

ception of an object not actually present, involving the recognition of its absence, an illusion is the subjective perception of an object actually present, but in characters which the object does not really possess. With appropriate alterations these definitions will cover the abstract hallucination and phantastic illusion of Wundt as well.

In his second chapter, the author ably, but we believe unsuccessfully, endeavors to defend his refusal to recognize the distinction between illusion and hallucination as the leading principle of classification, though he admits the necessity of making this distinction in accordance with the leading alienists. Wundt, an authority whose teachings in psychological physiology the author of the present volume has most successfully assimilated, has drawn attention to the numerous connecting links existing between illusions and hallucinations, and yet strongly insists on utilizing their general differences as a basis of classification. We find the chief drawback to the otherwise great value of the work, in its failure to give adequate space to the anatomical mechanism concerned in false registrations of the perceptual and conceptional sphere. If it be borne in mind that while even hallucinations may be based on actual impressions, the latter are not the determining factor of the hallucination, the difficulty in discriminating between these perversions is overcome; this is illustrated by the occasional persistence of dream-images in the waking state, and the moving of certain hallucinated images consonant with the movements of the eye-ball. If an actual or subjective impression, say in the shape of *chromatopsia* and *tinnitus*, be granted to exist in a subject hallucinating the vision and voice of the Virgin Mary, it will be instantly recognized by every observant alienist, that the real determining factor is here centrifugal, while in the illusion, which constructs, out of a ball rolling in an ill-lighted apartment, a mouse, the determining factor takes a centripetal course. In the former instance, the misinterpretation lies ready made in the Cortex, and seizes on the slight external pretext, whose existence we only admit for the sake of the argument, to incorporate it, in its substance; in the latter, it is based upon an imperfect registration and a gradual constructive interpreting process. Nothing could more forcibly illustrate the correctness of these propositions than the very case cited from Wundt by Mr. Sully of a forester who saw the real objects of the outer world, (furniture and tapestry, for example,) through the wood piles which formed the subjects of his hallucinations.

With these remarks on the propositions of the opening chapters, our criticism ceases to be adverse. In the last twelve chapters of the book, the author gives

a concise review of the chief theories held by alienists and metaphysicians on the perceptual illusion, the introspective illusion, dreams illusions of memory, and those of belief. We refrain from again pointing out places where the author encroaches on the fields of delusion and hallucination, as he has given a wider scope to his definition of the illusion, than we are inclined to consider proper. It is but just to say that he gives a just interpretation to the views of alienists, an interpretation which only occasionally manifests that tincture of uncertainty which is unavoidable on the part of one devoid of a practical knowledge of the insane.

The perusal of this work cannot fail to be profitable to the student of mental pathology as well as of metaphysics. More reliable in the latter field, than in the former, it is yet a successful attempt to present the modern German ideas on the subject, and to combine the teachings of the practical and the abstract psychologists. To the general reader we can only repeat, what we said at the outset, it is the clearest rendition of a difficult yet fascinating theme, to be found in our language.

E. C. SPITZKA, M. D.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. BURDON-SANDERSON, M.D., LL.D., F.R.S.

The two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shop of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed. The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of

which it is the effect or outcome? And (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labors. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e. g.* J. Müller—recognized that the knowledge they possessed, both of vital and physical phenomena, was insufficient to refer both to a common measure. The method, therefore, was to study the process of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly to the great physiologists still living and working among us, who were the pupils of J. Müller—*viz.*; Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise¹ on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discovery in physics, there can be no doubt, that as a physiologist, he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavor shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine in which fuel is continually being used on the one hand, and work is being done and heat

produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely muscle. Although, he said, a muscle acts under the direction of the will, it does not derive its power of acting from the will, any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i. e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscles of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and re-integrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process, either in the whole animal body or in a single muscle, during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,¹ continued by Pettenkofer and Voit,² and by the agricultural physiologists³ with reference to herbivora, they are not by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfactorily worked out, but that the quantitative relations of heat production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are

¹ Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig, 1852.

² Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866-80.

³ Henneberg and Stohmann, "Beiträge zur Begründung einer rationalen Fütterung der Wiederkäuer," Brunswick and Göttingen, 1860-70.

¹ J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhang mit dem Stoffwechsel; ein Beitrag zur Naturkunde," Heilbronn, 1845.

as certain as of any physical fact that the animal body in doing work does not use its own material—that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he is unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology—*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true “*Oel in der Flamme des Lebens*” is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately—has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendancy, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—*viz.*, first, the investigation of the properties of the coloring matter of the blood, since called “*hæmoglobin*,” the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a

barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The color of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the “*blood-pump*,” as it is called, we have learned a great many facts relating to the nature of respiration, particularly that the difference of venous arterial blood depends not on the presence of “*effete matter*,” as used to be thought, but on the less amount of oxygen held by its coloring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.¹ But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learned that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i. e.*, to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localized oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all,

that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "*Elementa Physiologiae*" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavored to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the provisions of J. R. Mayer, and that what we have learnt with so much labor by experiments on animals is but the realization of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the provisions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favorable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realization; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you today may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of diseases. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

METEORIC DUST.

BY PROF. SCHUSTER.

A committee of the British Association was appointed for the double purpose of examining the observations hitherto recorded on the subject of meteoric dust and of discussing the possibility of future more systematic investigations. With regard to the first point we note that in a paper presented to the Royal Astronomical Society in 1879, Mr. Ranyard has given what appears to be a pretty complete account of the known observations as to the presence of meteoric dust in the atmosphere. It appears that in the year 1852 Prof. Andrews found native iron in the basalt of the Giant's Causeway. Nordenskjöld found particles of iron which in all probability had a cosmic origin in the snows of Finland and in the ice-fields of the Arctic regions. Dr. T. L. Phipson, and more recently Tissandier, found similar particles deposited by the winds on plates exposed in different localities. Finally, Mr. John Murray discovered magnetic particles raised from deposits at the bottom of the sea by H. M. S. *Challenger*. These articles were examined by Prof. Alexander Herschel, who agreed with Mr. Murray in ascribing a cosmic origin to them. For fuller details and all references we must refer to Mr. Ranyard's paper. There cannot be any doubt that magnetic dust, which in all probability derives its origin from meteors, has often been observed, and the question arises, in what way we can increase our knowledge on these points to an appreciable extent. A further series of occasional observations would in all probability lead to no result of great value, unless they were carried on for a great length of time in suitable places. Meteoric dust, we know, does fall, and observations ought if possible to be directed rather towards an approximate estimate of the quantity which falls within a given time. Difficulties very likely will be found in the determination of the locality in which the observations should be conducted. The place ought to be sheltered as much as possible against any ordinary dust not of meteoric origin. The lonely spots best fitted for these observations are generally accessible to occasional experiments only, and do not lend themselves easily to a regular series of observations. Nevertheless experiments continued for a few months at some elevated spot in the Alps might lead to valuable results. The Committee would like to draw attention to an instrument which is well fitted for such observations. It was devised by Dr. Pierre Miquel for the purpose of examining, not the meteoric particles, but organic and organized matters floating about in the air. A description, with illustrations, will be found in the *Annuaire de Montsouris* for 1879. Two forms of the instrument are given. In the first form, which is only adapted to permanent places of observations, an aspirator draws a quantity of air through a fine hole. The air impinges on a plate coated with glycerine, which retains all solid matter. By means of this instrument we may determine the quantity of solid particles within a given volume of air.