TENDON REFLEX (KNEE PHENOMENON OF WESTPHAL).

BY H. M. BANNISTER, M. D., CHICAGO.

IN the *Archiv fuer Psychiatrie*, V., 1875, pages 492 and 802, Professors Erb and Westphal called attention to certain phenomena of contraction produced in muscles by percussion of their tendons, phenomena which, while some of them had long been matters of common observation, had received but little notice as regards their signification in health and disease. Erb found the patellar sinew reflex notably increased in several cases of spinal disease, *e. g.*, in three cases of compression of the cord, with or without myelitis, in consequence of decided spinal curvature; three cases of diffuse chronic myelitis, one of ataxia, etc. Westphal investigated the condition of

the patellar tendon reflex (knee phenomenon), and that of the tendo Achillis (foot phenomenon) in a large number of cases of hemiplegia and spinal disorder, and came to the conclusion, subject to still further proof, that it was always absent in well marked locomotor ataxia, and that whenever either the foot or knee phenomenon was present, that there could not be grey degeneration of the posterior columns in the lower dorsal and lumbar regions. A very little later Erb* published, under the title "A Little Known Spinal Symptom-Complex," an account of an affection corresponding very nearly, if not identical with the primary symmetrical lateral sclerosis of Charcot, in which as a characteristic symptom he mentioned an exaggeration of these phenomena. Besides several articles on the physiology of these phenomena, there have been several papers published on their clinical significance since the first publications of Erb and Westphal. Thus Henze,† in a rather comprehensive paper on these and other reflexes, confirms as far as he goes the statements of Westphal, and calls special attention to the point that in his observations of cases in which these phenomena were absent, there was also frequently cutaneous anæsthesia of the parts from which they can be excited. Then Westphal‡ himself, in an article on the diagnosis of grey degeneration of the posterior columns in paralytic insanity, reiterates his former statements and lays particular stress on the absence of the knee phenomenon in cases of dubious motor disorder of the lower extremities as indicating an affection of the posterior columns in the lumbar region. In still another communication§ he again calls attention to the significance of this symptom as an early symptom of locomotor ataxia, in such cases, for example, as those in which for a long time the pain is the only other symptom. In these cases, he says, the diagnosis may be made on this symptom alone, before any appearance of the ataxia or disorders of sensibility. The most recent clinical paper on the subject that we have seen is one by

* *Berliner klin. Wochenschr.*, June 28, 1875.

† *St. Petersburger med. Wochenschr.*, Oct. 30, 1876.

‡ *Archiv fuer Psychiatrie*, VIII., Hft. 2.

§ Ueber eines frueches Symptom der Tabes Dorsalis. *Berliner klin. Wochenschr.*, 1878, No. 1. Abstr. in *Centralblatt f. d. med. Wissensch.*, 1878, No. 26.

Muhr,* who gives his conclusions from an investigation of the knee phenomenon in fifty-one cases of general paresis. He finds it generally present in this affection, and agrees with Westphal in considering its absence as a positive sign of degeneration of the posterior columns, even in cases where no ataxia is observed.

If all these statements are correct, we certainly have in this knee phenomenon an important aid to diagnosis. But it is admitted by Westphal himself that it may be absent in perfectly healthy persons, a fact which very materially affects its value as such. Its absence cannot certainly be said to indicate disease in any part of the cord if it occurs in normal individuals. Nor can we ever be sure that it is not one of these exceptional cases that we have to deal with when we would make use of this sign in diagnosis, except in such individuals as we have a previous knowledge of in this respect. It does not appear, moreover, that the lack of the knee phenomenon in healthy persons is of such rare occurrence; in examining not over three dozen persons, two were met with by me, and one of these two was the first one examined. I have met with considerable variation in this patellar sinew reflex in normal individuals; besides the two mentioned in whom it could not be detected at all, there were others in whom it could be found only with difficulty and after several attempts, and still others who exhibited it to an almost exaggerated degree, the slightest tap on the sinew either above or below the patella producing the contraction. I have also thought I could perceive a considerable difference in this respect in the same individuals at different times.

Of the few cases of locomotor ataxia in which I have tested the patellar sinew reflex, in the majority it was absent, as in the cases of Westphal and others. In one rather curious case of ataxic symptoms limited to one-half of the body and resulting from intracranial disease, it was likewise absent. The following case however is apparently an exception, and therefore worthy of special mention in this connection:

The patient was from the country, and but one opportunity

* *Psychiatrisches Centralblatt*, No. 2, 1878. See PERISCOPE of this number.

for examination was afforded before he left the city and returned to his home; continued observation was therefore impossible.

T. N. F., American, sewing machine agent, aged 42, married, of good physical development, no family history of nervous disease, parents both living at an advanced age. No serious illness previously, excepting typhoid fever when eighteen and dysentery when twenty-three years old. In the winter of 1876 and 1877, some ten or twelve months or more before coming under observation, he had made a two weeks' trip in an open wagon and was much exposed. Subsequently he had undergone much mental worry about his business matters.

Some eight months or so before coming to me he began to notice a dull, heavy and numb feeling in the calves of the legs, and a special tendency to jerking of the lower limbs after going to bed. No pain was experienced at this time. This numbness gradually ascended his legs to the body to the intragluteal fissure, where it stopped. None was felt in front. With this he had all the time obstinate constipation, and soon began to have disorder of the innervation of the sphincters and slight dysuria. Had to assume a sitting posture when urinating; for a considerable time, could not control his bladder, either urinate when he wished to, or hold his water at times. At the beginning of his illness and just before, his sexual appetite was greatly increased. On a few occasions he also found difficulty in controlling his rectum and anal sphincter; it seemed at times as if he could not restrain himself, and then he would not be able to defecate for a considerable period.

Some six months before he was seen by me he had suffered for a time with severe neuralgic pains in his right eye. These were relieved by some treatment he thought, but could not give particulars. Possibly there was some inflammation, he thought. Had not slept well since the onset of his disorder, dreamed much, and waked often. Steadily getting worse in this respect. Had at no time any pain in the spinal cord, and mentioned no special feeling of constriction around the body at any point. Had always been temperate in living, neither

smoked, drank alcoholic liquors or used tea or coffee to any extent. Had always, however, been rather free in sexual indulgence.

Ataxic pains in the lower extremities had not been experienced up to the time I saw him, but he had felt them recently in his arms. Not very often, however. His mental power and memory were, he thought, somewhat weakened.

At the time of the examination he appeared well nourished; no atrophy was anywhere observable. No paralysis existed, but he complained of stiffness of his ankles, and was unable to walk any distance without fatigue. The co-ordination of the movements and the power of the upper members were good, his grasp seemed fully equal to that of other men of his age and stature; he carried the index of the dynamometer well around with either hand. In the lower limbs co-ordination was decidedly bad; he could not walk steadily or walk a line or bring his toes together without looking at them. Could walk with his eyes shut nearly, but not quite as well as with them open. There seemed to be weakness as well as inco-ordination, the leg was not thrown out with the force it is in some cases of ataxia. He staggered always to his left side, though his right leg was weaker than his left. The knee phenomenon was much exaggerated, and the contraction could be produced to some extent by percussion upon almost any point of the quadriceps muscle as well as upon the sinew.

The tactile sensibility was decidedly diminished in both legs, most in the right. The same could be said of the sensibility to pain and temperature. Muscular sense was impaired in both lower extremities; he could not always tell the position of his limbs or which leg he moved. Sensibility was apparently normal above the hips. Numbness has been already mentioned. All the special senses seemed normal except, perhaps, that of vision. The sight was good, but there appeared to be a partial color blindness, a dullness in the perception of certain colors. This may, however, have had no connection with his disorder. The ophthalmoscopic examination revealed nothing of interest; no paralysis of ocular muscles; pupils equal and reacting normally.

The digestion and appetite were good; the patient com-

plained much of sensitiveness to cold. Pulse ranged between 80 and 100. Sexual appetite diminished, but power, he thought, normal.

At the time, with my single opportunity for examination, I considered this as a rather aberrant case of locomotor ataxia; and, in spite of the statement of Westphal that the absence of the knee phenomenon is a useful diagnostic sign of tabes dorsalis when complicated with disease of the lateral columns, I was inclined, on account of the manifest weakness of the lower limbs and the great development of this phenomenon, to consider these latter as also implicated. The inco-ordination, loss of muscular sense, lack of pain in the cord, and to some extent the uro-genital and sensory symptoms, indicated locomotor ataxia, the trouble being located well down in the lumbar tract. The late appearing ataxic pains in the arms, and perhaps the neuralgic pains in the eye, the color blindness and the mental impairment, were evidences of the trouble much higher up in the cerebro-spinal axis. I feel well justified in considering that the posterior columns in the lumbar and lower dorsal region were diseased, and I cannot call it a case of simple myelitis. While it may perhaps be said that the morbid process in the lumbar cord had not reached the stage with which the symptoms of well marked locomotor ataxia generally appear, the case appears to me conclusive that it is not in all cases of that affection that the suppression of the patellar tendon reflex is an early symptom.

If we can find a case of typical locomotor ataxia with retained tendon reflex, it would be still more conclusive than the one detailed above, to prove that this phenomenon is not diagnostic of the disease. Such a one has very recently come under my observation, occurring in Dr. J. S. Jewell's practice, and which I owe the privilege of examining to his courtesy. It was a laundress, about forty years of age, who had had all the symptoms in a marked degree, the inco-ordination, ataxic pains, characteristic gait, etc., for several years before he first saw her in 1875. Then, for a period of several more years, she passed from under his care, and only recently came again under observation as a patient in Mercy Hospital in this city. At this time she had been suffering for about five weeks from an

acute attack of intercurrent spinal disease, a sort of acute ascending paralysis, which could hardly be called an aggravation of the long-standing disorder, but which undoubtedly had some connection with it. It commenced without apparent cause with suddenly appearing pains, and sense of constriction at the level of the upper abdomen, and almost simultaneously a feeling of weakness in the left leg. The paralysis extended to the other leg in a few days, making walking impossible. The sense of constriction rapidly ascended the thorax, and the paralysis likewise advanced, involving the sphincters, the abdominal muscles, and the muscles of respiration, in which condition she was when seen by Dr. Jewell. The state of affairs gradually became worse, the arms became paretic, speech and deglutition were made difficult, and after one or two short periods of apparent partial improvement the patient succumbed. No *post-mortem* was obtained. A few days before her death I was able to examine her, when the condition was about at the worst. Motor paralysis was complete to the upper portion of the thorax, respiration and speech were difficult, but sensibility was hardly more impaired than had been for several years previous to this acute attack. The knee phenomenon could be produced in an obvious degree by percussion of the tendon, the leg being slightly raised and supported in a partially flexed position as she lay in the bed. This was tested repeatedly and confirmed by both Dr. Jewell and myself.

It would not be justifiable to presume that the knee phenomenon so well developed in the latest stage of this patient's disease could have been altogether of recent date, having been suppressed during the previous years of the existence of the original disorder. Though the diagnosis was not confirmed by an autopsy, it was, nevertheless, as complete an *ante-mortem* one as could be made in any case, and was agreed in by several thoroughly competent physicians skilled in the diagnosis of nervous affection. If, therefore, the appearance of this phenomenon always depends upon a healthy condition of the posterior columns in the lumbar tract, the parts involved in locomotor ataxia affecting the lower limbs, then all the other characteristic symptoms of posterior spinal sclerosis in combi-

nation are not to be relied upon—certainly not the more probable of the two alternatives. The verification of the diagnosis by the autopsy would have been desirable, but its absence does not render the case inconclusive as to the presence of this symptom in well marked ataxia.

The generalization of Westphal, that this phenomenon is always lacking in cases of degeneration of the posterior columns in the lumbar region, appears to have been a purely empirical one. It is difficult, indeed, to see upon what physiological data it could be based with our present knowledge of the physiology of the symptom. It seems also to be contradicted by other observers: thus I may mention the case of "ataxia" noticed by Erb, in which it was exaggerated, and Leyden* has included this knee phenomenon among the motor phenomena of locomotor ataxia. The fact, however, that the diagnostic importance of its absence has been put forward so prominently, and under the sanction of so high an authority as Prof. Westphal, is, it seems to me, a sufficient excuse for offering the above evidence that, however infrequent it may be, this phenomenon is not *invariably* wanting in that disease.

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* Ueber die Betheiligung der Muskeln und motorischen Nervenapparate bei der Tabes dorsalis. *Deutsche Zeitschr. fuer prakt. Med.*, 1877, No. 49–51. Abstr. in *Centralbl. f. d. med. Wissensch.*, 1878, No. 20.

NOTE.—Since the above was written I have seen a report (*Brit. Med. Journal*, Aug. 31) of the discussion of a paper recently read before the British Medical Association, by Dr. Julius Althaus, on "Lateral and Posterior Sclerosis of the Spinal Cord," in the course of which Drs. Gowers, Sawyer, and Clifford Albutt stated that they had seen cases of posterior sclerosis in which this patellar tendon reflex was still present.

ART. III.—PROVISION FOR INSANE CRIMINALS.

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A SUBJECT which is everywhere forcing itself upon public attention, is the question of suitable custody for criminals who are insane. There is a notable increase of this class in all parts of the country, and to-day, all of the more populous and prosperous States of the Union are being made to feel keenly the necessity of establishing some efficient, economical and humane provision for the large numbers of their dependents in whom crime and insanity are associated.

The increase of this class is undoubtedly due in part to the clearer recognition of the agency of insanity in producing crime; so that many who would at one time have been held wholly responsible for their offences, and punished accordingly, at present are plainly seen to be victims of disease. It was formerly held that any person who in general terms knew right from wrong, or had the intelligence of a child of seven, or was not altogether imbecile, must be answerable before the law for his or her misdeeds. If mentally defective, it must yet be proven that the person "doth not know what he is doing, no more than an infant or a brute or wild beast."

Afterwards, it was required that the existence of insane delusions should be demonstrated, and that the crime should have been a direct outgrowth of these. Later, the knowledge of right and wrong as a test of mental integrity was restricted to the particular time and act. Then, a consideration not previously adduced, began to assume prominence, and this was the ability of the individual to *control his own acts;* and at the present day, it is unhesitatingly admitted by legal authorities, by public opinion, and by medical men alike, that offenders may and do commit crimes for which they are not answerable before the law by reason of disease

depriving them of power of self-restraint in the presence of a criminal volition or impulse. It is recognized that the will or the instincts and passions may become diseased, leaving the intellect and the abstract reasoning faculties apparently intact. Indeed, in recent years, we have seen the pendulum swinging far to the other extreme, and dangerous applications of the doctrine of moral insanity, and transitory mania, excusing or extenuating crimes which public sentiment held to be conscious and malicious infractions of law.

Public sentiment is, however, an unsafe guide in this matter. It follows a pendulum course itself and is swayed, now this way, now that, as the gravity of the fundamental interests of society, or the secret springs of sympathy and sentiment may direct. We have had in early times the punishment of numerous innocent insane persons; in later times a revulsion of feeling in favor of insanity as an excuse of crime; then a reaction against the improper use of this plea; and with each ebb and flow of popular feeling, guilty men have escaped and innocent men have suffered.

I wish here at the outset to disclaim all sympathy with the excessive and injurious benevolence which sees in all crimes, only the misfortunes and not the fault of their perpetrators, and inveighs against the present defects of society as the responsible agency. It may well be that the fatal snare of evil inheritance and environment makes criminals in many instances what they are, and that with the external conditions reversed, the relative position of the criminal and the executioner of the law would also be reversed; but the vicious and destructive influences which would annihilate all order and all progress, can only be met by stern repression and control. It is true that society has a duty toward the criminal beyond simply immuring him where he can do no harm; but swift and certain punishment is also an essential part of that duty, and the direction in which progress is to be made is (1st) in the more careful adjustment of the penalty to the measure of responsibility; (2d) in the judicious introduction of agencies within the prison which will carefully foster whatever there may be left of right-mindedness in the individual criminal.

The increase of insanity among criminals, spoken of above,

is remarkable not only through the greater number of acquittals of crime on plea of insanity, but also in the greatly augmented numbers of convicts who are found insane in the penitentiary, and it is with these latter that the present paper is more especially concerned.

In discussing this subject, I have propounded and sought to furnish an answer to the following questions:

I. What constitutes an insane criminal?

II. How large is the present number of such persons?

III. What is the present provision for this class?

IV. What are the defects of the present system?

V. How are these defects to be remedied?

The consideration of the first question—I. What constitutes an insane criminal?—involves a brief review of the leading aspects in which we see insanity and crime associated. Strictly speaking, insanity and crime are combined in as many different forms as there are insane persons who have committed crimes, or criminals who have gone mad. The two mischievous elements mingle in every conceivable degree and variety, and in the presence of a given infraction of law, it is often impossible to measure with certainty the blind propulsion of insanity, or the wilfulness of depravity. Between the felon's fault and the madman's misfortune, as between a threatening Scylla and Charybdis, human justice often finds a difficult, sometimes a disastrous course. Those who were held to be the most abandoned criminals have often been found insane, and irresponsible. Monstrous crimes have been committed with *malice prepense* by palpable lunatics; while insanity is urged as a specious and dangerous plea in extenuation of crime. There are criminals who simulate insanity with surprising and sometimes successful skill; criminals who have inherited and grown up in such depravity that they cannot justly be held wholly responsible; criminals whose own vices have rendered them incapable of controlling their acts. There are the epileptic criminals, a most perplexing class; and indeed among all criminals the percentage of insanity is at least sixteenfold higher than in the community at large.

On the other hand, there are lunatics who know perfectly well the nature of their crime: who are able to distinguish

right from wrong in the abstract, and who act with a criminal motive, thus answering in no respect to the popular or even the "expert" idea of insanity. There are lunatics who are wholly delirious at the time their crime is committed, and have no subsequent recollection thereof. Other lunatics plan and execute their crime deliberately, methodically and skillfully, alleging some fantastic reason in its justification. Insane persons are met with who *feign* insanity in the hope of escaping from the consequences of their crime; and again persons undoubtedly insane are encountered who are capable of reasoning so far as to rely upon their insanity for relieving them from all danger of punishment for crimes they may commit.

But for the present purpose, all these cases, both of lunacy and crime may be brought under two heads: the *First* comprising those in which crime is the essential and dominant element, and insanity a complication; the *Second*, those in which insanity is the primary and governing agency, overshadowing or extenuating the crime.

The first class embraces those who are always and everywhere criminals. Their insanity may be independent of their crime, or the crime with its consequences may be the exciting cause of insanity, or the insanity the cause of the crime; but throughout they bear the character of criminals.

In the second class are comprised those who are essentially insane, their crime often a direct result of the insanity, but often, also, traceable to causes such as produce crime in sane men.

The answer to our first question then is found in the description of the first class: Insane criminals are those who bear unmistakably the criminal character and history, whatever the relation of the insanity to the crime.

The second class are the "criminal," but non-culpable or irresponsible insane.

However difficult it may be in practice to determine to which of these classes a given individual may belong, the two are in themselves manifestly widely different, and should be differently managed; yet the law has been slow in recognizing this distinction, and practically proceeds upon the principle that all insane are birds of a feather—a state of mind trace-

able to the time when insanity and possession by the devil were regarded as identical states; and lunatics and felons being equally outcasts from society, were treated with equal barbarity.

To-day it may be that there is a sentimental tendency to run to the opposite extreme in dealing with human depravity, and to ascribe the responsibility of all crime to the unfavorable surroundings or neglected education of the criminal; but jurists and legislators have been, as a rule, sufficiently slow to feel this influence. Indeed, its existence may be regarded as a reaction against a too stringent application of unsatisfactory or worn-out tests of mental integrity. The law is necessarily conservative, and in cases where insanity and crime have conjointly come under its ban, society, instead of the criminal, has largely reaped the benefit of the doubt.

We enter now upon the inquiry—II. How large is the present number of insane criminals?

The facts and figures presented have been derived from published reports of prisons and from correspondence with their officers, and it is believed will present a state of affairs which has not been fully appreciated hitherto.

I commence by condensing from a very interesting communication received from Thos. S. Wilkinson, Warden of the Maryland Penitentiary, as follows: "We have now in prison, P. T., sent for ten years for murder; he was insane when received: A. G., deranged and paralyzed; he was but little different when received; J. B., sent for fratricide, has epileptic fits; W. W. and J. H., both insane when received, and both kept constantly locked in their rooms; H. S. confined to room since admission; rarely speaks." Of the six, three are white, three colored men. The average number in the prison is 726.

Mr. Wilkinson also gives the details of nine other cases that have been in the prison since 1872; five of the number were insane when received; one killed a fellow convict. He cites also two cases of suicide in 1877 and 1878 respectively, both Germans; both worked regularly up to time of suicide. He says, "If cases are of a mild type, we of course keep them till the expiration of their sentences."

In regard to this Institution, Dr. C. W. Chancellor, in his able "Report on the Public Charities of Maryland," says (p. 78): "Among the deaths (in the year 1876) were four insane men, who, according to the statement of the physician in charge, had been in close confinement for years. The recent action in granting provisional pardons to this class of prisoners, in order that they may be treated in the asylum, is certainly very humane, but proper provision could and should be made for their care within the prison walls, as some of the insane in the State Asylum are very sensitive about being forced into companionship with convicted felons."

The Ohio Penitentiary report for 1877, speaking of "The Insane" (p. 37), says: "The whole number of this class that has been on hand at different times during the year, was eighty-five. This, however, includes imbecility and dementia." 29 of these were sent to the Asylum, 39 remained in the Institution, 12 had been discharged on expiration of sentence. The average number of prisoners for 1877 was 1,592. Of the above 85, I infer that 50 would properly be called "insane" from the subjoined "Joint Resolution adopted by the last General Assembly," given in the report for 1877 of the Ohio Board of State Charities (p. 102): "WHEREAS, there are at present some fifty odd convicts in the Penitentiary for whose proper treatment no adequate provision is made." The resolution goes on to recommend the transfer of these to the State Insane Hospital, and concludes: "RESOLVED, That the subject of permanent provision for proper hospital care of insane convicts be referred to the Board of State Charities and to the Chairman of the Board of the Penitentiary, and that they constitute a commission to take the subject of such permanent provision under consideration and report to the next General Assembly such plan of relief as in their judgment may seem best."

The California State Prison Report for 1876 and 1877 says (p. 29): "During the last two years we have had 20 cases: six went to the Asylum, five recovered, nine are still in prison." A fair estimate for the number of insane for 1877 would be 12. The average number of prisoners for 1877 was 1,211. At this prison, of the total number discharged from

the beginning, 6,683, 72 have been sent to the Asylum as insane.

In this report, J. E. Pelham, M. D., the physician and surgeon to the prison, says (p. 28): "The prison is destitute of proper accommodations for the insane. * * It is an unavoidable necessity that insane cases, when they appear, should be retained in prison for a time in order to prove them; for their diagnosis is so difficult that the science and skill of the most experienced physicians will often be at fault. The conductors of our insane asylums, on account of the extra vigilance necessary to be exercised to prevent escapes, are probably disposed to discourage the introduction of insane criminals into their wards. Therefore, as the prison must of necessity be custodian of insane persons for a time, it seems to me that it is just a simple duty that proper provision should be made for their care and management. At present they are confined in cells in the main cell-buildings, and are not separated from the other prisoners except by the cell walls, and they often render night hideous by their ravings, thus disturbing the sleep of those who must labor during the day. To avoid this state of affairs, there is at present but one remedy—the dungeon, which has frequently to be used for the purpose. With present accommodations for treatment of such cases, results are unsatisfactory, and remedial measures cannot be resorted to, with reasonable prospects of success."

In New York, at the Asylum for Insane Criminals, 38 convicts were received in the year ending Sept. 30, 1877. They came from Auburn (17), Sing Sing (8), Albany (6), Clinton (6) and the State Reformatory (1). Deducting these from the 104 remaining in the State Criminal Lunatic Asylum Sept. 30, 1877, and also 10 per cent. of the remainder for the unconvicted cases (which latter proportion their statistics from the beginning show to be about correct), we have 60+38=98 the number of insane convicts in New York at that time by a moderate computation. For the same time, the average prison population of the four State Prisons was 4,614.

The "Asylum for Insane Criminals," at Auburn, was the first and remains the only separate establishment of the kind in the country, and its admirable workings and the excellent

policy which founded it are recognized and acknowledged wherever the question of provision for insane criminals has received attention. The committee appointed by the resolutions of the Ohio Legislature above given, visited this institution, and it is understood, will report favorably to the building of a similar asylum in Ohio.

The Report of the Wisconsin State Prison for the year ending Sept. 30, 1877, gives 17 insane in its population of 290. Five of these are violent, 11 mild, and one female not characterized.

The average prison population of Pennsylvania for 1877 was 1,820; 1,040 at the Eastern Penitentiary, and 780 at the Western (Rep. B'd Pub. Charities for 1877, p. 15). In regard to the insane convicts of the State, Diller Luther, M. D., Secretary of the State Board of Charities, writes me: "The entire number in and outside of the asylums may perhaps be 100." In the Report of the Eastern Penitentiary, at Philadelphia, for 1877, p. 69, the medical officer, Dr. Comegys Paul, says: "During the year, 11 men were sent here with developed insanity, six others were epileptics, many are registered of dull intellect. Two men developed latent insanity during the year." A table is also given showing the whole number of insane from 1851 to 1877. This was 30 out of a whole number in confinement of 6,511—an average annual percentage of .46.

At the Connecticut State Prison, the Report of Jan., 1878, gives three insane, one of whom it is necessary to keep confined in his cell. The warden remarks, "They need the benefit of the State Asylum, or some other provision suited to their condition." From the medical officer I also learn that there are now five in the State Asylum. The population of the prison is 266.

The medical officer of the Iowa Penitentiary writes me that in 12 years, out of an average population of 350, 12 have been sent to the asylum, six of these in the past 18 months. The physician at the Additional Penitentiary of Iowa reports three sent to the asylum out of a total since the beginning (four years ago) of 440 prisoners.

The Northern Indiana Prison Report, Oct. 31, 1877, calls

for 12 cells to be built especially for its insane. The average prison population is 648. It is reasonable to suppose that these special cells are only asked for for the confinement of the violent and uncontrollable insane.

The Report of the Indiana Prison, South, notes 10 cases of epilepsy among its prisoners, among which there could not fail to be, as every physician knows, much dangerous insanity.

The Kansas State Penitentiary Report, for 1876, gives a "daily average confinement of the insane of 3½, with a population of 406." The medical officer in his report (p. 40) remarks: "There are confined here as prisoners several who are and have been insane, some of them for years and to all appearance, hopelessly. The law provides that when any prisoner has become insane * * * the warden shall turn him over to the Superintendent of the Asylum, and take his receipt therefor. What equivocation is there here? None, it is absolute. And yet in the face of the law this has been denied us positively, though they had plenty of room at the time. Now, we have no wards for the insane, we have no facilities for their treatment of any character whatever. * * * Is it meet and consistent to keep a noisy maniac within a hospital ward, among cases of sickness, where quiet is an absolute essential? We have no other. Is it consistent to keep such an one in one of the cells where the prisoners who labor hard all day, need a quiet place at night?" This presents one side of the picture, and for the other, I trust it is correctly set forth elsewhere in these pages. Without countenancing any violation of law by asylum authorities, I would answer the question of the offended prison physicians by asking another—Is it "consistent" (that is just the word) to force the contamination which the felonious insane cannot fail to carry with them upon reputable insane persons, and fasten upon the asylum the stigma which is inevitably connected with convicted and notorious crime? In principle both the asylum and prison authorities are right, and their difference can only justly be met by relief, extended to both in the shape of separation of the convict insane from prisoners as well as from ordinary insane.

The New Jersey State Prison Report for 1877 gives an

average number of prisoners of 810, of whom two were sent to the Asylum.

In Massachusetts, I have no precise information as to the present number of insane convicts. Dr. Nathan Allen states it at "not over 20 or 25." From information kindly furnished me by Hon. Sidney Andrews, Secretary of the State Board of Charities, I learn that in the ten years from 1864 to 1873 inclusive, there were "discharged as insane" from the State Prison 19 out of a total number, 1,985. In the same period, from a total in the jails and houses of correction of 97,642, 266 were discharged as insane. From the "House of Industry," out of a total of 22,546, 36 were discharged as insane. In the State "Workhouse," out of 1,791, apparently none were found insane in that period, but Mr. Andrews informed me the past summer that 45 insane had since been reported from that institution. It is necessary to state, that in the jails and houses of correction, the terms are very short, and the same individual undoubtedly appears over and over again. The above figures give, out of the approximate number in the prisons of Massachusetts for ten years, 123,964, 321 discharged as insane. There is a special prison for women at Sherborn, Mass., containing 460, of whom eight are insane. The present number in the Massachusetts State Prison is 750, of which number five are in the cells for insane, seven are weak-minded.

The Report of the Nebraska State Prison for 1875–6, gives one insane and three epileptic out of an average of about 66 prisoners.

The Nevada State Prison Report gives, three sent to the asylum from 1875 to 1877, with an average of not over 130 prisoners.

The warden of the Minnesota State Prison informs me that he has four confined as insane, and one has been sent to asylum in past year. Present number of prisoners 166.

The warden of the Michigan State Prison writes that he has "803 convicts, 10 of whom are insane" (May, 1878).

In the State of Maine, there are seven insane convicts in the Insane Hospital. The state prison has also a department for confining the insane; number there not known. The average number of prisoners for 1877 was 188.

In the State of Illinois at the present time there are 28 convicts in the three State Insane Hospitals. The present number of prisoners in the State Penitentiary at Joliet is 1,665. There are 192 additional at Chester, where the new Prison is being built.

From figures kindly furnished me by Major McClaughry, warden of the Illinois Penitentiary, I learn that in the 18 years from 1860 to 1877 inclusive, 71 convicts have been sent to the Asylums of the State. From 1871 to 1877, during which years the average prison population has been 1,381, the average number of insane convicts each year has been $6\frac{3}{7}$ or .47 per cent. of the prison population.

Recapitulating the figures given above for the several institutions and states, and in every instance where there is doubt, making an under-, rather than an over-estimate, we have as follows:

TABLE SHOWING AVERAGE OR TOTAL PRISON POPULATION IN SEVENTEEN STATES, AND NUMBER OF INSANE CONVICTS IN PRISON OR ASYLUM.

STATE.	NO. IN PRISON.	NO. INSANE.
Maryland	726	6
Ohio	1,592	50
California	1,211	12
New York	4,614	98
Wisconsin	290	17
Pennsylvania	1,820	75
Connecticut	266	8
Indiana	646	10
Kansas	406	3
New Jersey	810	2
Massachusetts	750	20
Nebraska	66	1
Nevada	130	3
Minnesota	166	5
Michigan	803	10
Maine	188	7
Illinois	1,857	28
	16,341	355

The proportion of insanity among the whole is one insane person to 46.03 convicts, or 2.15 per cent. A percentage at least 16 times as great as is found among the population at large.

This is a rough statement with regard to the convict insane of the country so far as I have been able to elicit information on the subject. It is incomplete, and certainly does not exceed the truth with regard to the proportion of this class. Certain states give no account of any insane, as Texas, New Hampshire, Alabama, North Carolina, Kentucky, Tennessee, and from the remaining states no returns have been obtained. Are we to suppose that the last mentioned states are exceptions to a rule prevailing in all the most enlightened, progressive and best-policed commonwealths, or that their prison authorities have overlooked, or failed to report the frequent complication of crime with mental alienation? I hope to answer this question satisfactorily, as well as to present an additional amount of detailed and tabulated information with regard to a considerable number of insane convicts in a future paper.

As bearing upon this point, I will here quote from Dr. Dugdale's remarkable researches in his work entitled "The Jukes,"* wherein are given the results of a careful examination of 233 convicts. Among these were found 49, or 23.03 per cent.; nearly one in every four, who belonged to "nervously disordered stock."—"Under this head are included all convicts who are or have been afflicted with insanity, epilepsy, chorea, paralysis, or other nervous disorder, or who have any blood relations who are or have been subject to any of these disorders. The number tabulated is greatly under the actual facts, because so many are either orphan or abandoned children who know nothing of their ancestry. * * * This close relation between nervous disorders and crime runs parallel with the experience of England, where, 'The ratio of insane to sane criminals is 34 times as great as the ratio of lunatics to the whole population of England; or if we take one-half the population to represent the adults who supply the convict prisons,

* "The Jukes" (3d Revised edition), by R. L. Dugdale, M. D., member of the New York Prison Association Executive Committee. Putnam's, N. Y., 1878 (p. 86).

we shall have the criminal lunatics in excess in the high proportion of 17 to 1.' (Dr. Guy in the Journal of the Statistical Society, Vol. XXXII., p. 16.)"

Dr. Bruce Thomson, Surgeon-General Prison, Perth, Scotland, also says: "On a close acquaintance with criminals of 18 years' standing, I consider that 9 in 10 are of inferior intellect, but all are exceedingly cunning."

Returning to Illinois and the percentage of insane among its convicts; it is noticeable that this falls decidedly below the average for the other states, but with the very large prison population of the state, even this proportion gives a considerable number of the class to be provided for.

Taking past experience as a guide for the future, we shall have not less than nine annually found insane in the two penitentiaries, and we shall be fortunate if the actual number does not much exceed this. With the 28 now in the Insane Hospitals, this makes say, 37 before the close of another year. And on this basis at the end of five years there will have been in all 182 cases. Deducting from this a liberal percentage of .15 for deaths and .10 for recoveries, there will remain a fraction over 161 still in custody. But the great and growing State of Illinois can hardly fail to pay the penalty of prosperity by seeing its criminal population increase, and with these the number of insane criminals; especially as the experience of other states shows that Illinois has enjoyed thus far an unusual, and probably fortuitous exemption from this peculiar burden.

The above may be taken to represent the numbers of the strictly convict insane. But there is another group of the insane who are properly designated as "insane criminals," and justly belong with this class. I refer to such of the insane as are acquitted of crime on the plea of insanity, who yet give unmistakable evidence of such depravity and previous evil life as unfits them for association with the reputable insane. There will always be a certain number of criminal insane persons who will be universally recognized as free from any moral taint of crime; like the insane fanatic who sacrifices a life from exalted religious fervor, or the epileptic who destroys his friend in delirious fury; the mother whose madness impels her to kill her own child, or the maniac

who commits larceny under a hallucinatory command; but the large majority of crimes committed by insane persons are the outgrowth of mingled depravity and insanity. On this point Dr. W. W. Godding, now Superintendent of the Government Insane Hospital, at Washington, formerly Superintendent of the Taunton Asylum, thus expressed himself in a letter to the Board of Charities of Massachusetts (Eighth Annual Report, p. 130): * * "It must be admitted that the previous education has frequently something to do with the type of insanity, and that other things being equal, those men whose habits of life and moral training are of a low order are more apt to be violent and homicidal, * * and even in cases of special delusion, sometimes, the habit of a better life remains." This principle will certainly hold with equal force in regard to low and bestial crimes. But in the present state of affairs, a man acquitted of rape, or a woman of keeping a house of ill-repute,* goes to the State Asylum to mingle with innocent and reputable persons, whose peculiar misfortune should doubly assure them of protection.

The number of this latter subdivision of insane criminals increases every year; nine of them have been received at the Central Illinois Hospital for Insane, six at the Northern, and five at the Southern, since the law committing them thus went into effect in 1872. Of these 20 a large proportion would be found in all respects more proper associates for convicts than for the ordinary insane, and if an asylum for insane criminals existed in the state, would undoubtedly have been committed there by the court conducting their trial.

III. What is the present provision for insane criminals? A brief paragraph will contain all that can be said in reply to this question, whose solution indeed just escapes the *reductio ad absurdum*, by the fact that in one of the States something has been accomplished in the way of adequate provision for its insane criminals. New York, as stated above, has its asylum at Auburn, now accommodating 125 of this class. The new Massachusetts Penitentiary has an insane department designed for 25 or 30. The Maine State Prison has

* This latter is not an imaginary but a *real* case.

also separate cells for the insane. Beyond this, a joint-resolution passed by the Legislature of Ohio, and a standing disagreement in Pennsylvania between the Board of Charities and the superintendents of the asylums of the State, since the year 1873, are all that there is to indicate what is nevertheless the fact, that the injustice and inexpediency of the present system, and the crying need of a better, are recognized by every intelligent citizen who has given attention to the subject. There are in the various states various modifications of the plan, or want of plan, now in vogue. But the general features are the same. Either no special treatment is provided for the convict insane, or they are sent to the ordinary asylums and hospitals. Further, all such criminals as are acquitted on the ground of insanity, when not allowed to go free, are also sent to the asylums for insane. In Illinois, convicts are sent from the penitentiary on the certificate of the prison physician, and the medical director of the asylum must receive them *nolens volens*, although in all other cases he is given full power and discretion as to receiving cases. Here, also, all persons acquitted of crime on plea of insanity must be received at the asylums.

In Maine, I am informed by Dr. E. C. Neal, of the State Insane Hospital, there is a law which enables the judge to order a person charged with crime, for whom the plea of insanity is to be made, to the Insane Hospital for observation till the next sitting of the court, and subsequent proceedings are governed by the report of the superintendent upon his sanity.

In Michigan, in Maryland, and many other states, the Governor may pardon, commute or suspend the sentence of an insane convict and have him transferred to the Asylum, if in his opinion it is wise to do so.

In Massachusetts, a commission including one or more of the asylum superintendents decides upon the case of convicts, and their removal is governed, I believe, by the Board of State Charities.

IV. Our fourth inquiry is as to the "Defects of the present system." Some of the objections to insane persons of the criminal class and character being sent to the asylum to

mingle there with the non-felonious insane, are obvious enough, and probably no one would be found to dispute their abstract justice, but a practical exposition of the injurious workings of the present course seems nevertheless to be needed. I will endeavor to make this, and my remarks may be embraced within three divisions: First. The unfitness of insane criminals for association with the ordinary insane. Second. The impropriety of attempting to confine convict and criminal insane in an asylum of ordinary plan and construction. Third. The inexpediency and extravagance of such a course.

First: the unfitness of insane criminals for association with the insane who are free from crime. It is scarcely necessary to expatiate upon this. Among the 19 convict insane whose near acquaintance it has been my peculiar privilege to form in the hospital, have been: one professional pickpocket; one "song and dance man" from the lowest slums; one female receiver of stolen goods; two ravishers; three murderers; two burglars. Among the four acquitted of crime because insane, were one incendiary; one female keeper of a house of ill-repute; one murderer, who well deserved the name of a "cowardly assassin;" and all of these, except three or possibly four, retained their mental aptitude, force of will and moral (or immoral) traits in full activity.

Is not the moral infection of such as these in the hospital as much to be dreaded as any physical infection? If one sickly sheep can infect the flock, how much pestilence will these moral lepers bring with them? Are not the misfortunes of the insane already sufficient without subjecting them to this further degradation? Is it possible to suppose that the people of any community or state, into so many of whose families the ominous spectre of insanity sooner or later stalks, wish the afflicted ones to dwell familiarly with such companions? It may be asked why the separation of these cannot be made within the asylum walls. This I intend to consider in the next paragraph. It may also be urged that insanity masks the evil traits of these persons, but that is the fact only to a very limited extent. Indeed, those who are most familiar with the insane well know that, except in the comparatively

rare instances of complete decay and destruction of brain tissue, the former leading characteristics of the individual remain, and the direction and chief manifestations of the insanity will be determined thereby. This is why there is a probability, if other things are equal, that any insane person who commits a crime, whether culpable in the eye of the law or not, is of an inferior moral organization; though it is at the same time true that a small number of persons of reputable, if not excellent character, are constantly being driven by insanity into crimes as detestable to themselves in their normal state as to any right-minded man. It should therefore be possible to find an appropriate place for each class of insane perpetrators of crime. A certain proportion of them might well go to the general insane asylum, possibly an occasional convict might properly go there; and rightly constituted authorities should determine this matter, in accordance with the actual character and merits of the individual—either courts or special commissions or the Board of State Charities. What I contend for is, a separate and special provision for the majority of insane criminals.

Let it not be supposed that an attempt is made to give any but a sober and truthful color to this picture. Concerning the remainder of nineteen convicts above mentioned, details could be given as unpleasant to contemplate as any spoken of. Insanity but seldom renders its victims better than before in any respect, more generally indeed, it adds darker and more repulsive features to the character. The insane criminals (of whom over sixty have been in Jacksonville hospital, twenty-three at Elgin, and sixteen at Anna, in the five or six years since the two latter hospitals opened) retain in large part all the cunning which their career of vice and crime has given them. They are much more prone than the rest of the insane to commit dangerous and violent acts; to be concocting plans of escape, rebellion, or mutiny; to torment the feeble and irascible about them; to teach new lessons in depravity to the pupils so easily found around them.

I do not wish to claim that there are not among the non-felonious insane, individual cases as revolting and disagreeable in some respects as any found among the criminals, or that the

contamination of criminal association is felt or remarked upon *consciously* by any great number of the patients; indeed, I have seen occasionally a convict who was well liked and familiarly treated by his fellows in the wards; but the fact that there are bad cases among the non-criminal insane, and that many of these latter are too weak in intelligence or will to resist or recognize evil influences, only renders more clear the evils of the present system. The weakness of the insane entitles them all the more to protection, and because there are inevitably a few vicious among them, we are not justified in increasing the number still further. Moreover, there are numbers among the insane who do most keenly feel the infamy of the association thrust upon them. Did space permit, numerous illustrations of this might be given; let one suffice:—A friend of the man murdered by the above-mentioned "cowardly assassin" came to the asylum where the latter was confined, and as he improved was offered a place in the same ward with him, but he refused the proffered change, though for his interest in other respects, scorning to meet or associate with the murderer of his friend.

Second.—"The impropriety of attempting to confine convict and criminal insane in an ordinary asylum."

A natural inquiry to one not familiar with the merits of the case, would be; "Why cannot these criminals be kept by themselves?" They might indeed be kept permanently locked in their rooms, but this is just what they are sent to the asylum to escape, because it is unjust and inhuman, and destroys every hope of recovery. If, now, the difference in construction and discipline between the penitentiary and the asylum be taken into account, the objections to separation within the asylum are plain enough. The penitentiary is a penal, the asylum a charitable institution, for the care and cure of its inmates. These convicts and criminals when brought to the asylum are placed in the wards with other patients. They cannot be separated from the other inmates and be placed in a ward by themselves. The number in any one institution is not sufficient for this, moreover much injury and injustice would result to the criminals themselves from such mutual contact where they could

not be properly classified;* and finally, it would be folly to congregate them thus unless the penitentiary regime could also be introduced. The asylum has no machinery and should need none, such as is required for the safe-keeping of this class. There are no cells built of solid masonry, there is no system of quasi military guard and discipline. There is nothing of the penal character; on the contrary, with the insane who are free from crime, it is a well-recognized principle to extend freedom to the farthest limit compatible with welfare and safety; and the cardinal principles on which the asylum is conducted cannot be subverted for these exceptional inmates without great injustice to those for whom the institution is built and who have the first claim upon it. There are hence numerous opportunities for contact with all classes of inmates which cannot be avoided. The insane criminals inevitably enjoy an amount of liberty and receive privileges which the ordinary insane are indeed entitled to, but which the others are for the most part only capable of abusing.

The greater opportunities for escape must further be taken into account. It is well known what rigidity of discipline, security of construction and ingenious watchfulness are required to keep convicts when once they have been caught. The asylum, however, is not built to withstand the assaults of the jail-breaker, and should not be so built, since the enlightened management of the insane at the present day shows more and more clearly that their liberties may be carefully and slowly enlarged. The result of this state of affairs is, that sooner or later, those criminals who retain their cunning (as most do), and so many of whom are skilled in all the "cracksman's" arts, make good their escape. More than one-fifth of all who have gone to Jacksonville and a sixth part of all those at Elgin have eloped successfully, while their constant efforts and unsuccessful attempts are a source of expense, anxiety and indirect injustice to the other patients. Dr. Godding, in his report for 1872 (Taunton Lunatic Hospital), says: "Of the six successful elopements (during the year) four belonged to

* All of the larger States have them scattered in two or more of the State Asylums.

the criminal class," and similar accounts come from all institutions that have much to do with this class. The disproportion in the number of escapes is enormous, but no greater than might be expected. Now, an insane criminal is, if anything, less to be trusted with his liberty than a sane one, and should not be less, but more carefully guarded than the other. But superintendents of asylums have a full measure of responsibility without these additional duties, and in any event, all they can do with the means at their command will not keep these outlaws secure. They are compelled to assume their charge without facilities being provided to properly guard them, and when they do escape, the fault rests, not with the superintendents, but with the crude system which has laid upon them a trust without supplying the means to carry it out.

Third.—The inexpediency and extravagance of treating insane convicts and criminals in common with other insane persons. The inexpediency in this matter relates chiefly to the tendency among convicts (and other criminals as well) to regard the asylum as an easy escape from their proper punishment, and a convenient "short cut" to freedom. The wardens of all penitentiaries know this. They encounter much perplexity from time to time in dealing with convicts who are either feigning insanity to get to the asylum or are really insane; and a convict about whom there remains a shadow of doubt is never given the benefit of that doubt, but held rigidly to duty and put through the severest tests. If this were not so, "epidemics" of feigned insanity would frequently be appearing in the prison. The result in all cases of this kind is watched with the keenest interest by the other prisoners. Hence, injustice is doubtless often done to individuals, and some who ought to go to the asylum do not get there. This difficulty would be obviated by having an institution connected with the penitentiary for insane criminals. Wardens of penitentiaries, as a rule, favor this plan. I know this to be the case with Maj. McClaughry, the experienced and able manager of the Illinois Penitentiary. In Maine, the same view was taken by the warden and acted upon there. The same plan has been adopted in Massachusetts. At Auburn, the State Criminal Asylum is upon the grounds of

the State Prison, and an interesting fact was developed in connection with this institution, bearing on this point: "From 1846 to 1860, 67 insane convicts were sent from the State prisons to the Utica Asylum, and 69 to the Asylum for Insane Convicts. Of the former 14 feigned insanity, while of the latter only two were suspected of feigning it, showing, as the superintendent states, 'that the hope of escape from a large and general asylum was much greater than from a place designed for the detention and security of criminals.'" * The consequence of having an asylum connected with the prison would be that prisoners would feel it no object to feign insanity, and there would be no hesitation about sending any convict there who ought to go.

In the matter of expense, the chief point to be adduced is the saving that would be effected by having the insane criminals supported by themselves, in an establishment agreeing in its general scale of expenditure with the penitentiary. The insane convicts and criminals do not, as a general rule, need the elaborate treatment of a hospital for the insane, as a much greater majority of the cases are chronic and incurable, and there is less of that physical debility so largely coëxisting with insanity in the community at large. The average expense *per diem* of supporting patients in forty-one American asylums, as ascertained by Dr. Conrad, of Maryland, some four or five years ago, was 69 cents *per capita*. At the Elgin Hospital, in 1876, it reached the moderate figure of 65 cents. For the same period at the Illinois Penitentiary the expense was 40⅝ cents for each convict per diem. At the Auburn State Criminal Asylum, the cost of the asylum for all current expenses for the same period was $16,322. The year was commenced with 89 patients and ended with 104. Taking 100 as the average for the year, the daily per capita cost was 44 cents. It will be seen by this that the current expenses of such an institution are at least one-third less than those of an average insane hospital. Taking 35 insane convicts as a moderate estimate of the number to be supported by Illinois for the next five years, and allowing a difference of 20 cents

* Report Board State Charities, Massachusetts, 1871, p. 138.

per diem between the present cost of their support at the State insane hospitals and what it would be in a properly administered asylum specially built for them, and we have an annual difference of $2,548, or in five years of $12,740. This would go a good way toward the construction of the necessary building, which would be largely put up by convict labor if built near the penitentiary. Another saving secured by such a criminal asylum would be the expenses of transferring convicts. I compute that not less than $1,400 must have been spent in sending 42 insane convicts from Joliet to Jacksonville, 19 to Elgin, and 11 to Anna, in the past few years. The reply, then, to the question as to the defects in the present provision for insane criminals would be that they are: 1st, an injustice to the insane who are free from crime, in their forced association with felons; 2d, an injustice to the public in not properly guarding these dangerous and vicious criminals; 3d, unnecesary expense and unwise arrangements which result in injustice and suffering to the insane convicts themselves.

V. How are the defects in the present management of insane criminals to be remedied? The foregoing pages have been written to little purpose if it is not evident therefrom that a separate provision is needed for insane criminals, and that they should be cared for in an asylum by itself or addition to another building, specially constructed and adapted for this peculiar purpose, and assimilated in all needful degree, in plan and administration, to the penitentiary; since the intended inmates, although insane, are still dangerous outlaws and criminals, and retain, for the most part, all the criminal characteristics. No argument is needed to show that every state which has a considerable number of this class, would best provide for them in an independent institution; but where it is necessary to enlarge an existing building, the question arises whether the penitentiary or the asylum is the proper institution to receive the addition. It is perhaps naturally assumed on one hand that all insane persons belong at the asylum, but a little reflection or experience will make it plain on the other, that convicts and criminals are, when insane, not less vicious, dangerous and depraved, but often more so, than when sane. In-

sanity is a misfortune for both classes, but does not and cannot obliterate the great gulf fixed between them by their previous career.

Without entering minutely into the pro and con of this question, let us seek a practical solution by noting the opinion and action of those in all stations most capable of passing upon its merits. In the first place, we may be certain that the Superintendents of Asylums in this country, embracing many of our ablest alienists, and almost the only men having practical familiarity with the insane, have given conscientious consideration to this subject. Their "Association," at its meeting in 1873, passed a series of resolutions from which I extract as follows:

"RESOLVED, 1. That neither the cells of the penitentiaries and jails, nor the wards of ordinary hospitals for the insane, are proper places for the custody and treatment of this class of the insane."

"RESOLVED, 2. That when the number of this class in any state is sufficient to justify such a course, these cases should be placed in a hospital specially provided for them, and that until this can be done, they should be treated in a hospital connected with some prison, and not in the wards or separate buildings upon any part of the grounds of an ordinary hospital for the insane."

This is the opinion from the asylum point of view. Let us now inquire what officers of prisons think. I have yet to hear of one who would not prefer to have an insane department in connection with the penitentiary, rather than send insane convicts to asylums. I have already quoted Dr. Pelham, the able medical officer of the California Penitentiary, on this point, and his request for facilities for the care of the insane at the prison. Also, the plea of the Indiana Prison authorities for accommodation at the prison for their insane. Major McClaughry, of the Illinois Penitentiary, informs me that he considers convicts improper persons to go to the asylum, and favors making provision for them in connection with the penitentiary, thus saving trouble, expense and *shamming* among the other inmates. The opinion of the warden of the Maine Penitentiary corresponds with the above.

Judging now by the actual steps taken in the various States, the same conclusion seems to be warranted. In none of the States has a department been added to an asylum for the insane criminals, while all the special provision that exists in New York, in Maine and Massachusetts, is connected with the prison.

I believe that a different opinion has been entertained by the Board of Charities of Pennsylvania, and also at one time by the same board in Massachusetts. F. B. Sanborn, of Boston, the eminent secretary of the American Social Science Association, writes that his conclusion while member and secretary of the State Board of Charities was, "strongly in favor of a separate asylum or department in an asylum, for insane convicts and for persons excused from sentence on the ground of insanity." There are also one or two members of the Association of American Superintendents of Asylums, whose individual opinion is not averse to building a criminal asylum in connection with an insane hospital. Every one of these last, however, is equally stringent about separating the insane criminals totally from the ordinary insane, and it is very difficult to see how this can be done, with both on the same grounds, or inside the same walls. Mr. Sanborn's opinion, however, seems to apply chiefly to the conditions found in Massachusetts, whose State prison has now only 750 inmates, and a small proportion of insane.

There are difficulties to be met on either hand, but the large majority of them seem to stand in the way of sending convicts to ordinary asylums.

The advantages to flow from a separate and distinct establishment in proximity to the penitentiary, are: (1.) The convict insane will receive the only secure and consistent custody possible. In a State which has two or three asylums, with several convicts scattered in each, these can all be brought together, suitably classified and treated equally well at a great saving in expense. (2.) A large number of insane criminals, such as are now sent to asylums after acquittal of crime, but who are dangerous or peculiarly vicious in character, can, in the discretion of courts, or, as in some States, by order of the Board of Charities, be sent to the criminal asylum, thus re-

lieving the other institutions of a pest and scourge, and effecting a further saving in their support. There are now in Illinois, not less than three or four of these at each of the three hospitals, and their number increases with every year since the law thus committing them went into effect; $728 a year could be saved on ten of this class at once, which, added to the annual saving on 35 convicts, would be $3,276 per annum. Another very great saving in expense, if the asylum were built near the penitentiary, would be the employment of convict labor to erect it. (3.) There are certain of the prison inmates whose cases can, by the proposed plan, be tested and properly disposed of, who are now in danger of having great injustice done them. They are, (a) convicts who are feigning insanity, (b) convicts not absolutely insane, who are yet inferior in mental organization to such an extent as to justly require some modification of the prison discipline. The number of these (the "half sharp,") is large in every prison, but there is no provision for them. Every convict that will go on about his work regularly is allowed to do so, and many are insane for years and no attention paid to the fact until they suddenly break down, or commit suicide, or some act of atrocity. Many of these might be saved and restored, if any proper facilities were at hand in time. (c) Epileptics and persons suffering from paroxysmal mania, only needing treatment occasionally for a short time, could be cared for in an asylum connected with the prison, although it might never do to send them away to the general asylum. (d) There are many depraved criminals who are sentenced to the penitentiary who would be remanded by the court to a criminal asylum, if such a place existed; their mental defectiveness is developed more or less plainly during the trial, but the judge feels that they are utterly unfit either to go at large, or to go to the general asylum, and the prison is the only resort. This explains, in part, doubtless, why so many convicts, insane from the first, are found in the prison.*

The subject treated in the foregoing pages is one which has

* Eleven received already insane at the Philadelphia prison the past year, and nine out of fifteen insane since 1872 at the Maryland Penitentiary.

long failed of coming before the public with the force its importance would justify.

I have treated it with special reference to Illinois, as enlarged provision is now being made in that State for its criminals, an important subdivision of whom are the insane criminals.

I would sum up as follows:

I. Every community has among its insane, a class of "insane criminals," as distinguished from innocent and reputable insane persons, who have committed crimes (the latter generally styled the "criminal insane.") They are in large part convicts, but there are many unconvicted criminals among them.

II. The number of the former class is large; the proportion among convicts being probably not less than one in fifty or sixty.

III. There is no proper or suitable provision for this class.

IV. This lack of provision leads to great injustice and injury to the other classes of insane, and to the community at large.

V. The establishment of a special asylum for these, wherever practicable, would be a measure of justice, humanity and economy, and the preferable location for the same, when it cannot be an entirely independent institution, is in connection with the State prison.

ART. IV.—INTRA-OCULAR CIRCULATION: RHYTHMICAL CHANGES IN THE VENOUS PULSE OF THE OPTIC DISK.

BY DR. O. F. WADSWORTH AND DR. JAMES J. PUTNAM, OF BOSTON, MASS.

THE investigation to be described in this paper was undertaken by us originally for the purpose of studying for ourselves the effects of compression of the veins and arteries of the neck, and of inhalation of amyl nitrite on the retinal circulation. It is published, however, mainly for the sake of the observations whose nature is indicated in the title of this paper; and our results as regards the other points, which have so often been the subject of controversy, will be only briefly noted.

The method of experimentation was always the same, one of us always examining by the upright method the same eye of the other, the pupil fully dilated with atropine. To further fix the appearances a careful sketch of the disk and vessels was made.

It was soon found that the best point on which to fix attention was a main branch of the vena centralis, which pulsated distinctly as it turned to plunge into the physiological depression near the centre of the disk, receiving at its bend a smaller branch which also pulsated. Changes in the size and shape of the pulsating portion of these vessels formed the most obvious, indeed the only reliable signs of variation in the vascular supply to the fundus.*

1. *Compression of the Jugular Veins.* This was done by twisting a handkerchief about the neck. The compression could easily be carried so far as to cause the veins to stand out

* No *pulsation* was ever seen in the other veins of the fundus, and only during the experiments with amyl nitrite were any changes in their *size* observed. At no time could any general change in the coloration of the disk be made out.

in relief on the forehead, and the whole face to assume a dusky hue, not to speak of causing blurred vision and an uncomfortable sense of fullness in the head.

The effect of this procedure on the intra-ocular circulation was, however, so slight, if there was indeed any, as not to be made out with certainty. Sometimes an appearance of greater fullness of the veins at the pulsating point seemed to be present, but the pulsations went on as before.

2. The effect of *pressure on the carotid* of the same side with the eye examined, were studied in the following manner. The pressure was made at about one and a half inches above the clavicle by one finger of the examiner, and was increased to a degree sufficient both to stop the beating of the temporal artery, and to cause a distinct sensation in the head, quite different, it may be said, from that due to compression of the jugular veins. So soon as this result was produced all pulsation in the vein ceased abruptly, and the vein remained narrow, as in the condition of so-called venous systole, so long as the pressure was kept up. Immediately on relaxation of the pressure on the carotid the vein refilled completely, and the normal pulsation was re-established.

This observation seemed to us to have an important bearing on the theory of the venous pulse. It is well known that two prominent theories with regard to this phenomenon are held; one, that of Coccius, who maintained that at the time of arterial diastole the blood flowed out of the eye, through the veins, more rapidly than before, the veins at the same time partially collapsing near their point of exit; the other, that of Donders, according to which, at the time of arterial diastole, the vein at its point of exit is compressed, which causes a backing-up of the blood just behind this point. The latter view is the one which appears supported by the results of the experiments just described. Here the pressure on the carotid prevented the arterial diastole, and so long as the pressure was kept up that part of the vein which normally pulsated remained in a semi-collapsed condition, *i. e.*, in the condition of so-called venous systole.

So soon, however, as the pressure was removed, and the heart's impulse again allowed to distend the artery, this por-

tion of the vein refilled, entering into the condition of venous diastole, and then the pulsation went on as before.

3. *Inhalation of Amyl Nitrite.* The *first* result of two or three full inhalations of this drug, poured upon a handkerchief, was that at once, even before the full sensorial effect had developed itself, the vein at the pulsating point became reduced in size, as it had done when pressure was made on the carotid; the pulsation did not, however, as in that case cease altogether, but became rapid, and sometimes imperceptible, always slight. No change could be detected in the appearance of the arteries at this stage, nor of the veins, except at the point referred to.

As a *secondary* result of the inhalations, which made its appearance just about the time that the sensorial symptoms began to abate, the veins refilled at the pulsating point, the pulsations themselves becoming again more manifest. A more exact account of the condition of the vein at this period will be given after the description of the rhythmical changes alluded to at the outset of the paper.

4. *Rhythmical Changes in the Venous Pulse.* When the portion of the veins above referred to was attentively watched, it was seen that besides the changes which have been recognized as constituting the venous pulse, there were other periodical variations in the size of the pulsating portion of the vessel. These latter occurred at intervals which, by their length, recalled the rhythmical changes in arterial tension, described by Traube, Hering, Cyon, and Sigmund Mayer, and which perhaps are the cause of the long waves of movement of the brain, noticed by Mosso and others, including one of ourselves. In other words, besides pulsating in the usual manner, the vein, at the point alluded to, was seen to dilate and contract gradually, in periods corresponding to about five respirations. The vein thus seemed to pulsate under the influence of two distinct systems of waves, one synchronous with the cardiac impulses, the other, the long waves, due perhaps to changes in arterial tension. Under the influence of these long waves the diameter of the vein varied, independently of the ordinary pulsations, often as much as in the proportion of two to one.

While such a wave was at its height, each ordinary pulsa-

tion seemed to diminish the diameter of the vein by about one-half, whereas at the lowest point of the wave, ordinary pulsations almost obliterated the calibre of the vein.

The passage from the highest to the lowest point of these long waves was gradual and pretty regular, though not absolutely so, the period of each wave occupying, as has been said, the time of about five respirations.

These waves were to be made out at every examination, when carefully sought for, but were not always equally* marked.

To recur now to the condition of the circulation during the second stage of the amyl experiments, when, as was said, the vein refilled, as the subjective symptoms began to diminish in intensity. It was noted that after refilling, the vein remained for a few seconds persistently distended to the size which it usually had at the height of the long waves, the cardiac pulsation continuing however, and then the long waves again began to show themselves.

In the first two experiments with amyl nitrite the period of inhalation was short, and it occurred to us as a possibility that the coincidence of the secondary filling of the vein with the passing off of the subjective symptoms might be only accidental. To determine this point the inhalation was repeated, and continued a much longer time than before.

In this instance, also, the vein remained narrowed as long as the effect of the drug was kept up, and again refilled as the subjective symptoms diminished. During this last experiment it did appear as if there were some slight increased fullness of all the veins, but it was, at the most, of trifling amount. No change in the general color of the disk was observed in this or any other experiment.

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* Dr. Wadsworth has since then observed this phenomenon in several other cases, in fact wherever it has been sought for, provided the pupil was of fair size, the media clear, and there was a well-marked venous pulse. It is not, however, intended to assert by this that the phenomenon must always be present under such circumstances.

ART. V.—MERITS AND MOTIVES OF THE MOVEMENT FOR ASYLUM REFORM.

BY EDWARD C. SPITZKA, M. D., NEW YORK.

(*An Address delivered before the New York Neurological Society.*)

IF a sweeping charge is made against a given system, even though the accompanying evidence may show it to be essentially truthful, yet such charge cannot become the basis for a proper conviction of the public mind, and consequent reform of abuses, until a period of time shall have elapsed sufficient to give those interested in the impeached system a fair opportunity for defense.

At the March meeting of this Society, the writer had made such charges against the inner circle, which, it was claimed, practically controlled the American Association of Medical Superintendents of Insane Asylums. Those charges and similar ones made before the New York Medico-Legal Society, received endorsements from the two committees appointed to report on the subject. Wide publicity has been given to the matter, and it may be supposed that through direct or indirect sources every superintendent of an asylum has been informed as to the facts. Since then, the American Association of Superintendents has held its annual meeting; delegates of that Association have also met in their special section of the American Medical Association, and three quarterly issues of the organ of the Asylum Association have been published. It is therefore fair to assume that the proceedings of the two meetings referred to, as well as the articles and comments in the *American Journal of Insanity*, constitute the extent of such defense as the present system of managing asylums is capable of.

I have therefore thought it well timed to review the whole discussion, and to recall professional attention to an important subject through this society. In fact, as the attorney of the Asylum Association has seen fit to cast aspersions and slurs

on the Neurological Society, it is fair that these should be taken up, as far as they can be taken up within the limits of parliamentary discussion, on this very floor.

It was anticipated by the writer, judging from the manner in which the writings of Wilbur, Folsom, Corson, Tourtellot, Storer, Allen and Sanborn had been treated by the asylum circle, that either no answer would be made to his paper, or that the only answer would be the well worn cry that its writer had never had an asylum under his charge, that he could, therefore, not know anything about the subject of insanity, and must necessarily be animated by personal motives.

In the main that prediction has come true; the editorial in the *Journal of Insanity* relating to the pending discussion states that the charges will not be taken up, "because the physician's time is but ill-spent in controversy," and further, that its writer will not stop to inquire where those active in criticizing asylums acquired their knowledge of insanity. We can understand the motives for his delicate handling of *that* subject; the reasons are clear to every member of this society!

But one form of argument has been employed by the defenders of the Association, which was not anticipated, as it could not have been, judging by the ordinary rules supposed to govern discussions in scientific bodies.

A gentleman who on a previous occasion* acted as the apologist and defender of his colleagues, assumed, and it appears from the discussion following his paper, was strongly pressed to assume, the advocacy of their cause in the present crisis. The reading of his paper was greeted with thundering applause by the Association as a body, the author received a perfect ovation of hand-shaking by its members, his paper was accorded the place of honor, and has been the only paper read at this year's meeting thus far published in the official organ of the Association;† contributions to his paper came

* "Mechanical Protection for the Violent Insane," read before the Association of Superintendents, at the meeting held in St. Louis, 1877, by Eugene Grissom, M. D.—*American Journal of Insanity*, July, 1877.

† "True and False Experts," read before the Association of Superintendents, at the meeting held in Washington, D. C., by Eugene Grissom, M. D. —*American Journal of Insanity*, July, 1878.

from superintendents in this and other States; in short, it must be regarded as the almost unanimous sense of the Association — we accept it as its most elaborate defense.

This defense might be disposed of very easily if taken up on its merits, for on those merits as a purely personal attack, in its language exceeding anything which has yet appeared in a professedly medical publication, we might refuse to consider it at all, and content ourselves with stating that such argument and writing were the most potent reasons for considering the criticisms which provoked it as well proven. But preferring to take up this subject only on an objective and exact basis, we shall consider the paper entitled "True and False Experts" just so far and no further than it may be considered as an indirect answer to "Reform in Scientific Psychiatry." That from the character and number of the arguments employed this subject is speedily exhausted is no fault of the present writer.

In the course of this paper, the defender of the Association, after devoting one-half of the same to a vituperative attack on a prominent physician, finds it necessary to explain to his hearers his reasons for so doing in the following words: "But in truth he is the type of a *reckless class of men* who are attempting to control the medical and even the secular press of the country, and to poison the public mind until they shall have worked upon popular ignorance and passion, to the destruction, as they hope, of the present method of providing for the insane in the United States. As individuals they are insignificant, but wild and unreasonable waves of folly sometimes arise in this country, and sweep with the velocity of our own prairie fires; * * * in fact, the *natural conservatism* of widely differing and separated classes of men throughout a great country like ours, as an *important factor in the social problem*, has almost disappeared under the rapid spread of consentaneous sentiment by modern methods of publication, aided by the telegraph."

In dealing with the term "reckless" as employed in this extract, our acceptation of the same as applied to ourselves depends entirely on the manner in which it was intended. If this term refers to our indifference as to how many

monopolies are destroyed and how many incompetent superintendents are weeded out, then certainly we are "reckless" on our own confession. That we are attempting to convince (not control) the medical journals, and to enlighten the secular press, we likewise plead guilty to; but we are not working on popular ignorance and passion, for it is only to the advantage of our cause to get the medical and lay public to take a greater interest of a *healthy and proper kind* in asylums. It is, however, "working on popular ignorance and passion" to attempt to appeal to religious and sectarian prejudices of the lowest kind, by attacking the religious belief of prominent advocates of reform, as is done by the Association of American Medical Superintendents. If we were attempting to arouse public attention by romantic accounts of illegal commitments of sane persons to asylums, the resulting "wave" of public feeling might reasonably have the term "folly" applied to it; but as we are endeavoring to convince those interested in these institutions that there are grave systemic defects, and are asserting, on the grounds of objective and inexpugnable testimony, that incompetent medical officers are appointed to asylums, and that the erection of extravagantly expensive hospitals tends to crowd out more than half of our insane from such benefits as asylum treatment is expected to give, we can justly claim that our efforts are tending to a cool and deliberate conviction of an intelligent laity and our own colleagues. The wave of public and professional feeling which may be expected, and is already commencing to "arise in this country," may not sweep over it with the "velocity" indicated, but it will certainly be considered a beneficial wave when its results become appreciated; we think that the absence of any cant, commonplace appeal or revolutionary tendencies from the present movement is its chief excellency. As to the latter part of the extract from "True and False Experts," if we do not interpret it incorrectly on account of an evident obscurity in style, it clearly proves that the modern instruments for disseminating knowledge are considered by superintendents to be inimical to themselves, and that their chief support is sought in the "natural conservatism" resulting from sectional feeling, local interests of an

exclusive nature, imperfect methods of communication and suppression of evidence.

The writer of the passage in question would undoubtedly have done his cause a better service by disproving at least some of the allegations made, or showing the fallacy of the arguments employed by those who differ from him, both as to questions of fact and opinion. He however contents himself with making counter-assertions, unaccompanied by argument of any kind, although the absence of argument is liberally made up for by personal invective. Thus, in reply to the conclusion, supported by the printed evidence furnished by asylum superintendents themselves, that the latter were far from perfect in their specialty, and either neglectful or untrustworthy as to their reported results, it is stated that "it is not outside the ranks of those who have given their lives to the practical cure and care of the insane, that Science will find her guides."

Then, as doubtless characterizing the manner in which this scientific guidance is supposed to take place, by the superintendents who endorsed this paper, he adds the following allegorical aspersion on pathological researches in insanity: "But it is at least absurd, that a captain who has sailed his ship over many stormy seas, should know less of navigation than the junk-dealer, who cuts up the hulk in port." By the "captain" is meant the asylum superintendent, by the "stormy seas," the career of the asylum patient, which may be boisterous and checkered enough under the influence of barbarous methods of restraint, callous medical supervision and narcotics, to justify the parallel; by the term "junk-dealer," the cerebral pathologist, who cuts up the "hulk," *i. e.*, the patient's body, in "port," that is, the autopsy room, is elegantly indicated. It seems that of all the allegations made against members of the asylum association, that referring to the absence or imperfect character of pathological researches, has called for the most comment. No member of the association has undertaken to show that our statement that their reported autopsies and microscopic results were founded on misinterpreted normal appearances, or artificial precipitates, was incorrect, nor has any member attempted to vindicate the

flagrant neglect of an important study. It seems strange, yet suggestive, that charges capable of either exact rebuttal or not, were not taken up. In stating that the photographs of cerebral tissue made in an asylum of this State, showed artificial precipitates, the writer laid himself open to a challenge of proof; as that challenge has not been made, I merely repeat my readiness to produce the artificial lesions in question in the brains of healthy *animals*, before the Association of Superintendents or a committee of that body.

The pathological aspect of the question is abandoned by the Association evidently, for on looking over their proceedings for this year, I find a statement is made in no relation whatever to the subject then under discussion, by a prominent superintendent, that "he did not believe that asylums were built for the purpose of furnishing autopsies." One involuntarily asks, why was this truism asserted in that place, for no one has to our knowledge asserted anything that could even be remotely construed as a belief that asylums were erected for such a purpose. That asylums are not founded with regard to future autopsies, does not alter the fact, that it is an unquestionable waste of a valuable material to neglect autopsies where the opportunity is offered. In reality the statement of that superintendent, made as it was, entirely unprovoked, was intended as an appeal to indifferentism, and must be condemned as an illegitimate attempt to attribute opinions to those interested in the present movement which they do not and cannot entertain.

It is further remarkable, that while, when pathological work is advocated by those who are not asylum superintendents, these advocates are disposed of with some such assertion as the one alluded to, this same work, even when demonstrably crude, deceptive and incorrect, becomes, when carried on by members of the inner asylum circle, the fit theme for those excessive adulations for which the Association is becoming celebrated to such a degree that one of their own number* did not hesitate to characterize his colleagues as a body of

* See in proceedings of Association, *Am. Jour. of Insanity*, July, 1878, the remarks of Dr. Wallace, of Texas, on "True and False Experts."

mutual admirationists. The same gentleman who stated that he did not believe that asylums were erected for the purpose of furnishing autopsies, can hardly measure his terms when landing at the same meeting, the report of a post-mortem made by a Superintendent, whose achievement of one hundred and forty-five autopsies on less than forty-two subjects, we have recorded on a former occasion.

With the three quotations given we have exhausted the direct defense of the Association, made in "True and False Experts." In dealing with the remainder of the paper as briefly as we can, and avoiding all criticisms of its language or style, we wish it to be understood that we exclude from our scope, the fact that it was written by a violent partisan, and regard it merely as the official promulgation of the Association's defense. Whether the paper entitled "True and False Experts" is a pathological product or not, as far as its writer is concerned, does not interest us here; it was adopted almost unanimously, not in the heat of a moment, but after twenty-four hours of recess, and after the protest of Dr. Wallace had given the members of the Association abundant time for reflection. It consequently assumes a grave significance.

The gentleman whose record as an expert and an army officer is introduced into the paper referred to, was clearly not assailed because of that record alone. Its writer, if he had the purpose of making an example of incompetent psychological experts, could have found abundant and appropriate material for criticism, far more conveniently situated. We have on a previous occasion* adverted to the gross errors committed by asylum superintendents in several prominent medico-legal cases, and those very cases formed the subject of a paper read at the same meeting before which "True and False Experts" was read, and adopted without comment or criticism! If it had been his purpose on the other hand to condemn doubtful financial transactions, a glance at the review columns of the JOURNAL OF NERVOUS AND MENTAL DISEASE† would have indicated to him a fitter subject for discussion than is furnished

* "Real Asylum Abuses," a paper read before the N. Y. Medico-Legal Soc., March, 1878. Waltz and Grappont cases.

† Vol. V., p. 781.

by the fees paid to a medico-legal expert and consulting physician. That the latter was made the subject of an unprecedented attack for no other reason than because he was a prominent member of two committees appointed to report on papers dealing with real or pretended asylum abuses, is self-evident.

The question immediately presents itself, why was merely a committee member attacked, why were not the papers in question attacked, if not answered, instead? Why, notwithstanding the abundant and early notice given, did no superintendent venture to take up either paper, at the meetings, when they were read? That the notice given was taken cognizance of, can be readily demonstrated by the fact that a leading Superintendent of this State, through his willing instruments, endeavored to suppress the reading of one of them! That this was done proves further, that the leading members of the Association did not refuse to discuss the paper on account of its alleged unimportant character. By adopting the course they have adopted, of attacking not the arguments, and questioning the facts adduced, but of vituperating a third party, the Asylum Association has been guilty of a *petitio principii*.

To charges that medical superintendents neglect or distort the pathological study of insanity, they reply that asylums are not built for the purpose of furnishing autopsies; to the assertion that they are *not* appointed to their positions on grounds of special proficiency or ability, they answer that a member of this Society is a "false expert;" and to the statement that they treat their patients with a neglectful and cruel routine, and cause damage by employing unphysiological methods of restraint, their only reply is to call up religious and sectarian prejudices of the most doubtful character, in their favor! We need not ask the question of any enlightened body of colleagues, whether such a procedure could be employed by any Association which has the shadow of a right to call itself a scientific one. Nor need we suggest that the justness of their cause is to be measured by the weapons they have employed.

In declining to discuss the main points at issue, and first de-

viating from the path of open and truthful discussion, the Association has shown the innate weakness of its position, and we conclude this to be the best omen in favor of the ultimate success of Asylum Reform. For while it is easier to question an opponent's motives than to disprove his arguments, the pending question, which it would in its present state be absurd to refer to personal grievances, but which is founded on deep-seated systemic abuses, cannot be argued on personal, religious and political, but solely on scientific grounds.

The position of the outside profession, and especially of those interested in nervous diseases, is not that they have merely the right to inquire into the subject of asylum abuses, but it actually becomes their duty to do so, as the sole guardians of the public in medical matters. Nor need they wait as timid apologists for such fragmentary volunteer information as those at the head of asylums may condescend to furnish, but they should insist on a personal examination, with all the authority which their position as public enlighteners gives them. In such an examination, the personal feelings, the supposed monopolies of superintendents, and the slanderous opprobrium showered down on all who may venture to question the immaculate character of the latter, can form obstacles of no moment. The chief obstacles in the way of the inquiry will be the confused manner in which the records are kept, and the, I regret to say, misrepresentation to which some superintendents do not hesitate to resort. Such misrepresentation has not failed to blind the eyes of a no less acute observer than Dr. Bucknill, who, when he visited an asylum of this State, was told that they "did not use restraints" by the superintendent, but also found the latter indisposed to discuss the subject. The fact was that the cribs and other instruments of restraint were removed and locked up, and came to light only after Dr. Bucknill was safely on board the vessel which was to carry him back to England. That superintendent subsequently endeavored to elude the quandary in which he was placed, by stating that he had said "we do not use seclusions." What Dr. Bucknill, who had seen with his own eyes that restraint had been abandoned at least during the term of his visit, thought of this excuse may be judged from

the fact, that he has not changed one word of his text, but prints in a foot note the second misrepresentation (which was to remedy the first) without a word of comment.

I have strong circumstantial and also exact evidence, that in an asylum to be more specifically indicated on some future occasion, a certain proportion of patients who die from the results of excessive restraint and violence at the hands of attendants, or from poisoning by narcotics, are knowingly reported to have died from incidental affections, in contravention of both the truth and the law. How closely intrenched superintendents are behind the vail of secrecy which covers most of the transactions in an asylum, could be illustrated no better than by the following case: A patient is brought to an asylum in a very weak and atonic state, suffering, so the superintendent expresses it, from "softening of the brain;" he is quartered with another patient, who for years has been quiet and orderly, whose form of insanity did not manifest itself in maniacal or destructive acts. One morning it becomes necessary to transfer the first patient to the hospital ward, he being nearly beaten to death; he has long straight marks upon his back, across his face and chest. As a result he is so weak that he can hardly speak distinctly, but has sufficient recollection of the occurrence to state that the person who abused him wore the uniform of an attendant. He never rallies, but dies from the effects of the injuries received. The death certificate states the cause of death to have been softening of the brain, no autopsy having been made.

Now the relatives unfortunately not notifying the coroner in time, made the matter the subject of a newspaper inquiry, and the superintendent gave the following explanation of the occurrence: "The new patient attacked the other one, previously in the room, during the night; no noise was heard by anyone." The utensil used was stated to have been a chamber-pot, which produced marks linear in direction and of considerable length as aforesaid! At the same time no such utensil was reported as broken from that ward. The hospital attendant on being pressed to give his opinion as to the nature of the bruises by one of the commissioners, for a long time hesitated, fearful of losing his position if testifying against a superior officer.

but being guaranteed by the commissioner against such a result, stated with decision that the marks in question must have been produced by a club or strap. The decided way in which the Medical Superintendent gave his testimony sounds remarkable, when it is recollected that he was not in the asylum at the time, and did not return to it for twelve hours after the fracas. No one who has the ability of judging as to the value of evidence from its inherent character, will come to any other conclusion than that the patient in question probably died from the results of his injuries, or at any rate that his death was accelerated thereby, that he was beaten with a club or stick in the hands of an attendant, and that the story referring to a quarrel between two patients, and the usage of a chamber-pot as an offensive weapon, as well as the assigned cause of death, constitute a concocted evolution from the imagination.

But let us accept the explanation of the asylum authorities as true; what shall we say of a management which permits two patients to beat each other to death with weapons whose wielding implies the production of considerable noise at the dead of night? What of an asylum whose record of accidents during the current year, a record which is by no means exhaustive, sounds like the list of casualties of a Bulgarian campaign? Three patients beaten to death, one of whom has twelve ribs broken! One patient boiled to death, by having the hot water turned on him in a bath, while the attendant went out of the asylum building, leaving the helpless paralytic to his horrible fate, and several patients drowned, by falling off the asylum dock, in epileptic convulsions! These accidents do not exhaust the list. At least an equal number are not reported, and even the few brought to light, would not be so, but for the vicinity of the asylum in question to a large city. If concealment is possible even here, what may not be possible in an asylum remote from any capital?

We have not even the consolation afforded us, that of protecting the insane wards of the State; if the State Commissioner of Lunacy had the good intentions, he would be able to get witnesses to alleged atrocities and abuses. It is a notorious fact, that the financial and political patronage wielded by some superintendents, is such that no subaltern officer would dare

to breathe a whisper against the management or to testify even as to matters of fact, for fear not only of losing his position, but of becoming ostracised, by the town near the asylum, whose real or supposed interests are tied up in such an institution. Without remedying the disgraceful system of rendering it a matter of pride for a few provincial politicians, banded together as a board of trustees, to rob the State at large unblushingly for the benefit of local interests, and to hushing up everything that might reflect on their management, we cannot hope to obviate the individual abuses which are the outcome of this system. This system of fostering conservatism and corruption, which wisely endeavors to screen its mechanism from the public, is the most potent defensive weapon of the asylum association.

One would suppose that in the abstract, the appointment of a Commissioner in Lunacy should act as a check to abuse. Those who presupposed this evidently did not permit the reflection to enter their calculations, that to appoint an individual who had never had any experience with the insane, but was the close personal friend of the superintendents, to such a position, would defeat the true intent of the law. So it has proved. In the instance of this State a State Commissioner appointed to stand between asylum officer and patient, where abuses exist or are possible, actually stands between the public and the superintendents, to the prejudice of the former. We regret that as a single officer, the *man* is not separable from the *office*, which latter alone furnishes our subject for criticism; but the frequency with which superintendents have in this State crushed investigation by referring to the fact, that abundant supervision was provided for by the appointment of this officer, has become such that a little light shed on his activity may be of use in the study of the subject.

It has been shown by published official reports, quoted by Dr. Wilbur,* that our State Commissioner of Lunacy, to use that writer's words, "directly and indirectly commended the management" of the criminal insane asylum at Auburn. At

* "Management of the Insane in Great Britain," by H. B. Wilbur M. D., Medical Superintendent of the State Asylum for Idiots at Syracuse.

a time when patients had pistol-shots fired into them by the superintendent, where patients were hung up by the wrists for hours, where the wards were a stench to the nostrils, the food unwholesome, the bedding covered with vermin, and the closets reeking with filth; where punishments were the order of the day, even women being "paddled," and where attendants freely confessed that the superintendent had "blackened the eyes" of patients, and that they did not think anything of following his chivalrous example. It is a matter of notoriety to every one here present to-night, that the Commissioner of Lunacy has not unearthed or reformed a single abuse while in office, but that such unearthing has uniformly come from the daily press, or from the relatives of those involved in the maltreatment to which patients are subjected. It is equally a matter of record and public knowledge that changes of the most remarkable character have been made by the trustees or commissioners supervising asylums, changes involving the removal of indifferent men, replacing them occasionally by those who were worse, or no better, without that the State officer referred to, saw fit to interfere. So long as by these promotions, removals and supersedences, no member of the asylum circle suffered, or no opponent of the asylum circle profited, the Commissioner of Lunacy maintained a dignified reserve. But on a recent occasion his solicitude for the interests of the asylum circle has come out with so little concealment, and we were about to say with such strange ostentation, that we in this single instance discover what he conceives to constitute the true functions of his office, namely, to maintain inviolate the principles which, analogous to those which govern "trades-unions," rule the Association of Medical Superintendents. For all the commissioner cared, patients might force their way through defective window bars in the Flatbush Asylum, and kill themselves by falling from the fourth story of the building; weekly escapes of dangerous lunatics might vary the somewhat tame character of the Brooklyn newspaper column of items; attendants commit, unpunished, the most cruel atrocities on helpless patients. It was not until a physician known to be an advocate of reform in our institutions for the insane, was appointed to that asylum, that

he saw fit to interfere, and has endeavored, as we hear, unsuccessfully, to annul the appointment. What shall be said of such outspoken advocacy of the very circle he is appointed to supervise, what of the dictum coming from him who refers the etiology of insanity to a corn and salt pork diet,* that the new appointee has never had charge of the insane, and can consequently know nothing about insanity? Why has he permitted physicians to occupy such positions who not only had never even visited an asylum, but who did not make nor possess the remotest claim to being considered neurologists? Finally, before what tribunal has the State Commissioner of Lunacy demonstrated his own knowledge of the subject, such knowledge being necessarily a *conditio sine qua non* of his liberty to judge of the knowledge of others? Probably before a tribunal composed of such superintendents as have made out "feigned" and "moral" insanity to be identical, who are incapable of either classifying or diagnosticating insanity, or who from the fact that by employing the same reagents, they have produced the same artificial lesions in the brains of those suffering from the most opposite forms of insanity, arrive at the conclusion that insanity is the result of a single *diathesis*!† We can hardly find language appropriate to the occasion, to characterize the action of the Commissioner, without violating the principles of scientific discussion.

We think from what has been thus far adduced, that it is patent, that since our charges were first made, not a single one has been materially impeached. To our objections against the inhuman abuse of restraint, the answer is a panegyric of the crib,‡ the same apparatus which, singularly enough,

* Annual Report of the State Commissioner of Lunacy, 1876, pages 15-17.

† J. P. Gray. Pathology of Insanity. *American Journal of Insanity*, July, 1874, page 12, lines 29-30.

‡ *American Journal of Insanity*, April, 1878. Editorial. The editor, besides adducing confirmatory evidence from a British conservative, one of those who proves that certain characteristics of American Superintendents have not yet been reformed away in Great Britain, attempts to sneer down our statement that, the struggles of the patient in the crib coupled with the horizontal position induced by that instrument, produced congestion in an already hyperæmic brain, of such a degree as to provoke permanent structural damage, if not actually and immediately fatal to the patient. That

although abundantly represented in the wards of the Utica asylum, both before and after Dr. Bucknill's visit, could not be found (at least in unlocked rooms) while Dr. Bucknill was present.

Our charges as to the neglect of pathological and other scientific work, have been answered in the manner indicated previously. No attempt has been made to reply to our strictures on the financial management, the discharge of dangerously insane patients, the improper retention of those who are cured, or harmlessly insane, the designed or undesigned falsification of statistics relating to the increase of insanity and recovery of patients, as well as of the death-rate! And yet most of these charges are supported by the evidence of the best alienists in this country. But it is not only on the confirmatory evidence of the independent and fearless among the superintendents of this land, such as Pliny Earle, Chapin, Reynolds and a few others, that we find a strong support of the truth and justness of our allegations: even a transatlantic journal of no mean merit has endorsed them in the main.* Overwhelming evidence is crowding in from every quarter in their favor, and before long an official committee will doubtless

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opinion, confirmed by one autopsy, made by myself, was promulgated among others by the great psychiatric teacher of central Europe, Professor Meynert, and the contumely with which the editor of the Asylum journal treats our statement, applies as a result to Professor Meynert himself. We need not add that it falls back on that editor with double force. In some of his earlier compilations, congestion of the brain is obscurely brought in as a basis of insanity. He has evidently forgotten this, and this we could readily understand, if the statement is true that the editor in question writes his pathological products with the pen of a special pathologist.

* The *Journal of Mental Science* accepts the facts given in "Reform in Scientific Psychiatry" as correct, and warns the Association of Superintendents that the "close corporations way" should end, a warning which we do not think will be heeded. As to the strictures on the style and terms of that paper, I have nothing to add to the editorial vindication made in this journal's last number, except that I certainly have the excuse of precedent. Two years ago there was a controversy in the *Journal of Mental Science* between one of its editors and Dr. Crichton Browne, in relation to the classification of Skae. It was a purely scientific controversy, while our American Asylum controversy has to deal with financial and other misrepresentations of the most flagrant character; Dr. Clouston's reply to Dr. Browne is, however, couched in language far stronger than my own, when the occasion is taken into account.

have the opportunity of locating the abuses adverted to more accurately, than is possible within the limits of this paper.

I think that we all look to some official mode of taking evidence in this matter, in the hope that as soon as this evidence becomes a matter of record, the bitter tone adopted by those engaged in the controversy will cease, and every excuse for making personal onslaughts and questioning motives will be removed. The discussion of this subject has now reached a point beyond which it cannot proceed much for the present; thus far the object of those who are criticizing asylums has merely been to show that an investigation was not only desirable, but highly necessary. When the results of that examination shall be made public, then they will be called on to suggest remedies for abuses, and improvements of defects. If circumstances over which they have no present control should prove favorable, it will lie in their hands, not only to elevate science, to raise the tone of an important branch of the medical profession, to benefit both the practitioner, student and clinical teacher, and to ameliorate the condition of the insane, but even to improve on the asylums of the Old World, and to render ours models.

In none of the criticisms on American Asylums has it been even hinted that American physicians and the American people could do no better in this field. We were content to show that a close corporation and an unfortunate fusion of political with medical interests (always a monstrosity) has prevented them from exercising their genius and talents to the full of their natural extent. On the contrary, we see no reason why, supported by a public which has ever stood foremost among the nations of the earth when financial sacrifices were required for charitable purposes, the asylum physician of the future on this side of the Atlantic should not fairly rival his colleagues on the other. It is true that progress in this field cannot be sudden. Hard, earnest labor alone will be able to draw the field of psychiatry from the slough of despond into which it has sunken in every scientific and administrative respect, but a cool, honest, and logical exchange of views between the fair-minded will certainly result in a uniform and enlightened system of managing insane asylums in the near future.

No *a priori* or hasty generalization should be allowed to tincture our cause with even the slightest shadow of an error, nor should we declare ourselves the blind followers of any exclusive system, without having first made a deliberate and careful trial on the merits of such system ourselves. The progress of true opinion is necessarily slow, and it is rather checked than advanced by the employment of invective or commonplace. To invective and commonplace, as employed against the advocates of reform, the latter can well afford to be silent, for even the deserved irony with which those who wield such weapons may be treated by the maligned individuals merely furnishes an excuse for a repetition of the offense.

We already perceive the dawn of the day when incompetence and unreliability in high places will cease to exist; when the respectable minority of superintendents, whose only fault in the past has been that they have shown too little energy in combating the conservative spirit of their colleagues, will find it high time to awaken from their passivity, and deprive the opponents of the pending movement of that clamorous argument (untrue even as it stands) that not one of the advocates of reform has ever been in an asylum as a physician.

One prominent superintendent has already been cited before the Comptroller of his State to show his financial vouchers for the past five years, and it is very likely that if the investigation of these documents proves to be more than a surface examination that the revelations of extravagances and looseness in accounts will considerably startle the general public. Two superintendents, both bitter enemies of investigation and liberal management, have, we blush to record it, been dismissed from their positions for being repeatedly intoxicated, and there is other evidence at our disposal to show that breaches of professional behavior may become too gross even for trustees of asylums to tolerate. These acts, individually, are of no import, but they serve to show that, notwithstanding the strong political and personal influence enjoyed by such men, their positions are not always invulnerable.

The action of Dr. Hiram Corson in reference to the asylums of Pennsylvania may serve as an example which it will be advantageous to follow.

That physician, finding that many of the delusions of female patients bore reference to the male assistant, and imbued with the fact that where uterine or other pelvic disease had an etiological relation to the insanity of such patients, mutual embarrassment might and would operate as an obstacle to a free communication of the symptoms, was induced to consider the feasibility of having at least one female physician appointed for the female side of the hospital. He found that there were other reasons for considering such an innovation as a good one, and as trustee of the Harrisburgh Asylum, he suggested this plan. It was, as might be expected, met with ridicule, and opposed rancorously by the Superintendent. Dr. Corson, however, submitted his project to the Medical Society of his State. It was unanimously indorsed, and the influence of the society has been brought to bear upon governmental circles in such a way that we have no reason to doubt that this excellent innovation will be made. If it is, then I am borne out in my prediction that possibly American asylums may become the pioneers in such reforms as have not even been contemplated in the Old World.

It may be well to propose an outline of the objects of reform, both to give a basis for the discussion, which I hope will follow within as without the Society, as well as to deprive those who resort to improper methods of debate, of every excuse for misrepresenting our objects and motives.

I. Mechanical restraint of any nature, as well as the employment of such narcotics as permanently damage the nervous system, should be reduced to the greatest possible minimum.

II. Such methods of mechanical restraint as are cruel and unphysiological, should be forbidden by law, and no instrument, which like the crib, is open to such objection, should be permitted within the confines of any hospital for the insane.

III. A careful physiological and therapeutical study of those neurotic medicines, which like the bromides, chloral hydrate, opium and amyl nitrite are extensively used in asylums, should be made, as a basis for a future more rational therapeusis, founded not on routine principles, but on individual indications.

IV. Decentralization of the asylum buildings, being the plan

which is recognized as the best, both from a hygienic and financial point of view, by those engaged in constructing general hospitals, and demonstrated to have been accompanied by excellent results, in the case of such asylums as that at Willard, N. Y., should be adopted as a basis for all future asylum constructions.

V. More stress must be laid on furnishing employment, suitable to the habits and inclinations of patients, as an important factor in mental therapeutics, than is at present done.

VI. The medical superintendent ought to become an exclusively medical officer, and delegate such duties as have no immediate relation to the welfare of his patients to civil officials.

VII. Every district which is taxed to support an asylum, should have an adequate representation in the Board of Trustees of that institution. The cities of New York and Brooklyn pay more than one-half the cost of erection and maintenance of State asylums like that at Utica, but neither of these cities enjoys the slightest benefit from these institutions, preferring to build their own asylums to paying the exorbitant charges imposed by a local Board of Trustees. This is a clear violation of the principles on which a Republic is founded, whose forefathers declared that taxation without representation is tyranny.

VIII. An accurate register should be kept, not only of the number of patients as a whole, but of the actual form of insanity from which they are suffering. In the statistics of causation, only causes to which the disease can be clearly assigned should be taken account of: the elastic terms, "overwork," "excitement," and the like are too frequently rendered a refuge for all cases which cannot be otherwise determined.

IX. In order to be able to compare the death rates properly, the annual reports should not limit themselves to chronicling the deaths which take place in the asylum, but also those which take place in the case of persons discharged insane, and suffering from fatal diseases contracted prior to or during the asylum sojourn.

X. Each asylum should have its assigned district, and should

publish with its own tables a table exhibiting the number of insane in that district, who have either been already inmates of the asylum, now discharged, or whom it has never been found necessary or possible to send there. No opportunity otherwise exists of determining the utility of the asylum, or the rate of increase or decrease of insanity in a given territory.

XI. A general and equitable distribution of the means for treating the insane ought to be made; it is unjust and uncharitable to construct asylums at such a cost that only a limited number of the insane can be accommodated, while an equal or even a larger number are crowded out.

XII. In the registered tables of admissions to asylums, it should be distinctly stated how many are cases of re-admission, and whether on the occasion of their previous discharge they were reported as cured, improved or unimproved.

XIII. Every case of seclusion or restraint should be reported, and the reasons for such restraint or seclusion given. Every accident, its details and causes, avoidable or unavoidable, and the results of all official inquiries relating to the same, should be briefly published in the annual reports.

XIV. Autopsies in public asylums should be made compulsory by legal enactment, and the coroner of the district should be authorized to be present at every autopsy made, if he arrives within a reasonable time. Accurate register of the morbid appearances, especially of the nervous system, of the weight of the nervous centres in their aggregate, and in segments, and of asymmetries in the pedunculer tracts, and aberrations of the convolutions, should be taken, and preserved for future study. The autopsy should under all circumstances be made by the asylum physicians themselves, as none of the manipulations above alluded to need to conflict with the purposes of an inquest, when the latter is necessary.

XV. At least one-third of the number constituting a Board of Trustees of a public Lunatic Asylum, should be composed of physicians; the legal profession ought also to be adequately represented in that body.

XVI. Physicians should not be appointed for a given number of years, but for life and during good behavior. It may be desirable to fix a limit as to continuance in office during

old age, in which case a pension-minimum for old and faithful servants of the public may fairly be asked for.

XVII. In every large State, at least three Commissioners of Lunacy will be found necessary; one of these Commissioners should be a medical practitioner, the second a lawyer, and the third a physician who has at some time himself been a medical officer in an asylum.

XVIII. No asylum medical officer should be discharged from his position, except for incompetency, misdemeanor or disability in office. Deception in statistical reports, or financial misrepresentations naturally come under the second head, and should be so specified in the code relating to asylums.

XIX. The Statutes relating to the care and custody of the insane, in most of our States, require a careful and thorough revision.

XX. As soon as it will become possible to unite the supervision of our asylums in some central National Board of Officers, the State Commissioners in Lunacy should be superseded by National Commissioners.

These are a few of the suggestions which present themselves, and which, subject to the approval of the Society, may approximately reveal the objects of those now engaged in working out the problem of a scientific and liberal mangement of our asylums. There are many other topics relating to the care of the chronic insane as contrasted with the care and cure of the recent insane, and the combined or separated management of those of different sexes suffering from mental disease, as well as the distribution of the insane in wards of different construction, in accordance with the different requirements of their cases, whose consideration we defer to some future time. Then it may have become more evident, than it now is, that the medical profession as a whole, and those engaged in the study of nervous diseases in particular, will be called upon to rectify the short-comings of those who, as superintendents of insane asylums, have failed to recognize, that accuracy as to reported facts, correct logic in forming conclusions, and a liberal scientific spirit, are far better defenses against outside distrust and prejudice than monopoly, secrecy and cant.

ART. VI.—METALLOSCOPY AND METALLOTHERAPY.

BY H. GRADLE, CHICAGO.

DURING the last two years French medical periodicals have published a series of notes on the above subjects, of which each succeeding article surpassed its predecessor in the marvelousness of its results. Naturally enough the articles were received everywhere with avowed scepticism; the reputation, however, of the writers, as well as the agreement in results of different independent observers, must invite a conscientious study of the subject.

M. Burq, the inventor of the procedure, has maintained his ideas on this subject for some twenty-five years, but, with the exception of Trousseau, no one seems to have paid any attention to it until Professor Charcot invited Burq to experiment in his wards. Subsequently the Society of Biology, of Paris, appointed a commission, consisting of MM. Charcot, Luys and Dumontpallier to investigate the subject. Their various reports can be found in the proceedings of that society of 1877 and 1878. According to a citation of Charcot an instance of true metallotherapy is found in an old work by Wichmann, "Ideen zur Diagnostik," published in 1778. Repeated a few times by others, it was still never practiced methodically, and was gradually forgotten. M. Burq has, hence, the undoubted merit of creating the method of external applications of metals for diagnostic and therapeutic purposes.

The disease in which most experiments have been made is hysteria, of which a number of cases had the graver form of hystero-epilepsy. The typical case, always female, figuring in most descriptions, is about the following: Anæsthesia, more or less complete in degree and extent of one side of the body, impairment or inactivity of the organs of special sense of that side, and relative muscular weakness of the same side—amyosthenia of the French—besides usually tenderness of the corresponding ovary. But a disease as variable in its manifestations

as hysteria, creates of course numerous complications, such as hyperæsthesia, contractures, etc., to be found in some of the cases. The procedure of metalloscopy consists in removing or altering these symptoms by contact of the skin with some metal. Each patient is said to be sensitive to *only one* metal, rarely more, and to no others, and hence the first step must be to discover the idiosyncrasy of the patient. M. Burq gives the following list as the scale of frequency of influence of the different metals: iron, copper, zinc, gold, silver, tin, rarely platinum, and probably arsenic and antimony. Metallic discs of the size of large coins, or wooden discs coated with the metal, are employed, being arranged in the form of a bracelet around the member, or simply held in place by a bandage. Within one to fifteen minutes objective changes can be seen. The anæsthetic skin, formerly cold, pale, and almost bloodless when pricked, now becomes reddened, warmer, and even hyperæmic: the patient complains of numbness of that spot, and the sensibility begins to return. There is at first a stage of inaccurate sensibility when warmth is taken for cold, and *vice versa*, but soon the normal state is reached. The patient, formerly insensible to deep pricks, will now shun a mere touch of the pin. At the same time the special senses return to their normal activity, and the dynamometer indicates now a full strength of the formerly enfeebled muscles. Where a contracture existed it usually disappears. But on the other, formerly normal, side of the body a peculiar change can now be found. Exactly what one side has gained, the other side has lost. When the anæsthesia disappears only in the vicinity of the metallic disc, the symmetrical spot on the previously normal side of the body loses its sensibility to the same extent, while in those cases in which the metallic application removes the *existing* anæsthesia completely there still remains a hemianæsthesia, it being only transferred to the other side. What one ear, nostril, or half of the organ of taste gains, the other loses in perception. Even the one-sided increase in muscular strength is obtained at the expense of the other side of the body. In short, the patient has gained nothing, there has only been a "transfer" of sensibility.

The original anæsthesia disappears at first at the point of

metallic contact, subsequently in the vicinity, to the extent of some 6 or 8 centimetres. If the discs are now removed, the return of sensibility continues, and, finally, the original anæsthesia disappears completely. At the same time the *loss of sensibility* of the other side follows the identical course. The phenomena last some hours, rarely longer than one day in any hysterical case, when everything returns to its former condition. If, however, the metal *is left in place* continuously, an entire change in the course is observed within a short time. The returned sensibility, instead of continuing to spread, suddenly disappears again, a corresponding change taking place also on the other side. Both sides have returned to their original condition, while the metal is still applied. In fact, the anæsthesia and amyosthenia may be even more complete than they were originally. This state M. Burq calls the return anæsthesia.

The return anæsthesia is called by Charcot a test for the hysterical diathesis. If in a hysterical patient all manifestations of the disease have disappeared in the course of some treatment, the application of the metal, to which she was sensitive previously, causes no change whatever in the case of a perfect cure. If, however, the hysterical diathesis still exists, though latent, the metalloscopic application gives rise to a feeling of numbness, malaise and drowsiness, often causing sleep, while the sensibility disappears completely at the place of application.

It had been known to Burq for some time, that a metal to which a patient had previously been found sensitive seemed to lose its influence completely when covered with another disc of a different metal, which was known to possess by itself *no* influence over that patient. On investigating this question further, Dr. R. Vigouroux found, moreover, that the addition of a second *indifferent* metal prevented any further change of the condition existing at the time of application of the second metal. Thus, a patient found sensitive to the influence of gold, but not to copper, could have his sensibility restored (of course at the expense of the other side) by an application of some gold coins: if now an equal number of copper discs were placed either on the gold or simply on the adjoining skin, the *statu quo* could be preserved for an indefinite time. But, if the

copper was not applied until the return anæsthesia had set in, that state now became the permanent condition as long as the metals were left in place.

External metallotherapy consists in the repeated application of the active metal. No one, however, claims any actual experience in this except M. Burq. According to him a succession of metalloscopic attempts, continued every time until the return anæsthesia sets in, ameliorates the hysterical symptoms gradually, and a cure can be obtained in the space of some months. Lately, however, Burq has himself abandoned this method for internal metallotherapy.

Statements like these, which seem at first to baffle completely all attempts at explanation should be received with caution. But the question: "Are they facts?" must be answered affirmatively. The reputation of the French observers suffices to remove all suspicions. The experiments have besides been verified by others, for instance, by Westphal* and by Maragliano and Seppilli (*Rivista Sperimentale di Freniatria*, etc., 1878, fasc. I., p. 36). Scepticism is awakened in the minds of many when the word hysteria is mentioned. No doubt many hysterical patients do lie and simulate to their heart's content, but still no one is *a priori* justified in calling everything deception, when an impartial, careful observer describes what he has observed in hysterical patients. Numerous foreign physicians (the writer himself amongst them) had followed the researches in Charcot's wards, and found them as certain as physical experiments. Perhaps one of the best guards against deception on the part of the patients is the agreement of results in hysterical amblyopia, the details of which the patients could not possibly know beforehand.

The extreme periphery of the healthy retina cannot perceive all colors with ordinary illumination. The field of vision, larger for the perception of light in general, than for any one color, is not the same for the different colors. Violet can be perceived by only the more central parts of the retina, green has a larger field of vision, next in order are red, orange and

Berl. klin. Wochenschrift, No. 30, 1878. Unfortunately the original was not at hand.

yellow, while blue is still perceived by the most peripheral zone. In an ideal schema we can conceive the retina to consist of a series of superimposed layers, each for the perception of a different color, of which the *blue* layer is the largest, and the violet the smallest.

This normal condition is exaggerated in hysterical amblyopia. Besides the reduced visual acuity we find in this trouble a characteristic limitation of the field of vision, for light in general, and still more so for colors. Referring to the ideal schema, it might be said, that the difference between the extent of the different color-perceiving layers is increased. Hence all degrees may be found, from a mere limitation of the visual field, to the absence of one or more central colors, as violet, green, etc., or even the entire absence of all color-perception. In the latter case all nature appears to the patient like a crayon-drawing. On applying a disc of active metal to the temple of such a patient, the observer can follow step by step the increase in visual acuity, and the gradual peripheral extension of the color-perceiving zones. If all perception of colors had been lost, the colors re-appear in the order of their peripheral extension; blue can be recognized first, violet last. On keeping the metal in place the return anæsthesia can be studied with equal ease Violet disappears first, blue last, and the amblyopia is sometimes more intense than before the experiment, returning, however, to its former condition on removal of the metal. If the metal is removed before the return anæsthesia sets in, the visual improvement lasts for some hours. The positiveness and unanimity with which different, unsophisticated patients pronounce upon the order of appearance and disappearance of the colors bears very little resemblance to deception. A unilateral application of the active metal removes the amblyopia at the expense of the other eye, *i. e.*, simple transfer of sensibility. Cases have, however, been reported, in which no transfer occurred, a bilateral or median application improving simultaneously the visual power of both eyes.

Not alone in hysteria, but also in organic troubles is metalloscopy applicable. Strange to say, there was no transfer of sensibility in these cases, the normal side remaining unaltered,

and the effects were permanent. In a recent lecture (*Gaz. des Hop.*, March 14, 1878) Charcot refers to two cases of hemianæsthesia in consequence of cerebral hemorrhage, one a girl of twenty-four years afflicted since youth, the other an old woman of fifty-four years in that condition for twelve years, both without hysterical taint. A single application of iron in one case, of gold in the other, restored both special and ordinary sensibility at once, without transfer, and this result has remained so for the last fifteen months. Charcot's explanation is, that not all fibres of the internal capsule were involved in the destructive process, but that a number remained healthy, but inactive, dormant so to speak; the integrity of the latter would account for the restored sensibility. Another case of hemianæsthesia and hemichorea after apoplexy (perhaps the same one?) was reported by Landolt and Oulmont (*Progrès Méd.*, May 19, 1877), in which the sensibility was completely restored, and the hemichorea ameliorated by a single application of gold. In spinal affections, however, Charcot has never seen any success whatever from metalloscopic attempts. He admits also to have met with some hysterical patients in whom metalloscopy did not succeed.

In order to examine the role of electricity in these phenomena, M. Regnard investigated this matter at the demand of the commission with a delicate galvanometer. He found that the contact of any metal with the dry skin gave rise to an electric current, the strength of which could be determined by the deviation of the needle of the galvanometer. This current is due to the contact; it depends on the difference in electric tension on the principle of the dry voltaic pile, and not necessarily on any chemical action. Eulenburg* has, however, pointed out an error of Regnard's, in using polarizable electrodes, and hence obtaining exaggerated needle-deviations. He, himself, employed the unpolarizable electrode of DuBois-Reymond (glass tubes, closed at the end with a plug of clay, moistened with a 1 per cent. solution of common salt, and filled with a saturated solution of sulphate of zinc, into which are dipped zinc rods connected with the wires of the

* *Deutsche med. Wochenschrift*, July 22, 1878.

galvanometer). With these means he obtained smaller deviations of the needle, but confirmed the statement that a current is generated by contact of the skin with metallic discs, that the strength and direction of this current vary according to the metal employed, but are not the same with the same metal in all subjects. This variance between different individuals, Eulenburg refers to differences in the chemical nature of the cutaneous secretion, which is apt to be exaggerated in nervous affections. By placing between the metal and skin a piece of dry paper, the current was stopped; by employing a paper saturated with a solution of salt, the current was intensified. Chemically pure gold and platinum were found almost inactive by Eulenburg and by Regnard, and are also said to possess little if any therapeutic action.

Regnard,* moreover, found that all metalloscopic effects could be produced by employing a feeble galvanic current, yielding the same deviation of the galvanometer as the contact of the metal to which that patient was sensitive.

However, from his detailed description it appears that success is not closely limited to currents of *exactly* the same needle-deviation as that of the active metal, but that the limits are quite extended. Besides, the same deviation of the galvanometer does not by any means imply identity of any two currents in both quantity and tension. A patient, for instance, whose sensibility was restored by a current of 2° needle-deviation, was not affected by a current of 15°, while 40° and more again produced the desired effect. Another was affected by 15°, not by 40°, but again by 80°. A third was sensitive to 40° and to 90°, while a medium current of 70° remained inactive. There are hence in every case certain varying limits of intensity within which a current restores sensibility; between the extreme limits, however, there exist "neutral points" at which the current loses its influence.

But these researches, interesting as they are, are far from furnishing a satisfactory explanation of metalloscopy. No one can suppose that these feeble currents are diffused to the extent of the returned sensibility, either superficially, when the

* Societe de Biologie, Seance du Fev. 3 and 17, 1877.

returning sensibility spreads from the point of application over the entire side, or toward the interior, when the optic or auditory nerves resume their functions after application of a coin to the temple. Besides we can employ other means for the same purpose. It has been known since Duchenne's time, that hemianæsthesia, dynamic or organic, can sometimes be removed by a single faradization. Numerous cases are also reported of similar effects produced (especially in hysteria) by the action of magnets at a distance.*

By charging the patient seated upon an insulated chair with static electricity, the return of muscular strength, of color perception and of sensibility throughout, can also be effected, but these functions disappear again on interrupting the electrization.

M. Thermes† has even succeeded in obtaining all usual metalloscopic phenomena (including the transfer of sensibility) by the application of a piece of ice in a case of hysteria, while Westphal (loc. cit.) claims to have had an equal success with varnished or even non-metallic discs and sinapisms.

The most satisfactory explanation, as far as it goes, has been given by Vigouroux (*Gaz. des Hopitaux*, Nov. 17, 1877). He starts from the fact, that a metal known to be active in a certain case loses its influence apparently by being covered with another indifferent metal. In any voltaic pile the difference between the electric tension of the first and of the last metal does not depend on the intervening metals. If the first and the last are the same, there is no difference in tension. In the case of a patient sensitive to gold but not to copper, there is but little difference in electric tension between the skin and the copper disc, and this is not increased when the two are separated by a disc of gold. But if the copper is again covered with a second disc of gold, so as to make the order—skin, gold, copper, gold—we have the full difference in tension between skin and gold, and also the physiological effect. In the

* *Vide* Maragliano and Seppilli, loc. cit. R. Vigouroux, *Gaz. des Hop.*, May 21, 1878, and *Progrès Méd.*, No. 35, et seq., 1878. Charcot in Societé de Biologie.

†*La France Méd.*, No. 71, 1878.

case of feeble galvanic currents, two things are to be considered, viz.: the passage of the currrent and the tension of the electrodes due to their polarization. Vigouroux claims that only the latter circumstance is of influence, since by using *unpolarizable* electrodes, he could obtain no metalloscopic effects from currents perfectly active with ordinary electrodes. On the other hand he has seen the usual results produced by unipolar action, *i. e.*, by the application of only one metallic electrode, when of course no current could pass. As further proof of his views, he cites the effect of platinum, which ordinarily is inactive, but which obtains metalloscopic properties by polarization, after having been included in a feeble galvanic circuit. In short, Dr. Vigouroux claims that the essential condition of metalloscopic phenomena is a variation, of varying degree and duration according to the subject, of the electric tension of some point of the organism. (*Gaz. des Hop.*, Aug. 24, 1878.)

As far as this explanation goes, it seems in accordance with the facts. There is still much left to be explained, but attempts must be made in the laboratory, and not at the writing-desk.

While the above statements would tax our credulity severely if not supported by men of their reputation, the claims which M. Burq makes for internal metallotherapy are even more marvelous. That metal which previous metalloscopic attempts have shown to possess an influence when applied externally, is given to the patient internally in ordinary form and dose, with the result, Burq claims, of a perfect cure of hysterical symptoms and diathesis. As a test for the complete cure he re-applies the metallic disc; if no return anæsthesia follows the disease is eradicated. Charcot, while occupying a perfectly neutral stand-point, gives Burq the credit of having ameliorated the condition of four cases of hystero-epilepsy, all of which he had known for years as unmanageable by any treatment. Burq assumed the charge in July, 1877, and in October one of the cases influenced by copper had been completely cured by its internal administration; the other three, all treated with gold, had been free for some time of all hysterical manifestations, and had gained immensely in general

health; they were, however, still subject to the return anæsthesia on external application of gold coins. Another case of long-standing hysteria completely cured by Burq by the internal use of silver, according to metalloscopic indications, is reported by V. Revillout (*Gaz. des Hop.*, July 27, 1878): several others are now being published.

Results like these, vouched for by responsible parties, render further investigation a duty. All attempts of explanation, however, are at present but speculative.

Reviews and Bibliographical Notices.

I.—ERB: SPINAL CORD.

KRANKHEITEN DES RUECKENMARKS. Von Wilhelm Erb, Prof. an der Universitaet Heidelberg. Zweite Haelfte, Band II. *Ziemssen's Handbuch d. Spec. Path. u. Therapie.* (Diseases of the Spinal Cord.) Pp. 382-404. Leipzig, 1876, 1877.

This volume, or rather these volumes, for the diseases of the spinal cord are published in three parts of which the above are two, has appeared in its English dress while we have been preparing this notice.

The only work with which this can be compared is Leyden's, and the plan of the two is very similar. Leyden's is, however, much more of a clinical work, wherein numerous cases are reported illustrating the various affections. The present work is more didactic, only a very few cases being reported. The clinical treatise will naturally appear more life-like, and will be the more attractive to many readers.

Erb has made a book with fewer pages and less upon a page than Leyden. Therefore it has been necessary to condense more and there has been less space for discussion. It would be interesting to trace the differences between these two works, which are really the only exhaustive treatises upon the diseases of the spinal cord in any language, but it will be sufficient to say that the anatomy of the cord is given less fully by Erb; the physiology is considered under such different points of view that the two are rather supplementary to each other. The general symptomatology is quite fully given by both. Erb is fuller in the part devoted to considering the general etiology, diagnosis and treatment. The diseases of the vertebræ, which are placed together in a separate chapter by Leyden, are considered under other heads by Erb, chiefly as the causes of compression of the spinal cord. Sclerosis, acute and chronic myelitis are more briefly treated by Erb; who has disposed very summarily of many disputed points, has stated concisely that which it is most important to know, and has not pretended to know more than has yet been discovered. It is really refreshing to read an author who acknowledges to limitations in both knowledge and curative skill, who evidently knows when he don't know. There is very little to criticize adversely; and one reason for this is that Erb limits himself so closely to a statement of what is

known or most generally admitted, indulging little or not at all in weaving theories or developing hypotheses.

A very clear and intelligible account is given of the minute anatomy of the spinal cord, both of the nerve structures and the interstitial tissue. He accepts the view of Gerlach, confirmed by Boll, that the fine net-work of nerve fibres unites in the grey substance with the finest divisions of protoplasm processes of the multipolar ganglion cells, and this fine net-work of nerve fibres is not only spread through the grey substance, but can be followed through the septa of the white substance even to the grey cortical layer. This we think is not yet fully proved, and are unwilling to accept until more light has been thrown upon the nature of the very fine fibres which help to form these septa.

A summary is given of the more important facts so far as the anatomy of the cord has as yet been settled; first, those which are tolerably certain.

That all or at least by far the greater number of the fibres of the nerve roots pass directly into the grey substance: this is true for the anterior roots at any rate, for a small part of the posterior roots perhaps not. That a large proportion of these radical fibres are united with the ganglion cells or their immediate processes; this likewise seems quite certain for the anterior roots, but is still doubtful for the posterior.

That in the antero-lateral columns, and especially in the lateral columns, numerous fibres leave the grey substance, turn upwards and run in the white columns toward the brain. The nerve fibres which leave the anterior grey pillars pass in part through the white commissure into the anterior column of the opposite side, and run in this probably to the brain; but they pass in part laterally into the lateral columns and run in this to the medulla oblongata, where they cross in the decussation of the pyramids. That also numerous fibres leave the posterior cornua, enter the posterior columns, turn upwards and pass towards the brain.

That the ganglion cells of the grey substance are united with each other in the most manifold way by numerous processes, not only in the posterior and anterior cornua of the same side, but also by means of the commissures with those of the other lateral half; that further, from these ganglion cells, processes pass directly into the nerve roots, and also into the white columns, and there take a vertical direction.

That the fibres which enter with the posterior roots, in part pass horizontally directly forwards, to be lost in the fine net-work of nerve fibres, or to reach the ganglion cells of the anterior cornua, but in part they run by the side of the posterior cornua upwards or downwards, afterwards to bend again and enter the grey substance. That by means of the fine net-work of nerve fibres, and the bundles of fibres springing therefrom, there is formed a most complete connection of the individual groups of nerve cells with each other, with the entering nerve

roots, with the white columns of both halves of the cord in transverse and vertical directions.

As more or less probably, but at present not yet certainly established, may be mentioned:

That after the radical fibres enter a certain ganglion cell, processes from this very cell pass directly into the white substance, (out of the anterior cornua into the antero-lateral columns, out of the posterior cornua into the posterior columns and the posterior part of the lateral columns), and in this run directly to the brain.

That separate fibres of the anterior and the posterior roots meet in certain cells within the grey substance.

That a few bundles from the anterior roots only pass through the grey substance directly to the anterior part of the lateral columns, and there bend upwards. Their significance is still uncertain.

That the fibres of the posterior roots first enter the fine net-work of nerve fibres of the posterior grey cornua, and are connected by means of this with the ganglion cells themselves.

That every individual ganglion cell by means of its branching processes passes into a fine net-work of nervous fibres, from which are then formed again medullated fibres.

That the net-work of nerve fibres in which the posterior radical fibres are lost is in continuous communication with the net-work of nerve fibres in the anterior cornua; furthermore, that from this net-work arise numerous fibres which cross the median line in the grey commissure to ascend towards the brain, partly in the vertical fibres of the posterior cornua, partly in the posterior columns. Hence the grey substance seems to be much more intimately connected with the tracts of the posterior radical roots (by means of the fine net-work of nerve fibres) than with the tracts of the anterior radical fibres.

That from Clarke's columns bundles of fibres pass outwards into the lateral columns.

That the medial parts of the posterior columns have by their development and structure a special significance, which, however, is still obscure. Pierret considers them to be great longitudinal commissures which unite different parts of the grey substance.

Physiologically, the conduction of touch, pressure, temperature, tickling and such impressions, Erb thinks, is by means of the posterior white columns, the conduction of the sense of pain by the grey substance. The anterior columns and the greater part of the lateral columns, have nothing to do with the conduction of sensation.

The isolated conduction of individual motor impulses, like that of the sensory excitations, is explained by the supposition that of the many channels which are open some offer less resistance, and therefore are usually selected. But even in this region many translations to other tracts (associated movements) occur, either because the normal paths are not sufficiently used, or be-

cause the resistance offered by other paths is diminished, or because the irritation is increased in force.

The centres of co-ordination are in the brain only; the corpora quadrigemina, optic thalami and cerebellum seem to take the most prominent part in the co-ordination of movement. There seem to be no such centres in the spinal cord, which contains only those paths of conduction that convey the co-ordinative impulses to the muscles. The location of these paths is at present unknown.

Erb says that the vaso-motor nerves for the most part are to be found in the lateral columns, they leave the spinal cord by the anterior roots. Leyden says they are in the anterior columns.

The phenomenon of tendon reflex is briefly but clearly described, and it is of interest both because this is a newly-discovered phenomenon and was discovered by Erb. The tendon of the quadriceps, and the ligamentum patellæ, the tendo Achillis, and the triceps tendon in the upper arm, are the best points for demonstrating these reflex actions, as yet only observed in man. They are caused only by mechanical irritation (light tapping with the finger or percussion hammer), are very easily distinguished from the cutaneous reflex actions, and are strictly confined to the muscles and groups of muscles belonging to these tendons. Similar reflex actions can be originated, at least in cases of pathological increase of excitability, from the periosteum of many bones, from fasciæ and articular ligaments. From the peculiar nature of this phenomenon and from experiments made by F. Schultze and P. Fuerbringer, he concludes that these muscular phenomena cannot be due to a direct action through the tendon, but depend upon a reflex operation, the mechanism for which, in the case of the lower extremity, is situated in the lower segments of the spinal cord; and lastly, that a reflex act originating in the skin cannot possibly be the cause.

The application of physiological data to the explanation of pathological phenomena is summed up in twenty-seven propositions. It would require altogether too much space to quote all that is of interest in the volume, and we must pass on.

Under general symptomatology there is an interesting discussion in regard to delay of sensations, which, so far as is yet known, is most frequently or exclusively noticed with respect to painful sensations, and seems to be dependent upon a narrowing of the grey substance.

In regard to paralysis he says the nature and distribution of the palsy are not very characteristic by themselves, but their combinations with other symptoms give many points which assist in an accurate localization. Paralysis rapidly followed by a marked degree of atrophy, and by the reaction characteristic of degeneration, points to disease of the anterior roots (rarely), or of the grey anterior cornua (more frequently). In this case all reflex actions are absent. Paralysis with tension and contracture of muscles, without atrophy, is very probably due to an

affection of the lateral columns. Paralysis without loss of reflex function and without atrophy points to an affection of the paths which ascend to the brain, outside of the grey substance, or at least outside of the ganglia of the anterior cornua. Such are mostly cases of circumscript disturbances of conduction, the end of the cord below the lesion remaining intact.

Paralysis with trophic disturbances gives room for suspecting an affection of the grey substance, since primary affections of the roots are very rare.

Very extensive palsy with much atrophy, the reaction of degeneration, absence of reflex acts, points to a widely diffused lesion of the anterior grey substance.

Paralysis in the districts of certain pairs of roots (*e. g.*, in those of the upper extremities alone, or both crural nerves, etc.), points to a strictly localized affection of roots or lesion of the grey anterior cornua.

The conclusions which are formed regarding the *nature* of the lesion in the cord, are far less certain than those relating to its *place*.

Ataxia is defined as not a real paralysis, but the disturbance of movement produced by defective co-ordination of the latter; and it may be produced by abnormal extension of the motor innervation to too many or too few muscles, so that in some cases more, in others fewer than are normally required, are put in use. By abnormal strength of the innervation sent to each muscle in the case of a complicated movement. Erb states that he has examined two cases of ataxia without any disturbance of sensation; touch, temperature, pressure, pain, sense of tickling, muscular feeling, feeling of position of the limbs, of passive movements, etc., were all intact. Due notice is taken of the fact that inability to walk or stand firmly with the eyes shut is not a symptom belonging to ataxia alone, but may be found also where there is marked disturbance of sensibility of the lower limbs; indeed, if there is no such disturbance in ataxy, tottering, with the eyes shut, may be absent. The whole discussion in regard to ataxia is well worth reading.

Tetanus he says is probably caused by an (inflammatory or toxic) affection of the grey substance, which enormously increases the reflex excitability. The same result would follow a diminution of the power of resistance to irritations, as has been suggested by Sydney Ringer and Wm. Murrell.

In speaking of trophic changes, and the wasting of muscular tissue in infantile paralysis, and progressive muscular atrophy, he says that observers are not agreed as to whether the change of the ganglion cells is primary, or whether it is the consequence of an interstitial myelitis. This is an example of the author's caution in not deciding disputed points too dogmatically. Possibly he has not weighed the evidence in these cases carefully enough to be convinced himself, and so he honestly states both sides without expressing an opinion. While this may seem like

a weakness to some, and while some would be inclined to express an opinion, one way or the other, sure that notable names could be quoted on either side, we think the course pursued shows the author's real ability, and that he is a safer guide to follow when he does express himself clearly and decidedly in favor of a theory.

The whole subject of degeneration of muscles and nerves as a consequence of lesion of the spinal cord, is ably considered ; but as several pages are devoted to this we cannot follow him closely and will only give his conclusion, that a paralysis, or destruction of the central trophic apparatus, or a separation from the peripheral parts, produces the symptoms of degenerative atrophy. Upon the whole, we are justified by the present state of our knowledge in assuming a disease of the anterior cornua, when the electrical examination shows the existence of the reaction of degeneration, and consequently of degenerative atrophy of nerves and muscles, provided the disease is clearly of spinal origin.

Erb takes a different view of the cause of bed sores (decubitus) from that held by many authorities. He considers that in disease of the spinal cord they are due to destruction and paralysis of certain parts, rather than to an irritation ; that they arise from the paralysis of certain trophic centres in the spinal cord, or the separation of these centres from the periphery ; it is only in certain acute cases that we cannot as yet entirely reject the presence of an irritation as cause.

Changes in the renal secretion are very briefly mentioned ; the general opinion is mentioned that turbid and alkaline urine follow as secondary results of spinal lesion ; thus Rosenstein is mentioned as holding this view ; Charcot is referred to as holding the view that the spinal disease may be the cause of acute inflammation of the kidney. Erb seems to agree with Charcot, yet does not make it perfectly evident that he does ; but in the next subdivision he is more decided that the change is due to spinal lesion in many acute cases.

In considering the etiology of spinal disease, Erb does not agree with Leyden and many others, in the view that sexual indulgence and excess is of comparatively little etiological importance ; but he thinks that for many persons—not for all—the natural as well as the unnatural satisfaction of the sexual appetite, if carried to excess, forms an important disposing cause of disease of the cord, and enumerates as symptoms caused thereby, weakness of the legs, inability to stand long, tremor on making violent effort, pain in the back, shooting pains in the legs, loss of sleep, etc.

Masturbation may also cause serious spinal disease when long practiced. We are glad to see, however, that he also makes the statement that there is no reason to consider it more dangerous than normal coitus ; it is only when the practice becomes immoderate that there is danger of producing spinal disease.

"All this refers to the male sex. As to the female sex, very little is known as to these relations." Among women masturbation certainly has an injurious effect upon other portions of the nervous system, and so has excessive coitus, the brain and the general nervous strength suffer, but as Erb says, "I do not know that it causes a special disposition to spinal disease."

Under Diagnosis, the author makes the following very sensible remark, which may be kept constantly in mind with profit as teaching that skill in diagnosis can be acquired only by painstaking study of numerous cases, both clinically and pathologically. "It follows, therefore, that for a correct and sure diagnosis of a spinal disease, there must be not merely a very careful and exhaustive examination, not merely an exact collection and consideration of the etiological and other historical data; but also a comprehensive knowledge of the entire spinal pathology and a good share of personal practical experience."

Under "General Therapeutics" those remedies are considered which are most commonly used in spinal diseases. This method saves repetition in the subsequent sections, and is also of value as giving a comprehensive survey of the whole field, and laying a foundation for rational practice.

External applications are first noticed, including cold, warmth, baths, climate cures, electricity, blood-letting, derivatives, external frictions. Some of these are less understood in their application than they ought to be; it may be well, therefore, to dwell somewhat at length upon this section.

Both cold and warmth act in a reflex way through the cutaneous nerves; cold at first depresses, then reaction sets in; for the full effect of cold its application should continue over considerable time, not for only a few minutes. This point is not sufficiently dwelt upon. Warmth causes dilatation of blood-vessels and an increased flow of blood; tissue change is more rapid during its application; but over-stimulation and exhaustion may follow a too prolonged use of this agent.

Baths have not been systematically used here in America to such an extent as abroad, and so many ignorant persons have opened water cures that this method of treatment has not the repute it deserves. The direction for the use of baths is short but very satisfactory. Warm baths and cold water treatment are most fully considered; brine baths, chalybeate, mud and sea baths are more shortly noticed. In sending patients to the various baths, the temperature of the bath, the elevation above the sea level, the admixture of salts irritating to the skin, are all to be taken into account in coming to a decision. After mentioning a few principles which ought to help as guides, Erb remarks: "The *indications* which depend upon these effects cannot be easily decided. It is necessary to keep constantly in mind that there is present in spinal diseases an irritable weakness and exhaustion of the nervous system, and other organs than the

cord may be suffering in nutrition; otherwise the baths may result in injury to the patient."

It is especially in degenerative and sclerotic forms of chronic spinal disease that the greatest care seems necessary.

With cold baths a soothing effect is produced where the same portion of water remains in contact with the skin; an exciting effect when the body of water is frequently changed. Badly nourished, weak, irritable and anæmic persons, or those with degenerative disease of important organs, do not bear cold water treatment well.

Cold water strengthens the functions, improves the nutrition, increases the circulation of blood in the skin; it thus alters the distribution of the blood, and the process of circulation in the system; it relieves, at first temporarily, afterwards permanently, hyperæmia of internal parts; it quiets or excites the nervous system in various degrees; it tones the nervous system by the functional excitement and by the improved nutrition; it accelerates the process of change of tissue, and increases the total nutrition; it promotes resorption and formation.

If we add to this the effects which may be had from certain forms of baths, the increased sweating, the consequences of the increased consumption of water, the muscular movements which are necessarily increased, the effects of diet, climate, altitude in the case of cold-water cures, it becomes evident that we possess but few remedies which have so powerful and various an influence upon the nervous system.

Under climate cures it is said, that Beneke has shown that there is increased metamorphosis of tissue at the sea shore as compared with mountain regions.

A knowledge of the use of electricity would naturally be sought in special treatises on that branch of therapeutics, yet in eight or nine pages much information is given, but the hints are only hints, and do not furnish a complete guide to the use of electricity. The most valuable parts of this section are cautions as to how to apply the poles and the length of time and the frequency of the applications. We cannot, however, agree with the statement that "the principal object, in most cases, is to produce *as general and as powerful a passage of the current through the spinal cord* as is possible, especially in its diseased portions." (The italics are his.) A powerful current is too often injurious, and can only occasionally be used with safety.

He also says: "It is often well, after an electric treatment of several weeks, to interpose a considerable period of rest." This may be so if powerful currents are used, but some of the best results, essentially cures, we have obtained from long continued treatment without intermissions with mild currents.

"Manual skill, great technical experience, attention to a great variety of points, careful attention to the individual symptoms are absolute requisites."

He does not mention the method advocated by Brown-Séquard

of drawing rapidly over the back a *white* hot iron, or rather platinum bulb; a method of counter-irritation which has produced good results in the hands of many practitioners.

As to internal remedies he says, "We are still quite ignorant in regard to most of the remedies we employ, *how* they act upon the cord and its nutrition."

It is unnecessary to further specify the individual drugs; there seems to be nothing original in this section, except a praiseworthy acknowledgment of how little we know, which is refreshing as compared with the assumptions and assertions of some authors.

The second part treats of special diseases, beginning with hyperæmia of the membranes, of which little is known. "The anatomical evidence bearing upon hyperæmia in the spinal canal is very uncertain and ambiguous as possible." As to meningeal apoplexy, we think idiopathic cases are very rare, such at least as to give rise to symptoms sufficiently definite to lead to a diagnosis. Are the remedies advised, "energetic application of ice to the vertebral column, repeated powerful purges, and full local blood-letting (on the spine or anus)" really desirable? In the vast majority of cases the hemorrhage will have ceased before the physician arrives, the powerful purges, by causing effort and straining, would be likely to increase the blood pressure, and so start the bleeding again; the effort, the excitement of local blood-letting, would have the same tendency; the application of ice would have less of such effect, but it must be kept constantly applied; and if Chapman's views are correct, and he is quoted approvingly in a previous section, ice externally would lead to fullness of the internal vessels. It would seem that large doses of ergotin (fluid extract of ergot or its infusion might cause vomiting), and absolute rest, with opium if there is pain, is the best method of treatment.

In the introduction to diseases of the spinal cord itself, he says that he does not pretend to a strictly scientific division; his grouping of affections is, however, excellent and easily understood and remembered.

After enumerating the motor, sensory and other symptoms caused by spinal anæmia, he states: "We are not entitled to infer the definite existence of spinal anæmia from the above symptoms, unless the causes are clear." It seems to us extremely difficult to distinguish anæmia from spinal nervous weakness, which is subsequently described, or from a low sub-acute myelitis; thrombosis and embolism of small arteries may lead to softening and have the appropriate symptoms.

Spinal apoplexy is said to be as rare as meningeal hemorrhage. We think it is more rare; but minute hemorrhages into the substance of the cord, probably occurring during the agony, giving rise to no diagnostic symptoms, are not rare in inflammatory affections of the cord; but these do not constitute spinal apoplexy.

From the sixth division, concussion of the spinal cord, it might

be inferred that actions for damages following railway accidents are not common in Germany, if indeed they are allowable. As to the nature of such lesions he says that they are probably due to molecular disturbance; such anatomical changes as may be present in individual cases are accidental, and not essential adjuncts. We are inclined to believe his prognosis is rather too favorable.

It is not many years since German authors were unwilling to acknowledge that spinal irritation is a distinct pathological entity, claiming that it is merely one phase of hysteria. Erb, however, states that it is not to be confounded with the latter nervous condition, and gives it a place in his classification; yet he does not include in it all cases of tender spine, as so many authors are inclined to do.

In regard to the nature of spinal irritation, after giving the conflicting opinions of Ollivier, Hammond, Beard and Rockwell, Hirsch, he simply states that it is useless to try to decide between them, as the conclusion could only be that we know nothing definitely at present. This confession of ignorance is again refreshing after so much positivism to which the profession has been treated.

Section eight, functional weakness of the spinal cord—spinal nervous weakness—neurasthenia spinalis, is peculiar to this author, and is defined as a diseased condition in which marked and unquestionable disturbances of the functions of the cord exist, for which no considerable anatomical basis can be found or assumed; a disease, therefore, which must at present be classed among the functional disorders. It is most common among men, is caused by excessive mental efforts, sexual excesses, bodily over-exertion. Weakness and inability for prolonged exertion, dysæsthesia, paræsthesia, cold hands and feet, sleeplessness and general malaise are the prominent symptoms—a condition from which in this country quacks receive a large income, being able to work upon the fears of patients.

"We do not know at all what may be the nature of the processes of nutrition which cause the symptoms. We, however, believe that we are certainly entitled to locate them in the cord, especially the lower portion, the lumbar region. The most obvious view is that which supposes that the physiological fatigue of the nervous elements, which always occurs after severe and protracted irritation, becomes exaggerated and assumes a fixed form; in such a case we may suppose that the fatigue of the nervous elements does not become repaired in the prompt manner which is usual under physiological conditions."

Treatment consists in attention to regimen and diet, change to mountainous regions, electricity, iron and quinine.

The last sentences under treatment in myelitis need to be followed with some caution as to holding out false hopes to the patient, but contain a sensible caution to the physician not to discourage a patient too hastily.

"The courage of the patient must be maintained, his confidence in the various methods of treatment strengthened, and his hopes constantly aroused and reawakened. Unfortunately the physician must in only too many cases rest contented when he can succeed in this task, which is itself by no means a light one, and brighten the miserable existence of the patients by an occasional glimmer of fresh hope." It has been our experience that most of these patients are as conscious of their hopeless state as the physician, and before it is necessary to give an unfavorable prognosis are prepared by their own sensations of weakness and by the advancing disease to hear an unfavorable opinion. Also in many cases they are not especially despondent.

In section thirteen is described paralysis spinalis spastica—tabes dorsal spasmodique (sclerosis of the lateral columns?)—primary lateral sclerosis?—This is a division in spinal nomenclature only recently made. As in many other cases, a few scattered observations were recorded, the earliest in 1856, by Tuerck, the next by Charcot nine years later; a few others were recorded by Charcot and his pupils, until in 1875 Erb claims he first gave a full and detailed description of the disease. Since then there have been more frequent reports of cases.

There is motor paraplegia, generally developing itself slowly and gradually advancing upwards, to which symptoms of motor irritation early ally themselves—muscular twitchings, muscular tension, rigidity, and contractures, which lend to the disease-picture a peculiarly fertile character. To this is added a very striking increase of reflex activity of the tendons, much more rarely, also, of the reflex action of the skin, while there is an entire absence, at least for a long time, of any serious disturbances of sensibility, of the vesical or sexual functions, and of nutrition, and cephalic symptoms likewise never arise. Sometimes the disease begins in one leg and extends to the corresponding arm, at other times, more rarely, it begins in the arms. The account given is very full, and the whole series of symptoms is described twice, once briefly, again in more detail; being a newly described disease, this is perhaps excusable, but the first description is not brief enough for a general summary, and there seems to be an unnecessary repetition of the same ideas in nearly the same words with only a very little additional. The exaggerated tendon reflex, which may be so great that irritation of the ligamentum patella, or of the tendo Achillis, gives rise to clonic spasms (spinal epilepsy), is one of the most characteristic symptoms; this leads to a peculiar gait, which is described, but which no words can adequately describe. The absence of many symptoms is characteristic—no disturbance of sensibility, no vesical and sexual weakness, no muscular atrophy, no bed sores, no disturbance of brain and cranial nerve.

The "Theory of the disease" is a very careful and logical summing up of the reasons for considering that there is lesion of the lateral columns; this is a very satisfactory division, and the

author shows more conclusively than he is willing to admit that such is the lesion. Very few after such an array of proof would conclude. "But, unfortunately, we thus far lack conclusive reports of post-mortems. Until we have such, this assumption remains only a very probable one; and, in view of the exceedingly vacillating character which still attaches to our physiological and pathological experiences concerning the spinal cord, we shall do well to await the definitive confirmation of this view before exchanging our clinical designation for an anatomical one." Since the above was written such a confirmation has been given, so far as one case is sufficient, by Stoffella in the *Wiener med. Presse*, No. 18, 1878.

Those cases accompanied with atrophy of the muscles, whether in children or adults, whether acute or chronic, are very properly described under poliomyelitis anterior acuta and chronica; thus the pathology of these cases is indicated in the name, and uncertain division or classification dependent on symptoms is avoided.

But if every section, myelitis, sclerosis, bulbar paralysis, anomalies, etc., were noticed separately, this review would extend far beyond reasonable limits. Erb is cautious, sometimes too cautious, in expressing an opinion; it is, however, better to be over-cautious than over-bold. Any statement made has substantial foundations on which to rest. The few points on which we differ from him are comparatively unimportant. This part of Ziemssen's great work we can most heartily commend to any who wish to study the latest and most satisfactory treatise on diseases of the spinal cord.

S. G. WEBBER.

II.—EULENBERG: DISEASES OF THE NERVOUS SYSTEM.

LEHRBUCH DER NERVENKRANKHEITEN, von Dr. Albert Eulenberg. Zweite umgearbeitete und erweiterte Auflage. 1. Erster (allgemeiner) Theil. Pp. 377. 2. Zweiter (specieller) Theil. Pp. 718. Berlin, 1878.

The first edition of this important work appeared in 1871 (*Lehrbuch der Functionellen Nervenkrankheiten auf physiologischer Basis bearbeitet, etc.*), in a much less complete form than at present. Then it professedly related only to the *peripheral nerves.* But now, this portion of the general subject, as treated by the author, has been rewritten, and in the main brought up to the level of existing knowledge, not alone, but the plan of the work has been enlarged so as to include the more important affections of the central nervous system. As it now appears fresh from the hands of its accomplished author, we know of no manual on nervous diseases which comes nearer our ideal of what such a work should be. As may have been already noticed, the work consists of two parts, a *general* and a *special*, the latter by far the largest. In the following notice the attention of the reader is to be called exclusively to the first part, leaving the other to be noticed perhaps in a future number of the JOURNAL.

In the *general* part we find, as might have been expected, a discussion of those general facts or principles, anatomical, physiological and pathological, which underlie the entire field of neurological medicine. The discussion has been conducted by one who seems well fitted for it, both by nature and by his acquirements, with rare good sense and practical tact. Dr. Eulenberg joins to a wide and accurate knowledge of the experimental and practical phases of his difficult theme, as well as its literature, a sound, discriminating judgment. His analysis of his subject is judicious, his thought clear, and his style simple. Such qualities have enabled him to produce an exceedingly readable and instructive book. But without farther delay we pass to a short survey of the general principles of nerve pathology, in the light in which they have been placed by our author.

Disorders of the nervous system are divided by Dr. Eulenberg into *Neuroses of sensibility*, *Neuroses of motility*, *Neuroses of nutrition* and *Psycho-neuroses*, the first three alone being treated in his work. The first volume, as already intimated, is devoted to general consideration, in which morbid phenomena are brought face to face with our existing knowledge of the anatomy and physiology of the nervous system.

Neuroses of sensibility (*Æsthesioneuroses*) are first treated of

in their more elementary or fundamental forms. In order to explain morbid it is necessary to understand healthy phenomena. Hence Dr. Eulenberg enters on a brief but rather penetrating discussion as to the intimate nervous structures and processes, which are known, or supposed, to be involved in nervous sensibility when it is roused by appropriate stimuli. He supposes, as many others have done, that the sensory impulses, which are caused in many ways to pass along the sensory nerve fibres, are conveyed into certain nerve cells in which the fibres finally terminate, and that an agitation of the particles which compose the cells occurs and in some way this gives rise within the cell to sensation, and we presume, for that matter, even to those closely allied but higher nervous and mental processes which pass under the names of perception, emotion, etc. Dr. Eulenberg adopts the gratuitous supposition so often maintained by others, that the *vividness* or strength of a sensation — that is, of a response to a sense impression — depends on the *resistance* which the cell opposes to the molecular motion which the shock of the sense impression that arrives by the way of a fibre tends to excite. He appears to adopt the law of Bernstein, which may be stated as follows: "The *quantity* of a sensation may be estimated by the number of ganglion cells required to neutralize or arrest the molecular disturbance caused by a given sense impression." If the resistance is greater in the cells, the reaction to the sense impression is sharper and sensation is more acute. If it passes into a greater number of cells, it is because the resistance in them is less, and hence the sensation experienced is less acute. Farther, the strength of a conscious sensation is determined by two factors: the intensity of action of the irritant, and the quantity or degree of resistance met with in the sensory cells. Fechner (*Psychophysik*) is quoted under this head, where on the above indicated assumptions he seeks to bring sensation under the dominion of the calculus, and as a much paraded result, announces a barren formula which utterly misses the soul of sensibility and sensation in confusing *quantitative* with *qualitative* relations. We have no hesitation in declaring for our own part, that Fechner's laws of sensation do not in anything touch the heart of the subject to which they relate, valuable as they may be, in a hypothetical way, in the exterior nervous processes involved in sensation. It is a well-meant but in the end fruitless endeavor to carry mathematical processes into a sphere where nearly every essential element in a series of equations is hypothetical, or even fanciful. What we object to in this case and all such cases is not the endeavor to penetrate as far as possible by the industrious and judicious use of such methods of study, but rather to the unwarranted and misleading confidence placed in the *results*. To suppose, as seems to be done by some, that they enable us to explain the *real nature* of sensation, betrays a primary misconception as to what sensation really involves.

We have been rather sorry to see so judicious a thinker and writer, as is Dr. Eulenberg, laying so much stress on these practically almost valueless speculations. But we must decline entering any farther into a discussion, which could prove of interest to but few of our readers, and instead pass to more accessible and useful topics.

There is considerable merit in the classification of neuroses of sensibility given by Dr. Eulenberg. Increased sensibility passes, as usual, under the general term *hyperæsthesia*, and complete loss under that of *anæsthesia*. But a mere diminution in sensibility is called *hypæsthesia*. Pain is placed among "dysæsthesias," and is described as a case in which the *subjective* element in sensation rises above its normal level of intensity. *Paræsthesias* include that group of peculiar sensations such as pruritis, formication, morbid hunger, or morbid thirst, globus, etc. In the final analysis of these examples of morbid sensation, they are to be subordinated to hyperæsthesias or to hypæsthesias, but deserve to stand in a separate class for clinical reasons.

In the special discussion of hyperæsthesias, they are divided into three classes: *peripheral*, *conducting* (intercurrent) and *central*. These forms of hyperæsthesia are described throughout as being distinct. We wish to direct the attention of the reader to this subject for a few moments.

The *nerve cells*, of the nervous centres, or certain among them, it seems probable, are the sole seats of true sensibility, at least those of its forms which are manifested in consciousness. The nerve fibres which lead to them, and in connection therewith the peripheral apparatuses of sense in which the fibres terminate outwardly, are *not* the seats of sensibility proper. They are simply for the reception and conduction of sense impressions (not *sensations* as is so often loosely said) to the central sensory cells, where they may be appreciated, or known, or lower down, functionally speaking, minister to nervous reflexes, apart from consciousness and will. A pacinian corpuscle, or a nerve fibre has no such capacity, in the sense that nerve cells may have. No fact is better known in the physiology of the nervous system. But if this be so, how comes it to pass that we may have a *peripheral*, or a *conduction* hyperæsthesia? What is hyperæsthesia? It is in a few words sensibility exalted above the normal level of acuteness. Are conduction and sensibility convertible terms? Most certainly not. And yet conduction, and not sensibility, may be said to be the proper function of nerve fibres. Even if it be granted that by disease, a nerve fibre may be so changed as to *magnify* as compared with a normal fibre, the sense impressions it transmits to the seats of sensibility, could such a condition of things be called *hyperæsthesia*? It might be called *hyperconduction*, but it could not in any sort of strictness be called by the former name. At most that could be said there would be only an *apparent* hyperæs-

thesia, the fact being that the conductibility of the fibre is in such state as to magnify the intensity of the original sense impression as it passes along toward the centre, so that an exaggerated impression may be delivered at the true seat of sensibility. This we say, would be an apparent, but not a real hyperæsthesia. But what good proof have we that any such state as this ever occurs? We have no hesitation in declaring there is none. There is no such thing in the proper sense of the words as a "peripheral" or a "conduction" hyperæsthesia, for the parts of which these things are predicated have no such function in health as sensibility in the sense now supposed, and hence cannot have it in disease. It is true that localized disease of the peripheral sense apparatuses, or of the conducting tracts may lead to hyperæsthesias, but only because the related sensory cells are secondarily disturbed in their nutrition, or, to use a common but uncertain expression, in their molecular state, in such manner as to increase their sensitiveness. But there is no reason, so far as we can see, for thinking there would have been a true hyperæsthesia, if the condition of the seat of sensibility, in other words the centre, had remained in *statu quo*. In this brief discussion we have laid aside those cases in which through anæmia, or in a general way there is lowered nutrition, and, as a consequence, abnormal acuteness of general sensibility; for in this case, there can be no reasonable doubt but that the whole nervous system is involved in the change, especially the centres.

The divisions made of hyperæsthesias by Dr. Eulenberg, is, in reality, made chiefly from a causal point of view. There can be no doubt but that disease or injury of the peripheral sense apparatuses, or nerve trunks, or conducting tracts still more centrally situated, may lead to hyperæsthesias. The sole question in our minds is as to the real seat and inner mechanism of the same. We do not deny altogether the correctness of the views of Dr. Eulenberg, but do contend that enough stress is not laid on the condition of the central apparatus, which is the sole seat of true sensibility, the opinion of Mr. G. H. Lewes and others, to the contrary notwithstanding. Happily, however, such matters have a dialectical rather than a practical interest. But we would not despise the greatest attainable accuracy of apprehension and statement of a case.

In the first place, disorders of sensibility are discussed with unusual fullness and precision. In discussing the various methods of testing the space sense of the skin, he makes no reference to the rather elaborate researches of Leudet, in the domain of æsthesiometry. The following law is formulated from the researches of various observers working under the direction of Vierordt: *Parts are more sensitive, according as they are more movable.* Extended and valuable discussions as to the methods of determining the state of the senses of pressure, temperature, pain, etc., are given, which we commend to the attention of the reader.

A discussion also of the methods employed for determining the rapidity of conduction of sensory and motor impressions along the nerves, is had, in regard to its diagnostic relations. Particular reference is made to the valuable work of Burckhardt, (*Die physiolog. Diagnostik d. Nervenkrankheiten*, &c. Von S. Burckhardt, Leipzig, 1875, p. 284.) But this subject has been already examined at considerable length in a review of Burckhardt's work in a former number of the JOURNAL (vol. II., p. 566).

Under the head of hyperæsthesia of the sense of touch (Hyperpselaphesia) Dr. Eulenberg quotes apparently with approval, the opinion of Brown-Sequard, that in cases where in applying the two points of the æsthesiometer *three* points are felt instead of two, there is disease of the base of the brain—(basal system), more particularly of the crura cerebri, and pons. But we would rather account for such a phenomenon by supposing it to be an illusion on the part of the patient than to adopt the hypothesis of Dr. Brown-Sequard, who assumes the formation of new cells, which like pre-existing cells stand in connection with pre-existing fibres, so that there are new and uncustomary sense centres added to those which naturally exist. This ingenious hypothesis is purely gratuitous, however, though not unreasonable.

In discussing the nature of cutaneous paralgias, such as pruritus, Dr. Eulenberg would seem in most instances to consider them to depend on disease of the nerve terminations in the papillary eminences of the true skin. But we must think, that in this, as in other sensory neuroses, there is a possibility that many, if not the majority of such cases, may depend on central disorder, the outward symptom being merely a projection phenomenon. But in this and similar instances, we refer the reader to our remarks above, on our author's classification of hyperæsthesias. He does not overlook or deny that such symptoms may have a central origin, but fails to lay the stress on this point which it seems to us to demand.

Formication (Ameisenkriechen), subjective feelings of heat and cold, etc., are acknowledged to have a central origin.

Cutaneous neuralgias are discussed at some length, in the course of which the more common criteria of neuralgic pain are laid down. They are said to be: 1. That the pain shall be *spontaneous*, and provoked by morbid processes within the organism itself. 2. That it shall arise with unusual suddenness and intensity and be confined to the course and sphere of distribution of particular sensory nerves. 3. That it shall not be continuous, but appear in paroxysms, with intervals of partial or complete freedom from pain. These are the common criteria of a neuralgia.

The *radiation* or apparent propagation of pain along the course of a nerve, is referred to an affection of the *nervi-nervorum*, rather than the nerve trunk itself, and also, after Bærwinkel, to a neuritis. Upon the whole Dr. Eulenberg discusses rather acutely the intimate physics of pain, but arrives at no valuable conclusions.

No special stress is laid on the "painful points" of Valleix, nor on the "points apophysiaires" of Trousseau.

In the discussion of the general pathogenesis and etiology of neuralgias, they are referred in the last analysis to "changes in the molecular mechanism of the sensible nerve masses," but it is admitted that as to the nature of these changes, we are completely in the dark. A large number of neuralgias are said to be the outcome in all probability, of a "constitutional anomaly, which appears to consist in unusually abnormal functioning of the whole nervous system, or more particularly separate parts of the same."

Dr. Eulenberg appears to admit the existence of the "constitutional neuropathy," contended for by Griesinger, which is prolific not of neuralgias alone, but of hysteria, epilepsy, various convulsive and paralytic affections, insanity, etc. He also would admit the existence of heredity in the production of neuralgia, which was so much insisted on by Anstie, and also another fact as it seems to be dwelt on by this latter author, that there is a kind of commutation, or rotation of diseases, not only in the same person, but in different persons, in the line of hereditary descent. One nervous disease appears often to be substituted for another, provided such diseases do not involve serious or destructive local lesions. But we are unable to see any real difference between the "constitutional neuropathy" of Griesinger, and the "hereditary disposition" of Anstie. But it is of more importance to recognize the justness and importance of the views of these authors than many will be apt to suppose. Hence we have given some space to a restatement of views which have long since ceased to be novel, and yet which have not attracted the attention they deserve. The clear admission of such views would have no small influence, to say the least, on prognosis and prophylaxis of neuralgias, and related affections. Among the predisposing causes of neuralgias, according to Dr. Eulenberg, are abnormalities in arterial tonus. We are inclined to lay more stress on this point than the author seems to do himself. But we have discussed this factor in the mechanism of neuralgias, already at length in the pages of the JOURNAL. He gives also an abstract of the views of Uspensky ("Versuch Einer Pathologie der Neuralgien." *Deutsches Archiv f. Clin. Med.*, xviii., Heft. I., 1876) on the pathology of neuralgia with which we can entirely agree. Uspensky seems to adopt almost the same notions on this subject which we have set forth at length, "Pathology and Treatment of Neuralgia," in this journal (vol. IV., p. 207) on a former occasion. According to Dr. Eulenberg certain cases of neuralgias may depend on a *neuritis migrans*, such as has been described by Feinberg and others as experimentally produced in the lower animals. But while such a causation of neuralgia is possible, it is not probable, especially when Feinberg's results are now disputed by recent observers. It is not

often one can find so full and suggestive a list of the causes of neuralgia. But we pass it by for the present.

Dr. Eulenberg enters upon a thorough discussion of the fallacies which are being daily committed in the diagnosis of neuralgias. Many affections are called neuralgic which have no real title to that name, and *vice versa*. The whole chapter on the diagnosis of neuralgias needs to be carefully read to be appreciated. No mere abstract would do it justice.

The same may be said of the section on Prognosis. It is almost a model discussion within moderate limits.

"The Therapeutics of Neuralgias," says the author, "has upon the whole the same difficulties to struggle with as the diagnosis and prognosis. We have before us a complex of symptoms, but in many cases we cannot at all, or only with difficulty, recognize the nature, anatomical seat and cause of the disease. Moreover, in many cases where the cause of the neuralgia is known, we are incapable of removing it. Finally, even after the removal of the cause, the resulting affection may remain as an independent disease, possibly through secondary alterations of the cell elements of the central organs, and we are limited to a purely symptomatic treatment, which, though it may lead to valuable palliative results, rarely accomplishes a permanent cure" (p. 61). But it will not be possible in this notice to follow Dr. Eulenberg in his judicious discussion of the general therapeutics of neuralgias. He adds nothing new to the means for combating them, but presents us with a well elaborated and penetrating review of existing knowledge on this difficult, and painfully practical theme.

He prefers, take it altogether, the faradic to the galvanic current, in the treatment of cutaneous neuralgias. Nussbaum's method of nerve-stretching, for the relief of obstinate neuralgias, is favorably mentioned. In his rather extended remarks on the conditions of anæsthesias and hypæsthesias, is a very suggestive discussion as to the trophic action of the nervous system, especially in relation to the skin. But this subject is examined by the author at greater length in his chapters on "Trophoneuroses," which it is our intention to notice in a subsequent number of the JOURNAL.

III.—BATEMAN: DARWINISM TESTED BY LANGUAGE.

DARWINISM TESTED BY LANGUAGE. By Frederic Bateman, M. D. etc. London, 1877.

(*Conclusion.*)*

In order to establish the fact that the possession of articulate language constitutes a difference between man and the lower animals, not only of degree but of kind, Dr. Bateman enunciates three propositions:

"I. That articulate speech is a *distinctive* attribute of man, and that the ape and lower animals do not possess a trace of it.

"II. That articulate speech is a *universal* attribute of man—that all races have a language, or the capacity of acquiring it.

"III. The immateriality of the faculty of speech."

Taking up these propositions *seriatim*, we proceed to inquire, in the first place, whether or not it is quite certain that the lower animals are not possessed of the faculty of expressing their thoughts and emotions by articulate speech.

We must, in the beginning, divest ourselves of the idea that, because we are incapable of appreciating the various modulations and inflections of the sounds uttered by the lower animals, therefore such modulations and inflections do not exist. Those sounds have been studied to a very slight extent, and we all know how difficult, if not impossible, it is for us, if unacquainted with a language, to detect differences in the articulations uttered. All the words seem to run into each other, to form an incomprehensible lot of gibberish. It is only after long study and familiarity that we are enabled to pick out the words, and to give them their linguistic value.

That the lower animals are able to communicate ideas to each other by sounds emitted from the larynx, appears to be very certainly established. We see instances every day among the animals that man has domesticated. The cock calling his hens, the hen informing her brood that she has found something good to eat, the recognition of his companion in the sounds uttered by the horse, are all instances in point.

Leroy,† than whom few have investigated the mental faculties of the lower animals with more thoroughness, thus speaks of the subject of their utterance of articulate sounds:

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*See this journal, for April, 1878.

† The Intelligence and Perfectibility of Animals, from a Philosophic Point of View. London, 1870, p. 72. Charles Georges Leroy, born in 1723, succeeded his father as Ranger of Versailles and Marly. In this post he enjoyed ample opportunities for the observation of animals. His letters on the subject were written over the signature of "The Naturalist of Nuremburg."

"Do they speak or not? This question must be resolved by the solution of two others. Have they the necessary conditions of speech? Could they, without it, do what they do? The only requirements of language are connected ideas and the faculty of articulation. We have proved, and that without the shadow of a doubt in the preceding letters, that animals feel, compare, judge, reflect, etc. They have, then, in respect of connected ideas, all that is required for speech. As for the faculty of articulation, most of them have nothing in their organization which, so far as we can judge, forbids it. Birds, in other ways so widely differing from ourselves, succeed in forming articulate sounds precisely the same as ours. Animals have, then, all the necessary conditions of language. But if we closely investigate their actions in detail, we perceive further, that we cannot, for an instant, deny them the power of communicating some, at least, of their ideas to one another; and this by the aid of words. We are positive that they never fail to distinguish between the cry of terror and that which expresses love. Their different agitations have different intonations which characterize them. If the mother, alarmed for the safety of her family, had but one cry to warn it of its varying dangers, the family would always be sure to do the same thing when uttering the cry. But, on the contrary, their movements vary according to circumstances. Sometimes a hasty flight is the consequence of the cry of alarm; at others, concealment; and at another time they advance to the combat. Since, then, the actions which follow the mother's command are different, it is impossible to conceive that her language is uniform. Can we assert that the expressions of a male and female during the period of their intercourse are not very various, when we can clearly perceive in them a thousand movements all differing one from another—eagerness more or less pronounced on the part of the male; reserve, mingled with allurement, on the part of the female; pretended denials, rage, jealousy, quarrels and reconciliations? Can we imagine that the sounds accompanying these emotions do not correspond with the situations they express? It is true that the language of signs is in great use among animals, and that it suffices for the communication of most of their emotions. This language, familiar to all who feel more than they think, makes an immediate impression, and its communication of feeling is almost instantaneous; but it is not enough to express all those combined actions of animals which suppose concert, agreement, designation of place, etc. Two wolves, who, to facilitate their chase, have each taken a different part—that of the one being to attack the victim, while the other awaits it at some place agreed upon, to run it down with fresh strength—cannot have acted thus admirably in concert without some mutual communication, and this communication is inconceivable unless they have a spoken language.

"The education of animals is mainly conducted by means of the language of action. Example shows them most of the move-

ments necessary to the preservation of their natural life. But in cases where the objects of foresight and of fear increase with the increase of danger, this language becomes inadequate; instruction becomes more complicated, and words are necessary to convey it. Without spoken language the education of a fox could not be completed. It is a notorious fact that, long before they have had any chance of learning, by personal experience, young foxes leaving the den for the first time are more suspicious and watchful in places where they are much hunted, than are the old ones in more peaceful neighborhoods. This truth, which is incontestable, demonstrates, once for all, their need of language; for how, in its absence, could they acquire this science of precautions, which supposes in them a number of facts, and the power of comparing and of appreciating them? It seems, then, absurd to doubt that animals have a language, by means of which they transmit to one another those ideas the communication of which is necessary."

Houzeau* does not hesitate to ascribe, at least to birds, the faculty of forming articulate sounds for the purpose of conveying ideas, using for this purpose powers corresponding to our vowels and consonants, and hence enabling us to pronounce and write them. The cries of the whip-poor-will and the cuckoo are familiar to all, but there are many other notable instances to be met with, especially in tropical regions. Waterton speaks of a night-bird in Guiana, whose cry was distinctly, "Who are you? Who, who are you?" And of another that, in a tone of command and of anger, clearly said, "Work away! work away! work away!" And of a third that, in sorrowful accents, uttered the words, "Willy, come go! Willy, Willy, Willy, come go!"

Of course it will be understood that it is not alleged that the birds were uttering the words cited with any knowledge that they belonged to the English language, or were associated with any such ideas as they represent to our minds. The instances are only given as illustrations of the ability of these animals, even when not taught, as are the parrot and the raven, to enunciate articulate sounds which can be represented by our letters.

Attempts have been made, and with success, to represent in written words the sounds made by the lower animals, which have no similarity to any human language. Bechstein† has done this with the nightingale, and has ascertained that this bird, in the course of an hour or two, enunciates over forty distinct sounds, which are representable by letters of our alphabet. Can we be bold enough to declare that these are meaningless, and that they are not understood by other nightingales? These forty primitive sounds are enough to form a language as complicated as our own, and infinitely more so than that of the de-

* Études sur les facultes mentales des animaux comparées a celles de l'homme. Mons, 1872, t. II., p. 328.

† Naturgeschichte der Hof und Stubenvogel. Art. "Nachtigall."

graded human beings whose vocabulary is so imperfect that, in the dark, when deprived of the assistance of gestures, they cannot comprehend each other.

In attempting to represent the entire song of the nightingale, Bechstein writes it as follows, the letters having the powers given to them in the German language:

"Tiou, tiou, tiou, tiou—Spe, tiou, squa—Tio, tio, tio, tio, tio, tio, tio, tio, tix—Coutio, coutio, coutio, coutio—Squo, squo, squo, squo—Tzu, tzu, tzu, tzu, tzu, tzu, tzu, tzu, tzu, tzi—Corror, tiore, squa, pipiqui—Zozozozozozozozozozozozozozozozirhading—Tsisisisi, tsisisisisisis—Dzorre, dzorre, dzorre, dzorre hi—Tzater, tzater, tzater, tzater, tzater, tzater, tzater, tzater, dzi—Dlo, dlo, dlo, dlo, dlo, dlo, dlo, dlo, dlo, dlo—Quio, trrrrrrrrrr itz—Lu, lu, lu, lu, lu, ly, ly, ly, ly, lie, lie, lie, lie—Quio, dide, li, lulylie—Hagur, gurr, quipie, coui, coui, coui, qui, qui, qui, qui, qui, qui, qui—Goll, goll, goll, goll, guia, hadadoi—Conigui, horr, ha, diadia, dill si!—Hezezezezezezezezezezezezezezezeze, cowar ho dze hoi—Quia, quia, quia, quia, quia, quia, quia, ti—Ki, ki, ki, ki, io, io, io, ioioioio, ki—Lu, li, li, le, lia, la, len, lo, dide, io, quia—Kigaigaigaigaigaigaigaigai, guiagaigaigai conior, dzio, dzio, dzio pi."

In his remarks on the subject of the vocalization of which birds are capable Houzeau says:

"The language of birds contains a notable part of the letters of the alphabet, and even the double consonants are employed, such as *dz* and *ll*. There are also simple vowels and diphthongs. The greater part of the consonants used are combined with vowels as initial letters of syllables. The use of consonants after vowels, as final letters is much more rare, the letters *l*, *n*, and *r* being the only ones which figure in this situation. But from the lack of various letters, and of combinations or syllables we are not to conclude that there is a total absence of articulate sounds. We, ourselves, do not make use of all possible combinations of final consonants after a vowel. Many nations do not even use all the letters, and cannot even pronounce those which do not enter into the composition of their alphabets. Several African languages do not contain *r*. The Chinese not only do not have this rolling consonant, but they lack also other simple consonants, as *b*, *d*, *v*, *z*. They pronounce Holland *Golanki*, and France *Fulanteus*. The Polynesians do not have the sibilant *s*. They do not place consonants after vowels in the same syllable, so that all their words end in vowels. The Hurons have none of the labials (*b*, *f*, *m*, *n*, *p*, *v*,), and the vowel *u*, which is articulated by the lips, is also wanting. Garcilasso says that the Peruvians not only do not have the letters *b*, *d*, *f*, *g*, *s*, *x*, but that they do not even form a single double consonant. The Feejeans do not possess the *c*, the Somo Somos the *k*, or the inhabitants of Rakiraki the *t*. The Australians have no *s*. The Indians of French Port, in British Columbia, lack the letters *b*, *d*, *f*, *j*, *p*, *v*, *x*. But the poorest of all people in articulate sounds are those of New Zealand, whose alphabet does not contain the twelve fol-

lowing letters: *b*, *c*, *d*, *f*, *g*, *j*, *l*, *q*, *s*, *v*, *y*, *z*, and the double letter *x*."

In view of these facts, and of many others of fully as striking a character, which could readily be cited from other parts of the animal kingdom, it is certainly going too far to deny to all the beings lower in the scale of creation than man, the faculty of articulate speech. We are forced, therefore, to regard Dr. Bateman's first proposition as unproven, and, hence, as of no weight in the determination of the matter at issue. This is the least that can be said, the probability being, that so far as we can judge from an attentive study of the habits, and the actions of, and the sounds emitted by many of the lower animals, they are possessed of an articulate language by which they are enabled to communicate with others of their kind.

But we may go further and enter what in law would be called a demurrer. Suppose his proposition to be true, and that man is really the only animal possessing the faculty to articulate speech; what then? Would it then be a difference in kind such as Darwin demanded? We cannot see that it would. Articulate language is only a refined and exalted way of expressing ideas, and when we are by accident or disease deprived of the power of speech we have recourse to other methods for making ourselves understood, which by constant practice answer all our purposes. The signs, and gestures, and facial expressions of the ape, dog and other animals serve for all their purposes of communication with man, and are fully as unerring in character as any that human beings employ. They use a language which is in direct accordance with the ideas they wish to express, and the mere fact that it is not articulated by the tongue and lips is a non-essential point in the discussion.

But it may be asked, why does not the ape speak our language? For all we know to the contrary the chattering which these animals keep up when in their native forests, at the approach of man, may consist, to some extent, of like questions as to his inability to converse with them in their tongue—still we may very properly answer the question by replying that no patient and persistent attempt, so far as we know, has ever been made to teach him. Besides, it would be well, it appears to us, in case any such course of instruction is contemplated, not to begin with so difficult a language as English—one which even educated Chinese can never learn with tolerable thoroughness—but with some of the languages lower in the scale, such as those spoken by the human beings living in close proximity with this advanced animal. We are very strongly of the opinion that if such an attempt were made, not on a single ape, but on a group—a class it might be called—and carried out with some such degree of persistence as is employed with very imbecile children, an astonishing success would attend the effort.

And in this connection we may call attention to the fact quite recently demonstrated by Dr. Spitzka, since the first part of this

review was published, that the brain of the chimpanzee has an island of Reil not devoid of convolutions as asserted by Huxley and others, and hence not differing in kind from the same organ in man. As Dr. Spitzka's observations have not yet been published, the following synopsis of them which he has been kind enough to send us will doubtless prove of interest:

"In a young chimpanzee about two years old this region [the island of Reil] is as completely covered by the enveloping operculum and temporal lobes as in man; it is equally with the human, triangular in shape, and the acute apex of the triangle is directed backwards as in man. It had four distinct gyri, and the most posterior of these showed bifurcation at its upper end. In a large chimpanzee whose convolutions were much more intricate than those of the younger animal there were five such gyri, an inferior frontal convolution was present and well developed.

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I should add that proportionally to the size of the entire hemisphere, the island of Reil is as large as in man."

The ape, therefore, in all probability, does not speak our language first, because he has never been taught to do so, and, second, because the island of Reil and brain generally are not sufficiently developed to enable him to initiate through his own unaided efforts the various intellections and functionations necessary to the articulation of human speech. In this last respect he does not differ essentially from those children in whom from arrest of cerebral development the faculty of articulate language is never acquired. We doubt very much whether children of any nation, if allowed to grow up without association with other persons speaking a language, would in a single generation be any better off as regards articulate speech than many of the lower animals.

Dr. Bateman quotes a very interesting autograph letter to him from Prof. Vogt, as follows:

"The brain of man and that of apes, especially of the anthropoid apes (orang, chimpanzee, gorilla) are constructed absolutely upon the same type—a type by itself, and which is characterized amongst other things, by the fissure of Sylvius and by the manner in which the island of Reil is formed and covered. Thus in man, the third frontal convolution is extraordinarily developed and covers partly the inside, whilst the transverse central convolutions are of much less importance. In the ape, on the other hand, the third frontal convolution is but slightly developed, whilst the central transverse convolutions are very large.

"To show the bearing which all this has upon the seat of speech I would refer to the microcephali who do not speak; they learn to repeat certain words like parrots, but they have no articulate language. Now the microcephali have the same conformation of the third frontal convolution as apes. They are apes as far as the anterior portion of their brain is concerned. Thus man speaks; apes and microcephali do not speak. Certain observations have been recorded which seem to place language

in the part which is developed in man and contracted in the microcephali and in the ape. Comparative anatomy, therefore, comes in aid of M. Broca's doctrine."

The microcephali do not, as Prof. Vogt asserts, or as Dr. Bateman admits, speak, but they are none the less men, nor are apes any the less microcephali because they do not talk.

The second proposition of Dr. Bateman has no special bearing upon the subject under discussion. We may admit with him that there is no people, nation or tribe that does not possess some form of articulate language, imperfect as some of them are.

As regards the third proposition relative to the immateriality of the faculty of speech, we must confess to our inability to comprehend exactly what Dr. Bateman means. He says he is tempted to ask, "whether speech, like the soul, may not be an attribute, an immaterial *nescio quid*, the comprehension of which is beyond the limits of our finite minds?" To which we might answer by another question, "Of what is the soul an attribute?" and, by another, that "If the matter is beyond comprehension, why attempt to discuss it?"

But Dr. Bateman, under this proposition, adduces certain examples and arguments which, as he thinks, militate against the doctrine of the localization of the faculty of speech in the part of the brain assigned to it by Broca—the posterior part of the left third frontal convolution. In this he is, we think, creating a theory for the purpose of demolishing it, for very few, if any cerebral physiologists now contend for the limited localization of Broca. To this point we have already in the first part of this review invited the attention of the reader. We have there asserted that there is not a single instance on record of the "speech tract" being the seat of disease without there being at the same time impairment or loss of the faculty of articulate speech. It will however, probably not be out of place, to refer more at length to some of the cases cited by Dr. Bateman, and to point out the fallacy of his argument from them.

He says (page 181): "I have shown, and that upon the most indisputable authority, that persons could talk when the presumed seat of speech was invaded by an enormous tumor, completely disorganized by disease or destroyed by a pistol-shot."

The case of the tumor is the one reported by Velpeau, and to which all writers on aphasia make reference. "In the month of March, 1843, a barber, sixty years of age, came under M. Velpeau's care for disease of the prostate gland. With the exception of his prostatic disorder, he seemed to be in excellent health, was very lively, cheerful, full of repartee, and evidently in possession of all his faculties; one remarkable symptom in his case was his intolerable *loquacity; a greater chatterer* never existed, and on more than one occasion complaints were made by the other patients of this talkative neighbor who allowed them rest neither night nor day. A few days after admission this man died suddenly, and a careful autopsy was made. On opening the

cranium, a cancerous tumor was found which had taken the place of the two anterior lobes." Now in regard to this case we have observed in another place,* "very little faith seems to have been put by physiologists or pathologists in the history of this case. If it proves anything, it is that the anterior lobes are useless appendages to the rest of the cerebral system." And we find that Kussmaul† says of it: "In order to draw out Bouilland, Velpeau demanded the prize of 500 francs for a case in which a lobulated cancer had professedly destroyed the two anterior lobes; in truth, however, it had left untouched the greater part of them and especially two-thirds of the left frontal lobe."

Even admitting all that was claimed for this case by Velpeau, it does not in the least weigh against the theory of the localization of the faculty of speech in those parts of the brain to which we have already ascribed its situation—those which are nourished by the middle cerebral artery.

The case in which, according to Dr. Bateman, the presumed seat of speech "was completely disorganized by disease," is the one reported by M. Peter. In this instance the skull of a man was fractured by a fall from a horse; after recovery there succeeded "a *remarkable loquacity*, although after death it was found that the two frontal lobes of the brain were reduced to a pulp." Now in this case the man was in a state of furious delirium during the whole period of his life (thirty-six hours) after the reception of his injury, and in addition to the disorganization of the anterior lobes there was a fracture of the roof and base of the skull throughout its entire extent. The case simply militates against Bouilland's theory, and not against the one now held by the majority of neurologists; and Trousseau claims no more for it than as proving that the two frontal lobes may be destroyed in their anterior portion without causing a loss of the faculty of speech.

In the case in which the "*presumed* seat of speech" was destroyed by a pistol-shot, we find that two officers fought a duel; one of them fired first and the ball entered his adversary's head at one temple, passed through the brain and then raised the temporal bone on the opposite side. The ball was extracted, and the patient immediately made a sign with his hands and expressed his thanks in a very low voice. He recovered for the time being, and during five months thereafter could speak perfectly well, and was remarkable for the wit and fluency of his conversation and writing. He subsequently died of softening, and it was found on post-mortem examination that the ball had passed through the two frontal lobes in their middle portion.

This case is open to all the objections alleged against the other

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*A Treatise on Diseases of the Nervous System. Sixth edition, New York, 1876, p. 173.

†Op. Cit. (American translation.)

two, and so also is the one reported by M. Moreau (de Tours) in which the third frontal convolution was congenitally absent.

Now, Dr. Bateman admits that if M. Broca's theory could be shown to be correct, Darwin's views would receive confirmation. Of course this admission is equally good for any other localization of the faculty of speech. We have seen that there is a "speech tract," that this region embraces the posterior part of the anterior lobe, the anterior part of the middle lobe, and the lobe of the insula, or island of Reil—those parts, in fact, which are nourished by the middle cerebral artery. It exists on both sides of the brain, though more developed on the left than on the right side; and we repeat that there is no instance on record in which injury or disease involved the whole of this region on both sides which was unaccompanied by derangement of the faculty of articulate language. In very many cases comparatively slight lesions of this region on either side, especially the left, have been sufficient to cause such derangement.

In view of these facts—and they are but a small part of what could be brought forward—we are forced to the conclusion that Dr. Bateman has failed altogether to make out his case. Darwinism, as he calls it, may or may not be true, but it is not to be overturned by such assaults as his. Nevertheless he has written an interesting essay, and one which will, doubtless, add to his reputation as a perspicuous and thorough thinker; and we commend his little book both to evolutionists and anti-evolutionists. The former will not fail to see how weak are the arguments of their adversaries, the latter will probably get a few grains of comfort; and, at any rate, will be made acquainted with some of the scientific aspects of a most important subject.

W. A. H.

IV.—TUKE: INSANITY AND ITS PREVENTION.

INSANITY IN ANCIENT AND MODERN LIFE, WITH CHAPTERS ON ITS PREVENTION. By Daniel Hack Tuke, M. D. London: MacMillan & Co. 1878. 226 pages.

This very neat and readable volume is a popular rather than strictly a medical work, and part, at least, of its contents has appeared previously as articles in an English literary magazine. To the medical man, and to the alienist more especially, its main point of interest is to be found in the second of the three parts into which it is divided: that on "Insanity in Relation to Modern Life." The first section, on the prevalence of the causes of insanity in ancient times has also some interest to the physician, apart from its value as a discussion of a merely medico-historical subject, since it involves a comparison between the past and the present periods in regard to the questions of the etiology of insanity, and a general review of what the author considers the chief causes of insanity in all periods. The third division of the work, on self prevention, or, as the author calls it, anti-prophylaxis of insanity, is addressed more especially to the laity; the facts and precepts it contains are, or should be, known to the physician already. There are some points here also, however, that will claim our attention in this notice.

The causes of insanity which Dr. Tuke considers as the principal, comprehending more or less all minor ones, are stated in the first chapter, broadly, as follows: (1) Intoxication of all kinds, including under this head also all the hereditary effects of intoxication; (2) Mal-nutrition and poverty; (3) Causes chiefly moral and emotional, such as excitement, disappointment, etc.; and (4) and last, intellectual strain, which for the most part acts indirectly, and that on only a small portion of the population. According to this very general classification, the author reviews the probable prevalence of the causes of insanity in pre-historic and ancient times, in four interesting chapters, which, however, we can, for the most part, only slightly notice. The speculation, for it is largely only speculation, in regard to the prevalence of insanity in pre-historic times, brings up one collateral point in regard to which some remarks seem to be called for. As the pre-historic man was, in all probability, very similar to the existing savages in many parts of the world, the condition of these latter, as regards mental disease, may be taken as a fair index of his condition in this respect, and this method of estimating his liability is adopted by Dr. Tuke, who assumes, we think, incorrectly, that the modern savage is less liable, or as we understand him, more resistant to the influences producing insanity than his civilized contemporary. He says that civilization favors insanity, that an increased liability is "the penalty which

superior organisms have to pay for their greater sensitiveness and susceptibility." It would be more correct, it appears to us, to say that cases of insanity are more numerous among civilized people on account of the greater mental strain which civilization requires, the ability to sustain this strain, though in itself considerable, not always keeping pace with its increase. And even this may not be always correct; the statistics of insanity among savages are wanting, and we are inclined to think that mental disease is perhaps much more frequent among them than is generally supposed. Then we are of the opinion that the excitements of civilized life would tell far more severely upon the barbarian than upon those who habitually undergo them. And it seems to us also that in throwing out a certain class of moral and emotional influences causative of insanity, as of little or no effect among savages, he writes too positively—causes that we have evidence to believe are effective in producing mental derangement even among the lower animals ought not to be so summarily excluded. When parrots and dogs sometimes die from disappointed affection it appears almost unwarrantable to assume that such events may not happen even in the lowest condition of humanity. Indeed, with the exception of such as depend upon certain social conditions peculiar to civilization, and certain kinds of intellectual overstrain, the causes of insanity, we hold, are the same in savage as in civilized people. That they are less prevalent in the simple, and on the whole monotonous routine of the savage is true enough. The point we wish to make is that his mental health is as much or more likely to be disturbed by these causes when they occur as is that of his civilized brother. As to the frequency of insanity among uncivilized peoples, it is very difficult to obtain correct statistics, and very easy to make false estimates. Only the more pronounced forms of mental aberration would generally be recognized; the mild chronic maniac might very probably be amongst them, at least in many cases, a shaman or medicine man, and if his delusions took the direction of exaltation they would be very likely to be accepted by all or many of his tribal associates. If the insanity was of the acute and depressing kind, or if it seriously disabled him in the constant struggle for existence, he would very quickly be disposed of; there would be no Christian sentiment or charitable institution to keep him a burden on his fellows. We make these remarks, because, from personal knowledge of some savage or barbarian tribes, we have been led to think that mild chronic cases of insanity are not so rare among them as has been supposed to be the case.

According to the grade of civilization would naturally be the frequency of many of the causes of insanity and the mental habituation, or rather endurance, of them. Therefore, in ancient, though historic, periods we should look for just so much difference from the present in this respect, as might be expected from the different conditions of mental and social culture and development of the times. These are discussed quite fully in

the following chapters of the first part of the book. We need not dwell on them here, but will pass at once to the second part, where the subject of insanity in modern times is taken up.

The fact shown here, by a careful analysis of statistics in the first chapter of this section, that the great majority of the insane in Great Britain are of the uneducated class, indicates, to our mind, very plainly that it is the surroundings, and not any special mentally weakening influence of civilization that is the cause of the difference in the prevalence of insanity in barbarous and in Christian lands. The class that furnishes the greater number of insane is just the one that has least partaken of the advance in civilization, and whose mental condition is more nearly in the condition of that of the savage. Some of the facts and statistics of the causes of insanity here given would furnish good arguments for temperance reformers, all the more valuable as coming from so high an authority as Dr. Tuke.

The chapter on insanity in the higher classes dwells mainly on the evil effects of mental strain and overwork, touching only briefly on other causes, idleness and mental stagnation, moral and emotional disturbances, and religious excitement. As regards the latter, the author thinks that while, as a cause of mental disease, it has been overrated by some, others have unduly ignored it. It is a real cause, he says, and he lays great stress on the responsibility of those who through distorted statements of religion arouse the morbid tendency into active insanity. The guilt, whatever there is of it in these cases, of course depends upon the motive, but we are inclined to think, in spite of Dr. Tuke's apparent judgement, that the real danger is much more easily exaggerated than minified in these cases, and in our own limited observation the cases of insanity that might most readily be attributed to those causes have usually had a very different origin.

The last chapter in this section is a short one, but is valuable as affording a very careful and judicious discussion of statistics with special reference to the increase of insanity in modern times. His conclusion is that, taking the statistics of Great Britain, there has been in that country a great increase in the amount of recognized insanity, but that this can in great part be explained by the attention of the government and public having been directed to the care of the insane, to the decreased rate of mortality and the more accurate recognition of the condition. He is inclined, however, to admit that there may be an actual as well as an apparent increase. It is highly probable that his method of estimating the increase will be applicable also to the United States, but it cannot be followed with such exactitude here as in Great Britain, on account of the lack of accurate statistics for all the different States.

The third part, as we have already stated, is for the guidance of the non-professional reader, and does not contain much that the medical man ought not to know. Dr. Tuke's opinions on

some subjects, however, are worth mentioning. Thus, he speaks very decidedly, here as elsewhere, of the effects of alcoholics, and especially condemns an American physician, Dr. A. B. Arnold, of Baltimore, for saying, at the International Medical Congress, in Philadelphia, that "medical men allow themselves to be governed by the hue and cry of professional temperance orators, for it could not be proved that alcohol exerts much influence on the production of nervous diseases," a statement, the utterance of which is, we think, very properly characterized as an act of hardihood. We cannot, however, altogether agree with him when he takes American institutions especially to task as favoring the development of insanity, even though he quotes American authors as his authorities. We see no good reason for believing that there is any deterioration of the Caucasian race on this continent; according to our observation, though there are enough detrimental influences at work here, they are fully counterbalanced by those abroad. It is not merely patriotism that leads us to make this exception to what appears to be the author's drift, but our desire to see the real case fairly stated.

In conclusion, we can cordially recommend the volume to both the medical man and the public as an able and readable discussion of a very important subject.

SHORTER NOTICES.

I. Anatomy, Descriptive and Surgical. By Henry Gray, F. R. S. With five hundred and twenty-two engravings on wood. The drawings by H. V. Carter, M. D., and Dr. Westmacott. The Dissections jointly by the Author and Dr. Carter. With an Introduction on General Anatomy and Development. By T. Holmes, M. A., Cantab. A New American from the Eighth and Enlarged English Edition. To which is added Landmarks, Medical and Surgical. By Luther Holden, F. R. C. S. Philadelphia, Henry C. Lea, 1878. Chicago, Jansen, McClurg & Co.

II. Stricture of the Male Urethra. Its Radical Cure. By Fessenden N. Otis, M. D. New York, G. P. Putnam's Sons, 1878. Chicago, Jansen, McClurg & Co.

III. A Clinical History of the Medical and Surgical Diseases of Women. By Robert Barnes, M. D. London. Second American from the Second and Revised London Edition. With One Hundred and Eighty-one Illustrations. Philadelphia, Henry C. Lea, 1878. Chicago, Jansen, McClurg & Co.

IV. The Atlantic Islands as Resorts of Health and Pleasure. By S. G. W. Benjamin. Illustrated. New York, Harper & Brothers, 1878. Chicago, Jansen, McClurg & Co.

V. Annual Reports of the Supervising Surgeon-General of the Marine Hospital Service of the United States, for the Fiscal Years 1876 and 1877. (*John M. Woodworth, M. D.*) Washington, 1878. 213 pages.

VI. Atlas of Skin Diseases. By Louis A. Duhring, M. D. Parts III. and IV. Philadelphia, J. B. Lippincott & Co. 1878.

VII. Fowne's Manual of Chemistry, Theoretical and Practical. Revised and corrected by Henry Watts, B. A., F. R. S. A New American from the Twelfth English Edition. Edited by Robert Bridges, M. D. With One Hundred and Seventy-seven Illustrations. Philadelphia, Henry C. Lea, 1878. 1027 pages. Chicago, Jansen, McClurg & Co.

I. Gray's Anatomy has so long been the standard text-book in this country that it hardly needs any introduction to send off a new edition. This new American from the eighth English edition contains many changes from its predecessor. Besides the additions made by the English editor, which are most prominent in the portions which relate to microscopical anatomy and development, Dr. R. J. Dunglison, who superintended the bringing out of this volume, has added the whole of Holden's

Landmarks, which as a separate work was noticed in our April issue. This renders it, as a surgical anatomy, still more valuable than before, and is an addition that will be generally appreciated.

The omission most to be regretted, in this and former editions, is in the descriptive anatomy of the central nervous system, more especially in the brain. The student to whom terms now every day met with in neurological literature, such as the internal capsule, the lenticular nucleus, the claustrum, etc., etc., are new, will look in vain in this work for their definition. We trust that in future editions this will be looked to; these terms are fairly naturalized in our language, and represent facts in the cerebral topography, that a text-book like the present one is deplorably behind the times in ignoring. With this exception we can give the volume our best commendations as a text-book of descriptive anatomy.

II. This is purely a surgical work, and therefore, one that hardly comes within the scope of this journal. It is, however, an able treatise on a very important pathological condition, and written with the object of stating the eminent author's views in regard to the nature, the treatment and the prognosis of what has been considered to be in many cases an incurable affection. He seems to make out some very good evidence that he has conferred a real benefit upon an unfortunately large class of suffering humanity, by improving the methods and prospects of their relief. A large part of the work is taken up with a fair statement of opposing views, which are ably disputed by the author. The typography and general appearance of the volume are very good; it is a valuable addition to surgical literature.

III. The good words that have been spoken in the medical press of this country and Great Britain in regard to the earlier edition of Dr. Barnes' work may be recalled by our readers. This second revised edition, of course, deserves all the commendation given to its predecessor, with the additional one that it appears to include all, or nearly all, the additions to our knowledge of its subject that have been made since the appearance of the first edition. The American references are, for an English work, especially full and appreciative, and we can cordially recommend the volume to American readers.

IV. To those who are in search of pleasure and have to travel for it, and to a large class of persons to whom change of scene by a sea voyage is a desirable and available sanitary measure, this work will be a decided help. It gives in a pleasant style, and assisted by numerous attractive illustrations, accounts of a number of insular resorts for health or pleasure, and of some of which similar descriptions can hardly be found elsewhere. The descriptions include such widely separated localities as Cape Breton and the Isle of Wight, the Bahamas and Canaries. An

appendix at the close tells how to reach these islands, with also some account of their climatic and sanitary peculiarities. The book is elegantly gotten up.

V. Besides the usual annual report and statistical tables, this volume, like the preceding ones, contains several papers on medical subjects, contributed by officers of the Marine Hospital service. The first and longest of these is by Dr. Oscar Oldberg, chief clerk and medical purveyor of the service, on the adoption of the metric system of weights and measures for medical and pharmacal purposes, to which are appended comparative and posological tables which will be of service to those who do not at once apprehend the rules here given for the conversion of the English into the metric scale. Dr. Bailhache's article on the physical examination of seamen, calls attention to a very notable evil, but we doubt whether his recommendations will be easy to put in practice. The papers on yellow fever are *apropos* to the present time, but they give evidence of considerable differences in opinion between the medical officers who wrote them, as to the origin of the disease. Altogether, the report increases our already well-founded confidence in the scientific and professional ability of the present management of the Marine Hospital service.

VI. This work maintains fully the promise of its first number, and we can only repeat here our commendation given when noticing its first appearance. We know of no work of its class that excels it in appearance or general excellence. Each number contains four excellent lithographs of illustrative cases of skin disorders, with explanatory text—those in parts III. and IV. representing eczema (squamosum), syphiloderma (erythematosum), purpura (simplex), syphiloderma (papulosum and pustulosum), vitiligo, alopecia areata, tinea favosa, and eczema rubrum. As we said in our former notice, this work is a convenient clinic, always available to its possessor, and well worth the price to every general practitioner.

VII. This well known and permanently favorite text book appears again in a new and enlarged edition. We notice a change in the title, which, considering the size of the present edition, seems to us well made; the work is now a manual of chemistry, not of elementary chemistry as was formerly stated to be the case. As a complete and yet convenient manual for the medical student and practitioner, we know of none better. It will worthily retain the place it has so long held in the favor of the profession.

Editorial Department.

AS has been said elsewhere in the present issue of the JOURNAL, it is our purpose henceforth to give more attention than ever to the subject of Insanity. This would be naturally expected, of course, for it is a theme to which the JOURNAL is devoted *ex professo*. It is one of vast public importance, and seems to us to clearly invite discussion at present, and in lending our pages to it, we are unwilling to be misunderstood by any one, as far as our motives are concerned.

One of our chief aims is, and will be, to aid as far as we can, in doing away with the exclusive spirit which has so long pervaded the medical management of asylums for the insane in this country, and which finds its most pronounced expression in the plan of organization and policy of Association of Asylum Superintendents. It is only by the grace or courtesy of the association that any outside member of the profession, however capable or eminent, can meet the responsible medical officers of asylums for the insane, for purposes of discussion. The association cuts itself off almost by specific regulation from free contact with the profession at large. There is no objection to Superintendents of Hospitals for the Insane forming an association of their own as they have done, if any other organization or recognized mode of free intercourse existed between asylum chiefs and other non-official branches of the profession, which would satisfy the felt need of a more perfect intercourse, and of a better knowledge of the actual medical state and internal workings of these hospitals. But no other organization exists for this purpose, and Asylum Superintendents rarely attend State Medical Societies, and as a rule only in small numbers the National Medical Association, in the organization of which a subsection is

provided for medical Psychology, but has thus far proved almost barren of results.

Then again, in nearly all State and municipal asylums in this country, no provisions are made by law, or in fact, for adequate medical visitation and inspection, and the consequence is, that the profession at large is relegated chiefly to the annual "Reports" of their medical officers, as to the inner condition of the asylums. High-minded and capable as many of the superintendents are, and as much as the administration of the asylums in general may be above suspicion, it has come to pass that the present sources of knowledge as to their condition and management are felt to be inadequate, and it is the exclusive spirit still manifested in the presence of a reasonable demand for closer relations with the profession at large, which gives that demand no small portion of its vitality. Insane asylums are simply hospitals for the care and medical treatment of the sick, and the time has now come when very many capable members of the profession, who neither expect nor desire official relations to an asylum, nevertheless take a deep practical interest in the study and treatment of mental diseases, and naturally turn to our public asylums, and inquire as they have the right to do, how they are actually managed in the interests of science and humanity.

If hospitals for the insane were private instead of public institutions, in which the public feel a deep interest, pecuniary and sympathetic; if the inmates of these hospitals, from all ranks in society and from all parts of the country, were in condition mentally to recognize and secure their own rights, as in other hospitals; and if there were no opportunities for abuse of power, or neglect of duty, the case would be different from what it is. There are real difficulties in the case, and the only way out of them, so far as we can see, is for the Asylum Association to enlarge its scope, and show a more liberal and less exclusive spirit toward the outside profession, and to aid in exposing every phase of asylum management to a reasonable and thorough examination. This done, all cause for complaint or suspicion will speedily disappear, and good come, it may be hoped, to the important cause of the best management of the insane.

Attention has recently been called in an open letter by Dr. R. J. Patterson, a well-known asylum superintendent, to some of the evils resulting from the very peculiar laws of the State of Illinois regarding the commitment of the insane. Some years since, largely, we believe, at the instance of a rather celebrated female agitator, the legislature of this State was induced to enact that commitment to an asylum could only take place by a jury trial, thus it was thought, guarding the liberties of the citizen in this as well as in other emergencies by the most time-honored and effectual means. However well this plan may appear in theory, and even in practice, in rural-districts, it is shown by Dr. Patterson that it is open to serious objections. Indeed, some of these objections had been very well stated by a committee of a medical society in the city of Chicago which reported on the subject several years ago. Apart from the largely sentimental objection of its publicity, the compulsory jury trial for insanity is objectionable on account of the delay it often causes and the harsh treatment which the patients in many cases necessarily undergo while waiting their trial in ill-fitted county jails or any other improper places of detention. Besides this, a jury made up as it frequently is in large towns, and even containing one physician, is as little likely to give an intelligent or correct verdict in such cases as any equal number of non-expert and merely average intellects can be.

Cases are referred to in Dr. Patterson's letter in which insane persons have been subjected to two or three jury trials before conviction, and that in the face of the clearest evidence of their insanity. Moreover, the decision of the question whether the case shall be brought to trial appears to depend sometimes altogether on the opinion of the county physician, thus putting the whole responsibility upon one person, and that one generally a non-expert as regards insanity. We have in our own experience known of a case of clearly marked insanity kept from trial for months, during which time at least two attempts at suicide and one assault were reported.

The proposition offered by Dr. Patterson, that cases of insanity be examined by a commission of two physicians and one lawyer, appointed by any court of record, and the com-

mittal depend upon their sworn report, with the privilege of a jury trial when demanded by the patient or his friends, seems to us a good one, and the plan infinitely preferable to the awkward and expensive one now in vogue. What constitutional provisions stand in the way we do not know, but it will certainly be a shame if the evils of the present system, of which we have mentioned only a few, of the commitment of the insane in Illinois are allowed to long continue.

As will be seen by a glance at the cover, we have been able to add to the list of Associate Editors, the name of Dr. MEREDITH CLYMER, of New York city. Dr. Clymer has been so long and so favorably known to the profession both at home and abroad, as an author and teacher of rare ability and scholarship, as to make any farther words from ourselves out of place.

Periscope.

a.—ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

Functions of the Brain.—At the session of the Soc. de Biologie, June 28 (*Le Progrès Médical*, July 6), M. Tarchanoff presented a communication in regard to the development of the psycho-motor centres in various animals From researches on rabbits and dogs Soltmann has been led to the conclusion that these centres do not make their appearance until the tenth day, when the senses are developed; the action of external agents is therefore indispensable for the formation of these psycho-motor centres. Besides, in both an anatomical and a chemical point of view, the brain presents decided differences, according as we examine it in the new-born or the adult animal.

In such animals as from birth are in full exercise of their senses, and perfect in locomotion (Guinea pigs and hedgehogs), M. Tarchanoff found the brain much more developed, and the cranium more resistant; the moderating power over the reflex possessed by these psycho-motor centres appeared shortly after birth, while it was lacking in the rabbit. Further, in the Guinea pig the excitation of the pneumogastric arrested the movements of the heart, while it was ineffectual in the case of the rabbit.

Anatomically, the differences are also striking; thus, in the first, the circulation of the grey layers is more developed; and there are found there giant and pyramidal cells. The brain of the rabbit, less vascular, contains but few giant and no pyramidal cells; and, moreover, the nerve fibres are destitute generally of myeline. M. Tarchanoff explains these differences, less by the duration of gestation than by the intensity of nutrition, during intra-uterine existence. He found that by the administration of phosphorus to new-born animals, he could accelerate the development of the functions of the psycho-motor centres. The same result followed putting the animal head downward and thus producing a cerebral hyperæmia

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Internal Capsule.—In following the development of the medullary sheaths, P. Flechsig has succeeded in separating the systems composing the internal capsule. (*Bericht d. Muenchener Naturforsch. Vers.* 1877, p. 226.) In the especially adapted human fœtus, at full term, there exist three systems of medullated fibres, and several of unmedullated fibres. Of the former, one passes along the outer edge of the thalamus opticus (corona radiata of the same going to the parietal lobe); a second system is found along the internal edge of the nucleus lenticularis, so as to form the chord of the arch of the internal lamina medullaris. The fibres of this system come from the laminæ medullares of the lenticular body, pass along its internal section transversely through the internal capsule, and continue their course partly

above, partly underneath, and partly through the body of Luys into the tegmentum pedunculi. (Regio subthalamica. Forel.)

The ansa lenticularis passes with this bundle. A third system of medullated fibres, generally elliptical on cross section, is found in the posterior third of the internal capsule, occupying its full breadth. Downwards it is continuous with the third (external) quarter of the pes pedunculi, and finally the pyramids, while upwards it passes into the central convolutions. The systems of fibres not medullated, in the internal capsule of the fœtus, consist of a larger portion in front, and a smaller part behind the pyramidal tract. The former continue along the internal half, the latter along the external fourth of the pes pedunculi. Both sets can be traced downwards no further than the pons.

The Semicircular Canals.—The following are the conclusions of M. Cyon, from his own investigations, as announced to the Acad. des Sciences, Paris, Dec. 31, *Centralbl. f. d. med. Wissensch.*, No. 27, 1878.

1. They are the peripheral organs of the sense of space; the sensations produced by the irritations of the terminations of the nerves in the ampullæ of these canals, are destined to help us to form the idea of the three dimensions of space; the sensation experienced in each one of these corresponding to one of these three dimensions.

We form with their assistance an ideal space in which the perceptions of all the other senses are referred, in so far as concerns their relations to the surroundings, and the position of our own bodies. The empiristic theory of Helmholtz, in regard to binocular vision, and the nativistic theory of Hering, are by the presence of a special sense of space brought as it were upon a neutral basis.

The physiological excitation of the peripheral terminations of the special space-sense, is probably effected by the otoliths of the ampullæ; they can be put into vibration by active or passive motion of the head, or by currents of air.

The eighth pair of cranial nerves includes two altogether separate nerves: the auditory nerve and the nerve of the sense of space. The central organ of the space-sense determines the distribution and the degree of the innervation force for all the movements of the eyes, the head and the rest of the body. The disturbances from injury to the semicircular canals are to be referred (1,) to a visual vertigo, due to the difference between the ideal and the actually seen space; (2,) to the resulting false conceptions of our bodily position in space; and (3), to the disturbances in the distribution of innervation to the muscles.

Further details are promised on the subject.

The Motor and Sensory Cells.—At the session of the Acad. des Sciences, Paris, June 4 (reported in *L'Union Médicale*), M. Vulpian presented a communication from M. Pierret, Professor of the Faculty at Lyons, on the constant relations existing between the volumes of the motor and sensory cells of the nervous centres, and the length of tract over which incitations emanating from or returning to them have to traverse.

After the discoveries of Ch. Bell and Magendie, on the role of the anterior and posterior roots, and since we have come to recognize the ganglionary elements of the substance, it has been admitted by the majority of physiologists, that both motor and sensory cells exist in the nerve centres.

This division is sanctioned by the works of modern anatomo-pathologists.

We know, therefore, beyond question, that there exist in the myelencephalon motor and sensory ganglia, whence arise, or in which terminate, all the sensory and motor fibres. It seems logical to determine with care the morphological characters of the cells of these various ganglia, and thus to obtain information as to the true function of certain ganglionary groups, the anatomical relations of which are still undetermined.

This investigation, performed in an incomplete manner, and under the influence of preconceived ideas, has hitherto given only erroneous results. By comparative examinations, involving all the motor and sensory nuclei, I have obtained the following results:

These studies were made on the motor and on the sensory nervous systems.

A. *Motor Nervous System.*—(1.) The largest nerve cells are found in the lumbar region of the spinal cord, and in the fronto-parietal convolutions of the brain (paracentral lobe. Betz). These two points are in relation with each other, and the distance between them is considerable. Moreover, the largest nerves of the human body (sciatics) arise precisely from that point of the lumbar cord where we find the largest, so-called motor, cells.

In the dorsal region the anterior cells are one-half smaller than those of the preceding. The distance separating them from the brain diminishes and the nerves arising here are proportionately short.

(2.) In the cervical region the motor cells are larger than in the dorsal region, but smaller than those in the lumbar, this being related on the one hand to the length of the brachial nerves, and on the other to the lesser distance that separates them from the cerebral centres.

The same is the case with the nucleus of the hypoglossus, the cells of which are a little smaller than those of the anterior cornua of the cervical cord.

For analogous reasons the nerve cells of the superior motor ganglia diminish gradually in volume, as they are nearer to the brain, and as their connected peripheral nerves are shorter.

Thus, the external motor oculi nucleus contains larger cells than does that of the patheticus, or that of the motor oculi communis. The cells of the two latter are equal in size, but are very small, and have lost all those characters of elegance usually attributed to motor cells.

Finally, in the corpus striatum the cells are still smaller, and cannot be distinguished, as to form, from those of the thalamus, which are more voluminous, or of the geniculate bodies, or tubercula quadrigemina.

We may, therefore, say that the form and volume of the nerve cells cannot furnish any very reliable index of their function.

In fact, the form of the sensory cells in man is the same in all the ganglions composed of the brain. On the other hand the volume of certain motor cells (patheticus nucleus) is less than that of neighboring sensory cells (optic thalamus, nucleus of the trigeminus).

B. *Sensory Nervous System.*—(1.) The largest cells are met with in the columns of Clarke, in the neighborhood of the lumbar region. These columns, indeed, receive the centripetal fibres from the lower limbs, and are as far removed from the brain as is possible.

(2.) The cells of the restiform ganglia and of the trigeminus nucleus are smaller than those of the columns of Clarke. The former receive the sensory fibres from the brachial nerves; the trigeminus nucleus receives the fibres of that nerve; and both the two ganglionic masses are comparatively nearer to the cerebral cortex than the columns of Clarke.

(3.) The cells of origin of the optic nerve are smaller than those of the trigeminus, and larger than those of the olfactory.

(4.) The shortest sensory nerve and also the one most contiguous to the occipital lobe (auditory nerve) is also the one which in man has in its nucleus the smallest cells.

The law of increase or decrease of the ganglionary elements is therefore the same for the sensory as for the motor nerves. It may be stated as follows:

The dimensions of the motor or sensory nerves are *in man* in direct proportion to the distance which intervenes between the peripheral organ they innervate and the cerebral centres; or stated in a more general way:

The dimensions of the nerve cells are in direct proportion to the distances over which their motor or sensory impulses must travel.

This formula comprehends two terms, equal in value:

1. The length of the nerves in relation with the cells.
2. The distance between the cell of first reflection and its analogue in the convolutions.

This law holds good for the cortical cells, since we find the largest ones in the parieto frontal regions which are in relation with the inferior members; and we also meet with cells as large in certain portions of the occipital convolutions.

Termination of Nerves in the Unstriped Muscles.—M. Ranvier in a recent note to the Acad. des Sciences, May 27 (reported in *L'Union Médicale*) announces the following conclusions as to the terminations of the nerves in the unstriped muscular fibre: (1) The nerves terminate in the smooth as in the striped muscles, in a more or less arborescent expansion of the cylinder axis. (2) The nerve net-work for the involuntary unstriped muscles (muscles of organic life) is in relation, not with the elementary nerve action which calls the muscle into activity, but rather with a more complex action on which depends the functional synergy of an organ whose activity is dependent on the direct action of the nerve centres. In support of this, he calls attention to the fact that the muscular tunic of the œsophagus of mammals, formed in great part of striated fibres, but which does not contract under the direct influence of the animal's volition, possesses a plexiform apparatus, and that the same has been observed in the striated muscles of the digestive tube of the arthropods.

There is scarcely need now to study why the different authors who have studied the terminations of the nerves in the unstriped muscles have dis

cussed whether they terminate in free ends or in anastomoses. These anastomoses exist, but in reality they form simple plexuses from which arise the terminal fibres.

The Dilator Nerves of the Pupil.—At the session of the Acad. des Sciences, June 10 (reported in *La France Médicale*), M. Vulpian presented the following communication:

I showed, some years since (*Arch. de Physiol.*, 1874, p. 177), that the ablation of the superior cervical ganglion in dogs, did not prevent the reflex movements of pupillary dilatation from being produced on the corresponding side. Did this experimental result show that the sympathetic fibres destined for the iris came from the lower cervical ganglion or from the superior thoracic ganglion reaching their destination, passing with the vertebral artery through the canal in the transverse processes of the cervical vertebræ? Is it necessary to seek the explanation in another hypothesis, according to which the nerve fibres of the pupil dilating the pupil when put in action, arise directly from the brain with certain cranial nerves, the trigeminus and motor oculi? Such were the questions that I was striving to answer when the following facts came to light.

My recent researches, undertaken to test the interesting investigations of MM. Luchsinger and Kendall, Ostrumoff and Nawrocki, relative to the influence of the nervous system on the sudoriparous glands, led me to discover that, in the cat, under the influence of the electrical excitation of the cutaneous surface of the body, or of the upper portion of the divided sciatic, the pupil still dilates on the side from which the superior thoracic and all the lower portion of the cervical sympathetic has been removed. The dilatation, though much less than that of the opposite side, is very clearly perceived, and occurs whenever the above described excitation is repeated.

After having definitely observed this fact, I sought to find whether the same phenomenon would take place in a cat in which not only the superior thoracic ganglion, but also the superior cervical ganglion had been removed. This experiment was performed by cutting the thoracic sympathetic below the upper thoracic ganglion (which as regards the iris is equivalent to its extirpation) and excising completely the superior cervical ganglion. But under these conditions, we have observed that faradic excitations of the skin or of the superior portion of the divided sciatics caused a pupillary dilatation, each time of the same side, incontestable though feeble.

This experimental result refutes one of the two hypotheses which it seemed to me were required to explain the pupillary dilatation under painful excitations after the removal of the superior cervical ganglion. It appears to authorize the admission that nerve fibres acting as dilators of the pupil, come directly from the brain, mixed probably with fibres from such of the cranial nerves as have connections with the ophthalmic ganglion.

Muscle-Tetanus.—H. Kronecker and W. Stirling (*Arch. f. Anat. und Phys.*, 1878, p. 1). Ranvier has lately pointed out the anatomical and physiological differences between the pale and red muscles of one and the same animal, for instance, the vastus int. (white) and the semi tendinosus (red)

muscles of the rabbit. On registering the change of shape of the muscle during contraction by means of a "pince myographique" of Marey, R. found that a current of 56 interruptions per second produced an almost uniform tetanus of the red muscle, while the white still registered successive contractions. A current of even 357 interruptions did not suffice to put the white muscle into a state of complete uniform tetanus, although the tracing of the red muscle on the rotating cylinder was a straight line without oscillations.

According to Helmholtz, the single contraction of a frog's muscle is completed in one-fifth second, while Marey determined the time of contraction of a rabbit's muscles as one-thirteenth second. If a second contraction is induced before the maximum of the first is passed, the two superimpose, and hence a tetanic contraction requires a stimulating current whose interruptions are more numerous than the greatest number of isolated contractions which the muscle can execute per second. Human muscles are tetanized by less than 32 interruptions per second. A muscle when tetanically contracted will emit a sound, the vibrations of which equal in number the interruptions of the current irritating the muscle. If, on the other hand, a muscle is tetanized by irritation of the spinal cord, the muscle-sound corresponds always to about 20 vibrations per second. This proves that a complete muscular tetanus can be maintained by 20 impulses per second. Between this number and Ranvier's results there is a remarkable difference.

Kronecker and Stirling determined, in the first place, that 20-30 interruptions per second suffice to put the white muscle (gastrocnemius medialis) into a state of uniform tetanus, while ten shocks per second can tetanize the red muscles (soleus) of the rabbit. The elongation and contraction of the muscles was directly registered by the myographion. Ranvier, on the other hand, had measured the thickening of the contracted muscle by means of a "pince myographique," which consists of a receiving and a registering air chamber, closed by elastic membranes. Such an apparatus will readily register vibrations, and the appearance of oscillatory contractions of muscle was undoubtedly due to mere vibrations of their mass. With Ranvier, Helmholtz and Stirling found that the single contraction of the red muscle was slower and more protracted than that of the white. Furthermore, they determined the greatest number of interruptions still able to tetanize a muscle. The interruptions were produced by means of a new induction apparatus. The limit could not be found, since 22,000 interruptions per second still produced a uniform tetanus. A muscle will thus correspond to a stimulus of less than 0.00005 second duration.

Influence of the Accelerator Nerve on the Systolic Duration.—N. Baxt (*Arch. fuer Anat. und Phys.*, 1878, p. 122) has registered directly the movements of the heart, exposed in the curarized animal, by placing a light vertical rod of wood on the anterior side of the heart. The rod slid up and down in a glass tube; to its end was attached a glass pen writing on the revolving cylinder. It was thus found that normally the systole does not vary much in duration, lasting 0.25 to 0.28 second. The frequency of the beat depends hence mostly on the duration of the individual diastoles. During

irritation of the accelerator nerve, however, both systole and diastole are shortened, although the latter more than the former.

Movements of the Iris.—Raehlmann and Witkowski (*Arch. f. Anat. and Phys.*, 1878, p. 110), have made observations on the size of the pupil during sleep. In agreement with older observations, they found the pupil contracted during sleep, the actual size, it seems, diminishing with the intensity of sleep. Nevertheless, the pupils react slightly to light. All sensory impressions dilate the pupil, as mere touch or tickling; especially is this noticeable at the moment of awaking. During sleep the size of the pupil does not vary with the ocular movements. Movements of convergence have no effect on the iris, although during waking they are always accompanied by pupillary contraction. Similarly, the authors found that the occasional movements of convergence, which can be observed in new-born children, are not accompanied by pupillary changes.

The cause of the myosis during sleep and narcosis Raehlmann and Witkowski explain in part by the absence of any dilating tendency due to sensory impressions. They admit, however, that a role must also be attributed to the ganglionic cells of the iris.

A New Origin of the Optic Nerve.—J. Stilling, *Centralblatt f. d. med. Wissensch.*, No. 22, states that the opinion hitherto held that the fibres of the optic tract have no connection with the crus cerebri, is incorrect; on the other hand, he finds that a rather considerable part of these fibres arise from a large nucleus situated in the lower crus, which is laid open only when there is nothing to be seen of the substantia nigra in the section. In horizontal and vertical sections it appears shaped like an almond, hence it may perhaps be designated as the *nucleus amygdaliformis*. The optic fibres joining it must, to reach it, take a recurrent course from their former direction.

The whole situation of this nucleus, its size, etc., indicate that we must look at it as an intermediate ganglion for reflex excitations.

The Optic Decussation.—The following is the substance of a note by M. Nicati, presented to the French Academie des Sciences, June 10, by M. Vulpian, and reported in *La France Médicale*, June 26:

Biesiadecki, Mandelstamm and Michel have, in Germany, successively attacked the opinion current since the publications of Newton, Wollaston, and Hannover, of an incomplete crossing of the fibres of the optic nerve in the chiasm. The numerous memoirs that have since appeared have not settled the question, for quite recently Michel has sustained, in a long article in reply to Gudden, the existence of a complete decussation in all the mammals examined and in man.

1. The following experiment ought to destroy all doubts; it proves that the decussation is incomplete in the cat. When we divide the chiasm in the median line, and the animal still sees, the demonstration is made.

But this experiment has been performed by MM. Eugene Dupuy and Brown-Séquard, and by M. Beauregard; the latter operating on birds. M. Brown-Séquard does not state what animals were the subjects of his experi

ments, but from his memoir it seems probable that they were only rabbits and Guinea pigs. The result in the two cases was complete blindness.

The result I have obtained was very different. I operated on cats. These animals do not lose their vision at all from a longitudinal section through the middle of the chiasm. After the operation they move with assurance and give the most varied proofs of the existence of sight. It is needful to choose young cats for this operation, at the age when they begin to move about freely and with vivacity. These animals endure the operation well, and the liveliness of their movements permits the determination of the fact that they see.

The section is made through the mouth, penetrating the cranium through the bones at its base. I use for this a peculiarly shaped bistoury, the curve of which suggests a turnkey for drawing teeth. (It is composed of a single tempered steel strip, the extremity of which is filed so as to form a straight cutting edge of $0^{m}{,}012$, carried at a right angle on a long portion of $0^{m}{,}010$, a length corresponding to the entire thickness between the palate and the dura mater; the remainder of the steel strip is in the shape of a handle curved laterally and at a right angle to the preceding portion. This handle itself is again bent in such a way as to avoid the dental arch.)

By the aid of this bistoury we perforate the base of the cranium between the bony palate and the velum palati, then bringing the instrument more forward we bend the blade backward and downward on the chiasm, which we divide by pressing it strongly against the bone.

2. Desiring to prove that the result reached with the cat is applicable also to man, I sought to establish the structural identity of their chiasms. The chiasms of the cat, man, and the dog, are clearly distinguished from those of the rabbit and Guinea pig by being much larger. I have measured their areas in section, and find constant relations between them, which are the same for the cat and man, but absolutely different from those we find in the rabbit.

Thus, in man and in the cat, the squared surface of a longitudinal section in the median line, compared with that of a transverse section through the middle of the organ, is in the proportion of one to three. The transverse section, in other words, gives a surface three times greater than that of a perpendicular section.

In the rabbit on the other hand, where the decussation appears to be complete, these sections are equal in surface.

To the development in size of the chiasm in man and in the cat corresponds the fact that the nerves and optic tracts join each other in the chiasm at a very obtuse angle, contrary to the manner in the rabbit and Guinea pig.

Asymmetry of the Cranium.—Dr. Gustave Le Bon, in a communication to the Soc. de Méd. Pratiques, *La France Médicale*, No. 28, 1878, gives the results of a series of measurements of crania, to ascertain what was the constant condition as to symmetry, and on which portion was the predominance of development. These measurements do not altogether support the theory that the left side of the skull, and correspondingly, of the brain upon which it is modeled, exceeds the right side in its dimensions, which had gained

some credence on account of the general superior development of the right half of the body, which is functionally related to the left cerebral hemisphere. He found in the measurements of two hundred and eighty-seven crania, the following:

Crania in which the right predominated over the left side, 125; crania in which the left side predominated over the right side, 111; crania presenting various irregularities, but not such as to create any decided unilateral predominance, 51.

From this it appears that there is no general rule as to the asymmetry of the skull; that the predominance is even more likely to appear on the right than on the left side, where *a priori*, it might be looked for.

Dr. Le Bon was led by his measurements to at first think that the skull was most developed in intelligent persons on the left side, but the imperfection of his instrument with which he measured his living cases, led him to distrust this conclusion, at least as a certainty.

Though the skull may be said to be modeled on the brain, there yet appears to be an opportunity for error in using only its external measurements. Dr. Le Bon does not state, but the presumption is that these were all that he took. His crania were taken from different series in the Museum of Anthropology, and may, also, not fairly represent the average development in civilized man, and internal casts, under the circumstances, could hardly have been allowed.

Nucleus of the Facial and Abducens Nerves.—Dr. W. R. Gowers, *Centralblatt*, No. 23, 1878. The grey nucleus in the floor of the fourth ventricle underneath the eminentia teres, has heretofore been considered the common nucleus of the facial and abducens nerves. All fibres of the abducens nerve pass from it at its internal side, while from its external side there issue all those fibres of the facial nerve which do not pass over it in the "genu facialis."

It is taught that the facialis fibres originate in the cells of this nucleus. Gowers, however, had the chance to observe the real relationship in a medulla with complete degeneration of both abducens nerves. No fibres of the last-named nerves were found; the paralysis of the external recti muscles had been complete and of long duration. The facial nerves were intact. But the grey nucleus was completely atrophied, almost all nerve cells had disappeared. The few cells still existing were of less than normal size, and had lost their processes. The degeneration was uniform in degree throughout the entire nucleus. But through the atrophied nucleus there passed many normal nerve fibres, single and in bundles, upwards and outwards. These were the fibres of the facial nerve supposed to originate in this nucleus. It is therefore certain, that the grey substance of the nucleus belongs to the abducens nerve, and that the facialis fibres pass through it simply, but originate probably at the same place as the other fibres passing along the genu facialis.

Negative Oscillation of Muscle-Current.—The negative oscillation which the electric current of the muscle presents during every muscular contraction, has been interpreted by its discoverer, DuBois-Reymond, as the

electric manifestation of the physiological process of contraction. M. d'Arsonval, however (*Gaz. des Hôpitaux*, No. 59, 1878), contends that this phenomenon is purely physical in its origin, in proof of which he refers to the remarkable experiments of Lipman on the currents derived from the contact of non-miscible conducting fluids. The fundamental experiment of that physicist is the following: Two test-tubes are partly filled with mercury, and above the mercury with acidulated water. Contact between the water in the two glasses is established by means of a bridge of wet cotton, while wires touching only the mercury pass to a galvanometer. As long as the two test-tubes are held vertically no current exists, while the needle of the galvanometer is deflected as soon as one of the tubes is slightly inclined. The enlargement of the surface of contact between the two liquids, produced by inclining one tube, results in a positive oscillation, while a negative oscillation is always produced by diminishing the extent of the surface of contact. This current is not due to chemical action; it is simply a transformation of movement into electricity.

When a muscle contracts, it tends to assume a spherical shape. Since a sphere possesses less surface than the same quantity of matter in any other shape, a negative oscillation must result. On the other hand, when a muscle is stretched, its extent of surface is increased, hence a positive oscillation. If two muscles are so arranged that the contraction of one produces an elongation of the other, the currents derivable from each correspond to the requirements of the theory, while if the two poles are inserted into corresponding spots of both muscles at the same time, the opposite oscillations will neutralize each other.

These negative and positive oscillations can be derived not only from the living, but also from the dead muscle, by changing their shape mechanically. It can also be observed in rubber and similar substances.

The negative oscillation is observed also in muscles the actual contraction of which is prevented by mechanical restraint. Although under such circumstances the muscle does not change its shape as a whole, still, as Ranvier has observed under the microscope, there is a similar approach to spherical shape of the ultimate contractile elements (thick disks), and hence a diminution of their extent of contact with the intra-muscular fluid. Physiologically, therefore, the negative oscillation of the muscle-current indicates simply a variation in the surface of contact.

Sinew Reflex.—In order to determine whether the muscular contraction due to percussion of a tendon is reflex in its nature, or a direct muscular stimulation, G. Burkhardt ("Ueber Sehnenreflexe," Festschrift dem Andenken an A. von Haller dargebracht, Bern, 1877) undertook to measure the time required by the phenomenon. Between the concussion of the tendo patellaris *in man* and the contraction of the upper part of muscle quadriceps femoris, there elapsed a period of 40 thousandths of a second, while the contraction of the lower part of the muscle commenced within 38.7 thousandths second. This time is too short to permit a comparison with cutaneous reflexes, which latter consume double that time in passing only through the grey substance of the cord. Similar results were obtained

from rabbits. In that case the time consumed was 17.1 thousandth second, *i. e.*, one-half to one-fifth of the time requisite to obtain a reflex action from cutaneous stimulation. If on the other hand the contraction were due to direct stimulation of the muscle, there ought to be a perceptible difference in the time of contraction of the upper and lower end of the muscle. The velocity of the muscle wave being stated as one to three metres per second, a period of twenty to thirty thousandths second might be expected to elapse while the wave of contraction travelled through the entire muscle of about twenty centimetres length. In reality only one-tenth of that time elapses between the contraction of the upper and lower end of the muscle. Furthermore, B. found that (1) sinew-reflexes continue *without* change of duration after section of the spinal roots, or destruction of the lumbar cord, after which of course all cutaneous reflexes have ceased. (2.) Both cutaneous and sinew-reflexes continue partly after section of the cord at the level of the first lumbar vertebra. The time of the former, however, but not of the latter, is prolonged by this operation. (3.) Section of the crural nerve prevents cutaneous and sinew-reflexes. (4.) Small doses of strychnia intensify sinew-reflexes, but do not change their time. (5.) Crossed sinew-reflexes (from one side to the other) require as much time as cutaneous reflexes.

From these data B concludes that the sinew-reflex is really reflex in its nature, but that it cannot pass through the cord, possibly, however, through the plexus or spinal ganglia. The tendon is hence connected through a nervous route with the muscle, but this route goes neither directly to the muscle, nor does it pass through the spinal cord. The reflex contraction started by stretching of the tendon, as from over action of the antagonistic muscles, is a process of too rapid onset to be influenced by the will. *Centralblatt f. d. med. Wiss.*, No. 22, 1878.

We add the titles of some additional articles on the Anatomy and Physiology of the Nervous System and Mind:

Bufalini, The Structure of the Spinal Cord in the Fœtus, *La Sperimentale*, September; Baumgarten, On the so-called Decussation of the Optic Fibres, *Centralbl. f. d. med. Wissensch.*, No. 31; Gowers, Incomplete Crossing of the Optic Fibres, *Ibid;* Maragliana, Motor Localizations in the Cerebral Cortex, studied specially from the clinical side, *Rivista Sperimentale*, IV., 1; Tartuferi, On the Microscopic Anatomy and the Cellular Morphology of the Bigeminal Eminence in Man and other Animals, *Ibid;* Luciani and Tamburini, Experimental Researches on the Functions of the Brain, *Ibid;* Mayer, Results of Researches on the Stoppage and Restoration of Blood Currents in the Head, *Centralblatt f. d. med. Wissensch.*, No. 33.

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

THE PATHOLOGY OF HEMIPLEGIA.—Dr. B. F. Lautenbach, *Phil. Med. Times*, Aug. 3d, takes the view that the unilateral paralysis from cerebral disease is not due to destruction of the motor centres, or interruption of the routes of conduction in the base of the brain, as is usually held. In support of his opinion he cites numerous cases of extensive brain lesion in which motor paralysis did not appear, and the asserted fact that hemiplegia does not occur in the lower animals from destruction of brain substance. (Dr. Lautenbach seems to forget the results obtained by Ferrier in monkeys.) He likewise objects on similar grounds to the view that the paralysis is due to pressure, and also rejects the theory of Brown-Séquard that the paralysis is chiefly the result of an irritation, on the grounds that the effects of irritation are very unreliable and uncertain, and do not justify the conclusion.

The theory of the pathology of hemiplegia that he is inclined to adopt is, that it is the result of polarization. He commenced the study of the effect of irritation of a polarized nerve in 1876. Previously, Nobili, Matteucci, Valentin and Eckhard had shown that muscular contractions caused by the irritation of a motor nerve could be prevented by the passage of a constant or polarizing current through a portion of the nerve, either above or below the point irritated. Later, as is well known, Pflueger, using weaker currents, showed that the excitability was increased on the side of the cathode and diminished on that of the anode. Lautenbach found that the effect of the irritation of a polarized nerve as compared with that of a nerve not polarized, depended largely on the proportion existing between the strengths of the two currents. When an irritating current sufficient to produce tetanus was used, he was able to prevent all contractions by applying simultaneously a polarizing current of a certain strength. "If now," says he, "an irritating current, just sufficient to produce slight muscular contraction, was employed, according to the strength of the polarizing current would the contractions produced by the former current be increased, prevented, or not affected. In the same manner, varying the strength of the irritating current will influence the effect of the polarization.

"May these results not be of service in explaining the hemiplegia and other symptoms following hemorrhage or tumor in the brain? The irritation would here be the excito-motor impulse. It is well known that when we increase the strength or intensity of this impulse by exciting a hemiplegic person, he will often voluntarily move the palsied limbs, but if the excitement is now removed the hemiplegia will again be apparent. Varying the strength of the irritation (*i. e.*, the excito-motor impulse) under these circumstances, determines whether, for the time being, the patient is hemiplegic or not. The polarizing influence is produced by the hemorrhage or some other lesion setting up a constant current in the brain fibres.

"Does the blood in apoplexy act as a polarizing agent? In some experiments on the peripheral motor nerves I found that coagulating blood would replace a polarizing current in its effects on the irritability of the nerves. In other experiments blood taken from the jugular vein of one animal was injected into the brain, in the vicinity of the corpus striatum of one side of another. Very frequently hemiplegia of the opposite side of the body was produced. Sometimes instead of being palsied, the animal was convulsed; and in still other instances no effect was produced.

"Ammonia is another polarizing agent which, injected in very small quantities in the neighborhood of a corpus striatum, will frequently produce hemiplegia. The glucoside saponin acts in the same manner."

Dr. Lautenbach goes on to say that he believes these to be the first cases of true hemiplegia ever being produced in any of the animals below man. In this he forgets at least Ferrier's experiments on monkeys, in which complete hemiplegia was produced by destruction of cortical grey substance.

As to his theory, which is expressed in his final conclusion, "that cerebral hemiplegia is due, not to destruction of brain-substance, nor to irritation or pressure, but to polarization," we cannot see that it has any special basis for its support, the exceptional and largely ill-reported cases to which he refers not being sufficient to invalidate the accepted notions. It is natural, moreover, to suppose that if the fibres conducting motor impulses from the centres exist, their severance or interruption would produce motor paralysis. Dr. Lautenbach must carry his objections so far as to the existence of routes of nervous conduction (which he does not), in order to be consistent, so it appears to us. The statement that it is a well known fact that hemiplegics often make voluntary movements of paralyzed limbs, under special excitations, must be taken with some allowance, if we are to understand the word "voluntary" in its usual sense. Special excitement may call out more complicated reflexes from higher centres than ordinary, as it will produce complex oaths and other emotional expressions in aphasic patients, but these are not by any proper use of the term to be included among voluntary acts.

The comparative infrequency of hemiplegia in the lower animals is easily accounted for, or at least a very plausible hypothesis to account for it is afforded by the lesser development and importance of their higher centres, and the consequent relative approximation of the nature of all their voluntary acts to reflex ones.

Hemiopia from Cortical Disease.—In No. 21, of the *Centralblatt f. d. med. Wissenschaften* (1878), Dr. P. Baumgarten describes a case of left lateral hemiopia coming on suddenly after exposure to cold. The left half of the field of vision of both eyes was completely gone, the line of separation passing through the fovea centralis, while the other part of the retina which still possessed visual perception was normal. This state continued without change during several months, until death occurred from cardiac paralysis in renal disease. The autopsy revealed an apoplectic cyst the size of a walnut in the substance of the right occipital lobe. Its lower wall was separated from the cavity of the right posterior horn by a layer of normal

medullary substance several millimetres in thickness, while its upper wall was formed by the convolutions of the three gyri-occipitales, which were in a state of yellow softening, but still recognizable in shape.

There was found, besides, a spot of red softening of the size of a pea in the upper wall of the left anterior horn; also an apoplectic cicatrix of half that size in the centre of the right thalamus. No other gross or microscopical lesions existed within the cranial cavity.

Paralysis of one-half of each retina from a lesion of the occipital lobe of the same side exactly corresponding to this case, was likewise obtained by Munk in his experiments on monkeys.

A case resembling the former is also described by Dr. Pooley, in the *Arch. of Ophthalmol. and Otology* (VI., I). A gentleman of 55 years consulted Pooley about visual hallucinations, epileptiform convulsions and maniacal attacks. Thirty years ago he had contracted a chancre and been treated with mercury, since which time no syphilitic symptoms had appeared. The patient appeared dizzy and mentally deranged and excited. He possessed a myopia one-tenth; the visual power was normal. Pooley diagnosed cerebral syphilis and ordered iodide of potash, bromide of potash and bromide of ammonium. A few days subsequently a right lateral hemiopia of both eyes set in, the limit of the visual field passing through the centre of the retina. Within some weeks a paresis of the right side of the body and diminished sensibility of the right arm was noticed. Six months subsequently, vision was reduced to twenty-fiftieths, and the appearances of choked disc were found in the left eye.—Death.

The autopsy showed the left posterior lobe of the cerebrum enlarged, and in it there was found a yellowish-white, hard rounded tumor of about three-fourths of an inch in diameter and one-half inch in width. The left thalamus and the medullary substance surrounding it was softened. The tumor possessed all characteristics of a gumma. (*Wiener med. Woch.*, No. 20, 1878.)

Tendon-Reflex in General Paresis.—Dr. Muhr (*Psych. Centralbl.*, No. 2, 1878) draws the following conclusions from an examination of fifty-one cases:

1. The contraction of the quadriceps muscle on percussion of the tendo patellaris occurs almost always in general paresis, even in advanced cases with paretic lower extremities.

2. In fifty-one cases the tendon-reflex was absent six times. These cases were not at all moribund or even ataxic.

3. Neither the condition of the pupils, nor any other clinical symptoms indicate whether the reflex can be produced. The lack of the reflex is found as an isolated phenomenon in different pareses.

From a consideration of Westphal's latest researches (*Arch. f. Psych.* VIII., II. 2), Dr. Muhr finally concludes, that an absence of the tendon-reflex in any case of general paresis is a positive sign of degeneration of the posterior columns, even when no ataxia is observed.

Contractures in Locomotor Ataxia.—M. Onimus presented to the Society of Biology (*Gaz. des Hop.*, July 30, 1878) a paper on the above

subject. By contracture he wishes to imply simply a diminution of the normal muscular softness and a certain delay of relaxation after the voluntary contraction. Complete rigidity of muscular fibres is only a more intense degree of contracture. The first degree of contracture exists in a number of diseases of the central nervous system, even without alteration of the motor nerves. In locomotor ataxia, the transformation of the posterior spinal columns into a different tissue is a cause of excitation of the anterior columns, and thus provokes indirectly the contractures.

In this disease disturbances of the motility of the eyes are a frequent occurrence. Without denying the occasional occurrence of paralyses of the ocular muscles, Onimus claims that these disturbances are usually due to contractures of the affected muscles. The motor impairment of the extremities, however, is never traceable to any paralysis. But it can also not be completely explained on the basis of inco ordination or loss of sensibility. Onimus, on the contrary, refers it to a degree of contracture of the muscles. The patients complain of muscular tension, they feel the rigidity of their muscles. They feel a mechanical restriction in their movements, which, it is true, they can overcome by a special effort. The observer can also notice the muscular rigidity; frequently the muscles are even tender to pressure.

A phenomenon hitherto but little noticed, is the delay of muscular contraction. The patients themselves often notice how much time elapses between the effort of volition and the contraction of the muscle. A similar delay of sensibility is even more striking, and is in fact well known.

That which characterizes essentially the inco-ordinated movements of ataxia *is the suddenness of the movement, and its exaggeration when compared to the purpose intended.* The patient when attempting to move, feels a certain resistance, due to the contracture, and to overcome this he transmits to the muscle a greater impulse than is actually necessary. His motor centres are incorrectly informed of the static condition of the muscles on account of the muscular contracture. The retardation of the volitional effort only increases its intensity, hence the suddenness and exaggerated extent of all muscular movements. The latter, on the other hand, account for the fatigue so easily induced in ataxia.

Hydrophobia and Sexual Excitement.—As to the experimental production of rabies—its generation *de novo*—experimental veterinary science and the pathological observations of competent veterinarians in Europe furnish some facts worth recording. Fleming reports the case of a cross-bred spaniel, five and a half months old, which had never left its mother, never went out, and was naturally very quiet. When the mother became in rut, the young dog, excited by the odor, became extremely agitated, and at last refused food. Three days later, being asleep, it wakened suddenly, and flew savagely at the attendant. When taken to the infirmary of an eminent veterinary surgeon in Paris, rabies was diagnosed. The animal refused all food for the next three days; on the fourth, rabietic paralysis supervened; on the fifth it died. Another case of what Fleming styles spontaneous

rabies, occurring under similar conditions, is noted in a late number of the *Veterinary Journal*, London. In a box adjoining the kennel of a male was placed a female in rut, the effluvia from which caused the most ardent venereic excitation. For fifteen days, placed in such tantalizing proximity, the unfortunate animal manifested the most extreme agitation, and finally went furiously mad. Similar results have been produced by confining males near a female in rut, particularly by M. Toppolin, who describes them as so constant as to leave no room for doubt as to the agency of intense sexual or other excitation in producing rabietic madness. It is not an unfamiliar fact that rabies have been experimentally produced in the inferior animals by inoculating them with man's saliva, as well as with the saliva of dogs not rabid. A Guinea pig thus inoculated became subject to violent convulsions, which could be excited at pleasure by dashing a little water at him. A rabietic pig, which lay torpid under the severest beating, could be excited to convulsions at pleasure by flirting a piece of white paper in its face.—*N. Y. Med. Record*, July 6.

Muscular Contractions in Hystero-Epilepsy.—MM. Regnard and Richet presented to the Society of Biology a report of the application of the graphic method in the study of hystero-epileptic attacks. (*Progrès Méd.* No. 29, 1878.) A myographic tambour is placed on some muscle and connected with a polygraph registering the muscular contraction. It was thus found that various attacks observed at different hours of the day were alike in character and duration, and even in the duration of the different stages of which attack consists. There is first a real muscle-tetanus, followed by clonic single contractions (secousses); hereupon the extensive movements and finally the passionate attitudes during which the contractions are not regular.

The authors state, besides, that hystero-epileptic attacks can be aborted by the application of strong constant currents, forty elements of Trouvé, and especially by sudden reversion of the direction. The continued use of the current will finally put a definite end to the attacks.

Pathology of Tetanus.—Dr. E. Aufrecht detailed in the Med. Soc. of Magdeburg, Feb. 7, 1878 (*Deutsche med. Woch.*, 1878, Nos. 14 and 15), the lesions found in a case of tetanus. A laborer æt. 41, suffered from a complicated luxation of the thumb, treated antiseptically. Within a week tetanus set in, and although the median nerve was stretched, death occurred two days later. At the post-mortem the unaided eye could observe only a moderate effusion into the dura mater and considerable hyperæmia of the pia mater and gray substance of the cord. After three months' hardening in bichromate of potassium (5 per cent. sol.), the cord was examined microscopically.

In the lumbar portion, no change whatever was found in the ganglionic cells of the anterior cornua. The cells of the posterior cornua, however, were diminished in size and filled with pigment granules. Nucleus and nucleolus and

most processes had completely disappeared. Some cells had become converted into homogeneous yellow globules. The morbid change increased in intensity upwards. In the middle of the dorsal cord no healthy cell could be found. The anterior cornua consisted of yellow, oval or rounded bodies without processes, nucleus or nucleolus. No pigment-granules were found in these bodies. The cells of Clarke's columns were also stained deep yellow, but their nuclei were still visible, while the nuclei had mostly disappeared in the cells of the posterior cornua.

The greatest change was found in the cervical portion. All ganglionic cells were of a diffuse rusty-red appearance; neither pigment-granules, nuclei nor nucleoli could be seen. The nerve-cells of the left side were *smaller* than those of the right; in fact, many of them were smaller than a normal nucleus.

The general changes in the gray substance were most marked in the cervical portion. In the ground-substance fine dark granules as well as larger pigment-granules were interspersed, especially in the substantia gelatinosa centralis around the central canal. The epithelium of the canal was normal; its calibre somewhat narrowed in some sections. Brilliant structureless vesicles resembling oil globules were besides found in the ground-substance of the white commissure and still more numerous around the central canal. They could not be derived from pre-existing cells, since according to Henle, no cellular structures whatever exist in the vicinity of the human central canal. Granules were also found in the white substance; here a number of nerve-fibres showed also a granular appearance of their medullary sheaths. All vessels were intensely injected with blood; in the adventitia dark granules were found. Hyaline masses were seen on the outside of some vessels, also within the central canal; at some spots they possessd the appearance of fibrine.

These observations Dr. Aufrecht interprets as parenchymatous inflammation, starting in the gray substance of the cervical cord and descending. The hyperæmia and exudation he regards as secondary; the granules and oil globules he considers the products of disintegration of the nerve cells.

Erythromelalgia.—This is the title which Dr. S. Weir Mitchell proposes for an affection which he first noticed in 1872, but of which he gives a more extended account in the last number (July) of the *American Journal of Medical Sciences*. The following is his account of the symptoms:

The patient, nearly always a man, after some constitutional disease, like a low fever, or after prolonged physical exertion afoot, begins to suffer with pain in the foot or feet; usually it comes in the ball of the foot, or of the great toe, or in the heel; and from these parts it extends so as to involve a large portion or all of the sole, and to reach the dorsum and even the leg. More often it is felt finally in a limited region of one or both soles, and does not extend beyond these areas. At first it is felt only towards night, and is eased by the night's rest; but soon or late, it comes nearer and nearer to the hour of rising from bed. In like manner, while at first it is made to increase only by excessive exertion afoot, by and

by it comes on whenever the upright posture is assumed, or even when the foot is allowed to hang down. Since, however, the disease is not necessarily progressive, there are instances in which the pain never passes a definite limit. One case may for years have the trouble only in the evening; and a second may reach and remain at the point where only a long walk in summer causes it; a third may stand still, as it were, in a far more advanced stage of the malady, and though suffering horrible pain, become no worse; while in the gravest cases, more familiar signs of organic disease of the spinal cord may arise to shed light upon the pathology of the minor forms of the trouble.

In rare cases, the first pain is said to be an ache of the foot; but in the mass of instances, and soon or late in nearly all, the pain is of a burning character. "It is the pain of a burn;" the pain of mustard; "of intense sunburn;" at least these are the phrases used to describe it, and certainly the character of the suffering is often so well marked as to be clinically distinctive. In the milder cases it may come and go, or be present daily at some time, as upon exertion, and yet be but trifling in its intensity; while in severer cases the burning reaches the extreme of torture. The sufferer, when placed on his feet, rocks as if unsteady; and if his eyes be closed, may deceive the observer into the belief that he has before him a case of locomotor ataxia. Yet a vigorous effort of will is fully competent to preserve the balance; and this unstable equilibrium is not seen until, owing to the upright posture having been preserved for some minutes, the pain has risen to a maximum of anguish.

In the later stages of the disease the pain is throbbing, aching, and burning, owing, I suppose, to the vasal disorders, which are seen in some cases throughout, and always in the graver examples.

In every case, and at all stages, the pain is relieved or arrested by the horizontal position, and by cold. It is brought on and made worse by standing or walking; and in bad cases, by allowing the feet to hang down; while warmth, and, of course, heavy feet covers act in like manner. Summer is usually, not always, the season of greatest annoyance; winter, a time of comparative ease. The sufferer sleeps with uncovered feet, and goes about without stockings in his house; and finds, even in winter, a light slipper or a low shoe comfortable.

The next striking peculiarity of this disorder is the flushing of the part upon exertion. This symptom, which is usually absent in the very early stages, is a notable feature of the worst of the prolonged cases, and in some mild instances can always be brought on by great exertion afoot. In the graver examples, the area of greatest pain in the soles or hands is distinctly and permanently marked by a dull, dusky, mottled redness, as if the smaller vessels were always over-distended. In these, and in some of the less severe cases, the region of pain is in places tender, and firm pressure by the finger or hand will bring on increased pain, and even cause the whole foot or hand, or a part of it, to become red, just as it does when the man stands up.

The pain in these cases is also entirely inhibitory of walking, and if this action be persisted in, gives rise to intense redness, swelling from dilatation of vessels, and finally to blistering of the soles.

Where flushing is a part of the phenomena of this interesting malady, it comes on during the erect position, slowly in milder cases, and almost at once in others, and involves both veins and arteries. The foot gets redder and redder, the veins stand out in a few moments as if a ligature had been tied around the limb, and the arteries throb violently for a time, until at length the extremity becomes of a dark purplish tint.

In the worst cases, when the patient is at rest, the limbs are cold, and even pale. The flushing, which at first seems to be an active condition, accompanied with rise of temperature, in a few minutes becomes passive; that is, the arteries cease to throb, the heat lessens, and there is evidence of lessened oxidation.

The less severe examples manage to get along by rest at intervals, but the worst cases are unable to stand for more than a moment, and the sufferer crawls on his hands and knees, keeping his feet off the floor, or is obliged to be carried about.

I have seen lately two examples in which the disease seemed to have been progressive, and to have been associated in the later stages with distinctive evidences of spinal disease, such as atrophic states, the pain belt (*douleur en ceinture*), partial losses of power, and other phenomena, which vary in the two cases alluded to. Also in one of them all the singular features which in the early stages were seen in the feet, were at a later stage exhibited in equal fullness in the hands, or rather in the hand, since one had been lost by amputation.

One other peculiarity is common to all of them. They are rarely amenable to treatment. They are aided for a time by cold and rest, but either they remain unchanged for years, or else, in rare instances become gradually worse.

As to diagnosis, I am aware of no other malady with which the bad cases of this trouble can be confounded; but there are certain other more or less painful affections of the feet, with which the lighter forms might be confused * * * (gout, rheumatism, syphilitic tender feet).

Dr. Mitchell gives full accounts of a number of cases of this affection, which he thinks is connected with some cerebro-spinal or spinal lesion, and may yet be found to correspond to limited lesions of definite tracts in the brain or cord. Some of the cases are of his own observation, others were reported by Vulpian, Graves and Paget. He also gives for comparison, accounts of several cases of a very similar and troublesome affection with which this might be confounded; that described by Prof. Gross as *podynia*, but which differs from the one above described in being less of an apparently vaso-motor nature, and due probably to low inflammation of the joint structures of the foot.

Hyperæsthesia from Section of the Spinal Cord.—Dr. W. Koch, *Virchow's Archiv*, Bd. 73, Heft II., p. 273. Our previous knowledge on this subject may be summed up as follows: Section of one lateral half of the cord is followed at once by a hyperæsthesia of the skin of the other side of the body, behind the line of section. The hyperæsthesia diminishes in some degree after a few hours or days, and remains in this state of intensity

for many weeks, or even two years (Brown-Séquard), to be superseded finally by diminished sensibility; complete section of one-half of the cord, reduces the sensibility of the other side of the body more or less. Hyperæsthesia of the other side of the skin occurs when its corresponding half of the cord has also been mutilated. If after one-half of the cord has been divided, the knife severs a portion of the grey substance of the other half, the hyperæsthesia still continues, but it diminishes when about three quarters of the grey substance (of the second half) are cut transversely. A single narrow bridge of grey substance, however, suffices to maintain a feeble degree of hyperæsthesia (Schiff). Attempting to localize detailedly the strands, section of which increases the sensibility, Schiff names simply the posterior white columns. Tuerck, on the other hand, referred the phenomenon to section of the lateral columns and the proximate grey substance; he experimented mainly in the cervical region, where Schiff claims that the parts mentioned are but the continuation of the posterior columns of the lower part of the cord. According to Ludwig and Woroschiloff, hyperæsthesia of the hind legs follows a simple section of the internal half of the middle third of the lumbar lateral columns. Experimenting upon rabbits, Koch found the hyperæsthesia not limited to the skin, but to extend as well to the fascia, periosteum and especially the joints. In fact it is most marked in the latter parts, so that any extensive movement of a joint produces evidence of great pain. A section not lower than the level of the third lumbar vertebra produces hyperæsthesia in the entire hind extremity; the trunk and fore leg participate when the section occurs at the level of the sixth cervical vertebra. If the cord is severed one vertebra below, the fore legs are exempt.

With Woroschiloff, Koch agrees in localizing the necessary lesion in the lumbar region in the internal half of the middle third of the lateral column (corresponding to Flechsig's lateral cerebellar tracts). This strand passes towards the surface in ascending the cord, so that in the cervical region hyperæsthesia is produced by a section implicating only the outer half of the lateral column. The greatest possible degree of hyperæsthesia is at once produced by dividing the external quarter of one-half of the medulla oblongata. In no locality is a lesion of the grey substance necessary to obtain this result.

Koch succeeded in producing separately hyperæsthesia of the skin and of the joints. The fibres, section of which renders the joints hyperæsthetic, are found in the cord to the outside of the fibres related to the sensibility of the skin. The two strands, however, intermingle at the line of contact to such an extent, that the greatest possible degree of hyperæsthesia of either the skin or joints is obtainable only by dividing more than one strand. The longest duration of the increased sensibility which Koch ever met was 20 days (survival of the animal?). The feeble hyperæsthesia resulting from incomplete section does not seem to be as permanent.

A hyperæsthesia is sometimes observed clinically when due to a peripheral lesion, such as pressure of a tumor upon a nerve, or suppuration around the posterior roots. Vulpian claims even to have produced a hyperæsthesia experimentally by gradually ligating a mixed nerve. Notwithstanding numerous attempts Koch could not repeat this experiment successfully.

An exquisite hyperæsthesia of the entire body is produced by removal of the entire encephalon above the medulla oblongata in young rabbits. This diminishes in intensity after fifteen minutes, but it is still noticeable after the lapse of several hours. In this state of hyperæsthesia, the sensibility may be still more exalted by a hemi-section of the cord. The parts whose sensibility is ordinarily increased by such a section, become now still more hyperæsthetic, so that their irritation will induce the animal to cry, which cannot be done by irritating other less hyperæsthetic parts. The possibility of producing hyperæsthesia by and after removal of all parts above the medulla, proves the spinal origin of the hyperæsthesia. The crying of the animal under such circumstances overthrows, moreover, the validity of drawing a line between conscious and unconscious sensibility. The latter was usually considered a simple reflex, the former a reflex complicated with psychic excitations as manifested by the cry. But since at least in mammals no psychic functions are attributed to the medulla and cord, this distinction is no longer tenable.

As a matter of especial interest Koch finally found, that the hyperæsthesia could be made to disappear by irritating with a strong electric current any part anterior to the spinal section. This succeeded also after removal of the encephalon.

Reflex Tremor in Hemiplegia.—At the session of the Soc. de Biologie, June 1 (rep. in *Le Progrès Médicale*), M. Dejewine made a communication on the existence of reflex tremor in the sound leg in certain cases of hemiplegia. Reflex tremor in hemiplegia has been noticed several years since: to produce it, it is only necessary to flex the leg strongly on the thigh, when there is caused a trembling, feeling to the hand of the observer like a regular series of shocks. It is known under the name of reflex hemiplegic tremor, and is rather commonly observed.

Up to the present time this tremor had been observed only in the limb of the paralyzed side. A series of researches by M. Dejerine show very clearly that it may appear, with absolutely similar phenomena in the lower limb of the sound side, and that it may be found in a rather large number of cases.

In some fifteen hemiplegies observed with reference to this symptom, he observed it five times, and in these five cases the tremor was so marked in the lower limbs that he at first thought he had to do with cases of paraplegia by compression of the cord, or of multiple sclerosis. But in all of these the member of the well side was intact as regards motion and sensibility.

The explanation of this phenomenon is rather difficult since, up to the present time, autopsies have been lacking. May it not be that the lateral sclerosis, which is constantly present on the paralyzed side in old cases, has invaded the sound side?

M. Gubler remarked on M. Dejerine's communication, that tremor is a phenomenon of imperfect conduction, and could be produced under various conditions by placing the member in such a posititon as to compress the nerves.

The following are the titles of some of the recent articles on the Pathology and Pathological Anatomy of the Nervous System:

Schultze, The Anatomical Alterations in Acute Atrophic Paralysis of the Adult (Poliomyelitis Acuta Anterior), *Virchow's Archiv*, LXXIII., 3; Mann, Cerebral and Spinal Anæmia, *Phil. Med. Times*, Sept. 28; Glynn, On Cases of Cerebral Tumor and other Forms of Brain Disease, with Special Reference to the Doctrine of Localization, *Brit. Med. Journal*, Sept. 28; V. Holst, A Case of Neurosis from Shock, *St. Petersburg. med. Wochenschrift*, No. 32; Schultze, A New Pathologico-Anatomical Discovery in Tetany, *Centralbl. f. Nervenheilk.*, No. 8; Hallager, Cases of Progressive Facial Atrophy, *Hospitals Tidende*, Sept. 11 and 18, 1878; Krafft-Ebing, On the so-called Querulant Insanity, *Allg. Zeitschr. f. Psychiatrie*, XXXV., 4; Day, Neurosal Affections of the Heart in Children, *Practitioner*, Sept.; Bouteille, Sanguineous Tumors of the Pavilion of the Ear, *Ann. Med. Psychol.*, July; Cullerre, General Paralysis and Insanity, *Ibid;* Tiling, On the Classification of Insanity, *Psychiat. Centralbl.*, No. 3; Sawyer, Clinical Lecture on the Causes and Cure of Insomnia, *Lancet* (Am. Repr.), September; Caldwell, A Review of the Neurosis of the Pneumogastric and Sympathetic Nerves, &c., *Virginia Med. Monthly*, October; Brunton, Reflex Action as a Cause of Disease, and Means of Cure, *Brain*, July; Hutchinson, Notes on the Symptom-Significance of Different States of the Pupil, *Ibid;* Buzzard, On a Prolonged First Stage of Tabes Dorsalis, &c., *Ibid;* Lawson, On the Symptomatology of Alcoholic Brain Disorders, *Ibid;* Fothergill, The Neurosal and Reflex Disorders of the Heart, *Ibid;* Schultze and Rumpf, Degenerative Processes in the Human Spinal Cord, *Centralblatt f. d. med. Wissensch.*, No. 37.

c.—THERAPEUTICS OF THE NERVOUS SYSTEM AND MIND.

THE ACTION OF ANÆSTHETICS ON THE RESPIRATION AND HEART.—We take the following from the *Union Medicale* of June 8:

"We give *in extenso* the very interesting communication read by M. Vulpian to the Academie des Sciences, on the action of anæsthetics (sulphuric ether, chloroform, chloral hydrate) on the respiratory centre and the cardiac ganglia.

"The experiments of E. H. Weber have shown that, in an animal whose two pneumogastric nerves have been divided in the neck, the faradization of the peripheral or thoracic segments of these nerves, even with a current of moderate intensity, causes a suspension of the cardiac movements. We know also, since the researches of Traube, that a sufficiently energetic faradization of the superior, cephalic, extremities of these nerves, causes an immediate arrest of the respiratory movements. These facts, which are very interesting, have been studied in their lesser details by various physiologists. The experiments of Weber and of Traube may be repeated on animals that have not undergone any previous intoxication; we have ordinarily performed them on animals curarized, or under the influence of morphine, or anæsthetized by ether, chloroform or chloral.

"If we practice faradization of the peripheral segments of the pneumogastric nerves in a curarized animal, we observe, in a general way, as all the experimenters have done, the same effects as in an animal not poisoned. The heart is flaccid, arrested in paralytic relaxation, and after a few seconds it gradually begins again to contract, even while we hold the electrodes in contact with the nerves. The effects of the faradization of the peripheral segments of the pneumogastric nerves are nevertheless not absolutely identical in curarized and non-curarized mammals. While in the latter we can arrest the heart by electrizing only one pneumogastric, in the curarized mammal it can be caused only a little as the intoxication is pushed; on the other hand, the so-called diastolic arrest of the heart lasts a less time when the faradization is prolonged than in a non-curarized animal. If the curarization is profound there is a period during which the strongest faradization does not arrest the heart, nor even slow it; the only effect observed even is an acceleration of the organ.

"These are well known facts, showing that previous curarization, a procedure often employed to render the animal experimented upon immovable, does not leave intact, in mammals, the cardiac extremities of the pneumogastrics, a fact not in accordance with what was believed at the beginning of the study of this poison.

"The anæsthetics, *i. e.*, ether, chloroform, and chloral, which we also very frequently employ to render animals insensible, without destroying the motricity of the nerves, also modify in a certain way, different from the pre-

ceding one, the effects of the excitations of the peripheral portions of the divided pneumogastrics; and they also influence, in fully as manifest a manner, the effects of the excitation of the superior or cephalic portions of these same nerves.

"I take, for example, chloral hydrate, often employed at the present day in physiological laboratories in the form of intravenous injections. When we inject into a vein (the saphenous vein, for example) of a dog, a watery solution of chloral hydrate of the strength of one-fifth, and in quantity sufficient to produce deep sleep, we cause also a complete anæsthesia, on the characters of which I need not dwell. I should rather confine myself to the point that furnishes an explanation for the fact to which I desire to call attention. In chloralized animals the movements of the heart persist, and the same is the case with the spontaneous respiratory movements. All the physiologists who employ this convenient method of previous anæsthesia in their various experimental researches, have observed that in certain cases, not infrequent, especially when the intravenous injection is not performed slowly and gradually, the dogs (and other animals also) cease abruptly to respire, after a certain amount of the chloral has entered the circulation. It is a sort of respiratory syncope that is thus produced, and usually the cardiac movements are not simultaneously arrested; they go on still for one or two minutes or even for a longer time. The spontaneous respiratory movements may usually be revived by keeping up artificial respiration by means of repeated pressure on the thorax, and, still better, by faradization of the trunk. To apply this faradization, we apply one electrode to the face or neck, and the other to the base of the chest, or the subthoracic part of the abdomen. It produces instantly an inspiratory movement; we interrupt the current and the parts come again to rest, thus effecting expiration, and we repeat these some fifteen or twenty times in a minute. Artificial respiration, effected in this way, keeps up the heart's movements, up to the time when the bulbar centre retakes its function. Sometimes this result is only attained after eight or ten minutes of artificial respiration practiced as described. I have seen a dog only begin to respire spontaneously after twenty-two minutes of artificial respiration by constantly repeated thoracic pressures, and the instantaneous faradization I have described twenty times a minute. In some cases all means are inefficacious, the heart comes to a stop and the animal dies.

"Sometimes this sort of respiratory syncope does not occur when we inject chloral into the veins, nor for some seconds after; it is at the end of some minutes that the respiration ceases abruptly, sometimes without recognizable cause, sometimes when an experiment is begun, and probably under the influence of this or that traumatic irritation. The same means are necessary to re-establish spontaneous respiration.

"Effects of the same kind may be produced in animals anæsthetized by ether, chloroform, or other analogous substances.

"On the other hand, one other accident may occur in chloralized dogs, and this accident is most often irremediable. The heart may be arrested more or less suddenly, either directly during the injection, or subsequently during the performance of some experiment involving the sensory nerve

fibres. The heart stops before the respiration, the respiratory movements ceasing a few seconds later. It is rare that faradization, even when applied as soon as the disappearance of the arterial pulse is discovered, recalls the functions of the heart.

"This arrest of the heart, this *cardiac syncope*, is also seen in animals under the influence of chloroform or ether, and it certainly is more readily produced in animals who are stupefied by the anæsthetics in question, during vivisection, than in those who have undergone no previous intoxication or who are paralyzed by curare. In these the weakened state of the inhibitory action of the vagi is undoubtedly a condition which renders the reflex excitation of these nerves less dangerous.

"It follows from these preliminaries that in anæsthetized animals, and especially in those that are chloralized, the respiratory centre undergoes notable modifications. The increase, though slight, of the amount of chloral in the circulation, may suspend the function of this centre. It may also lose its function under the influence of more or less irritative causes, either arising in such or such organ, or produced during the performance of vivisections. On the other hand, the excitor ganglia of the movements of the heart may be paralyzed, under the same circumstances, when there is either an excessive quantity of chloral injected, or when the traumatic irritations of vivisection provoke the inhibitory functions of the cardiac fibres of the vagi into action.

"But, if we repeat on dogs profoundly under the influence of chloral, the experiments of E. H. Weber or of Traube, we observe the following:

"Faradization of the superior, cephalic, segments of the divided vagi, arrests the respiratory movements, as in animals not anæsthetized; but, while in the latter the respiration resumes spontaneously and naturally, in the great majority of cases, despite persistent electrization, it cannot be revived in the chloralized animals, and they die, unless we make haste to suspend the electrization of the vagi and make use of artificial respiration, either alone, or with the aid of faradization of the trunk, energetically applied for a moment and repeated every three or four seconds.

"Frequently it suffices to faradize the cephalic sections of the vagus for a few seconds (3–10) to cause an arrest of respiration, and this may be fatal unless resort is had to artificial respiration and faradization, contractions of the trunk, which act at the same time in producing inspirations of a certain amplitude, and waking up the nervous centres from their profound stupefaction.

"We, therefore, obtain readily, and very frequently, in these conditions, under the influence of faradization of the superior segments of the divided vagi the same effect that M. Bert had sometimes observed in non-chloralized animals, *i. e.*, sudden death, unless artificial respiration is resorted to.

"It is worth while to say here, that if we repeat the experiment many times on the same dog, we do not obtain the same result as when we repeat it only two or three times; it becomes impossible to produce the arrest of respiration with impending death. The respiratory movements revive spontaneously after a greater or less time, although faradization of the upper extremities of the vagi is continued.

"If, under the same conditions of complete chloralization, we submit the lower segment of the divided vagus to a jerky induced current, we find the arrest of the heart in diastole, as in non-anæsthetized animals, but also (what is rarely seen except under these conditions) that it may cease definitively if the faradization is prolonged. We find, moreover, that this permanent arrest of the cardiac movements does not usually occur if we suspend momentarily these movements two or three times by the aid of the faradic current, before submitting the cardiac segments of the vagi to prolonged electrization.

"These facts are not without interest as regards the physiological study of anæsthetics; they may, also, contribute to the explanation of certain accidents of clinical anæsthesia, hence I have communicated them to the Academy."

Ether in Sciatica.—Dr. C. G. Comegys, *Lancet and Clinic*, July 6, recommends the hypodermic use of common (sulphuric) ether in sciatica. He mentions two cases, one in detail, of this affection cured by this treatment. The injections were superficial, not deep, and though causing severe pain for a time, left no ill effects. He thinks one dose sufficed for a cure in one of his cases, and believes that the remedy will be equally effective in tic douloureux.

Ipecacuanha and its Alkaloid.—The following are the conclusions of an article, by Dr. John K. Foulkrod, in the *Philadelphia Med. Times*, Aug. 31, on the Physiological Action of Ipecacuanha and its Alkaloid. His experiments were performed on cats, dogs, rabbits, mice, and frogs.

1. When locally applied, emetia produces a progressive loss of functional power of the nerves and striated muscles. If such contact be continued even but for a short time, the tissues do not recover.

When directly applied to the brain and spinal cord, no action is manifest.

2. The fall in the arterial pressure produced by an injection of emetia is caused by the direct action of the drug upon the heart,—*i. e.*, by cardiac paralysis.

3. The movements of the heart are first accelerated and afterwards diminished by the action of emetia; the diminution in the frequency of the heart's movements is due to cardiac paralysis.

4. Emetia paralyzes the cardiac inhibitory fibres of the pneumogastrics.

5. The sleep and coma produced by the drug are due to its action on the brain.

6. The emetia convulsions are spinal in their origin. The abolition of reflex activity is also due to the action on the spinal cord.

7. The vomiting produced by emetia is due to a local action on the stomach.

8. The drug produces slowing of the respiratory movements; the slowing still occurs after the pneumogastrics are cut.

9. The voluntary muscles escape unscathed in emetia-poisoning, their contractility, however, may be destroyed by painting with a solution of the alkaloid.

10. Emetia is absorbed and is eliminated unchanged by the kidneys; it is also eliminated by the gastro-intestinal mucous membrane.

11. The salivation induced by emetia is due to its local action on the nerves in the mouth.

The introduction of emetia into the stomach subcutaneously, or into a vein is followed by albuminuria.

Glucose is found in the liver after poisoning by ipecac.

12. Emetia has no direct action on the blood.

13. Emetia has no action on the pupil, either when the drug is applied locally or introduced into the general circulation.

Ammonio-Sulphate of Copper.—In the *Bull. Gen. de Thérapeutique*, Aug. 15, M. Fercol, of the Hospital Lariboisiere, gives an account of a case of epileptiform neuralgia, the attacks occurring many times a day, and nearly driving the patient to insanity. There was complete insomnia; the nerve involved was the trigeminus, and all the divisions on the left side were implicated. Aconitine and gelsemium, the two remedies most recently recommended by M. Gubler, were faithfully tried, but without effect. On the recommendation of M. Davaine, *interne*, who had seen good results follow its use in the service of M. Bourdon, the ammonio-sulphate of copper was employed, in doses at first of five centigrammes (=¾ grain) with the happiest results, complete relief being obtained in less than ten days of the treatment.

Aconite.—At the session of the Soc. de Biologie, July 6 (rep. in *Gaz. des Hopitaux*), M. Laborde presented a communication in which he attempted to show that it was from failure of the lungs and not of the heart, that death occurred in aconite poisoning. Hence, according to this mode of view artificial respiration should be considered as the best antidote to aconite.

The following are the conclusions as to the action of aconite on the nervous system, of an article by Dr. G. H Mackenzie, in the *Practitioner*, February, March, and April of the present year. He used, besides the alkaloid, tinctures in his experiments, which were performed on dogs rabbits and frogs.

1. *It induces paralysis of the peripheral sensory nerves, the sensory nerve trunks, and posterior (sensory) nerve-roots.* The evidences of this are witnessed in the progressive diminution of power of conduction of the afferent nerves, commencing at the periphery, and subsequently extending along the nerve-trunk to the nerve root To this sensory paralysis is probably due the ataxic gait, inco ordinate movements, and diminished power of locomotion in aconitized animals. There is reason to believe, however, that paralyzation is not the primary effect of the action of the drug on the sensory nerves, buf that it is preceded by a period of hyper-excitation or irritation. This feature is especially prominent in dogs, which, during the early stages of aconitism, give evidence of considerable cutaneous irritation, especially

in the posterior limbs. The primary stage appears to be of uncertain duration, and during its continuance, reflex action seems normal. It may be the only sensory effect produced (mild aconitism), and may with safety be induced therapeutically.

2. *The irritability of the motor nerves is augmented.*—That this is the case is shown from their excitability being invariably of greater intensity and longer duration in the aconitized frog, when tested with another in its normal condition. The poisoned motor nerves of a batrachian also show greater irritability than those protected by ligature from the action of the drug. The degree of motor-nervous irritability present in aconitism may be summarized as comprising three stages: (1.) Primary hyper-excitation; (2.) exhaustion, the result of the first stage; (3.) secondary hyper-excitation. These results were obtained after repeated experimentation, and are directly antagonistic to those arrived at by Achscharumow, whilst they confirm to a certain extent those of Boehm and Wartmann, with this distinction, that the latter believed the excitability of the nerves and muscles in aconitism to be perfectly normal.

3. *The cerebro-spinal axis is variously affected, the functions of the cerebrum being unimpaired, the posterior (sensory) columns of the cord being also unaffected, and the anterior (motor) columns having their irritability greatly increased.*

As already stated, the author has been unable to detect cerebral paralysis as one of the conditions obtaining in aconitism, but, on the contrary, has invariably observed such an amount of intelligence present, even during the most profound stages, as to forbid the idea that it can be present to any great extent. He is not inclined, therefore, to agree with the idea of Liegois and Hottot as to the paralyzation of a perceptive cerebral centre. The absence of paralysis of the cerebral motor centres is indicated by their ready muscular response on electrical and mechanical stimulation. From the fact that the posterior columns of the cord maintain their normal irritability after the paralyzation of the sensory nerves and sensory nerve roots, and that the degree of muscular movement induced by their excitation is about as great as by the direct irritation of the muscles themselves, it is evident that in aconitism their condition is one of non-paralyzation. The sensory paralysis present, therefore, is apparently of a non-spinal nature, both in its primary manifestation and ultimate development. The condition of the motor columns of the cord is one of hyper excitation, as demonstrated by the greatly augmented severity of the convulsions on the cord being released from the control of the inhibitory centre by section of the medulla, by their idiopathic occurrence and general nature. Intermittent convulsive spasms invariably follow the administration of the tincture in poisonous doses, whilst hypodermic injections of strong solutions of the alkaloid frequently do not markedly induce them.

The author is unable to account for the statement of Boehm and Wartmann (already quoted), that increased reflex action does not occur in frogs poisoned by aconite, when released from the control of the inhibitory centre, for he has invariably found excessive muscular action to be the result of the above operation in aconitized frogs. This effect has been so frequently ob-

tained by him as to place its existence beyond doubt, and he can only conclude that the authors above named had used spurious preparations of the alkaloid in their experiments.

The absence of motor phenomena spoken of by Wood as being especially common in dogs, can only be accounted for in the same way. In the present series of experiments it was noticed that the administration of the alkaloid was not followed by such precise effects as characterized the exhibition of carefully prepared tinctures, doubtless owing to the presence of some impurity or pseudo-alkaloid.

The following are the summarized conclusions as given as to the action of aconite on the respiration:

1. Its effect on the respiration is primary, and due to the direct action of the drug on the sensory fibres of the vagus, and the respiratory centre.

2. It induces a series of symptoms closely resembling those developed after section of the vagi.

3. It causes death partly by asphyxia, and partly by the variety of collapse spoken of by Brown-Sequard, as "characterized by a great diminution of breathing, produced by a peculiar influence on the central organs of respiration, the heart continuing to beat with more or less vigor."

Alcohol as an Anæsthetic.—The following is a short communication by Alexis Horvath (of Kieff) in the *Gaz. des Hopitaux* No. 105, Sept. 10, 1878:

Knowing the painful sensation experienced by immersion of the hand in water at a temperature of 0. Cent., I was much astonished to not experience the same sensation with the hand in alcohol, the temperature of which had been reduced to—5 Cent.

The discovery of this curious fact, which was made by me as early as 1869, led me to undertake some experiments, of which I give briefly the results.

When the finger is held in sulphuric ether or mercury at the temperature of—3 C. the same painful sensation is experienced as when it is immersed in water at zero C.

On the other hand, when we plunge the finger, even for a longer time, into alcohol or glycerine at the temperature of—5 C. we feel no pain.

A prick in the finger, while it is immersed in cold alcohol, produces no pain, and only a sensation of contact, thus proving that there is a distinction between the sense of tact and that of pain, and affording us the means of proving it. It is seen thus that pain is not the sense of tact intensified, but rather an altogether different sensation.

These experiments demonstrate that it is not merely the cold that plays a part in the production of anæsthesia, but also the liquid employed; in this regard the various liquids act differently. Inasmuch as death following severe burns is partly attributed to the intense pain which accompanies them, I was led to utilize alcohol in their treatment. I have had numerous occasions to recognize its efficaciousness; among others in a child burnt in the hand (to the second degree); the pains disappeared at once on plunging the hand into alcohol, to reappear again on its withdrawal. Moreover, I ob-

served that burns submitted to this treatment were cured in much less time than those treated otherwise.

Anæsthesia by cold (according to the methods of Richardson, etc.), though known and appreciated for a long time, is relatively little employed, solely on account of the inconveniences of the procedure. These inconveniences, joined to the above observations, have led me to attempt a work on anæsthesia by cold in grand operations, and in extensive burns located elsewhere than in the extremities.

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Veratrum Viride in Epilepsy.—E. F. Mordough, *N. Y. Med. Record*, Sept. 14th, 1878, uses hypodermic injections of large doses (10–15 minims) of the tincture of veratrum viride in combination with morphiæ to abort recurring convulsions of epilepsy. The formula of his combination is:

℞

Morphiæ Sulph. - - - gr. iss.
Tr. Veratrum Viride
Aquæ distill. - - - *aa* ℥ ss.
M.

No unpleasant effects, he states, followed the injections, the morphiæ in every instance counteracting the nauseous emetic effect of the veratrum.

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The Constituents of Coffee.—C. Binz, *Arch. f. exper. Pathol.*, IX. 31 (Abstr. in *Centralbl. f. Wissensch.*) In a former examination of the secondary quinine products, the author discovered that caffein had an entirely opposite effect from quinine as regards its action on the bodily temperature; 0.7 gramme caffein, injected subcutaneously in two places into a powerful dog, increased the temperature almost 1° C. within an hour, without the animal's exhibiting any signs of poisoning. He followed up this experiment and found, after moderate doses of caffein (0.2 gramme) hypodermically administered to a dog, a rise of 0.3° C., after larger doses (0.5 gramme), one of 1.4° C., and all without any appearance of stiffness of the muscles, or otherwise "increased innervation" or any other kind of abnormality in the animal. Larger doses likewise caused increase of temperature, but soon produced death in convulsions. Curare and artificial respiration, or the latter alone, counteracted the effect of caffein. Besides this the author found that caffein in moderate doses increased the blood pressure; the dogs which served for the kymographic experiments were only narcotized by alcohol, neither curare nor artificial respiration were employed. That this increase of blood pressure was not dependent on any action of the vagus, was proved by the fact that after previous section of both vagi, with hypodermic injection of caffein, a rise occurred which was independent of the after effects of the vagus section.

In order to learn the action of caffeon—this is the name given by Boutron and Frémy to the transient rust product of the burnt beans, which has been isolated as an ætherial oily substance by shaking up the distillate with ether—the author made a distillation of an infusion of coffee (120 grammes burnt

coffee and 150 grammes hot water) and found the same like caffein, in small doses an excitant to the brain, the heart, the respiration and the temperature.

He agrees with Hoppe-Seyler and Voit that the use of coffee infusion or of caffein itself in dietetic doses has for a result a diminution of tissue metamorphosis, the distinction being so slight that further preliminary conclusions thereon are as yet not practicable.

The author deems that any influence on the organism worthy of mention of the potash salts is not probable.

The following are a few of the recently published papers on the Therapeutics of the Nervous System and Mind:

Masing, Nerve-Stretching, *St. Petersburg. med. Wochenschrift*, No. 34, 1878; Lesser, On the Physiological Action of Arsenious Acid, *Virchow's Archiv*, LXXIII., 3, July, 1878; Fenaioli, On the Therapeutic Virtues of Bromide of Camphor, *Archivio Italieno*, September; Toselli, The Aqueous Extract of Secale Cornuta in the Treatment of Mental Diseases, *Ibid;* Husemann, The Method of Bivine in the Treatment of Strychnia Poisoning, *Deutsche med. Wochenschr.* (cont. art.); Skillern, Combination of the Alkaloids of Cinchonia with Morphia, *Phil. Med. Times*, September 28; Sinkler, Cannabis Indica in the Treatment of Epilepsy, *Ibid*; Roberts, Chloral in the Treatment of Traumatic Tetanus, *Med. and Surg. Reporter*, September 14; Mills, Massage and Swedish Movements in the Treatment of Diseases of the Nervous System, *Ibid;* Stolnikow, On the Alterations of the Cutaneous Sensibility in Healthy Men by Cold and Warm Baths, *St. Petersburg. med. Wochenschr.*, No. 26; Ringer and Murrell, On Gelsemium Sempervirens, *Lancet* (Am. Rep.), Sept.

BOOKS, ETC., RECEIVED.

Anatomy, Descriptive and Surgical. By Henry Gray, F. R. S. With an Introduction on General Anatomy and Development, by T. Holmes, M. A., Cantab. A New American from the Eighth and Enlarged English Edition. To which is added Landmarks, Medical and Surgical. By Luther Holden, F. R. C. S. Philadelphia: Henry C. Lea, 1878. Chicago: Jansen, McClurg & Co.

Stricture of the Male Urethra. Its Radical Cure. By Fessenden N. Otis, M. D. New York: G. P. Putnam's Sons, 1878. Chicago: Jansen, McClurg & Co.

Fifth Annual Report of the Secretary of the State Board of Health of the State of Michigan, for the Fiscal Year ending Sept. 30, 1877. Henry B. Baker, M. D., Secretary. Lansing, 1878.

A Clinical History of the Medical and Surgical Diseases of Women. By Robert Barnes, M. D. Second American from the Second and Revised London Edition. Philadelphia: Henry C. Lea, 1878. Chicago: Jansen, McClurg & Co.

The Antagonism of Therapeutic Agents, and What it Teaches. The essay to which was awarded the Fothergillian Gold Medal of the Medical Society of London for 1878. By J. Milner Fothergill, M. D., Edinburgh. Philadelphia: Henry C. Lea, 1878. Chicago: Jansen, McClurg & Co.

Transactions of the Medical Association of Georgia. Twenty-ninth Annual Session, Atlanta, April 17, 18 and 19, 1878. Atlanta, 1878. 279 pages.

Transactions of the Medical Association of the State of Alabama. The Report of the State Board of Health. Thirty-first Session, 1878. Montgomery, 1878. 315 pages.

Bibliotheca Medica. A Catalogue of American and British Books, Periodicals, Transactions, &c., relating to Medicine, Surgery, Dentistry, Pharmacy, Chemistry, and kindred subjects. Classified by subjects, with an index by authors. Cincinnati: Robert Clarke & Co., 1878.

Introduction a une Série de Memoires sur la Physiologie et la Pathologie des Diverses Parties de l'Encéphale. Par le Dr. Brown-Séquard.

Recherches sur l'Excitabilite des Lobes Cerébraux. Par le Dr. Brown-Sequard. (Reprints from *Archives de Physiologie Normale et Pathologique.*)

The Therapeutical Society of New York. Proceedings reprinted from the New York Medical Journal, February and April, 1878.

Restriction and Prevention of Diphtheria. Document issued by the State Board of Health of Michigan.

Involuntary Action of the Nervous System. Read before the American Dental Convention, August 17th, 1877. By J. J. Caldwell, M. D.

The Treatment of the Genito-Urinary Organs. The use of Electricity, Damiana, etc., etc. By John J. Caldwell. (Reprinted from the St. Louis Medical and Surgical Journal, June, 1878.)

Faulty Innervation as a Factor in Skin Diseases. By Edward Wigglesworth, M. D., Boston. (Reprinted from the Hospital Gazette, March 1, 1878.)

Fifteenth Annual Report of the New York Society for the Ruptured and Crippled. May, 1878.

Twelfth Report of the Board of Trustees of the Connecticut Hospital for the Insane. State of Connecticut, 1878.

State Hospital for the Insane, Danville, Penn. Report 1876–77.

A Conspectus of the Different Forms of Phthisis, Intended as an Aid to Differential Diagnosis. By Roswell Park, A. M., M. D. (Reprinted from the Chicago Medical Journal and Examiner for Sept., 1878.)

True and False Experts. By Eugene Grissom, M. D., LL. D. To which is appended a Rejoinder to Dr. Hammond's "Open Letter."

The Hystero-Neurosis. With Especial Reference to the Menstrual Hystero-Neurosis of the Stomach. By Geo. J. Engelmann, M. D. (Reprint from Vol. II. Gynecological Transactions, 1878.)

A Hystero-Psychosis. Epilepsy Dependent upon Erosions of the Cervix Uteri. By Geo. J. Engelmann, M. D. (Reprint from the St. Louis Clinical Record.)

The Etiology of Intemperance. By Chas. W. Earle, M. D., Physician to the Washingtonian Home, Chicago. Supplement to the Seventh Annual Report. 1878.

Fracture of the Femur. By C. R. Parke, M. D., Bloomington, Ill. Read before the McLean County Pathological Association. July, 1878.

Cholecystotomy for the Removal of Gall-Stones in Dropsy of the Gall Bladder. By J. Marion Sims, M. D. (Reprinted from the British Medical Journal, June 8, 1878.)

Report of the Committee on Practical Medicine of the Illinois State Medical Society. With a paper on Exophthalmic Goitre. By Chas. W. Earle, M. D. 1878.

A Paper on Intra-Laryngeal Growths. By Clinton Wagner, M. D. (Reprinted from the Ohio Medical and Surgical Journal.) Columbus, 1878.

The Prevention of Diseases, Insanity, Crime and Pauperism. A paper read before the Conference of Charities, at Cincinnati, May 22, 1878. By Nathan Allen, M. D.

Studies on the Laws of Life. Reviews of various Essays by Dr. Nathan Allen, on the Principles of Physiology, as applied to Education, Health, and Changes in Population.

THE FOLLOWING FOREIGN PERIODICALS HAVE BEEN RECEIVED SINCE OUR LAST ISSUE.

Allgemeine Zeitschrift fuer Psychiatrie und Psychisch. Gerichtl. Medicin.
Annales Médico-Psychologiques.
Archiv fuer Anatomie, Physiologie, und Wissenschaftl Medicin.
Archiv fuer Path. Anatomie, Physiologie, und fuer Klin. Medicin.
Archiv fuer die Gesammte Physiologie der Menschen und Thiere.
Australian Practitioner.
Brain.
British Medical Journal.
Bulletin Générale de Thérapeutique.
Centralblatt f. d. Med. Wissenschaften.
Centralblatt f. d. Nervenheilk., Psychiatrie, etc.
Cronica Med. Quirurg. de la Habana.
Dublin Journal of Medicine and Surgery.
Deutsche Medicinische Wochenschrift.
Edinburgh Medical Journal.
Gazetta Frenocomia di Reggio.
Gazetta Medica de Roma.
Gazette des Hopitaux.
Gazette Médicale de Bordeaux.
Glasgow Medical Journal.
Hygeia.
Hospitals Tidende.
Journal de Médicine et de Chirurgie Pratiques.
Journal of Mental Science.
Journal of Psychological Medicine.
Journal de Médecine de Bordeaux.
La France Médicale.
Lancet.
Le Progrès Médical.
Lo Sperimentale.
L'Union Médicale.
Mind.
Nordiskt Medicinskt Arkiv.
Norsk Magazin for Lagensvidenskabens.
Psychiatrisches Centralblatt.
Rivista Clinica di Bologna.
Rivista Sperimentale di Freniatria e de Medicina Legale.
Revue Médicale du Nord-Est.
Revue Mensuelle de Médicine et de Chirurgie.
Schmidt's Jahrbuecher der In- und Auslandischen Gesammten Medicin.
St. Petersburger Med. Wochenschrift.
The Practitioner.
Upsala Lakareforenings Forehandlinger.

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 Medical Review and Index
American Medical Weekly.
American Practitioner.
Archives of Dermatology.
Atlanta Medical and Surgical Journal.
Boston Medical and Surgical Journal.
Buffalo Medical Journal.
Canada Medical Record.
Canadian Journal of Medical Sciences.
Chicago Medical Journal and Examiner.
Clinic.
Cincinnati Lancet and Observer.
Detroit Lancet.
Hospital Gazette and Archives of Clinical Surgery.
Maryland Medical Journal.
Medical Brief.
Medical News and Library.
Medical Record.
Medical and Surgical Reporter.
Michigan Medical News.
Nashville Journal of Medicine.
New Remedies.
New York Medical Journal.
N. Y. Med. and Surg. Brief.
Ohio Medical and Surgical Journal.
Pacific Medical and Surgical Journal.
Proceedings of Med. Society of the County of Kings, N. Y.
Pharmacist.
Philadelphia Medical Times.
Quarterly Journal of Inebriety.
Richmond and Louisville Medical Journal.
St. Louis Medical and Surgical Journal.
St. Louis Clinical Record.
Toledo Medical Journal.
Virginia Medical Monthly.

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