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THE REPUTED CONFLICT BETWEEN THE LABORATORIES AND CLINICAL MEDICINE[1]

By the late Dr. C. F. HOOVER

WITHIN the past few years the idea has frequently been expressed that there is a conflict between the "laboratories" and the practice of medicine. Just how this impression has gained currency is difficult to express in a few words because any particular conflict may start from so many different causes and may come from either side. We should not wonder at this conflict, because it is only in the past thirty-five years that the laboratory has shared much in clinical diagnosis. Formerly chemistry and microscopy contributed only to post-mortem studies, and instruments of precision that can be used in clinical studies are very new. Like the art of medicine, the merits of laboratory devices depend on a directing intelligence, and no kind of contention is inspired by such a warm spring of egotism as the defense of one's intellectual self-esteem. Until very modern times, laboratory work was the avocation of practitioners, but now the laboratory methods have grown so much in importance and in variety that their employment has attained the dignity of a vocation. Naturally want of knowledge, prejudice and professional vanity may lead one side to belittle the other's importance. Nothing so readily stirs the vanity of a member of any of the learned professions as an attempt to dim the light of which he is conscious. So we have not far to seek for the reason why we should occasionally meet with champions of contending views who resent either the tyranny or obstinacy of their opponents.

Every honest practitioner of medicine welcomes all

[1] This paper was read by Dr. Hoover before the Cleveland Academy of Medicine. It is presented for publication by his wife, Katherine Fraser Hoover.

aids to diagnosis and will employ all devices that furnish knowledge or supplement his diagnostic needs. Nor should he resent the substitution of an instrument of precision for his art if the instrument is more dependable. Such a mental attitude savors of the trade union and is unworthy of a learned profession.

A serious and estimable complaint has come from practitioners who do not decline laboratory contributions but do object to the complaisant submissiveness with which practitioners yield to tests and instruments of precision and thus seek to escape their professional obligations. This tendency is perceptible not only in private practice but also in teaching hospitals where the art of medicine may be poorly cultivated and voluminous laboratory reports are exhibited as evidence of profound clinical study.

Proficiency in the art of diagnosis is gained only at the expense of patient toil and much time. An unskilled craftsman gladly takes shelter under the pretensions of precise devices that hide his poverty of resources. By this statement it is not intended to scoff at instruments of precision or to discourage the mastery of their employment. What is disapproved is submission to these devices without control by clinical logic. Instrumental devices always give truthful results, but he who employs them must know if the premises are true, and only a critical clinician can tell if the instrument is employed under the conditions that supposedly are present. A simple instance of this kind is the use of the sphygmomanometer. We compress the brachial artery by pneumatic pressure that is measured by a mercury column. What could be simpler or apparently more dependable than those factors of air pressure and the weight of a measured column of mercury? Not many years ago an eminent surgeon who was discussing the modern progress of medicine said, "Now that we measure blood pressure, what has become of the tactus eruditus that was the boast of skilful physicians in former years?" By way of retort the same question may be asked in refutation of his argument, for without control by the tactus eruditus the blood-pressure instrument is a very undependable device. I have heard very good clinicians say that palpation of the pulse is no longer of service since we can measure the maximum and minimum arterial pressure. It should rather be said that since we pretend to measure blood-pressure it is more needful that we should cultivate the art of arterial palpation to avoid the pitfalls of the instrument. And so the same argument can be amplified by saying that supposed precision of all laboratory information demands a more diligent cultivation of the bedside art to escape the tricks these devices play us. On the other hand, nothing will more enlarge, adorn and enrich the clinician's point of view than to employ laboratory devices in the solution of clinical problems.

To flourish and develop the clinician needs rich and various sources to feed his mind. No man is creative enough to invent his own requirements in sufficient quantity. For this reason no man can afford to live on his own ideas. All must shrivel who live within their own world, no matter how large it may be. The zest for curiosity that exceeds the bounds of one's own activity must be cultivated or ossification will follow. By such a statement it is not meant that all clinicians to grow must work in laboratories, for there are men who throw themselves into the midst of busy practice, gather new knowledge and grow wise in medical philosophy without entering a laboratory or mastering its methods. But there is a great difference between retaining contact with practice and becoming its victim. If in the midst of practice one spends all his time in devising methods for his patients' comfort, or what is still more likely, if he becomes the victim of practice and spends his time in tittle-tattle and serves the prejudices of his patients rather than educates them, he will in a few years dry up the sources of mental life and lose all creative desire.

There is one kind of clinician who has become submissive in the presence of laboratory reports. He resigns his art in the face of instrumental devices and does not employ the information gained from both sources. For reciprocal criticism, each point of view should rein the other to obtain the best results. I am quite sure that I have been criticized as old-fashioned and reactionary, not because I have failed to welcome information from laboratory sources, but by reason of the fact that I have always urged medical students to retain a grasp on the art of diagnosis and never submissively to accept a laboratory report without submitting it to strict clinical criticism. The kind of practice I have disparaged is to measure blood-pressure without previously analyzing the character of a pulse and estimating the pressure with the fingers, or to have an X-ray picture made of the thorax and then examine the patient. If one examines a thorax after the X-ray picture is seen, he is very prone to make his examination conform to the interpretation of the Roentgenogram. Or with a patient complaining of symptoms that are not readily explained to make a Wassermann test and on this test alone accept or reject a diagnosis of syphilis. Or in the presence of fever and suspicious cardiac symptoms, to base the presence or absence of endocarditis on the results of bacteriological blood cultures.

or an agglutination test. Such practices are fatal to the cultivation of a refined clinical art. Such methods lower the art and dignity of a diagnostician to that of a conductor of tests. It reminds one much of the methods employed in the ecclesiastical courts of a few centuries past, where tests instead of evidence were employed to determine if the accused were guilty of witchcraft.

We should be wary of all tests that can not be incorporated in the sum of evidence. A test is not evidence but a touch-piece that becomes a standard or criterion for discovering the nature of disease. Such devices are welcomed because they relieve the need of mental effort. What are we to do when tests and collected evidence conflict? Obviously the thinker will abide by the evidence and the tester by his fetish.

The same complaint about the very modern tendency of clinical men to depend too exclusively on laboratory data is heard from laboratory men as well as from reproving senior members of the clinical side. Roentgenologists say that patients are referred to them with the general request to search with the X-ray for evidence of disease. And those who conduct microscopic and chemical laboratories complain of a want of clinical information for guidance in their work.

Microscopic, chemical and bacteriological endeavors that have no burning clinical needs or clinical concepts behind them are likely to be made with the spirit of factory standardization rather than with intelligent investigation. There are, of course, certain standardized laboratory tests that can be done satisfactorily by trained technicians and should be done by them, for they are not worthy of the time of a more broadly trained professional; but to centralize all laboratory work with the idea that better and unprejudiced skill is employed in such work is an error. To be prejudiced means that judgment precedes the known facts. This must be the mental attitude in all investigations. Had Pasteur had no convictions about spontaneous generation before the facts were known, would he have become the father of the science of bacteriology? Certainly prejudice must precede discovery or there will be little heat to bestir investigation. Conviction supported by fancy and conviction guided by intelligent criticism with reverence for the truth are two contrasted statements that describe two classes in the whole mass of investigation, the one ephemeral and the other permanent.

Specialization is very apt to mean contraction of mental field, and medicine is as broad as all the collected fields of biology. It has often been argued that to compare the collective efforts of many specialists with that of one broadly trained man is like comparing a mosaic with a painted picture. The simile is unfortunate and superficial, for the mosaic is a fragmentation of a constructed picture, and to reverse the process by constructing diagnoses out of fragmental views multiplies the dangers, because each fragment is a potential source of error.

Within very recent times there has come from highly credited sources a doctrine that inasmuch as the art of diagnosis has grown out of the sum of knowledge of physics, chemistry, physiology, anatomy and pathology, therefore one who is trained in these subjects possesses the components of clinical medicine and needs only a brief exercise of this collected knowledge in the clinic to become a clinician. This is an unfortunate error, for it has led to placing immature men in posts of clinical responsibility which they can never fill with credit, not because they want intelligence but because they have not spent sufficient time under the guidance of superior clinical skill. From one university we hear the complaint that they want "professors of medicine and not guinea-pig professors." In another university I have observed a ward class spend the entire morning in listening to a recital of the failure and successes of laboratory tests and hearing nothing about the many highly instructive and illuminating physical signs that should have occupied the teacher's time and must be learned by students if they master the logic of the clinic.

Such teaching methods waste the students' time, and are about as stimulating to mental effort as counting fingers or shuffling cards.

This criticism does not carry with it a denial of the great value of many laboratory methods, but it does condemn the neglect of direct observation which is apparent in some educational centers where the laboratory studies have superseded rather than illuminated clinical methods. The advancement of the laboratory should stimulate, not inhibit, bedside acumen.

Wherever laboratory technique thrives and physical diagnosis declines there is a poor school for medical students.

The very modern fashion to exalt the laboratory and lower the efforts in clinical studies has given rise to a criticism of medical education that frankly offers to lower our educational standards by eliminating a large part of the pre-clinical laboratory work that supposedly has taken the students' interest away from the patient.

To me it seems indisputable that if a keen bedside observer engages in laboratory experiment or in the use of instruments of precision to explain or confirm his clinical conceptions, it can lead only to accuracy

in physical diagnosis and sharpened bedside perspicuity.

If instruments of precision supplant physical diagnosis, there is an obvious confession of diagnostic weakness. If science dulls our wits, then we own a witless science. This has actually occurred in some schools where teaching chairs are occupied by immature clinicians whose diagnostic training has lagged behind their laboratory skill.

INSTRUMENTS OF PRECISION

The blood-pressure apparatus is one of the most useful instruments that have been devised for diagnostic aid, and it is so simple and generally so dependable that it has not only come to be regarded as indispensable but it has opened such a quick and easy way to gain important information about the pulse that the cultivation of the tactus eruditus has fallen into neglect.

There remain in the pulse many things to be learned that only the learned touch will reveal, and should the tactus eruditus be ignored, the blood-pressure apparatus will often leave us not only ignorant but misinformed. The blood-pressure instrument is sometimes misinforming when a hypertonic brachial artery offers high resistance to the compressing cuff so that the arterial pressure as measured in the arm may be 250 mm Hg and in the leg 100 mm less—obviously a gross error that can not be detected without the tactus eruditus that in some quarters is quite abandoned.

Unless the blood-pressure as measured in the arm by the instrument conforms to our estimate by touch of the femoral artery, we should not accept its readings. Usually the two methods give the same results; when they do not, judgment should be suspended until the source of the disparity is explained.

For records the instrument is absolutely necessary, but if uncontrolled by the touch it may often lead us into great error in estimating the hydraulics of blood flow.

It should be remembered that when we estimate the blood-pressure by the finger, we perceive the concrete phenomenon of bursting tension and not the more abstract factor of hydraulic pressure. Bursting tension is equal to the arterial diameter multiplied by the pressure, and therefore, the pressure remaining the same, the bursting tension varies directly as the arterial diameter. In a large artery like the femoral, in which the diameter varies little with vascular hypertonus, we have an unvarying multiplication of the pressure, which does not obscure the changes in pressure when increased and diminished. If the pressure in the radial or brachial is doubled at the same time the diameter is halved, the bursting tension is unchanged and therefore a rise in pressure escapes manual detection. This is a very simple principle in physics that was not applied until after the blood-pressure instrument was employed, so the tactus eruditus is not superseded by the instrument but has become more learned and more essential because it saves us from instrumental error.

The best service a sphygmomanometer can render a doctor is to teach him how to estimate blood-pressure with his fingers. If it has not taught him how to do this, then he is at the mercy of a witless instrument.

The sphygmograph was originally employed to detect modifications in the anacrotus and katacrotus. The results of the instrument can not be accepted unless confirmed by the touch. The instrument is now used to portray graphically what we perceive by touch, and there is no better method for educating the touch than to confirm it by the instrument. But the tactus eruditus must always be the arbiter where there is any question of interpretation. To say which is the better method is much like arguing on which hand holds the soap or which hand washes the other. Both hands are essential to obtain the best results.

One of my students when visiting a hospital in another city was critically palpating a patient's pulse when the chief told him that in that clinic they had given up trying to gain information by pulse sensations in the face of instruments of precision, and casually observed that the ancient Chinese were the only persons who had satisfying success in interpreting the pulse with the fingers. This observation is quoted because it shows how little one may profit by the use of instruments of precision.

The electrocardiograph is one of the most refined and exquisitely working pieces of mechanism that have been devised for clinical studies. The auriculo-ventricular activation is revealed with astounding clearness, and yet it is possible in most cases to predict what the electrocardiogram will reveal if we carefully determine the relative chronicity of the auricular and ventricular beats by observing the venous and arterial pulses. There are some cases in which this time relation between activations of the auricle and ventricle can be determined only by the instrument, but the instrument should teach a careful observer so that he may learn to dispense with it. It is dangerous to employ aphorisms in the service of science, but it is approximately true that any instrument of precision is of little value that does not teach us enough to dispense with its use.

THE X-RAY DIAGNOSIS OF CARDIAC AND PULMONARY DISEASES

Any criticism from an internist on the use of the X-ray that may be interpreted as detractive would sound ungrateful. No internist who had his training prior to the Roentgenogram should fail in gratitude for its aid and for its many contributions of direct evidence in problems for whose solution we formerly depended on logical interpretation. The Roentgenogram is only a silhouette of structures quite hidden from our view, and it can not pretend to think or supply evidence for any other attributes than those which form the silhouette, and in many instances of heart and lung pathology, even the silhouette is incomplete. Like the blood-pressure instrument, the interpretation of the Roentgenogram requires intelligent criticism, and this criticism must be based on evidence that is gathered from a history and complete physical examination. The X-ray should greatly stimulate and aid us in the cultivation of the art of physical diagnosis and not supplant the art as too often occurs. The X-ray evidence is pictorial and therefore satisfying to the uncritical, but photographic evidence very often suppresses a desire for procurable evidence that may refute what seems to be an obvious proof.

A disallowable practice is to use the X-ray as a preliminary step in diagnosis. The physical examination should first be carefully made, and then if the evidence is incomplete, a Roentgenogram may be made, and if need be in face of it the physical examination repeated and revised. But if a physical examination is made with the Roentgenogram in view, it will so strongly prejudice our findings that little will be learned from the examination. If physical examinations are made to confirm the X-ray picture, little progress is made in the diagnostic art. I have visited university clinics where much effort was expended in analyzing X-ray pictures, as if there were no other method of gaining information, for not a word was said about the results of inspection, palpation, percussion or auscultation.

Such practices are very misleading to medical students, who like all humanity are disinclined to use much effort where an easier way seems to offer a solution. Let them have unrestricted liberality in the provision of X-ray pictures and little real effort will be employed to learn the difficult art of physical diagnosis. If the student is not taught that he should endeavor to predict what the X-ray will show, he will become the servant of an instrument and not its master.

The contour of the lateral and upper boundaries of the pericardial sac is accurately revealed by the X-ray, but the inferior border and the inferior aspect of the heart are hidden in the nest of the central tendon of the diaphragm. A clear conception of this view of the heart is essential for the formation of a clear idea about protusion of a distended pericardial sac in a ventral direction, and it is also essential to gain a clear mental picture of the size of the two ventricles and the right auricle.

Cardiac diagnosis is incomplete unless we can form very definite ideas about the size of the left and right ventricles and the right auricle. The Roentgenogram does not give us as clear evidence for estimating the relative volumes of the heart's chamber as can be gained by a careful physical examination. Nor can the X-ray give us as clear a conception of the relative disabilities of the two ventricles as a careful examination will reveal. When we have learned the size of a ventricle, we have made only partial progress toward an evaluation of the heart's disease. Slight enlargement of the left ventricle in many cases of arterial sclerosis will be overlooked if only X-ray pictures are used to estimate its size. The hypertrophy of the right ventricle will also be underestimated in many cases of mitral stenosis and disease of the pulmonary arteries.

The significance of cardiac enlargement in arterio-venous fistula can not be rightly interpreted from an X-ray silhouette.

The X-ray gives us no information on the ventricular diastolic systolic excursion, which by physical examination can be plainly shown to be increased in arterio-venous fistula and also in Graves' disease.

Whenever the third, that is, the antero-posterior dimension of the heart of pericardium, is needed or the inferior or ventral aspect of the heart or pericardial sac is in question, we must depend on physical examination. If we depend on the X-ray to detect early syphilitic disease of the arch of the aorta, the diagnosis will as a rule be made too late to render aid by specific treatment.

In the study of pulmonary disease we find the X-ray of very little aid and often misleading when we try to detect incipient tuberculosis, or to gauge the activity of disease known to be present, and also in all diseases of the thorax that lie behind the phrenic dome. The X-ray fails, of course, because this region is obscured by the shadow of the diaphragm. For this reason subphrenic abscesses, abscesses at the bases of the lungs and sacculated pleurisies must all be detected by physical examination.

These outstanding and frequently occurring problems in diagnosis are mentioned as those in which the X-ray gives no aid or may be very misleading, but

there are, of course, many cases of intrathoracic disease in which the X-ray is of great service, and there are also some lesions that can not be diagnosed without its aid. The criticism is not made on the use of the X-ray but against excessive dependence on its services.

In the study of cardiovascular and renal disease, we often see voluminous laboratory reports on chemistry of the blood and urine when they belie clinical evidences that are undebatable, or confirm by a laborious and expensive process what can be learned with sound economy of labor and money by a competent physical examination. It is not pleaded that such studies have no investigative value, but that they often serve to impress the uninformed laity as a clinical diagnosis based on the accuracy of mathematics and quantitative chemical estimates that supposedly should surpass clinical judgment. And it is not only the wholly uninformed laity who are thus deceived, but often the clinician is quite contented to rest his diagnosis and prognosis on deductions made wholly from such sources.

The same criticism might be prolonged into a discussion of the disappointments of clinical microscopy and clinical bacteriology. Evidence from such sources is often final and may be all we can get on which to rest a diagnosis, but such sources of information do not justify neglect of critical physical examinations. The service of the laboratories and instruments of precision are comparatively new in medicine, and some of their revelations have been so satisfying that there has been a growing tendency to supplant diagnostic effort with the employment of tests and touch-pieces. In America our proverbial enthusiasm for efficiency, standardization, factory methods and mechanistic devices has led us to weaken our cultivation of the art of diagnosis. This is only temporary; it is a transient phase of the development of medicine in America that is more accentuated than elsewhere. It would not cause any concern if this view of medicine were found only among practitioners who employ these devices to support their drooping diagnostic skill, but it is a manner of thinking that has found its way into many educational institutions where men are prepared for medical practice. It has become apparent in the programs of many of our medical societies, where attention seems to have been withdrawn from the art of medicine and devoted too largely to the cultivation of technical devices and laboratory research that has little to do with the advancement of diagnosis or treatment. A year ago, while visiting a distinguished foreign clinician who had been engaged in visiting all the centers of medical teaching in America, I was told by him that he was greatly pleased with the effort at laboratory investigation in this country, but he said, "Everywhere I went I was presented with a great lot of evidence from the laboratory, but information or interest in the physical findings on the patient were notably wanting. It seems to me that in your enthusiasm for the pursuit of laboratory evidence you have forgotten the patient."

It is not advocated that we should lessen our endeavors in the laboratory, but that we must not slacken our efforts at the bedside. Men are imitative in their methods, so that fashions in thinking gain their vogue just as do styles of millinery. And styles of thought are slowly adopted or abandoned. Much time may be lost by an errant style of thinking that will prove very costly in the intellectual development of our profession.

In recent years the way of the laboratory has provided a speedy approach to clinical chairs because the evidence for accomplished laboratory work is exhibited to so much better advantage than evidence for clinical observations. Graphic records provide evidence that has all the advantage of the pictorial method, mathematical tables provide visual evidence for acumen and diligence, and the employment of touch-pieces provides statistics that disarm all but the very critical skeptics. These devices afford a tangible method for appraising work that admits of classifications. The candidates can be ranked in the order of their productions and the method allows an approximate estimation of industry which is certainly an estimable virtue, but the evidences for the more subtle merits, intelligence, originality and truthfulness, can not be measured and weighed. Many years ago a professor of the University of Vienna said, "The method of choosing teachers for advancement is based on the avoirdupois of their publications. So that now our faculties are classified like our artillery into one-pounders, five-pounders and ten-pounders." If this mode of thinking has taken possession of our teachers and leaders in medical thought, should we wonder that lesser lights in the profession and also the laity are being led by such computable pleadings?

In conclusion, let it be understood that I do not direct criticism against laboratory methods. I do not wish to undervalue them, or wish to see our students spend less time in laboratories. *All laboratory tests and technical instruments have been devised to explain problems or to answer inquiries that have been raised by inquiring physicians.* If the results serve to slacken our cultivation of the diagnostic art instead of to sharpen our clinical insight, then the present-day medicine is in need of a renewal of the

spirit that guided some of our older clinicians who never wasted their time in laboring for things beyond their reach, but who carefully and minutely studied every perceptible evidence that lay within the ken of their senses.

I have been led to offer these criticisms on a tendency in American medicine because a common complaint from physicians is their want of laboratory facilities. Often they are heard to talk as if their professional happiness would be complete if they had at their command an ample laboratory with abundant technical service. Such complaints are due to a want of cultivation of the seeing eye and sensible fingers. The exercise of intelligent clinical observation should be to a physician the most fruitful source of intellectual satisfaction. In this kind of work he can always gather new knowledge by employing originality, and he must be original to do it. Such work is more satisfying than to repeat tests which have been devised by the originality of others. A photographer derives some pleasure from a photographic reproduction of a painted picture, but how inferior is such pleasure compared with that of the original painter. When a complaining doctor leans too heavily on laboratory support for comfort in his practice, it probably means that he has failed to strengthen the powers that lie within his grasp. When by his perspicacity a doctor makes a diagnosis of an obscure case he has a glint of the same divine fire that lit the mind of Democritus when he said, "Rather would I explain the cause of a single fact than become king of the Persians."

OBITUARY

THE SCIENTIFIC WORK OF ETIENNE S. BIELER

BEFORE me are some original papers on a variety of scientific subjects published in British, Canadian and American journals—a noteworthy contribution from the pen of a deeply mourned friend and colleague cut off in the prime of his scientific activity and production. Etienne Bieler gave early proof in the advanced mathematical classes at McGill of a singularly lucid and penetrating mind and a power of concentration which his teachers felt assured would place him in the front rank of scientific men of his time. He was marked out for a career of distinction in the field of physics, and during his final years of study in Montreal covered a consistently broader range of studies than is usual. The writer well remembers in the spring of 1915 Bieler's announcing his decision to enlist in the McGill University Company, and immediately afterwards commencing to write a difficult examination in applied mathematics, in which he took brilliant marks. Returning invalided from France in 1919, he joined the postgraduate classes with a view to proceeding to his M.Sc. degree—a part of the work being taken in electrical engineering, in order to add a certain amount of breadth to a future career in pure science.

A class problem was the source of Dr. Bieler's first published paper. At that time methods had been successfully worked out in Great Britain for detecting submarines by their disturbance of the earth's magnetic field reacting on a loop of submarine cable. The complete solution of this problem was at a later date written up and communicated to Sir William Bragg, for use of the Admiralty. It was later released for publication and published in *Proceedings* of the Royal Society.[1]

[1] October, 1921.

It is impossible in a short article to do more than select a few papers describing Bieler's most important contributions to science. Passing over a short paper on the "Measurement of Small Capacities" (1921), giving in abstract the results of a highly creditable thesis on the distribution of electric potential across the suspension insulators used in high voltage lines, I turn to a paper published in the *Transactions* of the Royal Society of London (1924), dealing with the scattering of α-particles by light atomic nuclei. It is now well known as a result of Sir Ernest Rutherford's investigations that an atom consists of a central nucleus carrying a positive electrical charge about which are rotating a greater or lesser number of electrons or ultimate electrical particles of negative electricity. On arriving at the Cavendish Laboratory as 1851 exhibitioner, Bieler was assigned the task of investigating the nature of the electric fields in the neighborhood of nuclei of light atoms such as aluminum and magnesium. The experiment consists in firing α-particles (positively charged helium atom) from radium into a thin screen of the metal to be investigated. The deviation of α-particles passing at various distances from the atomic nuclei may be detected by the scintillations which are produced in a zinc-sulphide screen. The experiments are exceedingly laborious, as scintillation counts can not be made otherwise than by the naked eye, which must be rested at frequent intervals to be able to detect the faint effects observed. The theoretical aspect of the subject is considered in a page published in the *Proceedings* of the Cambridge Philosophical Society (1923), and there is evidence of high and mature mathematical power on the part of the author. The final result shows that the nature of the nuclear field begins to deviate from the "inverse square law" at a distance of 10^{-13} centimeters. The scientific results