No. 4 November, 1909 Vol. II

Bulletin

OF THE

Ontario Hospitals for the Insane

A Journal Devoted
to the interests of
Psychiatry in Ontario

Printed by Order of the Legislative Assembly.

EDITORS:
C. K. CLARKE, M.D., LL.D. ERNEST JONES, M.D., M.R.C.P.

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PREFACE.

It has been decided by the Government Asylum Service that, with a view to ensuring greater uniformity and regularity in the appearance of the BULLETIN, it shall in the future be not only centrally published in Toronto, as has previously been the case, but also centrally edited there. In the publication of THE BULLETIN two chief aims will be kept in view, first to arouse a more wide-spread interest in Psychiatry and its problems among the medical profession in Ontario, and secondly to furnish a permanent record of the scientific output of the staff of the Ontario Asylum Service. To achieve the first aim, papers will be published on Clinical Psychology and Psychiatry, on those aspects of Clinical Neurology and Neuro-pathology which bear on problems that arise in connection with Insanity, and on questions of Administration of Insane Institutions and the Care and Treatment of the Insane. With regard to the second aim an endeavour will be made to re-publish in THE BULLETIN all scientific articles that have been contributed by the members of the Asylum Service Staff to various medical journals. It is hoped that THE BULLETIN will be cordially supported in these aims by all those who are concerned or otherwise interested in the subject of Psychiatry. Intending contributors are kindly requested to forward their communications to Dr. Ernest Jones, 407 Brunswick Avenue, Toronto.

We are happy to be able to announce that, thanks to the foresight of the Provincial Secretary, Mr. Hanna, a definite step has been made in the direction of the long-promised Ontario Psychiatric Clinic. We all recognise that, although, as the past and present numbers of THE BULLETIN evidence, laudable efforts are being made under adverse circumstances by members of the Service, yet satisfactory scientific work in the study of Psychiatry cannot be expected until we have a suitably equipped Institution, *i.e.*, a Psychiatric Clinic,

for trained investigators. Only when this urgent lack is filled will Ontario, and indeed Canada, be able to begin to emerge from the backward position it at present occupies in Psychiatry in comparison with other countries. The step to which we have referred is the establishment of a Polyclinic for out-patient mental cases. Thanks to the courtesy and generosity of the Committee of the Toronto General Hospital accommodation has been obtained in their Gynæcological Department in Chestnut Street. The Polyclinic will be conducted on Wednesday and Saturday mornings, and reports of the work done and cases investigated will from time to time be published in THE BULLETIN. That in this early stage in its inauguration the first Department of the Psychiatric Clinic stands in close relation to the General Hospital should prove a happy omen for the future. One of the main objects in the establishment of such a Clinic is the bringing into closer touch with each other the workers in the different fields of Psychiatry and General Medicine, and it is warmly to be desired that the connection between the two Institutions now secured from the very onset will not only remain as cordial in intent as at present, but will constantly increase in extent. The relations of a Psychiatric Clinic to a General Hospital is as important as its relations to a Hospital for the Insane, and cannot fail to be productive of mutual value to both Institutions.

The Bulletin

OF THE

Ontario Hospitals for the Insane

A Journal Devoted to the interests of Psychiatry in Ontario.

THE RELATIONSHIP OF PSYCHIATRY TO GENERAL MEDICINE.*

C. K. CLARKE, M.D., LL.D.

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To the general practitioner the problems of Psychiatry, as a rule, appear unattractive, and ordinarily, distasteful. The graduates of a few years ago were fully schooled in the idea that the relation between Psychiatry and general medicine was not intimate, in fact, the so-called mental diseases could only be understood by a specialist, whose stock in trade might consist of a happy combination of divine inspiration, complicated theory and ponderous phraseology. The clinical method of investigation was applied, if at all, in a most perfunctory way, even by the hospital physician, who possibly belonged to a school of metaphysicians, whose hypotheses were as fine spun as a spider's web. The relation between cause and effect was not always well worked out, although the pathologist talked bravely about what he expected to find in the brains of the demented.

For years the psychiatrist waited for the findings of the pathologist, confidently expecting the unravelling of the most intricate problems medicine has been called on to face. The general practitioner lost faith in the psychiatrist, the psychiatrist pointed the finger of scorn at the neurologist and even to-day all is not harmony

*An address delivered before the Hamilton Medical Society, April 14, 1909.

between these specialists, simply because one class is too suspicious of the other, and not inclined to accept as valuable what experience teaches. You must decide for yourself which class is to blame, and if you will look back into what is not very ancient history, you will learn that even as eminent a man as Weir Mitchell made a mistake when he told the members of the American Medico-Psychological Association, how little they knew and how much he thought he could teach them. His caustic criticism was to many earnest workers unfair, but was received by the alienists in such an amiable manner that the neurologist no doubt began to wonder if he erred. As a matter of fact, and too late, he woke up to find that the most of his suggestions for the reformation of the much abused specialty had long before been tried and failed. In spite of this his criticism did good, and anyone who knows how much practical politics have interfered with advancement of the scientific side of our work understands why Psychiatry has not been, and is not, at its best in all parts of America. Fortunately these shackles have been broken in the most advanced parts of the United States, and with the building of Psychiatric Clinics at Boston and Baltimore a new era dawns. I had hoped that Ontario would be the first to attempt this work, but I fear others will lead us.

For many years our specialty was hampered by the bugaboo of classification, and the endeavour made to find a name for every case of insanity. It was a sort of picture puzzle which became more and more intricate and elaborate as time went on, and it must be admitted that some of the pieces did not fit in very well, nor did they add to the beauty of the picture. No broad conception of the nature of many forms of mental alienation existed, and while glimmerings of the truth came occasionally there was on the whole endless confusion. Good work was done, work that will stand for all time, but we all kept marking time, hoping that the pathologist would at least show us what had happened in the

brain, and perhaps give us a hint as to how it had occurred. In at least some diseases it was possible to give results, although causes were not so clearly made out. The microtome and microscope could, after all, only reveal results, and causes were still to be looked for in many cases of insanity. It is not a hopeful state of affairs when we must wait for the findings of the post mortem room to make our diagnosis.

I must frankly admit that even at present there is not any certain foundation on which to build a definite classification of the various psychoses we meet, because many cases of insanity are without any ætiological factors.

Kahlbaum, Wernicke and Kraepelin have enabled us to select a better starting point in psychiatry, and now the endeavour is to study a given psychosis, not only in some of its phases, but rather as an entity. In this way the old groupings of melancholia and mania have been practically obliterated, because it is discovered that melancholia and mania are truly phases of the same disease, or may even appear in combination.

The older way of doing things was to select the different kinds of melancholia, label them in an artistic way and treat them as practically belonging to one disease, whereas we know now that if a complete picture had been given of the whole course of the disease we should learn that depression might easily belong to several groups. It would be just as logical to say: in typhoid fever we frequently get a temperature of 105 deg.—this case has a temperature of 105 deg., therefore it is typhoid fever. I have been belaboured privately and publicly because I have adopted German methods of investigation, and German systems of classification, instead of showing a delightful imperialistic spirit of patriotism, which must in its narrowness select British models as the best. As science is international, or perhaps to put it in a better way, is without nationality, I am free to confess that I shall willingly accept a South Sea Island method if it is better than the

one we have. At last, though, Psychiatry has been free from the fetters which bound it, and it is now exactly on the same footing as the other departments of general medicine; and that being the case the medical man must learn to study the problems presented to him intelligently and broadly. He must adopt the clinical method of investigation, not because it is the best, for I believe that eventually a biological method even broader in its conception than the present mode will develop, but because this method affords us the most reasonable plan of studying any psychosis. At present the average physician thinks he has done his duty when he has certified that so and so is insane, and has informed the friends that under proper treatment the patient will recover. It may be a case of General Paresis in the early, or possibly the advanced stages. A few years ago General Paresis was rarely diagnosed by the average physician—shall I betray a secret—it was generally overlooked in several of our asylums. Dr. B. Hack Tuke, the eminent psychiatrist, was astounded at the want of knowledge displayed by the staffs in some of our institutions, and I fear I omitted to tell him the reason why both young and old on the staffs were so ignorant and so untrained. There can be no excuse, at the present time, for such want of knowledge, and I expect every medical student who passes through even the brief course on Psychiatry given at Toronto University, to make a fairly good diagnosis, and sensible prognosis, in the majority of cases of insanity. It is possible that we may teach him little, but the endeavour is to teach that little well. As perhaps the majority of you know, the modern method of classification is based very largely on prognosis, and while it is true that you may riddle many of the classifications with the small shot of criticism, yet if you take the trouble to reason the matter out, you will at least see that the scheme is a workable one, and can easily be remodelled as our knowledge increases. Then, too, in the past, while the most heart-breaking and brain-

puzzling lectures were given on what for want of a better name was called metaphysics, the importance of sound psychological investigation was overlooked. Now the importance of intelligent research in the realm of Psychology is on all hands recognized as absolutely necessary both for diagnosis and treatment.

Even the much criticised work of Jung and Freud on the value of association tests must be frankly admitted as having the greatest practical utility. After Kraepelin left Heidelberg for Munich, Nissl is said to have relegated most of the psychological apparatus of Kraepelin to the attic—it is significant that gradually it has all been rescued from the oblivion to which it was supposed to have been consigned, and is once more in use. Then again, while pathological research in the brain tissues is not discovering much that is new at the present time, the pathologist finds other worlds to conquer—blood conditions, body fluids of all kinds to be analyzed, disturbance of gland functions to scrutinize, in fact again the problems become identical with those of general medicine. Vaso-motor, secretory and trophic disorders are studied, in fact a good clinical examination of an important psychosis requires the work of some hours.

In nine cases out of ten your prognosis will depend on what you learn of the life history (not the present history) of your patient, and its importance should be appreciated by you. In practically every case of Dementia Præcox the early history will give you the clue to the diagnosis, and when you think that after all, ordinarily the question narrows down, not to how does this individual compare with the rest of mortals around him but rather how does he compare with himself, you will see where you stand. In short in any study of mental disease you must scan closely:

The family history,
The whole life history of the patient,
The history of the disease, and
The present condition.

rather a formidable undertaking. No wonder even a poor form of application is elaborate.

The physical examination cannot be too thorough, and even a simple system of psychological tests will reveal conditions of mind quite unsuspected by those not familiar with the results obtained by their use.

Blood pressure should, in suspected arterio-sclerotic cases, be taken as well as in conditions of depression and excitement. In suspected cases of General Paresis the cerebro-spinal fluid should always be examined chemically and microscopically. A diagnosis which is doubtful when the physical and mental conditions are alone considered is at once made clear. However, I cannot give you a comprehensive outline of Psychiatry by any chain lightning method. All that is possible is to point out the significance of thoroughness. I speak of this particularly because the ordinary certificate, as we see it, is not a tribute to the painstaking methods of the physician, but rather a document inclined to make one suspect that general rather than particular lines of examination have been followed.

Now as to the classification of mental diseases: The safest foundation for a classification would, of course, be one offered by pathological anatomy. Unfortunately, in but a few of the forms can such a foundation be discovered. So, too, when we attempt to find an ætiological basis, we can only go so far with our intoxication and infectious disease groups. Then again as Kraepelin has well said: "Any single pathogenic factor may make itself known by a great variety of symptoms."

Again to quote Kraepelin: "Judging from our experience in internal medicine it is a fair assumption that similar disease processes will produce identical symptom pictures, identical pathological anatomy and an identical ætiology. If, therefore, we possessed a comprehensive knowledge of any one of these three fields—pathological anatomy, symptomatology or ætiology—we should at once have a uniform and standard

classification of mental diseases. A similar comprehensive knowledge of either of the other two fields would give, not only just as uniform and standard a classification, but all of these classifications would exactly coincide. Cases of mental diseases originating in the same causes must also present the same symptoms and the same pathological findings. In accordance with this principle it follows that a clinical grouping of psychoses must be founded equally upon all three of these factors, to which should be added the experience derived from the observation of the course, outcome and treatment of the disease."

Reasoning from these premises the classification adopted at present is something as follows:

Psychoses which are due to auto-intoxication or faulty metabolism (myxoedematous insanity). It is supposed that dementia præcox should be included in this group also.

Organic dementias—These are dependent on traumatism, embolism, syphilitic lesion, etc.

The insanities belonging to the involutional periods such as senile dementia, etc.

A prominent group is that known as the manic-depressive in which a morbid constitutional basis plays an important role. In this form mental deterioration is not prominent and the prognosis is essentially hopeful.

Along such lines are the groupings made.

It would be well if every physician could devote something of his time to a study of even the simplest principles which underlie the subject of psychiatry at the present moment, and the reading of even such a work as the little handbook of Clinical Psychiatry, by White, would amply repay any one who wished to get in touch with what is being done. He would at once feel that his training in general medicine is the very thing to enable him to get an intelligent conception of diseases which have seemed to him in the past a mysterious

muddle. It would enable him too to detect the early danger signals which are so plainly apparent in the majority of cases of dementia præcox. I should not care to say that such knowledge would absolutely enable one to head off threatening trouble but we are not justified in saying that such a result might not occasionally be attained. It is quite certain at least that many of the acute exacerbations of disease in such cases might be averted if physicians could give intelligent warnings to the friends. We realize this when we see how simple a matter it is to precipitate such attacks in these weaklings, and how easily they may be steered clear of by those who understand the dangers and relieve the threatened ones of the strain that is imposed.

Take the subject of education alone. How utterly absurd it is to subject the developing dementia præcox patient to a strain imposed by our "all individuals equal" method. When I think of the many tragedies precipitated by just such methods I am amply justified in asking the physician to know his dementia præcox as well as his typhoid; it is just as important. I do not suppose that you will become full fledged psychiatrists, but there is no reason why the average practitioner should not have an intelligent conception of Psychiatry, assuming that it is simply a branch of general medicine, and he should at least have a fund of knowledge that would enable him to select those cases which should at once be placed under efficient treatment, as well as to give a reasonable prognosis to the friends.

I do not think that many cases will be efficiently treated outside of institutions, but I insist that it is the duty of the physicians of Canada to unite in demanding that the Government provide institutions where the really hopeful cases may receive the benefit of proper treatment in the earliest phases of mental disease, and that every facility be afforded the physician, in every day practice, to have his patient placed in such hospitals. Not only that, it is the right of the people that their physicians should have opportunity to acquire in

properly constituted clinics a thorough knowledge of one of the most important branches of general medicine. Our asylum system has not proved adequate, nor can it be made so. The day of argument regarding this has gone by, the herding of the acute with chronic is wrong, the demands for research are imperative, our duty is plain and we must keep in step with work that is being done in other countries. People have a natural repugnance to send their friends to any asylum where the stranded hulks of wretched humanity forever haunt them with the possibilities of what they may come to. We say that the psychological element is an important one in treatment, and yet in the asylum system it is the one almost utterly disregarded. We talk glibly of what we have done for the insane of our country at so much *per capita*—the cheapest system on earth—as if that were a thing to boast of; much better would it be to discuss the things we have not done, but must do, if we are to keep pace with modern progress. I am not the advocate of the specialist, but rather the champion of the physician at large, and my plea is for general medicine rather than for Psychiatry alone. Week after week, when I go out in consultation in cases of recent development, and feel with the physician how helpless he is in the present situation, I realize more and more the necessity for a more active crusade on the part of the profession at large for better facilities in the treatment and care of acute insanity.

Such patients as I mention cannot be properly treated outside of an institution; the asylum is a hideous nightmare both to friends and patients, and usually is not resorted to until the day of treatment is past. A prominent man said to me recently: "Doctor, we have heard much from you about modern methods, modern cures, continuous baths and so on, and yet the recovery rate from insanity is just where it was years ago." Such is the argument employed daily, and the layman is quite satisfied that his contention is correct, his figures say so, and figures never lie. In that very

question of this prominent politician rests the bitterest indictment of our inaction. What are the reasons that such a question can be asked, and arguments backed up with figures brought forward to prove its truth. When I looked into my figures for the year it proved just what I expected, out of two hundred and sixty admissions only some seventy odd, could by the wildest stretch of imagination be crowded into the curable class. Of these it may proudly be stated a large proportion recovered, as would be the case under almost any condition. What struck me was how many more would have been rescued had the conditions been ideal, rather than "the best that we could obtain under the circumstance." What struck me more forcibly still was, how many of those who were admitted in a hopelessly chronic state could have been cured, had they been taken in hand early in the day.

How can general medicine be helped, too, by our asylum system? The general physician cannot be brought in touch with it, nor can the asylum man get easily in touch with the one he should see much of. What must be demanded by medicine is, that hospital accommodation, at least as good as that of general hospitals, be provided for people suffering from psychoses, that Universities must have Psychiatric Clinics for the treatment of acute cases and the study of acute mental diseases.

The State has no greater responsibility than the care and cure of its insane. Medicine has no more important realm to conquer than that of Psychiatry, but until the physician comes to the aid of the psychiatrist and insists that the problems are really those of general medicine, we shall not succeed in what should be our aim to accomplish.

I have laboured long and hopefully in the psychiatric vineyard, have struggled against apparently hopeless odds, at times have seemed almost on the point of achieving what it should be any sincere man's aim to do, only to be shoved back when success appeared certain.

I shall continue to endeavour to make others see what experience has made plain to me, but I feel now that success can only be gained when the whole profession joins together and demands what is necessary, in the interests of the sufferers from the most dreadful of all diseases. The fight is not mine, it is yours, and when there is in medical ranks a really broad conception of the needs of Psychiatry the victory will come.

THE MUCH-HOLZMANN TEST IN THE ACUTE PSYCHOSES.*

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THIS paper deals with the work done in an endeavor to ascertain the value of the so-called 'psycho-reaction described recently by Much and Holzmann.[1] These authors, after the examination of the sera of four hundred persons; two hundred normal individuals, and an equal number of insane; concluded that it was possible by means of the new reaction described by them, to differentiate cases of manic-depressive psychosis and dementia praecox from all other forms of mental disturbance, and also from persons suffering from any physical disorder. They claimed to have been able to show that the sera of patients with either manic-depressive psychosis or dementia praecox would inhibit the hemolysis of washed, human corpuscles by cobra venom. The method of procedure was as follows: 0.35 c.c. of serum was set up with 0.25 c.c. of a 1-5000 dilution of cobra venom and 0.5 c.c. of washed 10 per cent. human corpuscles in 0.9 NaCL. The tubes, after being set up, were

* Read before the New England Psychiatric Association, Rutland, Mass., Sept. 29th, 1909.

put in the thermostat at 37 deg. C. for two hours, after which they were put in the ice-box for twenty-two hours when the results were read. The tubes were shaken before making the reading, and the authors regarded the result as positive when the mixture was entirely opaque; when hemolysis remained distinct after shaking it was negative. Incomplete but distinctly recognizable hemolysis was also regarded as negative. Controls were frequently not completely hemolyzed at the end of twenty-four hours, occasionally they are, however, and complete hemolysis is always present in the control tubes at the end of twenty-four hours. The authors then recorded their results and obtained positive results only in dementia praecox and manic-depressive psychosis. Cases of neurasthenia, pre-senile dementia, lues, paralysis, alcoholic pseudo-tabes, all gave negative results. The authors noted in a few instances that where there existed a family history of circular insanity plus epilepsy a positive reaction might be obtained. The examination of the cerebro-spinal fluid in all cases was negative. It was further noted that manic-depressive psychosis and dementia praecox could not be differentiated by means of this reaction. Altogether the writers give their results in four hundred cases, one hundred and eighty-one of which are tabulated, and they conclude that one should get the reaction in one hundred per cent. of clear-cut cases of manic-depressive psychosis.

Since the appearance of the work of Much and Holzmann quite an extensive literature on the psycho-reaction has appeared, and the findings have been very conflicting. Hübner and Selter[2] tested the reaction in eighty-two cases and their results were as follows:

	Not tried.	Pos.	Neg.	Slight.
Manic-depressive	27	12	7	8
Dementia præcox	24	10	5	9
Epilepsy	2	1	1	0
Various conditions, including tabes, general paresis, imbecility	29	21	4	4

They conclude, therefore, that the cobra venom hemolytic reaction is not specific for manic-depressive psychosis or dementia praecox, that it may be obtained in various physical disorders as well as in the psychoses neuroses and organic brain diseases, and that in not more than fifty per cent. of cases of manic-depressive or dementia praecox is a positive reaction obtained. Alt,[3] in another article, shows that the reaction is not specific for dementia praecox and manic-depressive insanity, his results are based on a study of fifty cases. Zaloziecki[4] was able to obtain a positive psycho-reaction with various forms of insanity and also in ordinary physical diseases and practically constantly in the newborn. He believes that its real diagnostic value is insignificant and that its presence might be explained as being simply attendant upon muscular activity. The work of Fraenkel,[5] Beyer and Wittleben,[6] and R. Kraus,[7] and his collaborators, would also go to show that the psycho-reaction is indeed of questionable value as a diagnostic agent. Through the kindness of Dr. Noguchi, of the Rockefeller Institute, who was good enough to supply me with cobra venom, and the generous co-operation of the staffs of certain of the Massachusetts State Hospitals, I was able to make the test in a fair number of cases, the results of which are given below in the form of a table.

It will be seen at once that while there were certain evidences pointing to an inhibition of hemolysis at the end of two hours in the thermostat that after twenty-two hours more in the ice-box complete hemolysis was present. In no case of manic-depressive insanity or dementia praecox was a clear-cut, positive reaction obtained; on the other hand in two non-insane patients, a positive reaction was obtained. If, as in ordinary hemolysis ex-

periments, a reaction was regarded as positive when hemolysis was complete in the control and not in the other tubes, then about one-third of the cases of manic-depressive and dementia praecox would have been regarded as positive. Noguchi (personal communication) is of the opinion that practically no cases of manic-depressive give the reaction, about 50 per cent. of cases of dementia praecox, as well as various other conditions. His opinion is founded on an examination of 187 sera, the work having been done in conjunction with Rosanoff.* One feels safe in concluding, then, that the so-called psycho-reaction of Much-Holzmann is not a reliable method for the serum diagnosis of manic-depressive psychosis or dementia praecox.

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*Since this article went to press the communication of Rosanoff has appeared in The Archives of Internal Medicine, October, 1909.

TABLE I.

0 = negative, *i.e.* no hemolysis ; + = positive, *i.e.*, complete hemolysis.

Name and No.	Clinical Diagnosis.	Hemolysis at end of 2 hrs., 37 C.	Hemolysis at end of 24 hrs.
1. M..	D. P.	+	
2. A.	D. P.	?	+
3. B.	D. P.	+	
4. H.	D. P.	+	
5. Z.	D. P.	+	
6. C.	D. P.	+	
7. W.	D. P.	0	+
8. D.	D. P.	Partial	Partia
9. D.	D. P.	+	
10. H.	M. D.	0	+
11. H.	M. D.	Partial	+
12. M.	M. D.	Partial	+
14. Tewksbury, 61	Endocarditis, acute..	+	
15. Tewksbury, O. E., 15	Chronic Gonorrheal Urethritis	0	0
16. Tewksbury, O. E., 22	Acute Dysentery	+	
17. Tewksbury, 96	Tuberculosis Pul.	+	
18. Tewksbury, O. E., 11	Acute Gonorrheal Urethritis	+	
19. Tewksbury, S., 56	Chronic Parenchym. Nephritis	+	
20. Tewksbury, 59.	Aortic Stenosis	+	
21. Tewksbury, E., 56	Lobar Pneumonia	+	
22. Tewksbury, O. E. W., 18.	Syphilis, primary and second	0	0
23. Tewksbury, E. W., 35.	Alcoholic delirium	Partial	+
24. Tewksbury, S. W., 15.	Broncho-pneumonia	+	
25. Tewksbury, 57	Acute Gonorrheal Arthritis	+	
26. E	M. D	+	
27. F.	M. D	+	
28. T.	M. D	+	
29. C.	M. D	+	
30. O.	M D	+	
31. T.	Epilepsy	Partial	+
32. G.	Epilepsy, c. M. D.	0	+
33. M.	D. P., c. Epilepsy	0	+
34. M	Epilepsy	0	+
35. E	Hysteria-Epilepsy	0	+
36. M.	Dep. Phase M. D.	0	+
37. A.	Dep. Phase M. D.	0	+
38. M.	Manic Phase M. D	0	+
39. M.	Alcoholic (?)	Partial	+

A CASE OF GLIOMA OF THE OPTIC THALAMUS.*

By W. C. Heggie, M.D., Toronto.
and Ernest Jones, M.D., Toronto.

Notes by Dr. Heggie.—Ruth Taylor, ten years of age, fourth of seven children, six living; one died in infancy of iliocolitis; father and mother healthy; no history of any specific disease; temperate; family history good on both sides. First saw child on December 16, 1908. Face was drawn to right side; internal strabismus of left eye; head inclined backward; walks with a kicking motion of left foot; patellar reflex exaggerated on left. Babinski's sign on left foot. Mouth egg-shaped, with large end to right. Seemed fairly intelligent, and on calling two days after she remembered me and my name. On sleeping she relaxed perfectly and looked normal.

Past History.—Healthy, except measles, until September, 1908,

1st. Noticed that she slept more than usual, going to bed at 6 p.m., and not waking until 8 a.m.

2nd. Noticed internal squint of left eye.

3rd. Mouth pulled to right side.

4th. Head pulled to right side.

5th. On eating, while raising the right hand she would lift up the left hand.

6th. Noticed the left foot drag and was brought forward with a kicking motion.

7th. Began to throw head backward.

8th. Complained of wanting to fall backward.

*Read before the Ontario Academy of Medicine (Section of Surgery), March 16, 1909. Published in the Dominion Medical Monthly, September, 1909, p. 95.

These symptoms were progressive and in the order named. There was an obscure account of a fall. I gave a tentative diagnosis of brain tumour on right side, and asked to have Dr. Clark, of the Asylum, see her.

Drs. Clark and Jones saw her at the Toronto Asylum about the 20th, and agreed that it was a tumour, probably of the right cerebellum.

On the 26th December she began to lose control of bladder and rectum, so I advised having her taken to the hospital for operation. She went to the Western Hospital, December 27th, was operated on the following afternoon, December 28th, by Dr. G. Bingham. The first stage of the operation, removing the bone and completely exposing the cerebellum, was completed successfully, and the patient removed to the ward. She died thirty hours after from shock. On post-mortem the trouble was found to be a glioma of right optic thalamus.

Comments by Dr. Ernest Jones. At the autopsy I performed on the case just related by Dr. Heggie, there was, to our surprise, no tumour visible to the naked eye. As, wever, the optic thalamus on the right side was darker and of a firmer consistence than its fellow, I removed it, together with some other portions of the brain, with a view to further examination. On studying it by means of the routine staining methods for cerebral tissue one found that the thalamus was the site of an infiltrating tumour. From the character of the cells and supporting tissue this was evidently of a gliomatous nature. The nervous tissue had been largely destroyed, but there was hardly any evidence of nervous degeneration of toxic origin, so that the mechanical factor was the dominant one. The mass occupied the posterior two-thirds of the thalamus and extended for a short distance beyond its margin.

Thanks to Dr. Heggie's courtesy I had the opportunity of seeing the case on one occasion during life, though under circumstances that prevented examination thorough enough to arrive at any precise diagnosis

as to the seat of the lesion. I might therefore add a few words on the clinical side of the case. One matter that reflection on the case prominently brings out is the caution that should be exercised before ascribing an absolute localising value to any single physical sign of cerebral tumour, and the greater accuracy attained by carefully considering all aspects of a case and by estimating the significance of a given sign only in relation to the others present. The insidious temptation to seek one or two short cuts to diagnosis has strenuously to be avoided, for there are very few such direct routes, at all events in neurological diagnosis. For instance, it is possible that in the present instance undue stress was laid on the peculiar attiude of the head—that combination of lateral flexion, extension and contralateral rotation which is known under the name of the cerebellar attitude. This symptom is especially common in cases of cerebellar disease, but I have known it to occur in cases of tumour of the frontal lobe, and it is by no means rare for it to be produced by pressure on the cerebellum exerted by a tumour of the cerebello-pontine angle. The differential diagnosis between a tumour of the cerebellum and one of the contralateral optic thalamus often presents great difficulty, the anatomical connections between the two structures being especially intimate. It is not always easy, for instance, to distinguish between the dynamic form of ataxia that is characteristic in cerebellar disease, and the involuntary and inco-ordinate movements that in thalamic cases interfere with volitional motor activities These latter movements are sometimes even confounded with choreic movements, and the disastrous mistake committed of making the diagnosis of chorea; this, I understand happened in the present case before it was seen by Dr. Heggie, though careful study of the type of movement should, quite apart from consideration of other physical signs, prevent such an error.

The case similarly illustrates the importance of forming a standard of relative importance for each

physical sign, so that when two signs apparently contradict each other we shall know to which of the two should be attached the greater weight. For instance, the hemiplegia in the present case pointed to a right-sided affection, whilst a transitory paralysis of the abducens nerve pointed to a left-sided one. As, however, the latter symptom is often produced indirectly through general intracranial pressure—or, more accurately speaking, through longitudinal stretching of the cerebral axis, as Collier has pointed out,—it was easy to realise that it should be disregarded as a sign of localising value and not allowed to discredit the correct conclusion indicated by the hemiplegia that the lesion was on the right side. In forming such an opinion great care had to be exercised to make certain that the left hemiplegia was of the cerebral type and not of the cerebellar, for in the former case the lesion would be right-sided; in the latter left-sided. It would be out of place here even to enumerate any of the large number of differential points that enable us to distinguish between the two, but I may point out that here again the difficulty of distinguishing between a cerebellar and a thalamic tumour is shown, for a hemiplegia due to thalamic disease resembles that due to cerebellar more closely than does any other cerebral hemiplegia, except possibly the cortical form. Not only may thalamic movements be mistaken for cerebellar ataxy, as has just been mentioned, but, further, the thalamic hemiplegia is apt, like the cerebellar one, to be of a flaccid variety and to be unaccompanied by those changes in the reflexes that we regard as distinctive of interruption of the cerebro-spinal motor paths.

In conclusion we are again reminded of the fact that every case of cerebral tumour, if carefully studied, serves to refresh or expand our knowledge concerning the important question of cerebral localisation.

ON THE USE OF CERTAIN NEW CHEMICAL TESTS IN THE DIAGNOSIS OF GENERAL PARALYSIS AND TABES.*

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The diagnosis of general paralysis of the insane is for two reasons a more important matter than the diagnosis of most forms of insanity. On the one hand, thanks to the clinical observations of Pilcz, Kraepelin, and a host of other workers, with the pathological researches of Nissl and Alzheimer, we have a clearer picture and more exact knowledge of the course and nature of the disease than about almost any other psychosis, so that we can foretell what events are likely to occur in it, and take measures to guard against them. On the other hand, the prognosis is more fatal and the lethal termination more rapidly reached than in any other form of insanity, so that it is important to recognise the condition as soon as possible, and to get the patient's affairs arranged on the basis of that knowledge. Although in a pronounced case the clinical picture is one of the sharpest in the whole of medicine, yet in an early or non-typical case the difficulties in diagnosis are often exceedingly great. So much is this the case, that it is found in asylum practice that the majority of the patients are admitted either with an erroneous diagnosis, or else comparatively late in the course of the disease. Hence the additions to our knowledge that of late years have accrued from a study of the cerebro-spinal fluid in the disease have been

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welcome and valuable aids to the practitioner. These newer laboratory methods, of course, can never replace accurate observation of the well-known physical signs of the condition, any more than Widal's test has replaced the employment of physical examination in the case of typhoid fever, but a knowledge of them is of the utmost importance when we have to deal with obscure and doubtful cases.

Before detailing some personal researches it will be well to review very briefly our present knowledge concerning the cerebro-spinal fluid in general paralysis, and this is best done, perhaps, by mentioning the various points in the chronological order of their discovery.

The first step in this connexion, and still one of the most important, was the discovery, made in 1900 by Ravault, Widal, and Sicard, that lymphocytosis in the cerebro-spinal fluid is one of the most constant accompaniments of both general paralysis and tabes. Many forms of cells are found, the most significant being large mononuclear lymphocytes and plasma cells. When in a non-febrile malady the lymphocytosis is very pronounced, then it may be regarded as practically pathognomonic of general paralysis. The cell increase is greater at the onset of the disease—a fact that obviously enhances its value in diagnosis.

A year later the discovery was made by Achard, Loefer, and Lanbry that considerable quantities of proteid are to be found in the cerebro-spinal fluid in general paralysis and, to a less extent, in tabes. As the present paper is chiefly concerned with this question, consideration of it may be conveniently postponed for the moment, and it will suffice here to mention the well-established facts that in normal cerebro-spinal fluid the quantity of proteid is minimal, that in the diseases in question the increase is mainly an increase in globulin, and that this increase is not always parallel with the degree of lymphocytosis, tending on the whole to develope later than this phenomenon.

The next discovery was the startling one made by Wassermann in 1906, that the cerebro-spinal fluid in general paralysis contained substances which, when combined with the syphilitic virus, have the power of inhibiting haemolysis. It will be remembered that in 1900 Bordet demonstrated that in various processes, of which haemolysis may be taken as a type, three bodies are essential components. These are, first, the antigen, or substance that is being destroyed—bacterium, blood cell, etc., as the case may be; secondly, a non-specific substance, or complement, found in all blood serums; and, thirdly, a specific substance, or amboceptor, found only in the serum of an individual that has been previously injected with the corresponding antigen—the amboceptor being thus evoked as a response to the foreign body. It follows that if two of these three bodies are present in a fluid and cytolysis does not take place, it must be due to the absence of the third; thus, if the antigen is not dissolved on being added to its corresponding amboceptor, the complement must be missing. In this way we can test for the presence or absence of complement. Now Wassermann found that syphilitic virus obtained from a foetal liver in which spirochaetes had been demonstrated, when in the presence of the cerebro-spinal fluid of a paralytic, made a combination with it, and the complement present in any blood serum. This complement was thus taken up or fixed and was no longer free to cause haemolysis when added to red blood cells and haemolysin in the way just mentioned. For this test he first incubates for an hour the syphilitic liver emulsion with the suspected cerebro-spinal fluid and the complement-containing serum of a guinea-pig; he then adds the mixture to an emulsion of washed red blood corpuscles of a sheep and some serum of a rabbit that has been several times injected with sheep's blood. If the blood cells dissolve—that is, if haemolysis or laking takes place—then the complement must have been free to do it, and could not have been fixed by the preliminary incubation. Wassermann maintains that this is due to the absence of any syphilitic anti-body or amboceptor in the cerebro-

spinal fluid. On the other hand, if laking does not take place, then the complement must have been previously fixed by the combination of the syphilitic antigen from the liver and syphilitic antibody in the cerebro-spinal fluid.

While Wassermann's observations have received the widest confirmation, his interpretation of the phenomenon has met with serious criticism, and is to-day practically discredited. It is quite true that the reaction just described is positive in over 95 per cent. of cases of parasyphilis, and negative in other diseases, so that beyond doubt there is some substance in the cerebro-spinal fluid, which, when combined with syphilitic virus has the power of fixing complement and of thus inhibiting haemolysis. But the nature of this substance is a far more disputable matter. That it is not a specific syphilitic antibody seems to be certain from the observations made, first by Weil and Braun, that it shows the same power of inhibiting haemolysis when combined with other substances than syphilitic virus—for instance, lecithin. It is now known that the place of the syphilitic antigen can be taken by a number of substances, including lecithin, bile salts, brain emulsion, normal liver emulsion, etc.; and Benecke has recently brought evidence to show that the efficacy of the syphilitic liver in Wassermann's experiments is due to the presence in large quantities of a peculiar soap pellicle that surrounds the fat droplets characteristic of that lesion.

Leaving, however, the theoretical aspect of the question, we have to note that all workers at the subject—notably Plaut, Morgenroth, and Stertz, Marie, Levaditi, Yamanouchi, Weil, Eichelberg, and Neubauer—are unanimous as to the high practical value of Wassermann's discovery. It may be said at once that it is the most certain sign of general paralysis we at present possess. Its only disadvantage is the complexity of its application, and before it can be of much value in practice it will have to be considerably simplified.

Two other methods may be briefly mentioned. In 1907, Fornet and Schereschewsky stated that the serum of a luetic patient gives a specific precipitate with the

serum of a paralytic. This observation has been received with much scepticism, and Plaut, who is, perhaps, the most reliable authority on the subject, says that this precipitate is just as common with normal serum. In 1908, Porges and Meier showed that the cerebro-spinal fluid of paralytics causes a heavy precipitate when added to the lecithin emulsion. This is of interest when one remembers the important part played by lecithin in the Wassermann reaction.

The theoretic interest that the increase in globulin has resides in the fact that most authorities agree in attributing to it the origin of the substance active in the Wassermann reaction. The trend of opinion is towards regarding this as resembling choline and nucleo-proteid in being katabolic products produced in the course of the disease. The relation between globulin and the substance that gives the Wassermann reaction becomes, therefore, a matter of great interest, and the phenomenon of globulin increase receives an accesion of both practical and theoretical importance from these considerations.

Now, the experience of the past eight years has amply confirmed the fact of globulin increase in general paralysis and its very great value in diagnosis, and the object of the present communication is to consider two new methods for the precise observation of this increase, together with the results of our experience with these methods. The first of them was described some five months ago by Noguchi, of New York; the second has not hitherto been described.

There are, of course, several methods in general use for the separation of globulin from albumin, but all of these leave much to be desired in reliability and delicacy. For the cerebro-spinal fluid the following are those that have been most employed. Guillain recommends that the fluid be saturated with magnesium sulphate and then heated; a precipitate indicates the presence of globulin. Nissl, Henkel, Nonne, and Apelt, who have all published extensive monographs on this subject, add to the fluid an equal quantity of a saturated ammonium sulphate solution. Cimbal adds a saturated zinc sulphate solution. In

our experience, however, these methods sometimes fail even after twelve hours to give a precipitate with fluids that at once give one in the two tests next to be described.

We shall consider first the technique of these tests and then the results obtained.

Whichever test be applied, it is essential first of all to be sure that no blood or pus has contaminated the fluid to be examined; results are of little value, even in cases in which there was a very high lymphocyte count, on account of the secondarily derived globulin. The test may be carried out at any date after the puncture, provided only that the fluid be clear.

The Noguchi reaction consists in the addition of 0.5 c.cm. solution of 10 per cent. butyric acid in normal sodium chloride solution to 0.1 c.cm. of the fluid to be examined, the application of heat, subsequent addition of 0.1 c.cm. of 4 per cent. sodium hydrate solution, with a further application of heat. The test tube should be read within three hours. A distinct opalescence is frequently found to occur even with the normal, but in cases of general paralysis and tabes a characteristic precipitate of a peculiar flocculent character forms. The flocculi tend gradually to fall, so that after twenty-four hours at the latest the bottom of the tube is occupied with a fairly bulky precipitate, whilst the supernatant fluid is clear. In performing this test care must be taken to ensure the absolute purity of the butyric acid. This was evidenced during the experiments by the following occurrence. We had finished the brand of butyric acid obtained from Dr. Makins, of New York, that had been found to be satisfactory, and on December 7th tried a new brand in six cases. To our surprise all of these gave negative results, a finding that was shown, by comparison with some that Dr. Noguchi kindly placed at our disposal, to be due to impure butyric acid.

The second test referred to is as follows: 2 c.cm. of a saturated solution of ammonium sulphate are placed in a test tube, and 1 c.cm. of the cerebro-spinal fluid is gently run on to the surface in the way done in the Heller

nitric acid test for albumen. The formation of a ring at the junction of the two liquids constitutes a positive reaction. The ring is clear-cut, thin, greyish-white, and has the thickness of a thin piece of paper. It should form within three minutes, and within half an hour it may be observed that the surface of the ring shows a delicate mesh appearance resembling a fine cobweb. Indirect illumination must be used, or it may escape detection. For this purpose we have constructed a black-lined box into which the test tube can be inserted and viewed at right angles to an electric bulb which is fixed within the box a few inches away. In applying the test it is essential to see, first, that the ammonium sulphate is pure, so that the solution is neutral and not acid; and, secondly, that the solution is quite saturated, which is best ensured by the use of heat in the manufacture of it.

We come now to the question of results. Up to the present we have examined only 27 cases, but the paucity of our material is partly compensated for by the uniformity of our findings, which has encouraged us to believe that the tests in question will prove to be of considerable utility.

The Noguchi test was applied in 15 syphilitic cases and 12 non-syphilitic. It was negative in all of the latter except in 1 case of tuberculous meningitis. Among the negative cases were 5 of dementia praecox and 4 of tumor cerebri, conditions which are frequently very difficult to distinguish from general paralysis. Among the 15 syphilitic cases were 12 untreated and 3 treated cases. None of the latter gave a positive reaction, while all of the former did so. The 12 positive cases comprised 3 of tabes, 5 of general paralysis, 1 of tertiary syphilis, and 3 of syphilis of the nervous system. The test was thus positive without exception in all cases of syphilis or parasyphilis that had not had recent treatment, and negative in all other cases examined.

The effect of treatment was shown not only by the non-reaction of the cases under treatment, but also in the disappearance of the reaction seventeen days after initiating treatment in a case that had previously shown a

marked positive reaction. It is known that there is an excess of proteid in the cerebro-spinal fluid in no chronic disease of the nervous system apart from syphilis, and that agrees with our findings. In cases of acute infection, on the other hand, there is often an excess of proteid present, whatever be the nature of the infection. This was so, for instance, in the only case of this kind—one of tuberculous meningitis—that we have been able to examine.

The ammonium sulphate ring test was applied in all of the above cases except two of tabes. The results agreed absolutely with those obtained by the Noguchi test, being positive whenever this was positive and negative whenever this was negative, so that the list of cases need not be repeated. The amount of proteid present in normal cerebro-spinal fluid is insufficient to give a ring with ammonium sulphate, though it commonly gives one with pure nitric acid. In general paralysis the amount is increased tenfold, and far exceeds that reached in any other disease, except, of course, acute meningitis. In the differentiation of general paralysis from syphilis with no nervous manifestation, we would rely not on the mere excess of proteid in the former, for that occurs in both conditions, but upon the striking *extent* of the excess. This can be roughly estimated with the ammonium sulphate ring test in three ways—by noting, first, the density of the ring; secondly, the time that elapses before its appearance, and, thirdly, by the dilution with which it appears. The last point has greatly interested us, particularly, however, in connexion with the globulins of the blood serum in syphilis, a matter with which we are not here concerned. Our observations on the point are as yet incomplete, but it would seem that we have in the dilution test a means of readily estimating the amount of globulin present, and, therefore, the degree of certainty of the diagnosis. We have several times, for instance, obtained a positive reaction in fifteen minutes after diluting the cerebro-spinal fluid eightfold, a phenomenon that certainly never occurs in the normal.

To sum up, we consider that we have in the Noguchi reaction and in the ammonium sulphate ring test two new methods of considerable value for readily recognising an excess of globulin in the cerebro-spinal fluid, and thus for determining the presence of some parasyphilitic affection of the nervous system.

PSYCHO-ANALYSIS IN PSYCHO-THERAPY.*

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The evolution of psycho-therapy, like that of all other modes of treatment, is marked by an ever increasing precision in method and an ever deepening comprehension of the conditions to which it is applicable. Progress in these two respects must always go hand in hand, for the moment therapeutics becomes divorced from pathology and diagnosis it leaves its scientific basis and stands in danger of approximating to that medical charlatanry which it is the highest interest of our profession to resist. The two studies are peculiarly interwoven in the case of the psycho-analytic form of psycho-therapy, for, as I shall presently indicate, treatment is here carried out by simultaneously laying bare and remedying the pathological mechanisms at the basis of the malady. From this point of view we can discern two stages in the development of any new method of treatment, and these I can best illustrate by a reference to more familiar methods, for instance the operations of trephining or of laparotomy. When the possibility of these operations was first realised we saw the first stage in development, in which, namely, they

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were regarded merely as an adjunct to the therapeutic armamentarium, and were applied in the relief of conditions that were already well known and studied on established pathological lines. The second stage arose when, through the repeated performance of such operations, conditions that could be relieved by them came to be studied anew, fresh aspects of pathology opened up, and questions of precise diagnosis that had previously been academic problems of trivial interest now became urgent matters of life and death. A moment's reflection on the history of appendicitis will remind you of how little we knew of the pathology, the diagnosis or even the existence of the affection until the surgeon's knife shewed that it could be cured. We might in fact paraphrase the motto underlying British Imperialistic policy, to wit, that Trade follows the Flag, and say that in medicine Diagnosis follows Treatment.

Now in psycho-therapy most of the medical world is at present only entering on the first stage. That the medical world of America will definitely enter on this stage as a prelude to further advancement will, I trust, be one of the results of this afternoon's conference. In this stage we clearly recognise that we have secured a new therapeutic weapon of the utmost value, which we may describe as the capacity to alleviate certain complaints by purely mental measures, in other words as psycho-therapy in its broadest sense. Our attitude towards the nature of these complaints, however, remains in this stage substantially the same as it was when they were treated only by physical remedies. Hence we may see the strange picture of a physician removing by verbal suggestion a symptom which he considers is produced by a toxin circulating in the blood. However, a thoughtful person who employs any form of psycho-therapy soon realises that a symptom which can be removed by mental measures is in all probability of a mental nature. It may parenthetically be remarked that he further realises how the suffering endured by the patient, so far from being unreal, is all the more dreadful and formidable for having a mental and not

a physical origin. A non-appreciation of this important fact is still all too common. Only recently an article appeared in one of the leading medical journals in which the writer remarked: "In this manner I hope that we will always be able to trick a malingerer or hysterical subject into betraying the falsity of his claim." This attitude, though rarely in such an outspoken form, is frequently implicit in medical writings, and cannot be too strongly condemned. Apart from yielding an inkling of the mental nature of various disorders, the first stage in the evolution of psycho-therapy is characterised by an indeterminate attitude towards the origin and pathogenesis of them. The older conceptions have begun to dissolve, but the knowledge won by the new method of treatment has not yet been formulated. Psycho-therapy is in this stage employed in a quite empiric way, and the physician either does not concern himself with the intrinsic *modus operandi* of his treatment, or else offers explanations of it which are so superficial as to be of little scientific value.

Psycho-analysis represents the second stage in the evolution of psycho-therapy. Here a deeper insight is sought into the essential nature and origin of the morbid phenomena with a view to obtaining a fuller understanding of the aims of treatment and so to achieving a greater precision in the application of it. The psycho-analytic method we owe almost completely to the genius of Professor Freud of Vienna, who in the past sixteen years has wrought it into an elaborate science of which I can here give only the most summary outline. The method is based on the knowledge that the symptoms present in the psycho-neuroses owe their origin to a conflict between different groups of ideas or mental processes which cannot be brought into harmony with one another. One complex of mental processes is for some reason or other of such a kind as to be inacceptable to the main body of the personality. The personality fails to assimilate it, will have nothing to do with it, tries to forget it, to submerge it, to "repress" it. The "repressed" complex then takes on an

automatic existence, and acts as an irritating foreign body in the same way as any physical foreign body that has not been absorbed. From this point of view we may define the pathology of the psycho-neuroses as a *defect in assimilation.*

Let me illustrate my meaning with a concrete instance. A man conceives an attraction towards the wife of a near friend or relative, and his imagination perhaps plays with the thought of what might happen were the friend to meet with a fatal accident. If he honestly faces his wish and realises its nature he will instantly see that, though possibly a perfectly natural one, it is of such a kind that for social and ethical reasons it must obviously be suppressed. If he adopts this healthy attitude he will probably think no more about the matter, except in the most harmless way. The wish-complex is here assimilated by the main body of the personality. If on the other hand he regards the mere possibility of entertaining such a wish as a sin and a sign of the most desperate iniquity he may refuse to own up to himself that he has ever felt it, even momentarily; whenever the thought occurs to him he endeavours to put it from him, to get away from it, in other words to *repress* ("verdrängen") it. The complex here is not assimilated, it therefore continues to act, and the more the man strives to escape from it, the more hauntingly does it torment him. He has now become the prey to a fixed idea which is out of his control, and which evinces its independence by appearing irregularly whether he wills it or not. In actual practice we never meet with cases so simple as this, but the instance will serve to illustrate the notion I am trying to convey, namely that certain mental processes, particularly strivings, desires and impulses, if they are not absorbed in the main stream of the personality are apt to manifest an independent activity out of control of the will. This activity is usually of a low order, of an automatic and almost reflex kind, and—if I may be allowed to use the term in a clinical and non-philosophic

sense—it is generally an *unconscious* activity, that is to say it operates without the subject's being aware of it.

As I have just said, matters are not so simple in practice, and what actually happens is that the activity of the repressed complex is manifested not directly but indirectly in some distorted form that is often hard to recognise. In the above example, for instance, the subject might have counterbalanced his real attitude towards his friend by developing an exaggerated solicitude for his welfare, and have shewn great concern and dread whenever the friend ran the slightest risk of accident or danger. Again, an abnormally strong emotion might be evoked by anything accidentally associated with the persons in question, a condition that Professor Morton Prince described some ten years ago under the name of "association neurosis." This distortion in the manifestation of the activity of the mental complex is often exceedingly involved, and one of the main difficulties in the psycho-analytic method is the unraveling of the confused end-product, which clinically we call a symptom. The psychological mechanisms by means of which the distortion is brought about are very intricate, so that in the time allotted it would be impossible for me to describe them. They have been worked out with great accuracy and detail by Freud and Jung, and an exact study of them is essential to the use of the psycho-analytic method.

Investigation on the lines presently to be indicated discloses the fact that every psycho-neurotic symptom is to be regarded as the symbolic expression of a submerged mental complex of the nature of a wish. The wish itself on account of its inacceptable nature is concealed, and the symptom arises as a compromise between it and the repressing force exerted by the main personality. The stream of feeling that characterises the wish is dammed up, it can find no direct outlet and so flows into some abnormal direction. The metaphor of "side-tracking" is, I believe, used in American psychiatric circles to indicate this process. In more technical

phraseology we may say that the effect of the original complex is inhibited, and so becomes transposed on to an indifferent mental process. This indifferent mental process has now become invested with the strength of feeling that properly belongs to the original complex, and so may be said to replace the complex. Thus arises what Professor Adolf Meyer calls a substitution neurosis, in which an abnormal outlet has been found for a pent-up affective process. The outlet may be in a purely mental direction, in which case we have such a symptom as a phobia, or towards various bodily processes, a condition that Freud calls conversion-hysteria, in which case we have such symptoms as a tremor or a paralysis. In the symptom the patient obtains a certain unconscious gratification of the suppressed wish, and this means of obtaining the gratification, however perverse and abnormal it may be, is still the only means possible to the patient under the circumstances. This fact explains the obstinacy with which such a patient may instinctively cling to his symptoms, and is one of the causes of the resistance that the physician encounters when trying to remove these. I need hardly remind you that the obstinacy is often erroneously interpreted even by physicians as indicating mere wilful perversity, a mistake that does not conduce to success in treatment. Not only does the observer commonly fail to understand the significance of the symptom, but the patient himself has no knowledge of its meaning or origin. In fact, *enabling the patient to discover and appreciate the significance of the mental process that manifests itself as a symptom is the central aim of the psycho-analytic method.*

In carrying out this method several procedures may be adopted according to circumstances. The hypnoic state, for instance, may be utilised in the search for forgotten memories. Only a very few of those acquainted with the psycho-analytic method employ this procedure at all extensively, for it has grave disadvantages which I need not here discuss. Personally I employ it only as a rare exception and for special reasons; under certain cir-

cumstances, however, it undoubtedly has a legitimate place. The procedure introduced and developed by Freud is the one most generally used, and gives by far the most satisfactory results. It is one of the ways of obtaining what is known in psychology as "free association," and is carried out by getting the patient to concentrate his mind on a given idea, generally one in relation to a symptom, and asking him to relate in the order of their appearance the various thoughts that come to his mind. It is essential for him to do this quite honestly, and fortunately we have several objective tests of his behaviour in this respect. He must suspend his natural tendency to criticize and direct the thoughts flowing in, and must therefore play a purely passive part during this stage. At first he will omit to mention a number of thoughts on the ground that they are apparently irrelevant, unimportant or nonsensical, and others because they are of a painful or unpleasant nature. After a time, however, the length of which largely depends on his intelligence and sincerity, he acquires the capacity of adopting the non-critical and passive attitude essential to success.

Other means of reaching buried mental complexes may briefly be mentioned. A study of various mannerisms, symptomatic movements and tricks of behaviour, and slips of the tongue and pen often reveals the automatic functioning of some repressed train of thought. The word-reaction association method as developed by Jung is of the highest assistance, particularly in furnishing us with a series of clues to serve as starting-points for future analyses. In this method a series of test-words are called out to the patient, who has to respond with the first word or thought thus called to his mind. From a general review of the kind of responses given much can be learnt about the mentality of the patient and the type of psychosis present. Further, by noting certain peculiarities in the individual reactions we may discover certain complexes or trains of thought that possess for the patient a high emotional value, and these

can then be followed and studied more fully. The peculiarities I refer to are ten or twelve in number. The chief are: Undue delay in the time of reaction, failure to respond at all, response by repetition of the test-word, perseveration affecting the succeeding reactions, anomalous clang associations, assimilation of the test-word in an unusual sense, and erroneous reproduction of the reaction when the memory for it is subsequently tested. Last but not least is the analysis of the patient's dreams by means of the special technique introduced by Freud. The study of dreams is in this connection of supreme importance, for of all the means at our disposal it is the one that best enables us to penetrate into and understand the most hidden parts of the mind. No one can have more than an outsider's notion of the psycho-analytic method who has not thoroughly studied Freud's *Traumdeutung*, for in this work he has laid down the technique of his methods, and discussed the principles on which they are based with a fulness to be found nowhere else in his writings.

By means of the methods just outlined we are enabled to determine the origin of the symptom by retracing the steps along which its pathogenesis proceeded. It is impossible to deal with the underlying complexes, to discharge their pent-up affect, to render them more assimilable by the patient, unless one succeeds in this task and brings them to the full light of day. The symptoms constitute a veiled language in which hidden thoughts and desires find the only means allowed them of coming to expression. We have to get the patient to translate his symptoms into more direct language, and thus to understand and appreciate the origin of them. In so doing we give the patient a deeper insight into the workings of his mind so that he is enabled to correct abnormal deviations, to overcome internal inhibitions and impediments, and to acquire a more objective standpoint towards the repressed mental complexes, the automatic functioning of which has produced the morbid manifestations. He is in this way able to free his per-

sonality from the constraining force of these complexes, and, by taking up an independent attitude towards them, to gain a degree of self-control over his aberrant thoughts and wishes that was previously impossible. The method is thus in almost every respect the reverse of treatment by suggestion, although several would-be critics have naively exposed their ignorance of the subject in maintaining that the successful results are produced by suggestion. In suggestion treatment the physician adds something to the patient's mind, confidence, belief, etc., and thus makes the patient more dependent on him. The psycho-analytic method does not add; it takes away something, namely inhibition. It enables the patient to disentangle confused mental processes, and, by giving him control over the disharmonies of his mind, leads him to develop a greater measure of self-reliance and independence. The training received by the patient is thus an educative one in the highest sense of the word, for he not only achieves a richer development of will-power and self-mastery, but acquires an understanding of his own mind which is of incalculable value for future prophylaxis. He grows both in capacity to know and in ability to do.

The conditions that lend themselves to psycho-analytic treatment comprise practically all forms of psycho-neurosis, the different types of hysteria, the phobias, obsessions, anxiety neuroses, and even certain kinds of sexual perversions. I shall refrain from relating any individual cases, for to do so would be only to weary you with the recital of a list of typical and atypical instances of these various conditions. It is further impossible for me to narrate any single instance of an analysis, for in every case the richness of material is so great that it would take several hours to give even an outline of the main points in the case.

The results obtained by the treatment, though by no means ideal, are yet very gratifying. They surpass those obtained by simpler methods in two chief respects, namely in permanence and in the prophylactic value

they have for the future. Although most symptoms can be removed by other methods, such as hypnotism, yet anyone who has devoted much time to the study of these cases knows how great is the tendency to relapse, to recurrence, and to the appearance of fresh groups of symptoms. Mild cases can indeed be not only alleviated but even cured by the simpler psycho-therapeutic measures, so that these all have their sphere of usefulness: severe cases, on the other hand, need a more radical treatment, an uprooting of the actual morbid agents. It is easy to understand how this must be so. Hypnotic and other suggestion acts merely by blocking the outward manifestation of the underlying pathogenetic idea. The idea itself persists, because it has not been reached and dealt with, and sooner or later it will again manifest itself either in the same direction or in some fresh one. The analogy of a tubercular, or better still of an actinomycotic, abscess occurs to me in this connection. If the suppurating sinus is forcibly plugged then the symptom of discharging pus is removed, but sooner or later the pent-up pus will find a vent in either the same or a fresh direction. Before satisfactory healing can take place the tension must be relieved by instituting free drainage for each pus pocket, and the more thoroughly the focus of the disease is dealt with the better will be the result.

A few words are now necessary on the clinical applicabilities and limitations of the method. It is a method that makes great demands on both physician and patient. Apart from technical knowledge the physician must evidently possess, not only unimpeachable integrity, but also a considerable measure of tact, patience and sympathetic understanding; without these qualifications he is unlikely to gain the patient's confidence to the requisite degree. The treatment further makes a great call on his time. Freud often finds it necessary to devote to a patient an hour a day for three years, but he acknowledges that the cases sent to him are generally of a very severe nature. In milder cases

one can achieve very satisfactory results in a few months, a fact to which I can fully attest from my own experience. The amount of time may appear excessive unless one remembers the hugeness of the task imposed, for in all cases the roots of the trouble go back to early childhood, and important modes of reactions have to be altered which have been fixed and stereotyped for many years. When we consider how much trouble and time frequently has to be expended in the orthopedic straightening of a deformed limb, we should not grudge the same to the far more intricate task of the ortho-psychic training of a deformed mind, especially when this results in converting an intolerable existence into a happy life, and a person paralysed by doubts, fears and suffering into an active and useful citizen.

The demands made on the patient are no less great. The results of the treatment will vary with the intelligence, courage, honesty and perseverance he shews. With stupid and quite uneducated patients relatively little can be done, so that happily we can most help those whose value to the world is greatest. Again, age sets a formidable barrier to our efforts. In old age, when the plasticity of the mind is diminished, far less can be done than at an earlier period, and furthermore the time necessary to trace back the erroneous mental reactions through so many years is naturally longer. Still I have had a few fairly satisfactory results even above the age of fifty.

It will be realised that the method is at present not one generally applicable by the practising physician. Not only is the time necessary for the treatment a great hindrance, but also a laborious special training is necessary before the technique of psycho-analysis can be acquired to an adequate extent. It is generally admitted that this demands three years' incessant practice, a good previous knowledge of neurology being assumed. Here, as elsewhere, therefore, good work exacts arduous labour, and there is no royal road to the art of

handling the most intricate and delicate machine we know of, the human mind.

You may now legitimately ask why I have taken up so much of your time by describing a mode of treatment which I acknowledge not many will have the opportunity to learn or to apply. My answer is a two-fold one. In the first place I am not one of those who hold that the general physician should be cut off from all advancing knowledge except that which he can immediately apply in his daily work. No physician can apply all methods of diagnosis and treatment, but it is surely well that he should at least be aware of the existence of them. I cannot believe that because a country practitioner is not expected to apply the Wassermann test in the diagnosis of syphilis, or to perform excision of the Gasserian ganglion for the relief of trigeminal neuralgia, it is therefore better for him not to know about such methods. In the second place I wish to contribute to the general effect that this symposium must have in bringing home to you in some degree the present unsatisfactory state of medical education so far as psychology is concerned, for this is the main cause of the helplessness of the medical profession against the very maladies that are the triumph of the quack, religious or otherwise. The sooner we honestly face the shameful but undeniable fact that unqualified empirics can relieve distressing affections in cases that have defied medical skill, can produce results where we fail, the sooner will this flagrant lack in our system of education be remedied, and the better will it be for the dignity and honour of the medical profession. While the present state of affairs lasts, in which most physicians are given not five minutes' training in psychology in the five years of their student life, and in which there is no teacher of clinical psychology in any University or Medical School in the country, our profession must submit to being the prey of the charlatan and the mock of the scoffer.

REMARKS ON A CASE OF COMPLETE AUTO-PSYCHIC AMNESIA.*

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The following case presents nothing remarkable from a casuistic point of view; it was a combination of hysterical fugue and complete auto-psychic amnesia, such as occurs commonly enough. Further, no detailed study of the case could be undertaken; my observations were confined to three interviews with the patient, of a couple of hours each. In spite of these facts a sufficient number of matters of interest was noted to make it seem worth while to utilise the case in illustrating a few of the simpler psychological mechanisms characteristic of hysteria. The actual interpretations offered of the various points must necessarily be of a tentative nature, but they are sufficiently in accord with the experience gained from extensive psycho-analyses to justify the pretension to a high degree of probability.

On the 20th of May, 1909, a man of about thirty walked into St. Michael's Hospital, Toronto, and complained of pain in an old appendicitis scar. On being asked his name he discovered that he had forgotten it, and, what was more, that he could give no account at all of himself or of his past life. He was at once admitted under the care of Dr. H. B. Anderson, to whom I am greatly indebted not only for the opportunity of observing the case, but also for kind permission to make use of it in this article. When I first saw the patient, on May 29th, his mental condition was as follows. He conversed clearly and intelligently. Though he was naturally bewildered at his situation his powers of attention, apperception and comprehension were quite

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intact. He gave evidence of having had a fair education, knew a little French and German, and spoke with a slight Irish accent. In short there was nothing noteworthy beyond his extraordinary lack of memory. This is perhaps best described by considering separately his memory relating to his own personality, and that to external matters.

Of his *personal or auto-psychic memories* only the following were present. He recollected having recently been on board a steamboat called the "Corona"—where, he knew not—and further had a dim remembrance of having been a good deal at sea. The latter fact we at once corroborated by finding on his body extensive tattooing, of the kind frequent among seamen. He also remembered having undergone an operation on his left elbow in the Boston City Hospital, about a year ago. Beyond this he knew practically nothing of himself. He could not tell us his name, address, profession or nationality, and could give no information about his family or his past life. The memory of one or two unimportant matters concerning his travels returned later, as will presently be mentioned. His amnesia for all auto-psychic processes was therefore complete. He had retained his somato-psychic memories, which in similar cases are often lost.

Of his *general memories* many disconnected fragments were present. He recalled the pictures of many seaport towns, with a few details about each. Thus, he said he had been to Hamburg, and, when asked if he knew the St. Pauli, smiled and answered, "Yes, it is in the gay district." He had been to Rouen, and, when asked if he recalled anything noteworthy about a bridge there, said "There is a high one we had to strike our masts to pass; it is a 'transporter.'" Capetown, he said, lay between Table Mountain on the left and the Devil's Peak on the right, as seen from the sea. He similarly mentioned Durban, Sydney, Cherbourg, and other ports. He dimly remembered having been in the Docks' district of London. When told he was in Toronto, he remarked that he had never been to Canada,

except on one occasion when his ship lay at Montreal. He did not know how long he had been in Toronto. The sight of a large departmental store through the window attracted his attention and brought to his mind the name J. C. Myers. He surmised that he must have had to do with some store of that name, but could not recall in what town. He could not remember whether he had ever been in Ireland, though it later came out that he had been born there and educated at St. Patrick's College, Cork. He caught sight of some praying beads in the hospital, and several Latin prayers automatically came to his lips. From this he concluded that he must at one time have been a Roman Catholic, though he felt sure he had not attended church for many years. Of Boston he could recall several streets, Boylston St., Beacon St., Tremont St., and knew the Common, though not the name of the pond in it. Of New York he recalled the Bowery, Chatham Square, the Battery Park and Central Park, and added that Brooklyn and Jersey City were both across the water. He dimly remembered taking part in an excursion from New York to Niagara with a crowd of about two hundred, but, except for one or two details of the town, nothing more. In reality, this journey had taken place some years before, and he could recall nothing of his visit to Niagara a week ago which had immediately preceded his coming to Toronto.

The "Corona" steamboat mentioned by the patient plies across Lake Ontario, so we asked him if he was an American. His answer was significant: "Yes, I guess I must be, for all your clothes look strange to me, and cut differently to those I am used to seeing." This led us to hope that indirect methods of questioning would succeed in restoring some of his memories where more direct methods failed, so as the first step in exploration we employed the "guessing" device. This consists, as is well known, in getting the patient to recall a given mental experience under the pretence that he is merely volunteering a guess, and is not being

expected actually to recall the experience as a personal memory.

The first application of this more than justified our expectations.

Q.—You say you can't remember whether you are married or not. Now, suppose you had to guess whether you are or not, which would you say?

A.—Well, if you put it in that way, I should say I was married, and have a baby, but I can't remember anything about a wife or a baby.

Q.—Not the wife's name?

A.—Not at all.

Q.—What sort of name would you give her, if you had to fit her with one?

A.—(Pause). I should think Annie; that comes easiest.

Q.—And the baby?

A.—Katie. (The correctness of both these answers was afterwards confirmed).

Q.—And your own name?

A.—Whenever I think about my own name the name Bert Wilson comes to my mind, but I am sure it is not mine. I can't remember my own name at all, except that I believe they call me Bert.

The last answer was so suggestive that it seemed legitimate to make the following speculations as being at least probable inferences from it; namely, that the patient's name was one resembling in sound Bert Wilson; that there was a real person called Bert Wilson, the memory of whom was playing an important part in the present symptoms, probably in the sense that the patient was for some reason unconsciously identifying himself with the other man; that, as he now recognised the falsity of that identification, the amnesia for his own personality was not likely to be profound, and would

probably yield to simple measures. It was also to be expected that when the memory of the true Bert Wilson returned it would do so only temporarily, and would again disappear for a longer or shorter period. The correctness of these seemingly fragile inferences was borne out by subsequent events.

As it was now probable that the patient had a young wife, who would be concerned about his absence, it became an urgent duty to try to recover his lost memories, particularly his name and address, as soon as possible. I, therefore, suggested to Dr. H. S. Hutchison*, who was present and in charge of the case, that we should facilitate this by inducing hypnosis. This was done, and after a little time the patient recalled that Myer's store (see above) was in Albany, and that he himself lived in that town. He could picture to himself his house, but not the number of it or the name of the street in which it was. On having his attention directed to neighboring streets, and particularly to the main ones, he slowly recalled that the one in which he lived was near Pearl Street. Then, by encouraging him to trace his steps from Myer's store along Pearl Street towards his home, one gradually got him more familiar with the neighbourhood, and, after an interval, he burst out with: "Williams Street, that's where I live, and that's my name, Richard Albert Williams."

With that came back a flood of memories from which we pieced together the following story. The patient had for many years been a sea-cook in British vessels, and for the past three or four years had been a *chef* at various places in New York State. He got married in May, 1908, and about two months later underwent an operation in Boston for a stiff elbow that had been badly injured in a railway accident. In the winter he was for some time ill with appendicitis, for which he also underwent an operation, and through which he lost

*I am greatly obliged to Dr. Hutchison for taking careful notes during this interview, and for rendering me every assistance, both then and later.

a great deal of work. In February, of the present year, he left his wife in Albany, and took up a post in Rome, N.Y. His wife bore a baby on March 12, and soon afterwards rejoined her husband. Early in April the restaurant where he was employed closed for the summer, and on April 15 he left Rome to seek work elsewhere. He failed to get any, partly because his equipment and clothes were needy; these rapidly deteriorated further or got sold. He travelled to Brockport, Utica, Syracuse, Rochester, Buffalo, Tonawanda, etc., tramping much of the way on foot. His small supply of money gave out, he had to sleep in the open, and got but little food; towards the end of the time he went for five days without any food whatever passing his lips. In addition to this physical stress he had to endure mental suffering of even greater severity, for he had left his wife with only money enough to last a couple of weeks, and these had now come to an end. He knew that she had no friends, was destitute in a strange place, and was not in a position to earn a living. He was exceedingly devoted to her, so that as time went by and his chances of getting work became more and more hopeless his anxiety reached an almost intolerable pitch. On May 17, when now in a very light-headed condition, he walked from Niagara to Lewiston, took the ferry to Queenstown and got on board the *Corona*. In Toronto he slept in a Workman's Home, and for three days wandered the streets seeking vainly for work, until the pain in his abdominal scar forced him to apply for relief at the hospital.

Before going any further it is desirable here to interpolate a few general observations concerning the production of hysterical symptoms. That these are the external expression of mental processes which have become split off from the main body of consciousness, in other words "disaggregated," has of course been known for over twenty years. The cause of this disaggregation, though equally well established, is less widely

recognised. It is commonly asserted to be due to the effect of psychical trauma acting on a mind congenitally unstable in certain respects. Those who hold this "traumatic" view would probably find no difficulty in explaining the case described above, and in attributing it to the action of the physical and mental stress just specified. This view undoubtedly contains a modicum of truth, but the incompleteness of it becomes more and more apparent the deeper we penetrate into the basis of the symptoms; the inadequacy of it in the case here described will presently be made evident. Closer investigation into the nature of the pathogenic mental processes demonstrates that there is always a very precise reason why they have become split off, or "disaggregated"; the defect in assimilation of these processes is due namely to the incompatibility of them with the main body of the personality. The patient cannot reconcile them with the rest of his mind because they are to him painful or unpleasant. He automatically strives to forget them, to submerge them, or, as it is technically called, to "repress" them (*Verdrängen.*)

From this point of view it is plain that every hysterical symptom essentially rests on a pathological amnesia, though in most cases not only the nature, but the very existence of the amnesia, is unknown both to the patient and to the physician. In some cases, as in the present one, the amnesia does not become converted into a symptom, but remains as such, and is manifest to all. This latter condition is not so simple, however, as might be supposed from these remarks, for the amnesia which is obvious, and which can usually be overcome by very simple measures, is a secondary phenomenon, being dependent on a deeper mental process, which has been still more profoundly forgotten. Even when we have reached this second group of mental processes it is only to find that it in its turn has been "repressed" and forgotten because of the action of a still deeper group, which is itself similarly conditioned. An hysterical symptom is thus seen to be built up by an

extensive series of amnesias, of different levels. If only the uppermost amnesia is removed it will readily recur, and the deeper the level reached in the analysis the less likely is the symptom to be reconstructed.

The treatment of a case such as the present one would consist in the following procedure. We ask why the patient wished to forget the memories in question, and we find it was because they are associated with other more painful thoughts he did not wish to recall. We then go on to ask why these other thoughts were too painful to recall, and we get a precisely similar answer, namely, because they are associated with yet deeper thoughts which he was still more desirous not to recall. We continue the investigation in the same way, constantly asking "why?" and continually penetrating deeper and deeper into the patient's mind, and reaching further and further back into his earliest memories. The pathogenic chain of associations is in this way traced to its original starting-point.

There was no opportunity of making any such analysis in the present case, but enough indications were present in connection with the terminal links in the chain to illustrate some of the mechanisms by which they were forged. The question with which we started was. "What motive had the patient for not wishing to know who he was and where he had come from?" or put in another way. "Why were his auto-psychic memories so painful to him?" The patient himself naturally wanted to recover these lost memories, but some conflicting motive for suppressing them was also struggling in his mind to gain expression, and this "repressed" wish had finally succeeded in attaining gratification.

A direct clue to these questions was obtained by innocently interposing in the conversation, which ensued on the patient's recovering his personal memories, the query, "Who is Bert Wilson?" He at once replied, "He was one of the cooks on board the *Louise*, the boat I went my first long voyage in." "What be-

came of him afterwards?" "I haven't heard anything of him since I was a boy. All I can remember of him now is that he was a darkey, and that in between his voyages he used to live with a white woman who kept a sailor's lodging-house in Shadwell, London."

At this point the reflection naturally arises that the patient's motive in "repressing" his personal memories might have been to escape from the unendurable situation in which he found himself. If we reconstruct his mental state at that period we might express it in the following artificially definite phrases. "Oh, if only I didn't have the frightful responsibility, to which I am not equal, of having a dependent wife. If only my wife could support herself without having to look to me. If only I could go away, as Bert Wilson used to on long voyages, and safely leave my wife, as he used to, in the knowledge that I should find her all right when I returned. If only I were like Bert Wilson." The passionate wish, although suppressed on account of the unmanliness and disloyalty it connoted, realised itself, as wishes so frequently do, in the belief that he really was Bert Wilson. I hold it very probable that some such process as here depicted actually occurred, though, like most interpretations of hysterical symptoms, it is merely part truth and is only a very incomplete explanation of the real events. We shall presently note, however, several observations that go to support the suggestion just made. The mechanism of *unconscious identification* (Freud's "*Identifizierung*") is exceedingly frequent in hysteria, and accounts for much of the so-called "imitation" of the symptoms of other patients. The unconscious fantasy fuses its own "repressed" wishes with the realisation of these wishes that occurs to someone else, and identifies the individual personality with that of the other person. In the present case it is likely that the similarity of the two names greatly facilitated the occurrence of the process.

Bert Wil—son.
Bert Wil—liams.

The significance of proper names to the personality varies considerably with different people, and is often very great. There was much evidence to shew that with the present patient this significance was unusually great. One instance may at once be mentioned; namely, he volunteered the statement that he loved his wife so much that he could not bear the thought of any other woman being called by her name, Annie. Two other statements made at the same interview go to strengthen the suggestion ventured above. First, he had as a boy greatly admired Bert Wilson, and had much envied him his access to his mistress on the convenient arrangement above referred to. Secondly, his journey to Toronto, where he knew no one and had no prospect of getting employment, had been suddenly determined on by his seeing a placard in Buffalo announcing that navigation was open on Lake Ontario. The picture of the steamship on the advertisement aroused his old longing for the sea as a means of escape from conditions he could no longer endure. In fact he had himself, as a boy of twelve, escaped from school by climbing through a window at night, and run away to sea.

My second interview with the patient was on May 31. He had fairly well retained his recovered memories, with one notable exception which will be mentioned in a moment. One of my first questions was, "Are you sure about your own name now?" He answered, "Oh, yes, Frederick Albert Williams." After a while he remembered that he had made a mistake and corrected the name to Richard Albert Williams. The origin of the mistake we shall come upon later. The most interesting feature of this interview, however, was the patient's absolute amnesia for the man Bert Wilson, an amnesia I had anticipated would probably occur, though I need hardly say that I let no inkling of this escape me which might act as a suggestive influence.

Q.—Tell me again about Bert Wilson?

A.—Wilson. You mean Jack Webb, don't you?

(Pause). Why don't you keep some fencing-foils or boxing-gloves here to pass away odd moments?

(We were in my consulting-room).

The unconscious deviating from a painful subject is very clearly seen here.

Q.—Who was Jack Webb?

A.—He was with me in the *Primeria* for a couple of years. We had a big fight, because I wouldn't stand his trying to boss the fo'c'stle, and licked him. (Evidently the reason for the combative suggestion made to me in his preceding answer.)

Q.—No, I mean Bert Wilson.

A.—Bert Wilson? You mean the fighter in New York. (Perseveration of the combative idea.) (Pause). No, I must be thinking of Bert Keyes. (Pause). I remember *Jack* Wilson; he was a schoolmate of mine and we ran away to sea together, but we went on different vessels and I have hardly seen him since. (Again the idea of "Wilson running away to sea," is in his mind, though in an innocent form; we also see now why the thought *Jack* Webb had come to him when he was asked about Bert Wilson—mediate clang association.)

Q.—The man I mean was coloured.

A.—That must be Frederick Stanley*. He was a fireman on the *Mary Thomas* boat out from Cardiff. He was a West Indian nigger.

Q.—No, the man I mean was a cook, not a fireman.

A.—The only cook I can think of whose name is like that is Bert Williams, a man of my own name. He's a *chef* in the Mansion House in T——.

Q.—No, that man is white. Bert Wilson was a cook, but he was coloured.

A.—That must be Frederick Kerr. He was the second steward on the *William Cliffe*.

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*The unimportant names I have for obvious reasons altered.

Q.—Who were the cooks on the *Louise?*

A.—Jimmy Macgregor was the first, and Jack Green the second.

The last questions were put in a leading way only after prolonged efforts, employed after inducing a hypnoidal state, had completely failed. In two hours I could evoke no memory whatever of Bert Wilson. One felt fairly sure that the first account of Wilson was correct, as was later substantiated. It is a safe rule when an hysterical patient gives two different stories to rely rather on the first, spontaneous one; the second is usually a product of subsequent "repression."†

In the third interview, on June 4, the patient at once recalled the names of the men mentioned on the previous occasion, but still had no knowledge of any Bert Wilson. He now stated, however, that on the *Louise* there had been besides the two cooks whose names he had before mentioned three other men in the galley; two of these were white, one the baker and butcher, and one whose duty it was to prepare the vegetables, and one was coloured, the scullery man. The name of the last man he did not know, but remembered that he "hailed from Dublin, and that he jumped (deserted) at Sydney."

At first he said he had never been to Shadwell and knew no one there, but after many efforts, aided by a map of Shadwell, he recalled the street in which the sailor's lodging-house was situate, and the name of the woman who kept it. He then volunteered that Frederick Stanley used to live with her when ashore. (We here get the probable explanation as to why the patient temporarily altered his name to Frederick in the preceding interview—further "identification" of himself with a man who could go to sea and leave his wife.) "But there was another darkey used to live with her (pause), he was scullery man on a boat with me (pause), running to Australia. He hailed from Dublin." After a

†Freud. Sammlung kleiner Schriften. 2e Folge. 1909. S. 11.

while he remembered that this boat was called the *Louise*. "I fancy he was one time fireman on the *Mary Thomas*"; (evidently now confounding Wilson and Stanley, the paramours of the same woman). He still could not recall the man's name, but from a written list of familiar and unfamiliar names he picked out that of Bert Wilson. Even now he hesitated, and seemed to think there was something wrong. "'Bert' is all right, but 'Wilson' seems to belong to Jack Wilson; when I think of the name 'Wilson' Jack comes into my head and fills it." After about half an hour's work on the point, however, the doubt was cleared up as follows. Bert Wilson was in fact the name of the negro scullery man on the *Louise*, but it was a false name he had taken after deserting from a ship so as to escape the penalties thus incurred.

Before again taking up the main theme of the "identification," we may shortly consider two little matters which illustrate some processes characteristic of hysteria. The first concerns what Freud terms "*Überdeterminierung*," that is to say, the convergent action of several factors to produce the same result. There may be two causative factors acting in the same direction, each of which may be unable alone to bring about the result, though the two succeed when they act in unison. The following is an instance of this. When the patient hesitated as to whether Bert Wilson was the correct name of the scullery-man I asked him whether any alternative name suggested itself. He slowly replied, "Perhaps Thomas. No, I am thinking of Captain Thomas, of the *Mary Thomas* boat, owned by Radcliffe and Thomas, and sailing from Cardiff." Now, in enquiring why the name Thomas occurred to him in this connection, we find at least two trains of association indirectly binding the name Thomas to that of Wilson, so that when the patient was in doubt about the latter name the former associated name suggested itself as a possible though incorrect alternative. The first train was that the *Mary Thomas* boat was the one on which

Frederick Stanley had served—the man whom we saw above he confounded first with himself and then with Bert Wilson. The second train reaches further; not only was there on the *Mary Thomas,* owned by Radcliffe and Thomas, a man who resembled Bert Wilson in the crucial respect of living with the same woman in London and under the same conditions, but another man, Captain Thomas, who bore more indirect relations to the name Wilson. Immediately after the remark quoted above the patient spontaneously continued: "There was another Captain Thomas, of Llanelly, who sailed on a boat belonging to *Williams* of Cardiff (the same seaport as the last), but I can't remember the name of the boat." He puzzled over this name and couldn't be got to leave the subject. I suggested the name "Sumatra,"* but he answered, "No, that was his first boat, I mean a later one." After a long pause the name flashed to him, "Gwalian." I asked him whether the name Gwalian reminded him of any other name, and he at once replied Gwilym. "Do you know the meaning of that?" "Yes, it is Welsh for *William.*" So that Captain Thomas had a double connection in his memory with the name Williams, which, as we have seen, was closely associated with the name Wilson.

These facts show the astounding network of associations that is unconsciously operative in the lower forms of mental processes, and also illustrate some of the ways in which one mental process gets linked to another. Thus, William—Gwilym (translation from one language to another; the identity form of Extrinsic Association)—Gwalian (Clang Association).

A very frequent occurrence in hysteria is that, of two groups of memories, it may be possible for the patient to recollect either at different times, but not both at the same time. Each of the two is harmless alone, but the two are incompatible because the relation be-

*It so happens that I knew the captain in question and his boat.

tween them is associated with deeper, painful memories. Thus the present patient had the greatest difficulty in retaining both the names Bert Williams and Bert Wilson. When he was first seen he knew the latter name; for a short time after hypnosis he knew both. When I next saw him he knew only the former.

The "repression" process also extends from the original memory on to harmless, but associated ones. It is a general rule in clinical psychology that an indirect, and apparently harmless, association is much more efficacious in evoking an external manifestation of a painful complex than is a direct association. Thus in the association-reaction test a word indirectly bearing on a painful subject is more likely to be accompanied by delayed reaction—time, etc., than one directly bearing on it; a man accused of theft is more likely to give an abnormal response to the word "left" than to the word "steal." In the present instance the patient reacted normally to the word Williams at a time when he could not recall the word Gwalian.

Another instance of this was given at the first interview. The patient could recall many facts about New York, even some particulars about his former address there, but when I asked him in what street was the Grand Central Station—which he had left to go to his later address—he could not remember.* On being persuaded to guess he suggested 24th Street.

This little example illustrates two common processes in hysteria. In the concealing of an unconscious complex the conscious manifestation frequently consists of the identical material of the complex, but in a distorted form, (Freud's *Verwendung desselben Materials*); when a word or a name in particular is being "repressed" the form that appears in consciousness is often composed of the identical letters of the word, in an altered order. It was no mere chance that the patient did not guess 95 or 37. Further, one of the commonest modes of this distortion is, as here, simply the

*The station is, as is well known, in 42nd Street.

reversal of the content of the complex. The reversal may be either in space or in time; it is also particularly frequent in normal dreams.

When I asked the patient to guess again he answered this time with 28 (24 + 4), then with 32 (24 + 4 + 4), then finally with 26 (24 + 2). The same play on the figures 4 and 2 is manifest throughout, 4 being naturally the more prominent of the two. It is perhaps also not without significance that the patient's address in New York was 4, Charles Street. Those who believe that the occurrence of these figures is a matter of mere chance are recommended to make a psychoanalysis of any figures which appear in their own dreams, or which they freely "choose" for any purpose without there being any reason why some figures should be chosen rather than others;† if they do this they will soon be convinced that the occurrence of "chance" figures is just as rigorously predetermined by previous mental processes as are all our "chance" thoughts. The same unconscious play goes on with figures as with words, in the lower forms of associative activity.

To continue the history of the case. After the return of the auto-psychic memories a number of other symptoms either developed or became more prominent, namely agoraphobia, auditory hallucinations, marked concentric contraction of the visual fields, reduction in visual acuity amounting in the evening almost to amaurosis, simultaneous micropsia with the left eye and macropsia with the right. His physical health was fairly good, especially towards the end of his stay in the hospital. We failed to find employment for him in or near Toronto, and as he said his prospects in Albany were good, we sent him there.

We may now summarise in the following way the "identification" theme developed above. The patient's unconscious fantasy had fixed his cowardly and "re-

†See Journal of Abnormal Psychology, Vol. III., p. 164; and Adler, Drei Psycho-Analysen von Zahleneinfällen und obsedirenden Zahlen. Psychiatr-Neur. Woch. 1.e. Jahrg. VII. S. 263.

pressed" wish, to escape from his difficult situation, with the old memories of a man whose life was the actual realisation of that wish. Bert Wilson was a man he had formerly envied, of the same profession and almost the same name as himself, who used to free himself of ties by going away to sea (as the patient had himself done when a boy), who could happily leave his wife to look after herself and would find her safe and prosperous whenever he returned, who had deserted from his duty, and had changed his name to escape from the responsibility of that desertion. The resemblance is certainly striking enough to influence a man in such desperate straits as was the patient.

Still it is evident that this process, intelligible enough as it may seem, would not have occurred in a normal person, however great the stress to which he had been submitted. We cannot, therefore, have more before us than the beginning of an explanation of the symptom under discussion. We have penetrated below the first layer, but we are only at the outset of the task of tracing the symptom back to its earliest origins. This task was for obvious reasons pursued hardly any further in this case, but a few indications were present to serve at all events as a clue to the next step in the analysis.

When I saw the patient for the second time, 57 hours after the first interview, one of my first remarks naturally was, "I suppose you have written to let your wife know you are all right." Not altogether to my surprise, he said, "No, haven't you done so?" He explained this by adding that "he thought the news would come better from a doctor, so that he had put off writing till he heard from her and knew that she was all right," though he had taken no steps to get any doctor to write. This lame excuse only added emphasis to the abnormal mode of reaction, which I think all will agree was certainly unnatural for a man who was deeply concerned about his wife and baby, as the patient sincerely was.

Still more noteworthy were the following facts, elicited in the same interview. When he left Rome, N.Y.,

on April 15 it was with the fullest intention of going to Albany, where he had good prospects of getting employment, and he was at a complete loss to explain why he turned westward to Brockport, and not east to Albany. Some abnormal, unconscious impulse was evidently guiding his movements even at that early period. But most remarkable of all in this connection was the next memory, which was recovered after very great efforts. When he was in Brockport he received a postcard, which was forwarded to him from his Rome address, offering him a situation for the summer at Saranac Lake. The salary was $90 a month, and it was specifically stated that his wife and child would be given free board and lodging. On getting this invitation, which was exactly what he wanted, he turned away from Rome and Saranac and proceeded to wander in the northwest part of the State looking for employment.

Light is thrown on these curious circumstances by a statement of the patient to the effect that all his life he had been subject to periodical attacks of what he called *Wanderlust*. He had not had an attack for a couple of years, but during the whole of March he had felt one coming on and progressively getting stronger and stronger. We thus have to do with some form of hysterical fugue, but, although several suggestive points concerning it were elicited, it would take us too far from the purpose of this article to enter on a discussion of this extensive subject. Enough, however, has perhaps been said to indicate the complexities to which even the partial elucidation of a single symptom leads, and to illustrate a few of the psychological mechanisms by which such symptoms are produced.

In conclusion, a word may be added on the different forms of amnesia. It used to be taught that memory depends on four processes, registration, conservation, reproduction, and localisation in time, and that an amnesia may be due to defective functioning of any one of these. The modern trend, on the other hand, is to look to reproduction as the source of every defect in memory.

It is obviously incorrect to call the result of defective registration an amnesia, for what has never been acquired cannot be lost, so that we are at once reduced to the other three. The fourth process, localisation in time, has long ago been shewn to be unnecessary for even perfect recollection of a given memory. The present case gave numerous instances of this fact. For example, the patient gave a detailed account of the public events concerned with the King's accession and coronation—his illness, etc.—but could not say whether it was before or after the Boer War, although he was in South Africa at the time of this; he gave the date of the accession as six years earlier than one he gave half an hour after for the death of the Queen.

Most forms of amnesia are usually classified under one or other of the sub-groups of either retrograde or continuous amnesia. The present case was, of course, mainly retrograde in type, but many instances of continuous amnesia also occurred. For example, on May 31 the patient went to Hamilton to apply for work in a certain hotel there, but in the evening of the same day he had quite forgotten the name of the hotel and also several important details of the excursion.

It is frequently assumed that the two main types of amnesia, retrograde and continuous, correspond respectively with defects of reproduction and of conservation. Thus Coriat* writes: "If the conservation of experiences is at fault, it is then impossible to have memory of any kind, because nothing is stored up. Impressions then are forgotten as fast as they are experienced, making what is termed a continuous amnesia." I must personally confess to the profoundest scepticism as to whether this latter process ever occurs, in other words as to whether there exists at all an amnesia due to a defect of conservation. The more carefully we investigate cases of continuous amnesia, the more do we find that they are due to defects not of conservation but of reproduction. It was easy to shew in

*The Journal of Abnormal Psychology, Vol. IV., P. 4.

the present case that the continuous and anterograde amnesias were of this nature, for by special devices all such forgotten memories could be recovered, and my belief is that theoretically, at all events, this is always possible. It is evident that no one has the right to speak of a conservation amnesia until he has excluded the possibility of the symptom being due to defective reproduction. The evidence is rapidly increasing which indicates that, provided apperception, and therefore registration, are sufficiently unimpaired, then the memories will be indefinitely conserved, and any apparent loss of them is really due to defective reproduction only. Such a view is, of course, very hopeful, for it encourages one to expect that with improved special technique cases of amnesia will always yield to treatment, provided that the mental functioning in general does not too greatly deviate from the normal. Corcket* recently reported a case of complete hysterical auto-psychic amnesia, more profound than in the instance here reported, which was unchanged at the time of writing, two years after the onset. We have every reason to expect that with increased knowledge and improved technique such a case would in the future be readily amenable to treatment.

*Corcket. Ann. Méd-Psychol. 1908. P. 37.

THE SIGNIFICANCE OF PHRICTOPATHIC SENSATION.*

By Ernest Jones, M.D., M.R.C.P. (London),

During the process of recovery from a hysterical anæsthesia a great variety of abnormal sensations may be experienced by the patient when the part in question is stimulated. Certain features characteristic of these sensations occur in such constant association that it seems justifiable to include them in a single group, which I have proposed to designate by the term phrictopathic (14). So far as I am aware the unity of the group has not previously been recognised, though, as will presently be shewn, indications of several of the individual features are to be found in the literature. A description will first be given of the six features characteristic of the symptom and then a discussion of its pathological significance. The symptom may be met with in all degrees of intensity, and naturally its characters are most easily to be studied in cases in which it is most marked. As the most marked instance of it I have observed was in a patient in the achiric stage of dyschiria, a syndrome I have elsewhere described (15), I shall refer particularly to this case at first and point out later the milder forms in which the symptom presents itself. The individual features of the sensation may now be considered in detail.

1. *Abnormal Persistence.*—Instead of the sensation ceasing to be experienced immediately the cutaneous stimulus is withdrawn, as it does in the normal, it here persists in unabated intensity for a variable time. In the case referred to above the sensation persisted for 50-60 seconds; the shortest period I have known it to persist has been—in other cases—six or eight seconds, and between these two extremes all gradations may be observed. The dying away of the sensation is a rapid process oc-

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cupying usually only a second or two. Many of the after-sensations which are well known sometimes to follow cutaneous stimulation in hysterical anæsthesia are probably of this nature. Binswanger (4) has described these "pathologischen Nachempfindungen" as "brennende, ätzende, kriebelnde Schmerzen," which might well pass as a description of the milder forms of phrictopathic sensation.

2. *Delayed Reaction Time.*—Any reaction time involving a conscious process is unduly delayed. An interval of between two and six seconds elapses before the stimulus is consciously appreciated. This fact has been noted in cases of allochiria by Bosc (6), Janet (10), and others. It will be remembered that, as was first demonstrated by Onanoff (20), reaction times in cases of hysteric hypoæsthesia are shorter than normal, provided the mental process is subconscious, as in a simple motor response.

3. *Non-perception When a More Normal Sensation is Present.*—I have not met with any previous account of this observation, which I believe to be of importance in connection with the theory of the symptom. Two sets of facts have to be distinguished according as the normal sensa on is evoked simultaneously with or subsequently to the application of the stimulus evoking the abnormal sensation; this may be illustrated by taking the case of the above patient who had achiria of the right limbs. (*a*) If stimuli were applied simultaneously to the right and left arms, only that applied to the latter was appreciated. (*b*) If the abnormal and long-lasting sensation was first evoked by stimulation of the right arm, and then, while he was still feeling this, a stimulus was applied to the left arm the abnormal sensation on the right side was instantly abolished.

Two further observations are necessary to complete the description of this point. In the first place sensibility in this patient's right arm was absolutely intact even to the most rigorous of the usual tests, so that although he could appreciate the lightest Von Frey hair when this

was applied alone he felt nothing at all in the right arm, however strong the stimulation, provided the left arm was simultaneously touched. In the second place the observations were perfectly constant, and in some thousands of tests I have never seen any exception to the above rule.

In a case of bilateral allochiria in which the defect in autosomatognostic feeling (see below) was more marked on one side, a stimulus applied to the more normal side acted precisely in the same way as a stimulus applied to the quite normal left side in the unilateral case; that is to say, it prevented sensation being evoked by a stimulus applied to the less normal side if applied simultaneously, and abolished it if applied subsequently.

4. *Tendency to Immediate Motor Response.*—The stimulus is followed by an instantaneous start which the patient is quite unable to prevent; this sudden jerk is so fatal and irresistible as to give the appearance of a direct reflex. This has also been noticed by Pitres in connection with a symptom called by him haphalgesia, which we shall presently have to discuss. He states (21) that immediately on experiencing the sensation in question the patient "se recule vivement." It is also met with in connection with what Head (9) has termed protopathic sensation. Like the latter, phrictopathic sensation is also very disagreeable, but there is no further resemblance between the two either in their other attributes or in their origin.

5. *Disagreeable Quality.*—This is the most striking feature of the group. The sensation is never one of pure pain. When only slight it is mainly one of "queerness," "unnaturalness," "strange disagreeableness"; when more pronounced it is one of unpleasant tingling or shuddering; when very marked this shuddering has an intense quality that can be described only as horrible. In other words, this peculiar quality of the sensation is always unpleasant, is often very disagreeable and sometimes constitutes a horrible shudder, on account of which feature I have described it as phrictopathic. The sensation is essentially diffuse and may radiate not only widely over

the limb stimulated, but even over the whole length of the body. In the achiric case referred to above it was most striking to witness the way in which the patient, sweating with horror, was relieved from his misery by the lightest touch applied to any part of the left side of his body.

Indications of this feature, though in a much less marked degree, are given in many cases described in the literature. Thus Janet, in a case of bilateral achiria, states (11) that cutaneous stimulation "provoque seulement une sensation vague de quelque chose de gênant, sans que le sujet sache ce que c'est. Ce n'est pas une douleur proprement dite, c'est surtout une sensation indistincte." Pitres (21) describes the sensation in haphalgesia as "une sensation indéfinissable de vibration chaude, plus désagréable assurément que celle que provoque du côté opposé le passage d'un fort courant faradique." Elsewhere (22) he describes it as "une sensation très vive de brûlure." Gilles de la Tourette (32) also described haphalgesia in one of his cases as "une sensation douloureuse qui ressemblait à une brûlure." Sollier has given a more detailed description of the quality in several of his cases. Thus in Observation V (25) he describes the return of sensibility in a previously anæsthetic limb as being accompanied by "des sensations de fourmillements, de crampes, de tiraillements, de brûlure, etc." Under like circumstances in Observation VI (26) the patient says "qu'elle a des fourmillements, et des élancements, des crampes, que ça tire," and (27) that "Cela me picote, me brûle comme si on m'avait frottée avec des orties"; on another occasion (28) she describes the sensation thus: "Ça tire; ça picote; j'ai des crampes et des brûlures en même temps; c'est comme des caoutchoucs; il y a quelque chose qui coule dans mes jambes; ça fourmille," etc. In connection with this case and also with that of Observation XVII (29) the tendency to immediate motor response is also fully described.

An observation I have frequently made is that the patient "tries to rub the touch off"; this no doubt is due

partly to the tendency to motor response, but mainly to the disagreeable irritating nature of the sensation. Sollier (27) remarked the same fact in one of his cases.

6. *Impairment of the Sense of Personal Ownership.*—The sensation gives to a patient a curious feeling that he usually describes as "being touched on some part that doesn't belong to me." Like the other attributes mentioned above this varies greatly in degree. When slight it feels as though the part touched is "strange"; "funny," "hardly belonging to me"; when it is more pronounced the part touched feels decidedly foreign, and when very marked the part touched is definitely repudiated by the patient as in any way belonging to him; it is then as though the information about the stimulus, which may be perfectly correct as regards its nature, position, etc., comes to him from nowhere. To give an instance of this, one of my patients touched on the forefinger would say "You are touching the back of some forefinger with a blunt pin; it isn't my finger and I have no idea where it is, but it causes an intensely disagreeable shudder to run all up one side of me."

We thus see that there are plentiful indications in the literature of similar attributes to those grouped here under the term phrictopathic. I have little doubt but that the symptom described by Pitres (23) under the name of haphalgesia is identical with phrictopathic sensation. Pitres, who at that time was unfortunately under the influence of Charcot's erroneous teaching on the subject of metallo-therapy, wished to indicate by this expression a disagreeable sensation that a patient experienced in an otherwise anæsthetic part when it was "stimulated" by contact with various precious metals. He described the symptom in several cases (24), certain metals being considered to be efficacious in some cases and other metals in others. The only references that I have found to the phenomenon besides those of de la Tourette just mentioned are in the writings of Janet (13), Loewenfeld (19), Bardonnet (2) Binswanger (5), Decoux (7), and Lannois (18). The latter describes the case of a

female tabetic with extensive anæsthesia of the left arm in which the symptom was produced by contact with copper only. All these authors seem to accept Pitres' description and explanation, though Janet would restrict it to only certain cases. It need hardly be said, however, that this supposed dependence of the symptom on contact with metals was due to the fact that at that time metals were thought to be powerful æsthesiogenic agents, the importance of suggestion in this connection not being recognised. As Dr. Jung once drily remarked, it is not only hysterics whose hands are excited by contact with precious metals.

We come now to the pathogenesis of phrictopathic sensation, but before we discuss this a few remarks are necessary concerning the two fundamentally different types of hysterical anæsthesia. These are best understood by considering for a moment the different sets of mental processes that have to do with a given part of the body in the normal. These are sharply divisible into two main groups. *First* there are the mental processes that depend on the incoming excitations flowing in from the bodily member at a given moment and which may be called the æsthesic* sensibilities. This group is composed of two subgroups, first the sets of common sensibilities (touch, pain, etc.) that have to do with the immediate relation of the member with the external world, and secondly the sensations, largely subconscious, which have their origin in the functioning of our internal organs; a good description of the latter subgroup, which is called cenesthesia, is given by Sollier (30) *Secondly* there is a group of mental processes that are essentially of the nature of memory feelings. All the mental processes of diverse origin that in the past have had to do with the member in question go to form this group. To enumerate only a few of these, there are the memories relating

*We seem compelled to employ this etymologically indefensible term, for the regular derivative formed from the Greek genitive has become so infused with meaning foreign to the sense here intended that its use here would tend to lead to confusion; to speak of "æsthetic sensibility" in the present connection might be ambiguous. We have the precedent of "cœnæsthesic" to support the use of the term "æsthesic."

to its functions, both motor and sensory, memories relating to its appearance, "sidedness," position, and all that the member stands for to the individual; in short, all the complex of memory feelings that may normally be aroused by the mention, touch, sight, or thought of the member in question. This second group might be thus described by the name autosomatognostic; in the past it has often been confused with the totally different group of cœnæsthesic sensibilities.

From the standpoint of pathology perhaps the most important difference between the æsthesic and autosomatognostic groups is the outstanding fact that whereas only the former can be affected by organic disease, either or both may be affected by functional disease. Affection of the second group is a form of amnesia, of either a retrograde or antero-retrograde type, and localised amnesia bearing on one set of memory processes is a form of psychical disaggregation highly characteristic of hysteria. On the other hand it is most striking that complete abrogation of all æsthetic sensibilities has no appreciable effect on the autosomatognostic group. Every nerve in the limb may be severed, the limb may even be amputated, and the patient still has the liveliest memory of what it feels like to have such a limb and enjoy its functions, and has a normal knowledge of all tha such a limb means to the individual; in the case of amputation in infancy such memories never form, but that is of course a different matter.

We thus see that in hysteria two entirely different forms of anæsthesia occur, according to whether only the sensibilities of the part or the sensibilities plus the memories of the part are dissociated. One important difference between these two anæsthesias concerns the amount of distress caused to the patient. With the ordinary hysteric anæsthesia in which only the first group is affected the direct result so far as the patient's well-being is concerned is almost negligible, and in fact is considerably less than the result of an anæsthesia of organic origin. This is strikingly illustrated in Janet's well-

known case (12) of a girl who came to the Pitié hospital complaining of the inconvenience she suffered from a patch of anæsthesia which was due to an injury of the median nerve. In the course of examination it was demonstrated, to the patient's surprise, that she was completely anæsthetic on the whole of the opposite half of the body. In fact the patients' ignorance of their anæsthetic stigmata was, as De Lancre mentioned three hundred years ago (17), widely known at a time when these were regarded as marks of the devil's claw, and the notorious frequency with which they are overlooked by the patient has led to the erroneous inference, by Babinski (1), Bernheim (3) and others, that they are usually artificially created by the observer.

In sharp contrast to the patients' ignorance of this form of anæsthesia is the great distress and misery suffered by patients who have in addition the rarer autosomatognostic defect. Autosomatognostic memories seem to be intimately connected with our feelings of capacity, adequacy and well-being, but above all with our sense of personality. In fact the condition in question is usually described by authors under the name of "partial depersonalisation" and the importance of it to the feeling of personality and well-being has frequently been noted. When it is very pronounced the loss of the sense of personal ownership of the part concerned is very striking. The patient may have so thoroughly forgotten what it feels like to have a given member that he becomes totally incapable of imagining the normal feeling in this respect or even of in any way understanding what is meant when it is referred to. Thus the patient of mine referred to above had so completely forgotten what it felt like to have a right arm that he did not understand one when one used the expression "right arm." He could as readily have imagined the feeling of a wing growing from his right shoulder as that of an arm, and would have felt as competent to use the one as the other; the amnesia for all that a right arm stood for was complete.

When now in this graver type of anæsthesia recovery comes about it often does so only gradually. In the majority of cases both groups of mental processes, *i. e.*, sensibility and autosomatognostic feeling are recovered synchronously. If however sensibility begins to return first then the dawn of sensation is a vague impression that something is being felt, but where, what, or how is to the patient a mystery. He has forgotten what such sensations mean and only slowly recovers the memories they normally evoke. He not only feels a touch on a part that is not accustomed to feel touches, but he knows nothing of this part even, as is shewn by the sense of foreignness it conveys. It is thus like feeling a touch with a part that has no previous mental history, a feeling that a normal individual can try to appreciate only by imagining that he were to experience a touch with some newly added part of a body, the nature, functions, shape and position of which were entirely unknown. The whole process is to the patient such a novel, strange and uncanny process as amply to account for the bizarre and disagreeable feeling experienced. An excellent descriptive account of these various feelings experienced on the "réveil de sensibilité" is given by Sollier (31).

This consideration seems to me to be of fundamental importance in relation to the problem of phrictopathic sensation. In the study of any case shewing this symptom one of the most striking facts observable is the close correlation between the intensity of the abnormal attributes above described and the depersonalisation or loss of the sense of personal ownership of the part concerned. If the patient above referred to came one day and told me that his right arm felt "funny," "strange" or "sleepy," I could be quite certain that the sensation elicited by a touch on the arm would persist for from eight to twelve seconds, that the delay in the reaction time would be two seconds, and that the disagreeable quality would be of only a moderate degree. If on the other hand it was found that such a sensation persisted for sixty seconds, I could predict that the patient would not describe his

right arm as "sleepy" or even as "dead," but that he would declare that he "hadn't a right arm" and that he could feel nothing whatever beyond the shoulder joint. The phrictopathic symptom is therefore intimately correlated with and probably dependent on what I have termed the autosomatognostic defect.

The essential point in the pathogenesis of phrictopathic sensation seems to me to be the occurring of a cleavage between the æsthesic sensibilities and the autosomatognostic memory feelings, and the intensity of the characteristic attributes to be a measure of the extent of this cleavage. This explains why in most cases of the severe type of anæsthesia, where the recovery of sensibility is only a little, if at all, in advance of the recovery of autosomatognostic feeling, the attributes are so elusive and faintly marked as almost to escape the observer's attention. In order that the cleavage should be pronounced it is necessary that the loss of autosomatognostic feeling should be profound, or, put in more modern language, it is necessary that the inhibiting or suppressing force of the underlying painful feeling-complex, which Freud (8) demonstrated thirteen years ago to be the cause of the resistance to becoming conscious that characterises disaggregated mental processes in hysteria, should be unusually great; this is so when the autosomatognostic feeling bears a peculiarly intimate association to the central complex, as I have been able to shew by the psycho-analysis of my cases. A further point worthy of remark is that the cleavage in question is usually, and perhaps always, accompanied by the presence of one or other of the dyschiric manifestations, which I have described in detail elsewhere (15).

Let us now briefly review the individual features of the symptom in the light of this hypothesis, keeping well in mind the difficulty with which the sensation has to contend before reaching consciousness, this being due to the resistance caused by the suppressed complex with which it is associated. The delayed reaction time is a direct consequence of this difficulty, and has been amply

demonstrated by Jung (16), in association experiments. to be characteristic of the stimulation of a submerged painful complex. The abnormal persistence of the sensation and the irresistible tendency to an immediate motor response are attributable to the uncontrollable, impulsive and automatic activity of a mental process associated with a complex that is unassimilable in consciousness, a well-known psychical phenomenon. The obviously defensive nature of the motor response is a typical instance of the resentment elicited by irritation of a painful feeling-complex, and is a measure of the resistance to the assimilation of this. The non-perception of the sensation in the presence of a rival normal sensation is similarly attributable to the difficulty with which the mental process contends in reaching consciousness; the resistance can be overcome only when no competing mental processes are present. The mental state of hystericals used to be described some years ago in terms of diminished power of attention; stated in this language we may say that the least opportunity to attend to some other normally experienced sensation is automatically seized upon and the painfully associated one is once more suppressed. The disagreeable quality of the sensation I would attribute directly to the amazingly strange and bizarre nature of the mental process, which is so indescribably foreign from any previously experienced by the patient. The impairment of the sense of personal ownership of the part stimulated is of course easily explicable by the defect in autosomatognostic feeling that is at the base of the whole phenomenon.

Conclusion.—Sensations showing the six features here grouped together under the designation phrictopathic are due to a cleavage between the æsthesic sensibilities and the autosomatognostic memory-feelings of a part of the body, which results from hysterical disaggregation implicating the latter group of mental processes; the degree to which the features are marked is an accurate measure of the extent of this cleavage.

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28. Ibid. Op. cit., p. 55.
29. Ibid. Op. cit., p. 260.
30. Ibid. Op. cit., pp. 491, 495, 496.
31. Ibid. Op. cit., Chapitre II.
32. Gilles de la Tourette. "Traité clinique de l'hystérie, Hystérie normale," Paris, 1891, p. 165.

THE DIFFERENTIAL DIAGNOSIS OF CEREBELLAR TUMOURS.*

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The problems relating to the diagnosis of intracranial tumours are matters no longer of theoretic interest only, but of urgent practical importance. This is so because in every case the question of operation, whether with a radical or a palliative object, arises, and in few conditions does success in treatment depend more closely on accuracy in diagnosis as it does here. In no condition so desperate as that of intracranial tumour have the results

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of treatment shewn in recent years a more marked and sustained progress, and at the present time they compare by no means unfavourably with those obtained in the case of intra-abdominal tumours. In order to secure still better results improvement is necessary on the part of both the diagnostician and the operator; it is only with the former possibility that this paper is concerned. It is very rare that a physician can attain to any high degree of skill at the same time in the niceties of neurological diagnosis and in the elaborate technique of intracranial surgery, so that collaboration is usually necessary between the neurologist and the surgeon. The surgeon demands that the diagnosis be made as *accurately* as possible, so that his task is thereby simplified, and as *early* as possible, so that operation may be undertaken while the patient's general health is yet unimpaired and before the tumour has extended so far as to render hopeless the attempt at removal. Intracranial tumours are sufficiently common to make it necessary to bear them in mind as a possible diagnosis in every case of nervous disease, as is illustrated by the fact that I have myself examined sixteen cases of this nature in the past twelve months only.

In the present paper I do not propose to describe any individual cases, but shall merely offer some general remarks concerning the differential diagnosis between tumours of the cerebellum and those elsewhere in the cranial cavity, paying most attention to the difficulties that are greatest in actual practice. These remarks are mainly based on a study of some twenty cases of cerebellar tumour I have observed in the past few years. Most of them were operated on.

It will save much repetition if I first give a short review of the symptoms that are most characteristic of cerebellar tumour, and then consider the differential points later. I will omit the indirect signs produced by pressure of the growth on neighboring structures. The *general* symptoms of intracranial tumour are usually very pronounced when this is situated in the cerebellum. The

headache, which is invariably confined to a sagittal plane, is severe, the optic neuritis is early and rapid in onset and intense in character, and the vomiting and vertigo are frequently very distressing. Beside the general feeling of giddiness and unsteadiness that may occur with any intracranial tumour we here meet with a special form of vertigo that consists in a peculiar sense of lateral rotation. On closer investigation we find that to the patient both his own body and external objects seem to be turning in the same direction—away from the side of the lesion.

The *attitude* and *gait* shew the following features: The head is held in a position of lateral flexion, the ear on the side of the lesion being approximated to the corresponding shoulder; the head is drawn backwards and is also rotated so that the face looks away from the side of the lesion. These three features make up what is called the cerebellar attitude. When standing, the patient is unsteady, though more so subjectively than objectively; the unsteadiness is not appreciably increased on shutting the eyes, so that Romberg's sign is absent. The patient stands with a broad base and rests his weight chiefly on the leg of the side opposite to the lesion. There is frequently present a considerable degree of lordosis. The gait has two special features: The patient reels as if drunk and tends to stagger and fall over on to the same side as the lesion. Again, in walking towards a given point he gradually deviates from the appointed direction, describing a path that is curved, with the concavity on the side of the lesion.

The *ataxia* of cerebellar disease is peculiar in being of the dynamic variety, thus differing from the static ataxia of, for instance, tabes or peripheral neuritis. It is, in other words, a dysmetria, being due, not to a lack of precise information from the periphery, as in these diseases, but to a defect in the central regulating mechanism of co-ordination. It is, therefore, not dependent on, and, indeed, is usually unaccompanied by, any sensory changes or any diminution in acuity of the "muscle

sense." The ataxia is always more marked in the upper than in the lower limb, and is usually confined to the homolateral side. It is manifested in several ways. During the performance of such an act as pointing to an object, or touching the tip of the nose, irregular incoordinate movements appear. Sometimes they have a tremor-like character, but they differ from an intention tremor, such as occurs in insular sclerosis, by not increasing towards the end of the act, and in disappearing as soon as this is completed. Indeed, a limb that is maintaining a fixed attitude, such as being held out straight, is held frequently preternaturally steady, a point to which we shall later refer. The carrying out of a complex movement frequently shews a defect that Babinski has termed "cerebellar asynergy." For instance, if the patient is told to straighten a lower limb that is flexed at both hip and knee-joints, he will extend first the leg and then the thigh, and not both simultaneously, as in the normal. Again, he is unable to carry out any rapidly reciprocating movements, a symptom known as dysdiadochokinesia. This also occurs on the same side as the lesion, so that, for instance, the patient cannot rotate his hand by the alternate movements of supination and pronation so rapidly on this side as he can on the opposite side, and he will frequently compensate for this incapacity by rotating the arm at the shoulder-joint when he is tested in this way.

A cardinal side of cerebellar disease is *hemiparesis*. This paresis altogether differs from that produced by cerebral disease, or by interference with the pyramidal tracts. It is distinguished by being more pronounced in the trunk than in the limbs, and in the upper than in the lower limbs; by never affecting the face; by being of a flaccid and not a spastic type, and by being unaccompanied by pain or any other sensory disturbance. Further, there are none of the changes in the reflexes so characteristic of pyramidal affections, such as ankle clonus, heightened activity of the deep reflexes, abolition of the abdominal reflexes, inversion of Mendel's

reflex, and the appearance of Babinski's plantar sign and its allies, the "fan" sign, Oppenheim's, Schäfer's and Remak's signs. The *hypotonia* that accompanies this paresis is a highly important differential sign. It is shewn by the diminished resistance to passive movements, by the flaccid feel of the muscles and the greater mobility of the joints. Special tests by which it can be revealed or estimated are: The extent to which passive hyperextension at the knee is possible (the knee-angle sign), the extent to which the seventh cervical spine descends below the level of the great trochanter when the patient tries to touch his toes without bending his knees, and by a useful test applied most conveniently with the forearm in the following way: If some one is powerfully flexing his forearm against resistance, and we suddenly remove this resistance, the forearm will flex to a certain extent and will then recoil. If cerebellar ataxia is present, then the forearm will continue to flex even to the maximum possible extent, and there will be no recoil. It is important to remember that the hypotonia of cerebellar disease differs from other forms in not being correlated with absent knee-jerks.

Of the *eye-symptoms,* three are of especial importance: nystagmus, sixth nerve weakness and skew deviation. Cerebellar nystagmus is characterised by being most marked when the patient looks towards the side of the lesion, in this respect differing from nystagmus of labyrinthine origin, by being of a slow and rather coarse kind, and in sometimes being more marked in the homolateral eye. It is practically always lateral in type. The paresis of the sixth nerve is an exceedingly frequent symptom; there is commonly also a dissociated paresis of the opposite internal rectus muscle so that it is weak in lateral movement of the two eyes, though not in convergence. In the latter case there may be a secondary conjugate deviation of the eyes away from the side of the lesion, a symptom of much greater significance than isolated affection of the sixth nerve. There are several kinds of skew deviation of the eyes. The commonest is

the Magendie type, in which the homolateral eye looks downwards and inwards and the contralateral eye outwards and slightly upwards. It is usually a temporary phenomenon and, therefore, has carefully to be watched for.

Two general characteristics of cerebellar tumours are further worth remembering, namely, the tendency of the symptoms to progress in definitely marked-off steps, and the continual variation in the activity of the deep reflexes.

If I were asked to place the above-mentioned signs in order of their diagnostic value, I should do so as follows: First, ataxia; then the characteristic vertigo, the hypotonia, paresis, nystagmus and skew deviation. With this clinical picture in mind we may next consider some of the problems of differential diagnosis that most frequently arise.

Of *supratentorial* tumours, those that give rise to the greatest difficulty in this respect are tumours of the frontal lobe, of the optic thalamus and of the corpora quadrigemina. It is uncommon for a cerebellar tumour to be confounded with one in the parietal, temporal or occipital lobe. It can be thought to be situated in the *Rolandic area* only if the observer mistakes for an attack of *petit mal* one of the giddy spells that occur in cerebellar disease and which sometimes gravely impair consciousness. In cerebellar attacks, however, there are never any local twitchings, as there are in the Jacksonian attacks; even in the true cerebellar fit there is no clonic stage, only a tonic condition of the muscles that lasts for a variable time. The subsequent paralyses are also of a totally different kind in the two affections.

A *parietal* tumour may occasionally be a source of embarrassment in diagnosis. It gives rise to a lack of dexterity in the limbs, which, however, is due to an astereognosis or sometimes to sensory asymbole, and so is quite different from cerebellar ataxia. When the tumour extends far back in the parietal lobe it may produce conjugate deviation of the eyes, but in such a case

there will probably be present a contralateral homonymous hemianopsia or else a mind-blindness for objects, together with evidence of visual aphasia.

Frontal tumours give rise to many symptoms resembling those of cerebellar tumours. In both cases there may be present nystagmus, conjugate deviation of the eyes, speech disturbances, unilateral tremor of the limbs, and even the so-called cerebellar attitude of the head. The conjugate deviation of the eyes in frontal tumour is, however, an irritative phenomenon, not, as in cerebellar tumour, a paralytic one, and the two can thus be readily distinguished. The speech disturbance is of a different kind, being aphemic and not dysarthric. The tremor has a different character, being very fine and rapid, and there is never present the typical cerebellar ataxia, though in making this observation care must be taken not to confuse the motor apraxia of frontal disease with the ataxia of cerebellar. Hemiparesis when present is on the opposite side and is of the cerebral type, with spasticity and the characteristic changes in the reflexes. Symptoms, such as anosmia, apraxia, bitemporal hemianopsia, agraphia and motor aphasia, may occur that are never found with cerebellar tumour; others, characteristic of cerebellar lesions, such as the typical gait, forced vertigo, hypotonia, lordosis and skew deviation of the eyes, are not found in cases of frontal tumour.

Tumours of the *optic thalamus* can very easily be mistaken for cerebellar tumours, and a case well illustrating this fact was recently reported by Dr. Heggie and myself to the Ontario Academy of Medicine. The hemiparesis is slight, and still more striking is the fact that it is hypotonic. Vertigo and the cerebellar attitude of the head may occur, and, further, the involuntary irregular movements that constitute a cardinal sign of thalamic lesions are sometimes very hard to distinguish from cerebellar ataxia. However, with tumours of the thalamus there are always pronounced sensory changes, particularly loss of deep sensation, and marked irritative

symptoms, such as pain and paræsthesia. Implication of the third nerve nucleus is common, most often manifesting itself as a bilateral mydriasis. Further, in all the cases of thalamic disease so far reported, there has been some implication, however slight, of the motor part of the internal capsule, so that changes in the reflexes indicative of a pyramidal affection are present. In the resulting hemiparesis the facial muscles are affected in a peculiar way, in that mimetic movements are more paralysed than volitional ones.

It is sometimes impossible to distinguish a tumour of the *corpora quadrigemina* from one of the cerebellum. A few points of value are that the deafness is an early symptom and not a late one, as in cerebellar disease; that it is on the side opposite to the tumour; that the affection of the third nerve, causing most often external ophthalmoplegia, is one of the first signs that hemianopia may occur, and that the ataxia and tremor are frequently bilateral. Paresis when present is frequently bilateral, and is always spastic, being due to implication of the pyramidal tracts.

The difficulties of distinguishing cerebellar tumours from other *subtentorial* tumours are even greater than in the case of supratentorial ones, and here accuracy in localisation is of vital importance because on it depends the operability of the case. Tumours of the *pons* and *medulla* are relatively easy to distinguish. The optic neuritis is late in appearing, and the general symptoms are not pronounced. Vertigo and ataxia may occur, but not of the cerebellar variety. The sphincters are frequently affected, and there are often vasomotor and respiratory disturbances. The paralysis may be on one or both sides of the body, but is always spastic and is accompanied by evidences of affection of the pyramidal tract, such as Babinski's plantar sign, etc. The lower cranial nerves are always affected, and in very characteristic ways. The paralysis of them is intense and permanent and is often bilateral. The nerves affected are grouped according to anatomical features. Bilateral

paralysis of conjugate movements of the eye is a frequent symptom, but not skew deviation or typical nystagmus. When the lowest group of cranial nerves is implicated, then there will be present a crossed paralysis of one of the four recognised types, named after Avellis, Hughlings Jackson, Schmidt and Tapia, respectively. The speech disorder cannot be told from that found with cerebellar disease.

Tumours of the angle between the pons and cerebellum, called the *cerebello-pontine angle*, are hardest to distinguish from cerebellar tumours. It is an important diagnosis to make, for, as Frazier has well shewn, the route of operation should be quite different in the two cases. Thanks to the observation of Holmes, Stewart and Weisenberg, we are now in a position to make the diagnosis in the majority of cases. Tumours of the cerebello-pontine angle are of two kinds. They grow either from the pia covering the under surface of the cerebellum, or more often from one of the middle group of the nerves in the posterior fossa, usually the eighth. They are most often fibromata, frequently with myxomatous degeneration, but sarcomata and endotheliomata are also met with here. They tend to press more on the middle peduncle of the cerebellum than on the pons, and hence clinically resemble cerebellar tumours more closely than pontine ones. The most important sign of tumours of the cerebello-pontine angle is the early and intense affection of cranial nerves. The seventh and eighth nerves are practically always paralysed, and frequently also the fifth, sixth and tenth. When the tumour presses on the cerebellum or its peduncle, then typical cerebellar ataxia and gait, homolateral paresis and hypotonia will occur. The paresis and hypotonia are, however, only very slight. A contralateral paresis due to pressure on the pyramidal tracts is common; this will, of course, be spastic and will shew the characteristic changes in the reflexes, such as Babinski's sign, etc. When this spastic hemiplegia is present the patient will in standing rest his weight on

the homolateral leg, and not, as in cerebellar tumour, on the contralateral. Two other valuable signs may be mentioned. *First*, a coarse tremor is frequently present in the homolateral arm when it is held out horizontally, whereas, as was mentioned above, this arm is held preternaturally steady in cerebellar tumour provided no hydrocephalus is present. *Secondly*, the feeling of subjective rotation is towards the side of the lesion, whereas in cerebellar tumour it is towards the opposite side.

Before concluding, I might add a few remarks on the important practical question of how to determine the side of the lesion once its site in the cerebellum is known. This is a matter that frequently causes considerable embarrassment, but the following points are of service in helping one to decide. The general rule is that every symptom of cerebellar tumour is either confined to, or most marked on, the same side as the lesion, but there are several fallacies of observation that have carefully to be guarded against. For instance, the patient as a rule tends to deviate towards the side of the lesion when asked to walk towards a given point. Later in the course of the disease, however, he becomes aware of this tendency and tries to counterbalance it. In so doing he advances with the shoulder on that side higher than and in front of its fellow, and we are thus able to detect the process. It may often be observed that on good days he actually over-compensates this defect and deviates towards the opposite side, a fact that easily leads to a false conclusion, whereas on bad days he deviates towards the side of the lesion. Again, although the symptoms of cerebellar tumour are as a rule paralytic in nature, yet occasionally they may be irritative and will then be in the reverse direction. The cerebellar attitude of the head is notoriously misleading in this respect, for when it is due to irritation the head is in the opposite position to the one that was described above in this connection. A similar remark applies to the direction of the skew deviation of the eyes. Further,

the increase of general intracranial pressure that is so great in these cases may give rise to false localising signs that are frequently on the opposite side to the lesion. As is well known, the paralysis of the sixth nerve is particularly unreliable in this respect, and I have several times seen it occur on the opposite side to the tumour. The signs on which one can most rely to determine the side of the lesion are as follows: The ataxia, hypotonia and cerebellar paresis are invariably most marked on, and often confined to, the side of the tumour. The movements of the nystagmus are slower and have a wider range in the direction of the lesion than in the opposite direction. The homolateral arm is held extended more steadily than the contralateral one. Lastly, when the patient is rotated in a chair and the movement is suddenly stopped, the sense of subjective rotation is less intense and the succeeding eye deviation and nystagmus are less marked when the chair has been rotated towards the affected side than towards the other. Here, however, as elsewhere in localisation diagnoses, care should be taken not to lay excessive stress on any single symptom, but to attach different standards of value to the different symptoms and then to make a diagnosis on the general clinical picture present.

THE PATHOLOGY OF GENERAL PARALYSIS.*

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Out of the vast subject of the pathology of general paralysis I propose to bring before your notice a few of the points that are of most general interest to the clinician, and that aid him both in clarifying his conception of the disease and in making an early and exact diagnosis of it. From the point of view of pathology the disease is in several respects one of peculiar interest, to the psychiatrist, to the practitioner and to the State. Of all mental diseases it is at the same time the most preventable and the least curable. Its occurrence could if thought desirable be avoided either by the individual or by the State. On the other hand, having once occurred it is absolutely refractory to treatment and leads to a fatal issue more rapidly and surely than almost any other form of insanity. This latter fact renders the early diagnosis of it a matter of high importance not only for the reputation of the physician in attendance, but also for the welfare of the patient's relatives and friends. Again, there are few nervous diseases and no mental diseases the pathology of which is better understood than that of general paralysis. The very fact that it has a specific anatomical substratum is, in itself, of great theoretic interest, and lends a striking and unfortunate support to the erroneous dogma that insanity is always due to disease of the brain. This dogma happens to be true in the particular affection of general paralysis, but in my opinion it would be most misleading to generalise it by applying it to all other forms of mental disorder. The existence of an anatomical substratum is important in two other respects.

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Through it general paralysis constitutes the main bridge connecting the domain of psychiatry to that of general medicine, and to it we largely owe the renewed earnestness with which the study of psychiatry has been undertaken in the past quarter of a century. Further, the existence of a sharply defined anatomical picture has enabled us to check our clinical diagnoses of the disease in a way that was previously impossible. This has been of the utmost value not only in clearly differentiating general paralysis from the various so-called forms of pseudo-paralysis, but in its educating effect clinically. There is no more salutary exercise for the clinical psychiatrist than to compare a series of his clinical diagnoses of general paralysis with the results of the microscopic examination of the same cases after death; whoever has not made this experiment would be surprised did he do so at the chastening effect it would have on his opinion of his clinical capacities.

As the *anatomical findings* are thus even for clinical purposes alone of great importance I will begin by giving a brief account of some of their most salient features. Outside the nervous system one finds, apart from definitely syphilitic lesions, atheroma of the aorta and atrophy of the heart, liver and kidney in a third of the cases. Little need be said of the changes in the nervous system except those in the cerebral cortex. In the peripheral nerves may be seen evidences of parenchymatous degeneration with some overgrowth of connective tissue, and, in the case of the optic nerve, of glia tissue. Neuro-retinitis is frequent, the retinal changes being of the same nature as the cortical ones. In nearly a half of the cases the membranes of the spinal cord are thickened and adherent, and there is visible shrinking of the posterior and lateral colunus. In nearly every case there is microscopic evidence of degeneration in the cord tracts, usually in both the pyramidal tract and the posterior ascending tracts; the latter changes are the more frequent of the two and are

similar in nature, though not in degree, to those present in tabes dorsalis.

The *macroscopic changes* found on opening the cranial cavity are thickening and adherence of the dura mater, especially along the middle line and anteriorly, the well-known "false membrane" between the dura and pia, with sometimes a hæmatoma in it, a tough, thick and adherent pia, which tears the cortex when an attempt is made to strip it, and an excess of turbid cerebro-spinal fluid, both on the surface and in the ventricles. The brain is atrophied, especially in the frontal region, where also the pia changes are most pronounced. The cortex is thin, opaque, hard and injected. The white matter is irregularly injected, and may shew patches of softening.

The *microscopic changes* may be divided into those that can be studied under the low power magnification and those that need a high one. They are all most marked in the frontal and Rolandic regions. Under the low power the most striking feature is the remarkable disorderly arrangement of the cortical nerve cells. The normal division into different layers is here disturbed to a degree found in no other disease. The atrophy and disappearance of considerable numbers of nerve-cells, sometimes of whole layers, also at once attracts the attention. An extraordinary number of blood-vessels are noticed, there being evidently present a great proliferation of them. With appropriate methods of staining can also be made out the disappearance of myelin-sheaths, which proceeds to a higher degree and in a more widespread manner than in any other disease, the secondary degeneration of nerve fibres, and the extensive overgrowth of glia tissue. The glia-increase takes place especially in the superficial layers of the cortex and along the vessel sheaths. The pial changes will be presently indicated.

Under the high power these processes can be studied in greater detail. The thickening of the *pia* is seen to be due, not to hyperplastic changes, as in alco-

holic, senile and arterio-sclerotic conditions, but to an enormous cellular infiltration. This infiltration differs from that which occurs in tertiary syphilitic lesions in being very diffuse and not distributed in foci or following the lines of the nerves and blood vessels, and in being exceedingly heterogeneous. Whereas in tertiary lues it is made up almost entirely of lymphocytes, in general paralysis it comprises all kinds of cells. Plasma cells of all ages may be seen, mast cells, lymphocytes, etc. The pial changes in lues are primary to the cerebral ones and the two can be closely correlated; this is not the case in general paralysis.

The changes in the *nerve cells* are very pronounced, but are not peculiar to this disease. Almost all kinds of degeneration may be seen, such as fuscous changes, vacuolation, tigrolysis, sclerosis, chromatolysis, as well as complete atrophy. In tertiary lues many swollen cells may be seen, but they do not disappear as a result of atrophy in the way they do in general paralysis. The degeneration of the nerve fibres has already been mentioned. It is greatest in the association tangential fibres of the cortex, but is also marked in the efferent projection fibres. In the senile psychoses degeneration of the myelin sheath occurs but there is no secondary degeneration of the axis cylinders, as in general paralysis.

The *glia* overgrowth is very diffuse. Characteristic is the presence of giant spider cells, and a rich formation of thick fibres which are attached in bundles to the blood-vessels. One never sees the plaques of fine unicellular fibres so characteristic of the senile psychoses.

The changes in the *blood-vessels* are of the greatest importance in making a differential diagnosis. The extensive new formation of vessels, which was previously referred to, takes place mainly by a process of budding from the walls of the old ones. It is found also in tertiary lues, and to a less extent in arterio-sclerosis, though in the latter case not apart from focal lesions, such as hæmorrhages. There is a great overgrowth of the intima with proliferation of the endothelial cells.

The thickened intima later becomes canalised, so that it may be impossible to determine which was the original lumen. The elastic tissue fibres are thickened and increased in number, changes that occur also in tertiary lues. The adventitial cells proliferate but do not reach the large size often seen in cases of lues. The adventitial lymph-spaces are enormously dilated, and are distended by a rich cellular infiltration. The chief constituents of this infiltration are plasma cells, lymphocytes and mast cells. Degenerative changes, which are mainly hyaline, are slight. They are found mostly in the small vessels near the surface, and chiefly occur towards the end of the disease. This contrasts with the advanced retrogressive changes found in arteriosclerosis and to a less extent in the alcoholic and senile psychoses. Retrogressive changes are also more pronounced in luetic arteritis than in general paralysis, and in that condition the artery often reverts to the embryonal state so that the three coats can no longer be distinguished. Further, the wall of the vessel in luetic endarteritis is not infiltrated with cells as it is in general paralysis, and in luetic meningo-encephalitis the infiltration of the vessel is only secondary to that of the pia.

A few words may be added concerning the cells that are most characteristic of the disease. The plasma cell, which is probably derived from the lymphocyte, is a fairly large cell with a thick nuclear membrane and metachromatic protoplasm, which gets lighter as the cell ages. Around the nucleus the protoplasm is lighter than at the periphery and is often tinted yellow. The cell frequently shews degeneration and vacuolation. Mitosis is rarely seen except in the pia. The cells are usually confined to the adventitial lymph space, and extend beyond this only in parts near an adherent pia or where there is intense infiltration. The same remark applies to the distribution of lymphocytes, but this differs in occurring more frequently in the wall of the large vessels, not, as plasma cells do, in that of the small ones. The mast cells are spherical or ovoid cells with coarse,

basophile granules and a large oval eccentrically situate nucleus, which is badly marked off from the surrounding protoplasm. They always occur discretely. Very characteristic are the rod-shaped or *Stäbchen* cells, which probably take their origin from the connective tissue cells of the adventitia. These are long cells with branching protoplasm, which frequently contains fat and pigment. The nucleus is often broken and shrunken. They lie attached to the outer surface of the adventitial wall of new blood vessels. They are occasionally seen in other diseases, particularly tertiary syphilis, when there is much new formation of vessels, but only in focal lesions. Even then they are short and atypical. In general paralysis they are exceedingly frequent and the distinguishing characteristics of the cell are evident to a degree never found in any other disease.

The anatomical picture, the outline of which I have here sketched, is perfectly distinctive of general paralysis, and by means of the post-mortem examination alone a trained observer can without hesitation decide whether a given patient had or had not suffered from this disease. The obvious importance of this fact both to theory and to practice need not be further insisted on.

We may next consider some of the pathological observations that may be made during the life of the patient. These are of great interest, not only as throwing much light on the pathogenesis of the disease, but also in that they enable us to make a certain diagnosis of its presence even in the earliest stages. Many of them, such as the hæmic leucocytosis which is so common, particularly after the seizures, are of no great diagnostic value, and I shall confine my remarks to the subject of the *cerebro-spinal fluid*. The technique of lumbar puncture is so well known that I will only offer a word or two concerning a few personal preferences which a considerable experience has dictated. The interval between the fourth and fifth lumbar spines is the most convenient one. The sitting posture is the most

advisable, but one must see to it that the patient lies down immediately after the operation and for the rest of the day. The amount of fluid withdrawn should never exceed ten c.c. at the most. The operation should never be performed if there is reason to suspect the presence of a tumour of the brain.

A great number of physical and chemical properties of the fluid may be investigated, but in routine practice it is only necessary to study three features, the pressure, the proteid content and the cells present The *pressure* is almost constantly raised in cases of general paralysis, frequently to four times the normal. The *proteid content* is always increased. The simplest way of determining this is by means of the ordinary Heller nitric acid test, applied as in urine testing. The extent of the increase can be fairly well gauged by the density of the ring formed at the junction of the two liquids, or by the time necessary for the formation of it when the fluid is diluted. The fact that this increase in the proteid content of the cerebro-spinal fluid is a constant feature in general paralysis was first demonstrated by Babcock of New York in 1896, though it was not until the publications of several French workers in 1903 that general attention was called to it. The proteid in normal cerebro-spinal fluid is probably globulin only, and it does not exceed half a gramme per litre. In general paralysis the amount is frequently four times this, and the proteid consists of both globulin and albumin, rather more of the former than of the latter. A matter of special theoretic interest is that the globulin is of a special kind, one of the varieties of euglobulin, and it has recently been shewn that it is this euglobulin which carries the peculiar Wassermann anti-body, of which we shall speak in a moment. Many methods have been employed for demonstrating this increase in euglobulin, but I will mention only the two which in my opinion are of the most service for this purpose. These were first described a few months ago, one by Noguchi, of New York, and the other by G. W. Ross, of Toronto, and

myself.* The former consists in adding half a c.c. of a 10 per cent. solution of butyric acid in normal salt solution to a fifth of a c.c. of the cerebro-spinal fluid, raising the mixture to boiling point, further adding a fifth of a c.c. of a 4 per cent. sodium hydrate solution, and again heating. Within a few minutes, if the reaction is positive, a coarse flocculent precipitate forms. The second test consists in gently pouring a little of the cerebro-spinal fluid on to a saturated solution of normal ammonium sulphate. When the reaction is positive a fine greyish-white ring appears at the junction of the two liquids.

In the normal cerebro-spinal fluid only three or four *cells* are found in a cubic millimetre, and these are exclusively lymphocytes. In general paralysis on the other hand there are commonly present forty or fifty cells to the cubic millimetre, and it is not rare to find several hundred. This cell increase occurs at the very outset of the disease, a fact of obvious importance for diagnostic purposes. Although the majority of the cells are lymphocytes, usually about two-thirds, still a great variety of other cells are also found, particularly endothelial cells, plasma cells, phagocytes and polymorphonuclear leucocytes. The increase is most marked in cases that run an acute course, and is greater, especially as regards the polymorphonuclear leucocytes, during the seizures that occur in the disease.

The three features just mentioned, the increase in pressure, in the globulin content and in number of lymphocytes present, are simple observations which can be made by any physician, and which in the majority of cases enable us to make a positive diagnosis of the disease.

I have now to speak of a far more complicated test, which has of late aroused wide-spread interest, the well-known *Wassermann sero-diagnostic reaction.* The credit for the laborious work done on this subject belongs as exclusively to the German school of psychiatry

*See British Medical Journal, May 8, 1909.

as that done on the simpler observations above-mentioned belongs to the French school. The amount of research that has been carried out on the Wassermann reaction may be indicated by the fact that within the past couple of years nearly three hundred papers, many of which are of the highest excellence, have appeared, incorporating conclusions drawn from the examination of several thousand cases.† The essential point of Wassermann's discovery was that the serum of a syphilitic, when mixed with an infusion made from syphilitic material, has the power of binding the "complement" present in normal blood serum. Complement is the substance which acts on various foreign substances, called "antigens," when in the presence of the corresponding "anti-bodies." For instance, normal complement-containing blood-serum will dissolve the red blood cells of another animal provided that an anti-body corresponding to those red blood cells is present. This anti-body has to be obtained by previously injecting the red blood cells of the second animal into the circulation of the first, this animal responding to the injection by pouring out anti-body into its blood stream. It is important to remember that the complement is a non-specific substance which can therefore act on a very large number of different foreign substances, or antigens, while the anti-body is a specific substance which can enable the complement to act only on the corresponding antigen in response to which it was formed. We can thus test whether the complement of a blood serum is bound or not by ascertaining whether it is or is not free to take part in a subsequent reaction. In the test in question, for instance, if the mixture of syphilitic material, which in this case constitutes the antigen, and the blood serum have bound the complement, then this is no longer free to dissolve any red blood corpuscles

†Those who wish to pursue the subject may be referred to a detailed review of it I have recently published in the American Journal of Insanity, April, 1909. A paper on the "Proteid Content of the Cerebro-spinal Fluid in General Paralysis," published in the Review of Neurology and Psychiatry, June, 1909, may also be mentioned.

subsequently added, in spite of the presence of the specific anti-body for these corpuscles. The test is carried out as follows: An emulsion of the syphilitic liver of a fœtus is made, and a fifth of a c.c. is added to an equal quantity of the fluid to be tested, together with a tenth of a c.c. of the complement-containing blood-serum of a normal guinea pig. The mixture is incubated for an hour or two, and is then added to a c.c. of a 5 per cent. saline emulsion of sheep's red blood corpuscles and to twice the amount of anti-body necessary to dissolve these corpuscles; this anti-body is obtained by taking the serum of a rabbit which has some time previously been twice injected with sheep's corpuscles. If the reaction is negative then the guinea pig complement, not being bound, is free to dissolve the red corpuscles; if the reaction is positive it cannot. A positive reaction, therefore, indicates that some substance, which we may provisionally term a syphilitic anti-body, was present in the fluid that was being tested, and bound the complement to the syphilitic antigen. Each of the five constituents of the reaction has independently to be tested by control reactions, and there are a large number of possible fallacies that I need not here enumerate. It will at once be evident that only a highly skilled expert can perform the test with any hope of securing adequate accuracy of results, and although several simplifications have recently been suggested it is very doubtful whether any modification of the original technique is satisfactory.

The clinical results are very striking and have been so widely confirmed as to be no longer open to doubt. They may summarily be stated in a sentence. The reaction is invariably positive with the blood-serum of patients suffering from general paralysis, and almost always with the cerebro-spinal fluid; it is commonly positive with the blood-serum of patients suffering from syphilis only, but only rarely with the cerebro-spinal fluid, even if the nervous system is implicated in the disease.

Wassermann's interpretation of the reaction has of late been discredited. It was simply that these peculiar substances found in the serum and fluid were specific syphilitic anti-bodies. It has been shewn, however, that the reaction can be obtained, though not so surely, if the syphilitic emulsion used in it is replaced by any one of a large number of other substances. The essential constituent in this antigen is of a lipoid nature, and is closely allied to lecithin. The substance in the serum or fluid, the so-called anti-body, is contained, as was mentioned above, in the euglobulin. It is present in small quantities in the normal, but is greatly increased in amount in syphilis and meta-syphilis. A great number of facts have been established concerning its physical and chemical properties, but nothing is definitely known of the origin or essential significance of it. Our present knowledge concerning the pathology of the reaction may be summed up as follows. Various antigens of a lipoid nature, which are present in especially large quantities in syphilitic organs, interact, probably as a colloid precipitation phenomenon, with some substance which is contained in euglobulin; the combination has the power of combining with complement, and thus of inhibiting hæmolysis.

If we now withdraw from these confusing details and with a broader perspective attempt to review them in their relation to the classical conceptions of general paralysis, we must at once be struck by the remarkable confirmation two of the most important of these conceptions thus receive. I refer to the specificity of the disease and to the close dependence of it on syphilis. General paralysis is in every sense of the word a *specific* disease. Even on the purely clinical side this has long been suspected. A disease which produces such an extraordinarily delicate lesion as that underlying the Argyll-Robertson pupil, a lesion so fine as to have escaped detection by the most exact methods of investigation, we must suppose to be due to some one constant agent, such as a complex toxin. This surmise reaches

almost certainty when we recall the highly peculiar histological picture I have above sketched, and further, the exceedingly characteristic changes in the cerebro-spinal fluid. It is unthinkable that such an elaborate and peculiar clinico-pathological picture should be produced by banal causes, such as sunstroke, trauma, worry, alcohol, etc., as is still frequently alleged. And when we search for the specific morbid agent that must be the cause of this elaborate picture our efforts are singularly rewarded. All the evidence, drawn from the most diverse sources, converges with fatal convincingness towards the one and only specific factor in the production of general paralysis, namely, syphilis. The amazing corresponding between the distribution of syphilis and that of general paralysis, in different countries, in different towns, in the different social classes and occupations, and between the age and sex incidence of the two diseases; the study of the inherited and conjugal cases; the incidence of syphilis in paralytics, as revealed not only by the history but by the physical signs, post mortem evidences, difficulty of inoculating the patients with syphilis, and the constant presence of the Wassermann syphilitic substances; the frequency with which patients with syphilis later develop general paralysis; all these considerations are in the fullest harmony with the pathological evidences I have above detailed, which demonstrate the essential relationship of general paralysis to syphilis. The subject, therefore, once more affords an illustration of the immediate practical value for clinical and preventive medicine of researches which, when viewed in too one-sided a way, appear to be abstract, dry and fruitless, and the results thus obtained should serve as an active stimulus to further and more profound investigations in these and allied realms of study.

AN ATTEMPT TO DEFINE THE TERMS USED IN CONNECTION WITH RIGHT-HANDEDNESS.*

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Any attempt to discuss the nature of the physiological features concerned in the problems of right and left-handedness is greatly hampered by the lack of precision in the nomenclature of the subject and by the number of different meanings with which most of the terms are at present invested. The object of the present note is to try to discover how far uniformity of meaning may be attained in the scientific use of these terms with the minimum of estrangement from the connotation in which they are currently employed. In a number of instances attributes supposed to be characteristic of various organs and functions of the body have been metaphorically applied to mental processes—we thus speak of a splenetic temperament, a hearty greeting, a short-sighted judgment, etc.—but in no sphere except in that of manual aptitude has this led to any noticeable confusion, the metaphorical nature of the expression being usually obvious.

The special capacity of the normal right hand, forming as it does a natural standard of aptitude, has given rise to a series of words etymologically based on this fact, but now only by metaphor connected therewith, and often used to denote intellectual or even moral superiority. Expressions built from the word "hand," such as, a handy implement, a right-hand assistant, a left-handed transaction, etc., give rise to little or no confusion, but the case is otherwise with those built from the Greek and Latin root "dexter." The latter includes a large number of terms, many of which are indispensable for certain purposes, and it is with these that

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we are here concerned. The current connotations attached to them are cited from Murray's Oxford English Dictionary, for this is by far the most complete work of the kind extant, and the New Century and other dictionaries contain no material that is not given there.

A. *Right-sidedness.*—If any adjective other than the simple English one of right-sided be required to denote this attribute, it should be the word "dexter." This adjective has been used in five different connotations, but all except that of right-sided are obsolete. Two other adjectives, "dextral" and "dexterous," are sometimes also used for this purpose, but this use of them seems objectionable, because not only is it superfluous but it tends to lead to confusion with other connotations of the same terms.

B. *The Special Capacity Characteristic of the Normal Right Hand.*—So far as I know there is no expression in use for the designation of this important and peculiar capacity. I would suggest that the term "dextrality" be used for this purpose,* for it is a rare word and is usually employed as an unnecessary synonym for the simple English expression right-handedness. "Dextral" would then signify precisely "pertaining to the excess of aptitude which one of the two hands, usually the right, normally acquires." The group of functions in respect to which this excess of aptitude is acquired may thus be called dextral functions. At present the adjective "dextral" is rarely used, and only to indicate "on the right side," a use which, as mentioned above, is entirely superfluous.† "Dextrally," which now means "to the right" would then signify "in a manner characteristic of the normal right hand."

*This definition of "dextrality" nearly resembles that given by Professor Baldwin in his *Mental Development in the Child and the Race*, 3rd ed., 1906, p. 56, where he suggests that the term be used to denote "the general fact of uneven-handedness."

†This older usage has been recently urged by Gould (*Right-handedness and Lefthandedness*, 1908, p. 206). He would speak of the dextral and sinistral hands as synonyms for the right and left hands, but does not mention any advantage to be gained thereby.

C. *General Adroitness comparable with Dextrality.*—This may refer to either manual skill or mental expertness, and the expression in question is clearly a metaphorical one. "Dextrous" (var. dexterous, dexterious), seems marked out by its characteristic plenary termination for use in this metaphorical sense of "full of right-handedness." The term has been used in seven different connotations. All these are obsolete except the one here indicated, though it is still sometimes used unnecessarily to signify right-handed. "Dextrously" is used as the corresponding adverb, though it also is occasionally used to signify "with the right hand." "Dextrousness" is obviously the corresponding substantive and should be confined to the metaphorical sense of indicating "an excess of aptitude over the normal in any given function to which it is applied, comparable with that excess over the left hand characteristic of the normal right." This, of course, is the current connotation of the word. "Dexterical" is sometimes used as a synonym for dextrous; it seems an unnecessary word unless it be found convenient to employ it to indicate manual adroitness as distinct from mental.

In the above account it will be noticed that definition of the commonest word of the group, "dexterity," is omitted. This word has been used in five different senses, the commonest being synonymous with dextrousness. It has entered so fully into common speech that it seems to me impossible to rescue it for any scientific purpose; it must, therefore, be recognised that it is a non-scientific expression used with no precise connotation.

Examples of the connotations above defined are best given by describing the common facts of right-handedness in the terms in question. Thus, in the normal person the dextral hand is the right, in a small number it is the left, and in a still smaller number both hands are dextral. All individuals can from this point of view be divided into three groups:

1. *Unidextrals.*—There are two subgroups (*a*) dextro-dextrals or right-handed individuals, dextrality being

confined to the right side, and (*b*) sinistro-dextrals, left-handed individuals.

2. *Ambidextrals.*—Dextral functions here are performed equally well on either side. Ambidextral is a more accurate designation for this purpose than ambidextrous, which should be used only to signify an exceptional aptitude on both sides; so also is ambidextrality a more precise term than ambidexterity.

3. *Bi-dextrals.*—Some dextral functions are performed best on the right side, others on the left. A typical instance is the average left-handed individual, who bowls better with the left hand but writes better with the right. Strictly speaking, the majority of mankind are bi-dextral, for it is exceptional to find a right-handed individual who does not perform some action better with the left hand. Still it is preferable to restrict the term to indicate only those individuals in whom the *important* dextral functions are best performed, some on one side and some on the other.

It is of course generally accepted that all individuals primarily belong to the first group, the second and third being the results of education.