Society Proceedings

NEW YORK NEUROLOGICAL SOCIETY AND THE SECTION OF NEUROLOGY OF THE ACADEMY OF MEDICINE

JOINT MEETING OF NOVEMBER 1, 1921

DR. FOSTER KENNEDY, President of the Society, and DR. S. P. GOODHART, Chairman of the Section, presided jointly

FOURTH VENTRICLE TUMOR WITH SURGICAL INTERVENTION

DR. I. S. WECHSLER presented a patient of twenty-nine, seen May 25, 1921, complaining that in November, 1920, she had dizziness and spells of momentary darkness before her eyes, lasting a few seconds. Occasionally she did fall, though to no particular side. To overcome the vertigo she had to hold her head in a rigid position. The spells of dizziness used to disappear for a few weeks, then set in again. Frequently she would lose her vision suddenly for periods lasting from two to five seconds. No headache, no vomiting, no tinnitus. She had gained thirty pounds and her menses had become irregular, prolonged and profuse.

Examination showed a somewhat acromegalic face, gait, coordination, deep and superficial reflexes, sensory function, and cranial nerves normal except for inequality of pupils, LR, and bilateral choked disk of five diopters. The diagnosis of tumor of the fourth ventricle was made, tumor because of the choked disk, and fourth ventricle because of remissions, of the need of holding the head rigid to overcome the vertigo, which was interpreted as a modified Bruns symptom, and because of the absence of other localizing signs.

On admission to the Mt. Sinai Hospital the blood Wassermann and urine were negative and the blood chemistry was normal. There was eosinophilia amounting to seven per cent. The X-ray showed a normal sella and no signs of pressure. Dr. Elsberg performed a suboccipital craniotomy and found a tumor filling the fourth ventricle. It was adherent to the floor and sides and to the calamus scriptorius. An attempt to remove a piece of the tumor resulted in bleeding, which, owing to the dangerous location, necessitated a rapid retreat. The tumor appeared to be either a glioma or ependymoma.

For a short time after the operation she remained unchanged, but later developed some dysarthria, a little staggering, vomiting and slight optic atrophy. She was discharged from the hospital within a

few weeks. At no time did she have nystagmus, pyramidal tract signs, cranial nerve palsies or sensory disturbances. The patient is still living, five months after the operation. She has improved somewhat.

The interesting points considered were: (1) The diagnosis of fourth ventricle tumor during life and its verification at operation, based on a modified Bruns symptom and choked disk. (2) The absence of all signs of fourth ventricle tumor, such as pressure symptoms on the medulla, pons, cerebellum and cranial nerves. Particularly noteworthy were the absence of vomiting, headache, rigidity of the neck, polyuria or glycosuria, and vagal symptoms.

DISCUSSION

DR. M. NEUSTAEDTER asked what was the pulse ratio?

DR. S. P. GOODHART said these cases are very rare and both the patient and the doctor were to be congratulated.

DR. B. ONUF asked if a titubation were present.

DR. WECHSLER said that the pulse was always within average limits and so was the respiration; during the operation and following the operation, for a few days, of course, that does not apply. The nature of the tumor could not be determined, and an attempt to remove it, if it had been carried on, would have resulted in the bleeding to death of the patient. From the appearance of it, it was between a glioma and an ependymoma. It appeared to be a soft tumor.

A PRINCIPLE, HITHERTO UNDESCRIBED, OF THE PHYSIOLOGY OF MOVEMENT AND POSTURE. THE PRIMITIVE SPINAL INTEGRATION OF MOVEMENT IN VERTEBRATES

DR. WALTER M. KRAUS said that at the present time movements are described in such terms as extension, abduction, internal rotation, etc. In other words, activity controlled by a neuromuscular mechanism is being described in terms of the muscular part of that mechanism alone. Every description of movement leaves out the neurological element of that movement. The description of its control by the spinal cord and its peripheral nerves is left out. When a complicated movement guided by integrations of the nervous system at levels higher than the spinal cord is described, a consideration of the final neuromuscular mechanism is omitted. In other words, the foundation of the control of movement by the spinal cord is left out when such words as flexion, extension, abduction, etc., are used. It would seem wise to add some terms to those describing muscular movements which would give a notion of the embryology of these muscles as well as their innervation. This, as will be pointed out, can be done very simply.

The nervous system is unquestionably built up of a series of physiological levels, of which the higher or more recent ones, phylogenetically speaking, carry out functions which are different from

the lower ones. It is his opinion that some conception of the integrating action of the lowest levels must be obtained in order to interpret the higher levels. It must be known, in other words, how the spinal cord integrates movements, in order to understand how such higher integrations as the cerebellar, act, and ultimately to understand why it has become necessary for these higher integrations to appear at all.

In order to examine this question of the existence of integration of movement of the spinal cord, it becomes necessary to consider in detail the general plan of peripheral nerves, the general plan of the innervation of muscles by them, and the embryological grouping of muscles. It was his intention throughout to present only the simplest facts in connection with these structures.

A typical thoracic spinal nerve, having nothing to do with the limbs, shows a number of divisions after it has been made up of the posterior and anterior spinal roots. The common trunk so formed divides into an anterior and a posterior division. The anterior division divides further into a lateral and an anterior branch. Furthermore, the lateral branch divides into two, a dorsal and a ventral. As far as the axial musculature is concerned, the portions of it developed from the ventro-lateral portions of the embryo are supplied by the anterior division and its branches, while the postero-musculature is supplied by the posterior branches. Thus a very simple scheme results from a physiological viewpoint, in the notion that general movements of extension are guided by the posterior division, while flexion and lateral movements are guided by the anterior division. When this is applied to the limb plexuses, two theories appear. Before stating them, it is necessary to emphasize that the limb plexuses do not include any part of the posterior division. One theory assumes that the plexuses are made up of the entire anterior division, the other that they are made up entirely of the lateral branch of this division. For the purposes of this discussion it does not seem at the moment very important to try to reach a decision as to which of these theories is correct. That theory which states that the plexus is made up solely of the lateral branch is chosen and the details of the problem are discussed in these terms.

The lateral branch divides into a dorsal and ventral branch. The primitive extremities of the embryo are flattened buds having a dorsal and ventral surface. The premuscle mass on the dorsal side gives rise to muscles, the majority of which, in the adult, have an extensor function. The premuscle mass on the ventral side of the bud correspondingly gives rise to muscles, the majority of which have a ventral function. The crux of the whole situation lies in the fact that when considering the simplest function of a muscle it is found that certain of them have a function opposite to that which would be expected from a knowledge of the side of the limb bud (ventral or dorsal) from which they develop; for example, the iliopsoas group develops from the dorsal muscle mass of the lower limb bud. However, they have a ventral function—that is, flexion.

To return now to the question of innervation, it is found that the

embryology of the muscles is beautifully emphasized by the innervation of these muscles. It is well known that the nerves of the brachial and lumbo-sacral plexuses divide into two very definite groups; one of which is posterior or dorsal and the other anterior or ventral. In the arm the ventral branches of the brachial plexuses innervate the subclavius and pectoral muscles as well as all the muscles supplied by the musculo-cutaneous and ulnar nerve. What is found when detailed table of muscles is made, based upon (1) whether they have been derived from the dorsal or ventral aspects of the limb bud, (2) whether they are supplied by dorsal or ventral branches of the limb plexuses, is that those muscles which develop from the dorsal layers of the bud are supplied by the dorsal branches of the limb plexus. The analogy to the condition in the axial musculature is obvious.

From all this one arrives at some possibility of schematization. It is found that the nerves derived from the brachial plexus are divisible topographically into two very large groups, one ventral and the other dorsal, which, in turn, supply muscle groups which are ventral and dorsal in origin (Paterson). On going further and adding, on the chart, the function of these muscles, one is struck with the fact that, as in the case of the iliopsoas mentioned before, a muscle which would be expected to have an extensor action has in reality a flexor action. In other words, were one to group the muscles activated in any given movement merely on the basis of their activity as muscles, and leave out of consideration the origin of their embryology and activating nerves, one would be leaving out entirely the division of these nerves and of their end organs, the muscles, into a large ventral and dorsal group.

To illustrate how this information may be used for the purpose of discovering just how the spinal cord may integrate movement, let us take the process of stepping. If the extreme forward movements of the leg be represented, it will be seen at once that the forward movement is carried out by the iliopsoas, quadriceps and tibialis anticus muscles. Though this forward movement of the leg as a whole is spoken of as flexion, it is quite obvious that it is of dorsal origin, since all the muscles which carry it out are of dorsal origin. On the other hand, the carrying backward of the leg is brought about by the hamstrings, gastrocnemius and soleus group, and the tibialis posticus, all muscles having a ventral innervation and being of ventral embryological origin. Consequently the movement of extension of the leg as a whole at the hip is, in reality, ventral in origin. To go further and take the associated movements of the arms and legs, when the left arm is forward and flexed, the left leg is extended at the hip, flexed at the knee and extended at the ankle. It is generally stated that the arm and leg on the same side are acting oppositely, in that the arm is flexed while the leg is extended, and the arm is, for example, forward while the leg is backward. If this be interpreted in terms of the neuromuscular mechanism controlling it, rather than in terms indicating the geometrical relations, such as flexion, extension, etc., it is found that the arm and leg on the same side are both directed, in this particular example, by ventrally innervated muscles,

so that the notion that the arm and leg go in opposite directions on the same side must be reversed when the neuromuscular mechanism is considered as the basis of movement.

It is proposed that to the words now used to indicate the activity of a muscle, such as flexion, abduction, extension, etc., two words be added, namely, ventrad and dorsad, which would serve to indicate three things:

1. The embryological origin of the muscle.
2. The nerve supply of the muscle (ventral or dorsal branches).
3. The primitive direction in which the limb was carried by that muscle.

By adding these terms we describe movement in terms of the neuromuscular mechanism, instead of leaving out the activity of the nervous system entirely.

Furthermore, the primitive integration of movements of the spinal cord is seen to be one which throws into activity the muscles which develop together and are innervated together. This very simple movement of a group of such muscles must be brought about by a correlation mechanism within the spinal cord, and forms the foundation of movement. So simple a movement as could be brought about by the simple activation of large groups of what were primitively ventral or dorsal muscles was obviously insufficient for the needs of animals. Further and different integrations of movement became necessary, and so not only the spinal cord, but the more recently developed portions of the nervous system, came into existence, in order to make possible these new and more complicated patterns of associated movement. An example of one of these within the spinal cord itself is the flexion reflex, "flexion of the three great joints." This term gives the impression of a certain homogeneousness of action which, when interpreted in terms of the neuromuscular mechanism, is found to be entirely absent, in that flexion of the hip is a dorsad movement carried out by the iliopsoas group, flexion of the knee is a ventrad movement carried out, in this instance, by the hamstrings, and flexion of the ankle is a dorsad movement carried out by the tibialis anticus.

Note: The paper as originally presented was but a bare outline of the matter which should have been presented. An abstract of such a paper must of necessity leave a great many points of the greatest importance untouched and must leave undiscussed many of the points presented.

DISCUSSION

DR. S. E. JELLIFFE said that he wished he knew enough to discuss intelligently the paper that Dr. Kraus had given. His modesty, he said, forbade him to enter into a criticism, because he felt certain that Dr. Kraus has emphasized a method, a new method of analysis of motion. Whereas for himself the embryological evidence always lags behind the phylogenetic argument, he should like very much to have Dr. Kraus, in his further expositions of the situation, link up

the phylogeny of these movements and show wherein they conform to the present embryological schemes. Then he thought that a certain number of the difficulties that Dr. Kraus himself had suggested would perhaps be clarified.

He said the paper had provided a stimulus whereby further observation of muscular activities will take on an entirely new aspect.

DR. KRAUS said, in reply to Dr. Jelliffe's remarks, he thought that the phylogeny of the matter is the only ultimate way to decide it.

The reason for bringing these little animals down was to emphasize that point. He did not think, however, that it was necessary to give the "why" of these things to emphasize the facts. The facts remain. What he tried to emphasize throughout was that what he was dealing with were facts, not theories. We must, I believe, speak in terms of the neuromuscular mechanism and not of muscular mechanism only.

Dr. Stookey's emphasis of the many functions of a single muscle is extremely pertinent. We must take a primary function first and then try to explain, if we can (and in many instances we can), why a muscle assumes a secondary function. Take, for example, the activity of the flexors of the wrist. There the prime action is flexion; but they have also assumed an action of adduction or abduction. We must, as you see, examine these movements carefully before reaching conclusions.

Now, as to the use of the abductor of the little finger, or let us take the abductor of the thumb—that certainly is abduction; it is from the middle line, the axis of the extremity. But as I am speaking of terminology, I should like to emphasize that when we speak of it as abduction, we think of it in the same way as we think of abduction of the arm, though the innervation and embryology indicate that one is dorsad, while the other is ventrad. If we use the terms ventrad and dorsad, we know how different is the innervation in these two cases. I do insist (and I will make this the only point I would like to insist entirely upon) that we must use the neuromuscular mechanism as a basis for the interpretation of movement and not the muscular only. If we are going to interpret the activity of higher levels, we must interpret that through the nervous system. We must not pass from the action of the cortex to flexion, for example, without knowing in the least how the spinal cord has come into that integrating action.

SOME THERAPEUTIC EXPERIENCES OF MYASTHENIA GRAVIS

DR. CHARLES L. DANA stated that the prognosis of this disease as generally given was very bad; according to Oppenheim, 26 out of 38 cases died; Dr. Hun found that most cases die in from one to three years; Dr. M. Allen Starr found that about 45 per cent. of the cases collected by him died within six months. Dr. Dana stated that his experience had been quite different, and he felt he ought to record it. He had had under observation 14 cases in the last 20 years; he had not been able to follow up the histories of all of them, and some

had died from natural causes; but, in general, he could say that none of them had died of the disease while under his observation, and he had had them under observation for from one to seventeen years.

TABLE SHOWING DURATION OF DISEASE WHILE UNDER OBSERVATION

1 year, 5; 2 years, 1; 4 years, 1; 5 years, 1; 6 years, 1; 7 years, 1; 12 years, 1; 13 years, 1; 17 years, 1.

TABLE OF RESULTS

Recovered	4
Practically well	3
Improved	4
Not improved	1
Died of cancer	1
Not followed up	1
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	14

Dr. Dana thought that the better prognosis of the cases was due in part at least to the treatment which he had employed. This consisted of massive doses of strychnine combined with rest and elimination. The doses reached in some cases one third grain given hypodermically two or three times a day.

The speaker also referred to the frequency of prodromal attacks; in 50 per cent. of his cases the serious attacks were preceded by short attacks lasting only a few weeks and characterized by some ocular symptoms and bulbar weakness. In some cases these prodromal attacks were not followed by any other attack for many years; in one case as long as seventeen years elapsed. Dr. Dana called attention to this fact and thought it quite probable that mild abortive attacks of myasthenia gravis characterized by diplopia and slight exhaustion of the bulbar centers were not infrequent. In some cases the prodromal seizure was simply an ocular one, and one of the causes of casual ocular palsies of permanent type was undoubtedly myasthenia gravis. This fact was not recognized in the systematic works on ophthalmology. Dr. Dana referred to myotonia as an occasional complicating symptom of myasthenia. He thought the disease toxic and endogenous and one that affected both motor neuron and muscle. He had seen no result of note in endocrine therapy.

DISCUSSION

DR. BERNARD SACHS said that Dr. Dana's contribution to this subject is of great interest, and he was willing to endorse his rational therapy. There is nothing surprising in the fact that he has employed strychnine. In the cases that he had had at the hospital during the past ten years the uniform treatment, particularly for the bulbar symptoms, has been the use of strychnine, but of course they have not employed it in any such doses as Dr. Dana now recommends, and he should be very glad, when the opportunity presents itself, to put that high dosage to a further trial. His experience has been an extremely fortunate one. He confessed that of all the cases in which he had made the diagnosis of myasthenia gravis pseudo-paralytica only very few recovered or remained alive long enough for him to record a temporary recovery.

He wondered, though, if Dr. Dana hasn't experienced very much the same thing that he had experienced in the course of these years: First of all, that the disease that some of us call myasthenia gravis pseudoparalytica includes a number of rather discordant types of disease, and he thought, particularly in view of the experience that we have had during the past few years with various forms of encephalitis, not only the encephalitis lethargica, he wondered whether, in view of that experience, we should not all be inclined to think that some of the cases that we labeled as myasthenia gravis were really some form of encephalitis. There are no doubt other forms of infectious encephalitis besides encephalitis lethargica, and personally he was inclined to think that some of these cases that have been covered may have been cases of that type and not cases of myasthenia gravis.

He was referring to this fact more particularly because of an experience which they have been undergoing for the last two or three years at the hospital. His associates and himself have been very deeply interested in all forms of bulbar palsies and we have seen a rather curious number of extremely complicated and difficult bulbar cases. Some of them they were inclined to regard as myasthenia gravis, and in other cases again they really were not able to label the cases in that way. One or two of the cases recovered; others died, and the entire subject still remains more or less of a mystery to him, except that he has no doubt that there are cases that are due to severe toxic products circulating in the body. It would be only a short jump from that to the supposition that some of the cases were due to infections of all sorts in the body, and he believes that a further study would probably prove to us that many of these cases are really distinct forms of encephalitis. The actual proof of that, as you can easily recognize, would be a rather difficult one to secure.

He would, however, like to know whether Dr. Dana himself would be rather inclined, in view of our recent experiences, to accept that view in regard to the interpretation of some of the cases that we labeled as myasthenia gravis. If he were simply to state a general impression as to the number of cases he would be willing to call myasthenia gravis, he would say that he had not seen more than six or ten of them in so many years. So that the subject is one, he thought, that is well worth considering in every way.

He would like to have Dr. Dana state whether the symptoms actually cleared up rather promptly on the administration of the large doses of strychnine; for instance, whether the ptosis disappeared and whether the tongue symptoms cleared up, whether there was a prompt recovery or not, on the exhibition of the strychnia. It seemed to him to be a rational method of procedure and he should be only too glad to adopt Dr. Dana's suggestion and benefit by his experience.

DR. L. P. CLARK said that a number of years ago at the National Hospital for Paralytics and Epileptics, Sir William Gowers was very keen on the use of strychnine in his myasthenia gravis cases. He was particularly impressed with the enormous advantage that he gained from such administration. Later he had the opportunity of noting cases in the service of Dr. Starr at Vanderbilt Clinic. He

saw advantages in the foreign cases, but did not recall advantages obtained on this side with either small or large doses. He continued to use strychnine for a period of two or three years, beginning with small doses and running up to a fourth of a grain.

He said he would like to ask Dr. Dana, who has had a very considerable personal experience, how he explains the rational therapeutics of the strychnine, if this is some form of glandular disturbance (which now seems more than likely); how does the strychnine operate against an organic conductivity of the systemic values?

He had been very much surprised to note the enormous suggestibility that these cases have and the neurotic attitude they take toward the disease. Several months ago he was asked to see a case, not because of the myasthenia gravis, but because there was possibly a neurosis superadded. The diagnosis seemed quite correct and everything had been done for the patient that was possible. He suggested that he get a change of environment by taking a rest in the country. He was very much surprised the other day when this man's wife came to see him and reported that he was walking two miles, was engaging in all sorts of activities, and everything but his speech (which was predominantly bulbar in type) was enormously improved. Simple advices had evidently impressed the patient and had much more to do with his betterment than any special therapy that had been undertaken.

Why would it not be a good idea to try the glands on other cases, if, as the doctors say, the disease is the fault of some gland? He was particularly impressed after he saw this patient that he was going through a rather definite, almost a psychotic delirium of his disease, and he was surprised to see the type of mental reactions he was taking through this organic detail. There are certainly enough cases on record for us to work toward eliminating that particular issue and also take up some of those that Dr. Sachs has suggested, of other types of infections.

He should like to end by asking Dr. Dana upon what rationale is the strychnine acting. Is it against the functioning of a certain unknown gland?

DR. JELLIFFE said that while listening to Dr. Dana's paper his thoughts had run along the lines that Dr. Sachs had so well emphasized. That whereas one can predicate type of reaction focused about what is termed myasthenia gravis, shading off from such a picture a great number of semi, demi types are observable, some of which undoubtedly belong in with the groups which Dr. Sachs has emphasized; also others that heretofore have been called periodic palsies, and a whole group of phenomena related to irregular muscular reactions due to acute or chronic conditions chiefly in the medullary nuclei. It seemed to him that that is a useful way of looking at the whole situation. Of course, if, with a certain trend of mind, one is apt to lump all these things together and speak of a disease *sui generis*, then he thought Dr. Dana's attitude of mind quite justified. However, if one is inclined to see so-called pure types and divergent types and a whole group of situations which clinically may

be grouped about a very definite syndrome, it seemed to him the results which had been reported were quite comprehensible.

Dr. Dana has spoken of the endocrine situation. It seemed to him that Dr. Dana sketched too broad and glaring a type of generalization. He felt disposed to think that if the particular group of glandular involvements had a certain specific action; either on one or the other branches of the vegetative nervous system, then one could commence to predicate an endocrine disorder. That is to say, one might say, "Here is a disturbance in a muscular mechanism which is doubly innervated." The sympathetic arc of the vegetative mechanism, it seemed to him, is the one that is most primarily involved in the pure types of myasthenia gravis. If that be so, there is a very good rationale why strychnine should be of value in that type, because it is, of all of the plant alkaloids, the most powerful stimulant to the sympathetic branch of the vegetative nervous system. That is also a reason why calcium is therapeutically of value, and why sometimes the myasthenia gravis cases appear very closely related to parathyroid disturbance.

DR. B. ROSENBLUTH said the subject of myasthenia gravis interested him greatly. In studying the literature, he had come across a similar disease occurring in Switzerland and also in Japan, which offered all the symptomatology of myasthenia gravis. This disease occurs in the most crowded quarters and among the poorest class of people who are undernourished. For example, in Japan it is found among the laborers in the rice fields, and it is found among people usually who are terribly underfed and undernourished. He had had only four cases, and instead of using any medication, he had put the patient to bed and kept on feeding him by means of the duodenal tube. Dr. Larkin cultured one case and was unable to discover anything definite; and he had a similar experience and negative results in another case. The two cases of myasthenia gravis which recovered might have been an erratic type of this form of disease. It is very possible that with more refined bacteriological technique one might recover and be able to correlate this disease with its occurrence in the different parts of the earth. That being the case, we can see the reason that any form of treatment that is kept up long enough, with stimulation and nourishment, will bring the patient over the bad part of it for two or three years. These cases take a very long time.

DR. DANA asked if this disease kept on recurring every year.

DR. ROSENBLUTH said it recurs every two, three or four years.

DR. CHARLES E. ATWOOD said he wished to call attention merely to one fact in relation to this disease. Of course, we have all seen a number of cases and we realize that they are subject to remissions, and that the disease renews itself frequently by fatigue. Possibly the explanation of the renewal of the symptoms is from the sub-oxidation which takes place in fatigue. He has a case of myasthenia now that has lasted since 1907. The case has done the best with hygienic symptomatic treatment and small doses of strychnine. The myasthenic reaction is one of the most reliable tests, perhaps, in differentiating myasthenia gravis from other diseases.

DR. FOSTER KENNEDY said his experience in this matter has been

that there is rather a difference of prognosis in the subjects who are young; *i.e.*, in the subjects who are under forty and in the subjects who are over forty, it being decidedly worse in those who are in their twenties. The disease, as he has seen it, has run a very much shorter course in the young than in the middle aged or aged.

He mentioned to Dr. Dana that he had been fortunate enough to see a series of autopsies on myasthenia gravis, with the extraordinary chance that in five of them an enormously enlarged thymus existed. This thymus was not so very great as regards glandular bulk, but it was great as regards glandular expansion. The thymus existed as an object perhaps two thirds the size of my hand, stretching down over the pericardium—a very extraordinary phenomenon, one which certainly can not be without significance in the disease, though its exact correlation with the symptoms I am not able to state.

DR. CHARLES L. DANA said he was much obliged to the gentlemen who have joined with so much interest in a discussion of this subject. It certainly is a very interesting one and one which seems to him to deserve further and closer scrutiny. With regard to the point that Dr. Sachs made, Dr. Dana had written this—he didn't read it: "Myasthenia Gravis is a disease which runs peculiarly true to type," etc. Therefore, he thinks that Dr. Sachs's suggestion that we ought to scrutinize these cases with especial care is a very wise one. He thinks that one case he had in mind was perhaps not true myasthenia gravis, though he thought it was at the time. You must not think that you have to give massive doses of strychnine always or all the time. He finds sometimes that a fifteenth of a grain is enough, and sometimes even a little less; but it seems to him that the most permanent and satisfactory results were those in which these larger doses were given for a considerable time.

With regard to the frequency of the time or the effect of the disease, the symptoms do not clear up right away. You will have to use the injections for perhaps three or four weeks before they begin to show, and it seems to Dr. Dana, from his experience, that there is nothing psychical in the patients. The patients he had seen were treated seriously, of course; when he saw them, they were put to bed a large part of the time; they were kept very still; they were given these very large doses and they knew they were being actually treated. There wasn't any suggestion about it, nor were there any psychic elements in the case.

He doesn't quite understand Dr. Jelliffe's remarks. It seems to him the disease is a poison of the somatic rather than of the sympathetic part of the system.

DR. JELLIFFE replied that the somatic muscles have a double innervation, sensori-motor and vegetative, and that involvement of the sympathetic was a better hypothesis, he believed.

DR. DANA said that he thought it was sensori-motor innervation that is affected.

In regard to the point made by Dr. Kennedy, I can only say that one of my cases, one of the very best ones, a boy of nineteen, has now been well for six years; but all the other cases were in the thirties or forties—perhaps more of them beyond forty than under it.