Remarks ON THE MOLECULAR THEORY OF ORGANISATION:

IN REPLY TO DR. BEALE.

BY

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DR. BEALE concluded his first paper with the following passage:—"But to limit the discussion to one point, the formation of vibriones. Dr. Bennett maintains that several of the molecules forming the scum unite to form minute filaments. I do not think that any one has ever seen two such living molecules together." I hope to have satisfied Dr. Beale in my last communication that not only are two such molecules frequently seen together, but that this is so common an occurrence as to warrant the conclusion that their union produces the filaments. In reply, he now says:—"I propose to devote a separate communication to the important question of the development of vibriones, and I thank Dr. Bennett for expressing his views upon this point so definitely."

Instead of limiting the discussion, however, to this point, Dr. Beale has seized the opportunity to introduce all kinds of queries, mingled with many remarkable assertions as to vitality, in none of which can I agree with him. It forms no part of my intention to controvert the peculiar views of Dr. Beale. I think they may be safely left to the judgment of physiologists. I shall only comment on the following statement:—"A molecule of oil, or phosphate of lime, may result from disintegration; but as such molecule, it can never live. A *living molecule* of oil, phosphate of lime, or any other substance having a definite known composition, is a simple impossibility."

Now, what is it that transforms cartilage into bone, if it be not the regular deposition of mineral molecules into its intercellular substance—a substance maintained by Dr. Beale to be incapable of living? If this is not a living process, what can be such? To say that it is mere infiltration of mineral matter is no argument against its vital character; for in the same manner all growth and nutrition consist of infiltrated matter. Neither is it any argument to say such matters are first in solution. It is only when molecular formations are produced from such solutions, that we have any evidence of their vital nature, as in the molecular substance of bone and muscle. Again, to maintain that, be-

cause the composition of the molecules in a texture is now known, *therefore* such molecules can never live, is certainly a curious proposition, and must lead Dr. Beale to support the paradox that nothing lives in a living structure. Lastly, to say of the theory of an opponent that "it is a simple impossibility" is in science the weakest controversial expedient.

Dr. Beale says, "It is a simple assertion, and I maintain, unsupported by evidence, to say 'the molecules of the ovary first coalesce to form the ovum.'" Thereupon he proceeds to ask no less than nine questions, the reply to which, in order to be satisfactory, would require me to write a volume. To save myself this trouble, when Dr. Beale on a previous occasion stated less positively that he was not aware of any observations that bore on this subject, I referred him to the writings of Martin Barry, Meissner, Nelson, Keber, and others, conceiving that this would recall to his memory a crowd of facts in confirmation of my statements. In doing so I especially guarded Dr. Beale from being led astray by the theories of those writers, and requested him only to regard the facts which they placed upon record, which, of course, I interpret differently from them. But, instead of doing this, Dr. Beale turns to the *explanation* of the figures given by Nelson, which he correctly says do not agree with mine. He does not seem to comprehend that the point at issue is not the correctness of the facts, but which is the true explanation of them; and argues that, because Nelson's is different from mine, *therefore* I must be wrong. Although, it will be confessed, it is difficult and laborious to argue with such an opponent, I shall endeavour to make this point understood, as I did that concerning the vibriones.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 1 represents the caecal extremity of the testicle of the *ascaris mystax*, magnified 330 diameters. It contains nothing but molecules, which Nelson calls granules, or seminal particles, and which, he says, are secreted or thrown off from the mucous membrane. In the upper portion of the testicle the tube presents the appearance represented in Fig. 2. By this time the original granules, according to Nelson, swell out and form nucleated cells, which, he says, were obscured by an immense number of minute opaque granules, as in Figs. 2 and 3 (*Phil. Trans.*, vol. cxlii, p. 565). Now this swelling out and so forming nucleated cells, was the theory prevalent when Nelson wrote. Looking at the figures them-

selves, I think it will be clear that the primary stage is *molecular*; that there is no previous cell from which they are derived; and that subsequently they aggregate together in masses, and that from their coalescence the subsequent sperm-cell, Fig. 3 *a*, is formed.

Fig. 4 represents the caecal extremity of the ovary of the *ascaris mystax*, also magnified 330 diameters. "It is formed", says Nelson, "of a membranous and perfectly transparent tube, much thicker at the very end (*a*). It is from this thick portion (*a*), which presents a finely granular structure, that the germinal vesicles are thrown off; it constitutes, therefore, the true ovary" (*Op. cit.*, p. 571). If this figure, which is an exact copy from nature, be examined, it will be seen that at the extremity of the tube the structure is molecular, and that the so-called throwing off of germinal vesicles, resolves itself into the coalescence of these molecules. There is no previous cell out of which they are derived.

Figs. 5, 6, and 7 represent the earliest stages in the formation of the ovum discovered by Barry in

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

the salmon, pigeon, and cat—magnified, the first 240, and the last two 440 diameters. They represent objects seen in the ovary *before* the formation of an ovisac. (*Phil. Trans.*, Part II for 1848, p. 308.) Of these Barry observes: "After an attentive examination of these objects, I do not think it hazardous to express my belief that they present one of the earliest stages in the formation of the ovum; the enveloped sac being, as I suppose, the *germinal vesicle* itself." (*Op. cit.*, p. 309.) Of this germinal sac it was believed by Barry, following Purkinje, that it was the *first* part of the ovum formed (p. 308). Here also, then, an inspection of these figures must certainly lead to the conclusion that the ovum is formed by an aggregation and coalescence of molecules, and not from a preexisting cell.

Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

Figs. 8, 9, 10, and 11 are described by Barry as the earliest stages in the formation of the ovum from the ovary of the rabbit. He speaks of them as vesicles. They do not prove the origin of such vesicles from molecules, but exhibit their tendency to aggregate together when produced. Only the one most advanced (Fig. 11) exhibits the characters of a cell. Although these are figured as the earliest parts discoverable in the ovary of the rabbit, it must be clear that a stage in formation existed previously, which was probably the same as that seen in the pigeon, or that given by Nelson, in which nothing but molecules existed.

Without proceeding to make further extracts from other writers, and refraining from giving numerous additional arguments and facts which tend to support the molecular theory of organisation, I ask the candid reader whether the figures now given from two authors only are not sufficient to refute Dr. Beale's confident statement: "It is simply an assertion, and, I maintain, unsupported by evidence, to say 'the molecules of the ovary first coalesce to form the ovum'." On the contrary, I submit that the facts of Nelson and Barry give my theory the most unequivocal support. It avails nothing to call the molecules ova. Let Dr. Beale give them what names he pleases, they are still molecules—as such, are formed in the ovary; do not originate in preexisting cells; and, by their aggregation, produce higher forms. Such is the essence of my theory.

Having now, in deference to the position occupied by Dr. Beale as Professor of Physiology in King's College, London, responded to his invitation to explain more fully some of the points contained in my published lectures on the molecular theory of organisation, I retire from further discussion, with the hope that he will give that theory more consideration than he has hitherto done. The title given to his papers appears to me altogether erroneous, as I defy any one to extract from them one single fact or argument opposed to my theory. In one point we at least agree; viz., that however the primary molecules originate, whether they be living or dead, they are in no degree necessarily dependent on preexisting cells for their formation. This alone is a great step in science. I trust before long that, in the same way he has thrown aside the fallacious doctrine of *omnis cellula e cellula*, so he may ultimately disabuse himself of the idea *omnis molecula e molecula*.

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