

## Address.

## THE IDEAL OF ACCURACY IN CLINICAL WORK: ITS IMPORTANCE, ITS LIMITATIONS.

BY RICHARD C. CABOT, M.D., BOSTON.

## I.

WHEN the history of clinical medicine comes to be written I think that the most striking characteristics of this half century will be regarded as, 1. The use of exact methods in diagnosis; 2. The use of instruments of precision; 3. The keeping of full and accurate records. We cannot be too thankful that it is so. I see three ways in which these tendencies have begun to accomplish the cleaning up of the dark places of our professional life.

(a) In the first place they have done a great deal to sweep out all such refuse as the concepts of the various *diatheses*. The "lithemic" diathesis; the "gouty" diathesis; the "rheumatic" diathesis, and similar traditions which cannot bear the tests of criticism and disappear when the search light of exact methods is applied to them. We have to thank these modern tendencies for the gradual disappearance from our vocabulary of sentences about "*bilious*" conditions, which now survive chiefly in the vocabulary of our patients, who always seem to know so much more about them than we do. So it is with the "*congestions*" of the brain, of the lungs, of the liver, of which we used to say so much and knew so little. So it is with many of the *anemias* diagnosed purely on the most deceptive evidence, facial color, and with many of the multitudinous affections classed as "*rheumatism*." These remnants of "traditional medicine" it has been the inestimable service of modern scientific methods to sweep out of our path; and I think we may say that the replacement of traditional medicine by scientific medicine is something we cannot seek too eagerly or be too grateful for, as fast and as far as it succeeds.

(b) A second inestimable service rendered by the tendencies which I am now describing has been to build up in us habits of mind that are instinctively antagonistic to habits of lying. It is not at first sight obvious how the use of instruments of precision and the insistence on precise methods in their use favors the establishment of truthfulness as a corner stone of our dealings with each other and with our patients. But although this is not obvious, I am convinced that it is true. Anyone who records his cases and makes his observations in an inaccurate or slovenly way, finds his mind filled with a kind of haze. To clear away this haze when he comes to state his results, it is almost inevitable that he should fall into the habit of drawing upon sources other than reality for his materials and for his terms. If, on the other hand, a man has made his analyses, his measurements, and his clinical records accurately and thoroughly, he learns to *lean upon fact* and to have confidence

that whatever is true will turn out to work well. Thus he gets out of the habit of improvising, modifying or embellishing his statements to suit traditional or preconceived ideas.

An enthusiastic advocate of manual training in the public schools once said in my hearing that a boy who had had a thorough course in sloyd work would never tell a lie. In this obviously exaggerated statement there is, I think, this much of truth: such a boy will find it much *harder* in the future to lie. The habit of taking our bearings straight from reality, a habit which wood work or any other form of manual training produces, makes us almost helplessly dependent on data observed for every step of our thinking and of our plans. It is as if an actor should get into the habit of being prompted continually from behind the scenes so that he never spoke his lines by rote or found the need of improvising them. The habit of improvising an embellishment cannot be acquired without practice, and the incessant use of scientific methods soon gets us fatally out of practice in the use of lies.

Quite without conscious intention or pious effort, therefore, I believe that our modern methods of medical work are steadily driving out habits of mind that make prevarication and lies of all shades possible.

(c) By sharpening the lines between what we know and what we do not know, between what we have achieved and what we have still to achieve, scientific methods of work make true progress possible. For it is only by knowing just where we are and just where our lacks are situated that we become capable of progress.

## II.

I think I must have made it clear that the tendencies which I have been describing are the very breath of life. We feel stifled and mouldy in the atmosphere of traditional medicine; we see clearly that the current of scientific medicine has made us all what we are. But we do not always see so clearly that we must *direct* that current; ceaseless vigilance on the part of the old-fashioned faculties of common sense is nowhere more in need than in the guidance and regulation of the gigantic and beneficent tendencies now at work in medicine. We are told that it is not so much the gun as the man behind the gun that makes the success of the American and Japanese navies. So in the use of instruments of precision, the brain behind the instrument, the man behind the microscope, is the achieving force. Now, obvious as this seems when stated in cold print, it is not by any means so obvious as to be always acted upon. There is a tendency which one can hardly recognize until he has felt its poignant force in his own person to get carried away by one's own methods until the methods come to replace the active brain that ought to be using them. I see in others and feel in myself the danger that we may substitute the routine use of some excellent method, say of history-taking or of physical examination, for the free play of the active, acquiring mind, the

mind capable of being surprised and of seeing what it does not expect to see. Conventionality, literalism and formalism are dangers just as formidable in medicine as they are in religion, perhaps more formidable because we are not so much upon the lookout for them, and because they creep in in so insidious and stealthy a way.

From a slightly different point of view I may describe the tendency just referred to as one of the Protean shapes assumed by our aboriginal laziness. Paradoxical though it seems, I have no doubt that the scientific method is at times nothing more than a disguise assumed by our ineradicable laziness. I know no form of labor which the average man shuns more instinctively and more constantly than the labor of thinking or of fresh observation. But the doctor is human and capable of temptation all the more readily when the tempter assumes the subtle and baffling guise of the "scientific method." I know a doctor who never forgets his stethoscope, his blood counter or his percussion hammer when he starts on his rounds in the morning, yet not infrequently he is so absent-minded as to leave at home one all-important instrument — his brain.

I suppose it is impossible for us to over-estimate the debt which we owe to Germany in the field of scientific medicine. For years we have been fed with the results of German industry and ingenuity until it has become habitual for us to stretch out our necks in that direction like fledglings in the nest. But we have also acquired the habit (and here is my point) of *bolting our food*, of gulping down in true American fashion the pabulum furnished us by our Teutonic brethren, forgetting that assimilation is a necessary prerequisite for nutrition. Now this habit of bolting down, unmodified, the nutritious gifts of our Teutonic brethren has given us several forms of scientific indigestion. One of these mental dyspepsias results in the idea that absolute accuracy should be sought for at all times and in all places. Now absolute accuracy is an ideal, not simply unobtainable but self-contradictory, as I think I can show you by a few examples. It is, of course, a commonplace that accuracy is always relative, — relative in the first place to the limitations of the instruments which we employ. "Accurate within the limits of error of the instrument employed" is the most that we can ever say of our observations, and that these instruments have limits of *error*, and very wide ones, is not always sufficiently realized. Neither is it realized that our accuracy is relative, not only to the limitations of the instrument employed, but also and chiefly to our *purpose*. What is accurate enough for one purpose is not accurate for a second and is too accurate for a third. I think it is safe to say that no physician in active practice can make physical examinations which are accurate even up to his own standard of possible accuracy. Such an examination would consume half a day, at least, and even then many points would be left uninvestigated because they seemed relatively unimportant. I think it is well for us to realize that this is always the case, and

that in consequence it is always our duty to *direct our accuracy*, like a search light, where it can do most good. We must be inaccurate somewhere. The wise physician is he who knows well how to decide, where and when to be accurate, where and when to get along without accuracy.

Ludicrous examples confront us now and then, of misplaced accuracy, of misdirected exactness. A physician of my acquaintance was consulted not long ago by a lady for the relief of dyspepsia. The doctor made a careful estimation of the size, position, motility and secretive activity of her stomach, analysed the gastric juices by careful quantitative methods, examined the urine, and rendered a report containing quantitative measurements of the different solids as well as the ordinary chemical and microscopical tests, counted the red and white corpuscles, measured the hemoglobin, made a differential count of the leucocytes, and examined minutely the condition of the thoracic and abdominal organs. From the indications obtained by these examinations he prescribed for the patient with great care, but as it turned out, without success, for the patient's nausea and other gastric symptoms continued unrelieved. Some months later it was learned that she was pregnant.

Now what I want to bring out by this example is this, the fact that although the pregnancy could not have been discovered at the time the doctor saw his patient by any method of physical examination, yet it might have been discovered, or at any rate strongly suspected, had he directed as much energy and accuracy into the taking of his history as he put upon the physical examination. He used plenty of accuracy but he used it in the wrong place. He was too accurate, that is, uselessly accurate in some respects and correspondingly inaccurate in other and more important respects. Now the *relative importance* of the different aspects of a case is something not to be learned by becoming expert in any or all of the known methods of investigating disease. It is and must remain the work of common sense.

Let me enumerate briefly a few other examples of what I regard as misdirected accuracy, or perhaps I had better say disproportionate accuracy: 1. The estimation of the urinary solids by quantitative methods is usually a pure waste of time, not because it does not tell us anything, but what it tells us is altogether unimportant for the diagnosis, prognosis and treatment of disease. The information that it gives us is information that we cannot make any use of; inferences drawn from it may be misleading and actually harmful. Take, for example, the quantitative estimation of urea, a solid which I suppose we measure more frequently than any other of the urinary constituents, under the quite mistaken impression that we are securing thereby knowledge of the secretory power of the kidney. Now we all know, if we stop and think of it, that the output of urea represents not merely the functional power of the kidney but the resultant of a complicated group of forces. The urea output depends not merely on the power of the kidney but upon the

amount and kind of nutrition furnished the patient and upon the success of his organs in dealing effectively with what is given him. To interpret an urea estimate we need a knowledge of the patient's total nitrogenous metabolism. In most cases in which I have known measurements of urea to be recorded there has been no accurate knowledge of the food taken into the body and no possibility of allowing for the modifications produced by such symptoms as vomiting, diarrhea, sleeplessness, confinement in bed and psychological disturbances; yet in measuring urea we are measuring something dependent upon all of these influences as well as upon the functional power of the kidney. It would be hardly more absurd if we attempted to measure the efficiency of a street transit system by the number of patrons deposited every night at the theaters. It is true that this figure would depend in part upon the efficiency of the transportation system, but to neglect altogether such factors as the degree of attraction of the plays running at any given time at the different theaters, the clemency or inclemency of the weather, and the state of the public purse, would be no more absurd than the attempt to measure the functional power of the kidney by the estimation of the urea alone.

It is quite true that if we had a definite and accurate knowledge of the patient's metabolism we could learn a good deal from the output of urea, but in the vast majority of cases it is not possible for us to get any such knowledge of his metabolism, and without it our urea estimations appear to me a pure waste of time. It may be said that we may gain from them, at any rate, a rough estimate of the functional power of the kidney after making allowances for all the sources of error above alluded to. In a measure this is true but it is also true that we can gain the same information without taking the time to estimate the urea at all, simply by measuring the twenty-four-hour amount of urine and the specific gravity. From these two simple facts we can glean all the information that is afforded us by urea estimations except in the very rare cases where it is possible for us to spend the time necessary to study his total metabolism.

A similar mistake in the distribution of our efforts at accuracy occurs in many of the quantitative estimations of hydrochloric acid in gastric contents which give us the appearance of great scientific force without the fact. Again and again I have known physicians to estimate most carefully the percentages of free or combined hydrochloric acid and of the acid salts, while they had neglected to get even an approximate idea of the two most important facts about the stomach in disease: namely, its size and its motor power. The measurement of the gross total of residue in the fasting stomach and of the size of the organ after distention with air or water, give us facts of far greater value than the more accurate quantitative estimations of acid which occupy the foreground in most accounts of the stomach functions. With Conge red paper an estimate accurate enough for most clinical purposes can be

obtained in a few minutes; and the time thus saved can be put into more accurate work in other directions.

In blood examinations far too much time is spent in making counts of red corpuscles. Not in more than one case in twenty-five in my own practice do I find it necessary to make such estimation. If the hemoglobin is normal and if there are no other obvious evidences of anemia in the general examination of the patient or in the examination of the stained film-specimen of blood, there is no need for consuming the time and energy necessary for a red count. The essential parts of blood examination in the vast majority of cases are the total count of white corpuscles and the examination of the stained film. This is the direction in which we want to put our accuracy.

I think it is unnecessary further to multiply examples. Anyone can think of similar cases as soon as his attention is turned to the matter.

I wish now to turn to quite a different aspect of my subject, yet one which is a branch of the same difficulty; I mean our failure to direct the current of the scientific tendencies of our time. It is a common, but I believe a very fallacious, belief among physicians, that "laboratory work" is a term as wide as "accurate work," and that clinical work must needs be comparatively inaccurate. Let us glance for a moment at the derivative meaning of these words. The laboratory is simply a place for work. The word means nothing more. Clinical means simply at the bedside. Now, my contention is that for most of us the best place for work is at the bedside. That is the place where the most essential information can be acquired, both for the benefit of science and for the benefit of the patient. There is no more pernicious fallacy extant than that which supposes that there is a necessary division between the "laboratory man" and the "clinical man" or between the "laboratory work" and "clinical work." This is a distinction which Dr. Deaver of Philadelphia has done much to harden. He is never tired of insisting that the point of view of the clinician in diagnosis is superior to that of the laboratory worker. But why in Heaven's name should we have to choose either one? Why should not each one of us make himself master of all the facts necessary for the diagnosis of his case, both the facts obtained by what we ordinarily call the clinical examination of the patient and those obtained with the help of the microscope and of chemical reagents? If one had to choose (for example, in the diagnosis of appendicitis) between knowing only the facts obtained by microscopical and chemical analysis, and knowing only the facts obtainable at the bedside without the help of these agencies, why, I suppose any of us would choose the bedside as the better standpoint for observation. But my point is that we never ought to be forced into making any such choice. Not either one without the other, but both. That is what we want. No one is so foolish as to depend wholly upon a single method of examination in diagnosis, whether that

method be either a measuring of temperature, which we ordinarily call a "clinical fact," or the counting of leucocytes which we ordinarily call a "laboratory fact." All such facts are of value only when considered in connection with all other available evidence. They are like single letters of the alphabet which in their isolation are almost meaningless, yet when grouped into words may be most significant. A diagnosis in most cases should rest upon a group of data which together spell out a word.

The harm done by the attempt to separate our examination of the patient's functions into two sharply differentiated portions and to assign one portion to the individual known as the laboratory man and the other portion to some one called a "clinician," — the harm, I say, done by this attempt consists in part in a loss of essential facts in the transfer from one man to another. For such data are not readily transferable like coin without loss of value. Few can interpret the results of blood examination, or urinary examination, unless they are constantly making such examinations themselves. Indeed the attempt so to interpret them is almost as hopeless as the attempt to convey satisfactorily to another what one feels in palpation of the abdomen. Laboratory facts are *personal* facts as much as the results of palpation, and they are almost as difficult to convey to a second person. Moreover, our interpretation of the crude data obtained by our senses is apt to be a very faulty one if we attempt, as a so-called laboratory man often has to do, to make this interpretation wholly uninfluenced by the clinical aspects of the case. Those best trained in microscopical and chemical analysis are coming more and more to feel unwilling to hand over to another man any hard and fast conclusions based upon the isolated facts in his position. More and more we are finding that the men who examine scrapings or fragments of a tumor want to see the case in the wards and get possession of all the facts ascertainable, just as the clinician is more and more unwilling to accept a report from the laboratory without seeing the specimens himself.

Dr. Welch has pointed out in a most timely way, in a recent discussion in the Johns Hopkins Hospital Bulletin, how unwise it is to attempt to construct clinical histories from post-mortem evidence. But the evidence obtainable by what we still insist calling the "laboratory man" is usually as one-sided as the post-mortem evidence just alluded to. It needs to be filled out and corrected by facts obtainable only at the bedside.

Summing up what I have said, it seems to me, (1) that we need to direct the great current of scientific medicine, first by observing a due proportion in the amount of time and the degree of accuracy assigned to the different portions of our examination of a patient, and (2) that we want to rub out as fast and as far as possible the distinction between laboratory diagnosis and clinical diagnosis.

## Original Articles.

### METATARSO-TARSAL VALGUS OR HUMPED FOOT AND ITS RELATION TO BOOTS.\*

BY E. H. BRADFORD, M.D., BOSTON.

It is claimed with probable truth that American shoes are the best in the world. American feet, however, are being deformed to a lamentable degree by foot wear. Foot deformities may be said to be prevalent because of the excellence and cheapness of manufactured boots. The intelligence and energy which have been concentrated in the manufacture of shoes have attracted a large amount of capital in the development of shoe factories. The market is a large one and the interests of capital demand large sales to compensate for financial outlay. The result is that great energy is devoted not only to the excellence of

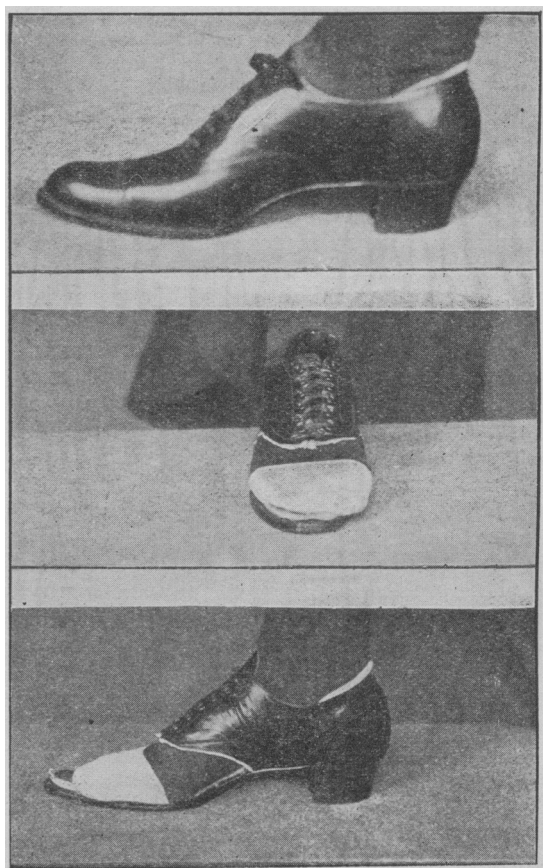


FIG. 1. — Showing shoe constriction of front of foot, with normal foot in shoe before and after removal of upper.

the output but to the development of a selling style. But as there is a profound ignorance as to the normal shape and functions of the foot, sales of boots depend to a great extent on fashion, and fashion has always prompted the wearing of foot wear which cramps the foot. Deformities of the foot from shoes are possible from the fact that the human foot is capable of a great deal of compression if compression is exerted without causing much pain. Although sensitive in cer-

\* Read at a meeting of the Boston Society for Medical Improvement, April 11, 1904.