A THEORY OF MUSCULAR CONTRACTION.

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In an article dealing with the structure of cross-striated muscle published in the April and July numbers of this *Journal*, I have briefly formulated a hypothesis as to the nature of the processes that immediately determine muscular contraction, and have stated that it is easy to offer plausible explanations, in terms of the hypothesis, of many of the properties and phenomena exhibited by cross-striated muscle. I wish to describe in this article certain observations that seem to give support to this hypothesis, and to show briefly how the hypothesis lends itself to these explanations.

I will first make some critical remarks on two hypotheses that claim to afford physical explanations of the process of muscular contraction; for these two hypotheses, suggested by Engelmann and Verworn respectively, are the most prominent of the many that are current at the present time.

Engelmann's Hypothesis.

Engelmann has recently made a suggestion as to the nature of muscular contraction,[1] and has offered to the Royal Society, in the Croonian Lecture of the year 1895, a 'proof' of the truth of the hypothesis (see *Nature*, 28th March 1895). The essence of the suggestion is, that contraction is caused by a heating of some small part of the fibrils of which he supposes the contractile substance of muscle to be made up. The 'proof' consists

[1] Ueber den Ursprung der Muskelkraft, 1893.

in the pointing out of certain striking resemblances between the properties of muscle and those of stretched violin-strings and other lifeless bodies of a similar nature.

Fick[1] has criticised the hypothesis, pointing out that the absolute force of the muscle is so much greater than that of any such system as Engelmann's muscle model, that the bounds of legitimate analogy are overstepped. Engelmann professes to have refuted this objection by pointing out the following facts, and making certain deductions from them: (*a*) that a violin-string ·7 mm. in diameter will *perceptibly* raise a load of 1 kilo, when its temperature is rapidly raised to 130° C.; (*b*) that the maximum load that a tetanised strip of human muscle 1 sq. cm. in transverse section can raise is 10 kilo; (*c*) that in tetanus a muscle may rise 1° in temperature. From these figures he concludes that the string exerts twenty times as much force as the muscle of the same cross-section. But this conclusion cannot fairly be drawn from the figures. For let us assume that the string was raised only 100° C. in temperature. The area of its cross-section is nearly ·5 sq. mm. Then the string ·5 sq. mm. in cross-section raised 100° C. in temperature lifts 1 kilo. Engelmann supposes that parts of the muscle are directly heated while others are not, and the rise of the temperature which would give an efficiency of 30 per cent. (which is that of muscle)[2] is 100° C. Then let us suppose the temperature distributed in this most favourable manner, and we have for the muscle the figures $\frac{1}{100}$ sq. cm. raised 100° C. in temperature lifts 10 kilos, *i.e.*, the muscle exerts five times as much force as the string for equal cross-sections. There are two considerations which show that the data of this calculation, as given by Engelmann, are far too favourable for the string. In the first place, he has neglected the question of absolute force; and probably the muscle raises the weight to a height which is a much greater proportion of its own length than does the string. Secondly, the tetanus during which a muscle rises 1° C. in temperature is one continued for some minutes (*cf.* I., s. 158). Becquerel observed a rise of temperature of 1° C. after five minutes' tetanisation, and (s. 159) Helmholtz observed a rise of temperature of ·18° C. on tetanis-

[1] Pflüger, *Arch.*, Bd. liii.

[2] I., s. 166. [I. refers here and hereafter to Hermann's *Handbuch d. Phys.*, ed. 1. II. refers to Biedermann's *Electro-Physiologie*, Eng. trans.]

ing frog's muscle for two or three minutes. The rise in temperature of the muscle during the actual raising of the weight is, therefore, certainly much less than 1° C., probably only a very small fraction of a degree. Allowing, then, for these considerations of the forces exerted by equal masses of equal cross-section of muscle and violin-string, raised equally in temperature, that of muscle must be at least 100 times greater than that of the string.

A similar conclusion may be more legitimately reached in this way: 1 sq. cm. of frog's muscle can raise in a simple contraction 400 grs. (I., s. 64), and the maximum rise of temperature for a simple contraction is ·001° C.–·005° C. (I., s. 159); then taking the lower estimate, 1 sq. cm. of the muscle with rise of temperature ·001° C. lifts 400 grs., *i.e.*, $\frac{1}{1000}$ sq. mm. rising 100° C. in temperature lifts 400 grs., and in the case of the string $\frac{1}{2}$ sq. mm. rising 100° C. in temperature lifts 1 kilo, *i.e.*, the muscle exerts the same pull with a cross-section about $\frac{1}{200}$ of that of the string, or, taking the higher estimate of the rise of temperature, about $\frac{1}{40}$ of that of the string. Here, again, the height to which the weight is lifted has been neglected; and if it were taken into account, the absolute force of the muscle would probably be found to be very much greater still in proportion to that of the string.

One of the most striking resemblances between living contractile structures and the lifeless substances with which the comparison is made, is the possession of doubly refracting parts by both classes; and Engelmann insists that one of the optic axes is always parallel to the direction of contraction. But he has pointed out, as particularly significant, the fact that in certain muscle fibres the contractile fibrils run in steep spirals round the fibres, their obliquity to the axis of the fibre increasing during contraction, while the optic axes of the fibrils remain parallel to the axis of the fibre in all degrees of contraction. He adds, "hence it is not the morphological axis of the fibrils, but the optical axis of their doubly refractive constituents which coincide with the direction of the contracting force." But the direction of the contracting force with which the optical axis of the fibrils coincides, is the resultant of the forces acting along the morphological axis of all the fibrils; for it cannot be contended

that the fibrils exert their pull in any other direction than along their morphological axes. These fibrils, then, form a notable exception to the law which Engelmann is attempting to establish, and prove that this point of resemblance between the two classes of objects is merely a coincidence. In just the same way all the other points of resemblance remain merely striking coincidences, and do not in any way amount to proof.

Engelmann, while admitting the essential similarity between 'rigor' and contraction, finds it impossible to explain the former as a thermo-dynamic effect, and falls back on his old imbibition hypothesis.

If there be any truth in the view of so many observers that the wing-fibril of insects is a membranous tubule (a view which I have endeavoured to extend to other muscles), it is difficult to understand to which parts of them Engelmann would attribute the rise of temperature of many degrees, which he is bound to assume. For since each sarcomere is contractile and takes part in a general contraction, he must suppose that some minute fraction of the substance of each sarcomere is the part which suffers the increase of temperature.

Verworn's Hypothesis.

Verworn, in his work entitled *Die Bewegung der lebendigen Substanz* (1892), claims to give a physical explanation of the contraction of muscle by suggesting, by a chain of argument which it is needless to examine, an analogy between it and the retraction of the pseudopodium of an amœba. He regards both the protrusion and the retraction of the pseudopodium as cases of positive 'chemiotaxis' or 'chemiotropism.' Of the protrusion he offers a physical explanation which may be summed up as follows:—The substance of the resting cell has an affinity for oxygen. In a medium devoid of oxygen, its surface becomes spherical through surface-tension. In a liquid medium containing oxygen, molecules of the latter will chemically attract molecules of the cell substance lying at the surface of the cell, and the attraction will be greater at some points than at others. At the former points the molecules of the surface will move to meet the molecules of oxygen in the medium. At each of these

points, therefore, the surface expands; the molecules from within having unsatisfied affinities for oxygen, must take up a position at this part of the surface, and be in turn attracted towards the oxygen of the medium, and so the protrusion increases and a pseudopodium is formed. Retraction he would explain as the result of an alteration of the chemical nature of the molecules at the surface of a pseudopodium, the alteration being the result of stimulation, and of such a nature that the new substance is attracted by substances—the 'Kernstoffe'—formed by the nucleus and distributed through the cell body with diminishing density from the nucleus outwards. He implies that there is thus produced by stimulation, conditions the exact reverse of those which determined the protrusion of the pseudopodium, but does not attempt to work out the molecular process in detail as before. If we accept as plausible his explanation of protrusion, and attempt to work out the corresponding molecular process to which he attributes the retraction, using his own methods of treating of the molecules and their chemical affinities, we find that it cannot be done. Thus on stimulation, molecules at the surface acquire an affinity for molecules of 'Kernstoffe' lying immediately below the surface, and pass towards them in virtue of chemical attraction. We may even suppose that being still unsatisfied by the layer of 'Kernstoffe' immediately next to the surface, they proceed still further inwards in search of more 'Kernstoffe.' But before they have proceeded more than an infinitesimal distance, they must form a barrier of inert substance between the surface molecules and any 'Kernstoffe' within the cell, and so the process must come to an end. However the argument be varied, it does not seem possible to explain retraction on these lines; and Verworn's explanation of muscular contraction simply consists of a very much forced analogy drawn between it and the retraction of the pseudopodium.

Verworn's hypothesis as to the causes of retraction of a pseudopodium may perhaps be radically modified by supposing that retraction is due simply to the cessation of those processes which cause protraction, surface-tension alone bringing about the retraction. But when so modified, the hypothesis no longer finds support in the observations on the retraction of pseudo-

podia following stimulation, and on the relations of the nucleus to the cell body, which are the mainstay of the hypothesis in its original form.

In the article referred to above, I pointed out that any increase in the volume of the fluid contents of a sarcomere must tend to cause its side walls to bulge outwards and its ends to be drawn together, and the whole sarcomere to go through just such a series of changes in shape as I have described as occurring during contraction. I accordingly formulated my hypothesis of the processes involved in contraction as follows: —Contraction is the result of an increase in the volume of the fluid contents of the sarcomere, and relaxation is accompanied by a diminution in their volume. I also showed that in the dead sarcomere an increase in the volume of its contents is an efficient cause of shortening. I wish now to record certain observations that make it seem highly probable that in the living sarcomere such an increase in the volume of its contents is not only an efficient cause of shortening, but also the actual cause of normal contraction.

The observations to which I refer were made on isolated sarcostyles of the wing muscles of the house-fly, on fibres from the leg muscles of the water-beetle, and on fibres from the claw muscles of the crayfish, subjected to the action of saline solutions of various degrees of concentration, of distilled water, and of weak lactic acid. They were begun in the hope of producing a contraction of the isolated sarcostyles by the action of distilled water; for I believe that the rigor that is produced in frog's muscle by the action on it of distilled water is a purely physical phenomenon, due to increase in volume of the fluid contents of the sarcomeres by endosmosis from the sarcoplasm.

If sarcostyles be teased out in distilled water from the wing muscles of the house-fly, they are generally found to be fully extended, and they show no marked swelling, though appearing a little turgid when compared with others teased out in normal saline solution. This failure of distilled water to produce either contraction or marked swelling does not mean that distilled water does not tend to cause distension of the sarcomere by osmosis. For, as I have said, the sarcomeres appear turgid (*b*, fig. 1) as compared with others in normal saline solution

(a, fig. 1); and if they are soaked for some minutes in 2 per cent. NaCl solution, and this solution is then replaced by distilled water, they swell rapidly to more than twice their normal diameter, while remaining of the same length. The failure is rather due to the fact that the fluid or viscid contents of the sarcomeres very rapidly sets or coagulates when the fibrils are exposed to any abnormal influences. Swelling of the contents of the sarcomere cannot, then, take place unless its side walls or its end discs a become stretched, and for this the osmotic pressure produced by the action of water on normal sarcostyles seems insufficient. But if the osmotic pressure is made greater by soaking the fibrils in 2 per cent. NaCl solution before putting them into water, the a discs yield to the distending force and swelling takes place, the whole fibril increasing uniformly in diameter (fig. 2).

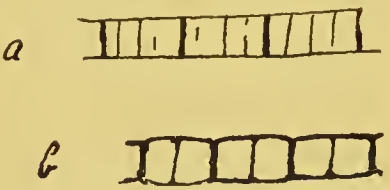

Fig. 1.


Fig. 2.

Weak solutions of acids cause swelling of isolated fibrils, and a ·2 per cent. solution of acetic acid will occasionally cause a very rapid shortening, followed by great swelling and dissolution. But it is very rare to see any marked contraction preceding or accompanying the swelling. Nevertheless, a study of the effects of very weak solutions of lactic acid is instructive. Fibrils are teased out in ·7 per cent. NaCl solution, which is then replaced by saline solutions containing ·7 per cent. NaCl and various amounts of lactic acid. The weakest solution of lactic acid that I have observed to cause swelling of quite freshly separated fibrils is one containing one part of the strong commercial lactic acid in ten thousand of water. With acid of this strength the sarcomeres usually assume a barrel shape (see fig. 3), with slight shortening, and no yielding of the a discs. If they are not quite freshly separated, but have been lying in saline solution for some minutes, acid of this strength produces no swelling, but


Fig. 3.

rather stronger acid (4 parts in 10,000) will cause a rapid swelling, with yielding of the α discs. The degree to which the discs exert any constricting action in such swollen fibrils is very variable. In some cases no constrictions are visible, and the whole fibril forms an even cylinder 4 or 5 m. in diameter (α, fig. 1). But usually both the α and β discs cause well-marked constrictions, and the γ discs are usually visible, and sometimes cause slight constrictions (fig. 4). By washing fibrils in this condition with saline solution they may be made to shrink rapidly to their normal proportions. But after undergoing this swelling and subsequent shrinkage they no longer appear quite normal: the α discs are a little irregular and ragged in appearance, and the whole fibril distinctly presents the appearance of having been overstrained. If fibrils be teased out in distilled water and then swollen by the action of ·03 per cent. lactic acid, they may be made to shrink again by washing them with water, just as by washing with saline solution in the preceding experiment.

Fig. 4.

By using slightly stronger acid the normally inextensible side walls of the sarcomeres may be stretched in the same way as the α discs. Each sarcomere then appears of its normal shape, but all its measurements are approximately doubled. Still stronger solutions of acid cause very rapid swelling and dissolution.

The exact nature of the chemical or physical process involved in this swelling of the sarcomere under the action of dilute acids is not clear. The process is perhaps analogous to the swelling of fibrine filaments when immersed in weak acid. But it is clear that the presence of extremely dilute lactic acid may cause a very rapid passage of a large quantity of water into the sarcomeres. And it is also clear from these observations that distilled water tends to pass into the sarcomeres by osmosis.

I have said that it is unusual to see more than a very slight degree of shortening produced in isolated wing fibrils by these agents that tend to cause distension of the sarcomeres, and I believe that this is due to the fixation or coagulation of the contents of the sarcomeres by their immersion in these abnormal

fluids. This statement is justified by a study of the behaviour of fibrils teased out in white of egg, and in white of egg mixed with equal quantities of saline solutions of various degrees of concentration. If fibres from the wing muscles of the house-fly be teased in either white of egg or a mixture of equal parts of white of egg and ·7 per cent. NaCl solution and examined at once, numerous fibrils are seen contracting rapidly; and after about thirty seconds all the fibrils, many of which were at first almost completely extended, appear fully contracted. White of egg is said by Hammarsten to contain about ·7 per cent. of mineral salts. If, therefore, it is mixed with an equal quantity of 5 per cent. NaCl solution, the mixture will contain about 2·8 per cent. of mineral salts. Fibrils teased out in such a mixture usually remain fully extended.

White of egg with an equal quantity of 2 per cent. NaCl solution is a mixture containing about 1·3 per cent. of salts. When fibrils are teased out in this mixture they usually contract to some extent, but slowly; and many contract only partially. If fibres be allowed to lie in this mixture for five or ten minutes before the fibrils are teased out, it is usual for all the latter to remain completely extended, though some may contract slightly. If isolated fibrils be allowed to soak for some time in these mixtures containing much salt, many of them soon show a distinctly collapsed appearance, *i.e.*, the sarcomeres have their side walls partially sunken inwards, so that they now seem no longer fully distended, but shrunken.

A mixture of white of egg and 1 per cent. NaCl solution contains about ·85 per cent. of salts. Fibrils teased out in this mixture contract well and uniformly, but if the fibres be allowed to soak for ten minutes in the mixture before the fibrils are teased out, contraction is much retarded and very imperfect in most cases, while a preliminary soaking for fifteen minutes is usually enough to prevent contraction entirely.

A mixture of white of egg and distilled water contains about ·35 per cent. of salts. Fibrils teased out in this mixture always contract rapidly and completely, and do so, even if the fibres have been soaked for half an hour or more in the mixture.

These results are readily explicable in terms of the working hypothesis of contraction that I have suggested. The sar-

comeres of the teased-out fibrils are in such a condition of distension that when they are subject to no extending force they remain contracted. Those which are isolated by teasing are necessarily drawn out in the process of being ruptured across. They then rapidly resume the condition of retraction or contraction so long as the medium in which they lie is one which does not materially alter their physical state. White of egg mixed with normal saline solution is such a medium. But if the mixture contains a large proportion of salts, there is caused a passage of fluid out of the sarcomeres (as proved by the conditions of partial collapse produced by the long continued action of the fluid), and this relieving their condition of tension or distension, removes thereby their tendency to contract. On the other hand, the mixture of white of egg with water probably causes a passage of fluid into the sarcomeres, and so, by still further distending them, increases their tendency to contract.

This interpretation of the facts is fully borne out by a study of the effects of similar agents on fibres from the leg muscles of the water-beetle and from the claw muscles of the crayfish. Fibres of the former kind, when separated with needles in ·75 per cent. NaCl solution, if not at first in a state of complete contraction, usually contract slowly to the maximal extent. If the saline solution be replaced by distilled water, contraction proceeds more rapidly and certainly. If the muscles be soaked for ten to fifteen minutes in a 2 per cent. NaCl solution and then separated in this same solution, most of them are found in a state of complete extension, and remain so. If, then, the 2 per cent. NaCl solution be replaced by one containing only 1 per cent. NaCl, in the first few seconds there is no apparent result, but usually after about ten seconds a slow contraction begins, and then, growing more rapid, continues until all the fibres are wholly contracted. If a 1 per cent. NaCl solution fails to produce this result, one containing ·75 per cent. NaCl will invariably do so. When weaker solutions are used, contraction begins and proceeds more rapidly, and most rapidly when distilled water is used. When a slow contraction set up by a 1 per cent. solution has begun, it may sometimes be stopped by swilling the fibre with a 2 per cent. NaCl solution.

Assuming that my account of the structure of these fibres be the true one, *i.e.*, that their muscle columns are similar in all essential respects to the cylindrical sarcostyles of the insect's wing muscles, these facts are readily explicable on the same lines as those just dealt with. The sarcomeres are in a condition of chronic distension, *i.e.*, they have tone, and when separated in an inert fluid they contract. When they are separated in distilled water, water passes by osmosis into the sarcoplasm, and thence into the sarcomeres also, so increasing their distension, and causing a more rapid and powerful contraction. When they are soaked in 2 per cent. NaCl solution, water passes from the sarcoplasm, and therefore also from the sarcomeres; the condition of distension of the sarcomeres is removed, and therefore the tendency to spontaneous contraction. When 1 per cent. NaCl solution is added, it acts on the fibres containing a raised percentage of salts just as pure water acts upon normal fibres,—it produces, in fact, a water-rigor.

In just the same way, a 1 per cent. NaCl solution will abolish the tendency to spontaneous contraction, though less rapidly and certainly than does the 2 per cent. solution. Contraction may then be produced by means of ·75 per cent. solution, but much more certainly by ·5 per cent. solution. This seems to be explicable in just the same way as the previous case.

If to fibres lying relaxed in 1 per cent. NaCl solution there be added a solution containing 1 per cent. NaCl and ·03 per cent. lactic acid, a contraction results after a few seconds, which is quite similar to that produced by distilled water. Now we have seen that a still weaker solution of acid than this will cause a very rapid passage of large quantities of fluid into sarcomeres of isolated sarcostyles, and it seems highly probable that the acid causes contraction in this case by leading to the passage of fluid into the sarcomeres, just as pure water and weak saline solutions seem to do.

Since the contractile elements of the claw muscles of the crayfish are unquestionably cylindrical sarcostyles exactly similar to those of the insect's wing muscles, it seemed worth while to repeat this last series of experiments on them. Water and saline solutions seem to exert on these muscles a very rapidly injurious effect; and when pinned out with their

natural attachments in the extended state, the fibres have so strong a tendency to contract that they usually break away from their attachments, and contract completely. But after being soaked in mixtures of white of egg and saline solution in equal parts containing 2 per cent. of salts, they may be obtained in a fully extended or relaxed condition. The addition of weaker saline solutions or water then causes contraction, which is rapid and certain according as there is a low percentage of salts. In a similar way, a mixture of white of egg and saline solution containing 1 per cent. of salts will cause a condition of complete relaxation; and fibres in this state contract on the addition of ·5 per cent. of NaCl solution or 1 per cent. NaCl solution containing ·03 per cent. lactic acid. In my previous paper I pointed out how, in regard to structure, these muscles form a natural link between the types of the wing muscles and the leg muscles of insects, and we see here that in these respects also they are intermediate in character to those two types of muscle.

My interpretation of these results finds further support in certain observations made by Mr W. M. Fletcher of Trinity College, which he has not yet published. He has most kindly given me permission to make use of such of his results as throw most light on the problem in hand. It has long been known that frog's muscle, when immersed in distilled water, or when water is pumped through its vessels, passes into a long continued state of contraction, and that this state of contraction may be abolished by immersing the muscle in 2 per cent. NaCl solution, or pumping this solution through its vessels. In the course of a series of very delicate estimations of the amounts of carbonic acid given off from surviving muscles in units of time at various periods after excision from the body, Mr Fletcher has found that when a frog's muscle passes into a well-marked water-rigor owing to immersion in distilled water, there is no increase in the rate at which CO_2 is being given off from the muscle at the time. This observation, although not conclusive, would seem to indicate that the water does not produce its effect by causing a chemical change in the same way as ordinary stimuli, but that its effect is rather a purely physical one. It therefore lends support to the explanation of water-rigor that I have suggested,

namely, that it is due to the passage of fluid into the sareomeres, owing to a reduction of the osmotie equivalent of the fluids of the sareoplasm.

Mr Fletcher has also shown that when a musele is immersed in a weak solution of lactie aeid (4 parts of acid in 10,000 of normal saline solution), a well-marked contraction rapidly ensues and persists. And, just as the water-rigor may be abolished by immersion of the musele in 2 per cent. salt solution, so this acid-rigor may be slowly abolished, at least in part, by immersion in normal saline solution. Now, I have shown that acid of less concentration than this (namely, 1 part in 10,000) will cause swelling of isolated sareostyles, even when they are already partially altered and set by immersion in saline solution; and that by washing with saline solution free from acid, the swelling may be as rapidly abolished. It seems, then, probable that the weak acid produces its effect on the whole muscle by eausing the passage of fluid into the sareomeres, and that the relaxation of the muscle on subsequent immersion in normal saline solution is due to a washing away of the acid. It is true that Mr Fletcher finds that the immersion of the musele in the aeid and the onset of the aeid-rigor is accompanied by a sudden inerease in the amount of CO_2 given off by the musele. But there are good reasons for believing that the sareoplasm of the muscle contains a certain amount of CO_2 in a state of loose chemical combination; and it is highly probable that the increase in the amount of CO_2 given off may represent this stoek of loosely eombined CO_2 suddenly set free by the advent of the aeid.

In my previous paper I suggested that eontraetion is due to a passage of water from the sareoplasm into the sarcomeres, owing to an increase in the osmotie equivalent of their eontents; and that this increase is due to the splitting up of large unstable moleeules into a larger number of smaller moleeules. The observations recorded above form a basis for further speeulation on the cause of contraction, and suggest that, while an increase in the number of molecules in solution in the sareomeres may be in part the cause of passage of fluid into them, probably a more important factor in bringing about the result is the presence of lactic acid among those newly formed moleeules. There

are reasons for believing that if lactic acid is the main agent in the process, it is produced in the sarcomeres rather than in the sarcoplasm. The experiments mentioned above, in which isolated sarcostyles were swollen with acid, and subsequently made to shrink by washing with saline solution or water free from acid, indicate that it is the presence of acid in the contents of the sarcomeres that determines the taking up of water and the holding of it by those contents. In making these experiments with acids, I have frequently noticed that the immediate effect of adding a solution of acid, too weak to cause a swelling of the fibrils, is to cause a well-marked sudden shrinkage of them. There seems to be a passage of fluid out of the sarcomeres, caused by the presence outside them of acid which has not yet permeated their substance.

In those experiments in which a solution containing ·01 per cent. lactic acid caused the passage of a large quantity of water into the sarcomeres of isolated sarcostyles, the latter had been lying in saline solution, and were already partly set or coagulated, and less easily affected by the acid than quite unaltered sarcostyles would be. This alteration is proved by the fact that sarcostyles only partially teased out from the fibres, *i.e.*, lying still embedded in sarcoplasm, and therefore less affected by the saline solution, are generally swollen by weaker solutions of acid than those that are lying free in the fluid. It seems probable, then, that the setting free of a very minute quantity of lactic acid in the chambers of the normal living sarcomere is sufficient to cause the passage into the sarcomere of a quantity of fluid. It would seem probable, therefore, that the production of so small a quantity of acid as would constitute the contents of the sarcomere a ·001 per cent. solution, or even of a still smaller quantity, may cause the passage into the sarcomere of enough fluid to bring about contraction.

I would suggest, then, that contraction is the result of the passage of fluid into the sarcomeres from the sarcoplasm, determined by the setting free of lactic acid in the fluid contents of the sarcomere, aided perhaps by an increase in the osmotic equivalent of these fluid contents through an increase in the number of molecules in solution. Then, so long as the acid remains present in the fluid of the sarcomere, the additional fluid absorbed

will be retained and the state of contraction will continue. But as soon as the acid escapes from the sarcomere, the additional fluid will also escape with it into the sarcoplasm, and allow relaxation to take place, just as in the case of the isolated sarcostyles swollen by acid, rapid shrinkage results from washing out the acid with water, and in the case of acid-rigor, relaxation results from immersion in normal saline solution. It is probable that in normal muscle the sarcoplasm contains a store of alkaline substances specially adapted for taking up the acid of the sarcomeres, and that the acid is no sooner produced than it begins to pass out into the sarcoplasm, either as lactic acid, or perhaps, having undergone a further change, as carbonic acid.

If we adopt this as a working hypothesis of the processes immediately concerned in contraction and relaxation, we can give explanations of many of the well-known phenomena of muscular activity. We see that relaxation is, in a sense, an active process, for it does not depend merely upon the cessation of some change causing contraction, but is the result of a distinct physical process, namely, the escape of fluid from the sarcomeres. Many considerations have led physiologists to the conclusion that relaxation is in fact an active process.

The form of the curve of simple contraction may be explained thus:—The sudden production of acid leads to an inrush of fluid into the sarcomeres, which is most rapid at first, and becomes slower as the acid escapes, until the amount of acid present is no longer enough to hold this additional fluid in the sarcomeres. The additional fluid then begins to escape, most rapidly at first, then more slowly, since the acid must escape most rapidly when it is present in largest quantity, and very slowly when most of it has already escaped. Hence the long drawn-out character of the lowest part of the curve of relaxation.

It is well known that the first part of a contraction is the most forcible, and that three small contractions give a larger proportion of work to heat than one large one that does the same amount of external work as the three together (I., s. 167). The action of the elastic discs of the sarcomeres must tend to produce just this result, for in a large contraction there must be a greater proportion of the whole transformed energy spent in doing work in stretching the elastic discs in the later than in

the earlier stages of contraction, and this part will appear not as external work, but as internal heat. Hence the greater economy of small contractions as compared with large ones. Probably other factors besides play a part in determining the greater force of the early part of a contraction. Thus, if the setting free of acid in the sarcomeres is very sudden, practically instantaneous, there will be developed a certain hunger for water, or power of attracting water, which becomes feebler as it becomes more and more satisfied in the earlier stages of contraction. It seems probable also, from a consideration of the conditions, that the distending force must act on the walls of the sarcomeres at a greater mechanical advantage in the early than in the later stages of contraction.

The facts of summation of stimuli are very fully explicable in terms of this hypothesis. When a second stimulus is thrown into a muscle before the contraction due to a former stimulus of the same strength has passed away, the second contraction rises from that point on the curve of the first contraction at which it is thrown in, as from a new base line, but does not rise quite so high as it would from the normal base line (II., p. 117). Thus, in fig. 5 (copied from Helmholtz), if the second stimulus be thrown in at *b*, the second contraction rises almost to an equal height with the first one. For at the point of time *b*, the sarcomeres of the muscle are distended with the whole of the additional fluid that has passed in as the result of the setting free of acid at the point *a*, while the acid itself has mostly escaped, so that it can no longer hold this additional fluid in the sarcomeres. When at this point, *b*, a second quantity of acid equal to the first is set free, the escape of the additional fluid already present is prevented, and the entrance of a second quantity equal to the first is determined. But, as we have already seen reason to believe, the distending force acts at less advantage when the sarcomeres are already partially contracted, and so the second rise is not quite so high as the former one. Just in the same way, when a second stimulus is thrown in at any point on the descending part of the curve, the further escape of the additional fluid is arrested, and the entrance of a second quantity is determined, and the contraction rises to a height corresponding to the sum of these two quantities of addi-

tional fluid. When the second stimulus is thrown in at a point on the ascending part of the curve of the former contraction (g in fig. 5), the acid set free at that point begins to escape in the same way as that liberated by the former stimulus; and so, after an interval g–h, equal to the interval a b, it has diminished in quantity to the point at which it can no longer hold the additional fluid. The height of the contraction therefore represents the amount of fluid that passes in during the time a h. The curve of relaxation we may regard as consisting of the two parts h i and i k, the former representing the escape of the additional fluid that has passed in during the period g h, the latter that of the fluid that passed in during the period a g.

The contraction resulting from the summation of two or more stimuli, each of which alone is subliminal, such as occurs in

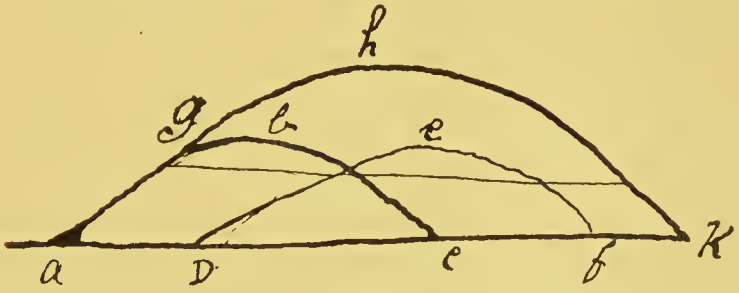

Fig. 5.

erab's muscle (II., p. 117), demands a different explanation. It must be supposed that the acid set free by the first stimulus is insufficient to cause a flow of fluid into the sarcomeres, but that when a second stimulus follows after the first before the acid liberated by the first has had time wholly to escape, then the percentage of acid in the sarcomeres is large enough to cause the inflow of fluid, and contraction results.

There is a group of phenomena—the staircase, fatigue in excised muscles, the effects of deprivation of circulation, etc.—which seem to be all explicable by means of one not improbable assumption. I have said that the sarcoplasm probably contains a stock of alkaline substances which, in a fresh muscle, are hungry for the acid products of the sarcomeres, and that after contraction they take up the acids, probably in the form of carbonic acid, and pass them on to the lymph and blood. That

there is some such storage of CO_2 is made almost certain by the observation by Mr Fletcher of the fact that a simple contraction or a few simple contractions do not cause any increase in the amount of CO_2 that is being given off by an excised muscle, while immersion in very dilute acid does cause the giving off of an increased quantity of CO_2 during the first few minutes. It is clear that if relaxation depends upon the escape of the acids, and this escape depends, as it must, upon the state of the sarcoplasm, then in an excised muscle, on repeated contraction, the alkalinity of the sarcoplasm must diminish, its hunger for the acids must be partially satisfied, and the acids will then escape from the sarcomeres less rapidly. We see here, then, the explanation of the lengthening of the curve of relaxation through fatigue in excised muscles, while the curve of shortening remains unaltered. The cutting off the circulation through the vessels of a muscle is said to affect the curve of its contraction in just the same way, and a similar explanation seems to apply to this case. The slow metabolism of the resting muscle, on which its tone may be supposed to depend, must lead, in the absence of the circulation, to a partial saturation of the sarcoplasm with the waste products of the sarcomeres. The acids will then no longer escape from the sarcomeres so rapidly as in a muscle from which the circulating blood continually carries the CO_2 and other waste products away from the sarcoplasm.

Fatigue of this kind may be in some degree removed by the circulation of a simple saline solution (which presumably acts by washing out the acid waste products), and Ranke has shown that by injecting an extract of fatigued muscle, fatigue symptoms may be produced. He has further shown that the active substances concerned are CO_2, lactic acid, and acid sodium phosphate; and Hermann has pointed out that other acids will produce similar symptoms, and that weak alkalies will remove these symptoms (I., s. 23). When a muscle with intact circulation is repeatedly stimulated, fatigue is manifested by a diminution of the height of the curve of contraction: it may in this case, perhaps, be regarded as chiefly due to a using up of the reserve substances on whose metabolism contraction depends. But all the facts quoted above support my suggestion that the fatigue symptoms of excised muscle are due to the accumulation

of acids in the sarcoplasm of the muscle. My hypothesis, then, explains the 'modus operandi' of this accumulated acid, and brings together, as effects of this one cause, a long series of phenomena, the relations between which have hitherto been quite obscure. For assuming, as we have seen reason for doing, that the rapidity of escape of acid from the sarcomeres depends upon the capacity of the sarcoplasm to take it up readily, and that this capacity, in turn, depends upon the presence of a certain store of alkaline substance in the sarcoplasm, then it is clear how the accumulation of acid in the sarcoplasm must diminish its capacity for taking up more acid, and so make the escape of acid from the sarcomeres less rapid.

We may now apply this conception of the processes underlying the fatigue of excised muscle to the detailed explanation of a series of phenomena. And we may begin by a mention of the 'verkürzung-rückstand.' When a muscle is weighted with a very small load, the curve of its contraction returns to the base line only very slowly. This long continued slight degree of contraction is what Hermann has called the 'verkürzungs-rückstand.' When an excised muscle has been made to contract several times in rapid succession, the amount of this residual contraction is much greater and persists much longer and is called 'contracture' (I., s. 23).[1] The 'verkürzungs-rückstand' may be explained by supposing that the rate of escape of the acid from the sarcomeres during relaxation progressively diminishes, so that the last quantities escape only very slowly, and so a part of the additional fluid absorbed by the sarcomeres during contraction escapes only very slowly also.

Then, in an excised muscle the acid taken up by the sarcoplasm in a first contraction does not escape from it, and the readiness of the sarcoplasm to take up more acid is thereby diminished. The acid causing a second contraction therefore escapes less rapidly than that which caused the first one, and the 'verkürzungs-rückstand' is therefore larger. And with each succeeding contraction the readiness with which the sarcoplasm will take up more acid is diminished, and the escape of acid from the sarcomeres is therefore less rapid and the

[1] Biedermann uses this word in another sense.

'verkürzungs-rückstand' greater, until it is so marked as to deserve the name 'contracture.'

The phenomenon of the 'staircase' seems to be another effect of this accumulation of acid in the sarcoplasm, and consequent slower escape of acid and water from the sarcomeres. The curve of fig. 6 illustrates this phenomenon in the case of an excised frog's muscle. Each contraction in a closely following series is higher than the immediately preceding contraction, and after a short interval the first contraction of a second series is lower than the last, but higher than the first, of the former series. In each series, each contraction leaves in the sarcomeres a certain residuum of acid and additional water which determine the 'verkürzungs-ruckstand.' And each contraction leaves the sarcoplasm less ready to take up more acid: the escape of acid in each contraction will therefore be less rapid than in the preceding contraction, and the residual amount in the sarcomeres will be greater. Each contraction-curve will therefore start

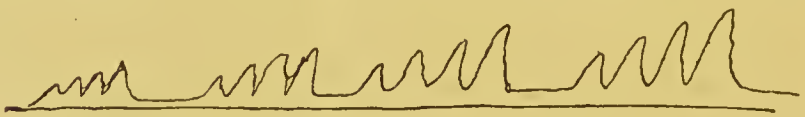

Fig. 6.

from a higher base line, and in so far will tend to be higher than its predecessor. But there is another factor tending towards the same end. Since the escape of acid from the sarcomeres is slower, a longer time must elapse after the point at which the acid was set free before the quantity of it in the sarcomeres is insufficient to attract more water. The passage of water into the sarcomeres will therefore continue for a longer period, the amount that passes in must be greater, and therefore the curve of contraction must be higher.

In exactly the same way, we may explain the fact that a stimulus will cause a higher rise of the contraction-curve after a short tetanus than one of the same strength thrown in before the tetanus.

Tetanus results when stimuli follow one another at such a rate that each one arrives before the contraction caused by the preceding one has passed away. A 'staircase' series of contractions may be regarded as the most incomplete form of

tetanus. In a complete tetanus, the contraction-curve rises higher than that of a simple contraction due to the most powerful stimulus, and there are no oscillations of the curve. We must suppose that each succeeding stimulus sets free a fresh quantity of acid in the sarcomeres, while the acid of the preceding stimulus is still causing an inrush of fluid. There is therefore maintained such a degree of acidity of the contents of the sarcomeres as will tend to cause a constant inflow of fluid; and when a maximal contraction is reached, will determine its persistence. "The height of the tetanus-curve grows with the strength of excitation, or, where this is constant, with its frequency. The steepness of the rise alters in the same proportions" (II., p. 121); for, with increased strength or frequency of excitation, the degree of acidity of the contents of the sarcomeres must be maintained at a higher level.

It seems impossible to draw any sharp line between 'contracture' and natural 'rigor.' For if an excised muscle, whose circulation has been cut off, be repeatedly tetanised, the curve of relaxation becomes more and more prolonged, until there is no perceptible relaxation, and this condition seems to pass imperceptibly into 'rigor.' I would therefore regard natural 'rigor' as an extreme state of fatigue, and as due to the same cause as the prolongation of the curve of relaxation in the fatigue of excised muscle, namely, the accumulation of acid in the sarcoplasm. We must suppose that in an excised muscle metabolism continues at a slow rate. The waste products must then accumulate in the sarcoplasm, and sooner or later a time must come at which the capacity of the sarcoplasm for taking up and fixing more acid is abolished. If the slow metabolism in the sarcomeres then continues, the acid produced must accumulate in the sarcomeres, and will then reach such a degree of concentration as results from a stimulus applied to the muscle. Fluid will then pass into the sarcomeres from the sarcoplasm and cause contraction; and since there is no way of escape for the acid, this additional fluid will remain in the sarcomeres, and the state of contraction will persist. In a late stage of 'rigor mortis' there is probably a much more profound alteration of the chemical constitution of the muscle, notably the formation of myosin; but that there is no such change in

the early stages of 'rigor mortis' is indicated by the fact that it may be abolished by the circulation of serum through the vessels of the muscle (I., s. 146), just as the acid-rigor may be abolished by soaking the muscle in saline solution. That there is no great sudden chemical change accompanying the onset of 'rigor mortis' is indicated by observations made by Mr Fletcher of the rate at which CO_2 is given off by a muscle from the time of its excision till the time of its death. He finds that the curve indicating this rate at successive intervals of time after excision has very constantly some such form as that in fig. 7. I must pass without discussion the interesting features of the first part of this curve, and merely state that the contraction of 'rigor mortis' occurs at a time corresponding to some part of the level plateau, and that there occurs at this time no increase

Fig. 7.

in the rate of escape of CO_2. These observations, therefore, support very strongly my view of the nature of the contraction of 'rigor mortis,' for they indicate the continuance of a constant slow chemical change of a nature similar to normal metabolism, and the occurrence of contraction when this change has continued for a certain time. The contraction would therefore seem to be an effect of the accumulation of the products of the slow chemical change. The 'modus operandi' of those accumulated products I have already suggested.

This view of the nature of the early stage of 'rigor mortis' is further borne out by the fact that heat, and everything that favours a rapid metabolism, hastens the onset of 'rigor,' while cold delays it, and that previous fatigue also powerfully favours the rapid onset of 'rigor'; for these are factors that tend to cause an accumulation of acids in the muscle.

The nature of water-rigor and the process of its abolition by means of 2 per cent. NaCl solution I have already discussed, and have indicated my view of the processes concerned.

The view here adopted of the influence on contraction of the waste products of metabolism finds confirmation in the fact that those muscles of which very rapidly succeeding distinct contractions are required, contain a relatively very large amount of sarcoplasm, and elaborate arrangements for carrying off the waste products of metabolism. Thus the insect's wing muscle has a very large amount of sarcoplasm which is permeated in all its parts with a very dense network of fine air-tubes. These tubes constitute a very perfect system for carrying carbonic acid away from the sarcoplasm. So also the fibres of the muscles of the bat's wing contain a very large amount of sarcoplasm, and are very slender; and each one of these slender fibres is surrounded by an extraordinarily dense network of capillary blood-vessels.

It is generally agreed that a satisfactory theory of muscular contraction must be capable of affording explanations not only of the contraction of striated muscle, but also of that of plain muscle fibres, and of the movements of cilia and amœboid protoplasm.

I do not at present see how my theory of contraction can be applied to the explanation of the contraction of plain muscle, and I am inclined to believe that this is due to our ignorance of some definite structure that exists in these fibres. There has been described a regular transverse striation in muscle fibres classed as smooth, and I have been able to make out indications of transverse markings in the fibres of the frog's bladder, by the application of Rollett's negative gold staining process. But, hitherto, I have endeavoured in vain to obtain more evidence of definite structure in plain muscle.

The movement of cilia may possibly be explained as the result of just such a process as I have suggested to be the cause of muscular contraction. If, as Schaefer has suggested,[1] a cilium is a hollow tubule, of whose wall one side is more easily extensible than the other, and if the cavity of this tubule is shut off by a permeable membrane from the general cell substance, then

[1] *Proc. Roy. Soc*, vol. xlix.

we may suppose that the bending of the cilium is the result of the sudden production of acid in the contents of the tubule, and the consequent passage of fluid into the tubule from the general cell substance; for such an inflow of fluid in distending the cavity would cause an extension of the more extensible side of the wall of the tubule, and therefore a bending of the tubule towards the opposite side. The water-rigor of cilia would also be explicable on just the same lines as that of muscle.

To extend this theory of contraction to the explanation of protoplasmic movement is a task that seems full of difficulty, yet I think that Bütschli's demonstration of the existence of alveolar structure in so many different kinds of protoplasm affords an indication of the lines along which the explanation is to be sought. If any part of the protoplasm of an amœba is similar in constitution to the contents of the sarcomeres of muscle, then the setting free of minute quantities of acid in those parts would be an efficient cause of movement of fluid towards that part. It may be that, on stimulation of some spot on the surface of a lobose pseudopodium of the amœba, acid is set free by chemical decomposition in the substances occupying the alveoli of that part, and that these then exert so strong an attraction for the fluids of the cell body that the whole cell flows towards the stimulated part, so that the pseudopodium becomes retracted.

In the linear series of alveoli that Bütschli has described in the contractile threads of certain infusoria, we may perhaps see the first step in the evolution of the cylindrical sarcostyle.

In concluding, I wish to thank Mr W. M. Fletcher very heartily for permission to make use of his unpublished observations.