

The Journal OF Nervous and Mental Disease

Original Articles

ADDRESS TO THE AMERICAN NEUROLOGICAL ASSOCIATION*

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If I had anticipated how serious would have become to me the responsibility of addressing you as your president, I should have accepted with even greater reluctance this honoring office. The men gathered here to-day constitute a group of physicians who without flattery may well be called remarkable. Your books and papers are in many languages; your contributions to the art of medicine and to the sciences on which it relies for growth and useful additions are known and valued wherever good work is esteemed.

A variety of temptations assail the man to whom you confide the privilege of uninterrupted speech, and I for my part have had much hesitation concerning what I should endeavor to set before you. I begin by assuming leave to be at will digressive:

And first, before venturing on more difficult ground, I would remark upon certain matters to which I am inclined to call your attention. One is the rarity in your proceedings of matters of therapeutic interest. This applies elsewhere as well as to you, but may be due to the fact that the therapeusis of neural maladies has made no such advance of late years as has the art of the surgeon. The present is the triumphant hour of diagnosis, and this is after all the parent of logical legitimate therapeutics. Amid enormous gains in our art, we have sadly

*Read at the thirty-fifth annual meeting of the American Neurological Association, held in New York, May 27, 28 and 29, 1909.

to confess the absolute standstill of the therapy of insanity and the relative failure, as concerns diagnosis, in mental maladies of even that most capable diagnostician, the post-mortem surgeon. I am satisfied, from many facts in cases of depressive and other manias, that somewhere in remote toxic products—outside of the brain—glandular or other, we shall one day detect the secret cause of a proportion of what we label insanities. The last forty years have at least taught us that the asylum is advisable only when poverty or extreme peril leaves no other resource.

I am most desirous that both the physiology and pathology of the negro should receive from the neurologists of the South such attention as has been given to his anatomy. Long ago it was made clear in my clinic that the negro is comparatively immune to chorea, and that despite his frequency of syphilitic infection, he is very rarely the victim of spinal tabes. I have some reason to suspect that he is less subject to migraine than is the white man; and finally, I have gathered evidence, as yet unpublished, that in the pure black exophthalmic goiter is exceptionally rare.

Concerning the relation of the black skin to heat and light rays, there must be in the negro some compensative automatic mechanism for regulating body temperature other than that which we possess. We need a new and full study of the blood in the negro. Why is he relatively to the white immune to yellow fever; would his serum protect; and why is he less susceptible to malarial fever than the white? The explanation of these insusceptibilities should challenge the attention of the tropical bacteriologists. What too can be learned of the pigments of the negro and of the relation of the suprarenal capsules in him to pigmentation? Does the black ever have Addison's disease? I saw, years ago, a negro in an extreme state of weakness who was spotted with large islands of white skin. The general symptoms suggested Addison's malady, but he disappeared from my clinical horizon, as interesting cases are apt to do.

Another untouched subject which concerns us might well be handled by a commission. I refer to the physiology of persons who have lost large parts of the body by multiple amputations. In these cases the temperature, pulse and blood pressure should reward careful research. Long ago I studied the diseases of stumps and the psychics of lost limbs, but I am not so vain as to think no one can better my work, although one eminent psychologist has confessedly failed to do so.

Leaving here these lesser matters, I turn to the first of the main subjects to which I ask your attention. In the more recent physiologies the chapters on the nervous system begin with a frank confession of ignorance concerning the nature of the excitatory nerve impulse. It has long puzzled the physiologist, illustrated ingenious failures, and found no place in the text-books of neurology. So utter is the laboratory's defeat that we too have accepted the consequence and scarcely even think of it in connection with the many diseases in which it plays so large a part.

A dozen labels record once confident theories as to the nature of this transmitted impulse which, without committal to any hypothesis, I shall for convenience call neural energy. The essays which report the laboratory on this matter might bewilder the most clear minded.

It is, I think, unfortunate that the transmission of energy in both animal and plant should not have been studied in comparison with the forms of exterior physical energy by some of the great masters of physics. To clear the ground, and at the risk of restating the familiar, let me say a word of what little we know, with more or less of certainty, of the phenomena of transmitted energy in plants and animals, and some of its comparative relations to the physical energies of the inorganic world.

The higher animal has ganglionic storehouses of potential energy which can by nerve tracts reach muscles and become kinetic. The nature of the energy thus transmitted is what I now ask you to consider with me. Setting aside the many other errands of this missive energy, let us dwell somewhat on its relation to muscular motion.

Conduction of energy along nerves to muscles was once crudely demonstrable through the visible responses of irrito-contractile muscle-tissues or by the accident of a severed nerve. The demonstration that the passage of energy gives rise to measurable electric signals by degrees reopened the way to new studies of this molecular change. In reminding you of familiar facts I am not without distinct purpose.

When we approach the study of the transmission of energy in its relation to animal structure, it is interesting to observe that in the lowest animal form—such as the amoeba of the protozoa—there is, on stimulation at any point, a diffused transmission of energy through all parts of this unicellular body. It is like the excentric waves from a stone let fall in water.

Without troubling my text with needless names, I may say that in some higher creatures, as also in the fetal heart, irrito-contractile cells appear to have conductive functions as well as contractile power.¹ In the next higher sub-types² we may still have the diffused response, or in some classes at last preferred paths, until we come to creatures whose sole mode of conduction is along distinct nerve ways. As these nerve tracts become more and more definitely developed, the ganglion storehouses of potential energy become more numerous and of increased importance.

My object being rather to comment on acquired knowledge than to hope to add, I shall but venture to call attention in our interest to the rate of transmission of nerve energy and to point out that when it is mere diffusion, as in amoeba, it is at its slowest (0.93 m.) three feet per second, and that in the higher vertebrates it is at its ultimate centrifugal rate of about (31 m.) 100 feet to the second, and on centripetal missions (47 m.) 150 feet to the second. The finer the organization the quicker the rate. It is as when in a wild land a man wanders unguided until with the trail, the path, the road and the railway his movement becomes more rapid as the ways on which he may move are defined. It is probable that in many diseases the rate of transmission of energy accepted as normal varies. This is an almost unexplored subject. Is the speed slower in some asthenias? That would be worth knowing. In certain spinal maladies several seconds may be required before the mind becomes conscious of a sensation of touch.

I just now said that the higher the organization the swifter is the rate of the movement of energy along nerves. This may be true of the trained individual as well as of the genus. He who watches the quick decisions and rapid actions of the best base ball players may well suspect that the nerves as well as the mind may share in the acquisition of exceptional rate of transmission of orders to the muscles.

The highest speed as yet assigned to neural transmission in health must be far below the speed of the impulses productive of the most rapid speech, viz: 250 words a minute. I suspect that also in the juggler and the practical pianist our accepted maximum may be much exceeded. I am here tempted to discuss the

¹ Metazoa—multicellular creatures of low type belonging to the Coelenterata.

² Cnidaria.

matter of the enormous output of energy relatively to the small size of the productive sources of supply, but must leave this to a note³ which I owe to Professor Donaldson's kindness.

Next, using only what concerns us, let us follow the outgoing energy on its way to the irrito-contractile muscles, setting aside its numberless other errands. It is admitted that the muscles may become exhausted and at last refuse to obey either the will or the electric stimulus.

It has long been accepted as proven that the nerves do not exhibit fatigue by any failure to respond to such electric excitation as does not obviously affect the integrity of their tissues. Were this true they would in this resemble the metal wire, which no length of employ as a conductor of electricity in any way alters. As to nerves I have long distrusted the decisions of the laboratories in this matter. Of late they are beginning to revise their beliefs. Even though we admit that electricity, the only excitator available for experiment, does not obviously exhaust nerves, we are far from knowing whether the normal nerve energy of the will may not do so. It is for us a question of practical moment.

In any distinct asthenia we should be able to answer certain questions. Is it muscular? Is it neural? Is it a ganglionic defect due to failure to store and restore the material for the production of potential energy? Or what combination of these possibilities have we to deal with?

Many agencies affect these delicately responsive neural conductors and their related centers. The one which is most common is variation in the atmospheric pressure in association with other storm conditions, such as humidity, heat, electricity, and the

³"I should like to emphasize the fact of the small amount of substance composing the cell bodies of the nervous system, indicating, as this does, how trifling in amount must be the metabolic changes in these master cells.

"At the same time these cell bodies give rise to fibers (that is the axone surrounded by its medullary sheath).

"I have been able to determine that in the peripheral system the volume of the sheath and the volume of the axis are equal to one another, so that one-half of the substance of a peripheral nerve, excluding the connective tissue, etc., is composed of axone substance. (See Donaldson and Hoke, 1905.) Unpublished observations by Donaldson and Hatai show the same relation to be true for the nerve fibers in the central nervous system.

"It follows from this that, on the average, each cell body is connected with a mass of axone substance about twenty-five times its own volume. This represents a set of anatomical conditions which are in need of detailed investigation."

ozone of the atmosphere. I know of but two essays on this subject in which the matter is placed on a scientific basis. Indeed we are very ignorant of the means by which, in rheumatisms and spinal maladies, in neuralgias and megrims, storms seem to increase pain or to determine attacks. We are but little wiser⁴ than Jenner when he wrote verses about the "Signs of Rain":

"The hollow winds begin to blow,
The clouds look black, the glass is low;
Hark! how the chairs and tables crack!
Old Betty's joints are on the rack."

So far the laboratories have failed to find proof that chemical changes accompany the transmission of neural energy along nerves. This decision is one which the laboratory of the mind refuses to accept as final. These living wires, the nerves, eat, drink and eject, are fed and do work, and must therefore undergo chemical changes through loss and for repair. We have as yet to admit the amazing fact that during the most violent functional excitation of a nerve no rise of temperature can be detected. Admitting that there must be chemical activities during the transit of energy, this becomes the more remarkable. That, however, such chemical reactions may occur as are of a nature to lower rather than to raise the local temperatures may not be an altogether absurd conjecture.

In our search for the nature of the molecular disturbance which we call nerve energy, it is interesting in passing to mention certain peculiarities which appear more and more to make it seem remote in quality from the ordinary physical energies. Thus, no evidence exists to prove that a nerve in action affects by induction a nerve beside it. Nerves do not appear to leak energy as do electric wires, and the avalanche theory of their gain in power during the passing of energy has been, I believe, disproved. It is of course familiar that many drugs destroy conductive capacity, as does the least break in a nerve, or pressure, or freezing. Yet even these facts which make for dissemblance are not all of them certain, and almost any statement I have had to make illustrates the insecurity of our present knowledge.

⁴With the aid of government weather maps it would not be difficult to obtain fuller information concerning the influential effect of the weather on many disorders. An excellent study of the influence of storms on the production of rheumatism, by Morris Lewis, is an illustration of what may yet be done.

See also case of Captain Catlin, *Am. J. Med. Sci., N. S., Vol. LXXIII*; and *Tr. Phila. Col. Phys.*, 3 Series, Vol. VI.

See also papers on Chorea by S. W. Mitchell and Lewis.

We come at last to ask whether the transmission of stimulus to muscles, of inhibitive impulses, of vascular and glandular excitations be by a mode of physical energy only exhibited through organized tissues, or by a system of minute, swift, orderly chemical interchanges, themselves the parent cause of the electric phenomena which signal the passage of a form of energy along a nerve.

There is much in all these facts to make us doubt if neural energy be at all like the great exterior physical energies such as electricity. But whatever be this transmissive impulse in animal life, chemical or physical, the primary determinative agencies which start it on varied errands, whether within, mental, or from the outside, sensory, will I fear—as a poet, half a doctor, said of a far different matter—“tease the soul with thought” for many a day. I repeat that this ultimate question of true nature of the stimulating energy in animal life radically concerns us and our work.

Maxwell believes with Hoff and Arrhenius that the effect of heat on reactive velocity affords a trustworthy means of discrimination between chemical processes and those accepted as physical. A rise of ten degrees of temperature increases the velocity of a chemical reaction two or three times, while no known physical process is to any like degree thus accelerated. In a paper of great ability, strengthened by Loeb's observations on artificial maturation of the eggs of *Lottia*, Maxwell concludes that the temperature coefficient of the speed of the nerve impulse⁵ indicates definitely that the conduction is a chemical process. If it should finally appear that the activities of the nerves are due to the mysterious propagation of inconceivably swift chemical processes, it will at once offer us a field for explanatory research in regard to the baffling problems of a wide range of neuritic maladies. The trend of opinion runs just now toward chemical theories, and it is worth while, as we mentally sum up the objective argument, to see what here a great observer, Jacques Loeb, permits me to quote from a letter, in which he dwells, with the cautious reserve of the trained scientific mind, upon the form this large problem is now taking. Repeating in his letter the conclusions of Maxwell, he writes:

“The gaps in our knowledge are of such a character as not

⁵Using the giant slug (*ariolimax columbianus*), (440 mm.) one and four-tenth feet a second.

to allow us to go beyond this statement, viz., the temperature coefficient of the nerve impulse indicates that chemical reactions are involved in the propagation of impulse. In which way they are involved we cannot exactly state at present; we only know that the salts, especially the ratio between the concentration of the sodium ions and the concentration of the calcium ions in the nerve, play an important rôle in this sense, that within certain limits an increase in this ratio causes an increase in irritability of the nerve, and a decrease has the reverse effect. It also seems pretty certain that oxidations are a necessary pre-requisite for the life of the nerve, and possibly also for the propagation of the nerve impulse. Rather," he adds, "than make hypotheses at this stage, I feel, with Professor Ernst Mach, that scientists must learn to endure an incomplete world-view. Our hypotheses mainly serve as an attempt at escaping an enforced condition of intellectual slavery."

Considering the tentative views held by Loeb and others and the present laboratory indecisions, you will see how far we are from the practical helpfulness which as neurologists we should anticipate if the nerve impulse could be proved to depend on minute and essential saline interchanges. I am personally of opinion that this research should recommence with fresh studies of that form of excito-contractile impulses which is found in plants and is probably identical with that observed in animals. The resemblance is so striking as to have over and over caused the assertion of identity of the phenomena of transmission to be made by botanists. If they are right, it must, I think, follow that whatever explains transfer of excitatory energy in the one kingdom must equally explain it in the other. But while this question in animal life has been variously approached, in the vegetable kingdom the possibility of chemical explanation has been as yet very little considered.

As I, who am not a botanist, am talking to men probably unfamiliar with vegetable physiology, I may be pardoned if, with brevity, I try to arouse in you concerning these two correlated physiologies the interest they have awakened in me.

For perhaps individual reasons many men have some curious sense of mysterious relation to the outer world of trees and flowers; but he who has watched the mimosa, in its most sensitive condition, make its quick responses, or as if its mood had

changed, its moveless state of temporary unresponse, must be dull indeed if for a time the logic of the imagination does not startle him with some strange sense of kinship. I can well imagine a neurotic crusade against cruelty to plants after watching experiments on *mimosa*. The hysteric protests of the vivisectionists of truth against our noblest work have almost reached this limit of folly.

There is stored in plants potential energy which for functional use passes through protoplasmic threads on which variously responsive cells are strung. The analogue of the irritito-contractile muscle of animal life is in the plant cell walls, which contracting expel fluid and thus cause in the leaves mechanical changes of position. This most interesting process may be determined by many plants by a mere rude pinch of a sub-leaflet. The form of voyaging energy, causing cell wall shrinkage and exosmosis of intra-cellular sap, may be chemic or physical. It is far more open to observation than is its animal analogue, neural energy, and whether here too, in the animal, as the muscle shortens there be any hydraulic interchange is perhaps worth a thought.^{6, 7}

In certain of the lower forms of vegetable life, like the *Myxomycetes*, the excitatory impulse at a touch moves diffusely, as in *amœba*. In *drosera*, the well known fly-trap of our marshes,

⁶ According to all that we know as to the condition of turgescient cells, from DeVries' researches on plasmolysis, Pfeffer's descriptions, and my own considerations and experiments, it can scarcely be doubted that the cellulose walls themselves are always in a high degree permeable to water, and that the condition of turgescence of the cells depends upon the protoplasmic utricle opposing the expulsion of the endosmotically absorbed water even under high pressure. A sudden escape of water from turgescient cells can thus be rendered possible only by this property of the protoplasmic utricle undergoing some change, or, in other words, by the hitherto non-permeable protoplasm becoming permeable in consequence of the stimulus, and thus letting water escape. We see thence that in the case of the irritable organs of plants two essentially distinct points come into consideration; on the one hand the action of the stimulus on the protoplasm, and on the other, the extensibility and elasticity of the cellulose wall.—Sach's "Physiology of Plants," Translation, pp. 653-654.

⁷ The visible effect (that we can exactly and appropriately call stimulation effect, in *mimosa* species—three or four at least—in *cassia*, etc., amongst *leguminosæ*, of *oxalis* species in *oxalidaceæ*, and of *droseras* and *diomæas*) of application of any form of contact energy to above plants, when the stimulus reaches an irritito-contractile center, is, we believe, (1) that stimulant energy causes molecular change in the protoplasm so as to render it porous or readily permeable to water; (2) that almost simultaneously the complex aggregation substance of each cell becomes decomposed, and in the process gives off water from its substance; (3) that some of this water escapes through the protoplasmic pores or gateways into intercellular spaces, so as to cause flaccidity in the cells and resulting motion of leaf or leaflet.—John M. Macfarlane.

this is true of any part of the half-leaf, but there are also distinct paths of distributive energy through the sensitive, hair-like threads. In *mimosa*⁸ this transmissive energy may be started above in the leaf and move down, or started below and move up. It may be sent on its way by a touch of ice or a hot needle or electricity applied to the stem or to the cushion (pulvinus) at the junction of leaf and stem. Even the stipules of each leaf have power to respond. Following up a suggestion made by me, Professor Macfarlane has shown within a month that if the small roots of *mimosa* be cut across, in twenty-five to fifty seconds the topmost leaflets in a plant some eighteen inches (0.45 m.) high begin to fold together.⁹

In the plant, as in the lower animal, the rate of transmission is slow, as seen in the lowest plants capable of visible response. In the *mimosa* the transmission is very tardy from root to leaf, but in the primary sub-leaflet it is not less than one inch (25 mm.) per second, and in some cases equals in speed the normal nerve rate in man. Recovery after making response to stimulation is, compared to that of muscles, slow, even when the environing aids are most favorable, and necessarily so because of the peculiar endosmotic mechanisms and the time requisite for refilling the cells concerned in the production of movement.

I leave untouched the assertion that plants—all plants, says Bose—have what he calls nerves; this is much questioned and must be relegated to the decisions of the laboratory.

Let me say in conclusion to this part of my theme that the negative galvano-electric variation accompanies the motor response to stimuli in *drosera* and *dionaea* as it does in the animal. Excitatory stimulation then is a phenomenon found in both kingdoms, and, as it seems to me, is less likely to find an explanation in physical than in chemical activities.

The one dissemblance is the absence of reflex movements in plants and any proof as yet of receptive centers whence excitatory energy is sent out on definite tracks. With this exception it thus seems that all the forms of response to excitation are to be found in a rising scale of perfection in plant and animal, and are

⁸ In *mimosa* there are distinct tracks which lie between the outer bark and the ligneous center. This space is full of closely packed crystals, which extend from the roots upward, and are believed to have some distinct function in the transmission.—John M. Macfarlane.

⁹ This curious fact, that only the upper leaves responded to injury of a root, suggests a further research, such as is now being made in the botanical laboratory of the University of Pennsylvania.

modified and governed by the increasing rise in complexity of structure, by favoring environment, and by generations of habit uses.

I have dealt inadequately with a question too large for the time of an address. I trust that I may have left with you some thought-stirring sense of the resemblance of the excitatory activities in the double dominion of organic life. I should like you to share the feeling my imperfect summary of a really large study has left with me, that we are on the way to a fuller comparative consideration of the forms of energy which are seen in plant, animal and the outer physical world.

Again I venture to digress and shall ask your careful reattention to a less considered subject with which our diagnosis is daily concerned. I wish to set before you some neglected facts and questions concerning the class of symptom signals we create and call reflexes. The line of thought will take us along paths which are most attractive.

In 1875 Westphal and Erb taught the value of the knee jerk muscle response. In 1883 Jendrassik discovered that it was possible to increase it by strong hand closure. He made no attempt to explain this fact, and here, with no further comment or addition, he left the subject. In 1886 Dr. Morris Lewis and I explored this fertile field and, using for the first time the term "reinforcements," made a long study of these phenomena with many interesting resultant discoveries. This research suggested and thus brought about the careful laboratory studies of reflexes and reinforcements and accurate determination of their times by Bowditch. His admirable work was followed by the laboratory research of Howells on mentation and emotion as reinforcements. A notable series of papers followed, by Lombard, Noyes, Heins, Taylor, Reichert, and Eshner, all American contributions. It is to this subject I desire to return. Of course, if I had not some novel views to present and some fruitful thoughts to offer I should not venture to bring before you what must be more or less familiar.

For us a reflex act should mean a definite, involuntary motor response to a definite excitation. There are but two forms—one, where an abruptly stretched muscle gives an answer by contracting; the second, where there is a muscular reply to an excitation of the skin. The first, which the books call a deep reflex, we ventured to speak of as muscle-muscle reflex, or for brevity, m.

m. r. The second we named the skin-muscle reflex, or briefly, s. m. r. I commend these labels to your use as convenient and descriptive.

The muscle to muscle reflex, best represented by the well known knee jerk, is, as you know, increased by reinforcing agencies. It is with these I would first deal, since the subject needs to be recalled to the attention of both clinic and laboratory.

Certain of the extensor muscles exhibit reflex motion on abrupt stretching. These reflexes are made larger, as we see daily in the knee jerk response, by strong hand movements. But, as we showed, much more feeble voluntary movements reinforce. To wink, laugh, swallow, speak, or to bend a finger, can be proved to reinforce these reflex responses. More than this, as we were first to discover, nearly every distinct sensation increases them. Sudden light or sound, remotely applied heat, abrupt cold, a pinch, the pull on a single hair, add to the response.

Emotion is the most efficient and lasting reinforcement. Next in power is a quick freeze of the skin. Motor reinforcement is the least effective. We may use for large influence combinations of violent hand closure with skin freezing, or a sharp pinch, and thus variously increase the resultant reflex reply. These reinforcements are not merely refined laboratory records but are the crude observations of the clinic and plain to be seen.

We may reasonably infer that all voluntary motion and all sensation, mentation and emotion have the same effects, even when too slight to be registered visibly as reinforcements. If we strike at one time two or even three tendons and close one hand or wink forcibly, all three muscles respond to the one reinforcement. The phenomenon then is a general one. The afflux of energy giving increase of a reflex appears to be due to a liberation of energy far in excess of the functional needs of the active organ. This excess we called overflow. It appears to be always sufficient to pass from the centers and, traversing numberless nerve paths, so to affect certain lower ganglionic motor centers as to add to their power of response to a coincident external stimulation.

This overflow appears to be always of cerebral origin; and whether, in inflammatory affections of spinal ganglia above the level of the one concerned in the reflex, they too may thus reinforce the lower center, I do not know, but I have suspected this to be in some cases probable. This is certain, that after section

of the cervical spine in dogs, a knee reflex can be had after a time, but is not reinforcible as in health by irritation of the nerves of the opposite leg.¹⁰ The cerebrum is needed.

To illustrate further. A flash of brilliant lightning illumines a landscape and affects the retina. The nerve energy thus set in motion, passing from station to station, causes reflectively winking and pupillary contraction. Each ganglionic center through which it moves sends forth an overflow of energy until at last there is, so to speak, a memorial impression of the thing seen left in the cuneus and a final overflow from this ganglionic center. If the primary sensory influence has been abrupt and very powerful, a general muscular movement tells us how enormous must be this distribution of energy. A less violent retinal stimulus, passing by many channels, is remotely detectible only as it reinforces an artificially evoked reflex, such as the knee jerk.

A sensation to reinforce must first reach the cerebrum, and thence by overflow affect a coincident reflex. It follows thus a longer track than the motor reinforcement, and the two modes of reinforcement must of course have different lengths of time. Several explanations of reinforcements occurred to us. That of overflow is the one we accepted. It has been strangely neglected, and yet what can be more interesting under any explanation than the conception of these large or small waves of liberated energy continually flowing through the body until lost by translation into forms of kinetic or other energy. So capable of proof, so demonstrable, this overflow, usually unfelt and unseen, must yet be constant; and surely scarce more amazing is the circulation of the blood. This flushing of the body with energy may have influential values in health and effects in disease as yet unknown to us and unsuspected. If the mere statement of it does not leave with you the bewildering conception of multitudes of nerve currents momentarily set free for remote transmission on unknown errands, I shall feel that I have inadequately presented a neglected and very positive phenomenon.

My best excuse for dwelling on this subject is that little or nothing is said of it in text-books. It seems to me to be a discovery of the utmost importance in physiology and to have some explanatory value for neurology.

If you see no fault in our conclusions you may feel interest in some of the further thought suggested by this seemingly

¹⁰ Reichert.

wasteful expenditure of effluent energy. How far the overflow goes we do not know, nor whether its direct effect ceases in the lower ganglia or passes on by the nerve channels of nutritive influences into the muscles and has something to do with preserving their tone. A fact to be stated presently makes this not unlikely.

Is it not conceivable that there are acquired conditions of the body, neurasthenic and hysteric, in which these endless normal overflows may be mischievously felt and may account for suddenly visible symptoms of nervous excitement?

To will movement of the lost hand after an amputation at the shoulder is a good reinforcement and is accompanied usually by a disagreeable feeling referred to the absent member. The sensory representation of a willed movement is normal. We may therefore suspect that what we call motor reinforcements may be after all in a measure due to coördinate excitation of centers appreciative of grouped movements. Indeed to will motion of the external ear muscle gave good reinforcements with persons who could not stir them by any such effort. A step further brings us to the possibility that some distinct acts of mentation give rise to reinforcing overflows of nerve force; all mental activities may add to the enormous number of ever moving currents of released energy.

When we recall to mind the form of words, verse or other, we may at will set in action the centers which render it audibly, or, as we put it, we may say it to ourselves without open speech. If a thin inflated rubber ball be put under the tongue and connected with a tambour and long recording pen and rotating cylinder, there is only a line record while the mind is at rest; but if we dumbly but strongly memorize some definite bit of prose or verse, the needle at once records in large tremors on the rotating cylinder the fact that unfelt movements of the muscles are affecting the rubber ball beneath the tongue. It is, therefore, I think, also probable that when we will any muscular action, or rather think of it without setting the muscle into visible motion, this would be the equivalent of unspoken speech, and there may be detected in the muscle concerned tremors capable of like means of record. This awaits and invites laboratory research. If the inference I make be justified by the laboratories we shall have an added proof of the constant diffusion of energy, now to be demonstrated only through the evidence afforded by the reinforcements

of reflexes. These illustrations of remotely active neural influences have some relation to the matter of reinforcement overflow, on which I fear I may have dwelt too long.

There are abrupt occurrences which we know as the complex symptom "shock," when the passing out of a gall stone or the sudden crush of a joint by a bullet gives rise to parietic feebleness, sweat, and lowered arterial tension. The shock of profound emotion may present similar symptoms. A vast overflow of released energy from violently assaulted sensory centers seems to me to account reasonably for what happens in these cases.

This hypothetical explanation is suggested by Bowditch's discovery of the temporary loss or enfeeblement of the knee reflex which follows even moderate reinforcements. The reinforced center is for a time more or less exhausted—a fact also proved by us from the failure of response upon over-use of reinforcements.

I turn for a moment to the reflexes which we ourselves labeled skin-muscle reflexes, and, for brevity, s. m. r., such as the sole reflex.

A recent able book on neurology says the deep and superficial reflexes differ in no way anatomically or physiologically. There is one difference which is never noticed and which I cannot explain and about which, because of intrinsic difficulties in the study, we may have been mistaken.

No form of motor or sensory activities reinforces the response to irritation of the skin-muscle reflexes, such as the sole, for example, or the more marked cremaster reflex. The cremaster is the one reflex unrepresented in women, but whether it may be invisibly represented by responsive movement on the part of the muscular tissues of the ovarian ligaments may be suspected.

Some of the practical applications of attention to reflex reinforcements may be worthy of statement. In a case which I saw with Dr. Osler, we felt sure that a fracture of the dorsal spine had isolated the brain from the lower cord. The summation of stimulus by repeated blows on the knee tendon at last brought out a slight response. Then and not before, violent hand motion, with grimace of the face, enabled us to get a full evocation of the knee jerk. This was a distinct signal of there being still open a neural pathway from the brain. It may be therefore that strong exercise of the upper limbs will remedially

aid to keep in condition lower centers when these are partially insulated by disease.

What are the tracts by which motor and sensory reinforcements reach the spinal centers we do not know. In some cases of absent or lessened knee jerk from disease or violence to the cord when the motor reinforcement has failed to act sensory reinforcement has won a reflex answer, or a combination of the two has succeeded. It is a question not without clinical interest.

Dr. Morris Lewis and I discovered that, when responsive to a blow on its belly, a muscle obeys all the laws which govern reinforcements of a muscle stretched by a sharp jerk from its tendon.¹¹ This whole range of reinforcements has failed to attract clinical interest. Why the flexors in general do not afford reflex replies is still to be learned. The failure seems to be due to a normal lack of excitability somewhere in the reflex arcs and is, after all, a matter of degree, for in spastic disorders the m. m. reflexes can be had in the flexors and are then, I think, reinforcing; but of this I am not sure.

There remain the unexplained cases of entire absence of reflex muscle-muscle response and reinforcements in some healthy people, in whom, I may add, there is perfect preservation of skin-muscle reflex—a further contribution to the list of puzzling differences between the two sets of reflex signals.

Another fact for a time seemed inexplicable. If we move by painless faradic currents the belly of the extensors of the hand no form of reinforcement increases this muscular response. The reason is plain. The stimulus is local and the electrically caused motion owes nothing to ganglionic excitation and, hence, is never reinforcing by additions to ganglionic energy—another and beautiful proof, if any be needed, of the fact that m. m. reaction is a true reflex and does not depend alone on the mere tonal health of muscle.

There may be other and more common applications of this law of ever constant overflow of released energy. We habitually regard the increase of heart action from exercise as due to the excess of blood flow caused by the pumping action of muscles. I think another element may be reinforcement by overflow of the cardiac ganglia.

¹¹ The muscle, owing to intrinsic excitability, contracts on being struck, even when its motor nerve has been cut. If the reflex arc is entire the blow evokes also a reflex reply. There are thus two contributory responses in health.

The mere act of closing one hand can scarce affect materially the blood flow, but try it with an ear over the heart and you will hear how notably it accentuates the heart sounds, as one of my assistants, Dr. John K. Mitchell, long ago pointed out.¹²

All these facts deeply concern us and are not merely ingenuities and puzzles. Many other phenomena of disease may be reasonably explained by this conception of the far-reaching influence of overflows of energy. Grave central nerve lesions appear in some cases to render over excitable neighboring or related centers not directly implicated. Then the ordinary constancy of normal restraining inhibitions fails, and effort to use the injured center results in the excess of effluent energy so disturbing these remote ganglia as to give rise to ungovernable movements of healthy parts, such as we see in the motion of the sound hand in response to vain effort to move the palsied hand. There are other like phenomena with which you are all familiar. With one more illustration I leave this subject.

When, in a case of violent facial neuralgia, to talk, to laugh, to chew evokes maddening pain, are we not in the presence of an example of reinforcement from overflow of related motor centers affecting a sensory ganglion excitably ready to call forth the phenomenon of pain? The phenomenon of overflow from sensation centres upon centres exquisitely sensitized by the torture of causalgia from nerve wounds I described long ago. Every positive sensation, sound, vibration, music, as of a band, added to the sum of torture. I wrote of this in language perhaps too imaginative that this terrible hyperesthesia was like a state of sensory tetanus.¹³

You may differ with me as to some of my conclusions—I may differ with myself soon or late—but I trust that in this too long claim upon your attention I have not left you without thoughts of interest. Much that would have lengthened my address I have relegated to notes.

I thank you for the kindness which has made me your president and for the patience and attention with which you have listened to much that may have been familiar, but which I have vainly tried to put in briefer form.

¹² This fact and others open the question of the reinforcement of all normal movements, voluntary and involuntary, by reinforcements due to other and remote volitional acts. It was one in which we failed to obtain competent answers. The gymnasiums should be able to settle at least a part of this complex problem.

¹³ Injuries to Nerves, page 181.