mental and pseudo-mental phenomena in man, the lower animals, and plants. For myself, I am not prepared to inaugurate any such revolution, being content to adopt the terms currently in use in their vague and comprehensive significations; applying them to all classes of organised beings; in other words, to regard *mind*, and all its essential or concomitant phenomena, as common in various senses or degrees to *plants, the lower animals, and man.*

Skae's Classification of Mental Diseases. By T. S. Clouston, M.D., F.R.C.P.E., F.R.S.E., Physician-Superintendent Royal Edinburgh Asylum.

When I saw in the last number of this journal that Dr Crichton Browne had essayed the task of criticising the system of classification of insanity devised by the late Dr. Skae, I knew the fact could not but be gratifying to Skae's friends. To have any system or theory subjected to independent criticism is very good for it. Then I could not forget that some of those who had advocated most earnestly Skae's classification had been pupils, assistants, and friends of his during life; and I was conscious, from my own experience, how much anyone in that position was inclined to look partially on his work. I felt sure that Dr. Browne, while seeing this, would not, in those circumstances, consider it a mortal sin, and would pass it gently and generously by. Indeed, I was a little afraid that he himself, as an old pupil of Skae, might be tempted to soften the stern tone befitting a critic, by something of the same pardonable feeling. He has striven to resist this impulse, and with much success. Another reason why I rejoiced that the merits of this system should be canvassed was, that I thought with, perhaps, natural partiality, that everyone must necessarily see something good in it; and that the fact of its being looked closely into by a competent and unbiased mind would produce a better understanding of Skae's point of view, and a more thorough sifting of the tares from the wheat. Not that such criticism had been wanting either at home or abroad. The system had been before the world for twelve years. The authors of all the standard books on psychological medicine and papers on classification published since that time had discussed its merits; and it did seem as if it were growing in favour. Maudsley, in

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each successive edition, had seemed to make more and more account of it; Blandford had assigned it a good place amongst other systems; Hack Tuke had given high praise to all the "somato-etiological" systems of looking at and classifying mental disease, and to Skae's in particular; Mitchell had declared it had taken hold of the medical mind; Thompson Dickson had said there was some good in it; and finally, that Nestor of alienists, whom Dr. Browne fitly describes as "the most illustrious representative of English medical psychology now living," Bucknill, had given it the truest flattery of all by incorporating its nomenclature in the orders, genera, and species of that classification which is the final result of his vast experience, the generalised sum of all his thinking. All these, and more, had found it had faults; but they all speak of it and its author with much respect. Then it is a mere matter of fact that its terminology had become a part—and an essential part—of recent writings on nervous and mental disease.

When, in addition to all this array of criticism, I observed that Dr. Browne had entered the field, I was surely justified in the expectation that here was a critic who would combine the modesty of youth with the judgment of experience, the calmness and dignity of science with the sense of responsibility of a physician.

I regret that I am obliged to take exception both to the matter and the manner of Dr. Browne's critique. I shall first endeavour to deal with its matter. To do this properly is no easy task. I do not mean that the arguments appear to me unanswerable, or the objections overwhelming; but that Dr. Browne, no doubt from his desire to be exhaustive, seems to have failed in arranging his ideas in that order, and in expressing himself with that clearness and point so very essential in a critique of any value.

Dr. Browne begins by a defence of Esquirol's system. His argument is that "day by day it becomes more apparent that we shall never accurately make out the molecular changes which correspond with mental aberrations," being "still as far as ever from mounting a delusion in Canada balsam or from detecting despondency in a test tube." "It is clear, therefore, that these changes can furnish no direct assistance in the classification of the *insaniæ*." A pathological classification is, therefore, an impossibility. The dream of so many patient workers in clinical and pathological fields is a mere chimera. Now, is this position, so confidently stated, a correct inference from facts, or a mere

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assertion? It lies on the threshold of the main question at issue in the critique; but its examination may help us to understand our critic's mode of reasoning in other places. Let us see what reversing all this would bring us to. "Day by day it becomes more apparent that we shall *soon* accurately make out the molecular changes which correspond with mental aberrations, being *on the eve of* mounting a delusion in Canada balsam. It is clear, therefore, that a knowledge of these changes will furnish a direct assistance in the classification of the *insaniæ*." Am I right in holding this position to be as good as the other, and as near truth? Neither of them are true inferences. They are both speculations taking the form of inductions from facts, but founded simply on the assertions and opinions of their authors. Now, this is a well-known mode of (so-called) reasoning used by the uneducated who can't see its unsoundness, or by the man who, knowing better, reasons with the ignorant. Supposing a critic of that time had said about Louis and Laenec's deductions, "day by day it becomes more apparent that we shall never accurately make out the pathological changes that occur in the lungs and heart which correspond with aberrations of the breathing, being still as far as ever from mounting a cough in a bottle of spirits, or from detecting palpitation in a spit-box. It is clear, therefore, that these changes can furnish no direct assistance in the classification of pulmonary and cardiac affections." What would now be thought of this bit of childish sophistry? Would it not simply be a standing record of the unwisdom of its author?

Founding on this inference, Dr. Browne goes on:—"We must be content to seize upon the signs and symbols of insanity, and by a thoughtful analysis and synthesis of these to distinguish, as well as may be, their cerebral starting points. There is no force, therefore, in Dr. Skae's objection to Esquirol's system that it is a classification of symptoms, and not diseases." Is not this arriving at one negative conclusion founded on a mere assertion, making use of it as if it were a positive fact, and then basing another conclusion upon it? Even supposing a pathological classification to be impossible, does that prove that nothing but a symptomatological one is possible? It is noteworthy that it was at that point it began to "dawn on 'the critic' that Dr. Skae's mind must have been in a state of confusion as to the real significance and relations of symptoms," because Skae had said symptoms were not diseases!

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Skae's illustration from fevers is next seized on. He evidently had selected this because, while definite groups of mental symptoms were present, yet all agree that they are not a sound basis of classification. Dr. Browne, apparently not seeing the real appropriateness of the illustration, makes a discovery. "Curiously enough, these fevers generally correspond with the kind of delirium which he has named." Of course they do. This was why Skae selected them. "It thus appears that the very symptoms which are dismissed as forming no foundation for division, guide to the very same division as is sanctioned by Dr. Skae." Just so; they "guide to it," but they have not, therefore, been taken by nosologists as the basis of the classification universally adopted. Skae, no doubt, thought that the term Acute Relapsing Mania might possibly "guide to" his "Insanity of Pubescence," but would scarcely, on that account, be held by any physician to express as much in regard to the origin, progress, and prognosis of any given case. Muttering delirium may occur in erysipelas or septicæmia, as well as in typhoid—surely a good reason for avoiding it as a basis of naming any of these diseases. But Dr. Browne then says that Skae's comparison is "disingenuous and indefensible," because he has taken a "*secondary* symptom" in fevers to compare with a "*primary* symptom" in insanity. He thinks that Skae ought to have taken the pyrexia. That would have been taking one kind of symptom in one disease to compare with a different kind in another. But apart from that, were fevers classified by the pyrexia, primary symptom though it is? Or by any one or series of symptoms, primary or secondary? Were they not classified according to an accurate clinical study of their origin, symptoms, course, terminations, and pathological anatomy? In other words, according to their "natural history?"

In the next seven sentences he makes as many statements as to the opinions and proceedings of Skae and his pupils, every one of which would be repudiated by them. When and where did they claim to be the first to insist on the truth that insanity is a disease of the brain? What Dr. Tuke said was, that Skae's "*nosology* was the first to *keep ever before us* the all-important principle that insanity is a disease of the body." Nowhere did they protest against any attempt to apply to insanity the same method of classification that has been applied to diseases of all other organs? or say that Esquirol's

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classification was unsound because it was "founded on clinical observations?" or say they gave the preference in classification to the fewest and most trivial attributes? or show that they "devote their attention wholly to these circumstances in insanity that have a minimum significance?" This is what some people would call a reckless mode of fastening the critic's own fancies on the shoulders of others, and a suitable ending to his preceding reasoning! The truth is, that Skae's words, in regard to Esquirol's classification are, "it is an excellent classification of *symptoms*, but not a classification at all of *diseases*, or *forms of insanity*." He agrees with Dr. Mitchell, that "we can never do without the old terms, *mania*, *monomania*, and *dementia*. They are useful for classifying patients; for a brief description of their state for practical purposes, such as indicating their mode of classification, the propriety of their isolation, and their mode of treatment; but to allow those terms to represent true *forms of mental disease*, is a scientific blunder." Now, I venture to say, that this expresses the whole truth in regard to the matter, in the opinion of nineteen out of twenty competent judges. Dr. Browne's defence of Esquirol was, therefore, unnecessary.

One test case settles this matter. Does Dr. Browne deny that general paralysis, with its alternations of mania, melancholia, and dementia, is a true cerebro-mental disease, a distinct clinical, symptomological, and pathological reality? If he admits this, how does he classify it among Esquirol's divisions? Of course he must admit that it cannot be done. The most distinct, the most real, the most undisputed, the truest cerebro-mental *disease*, therefore, cannot be provided for in the classification that he defends. Can anything more powerful be urged against our accepting it as final; or any stronger incentive be applied for us to invent a better?

But we come to the gist of the matter in hand when our critic says—"It might be that the old system is good . . . but that the new one is still better. Is this so? Is there anything especially excellent and commendable in Dr. Skae's classification?" At an early part of this critique he had answered this question, by saying that it was "philosophically unsound, scientifically inaccurate, and practically useless." At the twelfth page he begins to prove this. His arguments and objections in the fifteen pages that follow may be shortly stated thus:—

1. Skae's system has no principle of construction—no bottom.
2. It is of no service in practice.

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3. It withdraws attention from clinical observation.
4. It is incomplete.
5. There is no gradation, serial arrangement, or harmony in it.
6. It has a refractory ward.
7. Causes are confused with consequences.
8. Causes are assigned which are no causes.

Taking the first objection, as to the want of principle of construction and bottom, Skae himself said its principle was in accordance with the "natural history" of the disorders under consideration. I had ventured to define it as exclusively *somatic*, being thus distinguished from a *psychical* classification. Now I am not much concerned to defend my own definition. Its truth, or otherwise, does not affect the value of the classification. It may even deserve the scorn which the critic bestows on it; but I think Dr. Browne has not quite taken up my meaning. To take Dr. Browne's favourite illustration from plants: would it have been altogether a "magnificent *reductio ad absurdum*," if anyone, in pressing the natural system of classification on the acceptance of botanists, had defined it as the exclusion of everything connected with the number of the stamens and pistils of plants? Or, to take Dr. Browne's illustration on this very point from disease, when he asks "What should we know of neuralgia or of a stomach-ache but for subjective experiences? and yet these complaints are real enough, and piteously beg for assuagement." Now, I would say that neither the one nor the other is a "real" disease at all, but merely a symptom, and that the physician who, instead of attending exclusively to the "subjective experiences" of his patients, looked at their teeth, saw if they are pregnant or nursing, examined them for scirrhus of the duodenum, or obstruction of the bowels, or any other such lesion—such a man, I think, would be the most likely to assuage their pains, and come to a right prognosis, even though he should thus take an etiological method of going to work. If he had the experience and the insight still further to classify his neuralgias and his stomach-aches into epileptic, rheumatic, or malarious, hysterical, syphilitic, or metastatic, telling us how we could distinguish the one from the other, then, indeed, few would deny that he was a benefactor to humanity.

But, as I remarked, I am not careful to defend my definition. It was not sanctioned by Skae, and is merely negative, while his own, which I much prefer, is positive. When he said he had grouped the varieties of insanity "in accordance with the

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natural history of each," he expressly states that he used a phrase "more familiar to the physician's ear" than the "botanical term"—"natural orders, or families." His idea was not, therefore, the botanical, or even the biological one. It was the physician's. Now, either Dr. Crichton Browne has not yet attained to the physician's idea of grouping and classifying diseases, or he makes the assumption that Skae was a great botanist simply to point a sneer. Whenever he refers to it, he assumes that Skae's idea was the botanist's, viz., the analytical process of separating the characteristics of the plant into those of the class, and those of the class into those of the order, and those of the order into those of the genus, &c. Now, this method is not that on which a clinical physician works. His must be a synthetic process. He must first hear and mark each individual symptom of a disease: "a disease," I find myself saying, as if it were an entity, like a plant. It is, of course, no such thing, although Dr. Browne talks as if it were. A disease is merely, in nine cases out of ten, a creation built up by the physician out of individual symptoms related by the patient, out of the phenomena perceived by himself during life, and the appearances noticed after death. Causes of all kinds must come in, symptoms must be marshalled in order of occurrence, sequence, course, and duration; pathological appearances must be co-related with all these; and then the physician, with the generalizing faculty, constructs his fabric, and calls it "a disease." The "natural history" of this generalization is, of course, the sum of all the steps in the building up of the fabric. But all diseases have not yet attained the certainty and the completeness that belongs to such a typical generalization. Some of them are founded on generalizations of causes only, some on generalizations of symptoms, some on generalizations of both combined. It must be admitted by all, that a structure resting on one prop only is less surely built than one resting on two or three. Symptoms are doubtless very important, but to found generalizations in medicine on them, surely they must be taken into account from their origin to their disappearance. To take any one set of symptoms present at a particular time, and ticket them with a name, must necessarily be the crudest and most initial generalizing process in medical science. Esquirol's classification is of this character. If causes are taken into account, as well as symptoms, especially if the cause *which has the closest and most real relation to the disease* is selected out

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from the predisposing, exciting, and proximate causes present, and co-related with the *most distinctive symptoms present*, if, above all, pathological appearances are taken into account, whenever they are known, and so far as they are ascertained, and names given to such wider generalizations, surely this is an advance on the first plan. Skae's classification was formed on this principle. To say that a patient has "fever," is better than no information at all; to say that he has "malarious fever," is surely a deduction of greatly more value.

Dr. Browne, like some others, has got confused as to the real significance of Skae's nomenclature. Skae's nomenclature is, undoubtedly, founded chiefly on etiology; but this was because the symptoms of insanity are so changing, that a symptomological nomenclature would have been most difficult, even if there had not been the reason that the ground had been already occupied by Esquirol. But the name of a disease does not profess to take in all about it. If there is a distinct, definite train of cerebro-mental symptoms, having a relation to the tubercular diathesis, or to pulmonary consumption, and to an anæmic state of the brain with an irregular vascular supply, does it matter much whether it is called "phthisical insanity," or "dementia of suspicion," or "tubercular brain anæmia?" Skae, preferring a nomenclature descriptive of somatic conditions, showed both his insight and his experience, by selecting names for his varieties of insanity that conveyed an idea of the cause nearest related to, and most influencing the psychical disturbance.

Thus, Dr. Browne, not understanding what the natural history of a disease really means, and being led away by the nomenclature, or "basis," into supposing that Skae's system was an etiological one throughout, comes to the conclusion that he had found the least trustworthy foundation of all. But does he venture to assert that the causes of diseases are untrustworthy bases of grouping them, if we can find out the real causes? What is the morbid anatomy of disease, but one branch of its causation? Because he himself has confused "ideas as to the causation of disease," he seems to think that no one else can have more insight. He asks, helplessly—"How in cases of insanity is the information necessary to guide to an etiological classification to be obtained?" "How do we know that any case of insanity is post-connubial?" Why, from a consideration of its natural history, of course. He says, "Why should we not designate the case as one of melancholia?"

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Because that does not tell us the kind and course of either the mental depression or the motor or sensory neuroses; because, above all, that gives no indication that the way to cure the man is to tell him to be less uxorious, and to take oysters and champagne and nitro-muriatic acid.

What Dr. Browne says about the great difficulty of ascertaining the causes of the insanity from relatives is, for the most part, irrelevant; and if the physician has the knowledge or insight to seek for the cause or bodily condition having the most intimate relationship to the disease, and most affecting it, as I maintain Skae's system helps him to do, then it actually lessens the difficulty Dr. Browne complains of.

Let us next examine our critic's second objection, viz., that Skae's classification is of no service in practice. This, if proved, does, indeed, wound it mortally. He says, "if a physician imbued with this classification is unable, as I maintain he is, to place in their appropriate groups a number of cases of which full and correct histories have been given him, then it is, indeed, indisputable that this classification is a source of weakness and a snare." To show that this is so, he gives, what I presume he means to be, according to his own ideas on this point, "full and correct histories" of six cases. The whole six take up just three-quarters of a page. They are really the oases of fact in the desert of argument and assertion—the "half-pennyworth of bread to this intolerable deal of sack." He does not say that they were six consecutive cases; but we are rather led to infer that they were selected by him specially to show the inapplicability of Skae's classification. Let us, therefore, see if it is so entirely useless, even under these not very promising circumstances.

The first case had acute mental excitement of a delirious type coming on during an attack of acute rheumatism, lasting for some months, and accompanied by choreic movements. It was clearly a typical case of Skae's Rheumatic Insanity. Dr. Browne asks if it was that, or masturbational, or metastatic, or post-febrile, or choreic? Why, it surprises me that he cannot see that in that case there was one bodily condition which had the most direct relationship to the psychosis, which caused it, which influenced its phenomena so that they were of a special delirious type, which was the cause, also, of a special and most characteristic motor disturbance, viz., the chorea, which, if Dr. Browne had really studied Skae's system, would have enabled him to prognose its favourable termination,

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and which, above all, might have directed him to such treatment that his patient might have recovered in the time similar cases, published by Dr. Sander and myself, took to get well. That was the rheumatic diathesis, and the actual rheumatic condition present. I most confidently appeal to the unbiassed judgment of any medical man whether "rheumatic insanity" does not tell more about such a case than "acute mania?" I would desire no better instance of its practical value than this test-case, related in four lines, by an enemy of the system. Before that system was devised, the case might have been supposed to have some nearer relationship to the masturbation and the nervous temperament, two predisposing causes, no doubt; but it is almost inexcusable that at this time of day Dr. Browne should not have been able to fix on the rheumatism as the real mother of the psychosis.

Dr. Browne has been even more miserly of details in his second case than his first. He tells us nothing as to the nature of the mental symptoms of the patient at all, except that they were "recurrent attacks of excitement," while as to the motor symptoms present they are described simply as "convulsive seizures." Surely, Dr. Hughlings Jackson has not been to the West Riding Asylum lately, or we should have had some clinical facts about these convulsive seizures, where they arose, what muscles they affected, whether they were bilateral or unilateral, how long they lasted, whether they were followed by paralysis, or increased temperature, or cephalalgia, or double vision, or hallucinations of the senses, or the epileptic irritability. In the absence of some of these facts, the diagnosis of syphilitic insanity cannot be made, so that I should class the case as one of "Epileptic Insanity." The epilepsy might, no doubt, be traumatic; but as the greater includes the less, if an injury has caused typical epilepsy, and this is followed by insanity, then the latter is "Epileptic," if it has the characters described by Skae, and so well known to all alienists.

The third is, so far as the symptoms are given by Dr. Browne, one of amenorrhœal insanity, and would probably have recovered if the amenorrhœa, or its cause, had been removed.

The fourth, which in brevity exceeds all the rest—taking up less than two lines of print—was apparently one of climacteric insanity, the symptoms being aggravated by the preceding attack of gout. Nothing can show better how little Dr.

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Browne understands Skae's system, or how little real pains he has bestowed on its study, than that he should possibly suppose a case to be "podagrous insanity" whose chief mental symptom was depression, and which had occurred in the "weak and exhausted state after an attack of gout."

The two last cases, whose histories and antecedents were "undiscovered," and who both seem to have declined to give any account of themselves—the man believing that there was a conspiracy to poison him, and the woman being "silent"—need not have been insane at all, from Dr. Browne's description; and he cannot, therefore, fairly ask how they could have been classified under Skae's, or any other, system. Pray how did he classify them under Esquirol's?

The result of properly applying Skae's system to these cases is that much light is thrown on four out of the six, and the causes and bodily conditions that had the nearest relationship to the insanity, and influenced its symptoms most, are brought out.

It is a pity that Dr. Browne had stuck to his preconceived idea that Skae's system is a purely etiological one so closely, in relating those cases, that, though he gives so-called causes in abundance, and gets confused as to their significance, yet he takes no pains whatever to give clinical facts, mental or bodily. He makes a series of strong, but unsupported, assertions in the paragraph coming after his last case. One of the strongest of these is, "in no group is there any general indication for management, prediction, or treatment," or any "kindred features." Now, will he be good enough to point out any more definite group of mental and bodily symptoms than we see in his first case of rheumatic insanity? Or can he point out any variety of insanity that can be confused with it? Or any neurosis in which there are more particular indications for management, prediction, and treatment? The defence of Skae's system would, indeed, be an easy task, and its vindication complete, were all his varieties of insanity as definite as this one.

This brings me to the point at which it is proper to notice our critic's assertion that Skae and his pupils "give no such list of distinctive symptoms as would enable anyone to recognise and place a case of mental disease, apart from a knowledge of its history." Now Skae himself, and, I think, all his pupils, admitted that some of his varieties overlap each other, that some of them are not yet quite complete in their outlines, and

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that, in the case of some of them, the symptoms taken by themselves at any one time, without reference to their origin and sequence, or to their causation, would not enable a physician to determine which variety it belonged to. But the Skae's groups and varieties share this fault with many other forms of disease. Could a physician always diagnose a case of septicæmia from one of typhoid merely by seeing the symptoms at any given time? Or one of ague from one of relapsing fever from the same data? Or one of simple pneumonia from one of acute tuberculosis? And yet what maladies are more distinct than these? Mental symptoms, too, are so much less definite and distinctive than bodily, that it scarcely seems wonderful that they appear indefinite in description. When we know all about the physiology of the brain, and about that objectively studied psychology to which Dr. Browne alludes, then we shall be able to use a more exact terminology for our psychical abnormalities. And even if we suppose that some of Skae's minor varieties may not turn out to be real natural groups at all, surely that does not invalidate them all, or affect the principle of the system. But I appeal to the descriptions given by Skae, in the Morisonian lectures, of the symptoms present in the insanity of epilepsy, or of masturbation, or of pubesence, or hysterical insanity, to those of Dr. Batty Tuke of the symptoms in puerperal insanity and that of lactation; and I would even venture to adduce the symptoms mentioned by myself, as occurring in phthisical insanity, to show that these varieties of mental disease are not only true groups with a real natural history, but can be recognised by the mental symptoms only. Does Dr. Browne deny the facts of those cases, and the statistics there adduced? He makes many assertions on all subjects in his critique, but he never once denies that epileptics who become insane have really mental symptoms peculiar to themselves, and that cannot be distinguished by the terms mania, melancholia, or dementia. What does he purpose to call this psychosis? If in this case he must follow all writers on mental diseases and call it epileptic insanity, why should the same mode of naming real sequences and groups of associated mental symptoms not be adopted in other cases? And why, finally, not give Skae the credit for having been the first to see this, and to extend and illustrate it? Nay, I appeal to Dr. Browne himself before he made the discovery that this mode of describing varieties of insanity is of no service in practice. In the "British Medical Journal" for July, 1871, he published an excellent clinical lecture on a

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subject that he describes as "one well-marked type of madness to which the name of hysterical mania has been correctly assigned." Who assigned it this name but Skae, in his paper in 1864? Is Saul, then, among the prophets? Then, in the same journal for May 9th, 1874, he publishes another clinical lecture on "Senile Dementia," which, though the term may have been previously used, is a variety formed on Skae's principles, and is looking at a case from his point of view. Is Browne the clinical teacher when he wishes to describe the facts of mental disease to his students in the way that he considers most useful to them in practice, and up to the present state of knowledge on the subject—is he compelled to go to Skae's stand-point, to adopt Skae's principles; and even for a name to go to Skae's system, which Browne, the critic, now so "strenuously attacks," disparages, and "assails?" Truly this is of itself a psychological study.

In another place he says that there are not a dozen asylums in England where the classification is used. I believe I am far more correct in saying that there are not a dozen asylums in which many of its terms are not in use, and in which the somatic mode of looking at cases to which it gave so great an impulse does not prevail. If Dr. Browne himself can't resist its influence, surely that influence must be both strong and good. Could Dr. Browne give any proof (I should not be satisfied with an assertion, however positively given) that Esquirol's system was in use in a dozen asylums a dozen years after its promulgation? Did it never occur to him that to attack a thing with so much inconsistency, but with so little real art or force, may simply bring it into greater notice and favour?

Dr. Browne's third objection is, that Skae's system withdraws attention from clinical observations. To do him justice, he merely says this. He does not attempt to prove it, so that I need not seriously try to disprove it; but I would ask Dr. Browne, did Skae, in the illustrations of his varieties, go to nature for cases, or not? How can a system, based on the natural history of a disease, withdraw attention from the observation of the facts of nature? Have Skae's pupils been doctrinaires more than others, and wanting in clinical observation? Dr. Browne's reasoning, no doubt, still depended on his assumption that Skae's system was a purely etiological one, and on that he founded another assumption—that it *ought*, therefore, to withdraw attention from clinical facts. I would rather

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claim for it, as one of its greatest of merits, if not its pre-eminent merit, that it has directed more attention to the bodily and clinical symptoms of insanity than all other systems put together. If its place should be taken at some future time by another and more perfect system, this will still remain as its distinguishing merit. Its very essence is the observation of bodily symptoms, and the best proof of this is the rapid way in which its somatic nomenclature of mental diseases has spread into medical literature. Had Dr. Browne really caught its spirit in this respect, he would never have made the statement that "no sound principle can justify the distinction between puerperal insanity and the insanity of pregnancy." Why, let him look into Dr. Tuke's statistics, and see for himself the distinction as to occurrence, symptomatology, and prognosis, and into the works of the more recent obstetritians, who are very strong on the complete physiological and pathological distinctions between the two conditions. Sound practice, at all events, distinguishes between the two. If Skae was anything he was a clinicist; and to have it said that his system, which he founded on a life's experience of clinical facts, "withdraws attention from clinical facts," is enough to disturb him even now. It seems absolutely ludicrous, in its perversity, to say that the association of groups of mental symptoms with such conditions as rheumatism, gout, syphilis, or hysteria, withdraws attention from clinical facts as compared with calling them "acute mania."

The fourth objection, that there is no completeness in the system, applies, says our critic, by its inability to overtake all the cases we meet with. Now, I admit the proposition that the system is not yet complete, and so did Dr. Skae; but no one who has tried fairly to understand and apply the system, ever found that fifty per cent. of his cases were unprovided for; and no one who did not wish to caricature it would have made the heavy joke about the "insanity of the chloride of sodium." Its author said, with the modesty of a man who had looked at nature and found she was many-sided (and one involuntarily contrasts this with his critic's state of mind), "I by no means flatter myself it is a complete system;" but it "may culminate in a better—a more definite, and, at least, a more practical method than the one in present use." On turning to the report of the Carlisle Asylum for 1872, where I epitomised the results of ten years' experience (Table xxiii.), I find that out of 912 admissions, I had put down 108 as unclassifiable under Skae's

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system. This is at the rate of about 12 per cent. Considering the number of chronic dements and waifs and strays that come into a county asylum in ten years, with no history whatever, and no decided symptoms, this, I think, is not a large number.

The next objection we come to is, that there is no gradation, social arrangement, or harmony in it. This may be called the æsthetic objection to the system. My reply is, that diseases, when classified according to any natural system, seldom do gratify the tastes of those who desire to find such characteristics of biological classifications. If Dr. Browne's mind is set on such systems, he will find in Cullen, Mason Good, or Arnold, enough to gratify him. But, to quote his own words, "Disease, unfortunately, will not deport itself, according to our cut and dry notions, in a precise manner." "Indeed, as regards the functions of the nervous system, it would be vain to essay as precise a classification of them as we make of plants, animals, or minerals." "A classification of some sort is needful, and we need not be withheld from making one because our lines of partition will be artificial, vague, and shifting." It appears, therefore, "that if the so-called forms of insanity . . . merge into each other, that fact is much in their favour, and proves that they are conformable to the order of nature." Surely, our critic had forgotten those words, written in defence of Esquirol's system, when he is so severe on Skae's for not coming up to his ideal standard.

To show that Skae's groups can "associate themselves into general classes of higher generality," I need only refer to Dr. Batty Tuke's excellent paper in this Journal for July, 1870, in which he shows how naturally they fall into the "classes" of—1. Insanity resulting from assisted or impaired development of the brain, 2. Idiophrenic, 3. Sympathetic, 4. Anæmic, 5. Diathetic, and 7. Toxic insanity. In this way Dr. Tuke brought Dr. Skae's work into direct relationship with that of Van der Kolk and Morel, showing that the ideas of each of those men were harmonious with that of the other—each constituting an advance, from an independent point of view, on that of his predecessors—Skae clothing the skeletons, which the two others had framed, with flesh and blood. Dr. Tuke, in that paper, has refuted successfully, by anticipation, many of the objections which Dr. Browne now urges against Skae's system.

The objection that the system includes in it the class of "idiopathic insanity" which Dr. Browne calls a refractory ward, had been made by Maudsley and others before. Skae pro-

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fessed to found his system on the natural history of disease; and if the only circumstances in the natural history of the origin of certain cases of insanity to be found were sleeplessness, hereditary predisposition, and mental or moral causes, then why object to his calling such cases those of idiopathic insanity? He wished, above all things, to stick to the facts of nature, without regard to any predetermined principle, etiological or otherwise. And some people would say that it is a positive advantage for any system, devised before our knowledge of the facts is complete, to have a temporary niche in a convenient position in which to place our irreconcilables.

Dr. Browne's last two objections may be considered together, viz., that causes are confused with consequences; and causes are assigned which are no causes. Now, both of those statements, like many of the others, depend for their truth and force on the theory that Skae's system was entirely an etiological one—which he himself repudiated, and which no one, who looks at his list of groups, can believe. He merely said that the "basis" of his system was "essentially, though not entirely, an etiological one"—meaning that, in naming his groups, he had gone to etiology. Dr. Browne thinks that as "climacteric insanity (it will be observed that he uses the name as being a real thing) is dependent upon changes extending over several years rife with pathological risks, and it may, therefore, spring out of any one of a multiplicity of causes." This may be Dr. Browne's idea of "climacteric insanity." It was not Skae's, who never for a moment imagined that anyone could so misunderstand his system as to think that, if a woman became epileptic and insane between 44 and 50, her malady was to be called climacteric, any more than it should be so called if she, during an attack of rheumatism at that age, were to become delirious, have hallucinations of the senses, and choreic movements. But when arguing here our critic's mind was clearly in a state of bewilderment, for he expressly says, "Any woman attacked by mental disease, between the ages of 44 and 50, may, or may not, suffer from *climacteric insanity*." This is just what Skae would have said. In the very next sentence, however, he goes on—"To associate, as one form, all the varieties of mental disease that mark the epoch," &c. Skae did not do so, as his critic had clearly realised in the preceding sentence, by using the term "climacteric insanity" to express his idea of a definite group of mental symptoms. Skae's idea of this group was after all that of Dr. Browne, and not that attributed to him by

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Dr. Browne; and so, for once, they entirely agree. It is, of course, a mere matter of opinion, and a question as to which, I affirm, that Dr. Browne is no judge, from the animus he displays against the system, as to whether Skae's descriptions are hazy or clear. As to "rigorous definitions" being applicable to disease, such was Cullen's idea. What modern nosologist attempts it?

The critic says that probably the lung disease, in the cases of phthisical insanity, which I described in this Journal in 1864, was owing to hygienic defects in the Royal Edinburgh Asylum. If he had read the paper, he would have found the exact facts as to the commencement of the lung disease mentioned, and all the cases which had been long in the Asylum excluded. But exactitude as to facts is not his forte. He fails here to understand, too, that phthisical insanity is far more connected with the diathesis, than with the mere lung disease.

Determined not to give up his own idea of the etiological principle of the system, the critic says, "In several forms the etiological basis fixed upon is one which can have no genuine causal relation to disease. Lactation, for example, is a physiological process, and cannot of itself be the cause of insanity." On this principle neither child-birth, nor sexual intercourse, nor menstruation can be the cause of insanity, though all authors put them among its possible causes. But this occurs near the end of the critique; and some amount of mental exhaustion was excusable, after so severe an effort.

I now most unwillingly advert to the manner and tone of Dr. Browne's critique, and the better to illustrate this I shall quote a few of his expressions. In the first sentence he speaks with contempt of "all the classifications" of "recent times" by saying that we have been "afflicted" by them. We know that the chief of the men who have so afflicted us have been Van der Kolk, Morel, Bucknill, and Maudsley, in addition to Skae. After this I was prepared for much, but scarcely for such terms as these:—"stumbling and blundering," "hybrid-like this," "prodigious mixture," "piebald system," "hardly deserves criticism," "this system, like all other false systems," "meagre show of reasoning which these lectures present," "bungling," "end of his labours, turmoil and bewilderment," "who in his senses would make them a basis for classification," "no reasonable being could think of employing them in a practical classification," "Dr. Skae's system breaks down miserably and at once, and Dr. Skae himself helpless and forlorn," "such an exhibition," his reference to his friend who "survived the

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attempt" to apply the classification; and as the last, out of many others, I shall quote the assertion which occurs as a sequel and corollary to his theory that Skae and his pupils can't take an inside view of humanity:—"The physician who limits himself to an outside view of humanity must remain below the level of an intelligent dog!" Now I am not going to characterise such expressions and such a mode of conducting a scientific discussion. I leave them to the candid judgment of my professional brethren. I was, when reading them, tempted to think that a courteous and well-bred critic could not, and would not, have used such language. They surely don't strengthen Dr. Browne's arguments. Most happily such expressions are seldom employed except by imperfectly educated men, untrained in argument, who, in attempting to support bad causes, lose their tempers. I am, I confess, so jealous for that medico-psychological science of which Dr. Browne speaks with such contempt, that I am grieved that a gentleman occupying his position of grave responsibility should have added them to its literature. I am also surprised that a gentleman, who is not now writing his first juvenile essay for a debating society, but who has contributed copiously to the medical literature of the day, and who, above all, has stimulated his assistants to do good work, so that the institution over which he presides has become most favourably and widely known, should have put out of his hands such a performance. If cutting weapons had to be used—and why should they not?—one looked for a keen blade, deftly driven between the joints of the armour, and not a bludgeon swung wildly round the head. He should have remembered that many things had been said—and well said—courteously and earnestly against Skae's system; and what we now wanted to hear was something better said and more original.

One theory has occurred to me—the most charitable I can think of—and that was suggested by Dr. Browne's fear as to the "undoubted danger that it (the system) may be somewhat widely adopted" "among the younger brethren engaged in the study of insanity." The theory is not complimentary to those younger brethren; but they must blame Dr. Browne, not me. It is that he has written the critique not merely, as he says, to expose for them the deficiencies of Skae's system, but has modelled his arguments, and adapted his literary style to what he supposes to be their capacity and tastes. I can only say that my opinion as to the kind of milk suitable for the medico-psychological babes differs *in toto* from that of Dr. Browne. On this

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theory it is, perhaps, that the dogmatism of the critique is so strong and unwavering, the encyclopædic knowledge and mental philosophy so wide and deep as to be the envy and despair of the "younger brethren," not to speak of the seniors. The critic enters, without a halt, "the dark portals of metaphysics," and runs riot in its misty glades. He even condescends to state the real process of reasoning that should have led Skae to the construction of the system, and then touchingly says—"Alas! We should be wrong in ascribing to Dr. Skae as much logic as is involved in the above simple process." Ah! if this logician had only been at the procreation, what a progeny we should have had!

One other point I must allude to before I have done. Dr. Browne refers not only to Dr. Skae, but to the school which he founded, and the pupils that studied under him. He says that those pupils "perpetually parade" the system, "diligently vaunt" it, "obtrude it on attention," that its "great principles have been pronounced binding by an œcumenical council at Morningside, and he who profanely questions them places his promotion in jeopardy;" that they oppose the "study of mental symptoms," and have an "antipathy" to "everything mental;" and that "a philosophical problem is their detestation," &c., &c. He even ascribes to them Dr. Sankey's views as to the non-occurrence of primary mania. Now, these are a series of acts and sentiments ascribed to a number of gentlemen, not one of which would be acknowledged by them as correct. Can Dr. Browne prove any one of them? If not, I must take the liberty of repudiating them entirely. If those are the mere rhetorical embellishments of the critique, has not the Morningside school some reason to complain? Is it justifiable, in a scientific controversy, to employ such garniture?

Dr. Browne is quite right that Skae "would have been the last man to misinterpret the motives of any honest antagonist," and would have respected "hard-hitting, evenif directed against his own progeny;" but in a critique on a system whose author had entered the eternal silence, surely there was no room for noise and bluster, no provocation to envy and evil speaking.

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