suited to the wants of the animal system is evident, amongst other things, from the universal desire to mix amylaceous articles with fat, and *vice versa*, and from the presence of both fat and sugar in milk. As yet, we have not ascertained, by decisive experiments, the specific functions of each in the metamorphosis of tissue: Lehmann proposes, until this shall have been done, to take the mean constitution of human milk as the emblem of the combination of those four groups of bodies which is best adapted to the wants of the human organism—i.e., 10 parts of plastic matter, 10 of fat, 20 of sugar, and 0.6 of salts; but in considering this part of the subject, we must not forget that the proportions of the several species of aliment should vary as the wants of the system, under particular circumstances or at different times, may require, of which we have illustrations in the changes which the mammary secretion itself undergoes according to the age of the child, and in the variety presented by the milks of the sundry classes of animals.

REVIEW IX.

Epilepsy and other Affections of the Nervous System, which are marked by Tremor, Convulsion, or Spasm: their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M.D., &c., &c.—London, 1854. Svo. pp. 144.

DR. RADCLIFFE divides his work into two parts; the first treats of the physiology of muscular contraction; the second of the pathology, periodicity, and treatment of epilepsy, and of the affections which Dr. Radcliffe considers to be allied thereto. The physiology professes to be novel, and to be the basis of the pathology and therapeutics. It is wholly opposed to the current views entertained as to the nature of muscular contraction; we therefore propose to examine it critically, as well as the pathological and therapeutical views by which it is followed; moved thereto, however, rather by a feeling of regard for the author, than by a high estimate of the validity or importance of his doctrines.

Dr. Radcliffe is of opinion that the common doctrine of muscular contraction is erroneous. It is generally believed that it is induced by certain stimuli; such a supposition does not, he thinks, bear the test of examination. The condition of the muscular system known as *rigor mortis*, and the "contraction which occurs in the muscle-like tissue of the ductos under the influence of cold," especially suggested doubts to Dr. Radcliffe as to its accuracy. Dr. Radcliffe's hypothesis is this:

"That all stimulants, vital and physiological, antagonise muscular contraction; and that contraction happens from ordinary molecular attraction, when the muscle is not stimulated."

Or, in other words of the author:

"Because some stimulus has been removed which had prevented the natural molecular attraction of the tissue from coming into play, when the phenomenon becomes analogous to the contraction which takes place in a bar of metal, or in any other inorganic body, on the abstraction of heat."

By this hypothesis the contraction of muscle, the movements of the

blood in vessels independently of the heart, and the action of the heart, receive a (to Dr. Radcliffe) satisfactory "physical explanation."

Dr. Radcliffe builds up an obscure hypothesis on premises still more hypothetical. We will give illustrations of his method of procedure in establishing his doctrines.

"A muscle is said to be irritated, to contract by the simple contact of certain natural or artificial substances—the bowel by the food, a detached fibre by the point of a needle—how is this? What is the influence at work, and what is the manner of the operation? It is difficult to acquiesce in the common belief that the hollow involuntary muscles are excited to contract by their natural contents. If the morsel exerts this influence in the gullet, how can it effect an entrance? Or, if effecting this, how is it that it is not fixed immovably in one place? There is no physical necessity why the movements should follow in any certain and definite order. On the contrary, the morsel may move upward or downward indifferently, as it is seen to do in the throat of a cow during rumination. Indeed, so far from exciting contraction, it appears as if the food remained quietly in the stomach, and in every other part of the alimentary canal, until the digestive process is complete, and that contraction *happens* when those molecular changes are at an end which could have acted as a stimulus to the muscle." (p. 9.)

The entire difficulty encountered by our author arises, we need hardly say, from the circumstance, that he has not rightly comprehended the physiology of deglutition and of peristaltic action. It is very well known that there *is* a necessity, not physical, but *vital*, why the movements of the apparatus called into action during the process should follow in a certain and definite order. It is an essential point, indeed, in all vital processes, that they go on necessarily, according to a pre-ordained and fixed method and plan. Science investigates this order: hence physiologists have traced out the existence of a covering to these hollow involuntary muscles, of incident excitator nerves distributed through and upon that covering, of nerve-trunks taking their course to the central axis where the co-ordinating apparatus is placed, and of other nerve-fibrils coming from that axis and distributed to the muscles which they regulate; all these take their share in the act of deglutition; but the "natural contents" are never in "simple contact" with the hollow and involuntary muscles, which perform by their pre-arranged and combined action the act of deglutition. Nor is it in accordance with the facts elucidated by physiological research to say, that it appears as if the food remained quietly in the stomach until the digestive process is complete, for observations prove exactly the contrary.

We need not multiply extracts by way of illustrating Dr. Radcliffe's method, for his other arguments do not differ in weight or method from the preceding. The following summary of his first chapter ends, however, with a *chemical* argument, as facile and as insequential as his physiological; we therefore subjoin it:

"Contraction, then, as seen in ordinary muscle, would appear to be analogous to that contraction which takes place in inorganic bodies on the abstraction of heat, with this only difference, that more forces have to be abstracted from the organic than from the inorganic body. The analogy is indeed perfect, for even that remarkable degree of contraction which is witnessed in muscle, as compared with that which is seen in inorganic bodies, may be a natural consequence of the physical constitution of muscle: for as muscle is composed almost exclusively of

certain gaseous elements, it *may* contract to a great degree under a small abstraction of heat, because it is the law of its constituent gases so to contract." (p. 24.)

In chapter second, muscular contraction, as manifested in the coats of vessels, is elucidated. Heat generated during nutrition and innervation expands the vessels.

"If heat, and the agents associated with heat, excite expansion in the vessels, it is easy to understand how they must give rise to a force—a '*capillary force*'—which is independent of the heart; and how they must co-operate with the heart in facilitating the progress of the blood through the vessels of the living animal, in precisely the same way as the warm-bath co-operates with the injecting-syringe of the anatomist in procuring the admission of the melted wax into the vessels of the dead animal. It would be different if the vessels acted upon were composed of ordinary solids, for then the heat would cause a greater degree of expansion in the fluids contained within the vessels, than in the vessels themselves; but in reality the vessels are gases, *coerced* into solidity for the time, rather than ordinary solids." (p. 27.)

We never before suspected that the "subject" to be injected was put into warm water for any other purpose than to prevent the melted wax becoming solid too soon, by the abstraction of its heat; and as to the vessels being constituted of *coerced* gases, we would ask, why are they more so than water itself, or albumen, or the *materies* of the blood-corpuscles?

The third chapter treats of "muscular contraction as manifested in the heart;" and here we have a repetition of assumptions as groundless as they well can be. We subjoin an illustration:

"There is reason, also, for supposing that the systole is always contemporaneous with a lessened supply of nervous influence, and not with an increased supply. The blood escapes from the heart in gushes; and there is some reason for believing that the nervous energy, in some parts at least, is generated in corresponding gushes. At all events, the blood passes by gushes into the great cerebro-spinal masses, and causes corresponding pulsations in them whenever they are exposed to sight. It may further be supposed that nervous influence is developed and distributed the moment the blood comes in contact with the organ in which it originates, for this influence is sudden and subtle as electricity. All this may be supposed, and, if so, then the heart will be supplied with the gush of nervous influence, not during the systole, but during the diastole." (p. 32.)

There is no more interesting study than the circulation of the blood through the capillaries; and when the difficulty of the study is considered, there is no branch of physiology in which greater progress has been made. This we owe principally to microscopic research. Never yet, we believe, has the blood been seen flowing through the capillaries by "gushes," nor is there a single phenomenon known which would indicate any such condition of the capillary circulation. We have seen how, in the last paragraph, "all this may be *supposed*." In a page or two, these pure (and unfounded) suppositions assume the shape of "the rational and physical explanation of the heart's rhythm," as if they were demonstrated propositions.

"The heart contracts, and sends a flood of blood to the great sources of innervation. There the blood originates a gush of nervous influence, which, passing along the nerves to the heart, causes the diastole. The diastole cuts off the arterial stream, and the blood sets in from the auricles and veins. The cutting off of the arterial stream interrupts the development of nervous influence, and this interruption, reacting upon the heart, removes the influence which had caused the

diastole, and the systole returns. And so round the same circle. The arterial stream sets out at the systole, and develops the nervous influence at its proper sources, and the nervous influence inducing the diastole causes the systole by cutting off the supply of blood, and by thus suspending for a time the active development of nervous influence. And thus in a regular series systole gives rise to diastole, and diastole is followed by systole, so long as the vascular and nervous systems retain their integrity. This is *one* part of the process." (p. 35.)

It certainly is not. There are no "gushes of nervous influence," and no "cutting off" of the gushes. The whole scheme is purely imaginary.

To determine the *PATHOLOGY* of epilepsy, Dr. Radcliffe examines "the real state of the great systems chiefly concerned—the vascular, the nervous, and the muscular." In all he finds want of tone. In the vascular system, "all the symptoms are found to offer a direct contrast to those symptoms of plethora which are met with in the butcher." As to the nervous system, all the facts declare the same want of vital activity. As to the muscular, its condition he finds to agree with that of the vascular and nervous systems. Dr. Radcliffe finds—

"Everything is in harmony with the physiological premises; and, as might be anticipated from these premises, the convulsion would seem to depend upon *want* of vital stimulation, which want had allowed the molecular attraction of the muscles to come into play, and gain the ascendancy."

Dr. Radcliffe is silent as to the etiology, whether of the predisposition or of the paroxysm.

The affections which are considered as "allied" to epilepsy, are, in fact, neither more nor less than incongruous groups of diseases, in which there is the common symptoms of irregular muscular action, chorea, delirium tremens, poisoning by mercury, hydrocyanic acid, lead, &c.; fever, retention of urea in the blood, tetanus, hydrophobia, &c., are considered as allied affections, in so far as the muscular system is affected. In all, Dr. Radcliffe finds a corroboration of his views.

"In conclusion, therefore, it must be admitted, that the entire history of epilepsy, and of affections allied to epilepsy, is at complete variance with the idea that the muscles are provoked to excessive contraction by excessive stimulation. It is as much at variance with this hypothesis as it is in harmony with that doctrine of muscular contraction which was propounded at the commencement of this inquiry, which doctrine is—that all stimulants, vital and physical, antagonize muscular contraction, and that contraction is brought about by ordinary molecular attraction, when the muscle is *not* stimulated. This doctrine, indeed, supplies the key to the pathology, and the facts belonging to the pathology furnish the only proofs which were wanting of the truth and universality of the doctrine." (p. 108.)

The chapter on periodicity communicates nothing new. Dr. Radcliffe refers the reader for information as to diurnal, monthly, and annual periodicity, either to personal observation, or to "the admirable little treatise of Dr. Mead, '*De Imperio solis ac lunæ in corpora humana et morbis inde animalis*.'"^{*}

The chapter on the treatment of epilepsy and the allied affections, needs no special criticism. Dr. Radcliffe recommends a tonic and stimulant plan of treatment; a nutritious and animalized diet; a liberal allowance

* We need hardly state that a little library of books on the subject has been written since the date of Mead's essay; and that it has received a large development through natural history on the one hand, and meteorology on the other.

of wine or beer; coffee, to the exclusion of tea; celibacy; quinine, iron, turpentine, camphor, chloric æther; counter-irritants; hot baths, one every day, &c.

"In actual practice, I have rung changes upon these different stimulants, either giving them alone, or combining them with iron or quinine, substituting one for another, according to the changing circumstances of the case, and always allowing, at the same time, a liberal supply of dietetic stimulants—upon which, indeed, hope is mainly to be based; and I have had every reason to be satisfied with the results. I have never met with a patient who has not been benefited; for even where the case has been of long standing, and the fits have kept their ground, there has been a manifest diminution of intellectual torpor, the face had lost a good deal of the brutalized expression which had been creeping over it, and the distressing nervous headache has disappeared, if that symptom had been present; and I have met with many patients who have been completely cured." (p. 130.)

Dr. Radcliffe has grounds for congratulation as to the success of his treatment, whatever may be thought of his hypotheses. If he had occupied his pages with details of some of the cases he has so happily cured (or if not cured, at least relieved), his work would have had a practical value which it does not possess in its present form. Empirical knowledge of this kind is very estimable in the absence of information as to the true nature of epilepsy—we mean histories of carefully-observed cases; for it is mainly by such histories that the circumstances under which remedies may be prescribed with some reasonable hope of success, can be ascertained. We are sincerely sorry that we cannot express a more favourable opinion of Dr. Radcliffe's little work; for we are assured that he is a gentleman of personal worth and an estimable character. We should, however, do a wrong to our readers, if we were to say more of it than it deserves.

REVIEW X.

Cases of Bright's Disease, with Remarks. By SAMUEL WILKS, M.D. Lond.
(*'Guy's Hospital Reports,'* Second Series, vol. viii.)

IN a previous number* of this journal, we endeavoured to correct what we hold to be the errors of those pathologists who see, in the various abnormal states of kidney which are commonly included under the term "Bright's disease," only successive stages of a single morbid process; the continual tendency of the disease being, as they suppose, to produce that small, contracted, granular kidney, the weight of which is, perhaps, by one-half, or even two-thirds, less than that of the healthy organ. We adduced two classes of facts which appear to us to be quite irreconcilable with the theory of the *oneness* of Bright's disease—these are, *first*, the anatomical characters of the diseased organ, as revealed by a post-mortem examination; and, *secondly*, the condition of the urine, and the attendant symptoms, observable during the lifetime of the patient. If, by an edict of arbitrary power, any pathologist were restricted to the observation of one of these classes of facts, he would labour under a great disadvantage, and he might be expected for a long time to halt between

* Jan. 1853, vol. xi. p. 56.