Remarks ON THE MOLECULAR THEORY OF ORGANISATION: IN REPLY TO DR. BEALE.

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I HAVE carefully considered the second communication of Dr. Beale, denominated "facts and arguments opposed to my theory of organisation"; and with every desire to arrive at such facts and understand the arguments, I can see little of either one or the other that bears on the subject. "I am not aware," says Dr. Beale, "of any observations by Dr. Bennett or by any other observer, which tend in any way to show that the ovum is formed by the coalescence of the molecules of the ovary, or that spermatozooids and germinal vesicles disintegrate to form molecules." On these points, I refer Dr. Beale to the writings and figures of Dr. Martin Barry, Meissner, Nelson, Keber, and others. I do not wish him to regard their expressed opinions or theories; but the facts they have observed, and some of which I copied in my first lecture in the *Lancet*—Figs. 9, 10, 13, 14, and 15. If my interpretation of these facts be wrong, would it not be better for Dr. Beale, instead of calling such statements vague, and affecting to doubt whether I mean what I have deliberately stated, to point out the error in detail, and describe how they are otherwise to be explained?

Dr. Beale says he can see no logical connexion between my sentences; that I combine ideas which are quite incompatible with one another, and make assertions so opposite to each other that both cannot be correct. I should have liked Dr. Beale to have pointed out more particularly in what manner I have thus offended. Although it is possible I may have expressed myself obscurely, I have not generally been accused of either speaking or writing unintelligibly. Besides, what I wish to impress upon Dr. Beale is that, even supposing him to be correct in these statements, they do not constitute either facts or arguments on his side adverse to my theory, as the title of his paper would lead us to suppose.

Again, may not Dr. Beale's avowed incapability of comprehending my views originate in the strong bias he possesses in favour of his own doctrines, in consequence of which he is unable to regard mine from an independent point of view? If so, he cannot be expected either to criticise or understand them correctly.

"If," says Dr. Beale, "the matter is in a state of solution when it comes into contact with the living mass, there can be no juxtaposition of molecules." I ask why not? How can solid matters grow or increase in extent and density, unless from that very solution solid molecules are thrown down which, by aggregation, produce bulk? I have given many observations in my first lecture where this was

seen to occur in viscous organic and inorganic fluids. What fact or argument has Dr. Beale advanced to refute them? Dr. Beale doubtless considers himself justified in thinking "that living matter must *always* increase in the same manner, and living particles must *always* multiply in the same way." But the reiteration of this opinion is no proof of its truth; still less is it fact or argument in opposition to my theory.

Dr. Beale has given a series of figures to show what no reasonable person will dispute; viz., that a minute body viewed with a magnifying power of 3000 diameters linear appears proportionally larger than the same body when magnified only 1800, 700, and 250 diameters. As an illustration of the utility of employing high powers, these figures must be regarded as unfortunate, seeing that no further information is to be derived from inspecting a large circle than from regarding a small one. I am quite prepared, however, to employ these high powers the moment Dr. Beale tells us he can by their aid see in molecules anything that has escaped previous observers.

Dr. Beale's figures of vibriones are quite correct, so far as the imperfect printing of his woodcuts will allow me to judge. He does not say that he has seen them in the act of dividing; and until he does so their supposed division must remain a mere opinion. He declares, however, that he can prove, by direct observation, that the smaller molecules do not coalesce or become aggregated to form larger masses. He also affirms that this process cannot occur in the case of particles so large as those figured by me. Now, if Dr. Beale can prove these statements, why does he not do so, instead of making affirmations? I am of the contrary opinion, and shall state the grounds from which it is derived. The question, however, is not whether molecules unite to form masses—although if a mass of molecules exist, it must be clear that they are aggregated together—but whether molecules unite to form the vibriones.

On making an infusion of flesh, and allowing it to putrefy, there will sooner or later appear, on the surface of the fluid, a delicate pellicle or scum. On examining this with a power of 800 diameters (which I have recently done, to satisfy Dr. Beale), it will be seen to present the structures figured below.

Fig. 1.—a. Structure of the scum on a putrid animal infusion on its first appearance. b. Structure of the scum six hours afterwards. The molecules separated. c. Structure of the scum on the second day. d. The same scum broken up. e. Filaments in the scum on the third and fourth days. (800 diameters linear.)

Fig. 2.—Presumed mode of developments of vibriones. 1. Primary molecules. 2. Larger molecules subsequently formed. 3. Aggregation of two molecules. 4. Their union. 5. Further coalescence of molecules producing vibratile filaments. 6. Still further aggregation, and coalescence of molecules in rows, to produce the highest degree of vibrional development. (800 diameters linear.)

The mode of development of the filaments, as derived from these appearances, is differently described

by Dr. Beale and myself. He says the molecules increase by absorption till they arrive at a certain size, and then divide. How he explains the formation of the filaments I do not know. But the molecules are formed first, and the filaments last; hence the latter must be produced from the former, in some way. To render my views intelligible, I have numbered what I conceive to be the different stages of growth; and I would ask Dr. Beale if, when the development has reached No. 2, the molecules divide, how it is the filaments can form at all. Surely a constant division of molecules at that period would be opposed to the formation of Nos. 4, 5, and 6. But it may frequently be seen that No. 3 is composed of molecules of exactly the same size as those of No. 2, which are floating loose—a fact in favour of the molecules coalescing, rather than dividing, as then they would be half their original size. My view, therefore, is, that the smaller molecules, although they may increase by imbibition of fluids, have yet a constant tendency to aggregate together and melt into one another. No. 3 is not a proof of No. 2 dividing, but of two molecules coalescing; and when they unite, they form No. 4. Two or more of these uniting form Nos. 5 and 6. When a similar process to this goes on in mineral bodies, as shown by Mr. Rainey, it cannot suggest division, but union; and this for the obvious reason, that the former would lead to disintegration; whereas it can be seen in one case, as in the other, that development is the result. In short, in the same manner as a tube is formed by a coalescence of cells, so is this minute vibratile filament formed by the coalescence of molecules. It may be argued, however, that each molecule elongates itself; that is, No. 2 is converted into No. 4; this into Nos. 5 and 6; and that No. 3 are spores or ova, caused by the disintegration of No. 6. This view, however, is opposed by the fact that Nos. 1, 2, and 3 can be seen *before* Nos. 4, 5, and 6 are produced. Of this I have satisfied myself by frequent examinations of animal and vegetable infusions; and cannot, therefore, resist the conclusion that the development is such as I have described.

If, as Dr. Beale alleges, every one of my future lectures is likely to contain as many statements open to discussion as those which have appeared, it strengthens my conviction in the propriety of delaying further controversy until all are published. For although the task of criticism, as he thinks, would then be too great, I cannot help thinking that the labour of reply would be much diminished. Unless Dr. Beale, therefore, abandons generalities and assertions, and becomes more particular in his "facts and arguments", I cannot promise to continue the explanations to which he invites me.

DISCOVERY OF SKELETONS. A sensation has been created at Guildford, and for several miles round, in consequence of the discovery of five skeletons on a piece of land about a mile from Guildford. The skeletons were discovered by a man who has been trenching the ground and getting flints. They were each lying in a hole, or sort of rough vault, dug out of the chalk. On the breast of one was a dagger or knife. The bones were much decayed, but one of the skulls was in a remarkable state of preservation, as were the whole of the teeth. The skeletons have been examined by Mr. Ross, surgeon, of Guildford, who has given it as his opinion that two were old men, two middle-aged, and one a young man. The bodies were all lying east and west.

Short Clinical Lectures

ON

THE FIRST PRINCIPLES OF MEDICINE.

Delivered at King's College Hospital.

BY

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I.—SOME OBSERVATIONS ON THE NATURE OF CERTAIN HEALTHY AND MORBID CHANGES, AND ON COUNTERIRRITATION.

[Concluded from page 184.]

COUNTERIRRITATION has nothing whatever to do with allopathy, or any other special system. It rests upon a principle which, I shall now endeavour to show, is thoroughly sound, and confirmed by some of the most recent and most exact observations.

Let us, then, consider how a blister applied to the skin can produce any beneficial effect in a case where a quantity of pus, mucus, and fluid, are being formed upon the surface of the mucous membrane of the bronchial tubes—in a case of chronic catarrh. I shall not discuss the effects of moxa, the actual cautery, and the more violent forms of counter-irritation, in this lecture.

As to the above morbid condition, we know—

1. That the vessels generally of the mucous membrane are congested; that it contains more blood than in the healthy state. The muscular walls of the small arteries are relaxed; the elastic capillaries distended with blood. Their thin, transparent, permeable walls, being stretched, are necessarily thinner, and more permeable to certain fluid parts of the blood. A greater quantity of soluble matters, therefore, escapes through the vascular walls than in a normal state.

2. Much of this increased quantity of fluid is taken up by the epithelial cells. The young cells especially, being supplied with a greater quantity of nutrient matter, grow faster. The masses of germinal matter (nuclei) increase, divide, and subdivide; and the process proceeds so rapidly, that there is no time for the conversion of the oldest outer portion into the formed matter corresponding to the wall of the cell, as always occurs in normal nutrition. A vast number of imperfectly formed epithelial cells and pus-cuscles result. The more or less viscid material which surrounds these is all that represents the more hardened substance which forms the walls of epithelial cells generally, and gives to the healthy structure its characteristic properties and regularity of arrangement.

With regard to the influence of counterirritation, we must bear in mind—

1. That, generally, the afferent nerves distributed to the skin may influence, through the centre into which they are implanted, the efferent fibres distributed to the vessels of an organ lying beneath and perhaps at a considerable distance. It is possible, therefore, that the calibre of the small arteries dis-