DIAGNOSIS PER EXCLUSIONEM
IN ORDINE: GENERAL AND
PSYCHIATRIC REMARKS

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IS there any excuse for a communication on the general theory of clinical diagnosis? Should not one in the present concrete state of medicine apologize for any generalized or theoretical discussion of a topic which has long since passed into the realm of the dead? If medical schools rarely nowadays, and only as a curiosum, deal with the sometime familiar disciplines of so-called medical Propedeutics and the Logic of Medicine, is it not for the very good reason that medicine no longer requires any propedeutics or any special overhauling of its logic?

I have two possibly tenuous excuses for the attempted resurrection, namely, first, that it is a canon of mental hygiene to get in imagination as far away as possible from our terrible environment; and secondly, that the realm of dead notions somewhat well befits a pathologist who has gone into psychiatry.

Speaking to a group of eminent clinicians, I need not insist that I myself make no claims to being a diagnostician of the first water. But just as more or less valuable books on elocution are written by persons without color or pretense of oratory, so perhaps one who is not an especially good diagnostician may descant humbly on the matter. Some five years ago, when I became director of the newly founded Psychopathic Hospital in Boston, it became for the first time my duty to deal concretely with a large series of clinical diagnoses. To be sure, before that time, and in fact, in the year 1909, I had been charged with the task of supervising the clinical and pathologic research work of the state institutions for the insane of Massachusetts, and had already at that time begun to agitate upon the manifest errors in diagnosis which appeared in a small but definite minority of cases of mental disease in these institutions, as tested by the stern criteria of the autopsy table. I took an especial interest in errors of diagnosis in that mental disease entity, about which we know the most, namely, general paresis,4 and found that one of the most active and thoroughly educated state hospital staffs in the country, namely, that of the Danvers State Hospital,2 was none the less able to make an error of anywhere from 5 to 15 per cent, according to the fineness of the criteria adopted in the disease general paresis. After all, this work and sundry other articles by my associates and myself3,4,5,6 remained quite upon the theoretical or unapplied level.

Suddenly, in 1912, I was translated to a region in which exceedingly rapid, not to say provisional, or not to say "snap" diagnoses had to be rendered in what almost any one would concede was the most difficult field of clinical diagnosis, namely, the diagnosis of mental disease. When it is remembered that the task at the Psychopathic Hospital is largely the temporary care of acute, lucipient, and curable cases together with a consideration of all the most du-

fious cases in the community, it will be seen that the problem of a reasonably accurate diagnosis in these cases is not at all easy. It is necessary to remember, also, that our task is not that of the alienist so much as that of the psychiatrist, employing these terms in the differential sense recently advocated in a paper on the nomenclature of mental hygiene. It is not the task of the Psychopathic Hospital to determine, except in a minority of cases, the committability or certifiability of its patients. Of the first 5,000 discharges from the Psychopathic Hospital, over 1,500 were discharged as "not insane," a term which signifies, not that the patients had recovered, but that they were regarded as not having been insane during their stay at the hospital (the recoveries are over and above the 1,700 "not insane"). In short, the task here is one of securing a basis for treatment and counsel for cases that lie often entirely without the purview of governmental control as mechanized by the probate courts. A large part of our problem is medical in the strict sense and not medico-legal.

I may be pardoned for insisting upon this peculiar and almost unique feature of the Psychopathic Hospital work in Boston, because not even my colleagues in the specialty of mental diseases have readily grasped the point. Alienists are a type of physician specializing in medico-legal practice, and their task is to determine alienation; that is, the theoretical or practical committability of their cases in the interest of the public welfare. Psychiatrists are a type of physician specializing in mental diseases on a much broader basis, namely, the basis of finer diagnoses, involving groups of mental disease and defect, and the psychopathic trends which fall short of defined psychoses and even the quick-sand topics of eccentricity, crankiness, and oddity. Alienists,—a medico-legal field,—is pretty sharply distinguished from practical psychiatry,—a medical field. A good psychiatrist ought to be able to accommodate himself to governmental groups well enough to be a good alienist; but, on the other hand, many a good alienist who has dealt for years with the category of committability only, is unable quite to see the point of a diagnosis of "Psychotic, not insane," or of "Psychopathic, not insane." The question of nomenclature is subordinate; but aside from the question of nomenclature, if the practitioner can not grasp the distinction between insanity in the medico-legal sense and mental disease in the medical sense, he is a man unable to keep pace with the modern progress in psychiatry.

What we may ask, has the question of psychopathic hospital diagnosis of psychoses that fall short of insanity to do with the topic assigned for this communication? How does this work fit into a discussion of the general theory of clinical diagnosis? Psychiatrists have always taken a good deal of interest in classification. I will remind you only of the eminent instance of Pinel, who in 1809, published a nosographia methodica, which is one of the best-known of the old nosologies, a very good general view of which is given in Hosack's publication in 1819. I am afraid, however, that most modern practitioners, and especially in America, would be apt to deride it as out of date, if not fundamentally beside the point of these adventures in classification. Of course these scoffers have the entire history of science against them in their scoffing. But the modern practical fellow, especially the pragmatic American, little reeks such a
small matter as consistency with the general history of science. Nor will I attempt to defend the various excesses of nomenclature which my psychiatric superiors, both in point of time and in point of capacity, have sometimes perpetrated. It is another queer way which Americans (and I suspect other moderns) have, namely, to confuse the topic of nomenclature with the topic of classification. Classifying for these persons means assigning a name. To be sure, a slight re-reading of that apostle of clarity, John Stuart Mill, would perhaps set these confused minds aright. Still I suppose it is too much to ask of them to re-read even a few chapters of Mill's System of Logic.

What, after all, constitutes diagnosis? I do not know whether the Harvard Medical School curriculum is less logical than those of other medical schools that the American Medical Association terms Class I, but it is very hard to get from a Harvard Medical School student at any stage, even immediately after securing his M.D., an adequate definition of the terms diagnosis, symptom, and the like. Yet would any one of us suppose that these terms can be safely dispensed with in practice or in theory? And even if you could somehow show that a pragmatic view of medicine might throw overboard altogether the distinction between disease entity and disease symptom, nevertheless the books are full of these terms used in a variety of ways, and I am sure it ought to be the duty of every medical course to offer some pabulum (whether pedantic or by way of appendix) on these matters, which were doubtless comprised in the forgotten and discounted field of so-called Medical Logic.

Many a favorite textbook in the practice of medicine fails to make any point whatever of this distinction, and plumps one forthwith into a discussion,—e.g., of typhoid fever,—jauntily assuming that the student somehow knows by intuition the meanings of the terms, definition, entity, diagnosis, symptom, symptom-complex, syndrome, and the like. Perhaps, however, it is better to leave out general remarks in this direction than to put in some which are found in certain other textbooks that have run into many editions. Strumpell makes no special point of the matter but insists in a preliminary note upon the importance of diagnosis, of clinical observation, and of keeping the common courses of all diseases in mind. Here, perhaps, is the most commonplace remark nowadays about diagnosis: diagnosis, namely, is constituted by observation, according to these authors. In fact, there is one textbook (Wilson) that summarily states upon its title page, that the whole art of medicine is in observation. On a later page, it is insisted not quite so broadly, that "It is certainly true that the art of diagnosis is an art of observation," and again, "The facts of pathology and semiology and the natural history of the diseases constitute the basis of diagnosis." This latter statement seems almost equivalent to the statement that the facts of disease are the basis of diagnosis, which may be in one sense true, but is so little helpful that it might well be left unsaid. Another recent author (Hall) states that "The art of diagnosis consists, then, in gathering all accessible data and arriving at that conclusion which seems the most reasonable as the probable cause of the trouble." This definition evidently goes beyond the former, namely, "Diagnosis is constituted by observation," by considering that the reason is necessary over and above observation. Put in these terms, of course no one of us would care to say that diagnosis consists in gathering
data. Put boldly, it is certainly untrue to say that the collecting of facts constitutes the art of distinguishing amongst these facts. Yet the whole trend of recent work runs in the direction of insisting upon observation at the expense of reason. And, whether or no we teachers are entirely clear in our own minds as to the distinction between diagnosis and the mere recognition of fact as such, certainly our students are not always clear. A student will give the "diagnosis" fever or rose spot, when he would scoff at making the "diagnosis," It is raining, or The engine is skipping. If this be diagnosis, make the most of it! Of course, diagnosis is clearly a form of recognition, a form of knowledge, but there seems to be no warrant in raising every elementary form of awareness, kenning, or recognition to the level of diagnosis, which term bears within itself a manifest prefix, suggesting distinction, differentiation, sundering. All my own medical teachers, and I do not doubt the majority of those of my listeners, insisted upon the value of accurate observation. Few of them insisted upon the importance of correct reasoning. Most of them, I take it, partly as a matter of self-defense as they were growing older, were quite sure that maturity of judgment was a thing not readily attained. It never seemed to occur to these teachers that, as the powers of observation were on the wane, the powers of right judgment were to increase. In brief, these teachers, without ever once using the term logic, and hardly ever the term reason, managed to get into their homilies for students the idea that there was something besides observation, namely, what they called common sense or knowledge of fundamentals, or keeping essential cases in mind, or ripeness of clinical experience, and the like.

Though I believe some course in propedeutics might well be constructed to contain the main principles of the logic of medicine, yet that is not the point of the present argument. Basing my notion upon the practical experience of the last few years, in which I have had contact with clinical diagnosis, always with the autopsy criterion more or less consciously in mind, I want to argue for a more extensive application of the principle of logic in practical diagnosis, and especially the practical diagnosis of the tyro. Perhaps you will think my argument is nothing more or less than a rehabilitation of the diagnosis by exclusion of the older authors. To some extent, the argument does follow that line, though the arguments have been derived from recent practical experience in off-hand and professional diagnosis, and from certain logical considerations. That older argument is found very well expressed in DaCosta's book so long ago as 1864.13 DaCosta's book, according to an enthusiastic Philadelphian (Wilson14) marked an epoch in the progress of internal medicine; and surely many of DaCosta's distinctions are found in several textbooks built upon the lines of his book for many decades after 1864. DaCosta distinguished direct and indirect diagnosis. Indirect diagnosis by exclusion, according to DaCosta "is not on ordinary occasions much employed nor indeed is it to be recommended" (italics mine). It is this dictum of DaCosta which I would think ought to be combated in any summing up of present-day processes in diagnosis.

The reasons why DaCosta felt that diagnosis by exclusion was not to be recommended were assigned by him as follows: First, "to prove what a thing is by proving all that it is not, is a very tedious process." Secondly, DaCosta went on to say, "it is difficult to think of all the possibilities." And thirdly, he
said, "pathology is in an imperfect state." Now, as for the latter two reasons, it is clear that the process of direct diagnosis is equally subject to the pitfalls of imperfect pathology, and equally subject to the charge that it is hard to think of all the possibilities. I myself can only believe that these two latter so-called drawbacks are not drawbacks worse for diagnosis by exclusion than for any other form of diagnosis. Accordingly, I believe that DaCosta's objection to diagnosis by exclusion reduces to tediousness; but should we hesitate to say to our students that it is better to be tedious than wrong? The technic of DaCosta's process may be elicited in his own words:

"To cite a case in illustration. A person consults us for a cough brought on by exposure. He has been sick for four or five days, having been previously in good health. We notice, on examining him, that his breathing is hurried, and that he has fever; the lower portion of one side of the chest is dull on percussion, and the respiration there is wanting; the action and sounds of the heart are normal. The facts point to the lung or its covering as the seat of the disorder. We know, furthermore, from the history and the febrile symptoms, that we have to deal with an acute affection. What are the acute pulmonary affections? Acute bronchitis; acute phthisis; acute pleurisy; acute pneumonia. In all there is fever, cough, and impaired breathing. Is it acute pneumonia? No; for, notwithstanding there is in this complaint, in addition to the general symptoms mentioned, dulness on percussion, such as we have here, the dullness is associated with a blowing respiration; whereas in the case before us no respiration is heard. Let us look at the sputum, and see if it is tenacious and rusty colored. It is not; it is thin and frothy. Moreover, the breathing, although hurried, is, when counted, found to be less hurried than it is in inflammation of the lungs. But acute pleurisy may explain all the signs. The patient, too, when questioned, states that he had at the onset a sharp pain in his side; and this we are aware takes place in pleurisy. The vocal vibrations, likewise, are noticed to be absent on one side of the chest, which, when carefully measured, is evidently enlarged. This corresponds in all points with what happens in pleurisy in the stage of effusion. The disease is, therefore, acute pleurisy in the stage of effusion. We finish the diagnosis by ascertaining the existence or nonexistence of other maladies, and by taking note of the severity of the complaint; that it has occurred in a young and robust person of good habits; and that the symptomatic fever is very active."

But what is a direct diagnosis,—one not made by exclusion? According to Wilson, "A direct diagnosis is made when the history of the case and the clinical phenomena are sufficient to warrant a positive conclusion." Wilson goes on to say that a diagnosis by exclusion differs from differential diagnosis only in scope. Strange as this phrase may sound, it strikes me that there is a good deal of truth concealed in it. I feel that, so far from admitting with DaCosta, that diagnosis by exclusion is not much employed or to be recommended, practically every diagnosis is by exclusion, whether consciously or unconsciously.

Musser remarks concerning diagnosis that "the data collected are sufficient to warrant a positive conclusion. The direct method is scientific, rational, and most practical. It is a practice of purely inductive reasoning." I am not sure whether it would not be sounder to say that it is a process of purely deductive reasoning, but modern logicians have conclusively enough proved that practically all reasoning is both inductive and deductive, so that it would not pay to quibble over the claim that direct diagnosis is "a process of purely inductive reasoning." As to the method of diagnosis by exclusion, Musser remarks, "Thus, a symptom group may suggest several diseases; each affection must be passed in review and excluded until one is found which closely corresponds to the data of the case under consideration." In short, it would seem that diagnosis by
exclusion is an effect of the force majeure of complex and obscure data. The battle is easily won when it is a question of perceiving a symptom and suspecting a disease, whereupon all "the data collected are sufficient to warrant a positive conclusion."

Now, possibly it is the complex and obscure nature of mental disease which has caused me to feel that diagnosis by exclusion is a method that requires rehabilitation in the minds of teachers and in the pages of textbooks. In medicine and biology at large in the middle of the nineteenth century, it was the fashion to denounce speculation and extol observation. So many of the medical and biological sciences depended upon the microscope and the visual process, that nothing was more natural than to see great virtue in observation as compared with the vagaries of the Naturphilosophie. We are now so far beyond the range of that old Naturphilosophie (unless the idea of Pan-Germanism may be regarded as a remote result thereof), that it should be quite safe to uphold the reason as entitled to a seat beside observation in the process of diagnosis. Applied logic accords to observation its full worth. Secondly, a proper value is assigned to what may be inaccurately summed up in the phrase comparative method. It is the comparative method probably that is being used in diagnosis for the most part at the present day. That is, one picks a symptom, suspects a disease, constructs a supposititious diagnosis, and proceeds to compare it with all the data of memory or reference books. It is all one, whether you pick one symptom e.g., the so-called presenting symptom popularized by Richard Cabot—or assemble a symptom group; the process remains the same, that of matching the data of the case with some supposititious diagnosis. Now in the field of mental disease probably every one of you has had the experience of matching virtually all the symptoms of a case with the majority of those given in the textbooks for several different mental diseases. In short, the comparative or matching process has been only too successful, and the diagnosis might well be rendered by lot from amongst a not very small group of textbook headings.

Of course, the comparative method involves the use of observation also. The distinction between observational recognition of a condition and the comparative determination of a disease by a direct method, is not a mutually exclusive distinction. So also in respect to the third method of applied logic, namely, the statistical method. The statistical method requires both observation and comparison, but entails also certain new features. Probably Richard Cabot has popularized the statistical method of diagnosis more than any other recent author. Cabot finds a presenting symptom which becomes to him a lead. This presenting symptom is often something complained of by the patient, but also is often something noted by the physician that the patient did not subjectively recognize. Cabot has founded a differential diagnosis upon a selection of the most common presenting symptoms. He goes on to list the causes of these symptoms by their frequency, and then, to use his picturesque phrase, "follows the symptom home." Following the symptom home means, naturally, employing all manner of comparative and observational methods in addition to the merely statistical one. Nor do I wish to give the impression that Cabot overemphasizes the statistical method in his account. He merely emphasizes it more than most previous authors. Nor would his account fail to insist that observation is indis-
penable, for he carefully states that diagnoses are missed usually because physical signs are not recognized. That is, for Cabot, poor diagnosis is usually a matter of poor observation. But occasionally diagnoses are poor on account of poor reasoning, and Cabot’s book is an argument for a more comprehensive grasp of the possibilities of diagnosis. Others may insist upon the recognition of the symptoms and signs by proper methods of observation, and Cabot, like many others, has in other works given an account of these matters along approved lines; but he dreams for the purpose of differential diagnosis of a “cash-register of causes.” He wants possibilities sifted. He wants clues, radiations, and leadings followed out along lines that are essentially statistical. One proceeds from the presenting symptom to the possible causes; from the possible causes to the probable causes, and from the probable causes to the actual cause. In the technic of this determination, Cabot, as is well known, much insists upon the statistical configuration of the different conditions in which the various presenting symptoms are found.

Cabot notes that physicians in the matter of differential diagnosis “are very suspicious of any attempt to tabulate their methods of reasoning.” He speaks of profiting much by Herbert French’s Index of Diagnosis6th (first published in 1912), a book which I find is greatly affected by medical students of my own acquaintance. French claims his index to be an index of main symptoms, though there seems to be no internal evidence in the book just how these main symptoms were selected. French, like Cabot, deals largely though not exclusively with symptoms complained of.

It is readily seen that Cabot’s ideas (though they would have been readily comprehended by DaCosta in 1864, since they are founded upon the statistical principle which had already been a few decades in vogue in DaCosta’s time) would not have appealed especially to him as important in the process of direct diagnosis. Feeling as DaCosta did concerning diagnosis by exclusion as a dernier ressort, he would not have seen the compelling value of Cabot’s statistical frequency tables for the distribution of symptoms in different conditions. Moreover, it is doubtful whether in 1864 hospital records were so extensively kept as nowadays, despite the fact that intensive records of special cases may have been kept at a relatively higher standard than now prevails in institutions where the real work is done more by interns than by visiting physicians.

The situation, then, is that in some cases what might be called an observational diagnosis can be rendered. Personally, I should feel that the term diagnosis was being prostituted somewhat when the diseased condition is recognized forthwith on the basis of some pathognomonic symptom or combination of symptoms. But let the phrase pass; the result of naming the disease and getting a practical basis for treatment is attained by a process of pure observation. The so-called direct method of diagnosis is hardly on a higher plane logically speaking than that of observation in many cases; but in others, and perhaps in the majority of cases, this process of differential diagnosis by the so-called direct method, is one of comparison; that is, of matching the phenomena of the disease observed with those of some classical or textbook type. The unduly maligned method of indirect diagnosis, or diagnosis by exclusion, likewise employs comparison inasmuch as it excludes disease after disease by discovering that not
enough exists to match the types in question. Having risen, accordingly, from a process of direct recognition (gnosis rather than diagnosis in any logical sense) to a process of somewhat less direct differential diagnosis by a positive matching or a negative excluding process, we finally arrive at a more modern technic of studying the frequency of symptoms in certain large groups of comparatively well observed cases (the majority of Cabot's tables deal with 250 cases analysed at the Massachusetts General Hospital). We notice that the process of observation is indispensable throughout. Extreme exactitude of observation is not perhaps so necessary in statistics as in individual cases; at least errors of observation may be assumed to balance themselves to some degree.

Is there no further sharpening of our technic of diagnosis? In the practical handling of the diagnostic problem at the Psychopathic Hospital, in the course of two years' observation I became interested in the fact that, where diagnoses were not matters of mere observation and direct differentiation, but where processes of exclusion were necessary, I found myself endeavoring to exclude conditions in a certain definite order. It is this ordinal principle which I believe might constitute an advance in practical diagnosis, were it to be insisted upon in still other branches of medicine than psychiatry. I have recently made some endeavor to apply the principle to the situation in clinical neurology but shall not here mention the results of that inquiry in any detail. After the first two years of off-hand and provisional diagnoses at the Psychopathic Hospital, I then considered the matter of exclusion in a definite order for some three years before laying down the order in question (see below). I think it would be presumptuous for any one in any special branch of medicine to draw up a definite order of diagnoses in any other. For example, however convinced I might be that diagnoses in dermatology are actually made consciously or unconsciously by some process of successive exclusion of conditions in a definite order, I should not (as I am far from being a dermatologist) have the slightest ground for suggesting an order for dermatological diagnoses, or a list of conditions to be put in order. Not being an internist in the field of cardiac disease (that is to say, not being in any sense an expert with the electrocardiograph and all the rest), I certainly could not advocate any particular order of diagnosis for heart disease, however strongly I might be convinced that such an order must exist.

At this point, I looked into certain works on logic to learn what might be said concerning these different principles of diagnosis, and found in the late Josiah Royce's contribution entitled "The Principle of Order" in the Encyclopaedia on Philosophical Sciences, an exposition very much to the point.17

I presume that most of us have few memories of logic except the syllogistic logic of such college textbooks as that of Jevons. Some of the older amongst us will still have used Mill's System of Logic, or textbooks closely modeled thereupon. The more recent advances in applied logic are a closed book to most college and medical students. But in addition to so-called formal logic used as an irregular discipline in most colleges, there is an applied logic or methodology which deals with the norms of thought as applied to the methods of the special sciences. The formal logic of a book like that of Jevons is only a very subordinate part of logic as at present defined. Royce speaks of the new doctrine as
the Science of Order. This science, he says, is today in a very progressive condition, is in some notable respects new, and offers inexhaustible opportunities for future progress. Already in the Socratic and Platonic handling of these topics, the systematic nature of the world's construction, the objectivity of the world permitting inference concerning its various truths, and the order that rational processes show, had already become clear. Types, forms, and relations are at least as real as the facts of the physical world. Aristotle, Bacon, and still more importantly, Galileo, made important advances in logic. Today no one will question that every science in dealing with its facts must employ methods of classification, in no wise different from the method used by Socrates in his dialectics. To be sure, a science which uses only methods of classification must now be regarded as a very young science. Botany and zoology have long since passed the phase in which classification is the dominant interest. Anthropology, on the other hand, is largely classifying still, though there is some tendency to the development of a higher method of logic, the comparative study of the forms and results of human culture.

Royce mentions that, amongst the medical sciences, psychiatry is just now emerging from a stage in which the bulk of the science was made up of the classification of cases, symptoms, and disorders. Psychiatry is now about to work on a higher plane of methods. The more complex the facts, the harder it is for a science to get beyond this first stage of classification.

What are these higher processes of applied logic? There are mainly two: The method of comparison, and the statistical method. The comparative method compares the corresponding stages in the various processes of products of natural evolution in the science in question; whereas the statistical method uses exact enumerations as the basis of its science. Of course, the two methods are not always sharply to be distinguished; whenever one has to compare numerous evolutionary processes, all the results of these processes have to be enumerated. In these instances, however, the statistical method is merely an auxiliary to the comparative method. When, however, we come to such matters as the mortality tables of insurance, the sociology of marriage and divorce, of suicide and crime, or of commerce and industry, we are dealing with topics in which the statistical method is virtually the only one at present available. We study, for instance, how human mortality varies with age; how the science of an organ or an organism follows conditions determined by heredity or environment. As a result of such statistical processes, the statistician becomes able to deal with aggregations or blocks of facts; orders or aggregations which may be treated as units of a higher order.

When sciences become developed still further, these two methods of comparison and statistics are used in a still more intimate manner, combined in a method which Royce terms the organized combination of theory and experience. By means of the statistical and comparative methods, we discover laws of nature that have a certain degree of probability, the degrees of which we may trust on the basis of fair samples of the phenomena of the given science. We then construct laws on the basis of hypotheses derived directly from fair samples of fact. Hypotheses thus drawn may be of the simple type directly
refutable or directly verifiable by immediate recourse to the facts; or on the other hand, hypotheses may require much more work for their testing. In fact, the more direct the proof, the more valuable the hypothesis, since the success of the hypothesis would here depend upon the falling-in-line of vast numbers of facts which on no other basis could be supposed to fall in line. To combine hypothesis, theory, and observation, we first draw up a hypothesis regarding the constitution or laws of some region of physical fact. Upon the basis of this hypothesis, we make an extensive and exact deductive theory as to what ought to be present if the hypothesis is true. "The more extensive, exact, and systematic the theory thus made possible proves to be, the larger are the possible samples of the consequences of the hypothesis which are available." We then collect samples of facts by means of observation and experiment, and proceed to compare them with results of our elaborate deductive theory. The more elaborate and extensive our theory, the larger the range of facts that we can draw upon for comparison. We are not now restricted to noting what proportion of members of a sample have certain characters. On the other hand, if the deductive theory is a highly developed and elaborate one, we shall find that almost any sample available for comparison is minutely verifiable in detail with some part of the original hypothesis. In short, the value of this method depends upon "the exactness, the order, and the systematic character of the concepts in terms of which the hypotheses thus directly tested are defined." It would thus appear from this highly condensed statement of Royce's view, that "the organized union of theory with observation requires for its perfection concepts and systems of concepts which permit of precise and extended deductive reasonings." It is surprising how small the samples need to be when it comes to a judgment as to the applicability of a highly complex hypothesis. A few small samples taken from widely separated parts of the system will prove or disprove the virtues of the hypothesis.

Without asserting that medicine is yet in a stage to profit like physics and chemistry from this organized combination of hypothesis, theory, and observation, it is clear that many parts of medicine are far beyond the application of mere statistics or of mere comparison, and still farther beyond the modest virtues of pure observation.

Without venturing further at this time into methodology and modern applied logic, it is clear that mere observation, mere comparison of stages, mere enumeration, are not the whole story in any science. If the medical sciences are going to advance in their logic, it is more than likely that their logical advance will take place in the direction of a greater insistence upon order. I hold accordingly that, if Royce's exposition of certain modern developments in logic is a sound one, the next stage of development in medical thinking will be a reduction of its interests more and more to order and system. There is reason to hope that by the reduction of medical as well as any other scientific facts to order systems, a greater and greater success in deduction will be attained. I conclude, therefore, that when I found our process of diagnosis at the Psychopathic Hospital employing more and more the concept of order, I was simply discovering a small fact bobbing in the big stream of logical advance.
In the light of present knowledge, how almost naïve and childish appears the old dictum that disease is life under altered conditions! Here were but two elements: life on the one hand, and change on the other. Each contains a host of undiscovered, undigested facts. How slender a contribution appears the claim that diseases must be instances of either the augmentation, the diminution, or the mere modification without increase or decrease of structures or functions! To be sure, bacteriology shortly yielded a dyadic system of germ on the one hand and host on the other, which for the moment and for a decade or so allowed one to hope that the formula “Disease is life under altered conditions” might be simply rendered into, “Disease is the effect upon an organism of a germ.” Then came the contentions of Metchnikoff concerning phagocytosis, and the more complex interrelations of the host with its contained germ.

Shortly, in the nineties arrived triadic conceptions of the interrelation of the organism, the foreign substance, and sundry intermediary bodies; and early in this century was unfolded the still more mysterious situation of anaphylaxis. In short, the problems of medicine are becoming more and more rich and complex, more and more verifiable by small samples of fact taken at widely diverse parts of the system.

The rapid complication of the research situation in medicine at large, as typified especially by the facts of immunology, has its reflection also in the slower moving departments of clinical diagnosis. We saw in textbook after textbook insistence on diagnosis as a matter of observation (going back very far, for example, to Baglivi"). We then found, as in virtually all the medical textbooks in the latter half of the nineteenth century, a new form of assurance: The diagnostician is not now satisfied with what he calls direct diagnosis.

The aim here is to enroll the case in some definite group of diseases recognized by pathology. The difference between perceiving that “this fruit is an orange” and scientific recognition of a diagnosis is merely a question of degree. It is not so easy to say that “this is a case of typhoid fever,” as it is to say that “this fruit is an orange,” because of the more concealed and special nature of the points on which we rest our diagnosis of typhoid fever. The method is one of comparison of certain perceived characters or features with certain signs already known to us as pertaining to certain disease groups. Whether the symptoms are simple, like a friction rub, or complex, like the chemical analysis of a secretion; whether the symptoms are present at the time of examination or are merely anamnestic, from the diagnostic point of view, they are speedily divided into pathognomonic symptoms on the one hand, and more general ones on the other. The tubercle bacillus or the Argyll-Robertson pupil might be mentioned as examples of pathognomonic symptoms in this broad sense; whereas fever and vomiting would be examples of symptoms which belong in a great number of disease groups. But whatever the kinds of symptoms we recognize, the process of diagnosis appears to be one of comparison of those symptoms that we perceive with such symptoms as we know to characterize a given group of diseases, and upon the discovery that the symptoms of the disease in hand correspond with the symptoms of the disease type
which we know from the books or from experience. We draw a conclusion, then, on the basis of similarity of symptoms. We may draw this conclusion from a pathognomonic symptom, which beyond any question indicates the existence of a given disease; or if no pathognomonic symptoms are present, we may ground our diagnosis upon the harmony between the general symptoms in our patient and the symptoms that belong to some described disease; and of course we are throughout well aware how unsuccessful may be a diagnosis based largely on the similarity of general symptoms to those of the books. This method of diagnosis, which we may briefly term the "type-matching" method, is doubtless the one extolled by the older workers under the name direct diagnosis. It represents an advance upon purely observational diagnosis,—that is, diagnosis by inspection,—and is a form of diagnosis by comparison.

Diagnosis by exclusion or by differentiation is a method somewhat decried by older workers and now brought into great prominence by such work as that of Cabot and of Herbert French. Though there is nothing logically new in this work, there is a new emphasis in it. It is still, logically speaking, an application of the comparative method greatly bolstered, as Cabot handles it, by the statistical method.

The general nature of differential diagnosis is somewhat as follows: As it is impossible to consider all the phenomena in a given disease, we first select a group of diseases in which a certain sign is found. We look in the case under observation for some more or less outstanding symptom. This common symptom which we would use as the point of comparison amongst diseases, is called by Bieganski the index of differentiation or the difference indicator. Cabot has more recently termed one form of it the presenting symptom. This indicator or presenting symptom is chosen because we by experience know that it occurs in a large number of diseases. To be sure, it is much better not to take a too-general indicator. Thus, in the question of typhoid fever, it would not be so advantageous to begin with fever, perhaps, as to begin with diarrhea. The process of differentiation now begins: We compare the case under observation with those diseases which we have found to be somewhat similar thereto. We pass from one disease group to another. We constantly keep upon one side the observed case with all its symptoms, and on the other the total series of diseases which in a certain respect are similar to it, namely, in respect of the common trait, index of differentiation, or presenting symptom. We proceed to look for differences. The observed case may not belong in a group because it does not possess the symptom which is constant in that group; though the indicator is possessed by both diseases, a symptom constantly present in the compared type is absent in the observed case.

Or the observed case may not belong in the compared group because there is a symptom in the observed case which never occurs in the compared group.

Again, the observed case is not the disease with which we are comparing it because we know that in the compared disease a symptom constantly occurs which is the direct opposite of some symptom observed in the case in hand.
Moreover, there may be an incongruence of symptoms of a quantitative or qualitative nature, a difference which does not proceed to the point of complete absence or perfect antagonism of symptoms.

Following is a table which embodies the process-types of clinical diagnosis now in vogue:

**Table of Process Types of Diagnosis**

I. Inspection.
II. Expectation.
III. Induction.
IV. Comparison
   (a) Similarity.
   (b) Similarity and difference.
   (c) Difference.
V. Ex juvantibus.
VI. Ex nocentibus.

I may say at the outset that it is the endeavor of this paper, granting each of the three subordinate methods under the head of Comparison to be a separate logical method of diagnosis, to add a ninth method. This ninth method I shall choose to call "Diagnosis per Exclusionem in Ordine." *Diagnosis per exclusionem in ordine* is merely a method of employing logical comparison of Type IV (c) of the above table *in an orderly manner*.

But before attempting to speak of this newly described type of diagnosis I must briefly describe the eight classical methods so that I may, if possible, demonstrate that the ninth or proposed new method has novelty. For the purposes of this discussion I have gone rapidly over a great many of the older textbooks of medicine, studying largely the logical remarks by their authors in the prefaces and introductions thereto. I then examined many of the textbooks in Medical Logic which were for some years the vogue in medical schools. From the study of textbooks, largely in the latter half of the nineteenth century, and from the study of works on Medical Logic I have arranged and combined the methods of the above table. I have been especially aided by the modern work of Bieganski, a Polish work available to me in German translation from its second edition. This work may be referred to for a good summary of the older points of view, together with the fruits of some Polish polemic in which Bieganski had for some years been engaged. The older work of Oesterlen, published in Tübingen in 1852 and translated into English in the Sydenham Series in 1855, is almost barren for modern purposes (so much so that I found the leaves uncut both of the original and of the translation in the New York Academy of Medicine!) Of still older literature, the Medical Logic of Sir Gilbert Blane, illustrated by conclusions concerning yellow fever, is of great interest, though his classification of the elementary principles of life as generative, conservative, temperative, assimilative, formative, restorative, motive, sensitive, appetitive and sympathetic, is an example of categories which, however true and from various points of view feasible they may be, are of little diagnostic value. This older work often fails to distinguish the question of classification from the question of what is to be classified. Sir Gilbert Blane's work upon yellow fever, and especially upon scurvy, derived
from his expert contact with marine diseases in the British West Indian campaigns, is of greater value than his excursions into logic. Blumenbach calls Sir Gilbert Blane the most learned and classical physician of the age; but despite this German appreciation, little has been done in Germany of an original nature on medical logic. Oesterlen appears to be in the general nature of a physician’s reaction to the logic of John Stuart Mill. Apparently someone was impressed with the possible value of such work as Oesterlen’s, and Francis Ogston was given a new chair in Aberdeen as Professor of Medical Logic and Jurisprudence. In a syllabus25 of his highly interesting course of thirty-two lectures, he refers to his indebtedness to John Stuart Mill and to Oesterlen, traces the history of the topic from Aristotle to Bacon, thence to Blane and Oesterlen, and comments upon the absurd distrust of theory which he found prevailing in Britain. Ogston called the merest routine practitioner a bold speculator and imperfect observer of facts. He remarks that the limits of pure observation in medicine are soon reached and discusses in separate chapters the inductive or analytic method as opposed to the deductive or synthetic method, rightly holding that the latter is a much more important concern of medicine than the former. Under the inductive or analytic method Ogston considers the processes of observation, comparison and analogy, and the numerical method. Under the deductive or synthetic method he considers first the process of analysis for deductive purposes, then the process of ratiocination and of verification. He discusses, among the processes subsidiary to induction, such matters as simple observation, experiment, hypothesis, analogy, the numerical method, terminology and classification. Ogston denounces the current nosologies as obstructive and inelastic and devotes special attention to what he calls the fallacies of nonobservation as opposed to the fallacies of malobservation. Much of this suspicion of Ogston can be traced in Oesterlen and Sir Gilbert Blane, to say nothing of their masters in logic, John Stuart Mill and Sir Francis Bacon.

Sir Gilbert Blane had, for example, in 1819 spoken of the sources of error in medicine as (a) the fallacy and danger of hypothetical and theoretical reasoning as especially demonstrated (according to Blane) in Boerhaave, (b) the diversity of constitutions, (c) the difficulty of “appreciating the salutary efforts of nature and of discriminating them from the operations of art,” (d) superstition, (e) the ambiguity of language, and (f) the fallacy of testimonial.

Much can still be gained, no doubt, from a perusal of such works as those of Sir Gilbert Blane, Oesterlen and Ogston, and still more could doubtless be gained by the physician from a close study of John Stuart Mill and his predecessors in pure logic, but these studies were almost wholly given over with the onset of the evolutionary theory in the late fifties, with the crowding developments of pathology and physiology and especially of the bacteriology of the eighties and the immunology of the nineties of the last century. There has, in fact, crept over us the feeling that logical method is an inheritance of us all in like degree and that whatever we do not know of logic at the outset of medical practice we shall never learn. It is precisely the popular single volume textbooks that fail to yield much of practical value to the medical student seek-
ing to perfect himself in the logic of diagnosis. The dictum, especially of Da Costa and of his successors in the manufacture of single volume textbooks, namely, that the art of medicine consists in observation, has apparently abolished the necessity of logic. Carried away by the magic of this word “observation,” many authors of physiologic works dealing with the most intricate discussions and fine-drawn conclusions cheerfully called their inductions and deductions by the term “observations.” The obvious fact is that diagnosis does not consist in observation at all. Diagnosis requires observation, proceeds upon a basis of observation, but is in itself a process of combination, of reasoning, of calculation, or of some higher kind of intellectual process which takes observation for granted, uses and chooses amongst observations, but in no whit makes them.

Amongst the methods tabulated above I have first set down, following many of the books, diagnosis by inspection. Here, at the outset, it may be inquired: “Are we not terms a method of diagnosis what is nothing but a method of observation?” This I freely concede. The names of diseases are achieved by a process of inspection. The result is what we term a diagnosis, but the process by which the so-called diagnosis or disease-name is arrived at is not a process of reasoning at all, but (as stated above) a process akin to the statement, It is raining, or The engine is skipping. In short, no process of diagnosis in the logical sense is here being used. A diagnosis or disease-name is achieved, but reasoning is not employed. It is only, then, by courtesy that we include inspection under the process-types. The diagnosis follows the inspection as night follows day, or as black follows white, or as man follows woman. The process is one of association of a perfectly simple type. This kind of so-called diagnosis can be made by laymen often as well as by physicians. I am afraid that the ideals of many medical teachers, as they are got by the eager students, consist in teaching them thumb rules of inspection rather than the more tedious and complicated methods of diagnosis. I recalled above how Da Costa spoke of diagnosis by exclusion as a tedious method.

The second method tabulated above is one that I have presumed to term the method of diagnosis by expectation. We are familiar with the so-called expectant treatment in therapeutics, but, so far as I know, attention has not been called to the method of diagnosis by expectation. This is, of course, particularly employed in the fulminating, acute diseases on one hand and the chronic diseases on the other. In the one instance there may not be facts enough to permit a diagnosis by inspection and there may not be time enough to permit elaborate comparisons; accordingly, diagnosis goes by the board until an autopsy either is or is not made. As for the slow, hesitating course of certain diseases, the Micawber attitude is still easier to assume.

I suspect that the method by induction is commonly employed in a manner not far removed from the process of expectation. This method by induction, unlike the method by expectation, is mentioned in the classical works, and also in the modern work of Biéinski. One concludes that the patient looks tuberculous. Here is the suggestion of a diagnosis by inspection. Perhaps one is an office practitioner and time forbids exactitude. One examines the pulmonary
apex and discovers suspicions of dullness. One requests the sputum and proceeds to a positive diagnosis of tuberculosis on the basis of tubercle bacilli found in the sputum. Here is a successful case of diagnosis by induction by a sort of skipping of intermediaries (to use James's phrase) and by the least possible work. If the diagnosis is successful, well and good! If unsuccessful, one must proceed to more elaborate methods. It is always possible, too, that the patient may have been suffering from a combination of two or more diseases and that the tuberculosis e.g. found may be the lesser of two important conditions. One ought not, and I do not, condemn this method by induction just because it is not time-consuming and just because it does not succeed in all cases. In the hands of the expert the method of diagnosis by induction (as the phrase is here used) and the method of diagnosis by inspection are highly commendable methods. In a pragmatic age one ought not to denounce them. From the standpoint of a medical student, however, it would seem wise not to dazzle him with the brilliances of inspection and induction (in the above narrow sense), but one ought to expound to him the nature of the more complex methods which he must often use in cases that are themselves at all complex.

The methods of diagnosis ex juvantibus and ex nocentibus tabulated above are in a sense experimental methods of diagnosis on all fours with the method by induction. The diagnosis of syphilis by the administration of antisypillitic drugs and the procuring of a cure or an alleviation of symptoms is an example of diagnosis ex juvantibus. The provocative Wassermann reaction upon the administration of salvarsan is an example of the method of diagnosis ex nocentibus. It would be a mistake to consider the administration of drugs for these purposes therapeutic. The methods are methods of diagnosis purely.

Dismissing the methods of inspection, expectation, induction, ex juvantibus and ex nocentibus, we come to the methods of diagnosis by comparison, to which I hope to point out a small addition or modification which I conceive to be of value. Earlier in my communication I have sufficiently discussed these methods and I pointed out the necessity under which we labor of using the method of diagnosis by exclusion in cases where there are no indicator symptoms or presenting symptoms (to use Cabot's phrase for certain indicator symptoms). I wish to illustrate this method of diagnosis per exclusionem in ordine from the field of mental diseases, rehearsing the main groups of a practical key which has been built up on the basis of Psychopathic Hospital data.

In passing, I may say that psychiatrists are far more at one in the matter of diagnosis than the general medical man is apt to believe. The unanimity in point of view is far greater than one would suspect from certain nomenclatural differences. The nomenclature of psychiatry is by no means as fixed at the present writing as the nomenclature of dermatology. Yet the unanimity of psychiatrists concerning the major groups of mental disease is, I make bold to say, almost as great, if not quite as great, as the unanimity of dermatologists concerning their main groups. The specialists in psychiatry have for some years past been much interested in proposals of the American Medico-Psychological Association to secure a thorough and acceptable classification of mental diseases to which the State institutions for the custodial insane could conform. The
classification adopted by the Medico-Psychological Association in 1917 is, in general, a highly acceptable one.25 Probably no member of the committee charged with making the classification is entirely satisfied with it in details. Some diagnosticians go so far as to say that in the interest of the patient no classification in the sense of the statistical tables of institutions is worth while, since every instance of mental disease is virtually *sui generis*. There is, of course, much truth in this contention, and the good therapist never forgets this fact, borne in upon the mind as it is by the tremendous individual variety of the psychopathic reactions of the individual as such. I found that most of the leading American textbooks of psychiatry were also practically at one in the major categories, despite perturbing difficulties in terminology.

The task of the Psychopathic Hospital, dealing as it does not merely with custodial insane but with all the penumbra of "near-insane," of incipient, mild and curable cases, and with many psychoneuroses which would not ordinarily be classed among the insanities, is a somewhat broader and deeper task in diagnosis than that of the custodial institutions for the insane. The Psychopathic Hospital stands as a sort of vestibule between the community and the custodial institutions. From its meshes go back into the community numerous cases whose psychopathia is not far advanced enough to go through the interstices. Not only insanity in the narrow sense, but a variety of other conditions of the *non compos mentis* group, namely, feeble-mindedness, epilepsy, and even many forms of alcoholism come within the purview of the Psychopathic Hospital. The task of rapid diagnosis was one which had never faced the custodial institution for the insane, because to the latter flowed only cases in which diagnostic experts had made their decision outside the walls of the institution and with the concurrence of the Courts. It became necessary, therefore, to contrive more rapid methods of handling cases than had formerly been available, inasmuch as keen and mature experts were not available for the great run of cases (the Psychopathic Hospital ran the number of voluntary cases and of so-called temporary care group of cases up by the hundreds as soon as it was founded). Some provision had to be made whereby younger men could make reasonably accurate primary siftings of the cases. The rapid development of mental hygiene stole away the young men at such a rate from our staff that provision had to be made for shifting staffs of relatively inexperienced persons. The situation was not unlike that which faced the army itself at the outset of the first draft.

Mental diseases have no indicator symptoms, barring a few, and indeed a very few, so-called pathognomonic symptoms. Psychiatry was devoid of pointers, such as would be available to the specialist in the exanthems, for example. In the absence of indicator symptoms, some method had to be chosen by which every case would have every major hypothesis concerning its nosological grouping raised at some time during his hospital stay. The legal details and official regulations were such that these hypotheses had to be raised within a very short period. Early in our experience the laws and regulations were such that some kind of diagnosis permitting proper disposition of the case had to be made within four or five days. This proved to be too brief a period in
which to make proper disposition and the Temporary Care Law, under which the majority of Psychopathic Hospital cases are admitted for observation directly from the community (without court process), was modified so that a period of seven or eight days is now available. Official disposition is at present made in a total period of ten days, instead of in a total period of seven days; but in practice the external conditions of transfer to social adjustment take from two to three days, so that the total time available for group diagnosis is now a period of from seven to eight days. During this period of seven to eight days what can be done?

I assume that in private practice it may not be necessary to secure the Wassermann reaction of the serum in each and every mental case. For my part, I never feel certain of the diagnosis unless this precaution is taken. In any event, in Psychopathic Hospital practice, with the varying conditions of staff and history standard, it shortly proved necessary to get the Wassermann data from the serum in each case. Only a few of the more conservative hospital superintendents failed at first to approve this plan. The Danvers Hospital in Massachusetts and the hospitals in Michigan, under the influence of the Psychopathic Ward at Ann Arbor, had early adopted getting the Wassermann reaction of the serum as routine. Upon reflection, I found that the proper disposition of almost each and every case depended upon our securing these Wassermann data. If the Wassermann data were dubious, then a longer period of diagnosis was necessary. If the serum proved negative but there were other signs of organic disease of the nervous system, lumbar puncture was resorted to, with the diagnostic hope that the Wassermann reaction and the other reactions of the fluid would prove decisive. The practical disposition of each case I found to depend upon taking the Wassermann reaction of the serum. Accordingly, although there is no indicator symptom which is infallible or anywhere near infallible in the detection of neurosyphilis, the value of the Wassermann reaction of the serum is such that I feel entitled to place the syphilopsychoses, or mental diseases of syphilitic origin, at the head of all diagnostic consideration. This was the beginning of the plan of ordinal exclusion, the plan of diagnosis per exclusionem in ordine. The first thing to be excluded in the diagnosis of mental disease was, in short, syphilis.

The second thing to be excluded was, I found in practice, some form of feeble-mindedness. To be sure, the obvious imbeciles and idiots may secure their diagnosis by inspection, and not so much difficulty is ordinarily felt amongst the morons or the feeble-minded proper of the English classification. But when one came to the so-called stupids or subnormals, that we now find to lie between the morons below and the normals above, the value of the Binet-Simon tests or their analogues became extreme. One does not need to claim one hundred per cent values for these tests any more than for many another biological test. The tests are nevertheless of value. I found that in practice I needed to know of every lucid case, that is, of every case which could practically be tested metrically, the intellectual iter. Even were I dealing with such a condition as dementia precox or manic-depressive psychosis, I nevertheless found that I needed the mental test as a base line.
Thirdly, I found that the hypothesis of epilepsy or epileptic symptoms and of epileptoid phenomena is one very apt to be isolated from consideration by the tyro or even by the expert. I placed epilepsy third in the diagnostic order of exclusion partly because of the relative reliability of the clinical histories in these cases.

After Group I, the syphilopsychoses, Group II, the hypophrenoses (feebled-mindednesses), Group III, the epileptoses, Group IV, the pharmacopsychoses (including mental diseases due to alcohol, drugs and poisons), I come to Group V, the group of mental diseases due to focal brain disease,—the so-called encephalopsychoses. This is the group of cases in which we unconditionally need the data of the routine neurologist's diagnosis. We need to exclude conditions of heightened intracranial pressure and conditions of asymmetry, reflexes and the like, indicating focal brain disease.

Sixthly, after Group V, the encephalopsychoses or neurologist's group, I found that in practice I was asking for the data of the interns, namely, the data which would go to make a diagnosis of a mental disease somewhere in the "symptomatic" group,—Group VI, a group I have termed the somatopsychoses.

After these I tried to exclude presenile and senile phenomena belonging in a group which I have termed, following Nascher's Geriatrics, the geriopsychoses (Group VII).

Then come the stock difficulties of diagnosis for the psychiatrist,—the diagnosis of Group VIII, the dementia precox group, which I like to term (from their main symptom as defined by Blenner) the schizophrenoses.

After the schizophrenoses stand Group IX, the cyclothymoses, or a group including the manic-depressive psychoses and similar mental diseases.

The last of the ten well-defined groups is the familiar group (X) of the psychoneuroses including the hysterias, neurasthenias and psychasthenias; but if in the diagnostic order of exclusion one could not place one's case in any one of these ten comparatively well-defined groups, one would not throw it altogether out of the psychiatric lists, but would call it a plain psychopathia and put it in Group XI, the psychopathoses. A table of these major groups is shown on the following page.

I have spoken of the botanical and zoological analogues of this grouping in a paper read before the Neurological Association in 1917. Two words of warning are necessary concerning this analogy. The choice of the ending *asis* is obviously an attempt to get a medical analogue for the botanical ending *-aceae* or *-osae*, as in the orders *Rosaceae* and *Leguminosae*. These botanical orders are, as is well known, susceptible of splitting into genera and species. It would be an entirely superficial objection, for this account, to say that the term "species" has obtained in natural history a very special meaning which could not appropriately be given to a disease. Biologically speaking, a species is sometimes taken to mean such a group of plants or animals as are capable of breeding true to type. However, the genus-species distinction is a great deal older than natural history and is used throughout every province of logical
classification known. For example, the mineral kingdom, though without process of breeding, is in part susceptible to a classification of the genus-species sort. For that matter, it is not necessary to use the terms order, genus and species for these groupings at all. One might use entirely other distinctions, and still the process of classification might be entirely valid.

The other point which should be made is a kindred one. In zoology and botany one would ordinarily be satisfied if in the process of logical exclusion one arrived at the animal or plant which fulfilled the conditions of differentiation, but in the process of diagnosis per exclusionem in ordine as applied to mental disease one unconditionally needs to go through the entire list in each case.

For example, one might easily face an instance of mental disease such that the victim was a syphilitic, feeble-minded, epileptic, alcoholic old man, with focal brain disease and somatic disease, the whole contributing to his mental picture. He might conceivably have been psychoneurotic or cyclothymic in his life, for these conditions are doubtless consistent with a fundamental feeble-mindedness in some cases. Whether he could also have been a victim of schizophrenia (dementia praecox) and still have mental phenomena attributable to all these other groups is a question. We are aware of cases that start by being feeble-minded and apparently have their schizophrenic process grafted upon them; but whether these combinations of psychoses in so multiplex a form are common or really exist is not my point. Combinations of two or three of these conditions are not infrequent. It is, therefore, stringently necessary, in the rough analysis of psychiatric cases, to make all of these major hypotheses and dismiss them or fix upon them one by one. In short, diagnosis per exclusionem in ordine does not necessarily exclude ten out of the eleven groups, but it does endeavor to exclude each one of at least ten of the groups. If the process has succeeded in excluding the first ten, the case, if psychiatric at all, must remain in the unresolved or ragbag group of psychopathias, Group XI, the psychopathoses. I do not intend here to enter the minutiae of psychiatric diagnosis, although in another place I have made some endeavor to describe the main genera and species of the groups above tabulated.26 Incidentally, I have endeavored to place genera and species in the same sort of practical order, so that the tyro may consider the diagnosis

<table>
<thead>
<tr>
<th>Mental Disease Groups (Orders)</th>
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<tbody>
<tr>
<td>1. Syphilitic ................................................. Syphilopsychoses</td>
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<tr>
<td>2. Feeble-minded ............................................. Hypophrenoses</td>
</tr>
<tr>
<td>3. Epileptic .................................................. Epileptoses</td>
</tr>
<tr>
<td>4. Alcoholic, drug, poison.............................. Pharmacopsychoses</td>
</tr>
<tr>
<td>5. Focal brain (&quot;organic,&quot; arteriosclerotic).......... Encephalopsychoses</td>
</tr>
<tr>
<td>6. Bodily disease (&quot;symptomatic&quot;).................... Somatopsychoses</td>
</tr>
<tr>
<td>7. Senescent, senile ........................................ Geriopsychoses</td>
</tr>
<tr>
<td>8. Dementia praecox, paraphrenic...................... Schizophrenoses</td>
</tr>
<tr>
<td>9. Manic-depressive, cyclothymic....................... Cyclophrenoses</td>
</tr>
<tr>
<td>10. Hysteria, psycho-, neurasthenic.................. Psychoneuroses</td>
</tr>
</tbody>
</table>
of the species of syphilitic, hypophrenic and other forms of disease in an arbitrary order.

It is scarcely necessary for me to insist that I do not attach great consequence to there being ten or eleven more or less major groups of mental disease. In fact, I insist that the future must interpolate or extrapolate these groups. The groups are not groups based upon clinical resemblances alone, upon anatomical disease, or upon etiology. They are practical groups devised upon what has been found to be the best practical order of exclusion of major groups of mental disease, each seeming to require a special form of handling. I have described the method in my paper entitled "A Key to the Practical Grouping of Mental Diseases." I am not sure that I would not be warranted in using the term pragmatic instead of practical, for the end and aim of these groupings is, after all, a therapeutic one. The basis for these rough distinctions is a question of disposal of the patient. If the patient belongs amongst the syphilopsychoses, then antisyphilitic therapy is indicated, or at least must be thought of. If the patient is hypophrenic, then a school for the feeble-minded, an institutional equivalent, or some form of supervision looking to the safeguarding of the community and the development of the case must be adopted. If the case is epileptic, certain special safeguards are necessary, whether the case is an alcoholic epileptic or a traumatic epileptic. The etiology and the anatomy are not at all so much to the point as the practical handling of the epileptic. So, too, with the alcohol and drug cases. In fact, the practical consequences of placing one's patient in one of the first four groups of syphilitic, hypophrenic, epileptic and alcohol or drug cases are such that I think on these practical grounds alone it would be wise for the diagnostician to consider syphilis, feeble-mindedness, epilepsy and alcohol, drugs and poisons before he comes to the minutiae. He will thus get settled in his mind many of the practical social questions of a legal, institutional and social service nature. Take the group of mental diseases due to focal brain disease. Should the phenomena of the present war show us that the traumatic psychoses, that is, psychoses due to structural brain disorder, form a practical group on a level of practical interest equal to that of the listed groups, I for one should have no objection to raising these traumatic psychoses, which I at present place under the encephalopsychoses, to a higher dignity, namely, that of the traumatic psychoses. For the moment, I seem to feel, and I suppose most of my colleagues would agree with me, that the traumatic psychoses rather resemble genera under the focal brain disease group than the true great orders of disease. Let the list be telescoped or accordeonized as you will, no damage is done to the concept-value of the greater groups of mental disease.

Again, should someone wish to consider the somatopsychoses ahead of the focal brain disease group and exclude e.g. pellagra and cardiorenal disease from a role in the given patient ahead of brain abscess, brain tumor, arteriosclerosis, and the like, I should have no objection. The point I make is that some order, not any particular order, is desirable for the tyro in diagnosis. I insist, therefore, on no particular great groups, but on the principle that
there should be great pragmatic groups of mental disease analogous to the orders of botany. I insist, again, on no particular order of exclusion, but on some order of exclusion of these groups, and above all, I insist upon subjecting each mental case, so long as indicator symptoms are so woefully absent, to the hypothesis of his possibly belonging under every one of these great groups. Indeed, I feel that he may belong in two or more of these groups.

Whither, you may ask, does the pragmatic principle then disappear? Suppose a man is both epileptic and alcoholic; shall we classify him as in the group of epileptoses or in the group of the pharmopsychoses? All specialists will remember acrimonious discussions of this nature. Are we dealing with epilepsy or with alcoholism, or with both? Now, I find that in practice, if you couch the question pragmatically, there is rarely, if ever, any doubt into what group you wish to throw your case. Once you put the question thus: Do you wish to send this case to an institution for epileptics or do you wish to put him in an institution for alcoholics or under appropriate supervision suitable for alcoholics? There is almost never any doubt on the part of the careful physician whether his case is a case for an institution for epileptics or a case for an institution for alcoholics. The pragmatic question is forthwith settled. The academic question, whether one is an epileptic alcoholic or an alcoholic epileptic, becomes a vividly practical question when the pragmatic, that is to say, the therapeutic, question is raised. I find that this kind of consideration matures the tyro in psychiatry very fast.

SUMMARY

1. The writer apologizes for a communication on Medical Logic in general when he is only a psychiatrist and but recently a pathologist. His excuse is the necessity for reasonably accurate snap diagnosis in the sifting problem of the psychoses, psychoneuroses and psychopathias, as they flow through the Psychopathic Hospital clinic in Boston.

2. The medical student is found destitute of the ability to define entities and symptoms. The textbooks in medicine, especially the single volume textbooks, rather tend to make the student believe that diagnosis is observation. In point of fact, diagnosis is not observation, though it requires and indeed stands or falls by accurate observation.

3. Da Costa and his successors have lauded so-called direct diagnosis to the skies, and Da Costa rather decried indirect diagnosis by exclusion as a tedious process. An example is cited from Da Costa which shows how relatively simple the classical diagnoses of general medicine are beside those of psychiatry.

4. It seemed that diagnosis by exclusion ought to be rehabilitated. An examination of recent research work in logic indicated that higher and more complex methods than those of observation had become necessary in science. For example, Cabot and Herbert French have attempted to profit by the statistical method, which again, though it requires reasonably accurate observation, is not in itself a method of observation at all. Yet Cabot's statistical fre-
quency tables possess a certain diagnostic value. But the student is often misled by the brilliancies of so-called observational diagnosis in a clinic. Here diagnoses are often rendered on inspection by a process akin to the recognition of a fruit as an orange, or an automobile trouble as "the engine is skipping." This process is not diagnosis, it is a process of recognition that may receive a simpler term *gnosis*.

5. The offhand snap diagnostic work at the Psychopathic Hospital indicated that we were in practice relying upon the successive exclusion of certain great disease groups in a certain definite order.

6. A study of Royce's "Summary of Recent Researches in Logic" shows how an organized combination of theory and experience is the higher logic to which the more complicated sciences must resort. Royce himself mentioned psychiatry as a science about to climb out of the classifying era into the era of logical order, that is, of the organized combination of theory and experience. Such a dictum as that "disease is life under altered conditions" seems now childishly simple. The idea that disease is a matter of an organism plus a germ was found to be altogether too simple when in the nineties of the last century the concepts of immunology were developed.

7. Those departments of medicine in which the presenting symptom of Richard Cabot is of value are lucky departments. Those departments of medicine in which the indices of disease, or indicator symptoms of the elder writers are available, are also fortunate departments in comparison with psychiatry. In mental disease there are exceedingly few indicator symptoms.

8. Hence the need became apparent of a process of exclusion of great groups or phenomena in a certain definite order, so that nothing of large significance should evade consideration. To avoid the tediousness of exclusion, complained of by Da Costa, the phenomena of disease had to be logically grouped in certain great groups, and the process types of diagnosis in the books may be counted as six or eight, according to definition.

9. In the body of the paper, a special statement was made about each of these process types: Inspection (regarded as not really diagnosis but as merely recognition of gnosis), Expectation, a newly named but frequent method (far older than Michawber), Induction, *ex juvantibus, ex nescivitibus* (three methods in which in no very rigorous way experiment is used) and three methods of diagnosis by comparison are successively discussed.

10. The ninth method, diagnosis *per exclusionem in ordine*, is in one sense a minor modification of the old method of diagnosis by exclusion. It is of value in departments of medicine where there are no indicator symptoms, and where the so-called presenting symptom would merely indicate some kind of mental disease.

11. The general application of the method of diagnosis *per exclusionem in ordine* in the field of mental disease is demonstrated in the eleven groups of mental diseases into which most of the phenomena may be pragmatically cast. The groupings are not by clinical resemblances, by anatomical attack or by etiology. The distinction is a pragmatic and therapeutic one and will naturally
tend to become more and more etiologic as the causes are determined. But in the field of mental disease, causes are so apt either to be unknown or to be multiple that etiologic classification on any simple basis such as that of the infectious diseases is practically inconceivable.

12. It is hoped that other departments of medicine (where diagnosis is raised above the level of mere recognition) and where there are few or no pathognomonic or indicator symptoms, will find it to their advantage to set up a method of diagnosis per exclusionem in ordine, the great groups or orders being always determined on a pragmatic basis. In the body of the paper are given the general designations of the ten great groups of mental diseases, with the eleventh residual group.

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