CROONIAN LECTURE.—"On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)." By Dr. W. KÜHNE, Professor of Physiology in the University of Heidelberg. Communicated by Professor M. FOSTER, Sec. R.S. Received April 22, Delivered in the Theatre of the Royal Institution May 28, Revised August 15, 1888.

(Translation.)

Among the phenomena of life the movement of masses, or mechanical work, takes a prominent place. It is the most accessible of all the vital processes to our sensual perceptions, so universally distributed, and so bound up with most of the activities of organisms, that it might almost be designated the incarnation of life.

In saying this it must be understood that vital movement is by no means exclusively confined to animals, that it is not, as was once believed, a special animal function; on the contrary it is an attribute of *all* living matter, as well of the lowest creatures as of the most highly developed plants, so that, however extraordinary it may appear, the activity of our muscles which enables us to transform sensation into action finds an analogue in the plant. Our conviction of the inter-connexion and profound unity of all living things has thus a physiological foundation, based as it is not merely on the community of derivation and of structure of living things, but also on the proof of similar activities.

If a division of the morphological from the physiological is in any way permissible, it may be said that the unitary conception of life for which our age is distinguished rests in a higher degree on the knowledge of vital *processes* than is commonly recognised, and in fact is just as much founded on physiological experience as on that of the *forms* of the organism.

From the traditional conception of life, which scarcely contained more than that everything between life and death is the antithesis of the not living, it is a long road we have had to travel to attain to the modern conception of the *real* unity of life; and a remarkable road, since it bears witness to the confident anticipation of victory, in face of all impediments raised up by science itself. Movement, and nothing less, had been placed at the summit of that antithesis, which physico-chemical research in the animal and vegetable kingdom had revived with the discovery that the plant transformed kinetic into potential energy, and the animal the latter into the former. While the animal made use of oxygen to generate heat and perform work through the metabolism of its substance, the plant made use of the heat in reducing

and synthetic processes for the accumulation of potential energy in the form of its own consumable substance and the expired oxygen.

With whatever unassailable correctness this conception comprehends life as a whole, affording a pleasing solution of its antithesis by referring animal activities to nourishment by the plant, the latter to the products of the combustion of the animal body, and both in the last instance to the forces of the sun as original source of all life, yet *this* did but cast up the sum total of the processes of life, and did but express more intimately than before that which divides the most highly developed branches of the animal and vegetable kingdom, in which the divergence of forms and arrangements is greatest. For *by the side of* this distinction there exists even between man and the most highly elaborated plant a connexion of a kind quite other than the symbiotic interdependence through the medium of light, air, and food, a community, however, which is not disclosed until we go back to the ultimate elements of organisation.

As in the animal synthetic processes are not wanting, without which it could not even produce a molecule of the colouring matter of its blood, so in the plant we are acquainted with dissociations and combustion, and also with evolution of heat and movement of masses; not that by this I refer to those coarser movements which are referable to turgescence, but primitive movements, which we find first in the smallest elementary organisms, of which all living beings are made up.

We have almost in our own persons lived to see the old anticipation of a single kingdom of living things become gradually an established truth through the discovery of the cell. After the ground-lines of the construction of plants and animals out of originally similar *nucleated* cells had been established by Th. Schwann, and since Darwin's immortal work enabled us to derive everything that ever lived or will live from one single cell, we have come to realise that every single organism renews in itself the work of past ages, and again builds itself up from a germ similar to that from which its most ancient ancestors started.

This conviction has become so firmly implanted in our generation that now we scarcely feel the gaps which still exist in our actual knowledge, and almost unjustly underestimate that which the investigations of our contemporaries yet add to the cell-theory, as if it were mere work of repetition. And yet it has been very extensive and decisive—for example, the recent researches upon the intimate structure of the cell nucleus—since nothing less results from it than that the reproduction of the cell by fission takes place identically, down to the most minute details, in all animals and plants (1).*

Now if the *shaping* of the cell and all the *fashioning* of forms is an

* These numerals refer to the reference notes at end.

activity, and if Morphology, "since it has made the *arising* of form more its study than the describing of what is already completed," has become part of Physiology, it might be possible and conceivable that research directed to *all* activities and going beyond the *visible* form to the chemical components of the structures and the transformation of substance and force, should observe great differences in processes where all our morphological experience would only have shown identity. We were near enough to this point; for if it were true, as was long assumed, that that which is the bearer and the seat of the most essential of all vital processes *in* the cell is completely formless, it is not easy to see why the form should be so determinant of function.

We have hope that this is not so, and will endeavour to show in Movement the functional as well as the morphological unity of all living matter.

As I have already said, there is an elementary kind of movement in the cell, carried out by the cell-body—that part of the cell which in contradistinction to nucleus, membranes, and various enclosures, has been designated protoplasm. The protoplasm moves itself, as in the case of certain free-living Protozoa, like the long-known Amœba, like the so-called Sarcode—in many cases better comparable to the movement of the pseudopodia of Rhizopods. The resemblance of the latter to what was formerly called the sap-current in many plant-cells, led Ferd. Cohn (2) to interpret plant protoplasm as sarcode, an idea actively supported by Max Schultze (3), the best authority on pseudopodial movement. It is not necessary to say here how widespread protoplasmic movement is, for there cannot be a cell that does not present it at some stage of its existence. Doubt on this subject can only exist in regard to the smallest of all organisms, those of fermentation, of putrefaction, and of pathogenic activity which are too small for observation. But even in these, from the movement they perform as a whole, we have grounds to infer the existence of a protoplasm.

It is proved that protoplasmic movement does not follow external impulses or currents, but is a spontaneous activity. It may go on in opposition to gravity, and overcomes frictional resistance, as shown by the mass itself moving forward on surfaces of every kind, and being able to drag heavy bodies along with it. It is proper mechanical work.

The cause of the movement can only be an internal one, residing in the contractile substance itself, and can only consist of chemical processes taking place within the peculiar pasty, slime-like mass. Yet the question had to be put whether these processes were not first set up by something coming perhaps from the outside, for the movement changes, sometimes stops or takes place more slowly, or occurs but partially, and may by many means be artificially aroused or diminished.

At this point experimental physiological research had to step in, attacking the problem in the same way as it had long before done in the case of the most highly developed contractile structures, the muscles. A muscle behaves so far just like protoplasm that its contraction does work, which can only depend on chemical transformations of its own substance, during which potential is converted into kinetic energy; but it differs in that a distinct impulse from without is needed to set the game going. In normal conditions it receives the initiating impulse from its nerve, and nothing else appears able to take its place, since nothing that might otherwise act upon it, such as the motion of the blood or changes in its constitution, disturbs its repose. But if we let electric currents traverse the muscle, or if we suddenly change its temperature, or act upon it mechanically or chemically, contractions result which do an amount of work out of all relation to the insignificant impulse; the means employed only set going the process peculiar to the muscle, and this is what is meant when we term them *stimuli*, and the faculty of muscles to react to them irritability.

Now is protoplasm irritable in this sense? Experiments on objects of every kind have answered this affirmatively, and more than that have even shown a striking agreement with the irritability of muscle. Of the above mentioned agents, besides rise of temperature, which ultimately sets all contractile cell-substance in maximal contraction—a heat tetanus (4) which disappears with cooling—the electric current has shown itself the most efficient, the stimulus which most surely excites muscles of every kind as well as all nervous matter, and has thence become the most indispensable instrument of physiology.

I may be permitted to adduce an example because it illustrates what is typical and essential (5). It is the case of the fresh water Amœbæ. Every time these organisms, moving like melting and rolling drops, are subjected to an induction shock they contract almost to a sphere, and assume the spherical form completely if the shocks follow each other at short intervals, being by this means fixed for a longer time in this condition. Feebler shocks which singly have no effect, become effective by summation when applied in quick succession, just as in the case of muscle. If the movements of the animal by itself are sluggish, on electrical stimulation they are strengthened and accelerated. Thus the stimulation increases the natural movement, and if increased stimulation brings about repose, it is only the apparent repose of prolonged maximal contraction, like that of our muscles when we hold out a weight for some time at arm's length. All protoplasm behaves in this way from whatever source derived. Larger masses which cannot contract to one sphere (as in many plant cells, or those great cake-like giant masses of the plasmodium of the Myxomycetes) form several such spheres in part

connected by thread-like bridges. Everywhere the taking on of a figure with smallest surface is the result of stimulation, and the expression of augmented contraction (6). That which was outstretched becomes shorter and in like measure thicker, just as a muscle swells when it shortens itself.

Since protoplasm, which either does not move at all spontaneously or so slowly that we cannot perceive it, reacts in the same way to stimuli, we must in the case of ordinary movements infer the existence of processes originating them either in the interior, *i.e.*, automatic stimuli, or of external processes which had at first escaped us. Whoever sees for the first time the action of any one of the simpler independent Protozoa cannot avoid the idea that psychic activity in the strictest sense of the term lies behind it, something like will and design. He sees the elementary being seeking and taking up food, avoiding obstacles, and when touched by foreign objects energetically drawing back, so that he infers sensation also. Possibly he has struck the correct solution, at least we could not refute him, but we should put his deduction to a hard proof if we showed him the same phenomena in the colourless cells of his own blood, or in the protoplasm of a plant-cell; and if we placed before him the rhythmically contracting cells from the beating heart of a bird's egg incubated barely a couple of days, he would certainly wish with us that the search were for a more material cause, and hope that some chemical or physical cause might be found to set up the process. Biology cannot indeed yet claim to have established such causes in explanation of the automatism of protoplasm, but no one will blame the science for continuing the search for them.

Some causes are already excluded, *e.g.*, light, although there are a few micro-organisms whose movements are excited by it (7). Fluctuations of temperature may also be left out of account. On the other hand, oxygen has a notable influence (8). Withdrawal of the vital air stops all protoplasmic movement, though without killing the cell-body, as is seen from the fact that after the loss of automatism electrical stimulation can supply its place, and that the normal movements return on readmitting the air.

We might thus consider oxygen the prime mover in automatism and processes of oxidation its essence, did we not remember that many objects need very prolonged withdrawal of the gas to come completely to rest. This might, however, depend upon the difficulty of removing the last traces of oxygen completely, or it may be that these cannot be removed by the means adopted, but must remain until consumed by the protoplasm itself.

Since protoplasm is of pap-like softness, and may be in a state of rest or motion at any spot, its exterior limits are just as capable of change as everything within it is capable of quitting its position and

taking up any other. Thus the movement cannot become more ordered until obstacles confine and direct it. Between the perfected organisation of contractile substance in muscle and that of protoplasm capable only of unordered movement, we meet a succession of significant steps by means of which we can see how the ordering was attained. The first step would seem to consist in the uncommonly widespread flagellar and ciliary motion, in which an elastic structure, affixed on one side to the contractile mass, is drawn down or bent by its movement, straightening out again in the rhythmic pauses of repose. A further step, at which the contraction can only take place along an axis, consists in the arrangement of the protoplasm in fine strips wholly or partially surrounded by elastic walls, or again in elastic fibrils being embedded in protoplasmic processes. In this case we have actual primitive muscles before us, of which the most elegant examples are known in the Infusoria among the Vorticellæ and Stentores. The movement of these structures is quite like that of muscle. The strips lengthen and thicken, and they may also be contracted in quick twitches or in a prolonged tetanus, the relaxing, like the stage of diminishing energy of all muscles, always proceeding more slowly than that of the increasing energy *before* the maximum.

The muscles of the unicellular Infusoria, no longer doubtful in a physiological sense, show us muscle as a constituent of the cell, and differentiation, without the production of new cells specially endowed for the purpose, taking place in *one* cell to the extent of elaborating contractile elements determinate in form and precise in work. It is very noteworthy that side by side with these muscular strips provided with highly regulated movement, other protoplasm persists, which continues uninterruptedly its ordinary unordered movements, while no such unrest is to be remarked in the muscles. On the contrary, these latter are only used from time to time, apparently for attaining distinct objects. We get the impression that the automatism has, as it were, been lost by this portion, so that it must wait for stimuli to reach it from other parts of the cell. If oxygen really applies the first spur to the protoplasm, it has no direct power over the primitive muscle, so that compared with the protoplasm the muscle is endowed with a diminished irritability.

It has often been said that protoplasm presents the complete set of vital phenomena—assimilation, dissimilation, contractility, automatism, resorption, respiration, and secretion, and even reproduction by dividing. Leaving reproduction on one side, as now disputed and on good grounds, we can assent to the assertion, and examine which of those functions remain for the products of differentiation. In the case of the muscle, we find it to be all of them with the exception of a single one; for, while it undoubtedly takes part in nutrition as in

respiration and carries on a chemical exchange, all of which are indispensable for contractility, *i.e.*, for its work, and since secretion generalised signifies merely the throwing off of broken-down products, it is wanting *only* in automatism, that faculty of reacting to certain stimuli, which remained reserved for protoplasm. In this there is nothing opposed to the assumption that protoplasm as opposed to muscle possesses elementary *nervous* properties.

The above is sufficient to show the transition to the very highly developed motor apparatus, which distinguishes the animal kingdom from almost its lowest stages—I mean the bi-cellular apparatus, which consists of separate cells united only for *one* purpose, one of which presents the exciting nerve, the other the obedient muscle.

From past experience we know that division of the nerve, or more correctly speaking, removal of the nervous cell substance, condemns the muscle to rest. The stimuli then start from the nerve-cell, to them the muscles react by doing work, and they are conveyed to the muscles through the continuation of the cell which the nerve-*fibre* presents. We need not yet trouble ourselves how the excitation of the nerve-cell arises, whether through external—sensory—stimuli, or through an enigmatical psychic act, or through chemical influences; certain it is that these were before the division of the nerve the sole impulse to the muscle's movements. But what the muscles lack we can supply artificially, and more; we can put the nerve-remnant in such manifold states of excitement as it never before experienced from its cell-body, so that the muscle is compelled to undergo many kinds of movement quite new to it, and we can attain the same result by direct stimulation of the muscle.

In the circle of these experiences arose the controversy, not yet quite ended (9), as to muscular irritability, properly the question whether it was, in general, possible to stimulate anything artificially that is not nerve, that is, to set free the activity peculiar to a non-nervous structure by the means at our command.

Haller, who was the first to occupy himself minutely with the stimulation of muscle, and introduced the term irritability, decided, but only incidentally and by the way, that the stimulus could strike also the ramifications of the nerve in the muscle, and he was far from interesting himself in the question in the modern sense, or from suspecting the point of view from which the independent irritability of muscle would later on be questioned. We ought not to blame him much for the latter, since even to-day it is not easy to understand the motives of an opposition now continued for more than a century. At the outset, if I am not mistaken, the teaching of the Animistic, or as it might now be called, the Neuristic school, led to the conception that not only the excitation and regulation of the various functions, but the actual endowment of the several tissues with their respective

activities, was the work of that everywhere predominant and distinctly animal contrivance, the nervous system.

In connexion with this, there seems to have arisen the view of the ubiquity of nerves, that is, of so fine a penetration of the parts with nerve radiations that, especially in muscle, not the smallest particle free from nerve could be demonstrated, a view which on the strength of microscopic research is coming up again at the present day in a constantly new dress, and finds energetic adherents (10), but as we shall see is to be refuted, especially by experiment. If we disregard this, we shall find the tendency to consider only nerves as excitable, in some degree founded on the differentiation which transferred automatism to the nervous matter, robbing all the remaining tissues of irritability, so that they only retained the faculty of reacting to the stimulated nerve with which they were bound up. This was as much as saying it was impossible artificially to replace the nervous stimulus, or that if we did succeed, we were strictly imitating it, in which case, indeed, we should have come unawares upon the solution of the problem of motor innervation. Against such arguments it availed nothing to point out the excitability of nerveless sarcode, as was often done in favour of irritability: for, just as it was formerly useless, because the real genetic connexion of sarcode and muscle was not known, so to-day it would have to be rejected, because automatic protoplasm can also be correctly considered nervous.

A non-irritable muscle would strike us as strange enough, and, against all expectation, different from the nerve, when we consider that the nerve-fibre, although incapable of being affected by all the natural stimuli which excite its ganglion cells, free that is from automatism, is artificially excitable at every spot by the most different agents. However, we have no further need of such considerations, since the question of irritability lies within a region where instead of speculation, observation and experiment have become decisive.

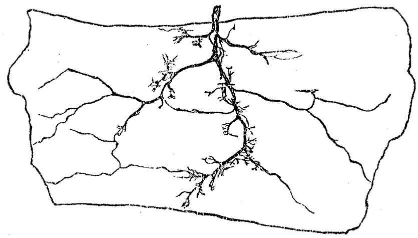
As a matter of fact, the older statements, long considered a good basis for opposing irritability, are incorrect, as for instance, that an excised piece of muscle in which no nerves could be seen with the lens did not twitch on stimulating it.

We can show you a little piece 3 mm. long from the end of the sartorius muscle of the frog, in which the best microscope discovers no traces of nerves, easily made recognisable by osmium-gold staining (fig. 1). Such a piece, transversely cut off, twitches as we know at each effective muscular stimulus. Pieces which can be obtained free from nerves from many other muscles, behave in the same way, as for instance pieces from the delicate muscles of the pectoral skin of a frog (fig. 2).

Further, the assertion was incorrect that everything that excited the nerve made the muscle twitch, and *vice versâ;* for we see here a

Fig. 1.

Fig. 2.



sartorius suspended in ammonia vapour, contracting powerfully, while a nerve entirely submerged in liquid ammonia appears wholly unstimulated, for it does not rouse the thigh muscles from their repose. (Experiment shown.)

Conversely, we see a thigh whose nerve dips into glycerine in maximal contraction, and on the other hand, a muscle in contact at its excitable end with the same glycerine remains at rest, yet it twitches if I dip it in up to its nerve-bearing tracts (11).

These are old experiments (12), and it is admitted they have overthrown the earlier opinion. But they have not been deemed sufficient to prove muscular irritability, because the ultimate endings of the nerves might have an irritability other than that of their stems. This is the only objection still raised. One could wish no other were conceivable, for this one admits of refutation.

To this end permit me to go a little into detail concerning nerves. Nerves are processes of nerve-cells composed of fibrils of immeasurable fineness, which in the so-called axis cylinder of the medullated nerves are united by a stroma inside a very fine membrane called the axolemma. In proportion to the microscopic dimensions of the ganglion cells of which the separate nerve-fibres form a part, these latter are for the most part enormously long, many as long as our arms and legs, and that is one of the reasons why the perception of the unicellular nature of the nerves made way but slowly. In fact it was not easy to accustom oneself amid the microscopic swarm of cells, to find single ones so grown in length that they could be wound about us like a cocoon thread. As it is the task and function of the motor nerves to lead towards the periphery the impulses sent out by their ganglion cells in the spinal cord, their activity always admits of ready perception through the muscular twitching. Even when the

nerve is divided and artificially excited at the peripheral end, the muscles betray it. On the other hand, no visible physiological reaction is found at the central origin of the motor fibre when stimulated at the periphery, so that at first we were quite in darkness as to whether in general it conducted centripetally. Nature, however, has presented us with a contrivance by which we are enabled to demonstrate the possibility of such an inverted or centripetal nerve-conduction. The contrivance consists in the branching division of nerve-fibres so frequently found in muscles, as will at once be seen in a preparation from a frog (fig. 3). In many muscles these branchings are so arranged that we can use them for an experiment as simple as it is conclusive of nerve-conduction in both directions.

Fig. 3.

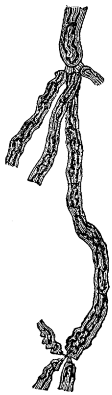
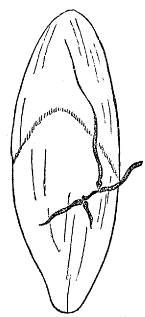


Fig. 4.



In the *gracilis* muscle of the frog the nervation is fashioned in the manner displayed schematically upon this diagram (fig. 4) and in more detail on the following (fig. 5). In reality the arrangement is like this. Now, if I cut up the muscle according to this diagram (fig. 6), we get at the tip Z nerve-fibres which are connected with the muscle-fibres at O and U only by the branchings at the points *xx*, but which in life served only for the parts of the muscle removed at *f* and *f′*.

An experiment (13), viz., the stimulation of *z* (fig. 6), will now convince you that nerves severed from their own muscle-fibres act quite well backwards upon those placed centripetal to them, which they can only do if nerves can also conduct centripetally, and so long as a path is preserved for this through the branchings. If we cut out the neighbourhood of the branchings it is all over with the reaction of the muscle.

We can make another experiment on the same muscle (14). We see that when we excite the lower tip of the muscle, only the lower portion twitches and not the upper. The two portions are in fact

FIG. 5.

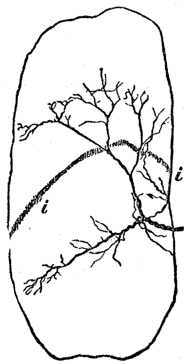
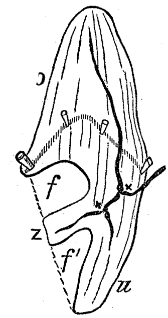


FIG. 6.



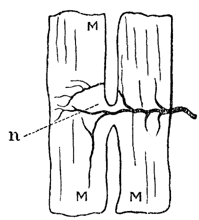
connected only by means of a very short tendon, the so-called *inscription*, which passes completely through the muscle (*ii* in fig. 5), so that it really consists of two muscles. If the nerve common to both is stimulated at any point, then both parts of the muscle contract, but if the muscle substance itself is stimulated, then the contraction travels no further from the place where the stimulus was applied than to the limits of continuity of the muscle-fibres.

The power of motor nerves to conduct in both directions is certainly of general significance in regard to the inner mechanism of nerves, but we have only approached it here, because it was necessary for the decisive proof of muscular irritability, as obtained in our last experiment with the *m. gracilis.* Whenever a muscle is provided with a nervation and branchings of the separate nerve-fibres like that of the *gracilis*, some group of muscle-fibres can serve to indicate whether a stimulus has affected this alone or the nerves lying in it as well. If nerves are present at the point of stimulation, and if the agent was at the same time a nerve stimulus, this is shown by the simultaneous contraction of distant parts which are accessible by means of the nerve's power of conducting in both directions. In cases where we can see the coarser nervation, the indirectly produced contractions can be predicted, and these form so certain a criterion of neuro-muscular excitations that by them the presence of the finest nerves may be proved, whose existence might otherwise be quite incapable of proof by any other means, as, for instance, by the use of the microscope. If these contractions are wanting, as was the case in our experiments with the lower end of the muscle, we know that either the spot stimulated is free from nerves, or that the stimulus employed was ineffectual as to the nerves and affected the muscle substance exclusively. In both cases then independent irritability is proved for those muscle-fibres which were directly excited and contracted.

Now since we have just employed an electric stimulus which is equally effectual on muscle and nerve, it follows that we had to do with the first case; that is to say, the muscle showed itself free from nerve at its end. We have reason for specially bringing forward this experimental proof of the absence of any kind of nerves in large tracts of muscle, because it compels those who in spite of all assume the presence of nervous matter in certain microscopic disks and striæ of the muscle-fibre as a whole, to deny that this supposed nervous element possesses any power of conducting in both directions or any irritability at all; for in fact it is not possible to excite the motor nerve of a muscle-fibre by any stimulus whatever applied to the actual terminations of the nerve within the fibre. The facts besides combine to prove, as need hardly be said, yet another proposition—they prove at the same time that pure muscular excitation does not travel back to the nerves.

This may be shown still better with the small pectoral muscles of the frog's skin than with the *m. gracilis*. We need only dissect it in

FIG. 7.

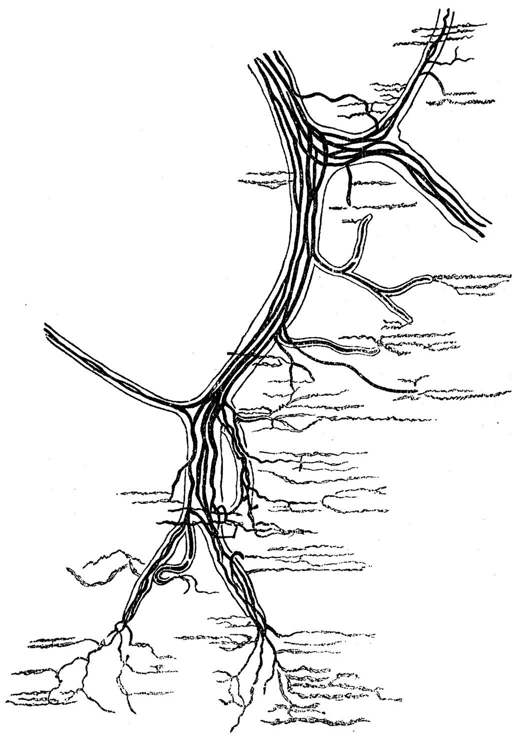


the manner shown in the drawing (fig. 7) and stimulate the spots *n* and M; if we stimulate *n* everything contracts, if M the excited half only.

The preparation which you now see (corresponding to fig. 2), and which shows the nervation of the very thin muscle with all the nerve-endings stained dark with gold, makes that relation clear, for here again in truth the result of morphological research is in gratifying accordance with results obtained experimentally. The muscle is seen to be for the most part free from nerves; indeed the entire nervation with all the nerve-endings might be said to be formed of one nerve *line* only, if we disregard the few digressing fibres which again in part are not motor.

Under rather higher powers we see the nerve-endings proper (fig. 8), the distinct demonstration of which by means of the gold method has now been achieved, in much the same way as here, in all the classes of vertebrates with the exception of the osseous fishes. In all cases these decisive preparations have proved that the vastly preponderant number of the muscle-fibres is entirely free of nerves, and that the

Fig. 8.



nerve-endings are confined to very small spots which we term fields of innervation. Most muscle-fibres have only one field of innervation, very long ones occasionally several, at the most eight. Thus the assumption, opposed to the idea of independent irritability, that muscle substance is well-nigh completely riddled with nerves, is refuted and rejected from the morphological side also.

From the absence of nerves in long tracts of muscle-fibre we immediately conclude that the latter shares with nerves the faculty of independently propagating its own excitation. This is what the beautiful microscopic observations of Sir William Bowman (15) on insects' muscles long since led us to suspect. As in the nerve so in the muscle, conduction takes place in every direction, and as the field of innervation almost without exception occupies a median position during a normal contraction, the conduction takes place in both directions, towards the tendinous ends. By way of distinction the velocity of

conduction is, according to species, temperature, &c., three to ten times less than von Helmholtz fixed it for nerve. As conduction in irritable tissues means nothing else than that one excited spot becomes the stimulus for the adjoining portion at rest, the independent irritability of the muscle-fibre comes into employment in every movement and during the entire duration of life; from the moment that the field of innervation becomes active all the muscle substance remains left to itself, and until the contraction is ended must be regarded as independent and acting in response to its own direct excitation.

Once clear on the fundamental question, and sure as to the method we have to employ in order to stimulate according to choice either muscle or nerve-substance alone, or both together, we may seek to determine in what respect the irritability of the two components of the motor machine differs. The differences as regards chemical stimulation appear very great; in respect of electric, thermic, and mechanical, on the other hand, only quantitative. However, under chemical stimulation, according to Hering's classical researches (16), a point formerly overlooked comes into consideration, namely, the complication introduced by the electromotive behaviour of the tissue, an automatic electrical stimulation one might say. When stimulation takes place by moistening the transverse section with conducting liquids, it is indeed difficult, if not impossible, to trace the chemical factor in presence of the electrical. Gaseous stimuli alone, like ammonia, have thus far remained free from the suspicion of acting electrically. To these a few others of similar action, such as bisulphide of carbon (17), have been added, and such as are conveyed to the muscle by the blood-vessels, and bathe the fibres from all sides. With these in particular we may class distilled water, which is excessively destructive to irritable substances, von Wittich (18) being the first who showed how strongly it stimulates muscles, while killing nerves without excitation. But, again, with this kind of stimuli, we cannot at present tell whether they do not set up in the tissues, over narrow but numerous areas, excitatory electric currents, thus working only indirectly by way of auto-electric stimulation. And since, finally, the same might apply to the thermic and mechanical actions which likewise arouse demarcation currents in the muscle, that is, to all stimuli, we find ourselves in the presence of the possibility of reducing all irritability to a reaction to electrical processes, and of seeing vital electricity elevated into immeasurable importance.

The means by which muscle may be stimulated interests us, in the first place, on this account—to ascertain, once for all, how it procures its excitation *in life*, or what may be the action of nerve upon it. Did we know that, we should have grasped at the same time the nature of nervous activity.

Nerves end blindly in the muscles; as a rule they are not even finely pointed, and still less do they spread out diffusely in such a way as might make the true ending difficult to find. They end quite distinctly. But the ends always lie beneath the sarcolemma, in such a way that no foreign tissue intrudes between them and the muscle, so that what is fluid in the muscle can directly moisten the nerve. The sublemmal nerve is clothed with nothing else than the axolemma. The nerve never penetrates into the depths of the muscle substance; on the contrary, it remains confined to the sublemmal surface of the contractile cylinder or prism. Each nerve end consists of several branches, like antlers, arising by division, which together form the terminal nerve-branch. Apart from the form of the antlers, this short description is exhaustive for many animals, since neither in the sublemmal nerve need any special additional structures occur, such as nuclei, nor any kind of modification of the muscle substance in the field of innervation. There is much to indicate that the nerve-fibre proper, or axis-cylinder, does not change its constitution in passing through the sarcolemma, still it is to be remarked that the twigs of the terminal branches, although as long as they live often apparently longitudinally striated, have not yet, even in the most favourable stainings, been found to present the general fibrillar structure of nerves.

According to these results of morphological research, it appears that contact of the muscle substance with the non-medullated nerve suffices to allow the transfer of the excitation from the latter to the former. The only strange thing is that in reversed order excitation of the muscle never extends to its own nerve. This is still stranger because, according to Matteucci's well-known discovery, a *foreign* medullated nerve simply laid upon the muscle is powerfully excited by the contraction—so powerfully that the smallest contracting muscle barely touching it in more than a mere point excites the strongest nerve, while, on the other hand, we never see muscles excited by nerves which are merely pressed against them.

In the investments, then, of the nerve and the muscle substance appears to exist one of the elements which admits the neuro-muscular excitation *exclusively* to the field of innervation, and among those investments it need not be the medullary sheath. The delicate membranes of the sarcolemma and neurilemma suffice, for muscle cannot be excited by superimposed *non*-medullated nerves. At any rate, I have tried in vain to excite muscles by the most intimate contact of the fine terminal ramification of the optic nerve in the retina or the *n. olfactorius* from the pike, or even the delicate nerves of Anodonta, by stimulating these non-medullated nerves.

If we imagine the activity of the nerve to start with a chemical process, and that a chemical stimulant, as du Bois-Reymond (19) once

suggested, is, at the same time, secreted in contact with the muscle, we understand very well the necessity of direct contact, and in this case it would suffice if the sublemmal nerve were to run in *any* form for a short distance under the sarcolemma. The branching then would mean the enlarging of the contact. But however rich and intricate the ramifications may be, we can by no means say they display throughout the principle of increase of superficies; on the contrary, they are often astonishingly poor and small. As concerns their form, they are *not* irregular, but so strikingly uniform that this point deserves particular attention as being apparently indispensable for innervation.

Instead of describing the forms, allow me to show you the object itself in a selection taken from the most diverse vertebrates. First from the Amphibia (fig. 9): rod-like branchings with long outstretched twigs, a form which crops up again in a remarkable way in many birds. The rule here is asymmetry of the divisions: all the twigs have the form of a bayonet.

FIG. 9.

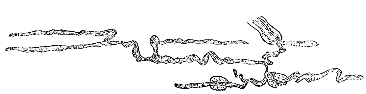
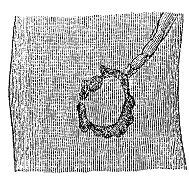


FIG. 10.



The following preparation shows the termination in the dog (fig. 10). Here the branches are crooked, and hence quite divergent, so that the points of agreement with the form of the Amphibia are at first overlooked. But if we examine the divisions, you will remark that these are again unsymmetrical and give off branches whose ends lie very diversely removed from the common place of origin. The ends are, as a rule, turned towards each other, and often so approximated that it is at times troublesome to find the gaps between them, and if they do not lie in the same plane they appear to be united into a ring. In other cases one end overlaps the other, but we then find that all the points of the branches which are turned towards each other lie at unequal distances from the nearest bifurcation. This law holds good in all the thousand cases of motor endings thus far observed and shows a strict order in the apparent chaos of these structures. And yet among the organic forms there is scarcely one which varies so much in other respects and often is so inextricably complicated as this.

The drawings (fig. 11, from the muscles of the guinea-pig, and fig. 12 of the rat) and a preparation from a lizard (fig. 13) may serve

FIG. 11.

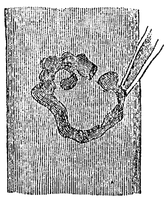


FIG. 12.

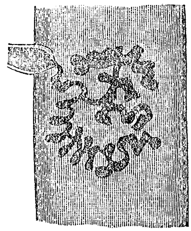
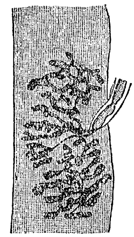


FIG. 13.



as a voucher for the truth of the above statement. We see there everywhere the hooks making their appearance with a short and a long claw, like the swivel we hang our watch on in the pocket.

The voluntary muscles of all vertebrates and of many invertebrates consist of fibres, the contents of which are perfectly regularly disposed in layers and transversely striped. For shortness, this striped mass may be called "rhabdia." This it is which has been universally identified with the contractile substance. But it has been ascertained that in many cases the nerve-ending does not come at all into direct contact with the rhabdia, but with another mass, which is highly nucleated and of pap-like softness. This latter is unstriped, and has all the appearance of protoplasm. It occurs in very varying quantity under the nerve-antler; in Amphibia, where the sublemmal nerves run out in a long course, it is not apparent as a separate layer, but it occurs more abundantly in the same measure as the branchings retract, and the field of innervation becomes smaller. At first it is found chiefly between the twigs, in the intervals of the branching, and then in the form of a "sole," which among the much contorted branchings of reptiles and mammals grows thicker, till it sometimes in some nerve eminences forms quite a thick cushion. Since we have succeeded in making the nerve-endings visible in uninterrupted series of very fine sections of mammalian muscle stained with gold, there can no longer be any doubt that the complete separation of the sublemmal nerves from the rhabdia by measurable layers of sole-protoplasm, though not the rule, is yet by no means rare, and that many muscles possess no other sort of nerve-endings than such as these with apparently indirect contact (20).

It would be difficult to understand why the innervation should have in some muscles, as in the Amphibia, no intermediate layer while having in the majority of cases an interrupted layer, and in others a continuous layer of varying thickness to traverse. But when we consider what the substance of the sole is, of what it consists, how it is distributed, and when we know its origin, it appears that it is

identical and stands in continuous connexion with the long-known second constituent of muscle-fibres, of which as well as of the rhabdia the fibres are composed. It is that substance, considered by Max Schultze to be the protoplasmic remnant of the cells composing muscle, which occurs in greatest amount around the nuclei of muscle, and extends in long threads throughout the entire muscle-fibre. So many transverse connexions occur on the very numerous stronger and finer nucleated threads that the whole mass, called sarcoglia, becomes a trellis-work almost of the same fineness as the better known transverse striation of the rhabdia, and everywhere surrounds and interpenetrates the latter. This minute internal structure of muscle has only become at all well known since the introduction of gold staining, thanks especially to Messrs. Retzius and Rollett (21). Had it been suspected earlier, and had we appreciated the volume of the sarcoglia whose existence is thereby shown and which rivals that of the rhabdia, we might have studied this component of muscle in its physiological relations to contractility, as well as in its morphological and genetic relations which are the only ones yet known.

If now in many cases it appears that the nerve comes in contact only with the surface of a thick layer of sarcoglia, while the rhabdia everywhere is covered by very fine layers of the latter, whose absolute absence in the field of innervation can nowhere be demonstrated, we have to conclude that in general the nerve does not act directly upon the rhabdia, but only on the sarcoglia. This at once gives the latter a physiological interest. We have to ask whether the glia is the medium that conducts the stimulus between nerve and rhabdia, or whether it is itself the contractile element while the rhabdia has a signification other than that formerly attributed to it when we were completely ignorant of the glia.

All contractile substance requires the co-operation of an elastic element. Where is this to be found in the muscle-fibre? The envelope of sarcolemma which is certainly elastic but delicate, and whose mass is almost infinitesimal compared with that of the muscle-fibre, cannot satisfy the requirement; but more solid structures freely distributed in the paste-like sarcoglia could perhaps do so, and such we find in the rhabdia, in the form of prismatic particles ranged with such constancy and with such regularity longitudinally and transversely, that we may hold them to be the elastic element. Then the sarcoglia would become the contractile element, and the nerve would have an easier task.

I could wish that this view might be accepted as an hypothesis. As far as I can see it does not contradict experience, for it only puts back the muscle nearer to the protoplasm and to all that is contractile, and so far coincides with experience that we find muscles in the same measure less elastic and more sluggish in protoplasmic

movement the richer they are in sarcoglia, as in the case of the red muscles, nucleated and rich in glia, which contract more slowly but with greater power than the white muscles poorer in glia which are quick and spring-like, and also the sluggish embryo muscles, in which glia predominates because as yet but little protoplasm has been converted into rhabdia; and further the cells of unstriped muscle-fibre, which are wanting in the regular transverse striation, and contain, as it appears, besides more abundant glia, an elastic material of special form and arrangement.

The hypothesis would be overthrown if contractile fibrils were found in which no sarcoglia was to be detected. But even in the finest fibrils of *Stentor*, the structure of which Bütschli (22) has recently elucidated, we must hold the significance of punctated transversely penetrating indentations to be protoplasmic, and we can therefore scarcely expect ever to find a contractile thread in which nothing whatever should be found of the primitive contractile material such as it everywhere exists.

Of late this view (23) has been defended from the purely morphological side (24), on the strength, namely, of the very fine reticular structure of protoplasm to which more attention is being paid, and which is demonstrable on objects of all grades of organisation. Protoplasm, in fact, is not so formless as at first appeared, but shows a structure comparable with nothing better than with the appearance presented by a transverse section of muscle with its glia framework stained with gold. We may expect that these reticular structures, whose consistency appears to vary extraordinarily, will some day lead to the establishment of a fruitful hypothesis of the inner mechanism of protoplasmic movement, in place of that held hitherto which affords no glimpse into the essence of vital mechanical work.

Compared with this larger problem, that of the causation of vital movement appears the more accessible of the two, the latter being considered as a physiological inquiry after the constitution of the normal stimulus by which work is done. Perhaps, indeed, the answer is to be looked for from the most perfected organisation of muscle, where the initiatory process is localised by a distinct nerve-ending, rather than from the primitive organisation where the excitation may set in at any place, and lies in the protoplasm itself. We know distinctly that the muscle-wave begins in the field of innervation, for we have long seen the natural contraction in the interior of transparent insect larvæ starting from the nerve eminences. We know this also from the experiments of Aeby, who followed the muscle-wave myographically from the nerve-line onward, and now we are able to display the beginnings of the contraction as local thickenings at the point of attachment of the nerves caught and fixed by sudden hardening. Since the nerve grasps the muscle in a restricted region it

expends its action upon this exclusively; that which follows on as muscular activity is the nerve's work no longer.

Galvani and his successors for more than a century suspected that nervous forces were electrical, and, in reality, the celebrated champion of electro-physiology in our day has been able with the galvanometer to render the excitation of nerves, unattached to muscles or ganglion-cells, evident as the negative variation of the natural nerve-current, to cause movement of a magnetic needle instead of a muscle, or to put the needle in the place of sensation. After this no consideration of the nature of nervous activity is conceivable which does not take into consideration this discovery of du Bois-Reymond's—least of all where the nerve has to excite something with which it is not fused, like muscle, but which it only touches, and that not directly, while still invested by the axolemma. Only during excitation, as Ludimar Hermann has taught us, are electric currents issuing from the nerve through its conducting surroundings, in which the course of these currents of action is to be estimated from the duration of the negativity of the nerve-tract excited, and from the speed of propagation of the nerve-wave, if we know the conductor and the disposition of the nerve. The motor ending fixes the latter, and so peculiarly that we can only presuppose from it a furthering of the excitor effects of the currents of action.

The currents of action of muscle, whose electromotive behaviour agrees so wonderfully with that of nerve, have long been proved to produce excitor effects, although only powerful enough to act upon nerves; but there are also, under certain conditions discovered by Hering, such effects from nerve to nerve (25). Is the possibility, we may hence ask, to be excluded of one muscle exciting another, and is it quite impossible that a nerve only throws a muscle into contraction by means of its currents of action?

The first question we can answer. I will do so by a simple experiment. Two muscles, the nerves of which are disposed of by poisoning with curare, need only to be pressed together transversely over a narrow area to make a single muscle of them of double length, in which the stimulation and contraction are propagated from one end to the other. Since the transference from one muscle to the other is done away with as soon as we bring the finest gutta-percha between the muscles as an insulator, or gold-leaf as a secondary circuit, the first muscle must have excited the second electrically (26).

NOTES.

1. The most complete exposition of these important later discoveries on the reproduction of the cell is to be found in the book of W. Flemming, 'Zellsubstanz, Kern und Zelltheilung,' Leipzig, 1882. *Cf.* the "Kurze historische Übersicht" (p. 385), with the quotations from the works of Schneider, Strassburger, Bütschli,

Flemming, O. Hertwig, and the researches of Auerbach, Balbiani, van Beneden, Eberth, Schleicher, Balfour, and others.

2. Ferd. Cohn: "Nachträge zur Naturgeschichte des Protococcus pluviatilis." 'Nova Acta Acad. Leopold. Cæsar.,' vol. 22, P. II, p. 605 (1850).

3. Max Schultze. 'Ueber den Organismus der Polythalamien.' Leipzig, 1854.

4. W. Kühne: 'Untersuchungen über das Protoplasma und die Contraktilität.' Leipzig, 1864, pp. 42, 66, 87, 102.

5. Kühne: *ibid.*, p. 30.

6. Th. W. Engelmann five years later confirmed the passage of protoplasm, especially of Amœba, to the spherical form on stimulating; *cf.* his "Beiträge zur Physiologie des Protoplasmas," 'Pflüger, Archiv,' vol. 2, 1869, p. 315, and 'Handbuch der Physiologie, herausg. von L. Hermann,' vol. 1, p. 367.

7. Engelmann: "Ueber die Reizung des contraktilen Protoplasma durch plötzliche Beleuchtung." 'Pflüger, Archiv,' vol. 19, p. 1.

8. Kühne, *l.c.*, pp. 50, 67, 88-89, 104-106. The cessation of the so-called sap-stream in the cells of *Chara* on excluding the air by oil was observed as far back as 1774 by Bonaventura Corti; and further by Hofmeister in *Nitella* under the influence of reduced atmospheric pressure. *Cf.* Engelmann in 'Handbuch der Physiol., von Hermann,' vol. 1, Part 1, p. 362.

9. *Cf.* J. Rosenthal: 'Allgemeine Physiologie der Muskeln und Nerven.' Leipzig, 1877; p. 255.

10. J. Gerlach: "Ueber das Verhalten der Nerven in den quergestreiften Muskelfäden der Wirbelthiere." 'Erlangen, Phys. Med. Soc. Sitzber.,' 1873.—'Das Verhältniss der Nerven zu den willkürlichen Muskeln der Wirbelthiere.' Leipzig, 1874.—"Ueber das Verhältniss der nervösen und contraktilen Substanz des quergestreiften Muskels." 'Archiv Mikrosk. Anat.,' vol. 13, p. 399.

A. Foettinger: "Sur les terminaisons des nerfs dans les muscles des insectes." 'Archives de Biol.,' vol. 1, 1880.

Engelmann: 'Pflüger, Archiv,' vol. 7, 1873, p. 47; vol. 11, 1875, p. 463; vol. 26, p. 531.

In these publications it is sought to prove that the motor nerves pass either into the interstitial nucleated substance of the muscle (therefore into the sarcoglia) or into the layers of the "Nebenscheiben." This latter view is opposed by, among others, A. Rollett in his thoroughgoing exposition of the structure of muscle (Vienna, 'Denkschriften der k. Akad.,' vol. 49, p. 29), and W. Kühne ('Zeitschr. f. Biol.,' vol. 23, p. 1).

11. The experiments were performed during the lecture by projecting on the wall images of the preparations enlarged some thirty times.

12. Kühne: "Ueber direkte und indirekte Muskelreizung mittelst chemischer Agentien." 'Müller's Archiv f. Anat.,' 1859, p. 213.

13. Kühne: "Ueber das doppelsinnige Leitungsvermögen der Nerven." 'Zeitschr. f. Biol.,' vol. 22, p. 305. To demonstrate the experiment on the *gracilis*, the muscle was fixed on a white piece of cork by needles, and held by elastic holders, and its image thrown on the wall highly magnified by a Krüss lantern.

14. Kühne: *ibid.*, pp. 312, 324.

15. William Bowman: "On the Minute Structure and Movements of Voluntary Muscle." 'Phil. Trans.,' 1840, p. 457; and "Muscle—Muscular Motion" in the 'Cyclopædia of Anatomy and Physiology,' edited by B. B. Todd, vol. 3, 1847, pp. 506-530.

16. E. Hering: "Ueber direkte Muskelreizung durch den Muskelstrom." Vienna, 'Sitzber. k. Akad.,' vol. 79, Abth. 3, 1879.

17. "Ueber chemische Reizungen; nach Versuchen von stud. med. C. Iani." 'Untersuch. aus der Physiol. Instit. der Univ. Heidelberg,' vol. 4, 1882, p. 266.

18. v. Wittich: 'Experimenta quaedam ad Halleri doctrinam de musculorum irritabilitate probandam instituta.' Königsberg, 1857, and 'Virchow, Archiv,' vol. 13, 1858, p. 421. In these papers, with the discovery of the excitation of muscle by distilled water, appears without doubt the first fact which overthrew the old theory of the equal irritability of muscle and nerve.

19. E. du Bois-Reymond: 'Gesammelte Abhandlungen zur allgemeinen Muskel- und Nervenphysiologie;' vol. 2, p. 700.

20. Kühne: 'Verhandlungen des Naturhist.-medicinischen Vereins zu Heidelberg;' Neue Folge, vol. 4, pp. 4, 5.

21. G. Retzius: 'Biologische Untersuchungen,' 1881.

A. Rollett: "Untersuchungen über den Bau der quergestreiften Muskelfaser." 'Wien, Akad. Denkschr.,' vol. 49, 1885.

22. 'Dr. H. G. Bronn's Classen und Ordnungen des Thierreiches, neu bearbeitet von O. Bütschli.' Leipzig und Heidelberg, 1888, vol. 1, p. 1298.

23. Kühne: "Neue Untersuchungen über motorische Nervenendigung." 'Zeitschr. Biol.,' vol. 23, pp. 88–95.

24. A. van Gehuchten: "Étude sur la structure intime de la cellule musculaire striée." 'La Cellule,' vol. 2, p. 289.

25. E. Hering: 'Sitzber. der k. Akad. zu Wien,' vol. 85, Abth. 3, 1882, p. 237.

26. Kühne: "Secundäre Erregung vom Muskel zum Muskel." 'Zeitschr. Biol.,' vol. 24, p. 383.

The drawings, figs. 1, 2, 3, 5, 8 are taken from the papers of Dr. K. Mays: "Histophysiologische Untersuchungen über die Verbreitung der Nerven in den Muskeln" ('Zeitschr. Biol.,' vol. 20, p. 449), and "Ueber Nervenfasertheilungen in den Nervenstämmen der Froschmuskeln" ('Zeitschr. Biol.,' vol. 22, p. 354); figs. 9—13, from the author's work in 'Zeitschr. Biol.,' vol. 23, pp. 1—148, Plates A—Q.

"Contributions to the Chemistry of Chlorophyll. No. III." By EDWARD SCHUNCK, F.R.S. Received June 19,—Read June 21, 1888.

Products of the Action of Alkalis on Phyllocyanin.—In the first part of this memoir I gave a general account of the action of alkalis on phyllocyanin ('Proceedings,' vol. 39, p. 355). I shall now proceed to give the results obtained on further examining the products due to this action. The description of the products appearing in the first stage of the process of change induced by alkalis forms the subject of the present communication.

The great trouble involved in preparing any considerable quantity of phyllocyanin in a state of purity made it desirable to find out a method, if possible, of obtaining the products of the action of alkali directly from chlorophyll itself. The object in view was attained by acting on chlorophyll first with alkali and then with acid, thus reversing the process previously adopted and at the same time leading to the discovery of several new and interesting compounds, the formation of which had not been anticipated.

FIG. 1.

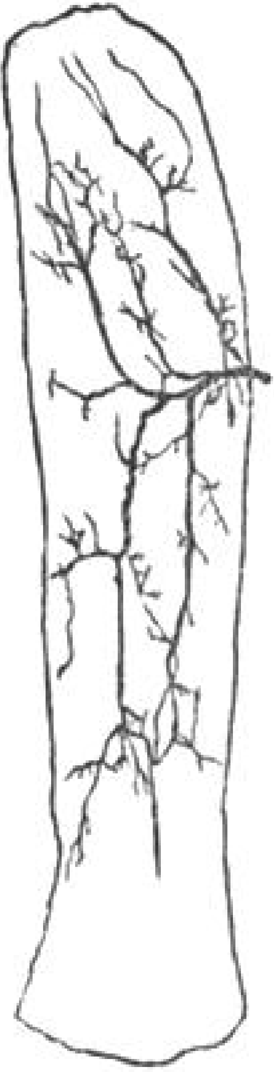


FIG. 2.

FIG. 3.

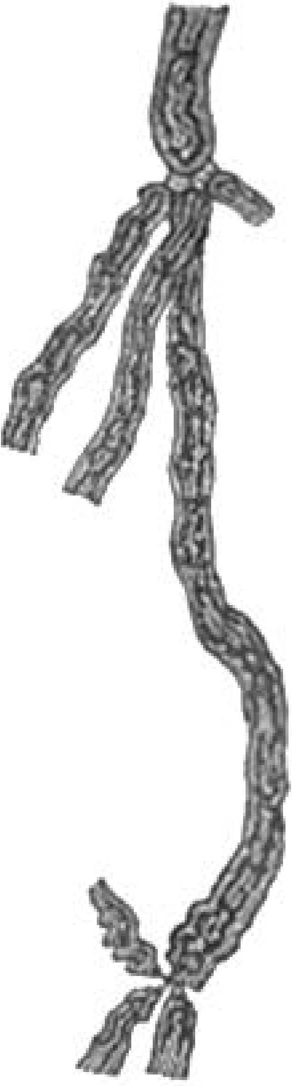
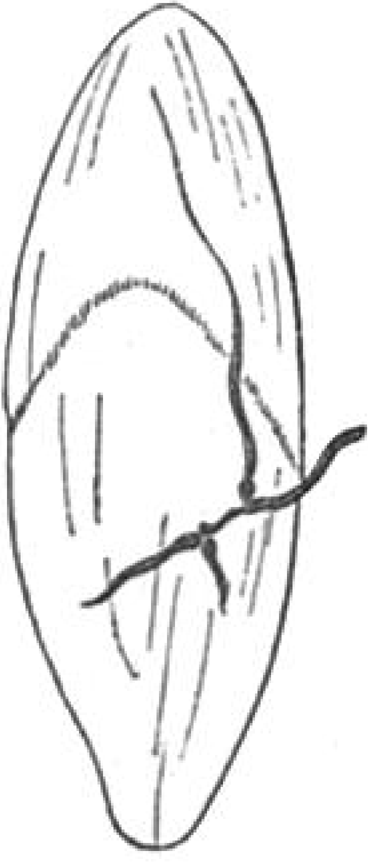


FIG. 4.



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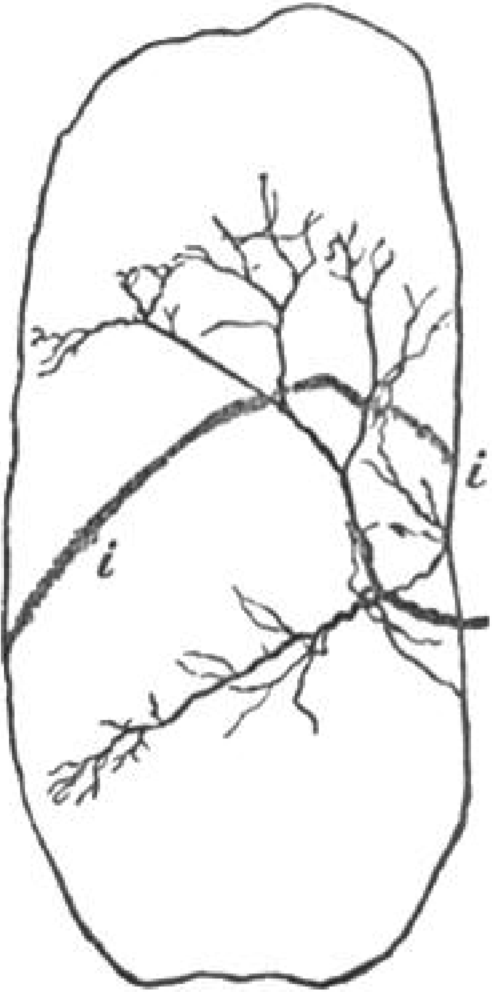

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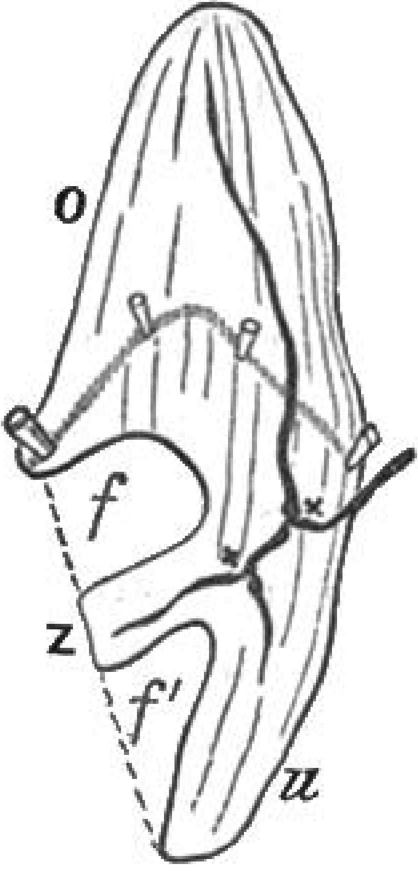

FIG. 7.

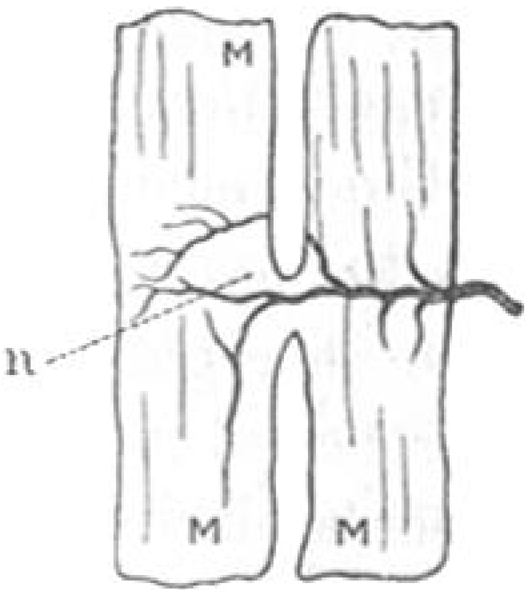

FIG. 8.

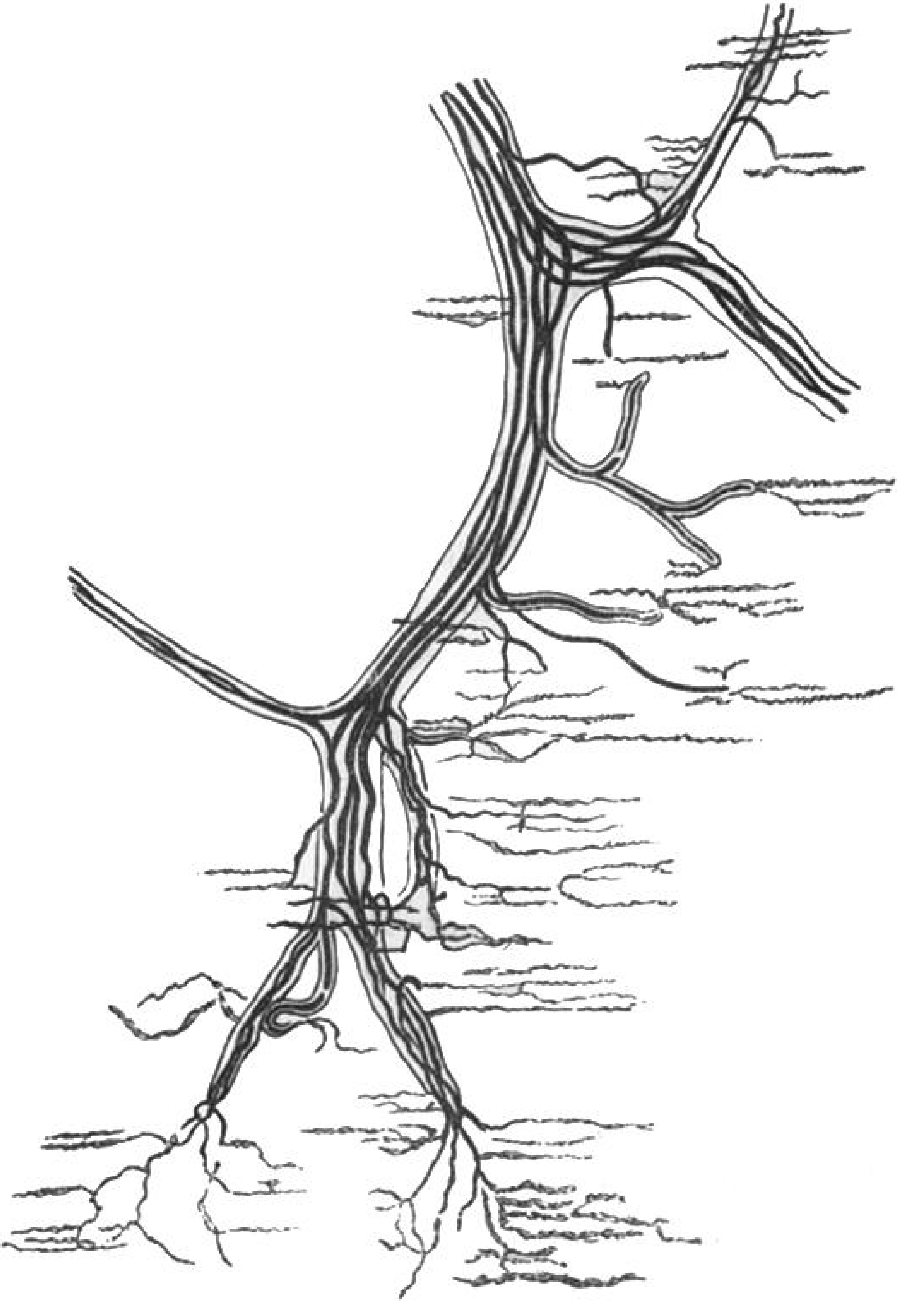

Fig. 9.

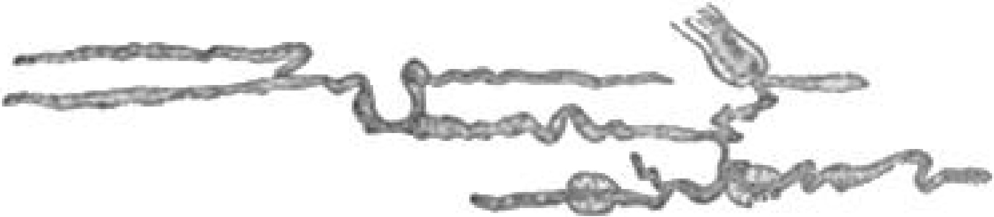

Fig. 10.

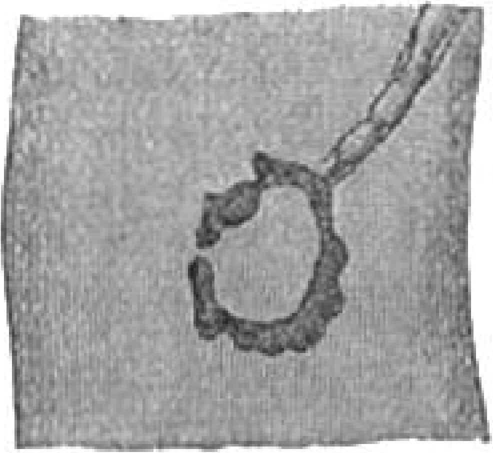

FIG. 11.

FIG. 12.

FIG. 13.