

Address.**THE RELATION OF THE CLINICAL LABORATORY TO THE PRACTITIONER OF MEDICINE.**

ORATION ON MEDICINE AT THE FIFTY-EIGHTH ANNUAL SESSION OF THE AMERICAN MEDICAL ASSOCIATION, ATLANTIC CITY, JUNE 4-7, 1907.*

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THE change that has been brought about in our conception of disease and in the manner of its detection through the discovery and invention of instruments and methods of precision and the application of improved laboratory technic is one of the most striking characteristics of the marvelous advance in medicine of the last half century. To realize what this means, think for a moment how comparatively helpless we should be were we suddenly deprived of bacteriologic aid in diagnosis with its attendant specific serum reactions; of the improvements in technic of the last fifty years in the way of microscopy, stains, section-cutting, etc.; of the study of the corpuscles of the blood and the estimation of its hemoglobin; of our knowledge of the chemistry of the stomach; of the cytologie, bacteriologic, chemical and physical facts concerning normal and pathologic fluids of the body; of the means of measuring blood pressure; of the numerous electric light appliances that enable us to explore nearly all the orifices and cavities of the body; of the Röntgen ray; of the improved qualitative and quantitative chemistry of the urine and feces; of aids from physical chemistry. Think of how poverty-stricken we should feel were these aids taken from us — and they are by no means all of this class that might be enumerated — and then, and then only, do we begin in faint measure to realize the enormous value of these additions to our knowledge that have been made in the last few decades and the almost revolutionary change they have wrought in the methods of diagnosis.

These instrumental and laboratory methods, depending as they do on applied physics, chemistry, anatomy and physiology, have to do with the more exact sciences. They are, therefore, commonly regarded as having in them something of mathematical exactness, something of the element of finality. A piece of tumor viewed under the microscope, a bacillus obtained from the blood, grown, studied and seen with oil immersion lens, a chemical test of urine with the substance in question not only proven to exist but actually recovered and weighed in the balance — what are these but accurate procedures whose results must be correct? The physician, not clearly understanding all the workings of the laboratory, and dazzled by the brilliancy of the splendid results that have come from it, stung by the taunt that this practice is but empiricism, and smarting because conscious that the taunt is justified, has hailed the laboratory as his deliverer from the thralls of empiricism, irrationalism and mere experience and has looked up to it as the em-

bodiment of science, as a never-failing aid in solving the mysteries of disease. The laboratory method has, therefore, in the minds of many, acquired a transcendent importance; anamnesis and physical examination seem old-fashioned and unreliable. The laboratory is viewed as a *sanctum sanctorum* pervaded by a purer light than the wards of the hospital; and the laboratory man as a superior being, the incarnation of all that is scientific in medicine and whose word cannot be questioned. Dr. L. Emmet Holt¹ recently put it in this way:

"The men of the laboratory to-day dominate medical thought. They form a sort of oligarchy, to whose decrees those who study medicine at the bedside only must needs submit, often with a subconscious feeling of their own inferiority."

And he adds:

"I cannot myself resist the opinion that the man of the microscope and the culture tube occupies just now a place of too great importance, at least in the minds of the medical student and the younger practitioner."

Now, nothing has given a greater uplift to medical thought and practice, and especially to diagnosis, than the laboratory. Its aid is indispensable. But it has its own proper place. The earlier tendency to overvalue its work and to undervalue that done at the bedside is having the natural result in a swing back of the pendulum, and we are now hearing voices on either side of the Atlantic cautioning against the undue magnification of the laboratory at the expense of other aids to diagnosis. In our own country several writers have recently referred to the subject, and within a few weeks a large body of select medical men — the Congress of American Physicians and Surgeons — has considered the subject in a symposium, "The Relative Value of Laboratory and Clinical Methods in Diagnosis."

I have thought the matter of sufficient importance to be made the subject of this address. And particularly because I feared the papers of the symposium just referred to might not reach the bulk and sinew of the profession, viz., the general practitioner, and because I felt that too much emphasis could not be laid on certain points, I shall, even at the risk of seeming to repeat much that has just been said by others, venture to speak on "The Relation of the Clinical Laboratory to the General Practitioner."

It should be understood at the outset that I am not referring to laboratories devoted exclusively to research, such as the Pasteur Institute, Koch's or Ehrlich's laboratories, the Lister Institute of Great Britain, the Rockefeller Institute or the Memorial Institute for Infectious Diseases. I refer particularly in this discussion to what is commonly called the clinical laboratory, the laboratory that is run as a utility, as an instrument of precision, if you please, in the armamentarium of the physician, the place to which he takes to examine himself or to have examined by others his specimens of blood, sputum, stomach

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¹ Holt (L. Emmet): Medical Tendencies and Medical Ideals, The Journal A. M. A., March 9, 1907, p. 845.

contents, cultures from the throat, or fragment of tumor, hoping for aid in diagnosis and hints as to prognosis and therapy.

But let me add that unless such clinical laboratory, no matter how small it may be, has in it the essence of research its work becomes routine and nonprogressive. It is the search for the new that activates. A slow but sure death overtakes the laboratory as it does the ward in which there is no spirit of investigation.

It seems to me that certain mistaken notions obtain regarding these laboratories, which lead at times to strained relations and misunderstandings between the practitioner and the laboratory expert. These are due to faults that lie partly in the laboratory worker, partly in the physician.

First as to faults of the laboratory. And I dismiss from this criticism laboratory work that is done by incompetent men, poorly trained or careless. That some of these men pretending to do scientific work are entirely unfitted for it is, of course, true, but this is no just reflection on the laboratory method as such, any more than an incompetent practitioner of medicine should lead us to condemn the entire system of practice.

In the first place there is often a failure on the part of the laboratory to acknowledge frankly its inherent limitations. The truly great men in this line of work are ever the most conservative in the expression of opinion. There is too clearly present before their minds the possibility of error in technic, of false interpretation of that which is seen, of our poverty of knowledge concerning some of the even common laboratory reactions. These are the men who are always on the lookout for artefacts, who refuse to state positively, from microscopic examination alone, that a smear from the throat does or does not contain diphtheria bacilli, who hesitate to declare that a cellular form is the *ameba coli*, who may decline to pronounce whether a given section is from a sarcoma or an inflammatory swelling. These are the laboratory men who have the courage to say, "I don't know," or "It is impossible for any one to tell." Were there a franker willingness on the part of the laboratory expert to admit these limitations of knowledge, just as the clinician must at times acknowledge his inability to interpret physical signs, there would be in the end a distinct gain in confidence in the laboratory rather than a loss when the practitioner finds from subsequent history of the disease, as he sometimes does, that the opinion of the laboratory so positively expressed was wrong.

In the second place there is at times a failure on the part of the laboratory to realize the gravity of the duty imposed on it by the physician. The family doctor, with the medical or surgical consultant, may have exhausted their means of diagnosis. The laboratory expert is called on. On his findings depends, perhaps, the loss of the uterus in a young wife for supposed carcinoma, the sacrifice of a limb for what seems to be sarcoma but may be really gumma, the giving up of life's plans and hopes because of supposed tuberculosis of the lung or kidney, the diagnosis being

based on some doubtful tubercle bacilli in the sputum or urine. The laboratory worker is often so remote from his patient that he fails to feel that personal responsibility of which the family physician is all too conscious, and so, scarcely realizing the importance which attaches to his decision, he may make it on insufficient grounds. Were he to feel the full import of this decision, in doubtful cases he might admit more frankly to himself and to the physician his inability to reach a positive conclusion. Repeated or later examinations could then be made, thus clearing up the diagnosis. Or, in the event that uncertainty still remains, the matter could be frankly discussed with the physician and the family and the best combined decision possible under the circumstances could then be honestly reached.

Third, there is a tendency for some laboratory workers to step beyond their province and make diagnoses that are unwarranted by the facts and perhaps unasked for, and even to advance a prognosis and advice as to treatment. Strictly speaking, the laboratory's function stops when it announces its findings. The interpretation of the laboratory findings rests with the practitioner. I am aware that no sharp line can or should be drawn here, for, as I believe, these two men should work together and should both be practitioners, both be in close touch with the patient. But I criticise such things as this. I have seen, and more than once, reports from clinical laboratories reading somewhat as follows: "This urine is from a typical case of chronic interstitial nephritis," when forsooth it was not; it was from a typhoid drinking huge quantities of water passing urine of low specific gravity and of large amount, with a trace of albumin and an occasional cast. Often I have seen the positive diagnosis of pernicious or secondary anemia made from a blood smear alone sent into a laboratory. We know this is possible, but it is risky. And many times have I seen a report that said in substance, often in so many words on the basis of a Widal reaction: "This is (or is not) a case of typhoid fever." And when free hydrochloric acid has been found in the stomach contents the statement is returned: "This is not a case of carcinoma." Such reports, I need scarcely say, are wholly unwarrantable. They are as inexcusable as for the clinician to percuss the chest and listen with the stethoscope and declare positively that the case is or is not tuberculosis of the lung, without paying attention to the anamnesis or to the other physical findings, the sputum, etc. Nothing has done more to bring the laboratory into disrepute than this irrational, almost meddlesome, method of venturing a diagnosis on wholly insufficient grounds. It is hardly an excuse that the laboratory is often asked for a diagnosis by the physician who is unable to interpret the findings. The answer even then should be a guarded one and one liable to revision in the light of clinical findings. The laboratory man should stand, if you please, in the nature of a consultant, the opinion in the case, while influenced, of course, largely by the laboratory findings, should always be the combined

opinion of the practitioner and his colleague, the laboratory man.

On the part of the practitioner there is often a failure to recognize the limitations of the laboratory. As I have said, the physician too often has the notion that the laboratory methods are supreme and that it renders unnecessary and obsolete the old-time careful bedside observation. The possibility of error on the part of the patient or of the physician who provides the specimen for examination, particularly a specimen for bacteriologic examination, the personal element of carelessness, haste, hurried conclusions in the laboratory operator, faulty or balky instruments or reagents, the intrusting of important details to immature assistants,—these are some of the ways in which error may creep in and ways concerning which the practitioner untrained in the laboratory is ignorant.

But while this kind of limitation of the power of the laboratory has to be reckoned with, it is, after all, not so much a fault of the laboratory itself as of the individual worker. But misinterpretation of laboratory findings by the practitioner is common and is grave. And this is due to no fault on the part of the laboratory, but rather to the shortcomings of the methods themselves and the failure on the part of the practitioner to take cognizance of this fact. Many of the aids to diagnosis are regarded as more far reaching and comprehensive than is the reality; they are looked on as pathognomonic and as, without exception, infallible. This comes about because often earlier reports as to the value of these tests are rosy-hued and conclusions are sweeping and definitely stated. The later observations throwing doubt on the method are slower to get into print and are less likely to attract the attention of the busy practitioner than is the heavy headlined favorable report. Frequently, too, he trusts for his information to a brief conversation with a friend, to an incomplete abstract in a corner of a medical journal, or to a half understood discussion in a medical society; and he grasps at this new supposedly infallible method as he may grasp at the "patent medicine" or the newly-advertised drug that promises to cure so much. How many physicians to-day are treating cases for something else than typhoid because a single serum test, made perhaps early in the disease, has failed to show the specific reaction, overlooking the fact that this reaction is very capricious as to the time of making its appearance, and overlooking, what a few years ago would have enabled them to make a diagnosis, the course of pulse and temperature, the spleen, rose spots, tympany, facies, etc.? Or how many times has the failure to find free hydrochloric acid been regarded as conclusive evidence of carcinoma of the stomach? And who among us has not had his hard fall from placing too great reliance on the presence or absence of leucocytosis? We should remember that many of the tests are not yet clearly understood, that in a sense they are still on probation and still to be regarded as only approximately correct. It is a good thing to have appear occasionally such

papers as those of Richard Cabot² on the "Limitation of the Urinary Diagnosis", and of Emerson³ on the "Accuracy of Certain Clinical Methods," the one showing how the interpretation commonly put on certain laboratory findings is not always in accord with the facts as shown by the examination of the patient *intra vitam* or post-mortem, and the other how some of our methods, by many regarded as quantitatively accurate, are in reality subject to a rather wide margin of error.

Unfortunately, some of our laboratory work is still more or less empirical. What Bismarck said about politics, "*Die Politik kann nicht im Laboratorium gemacht werden denn sie hat überall mit dem Menschen zu thun*," might be applied, as it has been,⁴ to medicine. Medicine cannot be made in the laboratory alone, for it has to do with living man.

Numerous observations may show that there is some general law that lies back of a certain laboratory reaction, but exceptions here and there to the law as tentatively enunciated make the wiser man realize that in dealing with the complex living body the conditions are different from those found in the test tube or the incubator; that his law may after all not be so general and broad as he had supposed, and that great judgment and, in a measure, an empirical judgment must be exercised in interpreting the findings. As Dr. Barker⁵ has well said, the science of medicine is only slowly reaching the stage of development when the formation of brief laws—so-called general laws—is possible. We are still largely busy establishing and collecting facts, comparing them and arranging them according to their mutual relations, and only occasionally cautiously venturing to hint at some general law.

Then, again, too often the difference between a positive and a negative finding is not clearly kept in mind. Because no plasmodia are found in the blood on a single, even though careful, search, malaria is not necessarily excluded, though the finding of even a few proves malaria.

This fault that I have just referred to is entirely on the part of the practitioner. The laboratory has reported exactly what it has found; the physician has misinterpreted the finding. Most diseases have yet to be recognized, not by some one symptom or sign that is pathognomonic, but by a *complexus* of symptoms and signs. Nearly all the laboratory findings should be viewed in the same light as important symptoms, greater or less weight being attached to them as we learn from experience or know from their inherent character that they approach the pathognomonic. To illustrate: The finding of typhoid bacilli in the blood is of greater diagnostic value than the Widal test; the latter outweighs in importance the diazo reaction of Ehrlich, and this in turn is of more diagnostic value than the finding of a concentrated urine with perhaps a trace of albumin.

² Cabot, R. C.: Bull. Johns Hopkins Hosp., 1903, xv, No. 158.

³ Emerson: Bull. Johns Hopkins Hosp., Jan., 1903, xiv, No. 142.

⁴ Hemmeter: Science in Medicine, Jour. A. M. A., Jan. 27, 1906, p. 244.

⁵ Barker, Llewellyn F.: Methods in Medicine, Boston, 1905.

The first test is conclusive of typhoid infection — at least, according to present notions — and outweighs anamnesis and physical examination. The Widal reaction, either positive or negative, must be more carefully interpreted because not yet clearly understood, because possibly delayed in making its appearance or perhaps influenced by a typhoid long passed, and, possibly, even due to micro-organisms closely related to the typhoid bacillus; the history of the case and the physical findings must here be taken into account. The diazo reaction found in other conditions than typhoid is so far removed from the category of a specific reaction as to be viewed only as confirmatory and as outweighed by many other findings. A concentrated urine and a trace of albumin are so common in febrile conditions as to lose nearly all real diagnostic importance. The physician must have some reasonably clear conception of the relative importance of these and analogous procedures or his interpretation is liable to be faulty and misleading, even to the extent of grievous error. The laboratory finding must be fitted into the symptom-complex as a cardinal symptom perhaps, but must not be permitted to usurp the position of prime importance unless its inherent nature entitles it to such a position. The fact that its place of birth was the laboratory does not give it such rank that it may look down on the symptom or finding whose origin was the lowly ward bed.

And, lastly, while the laboratory expert shows at times a lack of appreciation of the responsibility he is assuming in expressing his opinion, the physician has no right to throw the entire responsibility of an important decision on the laboratory. The decision in a perplexing case should be a combined one, the men working as colleagues, and each man sharing in the responsibility, each entitled to his reasonable fee.

Now these faults that I have specified are remediable. The solution lies in two directions: first, in a better practical training of the undergraduate in laboratory technic and methods so that he realizes the difficulties, limitations and possibilities of the laboratory; second, in the closer relation between the laboratory expert and the clinician and, when possible, their union in the one individual. The closer the laboratory and the patient the less the liability to error; the less the scattering of responsibility the surer is the responsibility to be met.

To these ends there should be laboratories in our medical colleges for the training of undergraduates, and these should be as closely connected with the wards of the hospital and the clinic as possible. No modern hospital should be regarded as well equipped that does not make provision for a laboratory. The "show room" of the hospital should not be alone the marble wainscoted, well-lighted operating room, but as well the laboratory where material aid is obtained in telling the physician or the surgeon the nature of his patient's illness, or, perhaps after he has operated, telling the surgeon for what malady he operated.

Departmental laboratories are desirable and,

on the same principle of keeping the laboratory and the attending staff in close touch and removing the dangers coming from divided responsibility, there should be, when possible, ward laboratories, or laboratories for the individual attending man. And the time will soon come, if it be not already here, when the laboratory of the clinic or the ward must have its subdivisions into chemical, bacteriologic, anatomic, etc.

And in private practice how is it? Ideally each practitioner should have his private laboratory. He attempts it now, thus admitting the truth of the statement, with his few begrimed test-tubes, his bottle of nitric acid and his Fehling's solution and the binocular microscope under that large glass globe on the marble-topped table in front of the window in the parlor. But there are many men, and their number is rapidly increasing, who have a microscope that they use, who are equipped to do in their own offices good work in the examination of urine, blood, sputum, feces, stomach contents, pieces of pathologic tissue, etc. And as they grow too busy to do these things themselves they employ the young man fresh from the college laboratory; and, guiding and directing him, instructing him in the practice of medicine, instructed by him in the later improved methods just taught him in college, they work together, mutually benefiting, keeping up with the times, and exemplifying the union of the laboratory and the clinical side of medicine. But not every man is able to do this or feels that he can afford it. Cannot groups of men combine to employ some young man to do much of this laboratory work? These group laboratories, or what amounted to such, I have seen in small towns where a recent graduate, perhaps not on any salary, was yet the man to whom the other physicians by common consent submitted their specimens for laboratory examination, and because all did it there was no feeling of lowered dignity or of acknowledged inferiority, but perfect harmony, good feeling and mutual profit.⁶

And similarly the county medical society might have its clinical laboratory under the charge of some young man paid a salary or fees for separate examinations who conducts a laboratory to which any member might feel free to go for the help that for various reasons he cannot get in his own office. Laboratories for counties have already been established and, if not too much entangled in politics, ought to be successful, especially in the way of guarding the health of the community and in aiding in the diagnosis of charity cases.⁷ In the same way municipal laboratories have become a recognized feature of the modern city, though the carelessness of the political appointee or his incompetence, with that lack of feeling of personal responsibility that comes from the remoteness of the physician and the patient, detract oftentimes from the value of their results. Commercial clinical laboratories in our large cities are of

⁶ Cowan, J. R.: *Value of Laboratory Methods to the Country Practitioner*, *Boston Med. and Surg. Jour.*, March 8, 1906.

⁷ Westbrook: *The Public Health Laboratory*, *Am. Med.*, March 3, 1906.

great service and have a legitimate place, their value depending on the *personnel* of their staff and on how far they permit the commercial side to overshadow the scientific.

What I would contend for, therefore, is not less laboratory work but more, more for the undergraduate and more for the practitioner. So well trained should our graduate be that certain laboratory procedures of established value should be no more the exclusive property of a select few—the laboratory men—than is the clinical thermometer, the stethoscope or the obstetrical forceps; all should understand and be able to practice them. And I would plead for the closer relation between the laboratory and the patient. Clinical means bedside. Let us have the microscope, the test-tube, the culture medium, the x-ray, at the bedside, making a real clinical laboratory. And not only should there be a training of the physician in laboratory technique, but a training of the laboratory worker in observation of disease. The more familiar he is with illness the better does he understand the import of his decision, the more acutely does he feel the responsibility resting on him and the more helpful does he become as the co-worker of the practitioner. In exceptional cases exceptional laboratory knowledge or skill may be necessary, and can be obtained of some one who has made a special study of some particular branch of pathology, bacteriology, physiology, chemistry, etc., in just the same way that the practitioner in exceptional cases calls for the help of the expert oculist, neurologist, laryngologist or other specialist. Because he asks the opinion of the ophthalmologist on the appearance of a retina is no reason why he should not himself have a practical working knowledge of the use of the ophthalmoscope or why he should not have the final decision as to the nature of the patient's ailment—using the findings of the oculist as one of the cardinal symptoms and the opinion of the oculist as that of a consultant but not necessarily the final arbiter of the case.

And I cannot let this occasion pass without urging that we have care lest we underestimate the value of close observation of the patient himself. Much of this is still empirical, at least in its results, *i. e.*, its interpretation. But the stored-up and collated observations of keen-eyed and keen-brained students of disease, from the time of Hippocrates down to the present, are not to be lightly thrown aside. The laboratory must not be permitted to drive out the arts of auscultation and percussion. Because bacteriology, the microscope and the x-ray often enable us to make an early diagnosis we should not rely on these agencies to the exclusion of the older methods. Rather should we be led to greater care in examination because of the possibility of earlier recognition of disease and more accurate interpretation of slight deviations from the normal, for the laboratory has sharpened our insight into the meaning of signs and symptoms otherwise obscure. To repeat what I have several times said, it is only by the correlation of the results of the examina-

tion by all methods that the conclusion is reached as to the nature of an illness.

I might quote from various writers who have recently deplored the tendency to overestimate the importance of laboratory methods at the expense of bedside observation and who urge the more close union of the two methods. Let me cite just two: Von Noorden,⁸ well known for his laboratory research, in his inaugural address on assuming the Vienna chair left vacant by the death of Nothnagel, while speaking of the enormous advances made in the way of chemical, microscopic, bacteriologic and electric diagnosis, deplored the fact that auscultation and percussion were treated in a step-motherly fashion and that students were graduated who were only superficially trained in these methods of physical examination, the very foundation stones of practical medicine.

Strümpell,⁹ emphasizing the importance of the Röntgen ray, of bacteriology and of a study of function in diagnosis, warns against permitting the old reliable methods of physical diagnosis to be relegated to the background.

"The fine art of observation of the older physician, the painstaking attention to the minutest changes, the careful consideration of all anamnestic statements of the patients—all this may seem oftentimes trifling and unnecessary to the modern physician. But he is very much in error."

And again:

"Therefore, I think that while employing all the newer methods of examination we should not allow to fall into disuse the art of pure clinical observation, the instrument of the practitioner whose place can be taken by nothing else."

A caution along another line ought also to be given. The use of instruments of precision and of laboratory methods in diagnosis has tended, at least in some quarters, toward a slighting of the study of pathologic anatomy. Changes in the size, shape, consistency and other physical properties of organs with resulting alteration in function, have been, and of necessity must be, the basis for many of our diagnoses. No bacteriologic or blood findings can detract from the detection and identification by physical examination of a large spleen, though the interpretation of such enlargement may be impossible without the study of the blood. There should be as careful comparison of clinical findings with those of the autopsy as in the days of Skoda and Rokitsansky.¹⁰ And encouragement should be given any method of diagnosis that tends to

⁸ Von Noorden: Antrittsvorlesung. Deut. med. Wochenschr., 1906, xxxii, No. 32, p. 1750.

⁹ Strümpell: Eröffnungsrede zum xxiii Kongress für innere Medizin in München. Deut. med. Wochenschr., 1906, xxxii, No. 18, p. 726.

¹⁰ Concerning the value of the study of pathologic anatomy two European clinicians of prominence, men well versed in the theoretical and practical side of laboratory work, speak as follows: Romberg (Erfahrung und Wissenschaft in der inneren Medizin in therapie der Gegenwart, 1905) says: "For the internist, pathologic anatomy is even to-day indispensable. . . . A thorough study of pathologic anatomy is the best school for the diagnostician." Neusser (Ueber Diagnostik und Therapie in der inneren Medizin. Vienna, 1893) speaks of pathologic anatomy as even to-day the "mistress of our diagnostic art," and says a little later, concerning the value of a control of our clinical work by autopsy: "Even yet the dead speak a language, and this language brings to the intelligent physician at the bedside of his patient calm satisfaction and lends him strength and accuracy."

emphasize a study of function as altered by disease. Pathologic physiology, I believe, will play a far more important rôle in the diagnosis of the future than at present.

And then, in conclusion, we should not look on the laboratory as a short cut to a diagnosis. Sometimes it is, but often its workings are slow and cumbersome, and because of their complexity the results are not easily expressed in practical, every-day usable terms, and are, therefore, inconclusive or confusing. In other words the laboratory diagnosis is not yet one of machine-like accuracy; it is no nickel-in-the-slot affair, if you will pardon the homely comparison. The time has not yet come, let us hope it never will, when a diagnosis can be made without the exercise of brain power. We do not wish our professional work to be degraded by being "dementalized," to use Weir Mitchell's¹¹ expression. Anamnesis, physical and laboratory findings, are to be studied and compared and a result reached that not only gives a name to the disease but gives us a conception of the disease as it exists in the particular individual, with all that such a comprehensive diagnosis implies in the way of prognosis and hints as to therapy.

Original Articles.

NOTE ON THE ARTERIES OF THE CORPUS STRIATUM.*

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Two years ago while studying the gross appearances of arteriosclerosis in the arteries of the brain, we were impressed with the barren descriptions of and the lack of available information relating to the circulation of the corpus striatum.

The plan was then formed to make fresh dissections. These were made and as the result of this work revealed new and interesting material, it was decided to publish an informal report, a part of which is now presented.

The following is a representative textbook description of the circulation of the corpus striatum: The a. cerebri anterior near its base throws off several inconstant branches which go to the caput nuclei caudati; and a group comes from the a. cerebri anterior in the region of, and sometimes from, the a. communicans anterior and pursues a retrograde course to the innermost part of the substantia perforata anterior and thence into the caput nuclei caudati. The a. cerebri media near its base gives off numerous branches, some of which supply the nucleus lentiformis and pass through the capsula interna into the nucleus caudatus, while others supply the thalamus (which also receives blood from the a. cerebri posterior) and one of these is longer than the others and is called by M. Charcot the artery of hemorrhage.

¹¹ Mitchell, S. Weir: *The Early History of Instrumental Precision in Medicine*. New Haven, 1892.

*These anatomical investigations, which are being conducted by Mr. Aitken, are a prefixed portion of a study on arteriosclerosis.

This description has been condensed from the reports of M. Duret in five numbers of *Le Progrès Medical* for 1873 and in the *Archives de Physiologie*, 1874, wherein he published the account of his investigations, though he communicated them to the Société de Biologie on Dec. 7, 1872, which, by remarkable coincidence, was the same day on which Dr. Heubner of the University of Leipzig published at Berlin an account of the circulation of this region which in all essential points was identical with M. Duret's.

J. M. Charcot, in his "Localization of Cerebral and Spinal Diseases," translated for and published by the New Sydenham Society, London, 1883, has by frequent quotation absorbed the above mentioned reports of M. Duret to such an extent that one cannot readily separate them; therefore M. Charcot will be referred to with the understanding that all relating to the circulation shall be accredited to M. Duret.

The published illustrations of the circulation of the corpus striatum are quite misleading. By following M. Duret's first drawing, they have been drawn invariably in cross section, though one can readily see from his description that they should be shown in sagittal section — for although the arteries of the corpus striatum enter the substantia perforata anterior in an order that is about at right angles to the fissura longitudinalis cerebri, those predominating at once bend outward toward the exterior wall of the nucleus lentiformis, and in so doing spread out, some going frontward and some backward, thus forming a "fan," so that their collective position is at right angles to that formed at their entrance of the substantia perforata anterior and parallel to the fissura longitudinalis cerebri.

M. Charcot's method of approach in dissecting out the arteries "consists in removing successively the gray matter of the island of Reil, the subjacent white substance, the claustrum and, lastly, the external capsule. In this manner the external surface of the lenticular nucleus is exposed in its whole extent. By means of this preparation they are seen to be arranged like a fan on the surface of the gray nucleus."

This method was followed in a general way, and it was noticed that these arteries formed themselves into three groups, each supported by one larger artery, there being one artery a little more than 1 mm. in diameter, taking a backward course, one medianward and one frontward; with these are several smaller arteries which grouped themselves in a manner suggesting the slats of the "fan."

These three groups will be referred to as

Group 1, which spreads frontward.

Group 2, medianward, and

Group 3 will be that which runs backward.

The present description will be confined to Group 1.

The arteries thus exposed were presumably from the a. cerebri media and an attempt was made to classify these three divisions and see if there were any precise order in which they were thrown off from the a. cerebri media.