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ON THE RELATION BETWEEN LABORATORY WORK AND CLINICAL WORK.*

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I SHALL try first to distinguish the real differences from the fancied differences between these two classes of work; next I shall endeavor to point out what seem to me to be the proper relations between the two branches.

I. SUPPOSED DIFFERENCES.

In almost any group of physicians who discuss the subjects that grow out of hospital work and medical teaching one hears a distinction made between the so-called laboratory branches of work and the laboratory men on the one hand, and the clinical branches and clinical men on the other. In the Harvard faculty, for instance, one constantly hears such a distinction emphasized, and there come near to being two parties within the faculty divided on these lines and voting quite consistently as groups.

1. The prevailing idea, I think, among men who use these terms is that laboratory work is *exact science*, while clinical work is rough and *inexact science*; that laboratory work is done with instruments of precision, while clinical work is done with our eyes, our fingers, our ears and other still more inaccurate instruments. Doubtless this distinction holds true in many laboratories

and in much clinical work, but there is no essential reason why it should be so, and in many hospitals it is not so. For example, the measurements of weight, temperature, blood pressure, may be as exact in the wards as anywhere else. They are all clinical procedures, yet all carried out with instruments of precision. It is true further that many of the procedures ordinarily carried out in the wards could be carried out in the laboratory, and vice versa. At the Massachusetts General Hospital we have small ward laboratories in which a good deal of the work on the blood is done close at the bedside. I have seen similar work in Johns Hopkins Hospital.

A considerable amount of laboratory work concerns itself with animal experimentation, and this, I suppose, will never be carried on in the wards or be called clinical; but I do not suppose that any one wishes to limit the term "laboratory work" to animal experimentation. On the other hand, the attitude of mind and the processes of thought used in animal experimentation are frequently applied at the bedside.

2. This brings me to a second distinction sometimes present in the minds of those who divide themselves into a laboratory group and a clinical group in a college faculty or any other body which is likely to discuss these questions. I refer to the distinction between (a) relatively *passive observation* such as one may use as he watches the course of a fever, and (b) the relatively active *construction and verification of hypotheses* which (in the great majority of problems studied in the laboratory) are concerned with animal experimentation. This last type of thought and work is sometimes said to be especially characteristic of the laboratory, while the relatively passive type is identified with clinical work.

This distinction, however, like that previously referred to, seems to me fallacious. One can and constantly does make experiments on patients; in fact, every item of therapeutics is in a very real sense an experiment, and if the mind of the clinician is active he is constantly forming and testing hypotheses in this as in other parts of his work. It is true that as a group clinicians are more apt to be men of the passively observing type than are the best type of laboratory workers, but this is a distinction which has no good ground for existing and which it ought to be the endeavor of all of us who care for either branch of work to wipe out.

In the laboratory one has no work unless he sets himself some problem, thinks out some hypothesis which he wishes to verify. On the other hand, in the wards one can and many do keep busy without forming or testing out any hypotheses worthy of the name. That is only to say that for some people it is easier to go to sleep about one's work in the wards than it is in the laboratory. On the other hand, it is clear to all of us who know much of laboratories that there are considerable areas of laboratory work in which the exercise of actual intelligence approaches the zero point. I refer, of course, to the mere purely technical repetition of processes previously

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worked out by others and the verification by a long series of similar experiments of hypotheses thought out by some one else, and often of doubtful value. *The high grade type of work in the laboratory is the construction of fruitful hypotheses; their verification may involve further exercise of first-class brain power in the elaboration of methods, but in practice it often does not.*

Summing, up, then, the supposed but, I think, unreal differences between laboratory work and clinical work I should say that if we are talking of good work in both fields, we cannot differentiate them by criteria or accuracy nor by the type of mental process employed in the two fields of work. In the laboratory and at the bedside there are relatively accurate measurements carried on with instruments of precision and relatively inaccurate judgments made without instruments; in both fields we must employ the mental processes of relatively passive observation and the processes of constructing and verifying hypotheses, if we are to do good work.

II.

Turning now to the real differences which, so far as I can see, will always exist, and ought to exist between laboratory work and clinical work, we may say that they consist essentially in this: *in the wards, as in practice of medicine outside a hospital, psychical elements are everywhere present, and are often paramount, while in the laboratory they are reduced to the minimum.* Deeply embedded in every item of the evidence which we secure when we take a patient's history in the wards, are psychical factors, psychical data, psychical judgments, psychical pressure exerted on both sides of the relation. The nature of the questions asked and their effect upon the patient's mind, the skill with which we interpret his answers, paying due regard to racial elements, to local habits of speech, to differences in age and sex, and to the patient's various prejudices, preconceptions and fears, — all these elements are essentially psychical and incapable, so far as one can see at present, of being automatically registered or exactly measured. The personality of the patient and of the physician is a factor and a variable factor in every observation and in every record.

Still more obviously is this true when one turns to the treatment of disease. Whereas in the laboratory one's constant effort is to reduce the personal factor, the personal equation, to a minimum, to remove from the field of vision all elements due to one's own personality, and to make oneself so far as possible passive before the phenomena which we are trying to record, the opposite is true of every successful clinician. He can never take the passive attitude if he is to do the best for his patient. He must always be trying to push the facts in some special direction, to interfere with the course of nature, and to make himself count. In my own work I have often felt a sort of wrench as in dealing with patients I have turned from the activities of physical diagnosis with their strenuous efforts

at impartiality and passivity, to the field of active therapeutics in which one succeeds only by being partial, not impartial, by taking chances and by the presence of a contagious hopefulness.

It seems to me most important that we should recognize this as the essential difference between laboratory work and clinical work. We shall understand, then, why it is that the first-class laboratory worker or the man who in the earlier stages of his medical course shows up brilliantly in anatomy, physiology and physiological chemistry is yet often a total failure when it comes to clinical work. One may be a paragon of excellence in those fields of medicine where skill in the give and take between personalities counts for little that is, in the laboratory, and yet be unfitted for clinical work.

In clinical work a different sort of training is necessary if we are to go beyond instinct and the light of nature in the processes of human intercourse on which a considerable portion of the successful diagnosis and treatment of disease depends. Most skilful clinicians are quite unaware of the complexity and difficulty of the psychical art which they constantly practice. In the investigation of the patient's past history and present complaints it has been often pointed out that the skill required is properly to be compared to that of a cross-examining lawyer, but I think it has many other phases equally difficult and equally important. I see no good reason why training might not be provided in this branch of the art of medicine, why skilled clinicians should not become conscious and communicative with the methods and devices which they have been using all their lives for the attainment of truth and the avoidance of gross error in questioning patients.

Of course we are all familiar with the type of man who says, "After all, the history is not of much value; we never can depend upon what the patient says; we must rest our whole case upon the results of physical examination." Such a man may be sincere, but he certainly does not know what he is talking about. All the best diagnosticians that I know are agreed that in certain diseases, for example, in gallstone disease, and in most diseases of the stomach, the history is of far more value than the physical examination. This state of things is, of course, more obvious but not more real when we come to deal with diseases of the nervous system, especially with those ordinarily called functional, and with all that fall within the realm of the alienist; but I think there is hardly an important group of diseases in the whole list in which the diagnosis may not be seriously affected by the manner in which the history is obtained. Indeed, I think clinicians are agreed that there is fully as much skill demanded and used in history taking as in physical examination.

It is not quite true to state as some do that in the wards we are dealing with life and in the laboratory with dead things; of course much that we deal with in the laboratory is alive. But I think it would be true to say that the essential

difference between laboratory work and clinical work consists in this; that in the wards psychical elements are mixed up with all of the data both of observation and of treatment, while in the laboratory psychical data are reduced to the minimum and may be often measured in the form of some type of personal equation which can then be allowed for in our conclusions. Personally, I believe that this difficulty should be recognized in our medical curricula and that more training should be given in the psychical dexterity to extract a good history and to give effective treatment.

III. WHAT SHOULD BE THE PROPER RELATION BETWEEN CLINICAL WORK AND LABORATORY WORK WITHIN A HOSPITAL?

It is obvious at the outset that we are here dealing with a problem of medical specialization. All the functions now separately performed by two sets of men, one in the laboratory and the other in the wards, were originally part of the work of a single group of men, and in some cases this is still true. Some of the English clinicians, for example, make their own autopsies and follow the tissues through. Yet I suppose we must recognize that this form of specialism has come to stay and, on the whole, if properly safeguarded, it makes for good work. What I wish now especially to insist upon is that this form of specialism, like all forms of specialism, brings with it particular evils as well as particular benefits, and that if we are to enjoy these benefits and avoid those evils, we must elaborate special methods, special apparatus for *neutralizing the evils*. If I may borrow a phrase from Sherrington whose remarkable work on the integrating function of the nervous system is doubtless familiar to you all, I should say that medical specialism within the hospital calls for special *integrating factors* if we are to get the good of it and not to run foul of obvious disasters. Some of the evils at which I have hinted are doubtless familiar to all of us. Not to dwell on the more obvious and avoidable types of evil, such as the mixing up of specimens in a laboratory, I will mention three points. First: many clinicians are incapable of utilizing the data given them by the laboratory expert. Secondly, many laboratory experts confess in moments of frankness that without some knowledge of the clinical aspects of the case they cannot make the decision which is asked of them as a result of their study of tissues.

Thirdly, many of us have been troubled by a lack of correlation between the ante-mortem and post-mortem facts. The man who does the autopsy often lacks interest in the special questions which have concerned the clinician during the patient's lifetime, and is reprehensibly indifferent regarding causes of death. On the other hand, we all of us can recall clinicians who calmly allow diagnoses to stand upon the clinical record after they have been flatly contradicted by the autopsy findings.

I will now take up these points somewhat more in detail.

(a) I am often astounded at the innocent confidence which is exhibited by clinicians when dealing with laboratory data concerning the examination of the blood, stomach contents, urine and feces. I see towering structures of therapeutic effort based upon laboratory findings which will not bear the strain. The interpretation of a hemoglobin measurement, of a Wassermann reaction, of a guaiac test, by clinicians is quite different in case that clinician chances himself to have done some of the work necessary to perform these tests. Unless one knows by actual experience the sources of error, the degree of accuracy, the kind of significance which any such test involves, he is quite incapable of using it at the bedside as an aid to his clinical work.

I do not mean to suggest that the clinician should do all these tests himself in each of the cases which he undertakes to attend and to treat, but I do most emphatically assert that no clinician can use effectively as a part of his clinical judgments, any physical, chemical or microscopic test which he has not himself performed often enough to know it intimately. In no other way can he understand where he can bear his whole weight and where he must step cautiously.

Occasionally men go to the other extreme. I have known a surgeon cut down upon an enlarged liver, despite the fact that he had seen in his clinical record of that patient a typical report of leukemic blood. This wholly unnecessary operation was performed because the surgeon was skeptical—in this case over-skeptical—of the value of laboratory tests. He might have known that a leukemic blood constitution is as certain as any fact in science; as certain, that is, in the conclusions which should be drawn from it regarding prognosis and treatment. On the other hand, I have seen a clinician base both diagnosis and treatment upon a single guaiac test in the feces, made without any knowledge of the patient's diet or the condition of his rectum. I have seen a negative Wassermann reaction taken as proper ground for abandoning anti-specific treatment.

But the laboratory worker also needs clinical data. Fragments of tissue from the prostate, from the uterus or from an enlarged gland will be more satisfactorily studied by a pathologist who has won in the wards an interest and comprehension of the patient whence the tissues come.

Further, the clinician and the man who does the autopsy need to get together more, both before and after the patient's death. In many autopsies the pathological items from different parts of the body and different systems of organs are written down in the summary of the autopsy findings, quite without any suggestion of which is the most important. One may study such protocols without getting any idea as to the causes of death and the sequence of events. This sort of impartiality seems to me quite unjustifiable. It would not occur, I think, if the pathologist had seen the patient often enough during life to acquire a genuine interest in the diagnostic problem involved. How did the disease start? What is to

be regarded as "complication" and what as the "original source" of the trouble? What is the cause of certain symptoms, of mysterious pains here and there, of sudden dyspnea, of unexplained cyanosis? What was the immediate cause of death? No physician has a right to neglect these questions merely because he calls himself a pathologist. The writing down of isolated items of pathologic anatomy in a summary of an autopsy protocol is a caricature of science, not the genuine article.

On the other hand, I have known physicians whose interest in the patient's disease ceased with the patient's death. Wholly erroneous diagnoses thus remain upon the record book, uncorrected by the results of the post-mortem examination. As an extreme case, I remember a diagnosis of neurasthenia written upon a hospital record and remaining there until this day, although the autopsy showed that all the symptoms attributed to neurasthenia were, in fact, due to a cancer of the pleura of which the patient died.

IV.

I think I have said enough to recall to our minds the evils which are familiar to us all, the evils of unguarded specialism in the field of hospital work. I turn now to consider what can be done to prevent these evils.

I have said above that in a hospital, as in the human body, effectiveness depends upon the presence of some integrating factor which will prevent specialism from running wild and will remind the different parts of the system that they must be members of one body if they are to be of any use. At the Johns Hopkins Hospital we may recognize that for many years the integrating factors were the personalities of Dr. Osler and Dr. Welch. These wonderful men held together the whole system, prevented the laboratory specialist from being a mere laboratory man, and kept the clinician properly ashamed of his ignorance. I do not suppose such institutions as Dr. Osler and Dr. Welch can be made permanent in every hospital. They represent special providences, special mercies upon the part of nature which are not likely to be often repeated. Is it possible that the hospital superintendent might furnish such a factor of integration? Certainly not, so long as the present conception of his duties holds good. College presidents sometimes act as integrating factors, controlling the unrestrained specialism of the different departments, and it is perfectly conceivable that by a total change in our conception of the functions of the hospital superintendent he might come in this way to be a most essential factor in the success of the hospital's medical work. I do not say that this ought to be done; I merely say that it might be done, and if it is not done we must do something else and not drop the problem because it cannot be solved in this way.

I recently had occasion to observe how among the disintegrated units of an out-patient department, an integrating influence was introduced by the presence of a layman, a social worker, who

happened to observe that patients treated for some weeks in the nerve room as cases of nervous prostration, turned up later in the department for tuberculosis because they were so obviously tuberculous that even the nerve specialists could not longer overlook the fact. This occurrence would have been wholly unknown to both departments and would have passed unnoticed but for the integrating presence of the social worker who passed unscathed through all departments and noted what she saw.

But if we cannot depend upon any such genius as Dr. Welch, nor upon a reformed hospital superintendent, nor upon a social worker, as an integrating factor in our hospital system, something may be done by the method used in the West Medical Service at the Massachusetts General Hospital during the past year, namely, by combined ward visits of a clinician, pathologist and others, by whose views our ward problems may be elucidated. Dr. J. H. Wright, director of our pathological laboratory, has made regular weekly visits in the wards with the visiting physician and his assistants, has read the histories of puzzling cases, sometimes making an examination himself or directing the point and method for the excision of some bit of tissue which needs examination. His point of view has been of constant value to the clinician, and I have no doubt has added to his own interest in the autopsy findings. We have also obtained much of value from the visits of Dr. Boos, the pathological chemist of the hospital, whose suggestions along pharmacological and therapeutic lines have been especially valuable.

We need, however, to extend this system much further. We should have regular weekly or bi-weekly visits of a neurologist, a physiologist and a surgeon, not separately but always in conjunction with the clinician; and, of course, I am equally convinced that the physician should make rounds with the surgeon in the latter's wards, and should be a frequent visitor in the pathological laboratory, not only to learn the results of post-mortem examinations, but to familiarize himself with the technic of the physical, chemical and biological tests, the results of which he is required to interpret at the bedside.

Out of such combined ward visits is sure to come freer and more fruitful discussion of cases, better service to patients, a livelier scientific and practical interest all along the line.

SUMMARY AND CONCLUSIONS.

(1) The essential difference between clinical work and laboratory work is not in the technical methods employed, nor in the degree of their accuracy, nor in the methods of reasoning used. The essential difference consists in the predominance of psychical elements among the data used in the wards, and their elimination in the laboratory.

(2) The clinician cannot safely utilize at the bedside, the results of laboratory tests which he knows only by reading. He should familiarize himself with these tests in the laboratory, in

order to understand the sources of error and the limits of reliability.

(3) No laboratory worker should remain out of touch with the clinical aspects of the cases which furnish him his material.

(4) In the absence of any single dominating personality as an integrating factor in the hospital system, the best available substitute seems to be *combined ward visits* in which the pathologist, surgeon, physician, chemist and physiologist work together for the increase of their practical and scientific fruitfulness. The evils of specialism may be thus in a measure neutralized and its benefits enjoyed.

SHORT RÉSUMÉ OF THE HISTORY OF CÆSAREAN SECTION.*

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CÆSAREAN Section is an operation for the delivery of the child at term by means of an incision through the abdominal and uterine walls. This is supposed to be an operation of great antiquity. It is commonly assumed that it takes its name from Cæsar who is said to have come into the world in this way. Pliny mentions several other celebrities, Scipio Africanus and Manlius, who were born in this same manner. These early histories, however, are possibly mythical. But undoubtedly the practice of Cæsarean section dates back to prehistoric times.

A Roman law, ascribed to Numa Pompilius, made it obligatory upon physicians to remove the child by abdominal section in case the mother died during pregnancy.

During the Middle Ages, the Roman Church adopted this law, for by means of it they sought to rescue the child, that the rite of baptism might be bestowed upon it.

To-day it is forbidden in Austria to bury a woman, dying in the second half of pregnancy, without first performing Cæsarean section with all the care and technical skill used in operating during life, in hopes of either saving the child or at least of baptizing it.

Cæsarean section performed *just before* life was extinct has been reported as successful in saving the child, by Runge, in maternal brain tumor, by Frank in maternal general burns, by Sommerbrodt in maternal fibrosarcoma cerebri, by Hay in cerebral apoplexy, and E. P. Davis, of Philadelphia, has reported the delivery of a living child five minutes after the mother had died of eclampsia.

It having been shown that many children were saved in this way after the death of the mother, the question arose whether it would not often be right in cases of protracted labor, and where the life of the child, although not that of the mother, was threatened. This proposal met with bitter opposition and it was urged that to open the abdomen of the mother, even if her condition was hopeless,

in order to save the life of the child, was a criminal procedure, and Virchow cites an instance in which a physician was prosecuted for performing Cæsarean section on a dying woman with hope of saving the child.

The first *recorded* Cæsarean section on a living woman was performed in the year 1500 by one Jacob Nufer, a butcher, who is said to have saved the life of his wife in this way. It is further stated that he operated many times later on other women.

In 1581 François Roussett published histories of fourteen successful Cæsarean sections, six of which were said to have been performed on the same woman. The authenticity of this report is doubtful.

About this time history shows that the operation was several times successfully performed, and many operators hastened to perform it without the proper indications. We are told that for a short time in Italy it became almost as common as blood-letting. Under these conditions a reaction soon set in and the operation fell into disrepute.

Later fresh proof was brought forward to show that it might be safe if performed by skillful hands. Undoubtedly this work of the seventeenth century in Cæsarean section has been greatly exaggerated by historians.

Women have performed Cæsarean section upon themselves. Harris reports that in one hundred and nineteen years six women have successfully performed this operation upon themselves.

The operation has been accidentally performed upon women, far advanced in pregnancy, by rips from horns of cattle. Harris also reports eleven cases of women ripped open by horned animals, eight of whom escaped death. The inhabitants of some of the uncivilized countries, notably in Central Africa, are known to have performed this operation, the result being favorable to both mother and child. Palm wine was the fluid used to wash the abdomen of the patient and the hands of the operator. The abdominal wound was closed by long pins, just as it was closed by the older operator among civilized nations, and sutures were placed over the pins in the figure-of-eight fashion.

The earliest well-authenticated case of Cæsarean section comes from Germany in 1610, by Trautman. The patient lived about three weeks after operation. No doubt was entertained with regard to her recovery. However, contrary to expectation, on the twenty-fifth day after the operation she suddenly became faint and died within half an hour.

Until very recently Cæsarean section was justly regarded as one of the most hazardous operations in surgery. Up to 1873 only from 37% to 40% of the operations resulted in recovery.

Spaeth, writing about this time (1873), said: "There has not been a single case in the lying-in hospitals of Vienna in this century which has terminated favorably for the mother." Godson states, in this same year, that for over one hundred years there was not a single successful case in

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