

## Part IV.—Notes and News.

### MEDICO-PSYCHOLOGICAL ASSOCIATION OF GREAT BRITAIN AND IRELAND.

THE usual Quarterly Meeting of the Association was held within the Royal College of Physicians, Queen Street, Edinburgh, on Thursday, February 19, 1925, at 2.30 p.m., Dr. M. J. Nolan, President, in the Chair. The Council and Committees met earlier on the same day.

#### MINUTES.

Minutes of the previous meeting, having already appeared in the JOURNAL, were taken as read and approved and signed by the Chairman.

#### OBITUARY.

THE PRESIDENT then expressed the deep sense of loss the Association had sustained in the demise of Dr. John Fraser, Dr. W. A. Parker, Dr. R. G. Rows, and Dr. C. Molesworth Tuke. He felt that some of the members present would desire to give full expression of their appreciation of the merits of these old colleagues.

#### *The late Dr. John Fraser.*

Prof. G. M. ROBERTSON, referring to the death of Dr. John Fraser, said that the Association had lost in him an honorary member, but the Scottish Division had lost an old friend whose memory would ever remain dear. Dr. Fraser as a student had gained no less than nine class medals, and graduated with first-class honours. He (the speaker) was told by the late Sir John Batty Tuke that he never met any person who could get up a subject in so short a period of time as Dr. Fraser. After graduation he received that blue riband of a medical student's career, an appointment as Resident in the Royal Infirmary. After filling that post he became *locum tenens* to the Perth District Asylum at Murthly under Prof. W. C. McIntosh. It was the interest he found in the duties of that position which had led him to select the treatment of mental diseases as his special career in life. He was first appointed an Assistant Medical Officer at the Fife and Kinross District Asylum at Springfield, and subsequently succeeded Sir John Batty Tuke as Medical Superintendent. He was appointed Deputy Commissioner in 1877, and acted as such for 18 years. He threw himself heart and soul into the duties of this post, and the experience which he gained in it made a lasting impression upon him. He came to know his Scotland thoroughly in all her moods, for he had hair-breadth escapes by flood and field in his many tours, and in his voyagings to the islands. He told him (the speaker) that he could not be placed in any part of Scotland without recognizing where he was. Not only did he know the country but he knew the people. He made friends wherever he went among the medical officers and inspectors of poor. In 1895 he was appointed a Commissioner, retiring in 1910, when he was presented with his portrait, painted by Mr. Fiddes Watt, by his numerous friends. His work as Commissioner was too well known to need any description. He was interested in all the movements in which they were interested. He sympathized with them in all their difficulties and responsibilities. There never was such a man who filled a post who had less of the cold official about him or more of the warm-hearted human being. He gave them suggestions, he gave them advice, and he sometimes reproved them in a kindly, fatherly way. They did whatever he asked them to do, because he was transparently honest and single-minded, and their friend. They would never forget his memory.

#### *The late Dr. W. A. Parker.*

DR. MARR, in paying a tribute to the memory of Dr. William Arnot Parker, said that although Dr. Parker did not take a very prominent part in the deliberations of the Association, they all knew he took a very sincere and general interest in the work of the Scottish Division.

[For the rest of Dr. Marr's remarks see obituary notices.]

*The late Dr. R. G. Rows.*

Dr. BOND said that there was yet the name of another of their members to be mentioned, whose death they would all deplore. He referred to the late Dr. R. G. Rows. They all knew his work in the Association. They had read his writings, which had been many, and they had also had the advantage of listening to him on frequent occasions at their meetings. By listening to him they knew the force and capacity of his teaching powers, which had been also exemplified to a very great degree while Dr. Rows was Officer-in-Charge of the "Moss-Side" War Hospital at Maghull, Liverpool. It was there that he had organized with his colleagues a set of instructional courses which would be remembered by many members of the Association who had attended those lectures in small batches, and the influence of that teaching they all knew had been very great in the successful treatment, not only of mental conditions arising in soldiers during the war, but in the development of similar treatment for civilians. He (Dr. Bond) was sure they were all glad to remember that the good work he did was to some extent recognized, in that he had conferred upon him the *C.B.E.*, and that the University of Manchester had conferred on him the honorary degree of Doctor in Science. They deplored his death, and their sympathies extended to his father, and to Dr. Orr, who had planned with him important developments, not only at Prestwich, but in conjunction with the neighbouring university.

*The late Dr. C. Molesworth Tuke.*

Dr. G. W. SMITH, in paying a tribute to the memory of the late Dr. Tuke, said that he had been associated with him for four years at Chiswick House, and they had lost one remarkable for his kindness and his charm of manner. Dr. Tuke was one of their oldest members, and had always taken a particular pride and very deep interest in the success of the Association. Another striking thing about him had been his kindly personality and his genial good humour. He was a good open-air sportsman, a shot, a fisherman, and more than all, a cricketer. It was some comfort to know that he had hardly realized that he was so acutely ill as he was during the last six months or so of his life. A wide circle of friends regret his death.

Members present signified their deep regret by rising in their places.

## ELECTION OF MEMBERS.

The following were unanimously elected as Ordinary Members of the Association:

LANDERS, JOHN JOSEPH, M.B., B.Ch.Camb., D.P.H., Medical Officer, H.M. Boys' Prison, Wandsworth.

*Proposed by* Drs. R. Fitzroy Jarrett, R. Worth, and G. Warwick Smith.

MCGLASHAN, WILLIAM REID, M.A., M.B., Ch.B.Aberd., D.P.M., Deputy Medical Superintendent, County Mental Hospital, Mickleover, Derby.

*Proposed by* Drs. G. N. Bartlett, F. M. Rodgers, and John Bain.

MACNIVEN, ANGUS, M.B., Ch.B.Glasg., Resident Assistant Physician, Royal Mental Hospital, Gartnavel, Glasgow.

*Proposed by* Drs. D. K. Henderson, W. M. Buchanan, and Aidan G. W. Thomson.

## DEMONSTRATION OF THE PSYCHO-GALVANIC REACTION.

Dr. DAVID SLIGHT, Lecturer, Psychological Department, University of Edinburgh, during the course of his demonstration said that the psycho-galvanic reaction was an example of how a scientific discovery might be made and then left buried in books. Apparently that phenomenon had been discovered about 1890 by two observers, Féré and Tarchanoff, working from two different aspects. Féré had found that when a current was passed through the body and an emotion stimulated, there was an apparent diminution in resistance.

Dr. Slight then proceeded to demonstrate Féré's method, and explained on a sketch the working of the apparatus.

He continued by saying it was agreed by all that the reflex was due to a change in the skin. He (the speaker) had proved that to his own satisfaction recently. He had passed a needle into his forearm and another into his leg and had passed the current through the needles. Under those conditions no reflex had been elicited, no matter what strength of stimuli had been applied. He had proved it

in another way by making cuts in the skin of the arm and leg and insulating them with paraffin. The current then passed through the body by way of the cut surfaces. Again he was never able to elicit any response whatever, no matter how strong the stimuli were. So that they could take it that the response was due to some change in the skin. What that change was no one seemed to know very definitely. It had been suggested that it might be a change in sweat secretion. That was a theory which had been proved and disproved by various observers. He thought that the general conclusion was that if the secretion of sweat did play some part, that was not the sole factor. He mentioned that the late Prof. Waller secured some evidence that sweat secretion did not take the whole part in the production of reflex by injecting atropine, and also by applying belladonna plasters. Another very striking example was the case Dr. Golla, of the Maudsley Hospital, had recorded of a patient with hyperidrosis. In that case there was an obvious layer of sweat, and Dr. Golla found the reflex present. In such a case it seemed very doubtful that any small addition of sweat would make any difference. The other possibility was that there might be a change in the blood-flow. This again seemed to have been as the sole factor generally disproved. Various observers had found the reflex still present even when the limb was exsanguinated. There was no question that emotional stimuli did cause a fall in resistance. Dr. Golla, in a recent communication to him (the speaker), seemed to believe that the change was one transmitted through the sympathetic nerve system. The late Prof. Waller had suggested much the same thing, *i.e.*, that the fall in resistance had something to do with the trophic nerves. There was some change transmitted, probably through the sympathetic system to the skin.

Dr. Slight then proceeded to demonstrate with the galvanometer, during the course of which he pointed out that one point which differentiated the real emotional response from any change due to movement was the fact that there was a latent period. If he deliberately moved his hands, the movement of the light followed immediately on the movement of his hands, but in the psycho-galvanic reflex a period of two or three seconds elapsed before the light moved.

Continuing, Dr. Slight said that he was sorry that demonstrations were rarely convincing, but he hoped he had demonstrated to those present that there was a definite response. It was a physiological change. They could not explain it away by saying it was due to movements of the electrodes, for the simple reason that any change which was due to movement of the hands was followed immediately by movement of the light. There was no latent period in that case.

In conclusion he would summarize his remarks by commenting briefly on the way in which the reflex might be useful in mental work. When investigating problems of emotion, they knew the difficulties of registering and recording any changes which occurred. For example, changes in the respiratory system, circulatory system, and so on, were all accompanied by great difficulties in registration, and various observers gave most conflicting results. The psycho-galvanic reaction was one change accompanying the emotions that was definite. There was no evidence up to the present that there was the least possibility of controlling the response by an effort of will, and in it he thought they had a very valuable means of investigating problems of emotion. They could use it, for example, in investigating the fundamental problems of emotion. In psychiatric work they could use it when conducting a mental analysis.

Another point was the problem as to whether a disease was of a psychic or organic nature. In organic disease, such as dementia præcox, they would find the reflex diminished. He pointed out that Dr. Golla, working by the same means, had found that the hysteric patient gave very diminished responses. Hysteria had been regarded by many as essentially of psychological origin, and yet that reflex was diminished, which would rather tend to show that hysteria had an organic basis.

Another point was the question of subconscious emotions. In analysing the patient, the sceptic asked, "How do you know there is an emotion or not?" They might say the patient did not know about it because he was unconscious. Now they had a definite means of proof. If there was such a thing as an unconscious emotion, then they had now a definite means of recording it. He was sorry that the demonstration had not been as successful as he should have liked it to have been.

Dr. BOND said that this demonstration had been very interesting to him, and

he was glad to see that work going on. He had seen when visiting the psychological laboratory the work that was being done in Cambridge, and it did seem to him that they had, when properly developed, a most powerful instrument for differential diagnosis. Dr. Slight did not tell them whether he was applying this to actual clinical cases. That is what he (Dr. Bond) would like to see systematically tried. He knew that Dr. Golla was working on the matter, and that he had scented a large number of fallacies, but he (the speaker) thought he had interpreted him rightly when he said that Dr. Golla was loath to say much about it until he had investigated the matter more closely and had found an opportunity of checking the results. Dr. Petrie (who was present), perhaps, would tell them to what extent it could be used clinically for diagnosis. He did not know whether it was put forward seriously or as a joke, and Dr. Slight had not mentioned it, but there had been a suggestion that in this apparatus they had, perhaps, a powerful means of criminal investigation.

Dr. A. A. W. PETRIE said he was afraid that what he knew about it was very much from superficial observation, but he did know that Dr. Golla thought it was now desirable to investigate a considerable number of cases which had a very definite diagnosis, and he was distributing a portable form of this apparatus to certain London County Mental Hospitals. When a considerable number of cases had been investigated Dr. Golla would be prepared to speak more definitely. The objection to using the early type of case such as they had at the Maudsley Hospital was the difficulty in giving a definite diagnosis at that stage of the disease. The lines on which Dr. Golla had always found the reaction so useful had been in war cases—the differentiation of hysteria from a genuine hyper-emotional response. He did not quite follow Dr. Slight's deduction that because there was a diminished response in dementia præcox, which was possibly an organic disease, and because there was also a diminished response in hysteria, that hysteria was necessarily an organic disease. The difference between anxiety neurosis and hysteria was that in the former there was a really undue sensitiveness, whereas in the latter there was merely an expression very obviously external of an emotion which they did not feel. He would like to hear other people's views on that point. The method had been used in a number of cases which were being analysed, and it sometimes did enable the observer to detect an emotional response which the patient either hid, or from some other cause was not apparent.

Dr. IAN D. SUTTIE remarked that he could not see what use the reaction would be in criminal investigation, because a criminal knew himself to be accused of something, and even if he was innocent, he would have an emotional reaction whenever a word was given to him or a stimulus applied which recalled to his consciousness the crime of which he was accused. This reaction only detected the presence of an emotion. It did not tell them whether there was an emotion of guilt, of anger, or of fear; it only detected the presence of an emotion, and an innocent person would give an emotional reaction in very much the same way as a guilty person. The real difficulty was to interpret the emotional reaction; it was far easier to detect an emotion than to interpret its cause.

The PRESIDENT said he would rejoice to see any attempt made to bring the matter into clinical operation. That day's demonstration appeared to him to have the drawbacks the last speaker had particularly pointed out, but he still thought it was on the right lines, and when it was developed in the proper way it might be of use to them all. Even that afternoon, watching closely the demonstration, they could see a great many points that might prevent a correct response. The young lad was sitting in anticipation of the stimulus, and his mind was concentrated on it, which could not be considered normal conditions. He thought that if the same thing could be carried out without preparation on normal and abnormal people the results would be interesting. For instance, if the stimulus could be applied without the men knowing it in a hundred normal cases and then in a hundred cases of insanity they might obtain some concrete evidence. It seemed to him to be rather dangerous to base any reasoned opinion on the results at present, and least of all in criminal investigation. The apparatus they had seen that day, of course, was pretty much in the form of the family parrot who will not talk when they wanted it. But he still felt that there was a future before that investigation, and to those who had to work among the insane it might be of material service some day. They were all very much indebted to Dr. Slight for giving them the demonstration.

Dr. SLIGHT, in reply, said that he did not think they could expect very much success with the reaction in the case of criminals, because if they put a series of people in a row and investigated them one by one, say by applying a list of words relating to the incident, he was inclined to believe that they would obtain responses from others than the criminal from the fear that they were being suspected.

At the President's suggestion he would mention that it was rather interesting to know that when a person was reciting dramatic poetry and exhibiting all the signs of emotion there was no change recorded by the galvanometer, so that apparently when actors portrayed emotion they did not feel any real emotion. A hysterical person is in much the same position. He might give the outward signs of emotion, for which there was no real physical basis. Dr. Petrie had expressed doubt that hysteria was an organic disease. It was not organic in so far as there was any definite pathological change, but it was organic in so far as such people were not capable of giving adequate organic response, even when they were not acting spontaneously. When hysterics were under examination and exhibited the phenomena of weeping and crying, it was a false emotion. But apart from that, when stimuli were applied which should cause emotion in any normal person, the hysterics did not give an adequate emotional response even then, so that apparently their affective mechanism was deficient.

It had been suggested that they should try and separate different types of people, but it was very difficult to obtain their subjects under comparable conditions. It was also difficult to apply standard stimuli. He had found that one's resistance varied, and corresponding with the change in resistance they found that there was also a change in response to the same stimulus.

Still, he hoped they might in the future obtain some useful and valuable information from using that method. (Applause.)

#### PAPERS.

"The Boarding-Out System," by Dr. GEORGE GIBSON, *D.S.O.*, Deputy Commissioner, General Board of Control for Scotland (see p. 253).

The PRESIDENT said he would like to convey their appreciation to Dr. Gibson of the excellent paper he had just given them on the "boarding-out" system. He would very much like to hear the views of other members who were interested in the subject, but he was afraid the question was too large to enter into that night. There had been a good deal of agitation about this subject many years ago. His late colleague (Dr. Conolly Norman) was most active in his efforts to introduce the system into Ireland, but there were circumstances which he (the President) considered were unfavourable to its adoption. Events showed, he thought, that he had been right on that point. He thought that Dr. Gibson's paper was worthy of full discussion at a later meeting.

"Results of Treatment of General Paralysis by Malaria," by Dr. W. M. McALISTER, Assistant Physician, Royal Hospital, Morningside, Edinburgh (see p. 236).

Dr. HENDERSON said he felt he ought to express his appreciation of Dr. McAlister's paper. He (the speaker) thought he had expressed the situation in a very conservative way, and that he had really brought the question of malarial treatment of paralysis down to a much better basis than they had generally been allowed to believe. He was glad to know Dr. McAlister considered that in this country the results they had accomplished did not at all compare with what they had been led to expect by continental observers or observers in America. It had been difficult to decide as to whether one should start this method of treatment or not, but after hearing Dr. McAlister's paper he was more inclined to leave it alone than to go on with it. He did not think there had been anything done in this country so far that gave them any security in treating cases of general paralysis with malaria. He would emphasize a paper recently published by Drs. MacBryde and Templeton on a series of fifteen cases. These observers had three deaths from acute malarial intoxication, and they emphasized the fact that the application of malarial treatment resulted in the patients so treated suffering from vomiting and some of them from incontinence of urine, and so on; so that the results altogether, he thought, must continue to be accepted on the very conservative basis that Dr. McAlister had put forward. He would like to express his high appreciation of the paper. (Applause.)

Dr. BOND remarked that he endorsed what had just been said, and expressed his appreciation also of Dr. McAlister's paper—such of it as he had been in time to hear. He did not deduce from what he heard that Dr. McAlister at all adopted a pessimistic attitude towards the treatment of general paralysis by the method under discussion. He might say that the attitude of his colleagues on the Board of Control in England towards it had been one of friendly and interested watchfulness, with a great desire to be kept closely in touch with what was going on in the different hospitals in this matter, and for themselves not to express any great pæans or remarks in its favour, nor unduly influence those who were not carrying out this treatment to do so. Just lately they (the Commissioners) had been so impressed with the results in certain places, and the time that the patients remained at home without relapses, that they were beginning now to wonder whether the time had not come to say to other places, "What are the reasons why you are not in favour of this method?" Fortunately for them they had had the advantage of being able to obtain the help of Col. James, of the Ministry of Health, whose knowledge of malaria was very great, and who had been able to foresee and therefore prevent mishaps and trouble by most prudent advice. He (Col. James) was most emphatic upon the desirability, for several reasons, of the inoculation being derived from the mosquito. So much interested was he that he desired to establish a couple of small rooms carefully fitted up, in two mental hospitals, one a London one (Horton), and the other one, he thought most possibly, in Lancashire, where a supply of mosquitoes would always be in readiness. Col. James wished to use those two laboratories as the centres of supply for the Kingdom. He hoped members would not cease that method of treatment without exploring it to the utmost, and even if no complete cures were obtained, still, if in a big percentage, like 25 or more, they could produce a quasi-convalescence, they surely would have made a good step. (Applause.)

Dr. DANIEL said he had had a little experience of this method in Hanwell, but the treatment had to cease because the hospital was not adapted for the purpose. He had found the treatment rather more hopeful than what Dr. McAlister had led them to believe. The treatment was not without danger, but the chances of apparent recovery seemed to be good. The exact figures had not been published yet, but he had no doubt they would be before long. He thought the treatment not only with malaria but also with anti-syphilitic remedies was to be recommended.

Dr. BOND added to what he had said previously that he did not know whether it had been tried in Scotland, but in England several mental hospitals had tried malarial infection for mental disorders other than general paralysis, in particular certain phases of dementia præcox, with an idea that the improvement which follows sometimes with a febrile attack may be produced by that method.

Dr. C. A. CRICLOW said that at Bangour they were indebted to Dr. McAlister for sending them some cases upon which to start experiments. They had inoculated six cases, and of those six one man had been discharged apparently cured, and the last report they had heard of him was that he was working as a dock labourer at Leith. Some thirty other cases were inoculated from a fresh strain. Four of these were women. The cases which improved most were the male cases. In many of the female cases the results were very disappointing. On the whole he thought that they had to take a very conservative view of the treatment.

Dr. DOUGLAS McRAE said he much admired the attitude Dr. McAlister had taken up in this matter. He thought they ought to go very, very cautiously, and a great deal more work ought to be done before they came to conclusions. Many cases ran a very prolonged course with little deterioration in the mental state. It was quite possible that only one-tenth of the cases of general paralysis were admitted into institutions at all. He mentioned that because it was in that very room, when he read a paper on general paralysis, that Prof. Wyllie came up to him and said, "It is a very interesting paper; all your remarks are very interesting, but has it ever occurred to you that I as a physician outside see far more general paralysis than Dr. Clouston ever sees?" Another thing he would like to mention was that the type of cases admitted was changing. He did not think the cases progressed so rapidly as they used to do. They came into mental hospitals, some of them apparently very advanced, and without any treatment or experimentation of any kind cleared up remarkably, and remained for several

years, and some for many years, more or less semi-sane members of the asylum population.

Dr. MACPHAIL said that there were just two points about Dr. McAlister's most interesting paper upon which he should like to say a word. One interesting point was that in Dr. McAlister's experience there was much more benefit on the mental side than on the physical side. Now in the case he had to do with (Dr. McAlister knew the case) there had been no mental improvement, but there was a distinct physical improvement in many respects. That man had had malarial treatment some seven or eight months ago. Before then he had two seizures, but none since, and in many respects he was better, but there had been no mental improvement. That was one point. The other point was Dr. McAlister's reference to syphilis and malaria. When he (the speaker) was a student one of the very definite lines of treatment for syphilis was a course of quinine, particularly as regards the Dublin School of Medicine. Syphilitic cases which had had malaria abroad were told when they came home to go on with their anti-syphilitic treatment and to drop the malarial treatment. As a result they got very much worse, and although they had no malaria at the time, they were advised to go on with the quinine treatment as well. He thought the point Dr. McAlister made about malaria and syphilis was one which should not be overlooked.

Dr. WORTH raised a question whether, if the successful treatment was due to the excessive high temperature, there was not some other method which could be adopted to produce high temperatures without giving them disease.

Dr. T. C. MACKENZIE said it had occurred to him that it would be interesting, in a series of cases of general paralysis, to have an experiment made along parallel lines. Some of them remembered that when Dr. Bruce introduced a method of producing high pyrexia in cases of acute mental disorder, in quite a number of cases so treated mental improvement followed. He also thought that there was something in the suggestion that it might be possible to devise some other means than infection by malaria of producing high temperature, and not involving the risk of infecting the patient with a new disease. In any case if it were possible to take one series and inject malaria, and take another series and treat them by Dr. Bruce's method, it would be interesting to see if there were any difference in the results.

The PRESIDENT remarked that Dr. McAlister's paper appealed to him in a particular manner, as he considered that his attitude was one that should be more generally adopted than it had been. However, if they did succeed in producing long remissions they would certainly have accomplished a great deal, and considering the disease was in any case a hopeless one he thought they would be justified in using the malarial treatment. Another point he (the speaker) wished to emphasize was that he had seen a large number of ex-service men who had general paralysis and who had had malaria.

Dr. MCALISTER, in reply, said he did not want his attitude to be misunderstood. He was by no means decrying the experiment provided it was conducted under proper precautions. His paper had been intended as a warning against the indiscriminate use of old strains, which, so far as he was able to gather, produce highly toxic results, and in many cases, if the results were fully known, produced death. He thought it was unfortunate so many people who carried out the treatment did not publish their results in a candid and straightforward way. He heard private reports of cases so treated regarding which deaths were by no means rare. Those things never appeared in print: only the good results were published, and that he submitted was an unscientific way of conducting any experiment. With regard to the use of substances that would produce a temperature, he had done a good deal of work on those lines, but the results in cases of general paralysis were utterly negligible, and not worth the time and trouble the treatment takes. With regard to malaria, he felt it was an experiment they were not pushing on with sufficiently. Cases must be suitable ones, and more than that, they should endeavour to secure inoculation directly through malarial patients. The whole thing was in the realm of experiment, and it required not only time but a great deal of trouble before generalizations could be made. He was very grateful to think that his paper had provoked such an interesting discussion.

The meeting then terminated.

In the evening members dined informally at the North British Station Hotel.