

No. 5

April, 1910

Vol. III

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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The Bulletin
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NOTES OF A CASE OF CATATONIA.

BY HARVEY CLARE, M.D.,

Assistant Superintendent, Mimico Hospital for the Insane.

Family History.—His father died at 60 years of age. The cause of death was heart failure. Nothing further is known of him.

His mother is alive, aged 56 years; she has always been healthy.

The patient had five brothers, but as they do not live in this country nothing is known of them.

The patient had been married five years, and his wife had given birth to two children. One child is now two years of age and healthy, the other was still-born.

Personal History.—He was born in Pennsylvania of Swedish parentage on November 19th, 1877. Very little is known of his early life, except that when eight years of age he was kicked by a horse, and he told his wife that for many days his friends expected that he would not recover. A small depression of the outer table of the skull remains from this injury.

When a young man he began work as a barber and he was always very industrious. He was married when twenty-seven years of age, and since then he has been an abstainer from all intoxicating liquors, also from tobacco. His married life was a happy one, and he always took a keen interest in social affairs; he was fond of company and was highly respected by his neighbors.

Present Illness.—During the summer of 1909 the patient was apparently well, no change in his manner being noticed by his friends. He worked very hard and did not take any holidays. In September his wife said that he looked a little dull and tired. In the morning he wanted to rest, and his wife was compelled to get up and start the fires; this was very different from his former habits.

On October 2nd he came home from the shop in the forenoon, and his wife afterward learned that he had been so dull and listless that the foreman had ordered him to go home and rest. At this time he seemed absent-minded. His wife noticed him sitting quietly staring at one object for long periods of time, she also noticed that when stirring his tea with a spoon he continued the movement for a long time until his attention was attracted. About this time the patient complained of a numbness and tingling in his left hand.

On October 3rd he complained of inability to walk because his left leg was too weak to support his weight.

He went to bed, and on October 6th he voided urine in the bed, and the bowels moved without his paying any attention to the act. When spoken to he appeared very sorry, but said that he could not help it.

On October 9th he was removed from Cobalt to his home near Toronto. He walked out of the house and, with a little assistance, climbed into the carriage. During the journey on the train he was quiet, but observed everything that was going on. After reaching the home of his friends he was put to bed and rested well.

When seen by the writer on October 13th he was in bed, and was either unable or unwilling to give any information concerning himself. Urine was passed and the bowels moved involuntarily.

The patient told me his name in a low and mumbling fashion. He was lying on his back with his legs drawn up, his head was rotated to the left and any movement seemed to cause him great pain. I attempted several times to rotate his head to the right, but he resisted and

turned his whole body to the right; a few minutes later I saw him turn the head freely from right to left, and look at everything in the room. At this time any movement of the hands or feet showed a distinct, jerky, irregular tremor of the part moved; this was so pronounced as to resemble inco-ordination; there was also a fine tremor of the lips and cheeks. At this time the pupils were unequal in size, the left being the larger; they reacted normally to light.

When his left arm was raised above his head it remained in the position in which it was placed. The right arm when raised would immediately drop to his side. At first he would not take hold of the physician's hand, but later his grip could not be loosened; he held on until he raised himself to a sitting position. His expression was anxious and his face was drawn.

When a hand was placed on his arms or legs the muscles were rigid and there was considerable resistance to passive movement. Sensation was difficult to test, because the patient could not co-operate. Ophthalmoscopic examination revealed nothing. The triceps and patellar reflexes were much exaggerated on both sides of the body. There was no Babinski and no ankle clonus.

He was taking plenty of liquid nourishment and seemed to have no difficulty in swallowing, but from the first he would make no effort to feed himself; the nurse had to place his food in his mouth. His body was well nourished, his heart and lungs were normal, although the pulse was slightly accelerated. Examination of the urine revealed nothing.

The patient was admitted to the Hospital for Insane on October 19th, 1909. On admission the pulse was 79, respiration 28 and temperature 97 3-5. He refused all solid food, but took plenty of milk. He could not move his right arm or leg. He made no attempt to speak, he took no interest in his surroundings, and when his friends visited him he paid no attention to them. The left pupil was larger than the right. The tendon

reflexes were exaggerated. He gradually became weaker, his pulse more rapid and his respiration shorter until the hour of his death, which occurred about seventy-two hours after admission.

Diagnosis.—The diagnosis that was made in this case was the catatonic form of dementia præcox. This seemed at the time rather a doubtful diagnosis, and we began to ask ourselves, "Is it Hysteria?" "Is it some form of Paresis?" "Or is it Peripheral Neuritis complicated by Hysteria?" Again one might suspect some form of brain tumor because of the stupor, the inco-ordination and the loss of control of the deep reflexes, but we had to remember that we had no change in the retina, no headache and no vomiting, and we did have resistance to passive movement, indifference and negativism.

The diagnosis was confirmed by the post mortem examination, which was made by Dr. Ernest Jones.

DEMENTIA PRAECOX.

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Dementia Præcox is a psychosis of peculiar interest at the present time, constituting as it does the most complex and the most frequent form of mental disease.

The importance of an early diagnosis cannot be too fully emphasized, and the object of this paper, which deals chiefly with the general symptoms and prognosis, is to bring the physician in closer touch with the malady.

This disease is essentially one of the period of puberty and adolescence. It is characterized by a dementia that tends to progress, but which is frequently interrupted by remissions. The majority of cases occur between the ages of twenty and forty, though cases have been reported

even earlier than the fifteenth year and as late as the fiftieth.

The grouping of cases under this head has been described by Kraepelin. He took psychoses which were formerly known as disease entities as the catatonia of Kahlbaum, Hecker's Hebephrenia, and added the paranoic form to complete the group. To this classification other forms have been added. White, in his text-book, describes Simple Dementia, or Heboidophrenia, Hebephrenia, Catatonia, Paranoid forms, and Mixed Forms.

However, as Kraepelin has well said: "Any single pathogenic factor may make itself known by a great variety of symptoms." There is consequently little to be gained by efforts to differentiate too sharply between the various subdivisions, for many symptoms are common to all.

Aetiology.

Of all mental diseases Dementia Præcox is one in which heredity is the great factor. White states that every individual born in this world has, if it could be determined, a definite potentiality for development. The impetus which starts it on its path is sufficient to carry it a certain definite distance. The predetermined goal in each case will be reached if no accident intervenes to prevent.

In the subjects of this disease the original impetus has been weak, only sufficient to carry them a short way. When its force is spent development stops. Retrograde processes are hastened, or perhaps immediately initiated, by some special physical or mental stress occurring at the critical point of puberty or adolescent evolution.

If this is a true conception of the hereditary factor in these cases it is readily seen that it becomes important to search for debilitating influences in early life, such as excessive study, masturbation, etc. Again, conditions affecting the health and strength of the parents at the time of impregnation or during pregnancy, such as alcoholism and tuberculosis, etc., will be unfavorable

features. There is no form of alienation in which an intimate knowledge of the individual from the time of birth is of greater importance as an aid to diagnosis than dementia præcox.

It has been stated that we might expect to find the future patient rather dull in early youth. However, we frequently find the reverse to be the case. This fact, especially when there is also no serious taint in the antecedents, is thought to throw some doubt on the hereditary basis of the disease.

Recently a hypothesis has been promulgated concerning a possible toxic factor in the psychosis. As the disease is so closely associated with puberty, it was thought that some toxin originating in the testicle or ovary might produce these symptoms, after the analogy of the toxin of thyrodism. However, this is at present considered very doubtful.

General Symptomatology.

The prodromal symptoms usually extend over a period of years, during which time there is gradual mental decline. Attacks of migraine, particularly in girls, may precede or usher in the attack. Following this there is usually some mental depression.

One of the early and most important symptoms is the failure of voluntary attention. Jung, in his "Psychology of Dementia Præcox," says: "When the power of attention disappears, the perception of external objects, the perception of our own personality and judgment, the ideas of relationship, faith, and certitude disappear." From this lack of attention things in the environment are often not perceived at all; but, when perceived, they are usually understood. Consequently, these patients are well oriented in all three spheres—temporal, spacial, and personal—and show no evidence of clouding of consciousness. The attention is readily attracted, but it is only with the greatest difficulty that one can hold his continued interest. Sommer states that the patient places such an excessive value on his mental pictures that he can only with difficulty rid himself of them.

The lack of interest, carelessness, and indifference to dress and environment shown by these patients is also marked. On a superficial examination this is frequently not observed. For instance: If a patient under treatment in a hospital for the insane is questioned concerning his confinement, he probably will answer that it is a great injustice to deprive him of his liberty and say how anxious he is to leave. When it is pointed out to him, however, that he receives for nothing three meals and a good bed, he will often state that, after all, he is comfortable and quite satisfied. The listless, apathetic attitude towards affairs is only too apparent if the patient is kept under observation for any length of time. The power of recollection is usually impaired, particularly for recent events. When questioned concerning events occurring within the last few months, the patient has a very incomplete notion concerning them, unless they have occurred at a lucid interval or been particularly impressed upon him. About more remote events, however, such as childhood happenings, he is quite certain and definite. This failure in the power of recollection for recent events depends to a great extent on the desultory manner in which the patient appreciates events occurring around him.

From the inability to fix the attention, which results in defective registration, there is a lack of retentiveness. Jung remarks: "But just such a peculiarity shows what the nature of the memory is. It is nothing but a passive registration of events that take place in the nearest surroundings. All that requires an effort of attention passes by without heed by the patient, or at most is registered on a level with the daily visits of the doctors and dinner."

The emotional deterioration present is also very characteristic. This probably underlies and is responsible for the lack of interest shown. The expressions of joy and sorrow manifested are superficial, and of short duration. In other cases, however, there is a certain "stolidity." Information imparted to them that nor-

ually would be of great interest is received with striking indifference. Frequently patients make such statements as: "It is as though I see things through a veil, a mist, or through a wall, which separates me from reality. There is an enormous thickness preventing me from feeling moral impressions. I would like to try to think of my little girl, but I cannot. The thought of my child barely passes through my head, and does not leave any feeling."

In addition to this, the intellectual enfeeblement, indicated by the content of thought and shallowness of ideas, is readily perceived. Here comes in the importance of having the previous history of the patient and the normal intellectual capacity of the individual well established. In illustration of this enfeeblement is the total lack of insight that the patient shows into his own condition.

The physical symptoms of dementia præcox are multiform, but none are characteristic. The reflexes are frequently exaggerated, but, beyond this, they show no feature of importance.

The symptoms so far described occur in all three forms, but are more typical of the hebephrenic and catatonic varieties. This matter, however, will be more fully exemplified in the discussion under the different subdivisions.

Hebephrenia.

The prodromal symptoms of this disease frequently extend over months, and even years. In this type we have a chronic, slowly progressive form of dementia, with few evidences of negativism, stereotypy, motor excitement, or impulsivity. The mental apathy and progressive dementia are the prominent symptoms.

If a careful history is obtained of the onset, it is generally found that the patient has for years shown definite symptoms of alienation, such as eccentricities of character. The friends will say that he was seclusive, jealous, quick-tempered, and often careless as to personal appearance. At or about puberty these symptoms are

exaggerated, and the eccentricities of character become more pronounced. However, it frequently happens that the precocious dement of this class undergoes rapid intellectual development, but shows capriciousness and emotional instability. He is given to brooding at times over subjects that do not interest children of his age, and is thought by his friends to be singularly unconventional. The seclusiveness is frequently the result of the vague suspicions the patient entertains. He imagines people to be talking in an uncomplimentary manner about him, and that people do not care for his society. The delusions and hallucinations are usually of this unpleasant type. In the milder cases the fallacious sense perceptions are lacking in vividness, but are of a very grotesque character. Visual hallucinations are said to be more common in this form than in the others. There is marked intolerance for alcohol. Intermissions rarely occur, the patient progressing to chronic dementia. The prognosis is accordingly very poor.

Hebephrenia. Male. Aged 25. W. R. Admitted, May 11, 1909.

Family History.—Both parents of very neurotic type. One uncle insane. Alcoholic history negative.

Personal History.—Early history somewhat indefinite. Was always regarded as a very self-willed, stubborn child. Had few companions, and since 14 years of age has never entered sports or taken active part in things which naturally interest boys. Patient was somewhat backward at school. Left school at 16, and subsequently followed many different occupations, seldom remaining long in any one place. Read considerably, and regarded by family as quite a philosopher. Such matters as the "divinity of Christ" have caused a great deal of speculation on his part. Has been a persistent onanist. Several previous attacks.

Mental Status.—Patient is well oriented. Attention is hard to gain. Occasionally patient did not answer at

all, but lapsed into a brown study, and either repeated examiner's questions in a pondering sort of fashion or would say: "How is that, doctor?" Spends a great deal of his time staring into vacancy, with expressionless features. Emotional reaction is of a very superficial type. He followed a request for a razor to commit suicide by a vacant smile and the remark: "I guess it is too much of a speculation."

Delusions are elaborated throughout the examination, *i. e.*, he believes that a girl across the street, whom he does not know personally, was married to him mentally, and is determined to have an actual marriage in order that her good name shall not suffer. Auditory hallucinations apparent when he states that he frequently hears her talking.

His comprehension of situation, apart from delusions spoken of, appears to be fairly good, though he says himself that his mind feels at a complete standstill.

Physical Examination.—Negative.

Note.—January 2, 1910: Patient is very quiet, lounging about the ward most of his time. Is very forgetful. When asked to assist with any work will walk up and down a few times, then stand still until request is repeated. There is seemingly a gradual increasing mental reduction.

Catatonia.

This form is characterized by cyclic alternating periods of depression and excitement, with motor disturbances, stupor, and confusion. The relative prominence of these symptoms varies considerably. Hysterical attacks, with epileptiform convulsions, are common.

In catatonic stupor the principal symptoms are stupor, negativism, and excessive muscular tension. The negativism is manifested in various ways. The patient refuses to eat, pays no attention to the calls of nature, and every request is met by a response that diametrically opposes to the desired act. Attempts to move his limbs meet marked resistance.

In other cases quite the reverse of this picture is seen. There is a condition of marked flexibility. The limbs may be placed in any position without resistance, and will be maintained there indefinitely, "flexibilitas cerea." Moreover, pain stimuli are not met by any response, *i. e.*, the conjunctiva may be touched without reaction.

In the stage of catatonic excitement there is an increased psychomotor discharge. The patient is noisy, talkative, and active, but the actions are not consistently directed to an end. Impulsivity is a prominent symptom, and attempts at violence are usually the results of this feature. Quick and intense likes and dislikes occur with rapid changes from one to another. This illustrates the emotional instability which is characteristic of the excited periods. Verbigeration, or the constant repetition of the same usually senseless phrases, occurs in the later stages of the disease. Mannerisms are very common.

Catonia. Female. Aged 18. Admitted, February 16, 1909.

Family History. — Father pronounced alcoholic. Mother of very nervous temperament. History of alcoholism in maternal antecedents.

Personal History.—Patient was a bright, strong, healthy child. Progressed at school exceptionally well, nearly always heading her class. Attended collegiate two years, but latterly failed in all departments of her work.

After leaving school took situation as a nurse girl, but was found to neglect her work. Required to be told what to do over and over again, and was found to be so much trouble that she was discharged. Then accepted two or three positions of a similar character, with a like result.

Patient then attended a business college, but did not get along at all well. Teachers stated that she seemed

capable, but never seemed able to apply herself or concentrate her attention. Then took up elocution and music, but only for a short time. This flying from one thing to another, accomplishing nothing, was very characteristic at that time.

In 1907 a change in patient's disposition was first noticed. Mother states she would sit, quietly gazing into space, seeing nothing, and seemingly thinking of nothing. This was in great contradistinction to her usual manner, which was exceptionally bright and active. In 1908 this change was more marked. Patient developed grandiose ideas, believing she was superior to most people. Would fabricate in the most fanciful manner. Judgment greatly impaired. Became careless of her personal appearance, whereas formerly she was very neat.

Definite onset was very acute. In the evening of February 14, 1909, suddenly grew hysterical. Physician called and hypnotic given. Following day quieter. Patient, on seeing her mother, exclaimed: "Oh, mother! this will be so hard for you! God wants me while I am good, but the devil wants to keep me until I am bad." She explained this by saying that someone had given her a hypnotic, and had sexual intercourse with her; that she was now pregnant, and God wanted her before the baby came. This was followed by a stage of depression, which lasted until May, 1909. She was at first very negativistic, refusing to eat or speak. Later showed marked suggestibility, a condition of *flexibilitas cerea* occurring, which lasted two or three weeks. During the whole of this time patient was very dull and confused. Was constantly afraid of being turned into some animal. Responded to simple requests, but slowly, as if after labored thinking. Patient's whole reaction was childish in the extreme.

In June became much brighter, conversed freely about her past life, and took an active interest in her surroundings. This was followed by a period of pronounced excitement, which lasted until December, 1909. During this time suffered many hysterical attacks, par-

ticularly at the beginning. All interest in work diminished, until it was given up entirely. Patient became talkative, noisy, and violent.

Mental Status.—Orientation is well preserved in all three spheres. Patient shows no insight into her own condition. There is a marked intellectual and emotional deterioration, with considerable euphoria. Patient at present shows a gradual increasing mental reduction, but is bright and industrious.

Paranoid Form.—Delusions of persecution, which are not well systematized, occurring in conjunction with marked intellectual impairment, are the diagnostic features of this form.

The delusional system is loosely organized, and consequently the stories told by the patient are illogical and lack continuity. Grandiose ideas are common. The dement believes himself to be a king, and frequently attributes his persecutions to envy. Auditory and visual hallucinations, the former predominating, exist in almost all cases. The patient complained that he is chased by devils, that people are talking about murdering him, etc. Not infrequently hypochondriacal symptoms are present. The patient, when asked why he does not work, explains that he is unable, that his heart is displaced, that he has no stomach, and cannot digest his food.

In the early stages catatonic symptoms frequently occur, such as excitement, rigidity, negativism, etc. The patients are often excessively capricious, full of fads, crotchety, inconsistent and irritable. Dementia, however, is very slow in coming, and it is found that after a time they get more or less accustomed to their delusions, and consequently more settled.

The following case well illustrates the mental symptoms of a case of paranoid dementia præcox:

Admitted to hospital, December 15, 1896. Male, aged 34, single.

Family History.—Mother nervous. Rest of family history negative.

Personal History.—Early history of patient somewhat indefinite. Although said to have been unusually bright at school and college, and always ranking well in his favorite studies, he showed no aptitude for others. Was a close and hard-working student. Manner diffident, disposition retiring. While abroad in 1892 he broke down from overwork, and was in a hospital for some time, undergoing treatment. Was brought home in 1894. While living at home he worked for some time in a desultory way, was very reticent, secluded himself from others, was inclined to take violent dislikes towards members of his family, but was never violent. became suspicious of his friends without cause, and refused to be controlled. Came to the hospital willingly when told he had been committed. Upon admission he was quiet and reticent, answered questions in monosyllables, but quite coherently, and said he would remain quietly here.

Physical condition.—Spare in flesh. Movements nervous and awkward, head ill-shapen, forehead flat. Complained of dyspepsia of the intestinal type. Remained in hospital until January 10, 1896, when he was discharged, and left for home. During stay at home he was diffident and seclusive, ate and slept fairly well, improved somewhat in weight and strength. Several attempts were made to employ him, but he showed no power of attention.

Re-admitted December 15, 1896. Condition about the same as when in hospital, except that he had gained somewhat in flesh and strength. During 1897 his general condition remained about the same. Returned to the hospital, and was allowed to go home for a day or two at a time. Expressed the delusion that a battery was being worked on him. Once he left the dinner-table suddenly, tipping over his chair in doing so. When asked why he had done so, he said that a galvanic battery had been applied to him, and that the doctors knew all about it. Occasionally was somewhat agitated and markedly discontented, and often asked to be allowed to

go home. At other times would sit for long periods, staring vacantly into space. Ate and slept well. Would read newspapers and medical journals to some extent, but gradually lost interest. At one time helped with the urinary analyses in the laboratory, but was erratic in the work and not to be depended upon. During 1898 kept a great deal to his room, came late to meals, was easily disturbed by the noises in the adjoining room, moved away from people because he thought "they wish to say things they do not want me to hear." Swore considerably, sometimes at the doctor. Always late in going out for exercise, irritable, seldom smoked. He thought the nurses were trained to keep food away from him. Tiptoed around in a suspicious manner; thought he heard noises coming from the registers and these frightened him. Complained of chirping of birds and kept his window closed. Was apt to wander from the walking-party "to look for bones of dead animals." One day he became impulsive, and without provocation threw a cup at a nurse. Very slow in dressing. Would pick up his collar and shirt and blow them off as if trying to get them clean.

In 1901 he exhibited the following peculiarities: Excessive washing of hands. Would bathe from one to two hours. His manner and position were decidedly awkward. Would not look at observer straight in the eye, simply glanced at him and then immediately looked away as if embarrassed. Replied to questions put to him in as few words as possible. Did not volunteer any information. Objected to examination on the grounds that this procedure, as conducted by the doctors, was unfair and that the information thus obtained might be used to identify him if he escaped. He said the doctors maligned him and made fun of his gait and other peculiarities. Very suspicious and thought that people were trying to injure him and accounted in this way for his confinement in the institution.

Physical Examination.—No defect in speech. Thorax shows nothing remarkably abnormal. Reflexes

slightly exaggerated. Memory for past events accurate and correct. Mental reactions quick. Patient has an exaggerated sense of modesty. Orientation normal. Has a number of auditory delusions.

December, 1903. The mental reduction is gradually becoming more pronounced.

Prognosis.—The hebephrenic form is distinctive in that it leads slowly but progressively to complete dementia. Consequently the outlook is almost hopeless.

The paranoid form is the slowest in evolution and runs the most chronic course, but these patients do not get well.

The prognosis in catatonic cases is by far the best as it is in this form that remissions, which may last for years, especially occur.

Krapelin states that 8 per cent. of hebephrenics and 13 per cent. of catatonics make practical recoveries. He probably means by these estimates patients in whose future life nothing abnormal would be noticed.

Other observers state that 35 to 40 per cent. make partial recoveries, but many of these cases relapse.

SOME ASPECTS OF THE SERUM DIAGNOSIS OF SYPHILIS.*

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Since the appearance of the original communication of Wassermann, Neisser, and Brück (1) announcing a new method of syphilis diagnosis the medical literature both in Europe and in America has been literally flooded with papers dealing with this subject, and it is highly

*Read before the Section in Pathology of the Buffalo Academy of Medicine, November 18, 1909. Published in The New York Medical Journal, January 29, 1910. P. 221.

probable that no other single topic has received so much consideration from so many workers in the various fields of medicine, and a very great deal of this work has been of the most careful and painstaking character. While the subject is a relatively narrow one the significance of a luetic infection is such that every aspect of the question of serum diagnosis has been subjected to the most careful scrutiny and has been investigated from both the clinical and experimental points of view, and now, after over three years, certain facts have been definitely determined. With these facts I purpose dealing in a general way to-right, confining myself first to a general outline of various methods with a critical analysis of each, and subsequently with a presentation of results obtained by the use of the better known methods.

First of all it is necessary to know that the serum diagnosis of syphilis has been attempted in three general ways: Firstly, by certain so called "specific" methods; secondly, by methods which while not strictly speaking specific are in a sense analogous to the "specific" reactions, and finally, the methods which are based simply on the observation that certain quantitative differences can be demonstrated in the body fluids when syphilis is or has been present. I have used the word specific in speaking of the first general group. It is perhaps as well at the outset to say, however, that no one of the reactions to be described can be regarded as absolutely specific.

While the Wassermann reaction and the modifications thereof are of the greatest importance and have priority in the matter of their appearance as diagnostic methods, I will postpone a discussion of them until I have dealt with the methods included in the other two categories. A brief reference will be made, firstly, to several of the methods which were introduced in order to simplify the technique of the original Wassermann reaction and are included in the second group of reactions already spoken of. The first and perhaps best known of these methods is the Porges-Meier (2) precipitin reaction. These investigators found that when the serum from a luetic patient

was added to a small amount of an emulsion of lecithin prepared from the white of an egg, a flocculent precipitate appears. Unfortunately, the value of this test is not very great because while a fairly high percentage of syphilitic sera will give positive results many nonsyphilitic sera will also give it, and it has been found that the serum of advanced cases of tuberculosis very frequently give it (von Eisler) (3). This method was modified by Porges and Salomon (4) who used Sodium glycocholate, and by Nobl and Arzt (5) who substituted sodium oleate for lecithin. Neither modification, however, materially increased the value of the method for the reason that the test originally nonspecific remained so, even after being modified as outlined. Klausner (6) described a simple method which he stated gave a high percentage of positive results, especially in early, active syphilis. The method was exceedingly simple and consisted in adding distilled water in a large amount to luetic serum, and a precipitate was present after the mixture was allowed to stand several hours. Despite the fact that Klausner included more than forty per cent. of controls in his list of cases in which the reaction was tried, in none of these did he get a positive result. Other authors, however, soon reported positive results with this method in cases of tuberculosis and scarlet fever in which syphilis could be excluded. For this reason then the method of Klausner is but little used. The reaction described by Fornet and Schereschewsky (7) and believed by them to be specific has been pretty conclusively shown not to be, and it need only be briefly touched upon. It was asserted by these writers that when equal quantities of luetic serum and serum from a patient suffering from some parasymphilitic condition were brought in contact a specific precipitate was brought down at the line of junction of the two sera. This phenomenon, however, has been shown to occur when known normal sera are used instead of luetic sera, and one may safely conclude then that the test is of little value. Specific agglutinins for the *Treponema pallidum* are said

to be present in the serum of individuals suffering from syphilis, but since considerable difficulty has so far stood in the way of the artificial cultivation of the treponema; it seems unlikely that an analogous reaction to the Grüber-Widal phenomenon would at the present time be of much value although this has been suggested by Landsteiner (9) and others. Neisser (10) and Wolff-Eisner (11) have suggested the use of a cutaneous luetic reaction analogous to the tuberculin reaction of von Pirquet; where general infection with syphilis can be determined, according to these investigators, by a marked local reaction, indicating an over-sensitiveness which is interpreted as meaning infection with the specific micro-organisms used in bringing about the local reaction. This being analogous, not only to the tuberculin cutaneous reaction but also probably to the ocular typhoid reaction of Chantemesse and Hamburger and the gonococcus reaction of Irons. The method outlined by Neisser and Wolff-Eisner, however, has but little practical application of the scant literature in regard to its usefulness may be taken as a criterion. Michaelis (12) reported a method which may simply be mentioned because it has no value whatever. It consisted in mixing luetic extract and luetic serum, the precipitate which comes down, however, can be readily obtained also by using nonluetic serum. In summarizing the value of the various precipitin tests it may be concluded that they have but little usefulness and are of but little assistance in determining whether or not a given patient has syphilis. Articles by Butler (13), Wieder (14), and others in America have summed up in a judicious fashion the exact status of these methods.

To pass on, then, to a review of the methods of diagnosing syphilis by means of an increase in the body fluids of some of their native constituents; the first of these then is the method of Noghuci (15), which consists essentially in determining whether or not there is an increase in the globulin in the blood serum of syphilitic individuals. The increase in the globulin fraction of the

serum is shown by a half saturation of the serum with a saturated solution of ammonium sulphate. The washed globulin fraction is dissolved in salt solution and has an equal quantity of butyric acid (ten per cent. solution) added. The increase in the globulin fraction is determined after the mixture has been allowed to stand for two hours by marked opalescence, much more pronounced than in normal sera and by the presence of flocculi. This method has proved to be of some value in the hands of certain workers. However, owing to its not being thoroughly satisfactory even as far as a test of this sort, nonspecific in character can be satisfactory, a modification was suggested by Gay (16), of Boston, and the writer. In this method 0.1 c.c. of the serum is taken and to this is added 0.9 c.c. of normal salt solution and 0.5 c. c. of a saturated solution of ammonium sulphate. In some cases of syphilis a precipitate is at once evident but not always so. In any event the precipitate is then rapidly thrown down by centrifugalizing, dissolved in 1 c.c. of normal salt solution and an equal amount of ten per cent. butyric acid added, the mixture then being heated to boiling and the result read at once. Marked opalescence with flocculation is regarded as a positive result. The test is suitable in certain cases, but not suitable where differentiation between syphilis and acute infectious diseases is necessary, as the euglobulin increase can also be obtained in such conditions. The test is nonspecific and therefore has a limited field of usefulness. Another test similar to the last two was devised by Nonne but has no advantages over either of these and has the same limitations; it indicates when positive, an increase in the protein content of the blood serum. Finally it may be best to include here the Noguchi test which is applicable when cerebrospinal fluid is to be tested. Here 0.5 c.c. of clear cerebrospinal fluid free from any trace of blood is taken, and to it is added 2.5 c.c. of ten per cent. butyric acid, and the mixture heated to boiling; 0.5 c.c. of a normal (four per cent.) sodium of hypoxide solution is then added and

the mixture again boiled; opalescence at once and flocculation within a few minutes are indicative of a positive result. Not only is a positive result obtained in syphilis but also in the parasyphilitic conditions, tabes, and general paralysis; however, the meningitides also give a positive result, so that here again the limitations in the field of usefulness are obvious. While it might be advisable to use any of the last group of methods where laboratory facilities are more or less limited if one wishes the most satisfactory and the only conclusive tests it is necessary to resort to either one or other of the methods next to be described.

The technique of the Wassermann, Neisser, and Brück procedure was established on the basis of facts determined by Bordet and Gengou (17) and known generally as "the reaction of fixation." Bordet first showed that when an animal was treated with a given microorganism there developed in the serum of the animal so treated a specific sensitizer which had the property of bringing about the solution of the microorganism when an appropriate mixture was prepared. He first demonstrated this by showing that the serum of a rabbit that had been injected with cholera vibrios was able to cause a dissolution of the vibrios if the serum was *fresh and had not been heated*. If, however, the serum was heated then no bacteriolysis (Pfeiffer's phenomenon) occurred unless a fresh, normal serum was added. Bordet (18) was later able to show that this same principle held good in the case of red blood corpuscles, and about the same time Gengou (19) showed that sensitizers for albuminous substances were also formed in the serum of animals properly treated. These sensitizers are designated amboceptors by the adherents of the Ehrlich nomenclature. The next logical step in the work of Bordet and Gengou was to show that when these sensitizers (antibodies) and the substance used in bringing about their production, the so-called antigen, were brought together and mixed with alexin (complement) this alexin was fixed. And on this principle, the "reaction

of fixation" mentioned before and here described in detail are based the Wassermann and Noguchi methods for the serum diagnosis of syphilis. Soon after the reaction was described by Bordet and Gengou in 1901, it was utilized for the diagnosis of various infectious diseases where the antigen was easily prepared.

It will be obvious that the method would be useful in determining not only whether an antibody or sensitizer was present, but also whether an antigen existed in a mixture, provided one knew that the antibody was present and this would be determined by learning whether or not the alexin was fixed. For our purpose this evening it is sufficient to say that if we suspect that a patient has syphilis we use as antigen, luetic material (or some satisfactory substitute), and endeavor to find out whether or not a so-called antibody exists. If alexin is bound or fixed we conclude that it is. Now in order to learn this we must use an indicator, and for this purpose an hæmolytic system is used. This consists simply of fresh washed blood corpuscles and the serum of an animal that has been immunized against these corpuscles, inactivated (heated to 56° C. for thirty minutes); if alexin is not fixed the corpuscles that have been sensitized by the immune serum are hæmolyzed by the free alexin. Having these facts in mind, Wassermann conceived the idea of applying the principles already enunciated in the diagnosis of syphilis, and because the antigen could not be obtained in any other way he utilized a watery extract of syphilitic foetal liver. He found that this extract when mixed with the serum of a syphilitic patient and a small amount of normal guinea pig serum as alexin (complement) the alexin was fixed so that when subsequently a mixture of suitably sensitized sheep's corpuscles were added they were not hæmolyzed. He therefore concluded that the blood serum of a syphilitic individual contained an anti-substance to the cause of syphilis, or a so-called fixation antibody, and the combination of the two is known as an antibody-antigen reaction, the union being brought about by means of the alexin. In the original method of

Wassermann he used the following ingredients, 0.2 c.c. of a salt solution emulsion of a luetic foetal liver added to an equal quantity of inactivated (heated to 56° C. for thirty minutes) patient's serum and 0.1 c.c. of the blood serum of a normal guinea pig. This mixture is incubated for one hour at 37° C. When 1 c.c. of a five per cent. suspension of thoroughly washed sheep's corpuscles and two minimal hæmolytic doses of a rabbit antisheep sensitizer made up to a volume of 1 c.c. is added. The mixture is again put in the thermostat for one hour, and complete exhibition of hæmolysis at the end of that time is to be regarded as a positive result, as fixation of the alexin is thus shown to have occurred. Wassermann used various amounts of his antigen and serum that was being tested; and in each instance had a control tube in which either the antigen or antibody was replaced by salt solution.

For some time no one questioned the explanation of the reaction advanced by Wassermann that it was a true antibody antigen reaction. Wassermann had Porges and Meier (20) investigate various antigenic mixtures, and they found that not only did a watery extract of a syphilitic liver give fixation, but also that an alcoholic solution of normal liver and pure lecithin gave similar results. Slightly earlier than this Marie and Levaditi (21), and Michaelis also showed that an extract of a normal liver would serve as well as the extract of the luetic liver. Levaditi and Yamanouchi (22), and others also showed that an alcoholic extract of either a normal or syphilitic liver would act equally as well as the salt solution emulsion proposed by Wassermann. All this evidence threw doubt on the specific character of the Wassermann reaction and very considerable discussion has been aroused as to whether or not the reaction really is a true antibody antigen complex.

In order to clear up this question much work has been done in investigating the nature of the substances concerned in the fixation process. First of all it was thought that possibly since an antigen antibody reaction was

present, or at least something analogous to an antibody antigen reaction, that it would be possible to demonstrate the presence in the patient's serum of the specific antigen. The variety of substances, however, that were found to give fixation made this impossible and the chemical character of the antigen was then investigated, and Levaditi and Yamanouchi (23) were able to show that it was a substance that was soluble in alcohol, the solution was not changed by heating to 80° C., and it was found to be a lipoid, present in the luetic liver which material had been used as an antigen. This observation has also been reached by Landsteiner (24), Noguchi (25), Porges and Meier (26), and others.

While it is known that lecithin will act as antigen, as has already been mentioned, yet Gross and Volk (27) were able to show that it was not the lecithin in an alcoholic extract of the human heart, used as antigen, that gave fixation, but some other lipoid not unlike lecithin. Nearly all writers on this aspect of the subject have maintained that the antigenic substance is a lipoid. Weil and Braun (28), however, have pointed out that when a watery extract of a liver is taken and all the lipoid extracted (by means of petroleum ether), the antigenic property still remains. Sachs and Altmann (29) profess to have shown that sodium oleate can act as antigen. In the face of all this mass of conflicting evidence it is satisfactory to know that the majority of workers using the Wassermann method prefer the luetic extract to any of the other substitutes, and various observers state to be able to get a higher percentage of positive cases in this way. While much has been done the exact nature of the antigen remains unsettled.

In regard to the character of the antibody or substance in the serum of the syphilitic patient that combines with the antigen to fix the alexin, the work of Noguchi (30) on the subject has been the most exhaustive and most conclusive; he was able to show that the active component of the substance combining with the antigen is contained in the euglobulin fraction of the serum, can be inactivated

by heating for twenty minutes at 76° C., is destroyed by acid and alkalies and by the combined action of Eosin (a photodynamic substance) and sunlight. He also showed that the serum remains active for one year, and these observations have been confirmed by various observers.

Regarding the exact nature of the phenomenon, then, Wassermann maintains that it is caused by the alexin being fixed by specific substances in the luetic serum (produced as a result of the infection) combining with a lipid substance in the antigen. This practically reduces it to a specific antibody antigen reaction. From this extreme view of Wassermann on the one hand, opinions range all the way to the exactly contrary opinion of Elias Neubauer, Porges and Salomon (31), and others, who hold that the reaction is nothing more than an interaction between certain colloids. This view, however, has not been finally accepted. In a recent article Flashman and Butler (32), after going over the ground in an extremely careful and judicial fashion, conclude that "the reaction in the infection would thus seem to be placed in the same category with the alexin fixation which occurs with all antigen antibody reactions, and it seems necessary under this supposition to consider that certain substances of a highly organized nature produce the antibodies whose presence is indicated by alexin fixation," and later say: "It would seem that the minds of many observers have been so diverted by the discovery of the possible explanation of the Wassermann reaction by the interaction of known chemical substances that their attention has in some degree been drawn from the biological aspects of the test. All antigen antibody reactions are probably ultimately chemical in their nature, and sooner or later the nature of the substances which act as antigens in infection, whether contained in the body substance of a microorganism or formed by its influence, and of the other reacting substances, the antibodies, will be more or less exactly determined."

Having thus considered the theoretical aspects of the

reaction I will outline very briefly the method of making the Wassermann reaction, which I have followed in a series of approximately one hundred and forty cases with exceedingly satisfactory results. For antigen I have used a dried extract of syphilitic foetal liver extracted with ether, washed with alcohol, and dried in a vacuum desiccator over well washed sheep's corpuscles and 1 c.c. of this emulsion in 1 c.c. of normal salt solution being used; 0.2 c.c. of the serum or cerebrospinal fluid to be tested and 0.1 c.c. of normal guinea pig serum as alexin. This mixture is put in the thermostat for one hour at 37° C., then taken out, and 1 c.c. of a five per cent. suspension of well washed sheep's corpuscles and 1 c.c. of rabbit antisheep sensitizer in the strength of three minimal hæmolytic doses is added. The tubes are again put in the thermostat, and read finally at the end of one hour or when hæmolysis is complete in the control tubes.

In a rapid review of the literature I have been able to find recorded the results in over ten thousand cases examined by the Wassermann method, and the results on the whole have been thoroughly satisfactory. A positive reaction is obtained in a high percentage of cases of syphilis and not in other conditions, with the exception of certain of the trypanosome infections and in some cases of leprosy. Mûch and Eichelburg (33) stated to have obtained positive results in scarlet fever, and certain other workers assert to have obtained similar results. Jochmann (34), Meier (35), and Hoehne (36), on the contrary, obtained negative results in scarlet fever, and after some personal experience I am decidedly of the opinion that the reaction does not occur in scarlet fever.

In regard to the results in primary syphilis, from ninety to one hundred per cent. of cases give positive results when the serum is examined, about ninety per cent. active secondary cases, and from forty to eighty per cent. of tertiary cases are also positive, the variation depending upon whether or not the case is active or latent. In the parasymphilitic diseases, Plaut (37) in an examination of the blood in 186 cases of general paralysis

obtained one hundred per cent. of positive results, and with an examination of the cerebrospinal fluid from these same cases obtained ninety per cent. of positive results. It is interesting in this connection to note that in syphilitic nervous diseases the cerebrospinal fluid is positive in only about ten per cent. of cases. This has led recently to a reiteration by Edel (38) of the dictum of Plaut and others *Keine Paralyse ohne Syphilis*. In my own series of 133 cases I obtained eighty-seven per cent. of positive results in general paralysis where the cerebrospinal fluid was examined, seventy-five per cent. positive in secondary syphilis where the serum was examined, seventy-five per cent. positive with the serum in cases of general paralysis, and twenty-five per cent. positive in latent tertiary lues. Several cases of scarlet fever were negative and no other nonluetetic condition gave a positive result.

The work of Neisser (39) is interesting in connection with the question of results obtained in a very extensive series of tests done with the blood serum of artificially inoculated monkeys and apes. He was able to present a remarkable series of controlled results, that is, inoculating the animals and observing when the primary and later lesions appeared. He also pointed out certain other features of interest, and one of these was most striking: He showed that in a large percentage of cases a positive result in serum diagnosis could be obtained after the animals were inoculated but before the primary lesion appeared, and in such cases excision of the primary lesion, later when it appeared, was of course of no value, secondary symptoms developing. If, on the other hand, after artificial inoculation and after the appearance of the primary lesion a complete excision was done and a negative result was obtained, when a Wassermann reaction was made, then the removal of the primary lesion could be shown to have resulted in a complete cure, secondary manifestations never developing.

A consideration of the Noguchi modification of the Wassermann reaction and the other modifications that have been proposed need not be dealt with at any great

length here. Bauer's modification consisted simply in substituting for the rabbit antisheep serum in the hæmolytic system, spoken of before, the normal sensitizers for sheep's blood present in the human serum. This is a very variable factor and cannot be safely relied on. The modifications of Hecht, Stern, Detre, and Tschernogubow are all open to serious objections and need not be discussed any further. The Noguchi method, on the other hand, is being largely used and is warmly advocated by those who have used it. Fundamentally it differs from the Wassermann method in substituting for the rabbit antisheep hæmolytic system, a rabbit antihuman system, thus obviating any possible source of error due to the presence of natural sensitizers for sheep's blood in human serum. The originator of this method has also prepared the various factors entering into the reaction so that they may be kept stable for long periods. It is possible by this method to use exact amounts of the various substances.

The wide field of usefulness of the two methods of serum diagnosis is constantly increasing and at the present time it is used in many clinics abroad not only for the diagnosis of syphilis primarily but also as a guide to when treatment might be discontinued or whether it should be resumed. It might be noted in this connection that under active mercurial treatment a positive reaction frequently is changed to a negative one, but again becomes positive if treatment is discontinued too soon. The test has also assumed sociological importance and it is frequently used to determine whether or not it is advisable for an individual who has had syphilis to marry or whether one who has had syphilis and is married could be advised in regard to whether or not it would be safe to have children. In a recent number of the *Münchener medizinische Wochenschrift*, Staathoff dilates at considerable length on the wide and varied usefulness of the Wassermann reaction. He has never obtained a negative result in active syphilis and in no case in which

a luetic infection could be definitely excluded has he obtained a positive result.

Many of the cases quoted in this article are most enlightening and show how it is possible by means of the Wassermann reaction to clear up certain cases which the internist has to deal with in a way that no other method yet devised has been able to do. The obscurity of so many conditions often leads to the greatest confusion in regard to the possibilities, and when the question of syphilis can be at once settled, as it so frequently may be, it is indeed looked upon with favor. Some German internists have gone so far as to assert that in every obscure case where the diagnosis is uncertain a Wassermann reaction should be done. While I am not prepared to argue the correctness of this view, I do feel that every medical clinic should have attached the services of a man familiar with the technique of the Wassermann reaction. At the present time it is distinctly a diagnostic method that should only be done by those who have had some training in immunity, are familiar with the fundamental phenomena in this field of research, and capable of checking up the results obtained. Like all other laboratory methods, those of value in the serum diagnosis of syphilis are most useful when done in close connection with the clinical work and when the clinical facts have been most adequately determined. No other diagnostic method of recent times has met with such general approval after several years of trial and scrutiny, and the interpretation of the phenomenon, *not the method* or the results, have led to the controversies that have been waged. And while the last word has not been said, the reaction has an assured place in the list of diagnostic aids.

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THE PATHOLOGY OF DYSCHIRIA.*

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I.—Introduction.

As the result of a number of observations, some of which will presently be related, I have been led to re-open the problem of allochiria, one that has attracted little or no interest for many years past. In a previous paper (29) the clinical aspects of the problem were considered in detail, and perhaps the most satisfactory way of recalling the new standpoint there adopted would be to quote a

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selection of the conclusions that summarised the paper.

“Under the name of allochiria two fundamentally different conditions have hitherto been confused. A patient's mistake in determining the side of a stimulus may be:—(1) Part of a general defect in localisation—alloæsthesia; or (2) a specific defect independent of any error in localisation—dyschiria. The name allochiria has further been incorrectly applied—as in the terms electro-motor and reflex allochiria—to symptoms which are in no way related to either of these conditions.

“Dyschiria may be defined as a state in which there is constantly either ignorance or error in the patient's mind as to the side of given stimuli, quite independently of any defect in sensorial acuity or in the power of localisation. This corresponds closely with the definition of allochiria given by Obersteiner, though he did not distinguish the condition from alloæsthesia.

“There are three stages of Dyschiria: achiria, in which the patient has no knowledge as to the side of the stimulus; allochiria, in which he refers the stimulus to the corresponding point on the opposite side; and synchiria, in which he refers it to both sides; there are three sub-varieties of the latter.

“All writers subsequent to Obersteiner have abstracted one feature from his definition, namely, the reference of the stimulus to the opposite side, and have used it to define allochiria. It is suggested that the term allochiria be always used in this its current sense, with the important proviso, however, insisted on by Obersteiner, that the symptom is independent of any defect in sensorial acuity or in the power of localisation. The significance of this proviso has been entirely overlooked hitherto, and even Obersteiner did not recognise that a direct corollary of it is the separation of the alloæsthesic from the allochiric error.

“There are seven precise clinical features that enable a differential diagnosis between alloæsthesia and allochiria to be made with certainty.

“Dyschiric manifestations may be general in distri-

bution, or may relate only to certain segments of the body. There are characteristic introspective, motor and sensory manifestations of each member of the group. The last-mentioned may occur in connection with all varieties of stimuli or with only some. The motor allochiria has been unnecessarily termed "allokinesia." Sensation resulting from stimulation of a dyschiric part has six peculiar attributes, which I have grouped under the designation phrictopathic."

The first part of the present paper is a condensed account of a clinical study of two cases of dyschiria, and serves to afford both confirmatory evidence in support of the position taken up in the previous paper, together with an amplification of the description of dyschiria there given, and a material basis of fact from which the problems of pathology can be investigated. The latter part, dealing with its pathology of dyschiria, has been divided into two, as it was thought advantageous that consideration of the problems concerning the essential nature of the varieties of dyschiria should precede discussion of the pathogenesis or mode of origin of them. Throughout an attempt has been made to simplify the problems at issue and to deal with only the more elementary of them. I hope later to publish an account of some further experimental observations, together with a discussion of some of the more recondite aspects of the subject. The conclusions offered at the end of the present paper are therefore to be regarded as only the necessary preliminaries to a more exhaustive consideration of the problems concerned.

II.—Study of Two Cases of Dyschiria.

Case A.—Hysteria. Unilateral Dyschiria.

I am indebted to Dr. Rose Bradford for the opportunity of investigating this case, and for permission to publish an account of it. The patient was a man of 31. He was suffering from severe hysteria that followed on a railway accident he had met with six years previously.

He had, amongst other hysterical symptoms, complete bilateral amaurosis and ageusia, profound retro-anterograde systematised amnesia and recurring "Dämmerzustände," accompanied by very severe pain in the head. One aspect of his sensorial state has already been described elsewhere (26), and the present account will be confined to the dyschiric manifestations.

At the outset it should be stated that the case was an instance of that very rare condition, pure hysteria. The patient's freedom from neurasthenia complications was very striking, and his courageous endurance and clear-sighted intelligence were invaluable aids in the study of the symptoms, for they rendered possible an introspective analysis that elucidated problems otherwise difficult or impossible of solution.

During two years of treatment, which ended in recovery, the symptoms now in question passed through various stages, and it will conduce to clearness if we divide these two into four: Anæsthesia, achiria, allochiria and synchiria respectively. They will be described in this order, which represents the relative degree of the disaggregation present, and in each the sensory, motor and introspective features will be considered separately.

First Stage.—Hemianæsthesia.

(a) *Sensory*: There was anæsthesia of the right half of the body, implicating all the accessible mucous membranes, and everywhere sharply marked off by the middle line. The anæsthesia was complete; tactile, painful, electrical and thermal stimuli, all equally failed to elicit any response. Catalepsy could always be easily induced on this side. Sensibility on the left side was quite normal.

(b) *Motor*: The left side was quite normal also in this respect. On being asked to carry out any movement on the right side, however, the patient failed to comprehend the order, merely feeling vaguely that he was being asked to do something out of his power; the reason for this will be made clearer when we consider the intro-

spective aspect. He further had an intuition that his failure made him look foolish because it was something simple that was being asked of him, something which other men could do easily.

In spite of this fact, certain habitual and automatic movements were often carried out by the right hand, so that the defect was limited to conscious processes. Further, if he was first asked to perform a simple movement with the left hand and then the request made "Now do the same with the other hand," the word "right" not being used, he would make strenuous attempts to carry out the request; even then, however, he usually failed to do more than move the right hand a few inches, although intense effort was visibly being employed.

When asked to touch with the left hand a point on the right side of the body, he had the very greatest difficulty in doing it. Under no circumstances could he be got to touch any point on the right side with the right hand, and one could get him to touch with this hand points on the left side only by the device of not using the word "right," but by saying "Now the other hand" just after he had used the left; even then it was exceedingly difficult.

(c) *Introspective*: The most striking feature in this respect was that the patient not only could feel nothing *with* the right side, but he could feel nothing *of* the right side; in other words, so far as feeling went, he had no right side to his body. This loss was so complete that the patient's mental attitude on the subject must be described in some detail.

(1) *Right-Sidedness*: He was of course aware that, like other men, he had two pairs of limbs and two sides to his body, but this knowledge was barren, abstract and devoid of meaning; as a matter of direct feeling a man was for him a one-sided creature, and it was only by constantly reminding himself that he could remember the contrary. The absence of meaning from the knowledge was more complete in the case of his own body, so that he

looked upon himself as something apart, as one differing from other men in having only one side; at the same time he had no clear idea as to wherein this difference lay, nor could he imagine what it would feel like to have two sides, or what advantage could be derived therefrom.

(2) *Left-Sidedness*: He looked upon his left side not as being a whole, as might have been expected from the foregoing, but definitely as being a half of something, the other half of which was missing. In fact, he felt an abrupt cleft in the middle line, at which his body seemed suddenly to stop. Further, he was clear that this half that remained was a *left* half, though he could not express the meaning of leftness because of his ignorance of its complement. When asked "What is meant by left-sided," he would indicate his left side and say "something on this side," whereas, when asked "What is meant by right-sided?" he invariably replied "I don't know." He therefore regarded the question of restoration of feeling in the other half of his body as probably meaning the addition of another left half, though he had a vague suspicion that it might mean the addition of something differing from his present left half in some undefined way that he could not understand.

(3) *Right-Handedness*: He had no conception of what right-handedness meant, or in what way it differed from left-handedness. Consequently he had no idea of the functions that are specifically right-handed, for, being originally a right-handed man, he lost all knowledge of these with the loss of his right side. He avoided all conversation in which the subject occurred, for whenever his infirmity became evident he experienced great annoyance. Thus, if he was asked by a comrade whether a given nut or screw was a right-handed or left-handed one he would get out of the difficulty by offering to help with the task going forward.

(4) *Left-Handedness*: Here again he seemed to retain some conception of the meaning of this term. He realised that a left-handed person was somehow anomalous, that he was at a disadvantage in the use of tools, and that left-handedness was generally an impediment. This he felt as an intuition; he could in no way explain its significance or give any further description of the term.

Summary of Anæsthetic Stage.—

1. Complete and total anæsthesia on the right half of the body.
2. Complete amnesia for the feelings of right-sidedness and right-handedness, and for the meaning of these terms.

Transition from Anæsthetic to Achiric Stage.—The essential feature of this transition was the recovery of the sensorial acuity of the right half of the body. This was gradual, and came about in the following way. The first sign of recovering sensibility was an intensely severe pain at a localised spot on the right side of the head, as it later transpired, that had been struck during the accident, which occurred on the application of a strong faradic current to the right foot. In Bosc's case of allochiria (1) the same phenomenon was noticed. Subsequent stimuli always caused this pain in the head, though it was not so severe as at first. Then, in addition to the head-pain, came an intensely disagreeable "shuddering" sensation. He could not say where he felt this; it seemed to be in space at some indefinable distance from his real body; it was foreign, strange and essentially not part of himself. The various normal attributes of sensation, nature, quality, position, etc., only gradually appeared. The sensibility passed through all stages of acuity before reaching the normal; thus sensation was elicited at first only with strong stimuli, later with weak ones also.

Second Stage, Achiria,—

(a) *Sensory*: After the transitional stage was passed through and achiria definitely developed, the sensorial acuity was absolutely normal. I wish to emphasise this fact, for it has a decisive importance in connection with the theory of the condition. There was no hypæsthesia to the most delicate tests, and every attribute of a stimulus could be accurately determined except the one point of its sidedness. The patient could localise a stimulus applied on the right side precisely as well as one applied on the normal left side, but he had not the least idea as to which side it was on; there was no feeling of sidedness attached to it. It obviously was not on the left side, the feeling of which he knew. It could not be on any other side, for he knew of no other side; the expression "right side" conveyed no meaning to his mind and aroused no feeling of a side at all. This state of affairs applied to all kinds of stimuli equally. The patient could thus distinguish the precise nature of stimuli applied to the right side, their quality, temperature, exact locality, etc., as well as a normal individual, but he had no notion whatever of their side.

Any sensation evoked by right-sided stimuli had the following six peculiar features, which I have grouped under the designation "Phrictopathic" (27). These were present in every stage of dyschiria, but as they were most marked in the achiric stage I shall briefly mention them here as they then appeared:

(1) *Abnormal persistence*. A momentary stimulus evoked, instead of a momentary sensation, one that lasted in unabated intensity for 50-60 seconds.

(2) *Delay of conscious reaction-time*. An interval of between four and six seconds elapsed before the sensation was consciously appreciated. This delay has been noted in cases of allochiria by Bosc (2), Janet (17) and others.

(3) *Non-perception when a more normal sensation is present*. The sensation was altogether prevented from

being appreciated if a stimulus was at the same time applied to any part of the normal left side, so that if the patient was touched on both sides at the same time he felt only one sensation, the normal one on the left side. Further, if any part of the left side was touched while the patient was still feeling the unduly persisting sensation that followed a touch on the right side, this abnormal sensation instantly ceased. I am not aware that any similar observation has been previously described.

(4) Tendency to immediate motor response. The stimulus was followed by an instantaneous and irresistible start on the part of the patient.

(5) Disagreeable quality. This is the most striking of all the attributes. The quality was not one of pure pain, though it was always exceedingly unpleasant. The sensation radiated widely, often along the whole length of his body. When most marked it would be described only as a horrible shuddering, the objective manifestations of which it was painful to witness. In Janet's case of achiria, where the feature was only slightly marked, he describes it as "une sensation vague de quelque chose de gênant . . . ce n'est pas une douleur proprement dite" (15).

(6) Impairment of the sense of personal ownership of the part. This has been described above; in the achiric stage the feeling of personal ownership of the part stimulated was practically absent.

(b) *Motor*: The only difference in this respect between the first and second stages was that in the latter the patient could be got, by the indirect means mentioned above, to exercise rather more power with the right side.

(c) *Introspective*: The introspective attitude toward a part being stimulated has been referred to above, and the peculiar foreignness of the sensation pointed out.

Spontaneous introspection, i.e., not during stimulation, showed only one change from the first stage, to wit: transition from the anæsthetic to the achiric stage was accompanied by a peculiar feeling as though the left side were being powerfully drawn in towards the middle

line. This became almost complete when achiria was reached, so that the left side, and therefore the whole body, felt as though it were situate in one plane, the middle line of the body. The abducted left arm felt no further from the middle line than the shoulder; thus the patient seemed to himself in this stage to occupy two dimensions instead of three. The feeling was accompanied by a state of great misery, which the patient described as "feeling shrunken, decrepit, good for nothing."

Summary of Achiric Stage:

(1) Complete amnesia for the right side, except when it was being stimulated.

(2) Complete amnesia for the feelings of right-sidedness, dextrality* and right-handedness under all circumstances.

(3) Sensorial acuity absolutely normal on right side, but presence of the six phrictopathic features to a marked degree; impersonal feeling in part stimulated.

(4) Loss of feeling of lateral extension on the other-wise normal side.†

Third Stage, Alloc'hiria:

The motor and introspective aspects of alloc'hiria have an important bearing on the theory of the condition, and, as they have been largely overlooked hitherto, I shall describe them in some detail.

(a) *Sensory:* As in the achiric stage, the sensory acuity on the right side was absolutely normal in all respects. Similarly all sensations arising from stimuli applied to this side were accompanied by the six phrictopathic features mentioned above, though these were less marked in this stage; for instance, the delay in reaction-time was three seconds, the abnormal persistence reached

* This term is used to indicate the superior capacity that usually the right shews over the left.

† This symptom has only an indirect relation to dyschiria, and so the pathogenesis of it will not be discussed in this paper.

to 35 seconds, and the diffuse shudder was only very disagreeable, no longer horrible.

In striking contrast to the achiric stage, however, the sensations now evoked by right-sided stimulation were accompanied by a definite feeling of sidedness, but this was left-sidedness. In other words, all stimuli applied to the right side were invariably referred to the left. This was done with an air of the most absolute conviction, so that the patient felt no more certain that a stimulus was on the left side when it really was applied on this side than he did when it was applied on the right. Other distinctions, however, between the two sets of stimuli will be described in connection with the introspective aspects.

Three further remarks must be made in the present connection. The transference applied equally to all kinds of stimuli, tactile, thermal, electric or painful; it was thus general, and did not shew the selectivity sometimes met with in allochiria. It was absolutely invariable; not some or most stimuli were referred to the left, but all without exception. Lastly, the place on the opposite side to which they were referred corresponded precisely with the symmetrical point touched, so that, apart from the error in side, the patient localised a touch applied to the right side exactly as well as one applied to the left.

(b) *Motor*: When the patient was asked to make a simple movement on the right side he did not, as in the achiric stage, merely fail to understand the request, but he promptly carried it out, on the left side. This was done invariably, and with no trace of doubt as to the correctness of the performance.

The question of right-sidedness, as well as several more complex questions concerning the motor functions, will be considered in relation to the introspective aspect. Two points only will be dealt with here.

In the first place it is clear that dextral functions* were transferred to the left hand under certain circumstances. This may be illustrated by considering a speci-

* Functions for which one hand, usually the right, manifests superiority. I have recently published a paper (35) dealing with the nomenclature of this subject.

fically dextral act, such as that of winding a watch or using a screwdriver. If a patient was asked to insert a screw with his right hand he would take the screwdriver with his left and use it quite dexterously and naturally. If, on the contrary, he was asked to do so with the left hand, he would again take the tool with this hand, but would now be practically unable to use it, merely fumbling with it in the clumsiest way. If one simply put the screwdriver into the right hand it was not used, not because the hand seemed specially clumsy, but because the patient did not seem to know how to use it; if one put the tool into the left hand he used it quite dextrally if he thought he was engaged with the right hand, but not if he thought he was engaged with the left. It was astonishing to witness the change that took place if, while he was in the act of confidently using the tool in a dextral manner with the left hand, one suddenly said, "Now use it with the left hand"; under these circumstances he would become puzzled, drop the tool, and pick it up again with a hand that was anatomically the same as before, but functionally an entirely different member. In other words, the left hand was used either right-handedly or left-handedly, according to whether the patient felt he was using the right or left hand respectively.

In the second place, this transference of dextral functions to the left side concerned, like all dyschiric manifestations, only conscious processes. For habitual movements subconsciously and automatically performed in daily life the patient would use the right hand normally, unless some difficulty or other element directed his conscious attention to the act when the above mechanism appeared. This might happen during the performance of an individual act. In the instance just taken, the patient, if he were left to himself, might automatically use the screwdriver with the right hand until the last threads of the screw were reached, when he would change over to the left, under the impression that he was using the same hand throughout.

(c) *Introspective* :

The left side now again felt quite normal, the feeling of lateral extension having returned.

As in the previous stages, the patient declared that he had no right side to his body. the word "right side" conveyed no meaning to him. It would seem a natural inference that a left side being present but no right, therefore, as before, only one side was present. This was how the patient himself expressed it, and it was some little time before I discovered that this apparently obvious conclusion was incorrect. Introspective analysis, towards which he showed great aversion, as will presently be described, demonstrated the following state of affairs. It was true that he could feel no right side and that all he could feel was on the left, but it was not true that he could feel only one side. He could feel two, *both on the left*. We next ask what was the difference between these two left sides and how they were to be distinguished from each other? Light is thrown on this question by considering the change that had occurred in relation to certain semi-voluntary actions. If the patient was directly asked in the former stage whether he could use various right-handed implements, such as a hammer, screw-driver, etc., he was only puzzled, for, although in fact he could in an automatic manner use them with his right hand, he did not seem to know that he could, and usually had, amnesia for the various occasions on which he had done so. On the contrary in the present stage if he was asked a similar question he promptly replied in the affirmative and said that he could certainly use them—with the left hand. In fact, however, he still used them with the right hand. Therefore the transition to the present stage seemed to him to signify an accession of further capacity, for whereas formerly he could, so he thought, do only left-handed things, he could now also do things that were not left-handed, though he did not specifically recognise that these were right-handed. Later on, when he recovered completely from his allochiric condition and recognised that these right-handed things were

being done by the right hand—as habitually they really had been all along—he translated this fact as a loss of the extraordinary power that the left hand had been enjoying. This subsequent interpretation thus ran as follows: He had lost the use and feeling of his right side, and gradually his left side learnt to be able to do things it had previously been unable to do; with the recovery of his right side, his left lost this increased capacity. When, however, I demonstrated to him that he was totally unable to use a screwdriver with the same left hand that, according to him, had recently been so expert in such matters, he was considerably puzzled, for, of course, he knew that when a left hand replaced an injured right hand and learnt its work, it generally retained this acquired capacity, at all events for a long time. He had previously encountered difficulties in the way of his explanation, and these had caused him such intellectual bewilderment that he had striven, ultimately with considerable success, to ignore them. The most important of these difficulties was that he could really distinguish in feeling that he had two separate “sides,” both of which, however, were on the left. There was, first of all, the true left side, which could perform only left-handed or ambidextral actions, and which felt to him *alive*, real and natural. Then there was a ghostly other side, which was also equally beyond doubt situate on the left and superimposable on the former side, but which could do many things that were impossible to the left, and which felt to him *dead*, unreal, strange, foreign and as if it did not belong to him; it was not part of his real self, and actions carried out by it seemed really to be done by something else or somebody other than himself. The latter he called the “clever” or “dead” side. As is comprehensible enough, the effort to grasp this extraordinary state of affairs was one that would have embarrassed the keenest thinker, and we can sympathise with the tenacity with which the patient clung to the simple explanation given above, to wit, that he still had only one side. In doing this he was greatly aided by the intense conviction that everything he felt

was really on the left, for the two sides had this in common, that they gave rise to the feeling of leftness with an equal degree of certitude. It is interesting to note that in spite of this equally plain feeling of leftness in the two sides and the strong incentive there was to submerge and ignore the differences between them, the patient never succeeded in making them feel completely identical, and, much to his discomfort, could not escape the fact that they were fundamentally two.

He shewed a marked aversion to making an analysis of his feelings that would serve to emphasise the differences between the two sides, and a strong disinclination to make any statement other than that he had only one side, a left one. I was aided in getting him to accomplish this analysis by the device of fixing his left hand at the same time that I moved the right hand, which, of course, he felt on the left side. He thus felt his left hand apparently being simultaneously moved and held still, a paradox which forced him to reopen the question as to the true identity of the two left hands.

We can now understand the following observation. If, after the patient had moved his left arm in response to a request to move the right, he was then asked "Was that the right arm that you moved?" at first he would apologetically reply "It is the only arm I have." Later when he had been induced to distinguish more clearly between his two left arms he would say that he was moving the "clever" left arm. Later still he learnt to call this clever left arm a right arm, even though he didn't feel it to be right-sided, so that he would then answer the question in the affirmative.

From the possibility of the analysis between the two "left" arms, which at first appeared to be identical, I was led to try whether any similar distinction could be made between the two sensations evoked by stimulation of contralateral points. At first this similarity seemed impossible, for both were localised to the same spot with an equal degree of precision, and on account of the intense conviction with which stimuli applied to the right

side were felt as left-sided, the patient drew no distinction between them and stimuli really applied to the left side. He therefore was inclined at first to say that they were the same, but on carefully directing his attention to the comparison of different stimuli—without letting him know the side on which they were applied—the following differences became evident. Sensations evoked by left-sided stimulation were normal in every respect, but, as was pointed out above, those evoked by right-sided stimulation were always characterised by the six typical phrictopathic features.

On closer investigation it was further found that a stimulus applied to the right side produced, not, as was at first thought, simply a sensation referred to the corresponding point on the left side and having the peculiar features mentioned above, but *two separate sensations* referred to the same spot. These differed, just as the sensations produced by stimulation of the two sides differed. The one was felt to be on the true, "alive" side, felt natural, and lasted but a moment; the other was felt to be on the other "dead" left side, and had all the phrictopathic features described above. The normal one was, however, delayed, so that both sensations began synchronously, though of course the phrictopathic one lasted longer than the other. It is again interesting to note that the conviction of the left-sidedness of stimuli applied to the right side was so absolute as to enable the patient practically to ignore the differences that distinguished the sensations elicited by right and left-sided stimuli.

Summary of Allochiric Stage:

(1) Left side normal; right side felt as a second "clever" or "dead" left side.

(2) Amnesia for the feelings of right-sidedness and right-handedness. Feeling of dextrality appreciated when the left hand was used while the patient was under the impression that it was the "clever" or "dead" left hand.

corresponding part on the left, the patient feeling that the "dead" left side was moving.

(4) All stimuli applied to right side invariably and with conviction referred to exactly corresponding point on left.

(5) A stimulus applied to the right side evoked two sensations, both referred to the left side with equal conviction. One was indistinguishable in quality from a stimulus really applied to the left; the other had all the typical phrictopathic attributes.

Fourth Stage, Synchiria.—

(a) *Sensory:* Here again the sensorial acuity on the right side was absolutely normal. Characteristic of the stage was the fact that every stimulus applied to the right side evoked two sensations, one referred to the right side and one to the left. At first the one on the left was felt to be more distinct, then as recovery progressed both were felt equally distinctly, and lastly the one on the right was the more distinct; there were thus three sub-stages.

There was throughout a sharp distinction between the two sensations. That referred to the left side was quite normal, of momentary duration and natural quality. That referred to the right had all the phrictopathic features mentioned above, which need not be again detailed; these were however decidedly less marked than in the former stages, and the shuddering had now dwindled to a diffuse tingling that was only moderately unpleasant.

(b) *Motor:* When the patient was asked to carry out any movement with a right limb he did it simultaneously with both, under the distinct impression that he was moving only the right. He could not move a right limb alone, and if this was passively moved it felt as if both sides, which were distinctly right and left sided, were being moved. Thus passive movement of a right hand gave him the feeling of both hands, while active movement of both hands gave him the feeling of

only the right hand, provided he thought he was moving only the right; passive movement of both hands gave him, of course, a normal feeling of both.

(c) *Introspective*: The feeling of the right side differed from that in the allochiric stage in four particulars:

(1) It felt much more "alive" than in the allochiric stage, and the patient described it as "sleepy" and "strange." Consequently the sense of personal identification was now considerably more vivid in this stage, though it was still impaired.

(2) Although the right side felt much more obviously part of the patient's self, yet curiously enough he had far less control over it than in the last stage. In the middle of carrying out any duties he was always anxious lest it might "strike work," which it sometimes did independently of his own desires.

(3) As regards the "sidedness" of the half in question, this at first seemed to very indefinite. The patient described the half as being "out of place." It shifted between the two sides, being sometimes nearer the right, sometimes nearer the left, and sometimes in the middle line; it never felt to be quite over on the left or on the right. This shifting corresponded accurately with the three sub-stages described under the sensory aspect; thus when the half felt on the left side of the middle line the contralateral sensation was the more distinct of the two, and *vice versa*. A point of some little clinical importance in this connection is that when the half was very near to its normal position on the right side no contralateral sensation could be appreciated, although the homolateral sensation still showed distinct phrictopathic qualities.

(4) As a consequence of the last two features the patient in this stage was always in a state of disagreeable uncertainty, which he disliked intensely,

and it was striking to witness the relief with which he would slip back into the allochiric stage, where, although the affected side felt more dead and impersonal, yet he had perfectly sure control over all its functionings.

Summary of Synchiria.—

(1) Right side felt more alive, more personal, but less under control: this stage was more unpleasant to the patient than the previous one.

(2) The right side had no definite "sidedness" spontaneously, but the patient obtained the clearest notion of right-sidedness and dexterity by simultaneous movement of both sides.

(3) Right-sided stimuli evoked two sensations, one obviously right, the other left. Either or neither might be more distinct, three sub-stages thus occurring. The contralateral sensation was a normal one, the homolateral phrictopathic.

Course of the Case: Recovery, as has been hinted, did not progress in the even manner indicated above. Furthermore, it did not proceed uniformly over the whole side. Feeling first returned in the right thumb, then in the finger-tips, a bracelet, the right ear, an anklet, the olecranon in order. These parts appeared to be away in the air at some indefinable distance from his body, producing a feeling of indescribable eeriness. The recovery of feeling was also gradual in degree as well as in distribution. Thus the right thumb was felt perfectly well at a time when the rest of the upper limb seemed to be made of wood, producing a feeling almost as bizarre and disagreeable as the above-mentioned one of having isolated fragments of the right side away in space and apparently unconnected with the left side.

For a greater part of the time the right side of the body was thus broken up into areas shewing different stages of dyschiria, and the nice unravelling of the condition was a matter of great complexity until the charac-

teristics of each stage were recognised and exactly defined. Much valuable information was obtained from studying the various combinations thus presented, and their effect on one another, but this matter will be excluded from the present paper, which is concerned only with the problems relating to the pure types.

Case B.—Hystero-Neurasthenia. Bilateral Dyschiria.

The patient, a man aged 49, came to the out-patient department at the Seamen's Hospital, London, in April, 1907. He had suffered for nineteen years from nocturnal epileptiform fits, which had been uninfluenced by bromide treatment. In addition he had continual somnolence and would fall asleep on the least provocation. About a week before attendance at the hospital he had a series of fits differing from all previous ones in their duration, and spent five or six hours in what seems to have been a status hystero-epilepticus. Following this a train of neurasthenic symptoms developed, which were very pronounced when I saw him. He was very timid and trembling. People, and even his wife, seemed to him to be staring at him in a significant way, and he felt that he couldn't stand this any longer, and that he was going out of his mind. The back of his head felt numb and strange, and as though it didn't belong to him. His legs felt weak and shaky, and he couldn't walk more than a couple of hundred yards. For the first time in his life he was unable to work, and this greatly depressed him. He felt "dead inside," out of the world, and unable to get into touch with outside happenings. He presented a generally confused, dazed and almost maudlin appearance, and was evidently fast losing all grasp on his immediate environment. The importance that the existence of this neurasthenic condition has for theory will be indicated later.

On investigating the nervous system, all that was found was a complete, total, bilateral analgesia. Tactile

stimuli were actually appreciated, and their site and side correctly indicated. The same results were obtained whether the eyes were open or shut, but after they had been shut for a short while a change took place in the patient's general state. He sank back in a kind of sleep, evidently of a hypnoidic character, and his limbs relaxed. On now being asked to move a given part, he moved the corresponding part on the opposite side, and in addition referred all stimuli to the opposite side. There was thus present bilateral allochiria. I shall first give a short description of this state, and then mention the various modifications that occurred later.

Bilateral Allochiria: (a) Introspective. The patient felt both sides of his body and was unaware that he indicated them wrongly. His feeling of them was not quite natural, a fact he expressed by saying that they seemed numb, sleepy, bloodless and like india-rubber. Moreover, he constantly said that he felt one half of his body—which he incorrectly called the right half—much less distinctly than the other.

(b) Motor: When asked to perform a movement with a given limb he invariably did it with the opposite one, under the impression that he was doing it correctly. This transposition concerned other requests besides those in which the name right or left was used. For instance on being simply asked to shake hands the patient did so with the left. When asked to move the hand he wrote with, he also moved the left. He was taken at his word in this matter and told to write his name. He managed to do this with the left hand with fair legibility, although when awake he was quite unable to. On the pen being put into his right hand he handled it clumsily and made some abortive attempts to write. After a few trials, however, he was able to write, and did so more legibly than with the left hand; during the progress of the act he remarked that his hand was getting "handier," and this caused a good deal of mental confusion, which ended in the temporary disappearance of all allochiric symp-

toms. Certain other motor manifestations will be referred to in the next paragraph.

(c) *Sensory*: All stimuli were normally appreciated except painful ones. They were correctly localised, except for their side.

(1) *Single Stimulus*: The patient invariably named the side incorrectly, but moved the correct part when indicating which member was touched. This was so whichever process, the naming or moving, was first tested.

(2) *Successive Stimuli*: The patient always felt one of these more distinctly than the other, and except on two occasions it was the one applied on the left side. This was determined in a way that will be described presently. He responded as described above, moving the limb actually touched and naming it wrongly. The observations were repeated several times, the method being varied. Neither the order of the limbs touched nor the order of response given made any difference to the results.

(3) *Simultaneous Stimuli*: When two stimuli were simultaneously applied on corresponding contralateral points, only one was appreciated. This one was always, except on two occasions, the one really applied on the left side, as was found by comparing the sensations with those obtained by successive stimuli. It was always indicated by moving the limb actually touched, which was named incorrectly.

Considering now the results of single stimuli, we naturally enquire whether the phrictopathic features described in Case A were present here also. They were, but undoubtedly to a considerably less marked extent; this remark applies to each of the six features, which we may now mention in order.

(a) A momentary stimulus evoked a sensation that lasted for five or six seconds.

(b) The conscious reaction-time was two seconds longer than normal.

(c) As was seen above, a stimulus applied to the right side was prevented from giving rise to any sensation by the simultaneous application of a stimulus to the left side. As there was no normal part of the body, one could not test whether the sensation resulting from a left sided stimulus could be prevented by touching a normal part.

(d) There was a distinct tendency towards a motor response in the direction of "rubbing the touch off."

(e) The disagreeable quality was not well marked but one was reminded of the diffuse tingling present in Case A by the description the patient gave of the sensation evoked by a simple momentary touch as being "like a knife scraping the flesh," "like a fly crawling," "like a scratch with a nail," etc.

(f) The feeling of natural, personal ownership of the part stimulated was impaired, as was evident from the patient's description of it as "dead, like india-rubber, like a limb gone to sleep and yet not tingling that"; he would say that the part belonged to him "in a way," "somehow."

As the asymmetry of this case has an important bearing on the theory of bilateral allochiria, I will relate in more detail an example of the series of experiments that demonstrated its existence.

In the typical allochiric state, as described above, the patient said that one-half of his body, which he erroneously called the right half, felt less distinct than the other. No objective hypoæsthesia was detectable by careful Von Frey tests. The following single experiment, made up of 15 separate tests, each of which was many times repeated, confirmed the truth of the introspective observation. The results were the same as regards the face, hands and legs.

	QUESTION.	RESPONSE.
<i>A. Single Stimulus:</i>		
1. Right touched.	Which side?	Left.
2. Right touched.	Which side?	Right moved.
3. Left touched. \approx	Move side touched.	Right.
4. Left touched.	Move side touched.	Left moved.

B. Successive Stimuli:

- | | | |
|------------------------------------|-----------------------------------|-------------|
| 1. First right touched, then left. | Which touch was more distinct? | Second. |
| 2. First right touched, then left. | Which side was more distinct? | Right. |
| 3. First right touched, then left. | Move side of more distinct touch. | Left moved. |
| 4. First left touched, then right. | Which touch was more distinct? | First. |
| 5. First left touched, then right. | Which side was more distinct? | Right. |
| 6. First left touched, then right. | Move side of more distinct touch. | Left moved. |

C. Simultaneous Stimuli:

- | | | |
|---|--|-------------|
| 1. Both sides touched at same time. | How many touches felt? | One |
| 2. Both sides touched at same time. | Which side was it? | Right. |
| 3. Both sides touched at same time. | Move side touched. | Left moved. |
| 4. First right side touched, then left. | Was previous touch same as first or second of these? | the Second. |
| 5. First left side touched, then right. | Was previous touch same as first or second of these? | the First. |

The result of this experiment may be generalised as follows:

(1) During a period of bilateral allochiria, when every stimulus was referred to the opposite side, a stimulus applied to the left side evoked a sensation that felt more distinct than one evoked by a stimulus applied to the right side, and was the only one felt when stimuli were applied simultaneously to both sides.

(2) A stimulus applied to either side was indicated by the patient's moving the limb actually touched, but by his naming the side incorrectly. This contrasts with what occurred when an allochiric limb was stimulated in the unilateral case.

The above results were constantly obtained except on two occasions, when a peculiar mode of response was obtained which I shall not describe here, as it is not relevant to the subject of the present paper.

Course of the Case: Although the patient's dyschiric manifestations were on the whole not very variable, there were certain differences noticed in them from time to time.

I. The most striking difference was in the *variation of the dyschiria with the patient's general mental state*. One ventured to predict on the basis of the view of dyschiria that will be described later that the dyschiric and neurasthenic symptoms would be mutually exclusive. This, in fact, was fully borne out by observation. The patient might appear on a given day after a prolonged domestic wrangle and in a state of almost mental collapse. On such an occasion, when the neurasthenic symptoms were very prominent, the patient found it harder to subside into the somnambulic state mentioned above, and no evidences of dyschiria were ever found until this state was pretty advanced. On other days, after the patient had been responding satisfactorily to psycho-therapeutic measures, the neurasthenic symptoms would be almost altogether in abeyance, and he would feel clear, strong, fresh, hopeful and in every way a more capable individual. On such occasions it was always found that the dyschiric symptoms appeared in the waking state on merely closing the eyes, or even without this. This observation has an important bearing on the theory both of dyschiria and neurasthenia, and will be discussed later.

II. As regards *variations in the nature and distribution of the dyschiria*, the chief of these were the following. In general the deficient feeling of the part was most marked in the lower limbs and least in the upper. Synchiria was never observed, but achiria was, and the following instances of it may be quoted.

(1) On certain occasions when allochiria was present, as regards all the rest of the body the *left side of the face* was achiric and the right side quite normal. No description of this need be given, as the features of unilateral achiria were mentioned in the account of Case A. The phrictopathic characteristics were all present, and to a

more marked extent than in the allochiric state. The main difference between them in the achiric stages of Cases A and B was that in the latter the loss of the sense of personal ownership of the part stimulated was never so profound as in the former.

(2) On other occasions the patient declared that he could feel his right arm but not his left. On examination the *right arm* was found to be achiric and the left allochiric. Thus, a touch on the left arm was said to be on the right and the left arm was used, under the impression it was the right, in a dextral way. A touch on the right arm was appreciated and localised, but it did not feel to be on any side. The remarks made above on the subject of phrictopathic attributes apply equally here.

III. Another point that has a bearing on the theory of dyschiria is that the *sensibility* shewed some variation. Total bilateral analgesia was always complete except on the following occasions:

(1) Right side of face of normal acuity, left side of face hypoalgesic, rest of body analgesic. It is interesting to note that at this moment the right side of the face showed no dyschiric manifestation, while the left side of the face was achiric. In other words, improvement of sensibility to pain on both sides of the face was accompanied on the one side by increase of the dyschiric defect, and on the other side by diminution of it.

(2) Right side of face analgesic, rest of body hypoalgesic. This was accompanied by total bilateral allochiria. Of simultaneous touches, one on each cheek, only the one on the left was felt, and erroneously called the right. There was no hypoæsthetia present on either side in this or in the previous instance.

(3) Hypo-algesia of right hand, analgesia of rest of body. At the same time there was hypoæsthesia on the whole left side and, to a less extent, on the right side of the face. There was no allochiria when the patient was first examined on this occasion, though it developed in a

few minutes coincidently with a slight improvement in sensory acuity.

There was no diminution in sensibility except as stated above. Perusal of the above account will make it evident that the dyschiric manifestations in no way corresponded to the defects in sensibility, a matter that will be fully discussed later.

IV. The following is an illustration of a general rule that applied to this case, namely, that the feeling of left sidedness was more easily and vividly transposed than that of right sidedness. It occurred when bilateral allochiria was present and total except for the face, which was achiric on both sides to direct stimulation. On getting the patient to stroke his moustaches with what he incorrectly called his right hand, ordinary allochiria developed on the face, so that, for instance, he called the right moustache left and *vice versa*. The following four tests were then applied:

STIMULUS.	QUESTION.	RESPONSE.
1. Right side of face touched.	Which side?	Left.
2. Right side of face touched.	Is this the right?	No, it is the left.
3. Left side of face touched.	Which side?	Right.
4. Left side of face touched.	Is this the left?	No, it has no side.

III.—Nature of Dyschiria.

A clearer view of the problems at issue will be obtained by considering first the nature of the defects that characterise dyschiric manifestations: general considerations on the psychological significance of them, the mechanism whereby they are produced, and the relation of the individual members of the group to one another will be reserved for a succeeding section. It is of advantage to discuss the three conditions *seriatim*, and to begin with the most advanced stage, that of achiria.

1.—Nature of Achiria.

Only one case of achiria has previously been described, that of Janet's, and he is the only writer

who has discussed the pathogenesis of the condition. His view is summed up in the categorical statement (19) that "En un mot l'allochirie simple (the term with which he designates achiria) est une trouble de localisation qui résulte d'un certain degré d'hypoesthésie, elle disparaît ou se transforme si le sensibilité diminue ou si elle augmente."

Now each of these three statements seem to me to be decisively contradicted by the observations above detailed. Taking first the assertion that achiria is due to hypoesthesia, we saw that in the achiric stage of Case A tests carried out with the most sedulous care failed to reveal any defect whatever in sensorial acuity, and that the patient could obtain all possible information about a stimulus except the one matter of the side on which it was applied. This fact clearly demonstrates that achiria is independent of any sensory deficiency, whether of a central or peripheral nature. That sensory defects may also be present in a given case, as they were in Janet's, is, of course, comprehensible, having regard to the hysterical nature of the condition, but their presence is certainly not essential for the existence of the symptom.

Then with regard to the statement concerning the disappearance or transformation of achiria when the sensibility becomes less or more acute. The lack of correspondence between the intensity of the defect in the feeling of sidedness—and thus the variety of dyschiria present—and the changing state of sensibility was emphasised in the account of Case B (see section III. of the Course of the Case).

Similarly with regard to the first assertion. In no ordinary sense of the term can achiria be called a "localisation trouble" when the patient can localise a stimulus with absolute precision, apart from determination of the side on which it is applied. Never in the hypoesthesia or allosthesia of organic disease do we find a perfect ability to localise the part touched combined with a total loss of the feeling of sidedness.

This sharp contrast between the complete failure of

the sensation to give rise to any feeling of sidedness and its capacity to give information about all other attributes of the stimulus, its exact site, quality, etc., is most striking. The amazing specificity about this particular loss irresistibly reminds one of the other elective losses that are so characteristic of hysteria. The highly specialised nature of the defects often met with in this affection, such as a systematised amnesia for a single series of events, is such a well-known feature of the disease that it need not be dwelt on here.

Further, the patient's failure to appreciate the feeling of sidedness need not be general, but may be localised to certain mental processes. In Case A the right arm might be achiric at a time when the right leg was not, so that a stimulus applied to the former limb did not arouse the feeling of right-sidedness, while one applied to the latter did. The condition might, from the standpoint of memory, be regarded as a complete antero-retrograde amnesia of a localised form.

We may *sum up* the argument so far by saying that the essential feature of achiria is an amnesic failure to associate a given feeling of sidedness with mental process concerning different bodily members, that normally are associated with it, of such a kind that the presence of these in consciousness failed to arouse the feeling in question.

2.—*Nature of Allochiria.*

Four hypotheses of allochiria have previously been offered. I have dwelt fully elsewhere (32) with all except Janet's, and so may confine myself here to this one after briefly enumerating the others.

The first hypothesis, devised independently in 1880 by Fischer (6) and in 1883 by Hammond (8), and usually styled "Hammond's theory," is apparently still the currently accepted explanation of allochiria, as may be seen by a reference to Nothnagel's *Spec. Path. u. Ther.* (36), or to the numerous other authorities I have elsewhere cited (31). According to this hypothesis an ob-

struction in the afferent path causes a passage of impulses to the contralateral side, and the cerebral hemisphere they reach refers them to the side opposite to that of their origin. This conception has usually been confined to organic cases, though Weiss (42) in 1891 extended it to the hysterical cases by speaking of a functional block in the posterior columns, a view concerning which comment is superfluous at the present day. One of the many objections to this physiologically impossible hypothesis is the fact that allochiria has never been known to occur in the Brown-Séquard syndrome, even in those cases of this nature that have been examined with extraordinary care—as by Spearman (41), Head and Thompson (11), etc.—although the supporters of the hypothesis expressly maintain that the symptom should invariably occur in this condition.

The second hypothesis, emitted by Bosc (3) in 1892, postulated for the hysterical cases “an altered dynamic state of the cerebral hemisphere concerned, of such a nature that the impulses reaching it crossed through the corpus callosum to the opposite hemisphere, which therefore become endowed with the dynamic state of both hemispheres.” This hypothesis is a typical instance of the common assumption that translation of the psychological facts of hysteria into physiological language furthers our comprehension of those facts, and illustrates well the fruitlessness of the metaphysiology thus erected.

The third hypothesis, an amplification made in 1905 by Spearman (39) of a suggestion offered in 1893 by Head (9) is to the effect that in certain cases of erroneous localisation the mental images aroused by peripheral excitation are too faint to give adequate information about the site of the stimulus when these excitations, particularly those of the “articular” group, are diminished in intensity. Spearman (40) extends this explanation to the hysterical cases by postulating a hypothetical functional affection of the cortex, which impairs the incoming articular impulses. It is evident, however, from the writings of these two authors that they are really

discussing the question of alloæsthesia under the incorrect title of allochiria, neither having recognised the fundamental distinction between the two conditions. If Spearman were correct in attributing allochiria to a defect of articular excitations, we ought in allochiric cases to find the other manifestations—inco-ordination, etc.—that he has described as being characteristic of that defect; this is not the case.

In the fourth hypothesis, that of Janet's (20), both the nature and the mechanism of production of the phenomenon are fully discussed; we are concerned at present with only the former question. Shortly put, Janet holds that normally a stimulus evokes at least two images, that of the point stimulated and that of the corresponding point on the contralateral side; and that allochiria consists in the undue prominence of the latter, for a reason that we shall presently discuss, and a suppression of the former, brought about by hypoæsthesia, of either mental or organic origin. The last-mentioned feature is a cardinal basis of the hypothesis. He sums up by saying (24): "Il reste vraisemblable que l'allochirie même complète (allochiria) résulte comme l'allochirie simple (achiria) de l'hypoesthésie. C'est la diminution de la sensibilité consciente qui, après avoir supprimé la distinction délicate des signes locaux symétriques a amené la confusion et l'association de ces signes," and further (25) that "il faut à mon avis la rechercher, chez les malades qui pour une raison ou pour une autre ont présenté pendant assez longtemps un degré assez accentué d'hypoesthésie et non d'anesthésie complète," and that "les détails de cette observation ont montré que l'allochirie dépendait d'une anesthésie plus ou moins profonde quelle qu'en soit l'origine." It follows from this view that, according to Janet, who did not distinguish between alloæsthesia and allochiria, or recognise the essentially hysteric nature of the latter condition allochiria may arise from any affection of the nervous system, either functional or organic, provided that the requisite degree of hypo-æsthesia is present. He implies

this throughout, and in the case of achiria definitely commits himself to this opinion (19).

We thus see that previous writers have been unanimous in the opinion that allochiria is the result of a faulty perception of incoming sensory impressions, this being due either to an organic defect in the physiological processes that are concerned with the genesis of such impressions, or to failure on the part of the mind properly to appreciate them when they are in themselves normal. In contradistinction from this universally accepted opinion I venture to maintain, in the light of the above detailed observations, that allochiria is totally independent of any defect in the perception of incoming impressions, but is, on the contrary, the result of an erroneous association of certain groups of memory processes. In Case A it was described how typical allochiria was present at a time when the sensibility was exactly as acute on the affected as on the unaffected side, and was, so far as the most rigorous tests could determine, of normal acuteness. The complete lack of correlation between the dyschiric manifestations and the varying defects in sensibility in Case B is equally unequivocal in this respect.

I would here call special attention to the fact that in previous discussions on the pathology of allochiria interest has been focussed on the sensory phenomena to the exclusion of the motor and mental. These yield more important clues than the former, and show clearly the essential part played in the process by the feeling of sidedness, which I have termed "chirognostic" feeling (30). Victor Henri indeed went so far as to suggest (12) that allochiria might be dependent on a primary movement defect. There is, however, no evidence in support of this view, though it has perhaps this of truth in it—that the chirognostic feeling of a member may normally be associated more with the individual's memory of its motor functions than with his memory of its sensibilities.

The subject must therefore be viewed from a broader

standpoint than that of sensibility, and we thus see that in allochiria certain stimuli, whether perceptions or conceptions, arouse a body of memories together with a feeling of sidedness that normally is not associated with them. For example, in Case A, drawing the patient's attention to his right arm, whether by asking him to use it or by stimulating the limb directly, aroused together with the memories of this limb—its functions, etc.—the feeling of left-sidedness instead of the normal one of right-sidedness. It is to be remarked that the reverse mechanism never obtained; that is to say, that recalling the feeling of left-sidedness never aroused the memories concerned with the right limb.

To sum up, the essential feature in the nature of allochiria is the presence of an abnormal association between a given feeling of sidedness and the memories of a contralateral part, of such a kind that any recalling of the latter simultaneously arouses the former in consciousness.

3.—*Nature of Synchronia.*

Two cases only of synchronia have been previously recorded, by Ferrier (5) and Janet (18). Janet's explanation, the only one so far offered, is an extension of his image hypothesis that will be considered presently in connection with the pathogenesis of allochiria. He says further (24) that "la synchronie, lorsqu'elle est consciente, exige une sensibilité plus délicate que l'allochirie simple."

Here again I would maintain that as synchronia synchronised in Case A with an absolutely normal sensorial acuity, no defects of sensibility are necessary for the production of it, so that an explanation must be sought elsewhere.

After the preceding remarks on allochiria it will suffice here to state that the essential feature in the nature of synchronia is the presence of an abnormal association between the memory processes relating to a given part and the feelings of right and left sidedness, of such a

kind that any recalling of the former simultaneously arouses both the latter in consciousness.

IV.—*Pathogenesis of Dyschiria.*

Again it will be convenient to discuss the individual condition *seriatim*, and afterwards to review the group as a whole.

1.—*Achiria.*

We start from the position that this state consists essentially in an amnesic failure to associate a given feeling of sidedness with certain mental processes that normally are associated with it. As was hinted at above, whenever we meet with a unique and highly specialised mental loss, of the peculiar nature above described, we shall be right in suspecting the presence of hysteria. Therefore on enquiring further as to the precise nature of this specific inability on the part of the conscious mind to apprehend the given feeling of sidedness we at once reach the warmly contested field of the pathogenesis of hysteric symptoms. It is no part of my purpose here to enter into that tourney, and I shall content myself with pointing out the resemblances of the achiric losses to other hysteric losses. That the defect is what Janet (14) has termed a psychological disaggregation, or what perhaps might preferably be styled a failure in physical assimilation, is abundantly shewn throughout the present study; when, for instance, the achiria was partial and limited for example to the right arm, the patient could conceive of the feeling of right sidedness in connection with the right leg, but could not associate it with the feeling of the right arm even when this was stimulated. Further, that the failure was caused by a repressed feeling-complex, of the type that Freud (7) has shown to be characteristic of hysteric symptoms, was clearly to be demonstrated in Case A, the full psycho-analysis of which I hope later to publish, together with a discussion of the mechanism whereby these complexes brought about achiria.

The only other question that will be here considered is the relation of achiria to the other amnesias that were present in connection with the parts of the body. In order to do this a few preliminary remarks are necessary about the mental processes that concern any given part of the body. These are sharply divided into two groups. First, there are the æsthetic sensibilities that depend on the excitations flowing in from the bodily member *at any given moment*; they are subdivided into (a) the sets of common sensibilities (touch, pain, heat, cold, etc.) that have to do with the immediate relations of the member to the external world, and (b) the cœnæsthetic sensations, largely sub-conscious, that have their origin in the functioning of the internal organs. Secondly, there is the group of memory feelings that originate in all the diverse mental processes that *in the past* have had to do with the member in question; to mention only a few of these, there are the memories relating to its functions, both motor and sensory, those relating to its appearance, sidedness, position, and all that the member stands for to the individual; in short, all the memory feelings that can be aroused by the sight, mention, touch, or thought of the member in question. This group, to which I have applied the term "autosomatognostic" (34), has often in the past been confused with the totally different group of cœnæsthetic sensibilities.

The first of these two groups can be affected by either organic or functional disease, the second only by functional. No organic lesion, not even amputation of the limb, can affect the past memories that form the autosomatognostic group. We thus see that in functional disease two totally different forms of anæsthesia may occur, according to whether the dissociation implicates both groups or only the first. An important difference between the two forms is the different amount of distress caused to the patient. As is well known, the common type of hysterical anæsthesia causes so little conscious discomfort as to be commonly unknown to the patient. In sharp contrast to this is the distress, or even misery,

endured by patients who have in addition an autosomatognostic defect; the distress is largely due to the impairment of the sense of personality with which the group is intimately connected, and indeed the symptom is usually described under the name of partial depersonalisation. It is plain that the autosomatognostic amnesia is fundamentally different from any sort of simple anaesthesia; it is a complete gap in memory; a whole group of past experiences is wrested from the patient's consciousness as completely as if they had never been.

The intimate correlation between achiria and the intensity of the autosomatognostic defect was indicated in the description of Case A. Full consideration of the relation of chirognostic to autosomatognostic feeling is impossible here, but the following remarks may be made on the pathological aspects of the problem. The majority of cases of anaesthesia presenting an autosomatognostic defect recover in their progress to restoration both groups of mental processes synchronously. In the cases of this nature that I have examined I have never found any indication of achiria, or of any other dyschiric manifestations. On the other hand, in the above related cases the recovery of the aesthetic group was evidently in advance of that of the autosomatognostic group, and the question naturally arises as to whether this is necessarily so in all cases of dyschiria; and further, as to whether dyschiria necessarily arises when these conditions are present. In other words, when cleavage between the two groups occurs—of the kind just mentioned—is achiria (or any secondary dyschiria modification of achiria) a necessary consequence; and secondly, can achiria arise in any other way? The answer to the former problem is probably, and to the latter possibly in the affirmative; so far as I know, there is no positive evidence deciding the points.

With regard to the first question, it is clear that if the affirmative answer is correct, i.e., that achiria must occur whenever their cleavage between the two groups occurs, then only negative evidence is obtainable. On

the other hand, if this answer be wrong, then a single case showing this cleavage but no achiria would be sufficient to prove it so; no such case has, to my knowledge, been published. Chirognostic feeling seems to be an integral part of the *autosomatognostic memory group*, and is certainly associated with it more intimately than with the *æsthetic group*, as is shewn by the mere fact of the non-occurrence of achiria in the common type of cleavage between the groups, i. e., in simple anæsthesia (or rather *hypoæsthesia*), whether *organic or functional*, when the *æsthetic group* is abrogated but not the other. If that is so, it follows that achiria must necessarily occur whenever the rarer type of cleavage—which, from its being characterised by the individual's capacity to feel with a part of his body that he cannot feel, might well be termed the *paradoxical type*—occurs. Further, in a study of the pathogenesis of *phrictopathic sensation* (33), I have tried to shew that this syndrome is produced by the same *paradoxical type* of cleavage, so that the three syndromes of *achiria*, *phrictopathic sensation* and *paradoxical suppression of autosomatognostic memories* are intimately associated one with the other, and in all probability form a unitary morbid state.

With regard to the second question, as to whether *achiria* can arise in any other way than as a result of the above mentioned *paradoxical cleavage*, it is of course conceivable that an isolated dissociation of *chirognostic feeling* may occur apart from any other symptom, for this is a possibility characteristic of a certain type of *hysteric symptoms*. It seems to me, however, an exceedingly unlikely occurrence, for the reason that when the repressing mechanism of the underlying complex bears on the *chirognostic feeling* of a given part of the body it will probably not succeed until the other, closely associated, *autosomatognostic memories of the part* are also implicated—a secondary phenomenon allied to what *Riklin* has shewn to be the essential one operative in the causation of *Ganser's syndrome* (37).

Summary: Achiria is a psychological dissociation of chirognostic feeling, a di-aggregation of the kind characteristic of hysteria. It is probably always associated with what is here called the paradoxical type of cleavage, i.e., dissociation of the autosomatognostic memories of a given part of the body unaccompanied by abrogation of the æsthetic sensibilities of the part.

2.—*Allochiria.*

It is first necessary to complete the summary of Janet's hypothesis. The non-acceptance of this author's view concerning the essential nature of allochiria does not necessarily carry with it the rejection of his explanation of the mechanism of the phenomenon, and the arguments he uses are so plausibly ingenious as to demand a detailed consideration.

In 1893, Head (9) suggested that normally a cutaneous stimulus arouses a complex of sensations and memories, which he shortly terms a "mental image," in connection not only with the point actually stimulated, but also with neighbouring points, though the images corresponding to the latter points are suppressed and never become conscious. He further pointed out that the corresponding contralateral point must be regarded as the one of this series of neighbouring points that physiologically is most intimately related to the point stimulated, Janet (23), adopting this hint, thinks it probable that during the achiric stage, when, in a bilateral case, no difference can be detected between symmetrical stimuli, the intimate relation between the corresponding sensations—so intimate that they cannot consciously be differentiated by the patient—leads to an unusually close association between the mental images of corresponding points on the two sides of the body. He further thinks that, as a result of this unusually intimate association between corresponding contralateral images, a stimulus applied to an allochiric part cannot evoke the proper image of the point stimulated without evoking at the same time that of the corresponding contralateral point. He

explains the fact that the patient perceives only one image by the contraction of the field of consciousness, whereby a hysteric is unable to apprehend more than a certain number of simultaneous impressions. The further question why the patient invariably perceives only the wrong image Janet acknowledges to be very obscure; the only suggestion he offers (24), apart from some speculative remarks about "obsessions of contrast," is that perhaps the contralateral image arises a fraction of time later than that of the side touched, and that this latter, being earlier in time, fades and is forgotten or ignored, while the second one—that of the opposite side—is apprehended in consciousness.

It would be easy to adopt a hostile attitude towards this hypothesis on account of several defects, such as its over-concentration on the sensory features of the phenomenon, but there is no doubt that a sympathetic critic could find much to support it in the study of Cases A and B, recorded above. For instance, Janet's assumption that a stimulus applied to an allochiric part evokes two "images" is borne out by the fact that Patient A could, under favourable circumstances, actually observe the two sensations.* It is true that habitually the patient perceived not the contralateral sensation above, as Janet supposed, but one that resulted from a fusion of the two sensations, though it had the "sidedness" of only one of them. Further, it is not the case that the contralateral sensation arose later, as Janet assumes, nor is it probable that the homolateral sensation could fade and be forgotten in the way he describes, for in fact its duration is very substantially greater than that of the contralateral image. Still these observations could possibly be assimilated to the hypothesis without rendering necessary any very profound modification of it.

However, there are even graver objections to Janet's hypothesis than the ones just mentioned. They arise

* I use this term, in its clinical sense, as distinct from the strict psychological sense of "pure sensation," in preference to that of "image," as it seems to me to involve fewer intellectualistic implications than the latter.

mainly from a comparison of unilateral with bilateral allochiria, about which a word is necessary. It is improbable that such a peculiar and specific condition as allochiria can be brought about in quite different ways. Important differences between the mechanisms of production of the two varieties may exist, but it is unlikely that these are fundamental. We shall therefore be right in asking of any hypothesis that it be equally capable of explaining both varieties of the condition, and also in giving preference to that one of alternative hypotheses which best includes both these varieties. For instance, an hypothesis might be invented to explain Case A based on Head's suggestion (10) that the image corresponding to the site of greater sensibility is perceived. It might be said that the patient's true left side felt to him much more distinct than the other "dead" side, and that his feeling of left-sidedness was on this account so vivid, as compared with that of right-sidedness, that it was attached to all sentient impressions, irrespective of their real origin. Apart however from the fact that Head refers here to the æsthetic sensibilities, which are not necessarily impaired in allochiria, it is clear, as Janet remarked (22), that this hypothesis would be tenable only in the case of the unilateral variety; the transference that constantly takes place *in both directions* in a case of bilateral allochiria cannot of course be explained in this way. We are thus justified in discarding such an hypothesis unless it be first shewn that the two varieties of allochiria are totally different from each other.

Now in connection with Janet's hypothesis several of the observations made in the unilateral Case A may perhaps with an effort be fitted in, as hinted above, but there are others that are quite inconsistent with it. Let us compare certain features of Case A with the corresponding ones of Case B. In both cases, if the patient was touched on the right arm he declared that he had been touched on the left arm, and if he was asked to move the right arm he always moved the left without knowing that he had made any mistake. But here the resemblance

ceases. For if one touched both patients on the right arm and simply asked them to move the arm touched, B moved the right arm, while A moved the left arm; in other words, B's movement was correct though his naming was wrong, while both A's movement and his naming were wrong. Apply the image hypothesis to this experiment. According to Janet both patients named the side incorrectly because they perceived not the image of the point touched, but that of the corresponding contralateral point. But in neither case was this true. In Case B all the evidence—from the way in which the patient indicated the limb touched, from the numbering of successive bilateral stimulations, etc.—went to shew that the patient experienced the sensation corresponding in fact to the point touched; his error consisted in thinking that that limb was the left one. His reference of the sensation to a given point in space was correct enough, though according to the image hypothesis it should have been faulty. If for instance he was sitting between a window on his right and a table on his left, he was sure that the touch was in the "window" side of him, as of course it was. In other words, he did not experience a sensation on the opposite side, he experienced it on the correct side, but named this side wrongly. Quite otherwise was it with Patient A. He did experience the sensation on the wrong side, and under the above circumstances was quite sure that the touch was on the "table" side of him. But he experienced two sensations, both equally on the left, one on a "dead" part and the other on an "alive" part. According to the image hypothesis he should have experienced only the latter, a hallucinatory sensation of the wrong side. But in fact what he felt was mainly the former, as was shewn by its duration, etc.; the sensation on the "alive" side was quite momentary, as in the normal, but the full sensation experienced did not begin to fade in intensity till half a minute later. Again, if the image hypothesis were a correct explanation, the patient should not have been able to distinguish between a touch applied to the right side

and one applied to the left, for his sensory acuity was equal on the two sides, and he was equally sure of the "leftness" of both; but though he habitually made no distinction between two such touches, he was quite able to do so with certainty, and there were marked differences in the features of the two sensations, as was mentioned in the description of the case.

Further, it is evident that the previous pathologically intimate association between the symmetrically opposite images, the assumption of which lies at the basis of Janet's hypothesis, cannot occur in unilateral achiria even if it does—and there is no evidence in support of this—in bilateral achiria. Nor is there any evidence to shew that achiria often or even ever precedes allochiria.

It therefore does not become necessary to consider the many objections to the hypothesis in connection with bilateral allochiria—Janet himself with his usual scrupulous scepticism confesses the extreme tenuity of his suggestion, which was offered *faute de mieux*, as to why the wrong image is invariably perceived—for the above considerations shew that there are insuperable difficulties in the way of the application of it to the unilateral affection.

Finally, it may be remarked that the inadequacy inherent in Janet's image hypothesis originates in its basal assumption that the allochiric reference of sensation is a variety of alloæsthesia, i. e., an error in localisation due to hypo-æsthesia. He has maintained this opinion from the time of his first article on the subject, entitled "Une altération de la faculté de localiser les sensations" (13), whereas it is here advanced that allochiria is not an alteration of this faculty, nor is it due to hypo-æsthesia. The hypothesis thus resembles those of Head and Spearman as being in no way applicable to allochiria, for it is in fact an explanation not of this condition, but of alloæsthesia. It becomes necessary, then, to find a new point of view from which the problem can be examined afresh.

As was previously stated, it is important to view the manifestations of allochiria, including the introspective

aspects, as a whole, and not to confine one's attention to the sensory phenomena. If now from this standpoint we consider the transition from the achiric to the allochiric stages in Case A, we note two especially prominent features. To the patient the change meant essentially, two things, one dependent on a gain in dextrality, the other on a gain in autosomatognostic feeling. In the first place he was again aware of a whole group of motor functions—dextral acts—the meaning of which he had previously forgotten. In the second place, we can hardly doubt that what we described in the allochiric stage as a "strange dead half on the left side" was, in fact, the true right half of his body incompletely felt. This partial recovery of autosomatognostic feeling was always accompanied by a striking change for the better in the patient's sense of well-being. Not only was the bulk of his bodily feeling multiplied by two, but other qualities seemed similarly to expand; thus his courage of feeling of power, strength and capacity also grew in the same moment. We see, then, that what may be called the total assimilative capacity of the patient, or what Janet calls the capacity for mental synthesis, had decidedly increased in the transition from achiria to allochiria.

These considerations introduce the suggestion that perhaps the erroneous allocation of chirognostic feeling, described above as being the essential characteristic of allochiria, is in some way a process whereby the just-mentioned gain in assimilative capacity may be brought about. Such replacements are well known in hysteria under the name of transference phenomena. The mechanism by which they occur is too large a subject to be discussed here, but it may be remarked that they represent to a patient an advantageous compromise whereby one defect is replaced by another less unendurable one. A clinical fact that seems to be fairly well established in this connection is that this interchange of symptoms is a process which occurs much more readily

in some patients than in others, a matter that has a considerable bearing on prognosis.

This hypothesis again raises, from another point of view, the question of the relation of chirognostic to autosomatognostic feeling. The observations detailed in the above cases show clearly that there are numerous degrees in the extent to which the latter feeling may be lost. Such variation is never observed with chirognostic feeling. Either it is surely present in all its distinctness, or it is totally absent; there seems to be no stage between the two, and it is certain that the conviction with which a given chirognostic feeling is experienced is altogether independent of the question whether it is correctly or incorrectly associated. Now the presence or absence of chirognostic feeling makes all the difference to the capacity to recall autosomatognostic feeling, i.e., it has a most important influence on the individual's capacity to feel a given part as an integral part of his personality. It seems to be well-nigh impossible for a normal individual distinctly to feel just "This is my hand"; he can feel only "This is my right hand" or "This is my left hand." In other words, "hand," apart from "sidedness," is an intellectual conception and does not correspond with direct personal feeling. It would further appear that the appreciation of an inappropriate chirognostic feeling is effective in this respect in the same way as that of an appropriate one, though not by any means to the same degree. An erroneous association of an inappropriate chirognostic feeling is thus a makeshift, the only one possible under the circumstances, whereby an achiric patient can recover the memories of a given part, together with the knowledge of its functions. In Case A we might say that the patient had no option between feeling nothing of the right half of the body—and suffering all the serious disadvantages that ensue from this loss—and feeling it on the left side. Allochiria presented, then, a means of escape from the disadvantages of achiria. For the mechanism to be successful it was

essential that the erroneous association should be firm; the patient had to feel with the utmost conviction that the right limbs were on the left. Any trace of doubt on the matter brought back to his mind the strange depersonalised feeling, which was so intensely unpleasant. The strong instinctive tendency to cling to the feeling of certitude offered by the transference amply accounted, therefore, for the patient's aversion to dissecting the feeling of his so-called only left side, and for the difficulty I experienced in getting him to analyse the differences between the two left sides.

It will be noticed that we cannot call the erroneous allocation of "sidedness" the simple absence of a given function, as we can the other hysteric defects referred to, but it has in common with these the feature of being an obvious disadvantage and drawback in itself, a penalty paid in order to secure another advantage. Really its very appearance of meaninglessness as a phenomenon in itself should suggest some process of a compensatory nature, should hint that it occurs in order that something else, of a useful nature, may occur. To anyone inclined to regard it from this teleological standpoint the main difficulty must hitherto have been in seeing in what possible way such an apparently meaningless occurrence could have any function, in seeing what gain could accrue, however, indirectly. I am convinced that the clue to this mystery lies in the relation of chirognostic to autosomatognostic feeling. To the patient under consideration the endowment of the feeling of the right half of the body with left-sidedness was, so far as it went, merely an embarrassing awkwardness, but it had the solid advantage of enabling him to feel something of his right half and its functions; it was in this respect the next best thing to endowing that feeling with right-sidedness, a consummation that was prevented at this stage by the completeness of the loss of "right-sidedness."

Contrast of Unilateral and Bilateral Allochiria.— Before discussing the reasons why unilateral allochiria should arise in one case and bilateral in another, it is

first necessary to define the actual differences between the two conditions, beyond the mere fact that one is unilateral and the other bilateral.

In the series of observations, mentioned above in the discussion of Janet's hypothesis, the differences noted between the two cases were mainly three: (1) A referred a point touched across to a point on the other side of his body; B referred it to its correct position in space, a position which, however, he happened wrongly to call left. (2) A felt two sensations, both on the left side, one phrictopathic, the other normal; B felt only one, a phrictopathic one. (3) At a time when both motor and sensory allochiria were shewn to motor and sensory tests applied separately, if these were combined by first touching the right arm and then asking the patient to move the arm touched the following difference was seen: A indicated the arm incorrectly both in speech and in movement by calling it left and moving the left; B indicated it incorrectly in speech but correctly in movement by calling it left but by moving the arm actually touched.

In generalising from these observations we may say that the essential difference between the two cases was that A felt that his right side was actually displaced "over on to the left side," as he often expressed it, whereas B did not; B's error was merely one of incorrect naming, and was one that he did not recognise. In other words, A appreciated not only that something was wrong, but exactly what was wrong; B didn't know that anything was wrong.

This explanation seems to me to account for the three above-mentioned differences. The *first* one is a direct corollary of it. The *second* difference may be otherwise expressed by saying that both patients experienced a phrictopathic sensation, but patient A experienced in addition a normal sensation. The phrictopathic sensation is probably best explained as being the sensation arising as the direct result of stimulation of a bodily member, the autosomatognostic memories concerning which are dissociated to a greater extent than the

æsthetic sensibilities ("paradoxical cleavage"). The normal left-sided sensation that patient A experienced in addition appears to have been an hallucination, the function of which was, by practically fusing with it, to confer left-sidedness on the originally sideless (achiric) phrictopathic sensation; in accordance with this is the fact that the normal sensation showed the same de' *ŷ* as the phrictopathic, so that both always appeared synchronously. Janet's description of allochiria transference as a kind of hallucination (21) is, according to this view, legitimate only so far as the unilateral variety is concerned, for the following reason. In a bilateral case there is no normal side to serve as a standard; both sides are incorrectly named. Therefore, when a touch on the right arm is called left-sided there is no memory or knowledge of any other kind of left-sided touch that might conflict with this and introduce doubt. The matter is not so straightforward in the unilateral case. Here the memory of a true left-sided touch could soon dispel the belief that a touch on the right arm was left-sided, but for the fact that it itself is also experienced as an accompanying hallucination. This occurrence is no doubt aided by the perfect superimposition of the two halves of the body that the patient experiences on introspective examination. The *third* difference is similarly explicable on this view. When the right arm was touched and the patient asked to move the arm touched, A moved the left, but B moved the right, though both thought that the left arm had been touched. It might at first sight be thought that this difference was due to the fact that both patients were trying to move their left arm, which they thought was the one touched, for their mode of response to this attempt was certainly different, namely, when A was asked to move his left arm he did so correctly, whereas B under the same circumstances moved his right. This is probably a correct explanation of the phenomenon so far as B is concerned, but with A it would be more accurate to say that he was trying to move, not his left arm, but *one* of his left arms, the

"clever" one. It will be remembered that A could not move his right arm when a conscious effort was employed; he got the feeling of moving his "clever" arm only by actually moving his real left arm in a certain way. I would therefore attribute the difference in question to the fact that A used the left arm, in conscious efforts for both the "clever" left and the real left; it was the former he was moving in the test in question. Looked at in this way, it will be seen that the difference is less than at first appears, and, in fact, it may be said that both patients moved the arm on which they felt the touch. B felt the touch on what he called his left arm, and therefore moved what he thought was his left arm; A felt the touch on what he called his "clever" left, and therefore moved what he thought was his "clever" left.

Further consideration of the above generalisation well illustrates how inaccurate it is to describe allochiric transference of sensation as an error in localisation. In both cases the stimulus is correctly localised, not only in other dimensions, but even as regards the sides in the sense that the patients knew which half of the body was touched. This statement refers to even the response to a simple stimulus, and not merely to the combined test that we have just been discussing. Patient A, when touched on the right side, knew it was his "clever," "dead" side that had been touched, and not his "real" left; patient B, under similar circumstances, was equally sure it was his right side, as indicated in every way except by naming it. Both patients erred in calling the touch a left-sided one, and felt it to be evidently left-sided, but for different reasons; A because he felt that his "clever" side had been swivelled round a vertical axis on to the left side, B because he directly felt the side in question to be a left-sided one. With B a complete transference between the two sides, including all their functions, had taken place; with A only a folding over of the right side on to the left. Both patients, therefore, knew surely which side had been touched, and erred, not in this perceptive process, but in the subse-

quent mental processes that occurred when they tried to indicate the side of the part touched. If they were left to themselves and allowed to indicate the side in their own phraseology, and by various adjuvant devices, no mistakes were made, error creeping in only when the nomenclature of sidedness was reached. Put more summarily, both patients knew correctly which side had been touched, though they were wrong in their opinion as to which side this was; they knew the half of the body, but not on which side it was.

3. *Synchiria.*

Janet's image hypothesis, which he applied also to dyschiria, need not be further discussed here, as it has already been fully considered in connection with allochiria.

Synchiria is evidently a state in which the production of the erroneous allochiria association is occurring at a time when the normal association is still intact. The peculiarly transitional nature of the symptom will be pointed out when we consider the relation of the different members of the dyschiric group to one another.

The chief interest of the condition for pathology is the additional light it throws on the mechanism of allochiria. If the teleological explanation of allochiria offered above is valid, then it might be said that the patient can tolerate in consciousness the feeling of a given part of his body only on condition that it is felt on the opposite side. In synchiria we see the beginning of this process. Stimulation of the right arm arouses memories of an arm that can be accepted as right-sided only if it is at the same time left-sided. The strangeness and incomprehensibility of this feeling accounts for the patient's repugnance to the condition. This he thought was due to his lack of control over the limbs concerned; as long as he experienced the unsettled feeling described above so long did he feel unsure about the use of the limbs and about his capacity to perform the functions relating to them. Allochiria then meant to him essentially the exchange

of insecurity for certitude. This illustrates the great importance that synthesis of chirognostic feeling has for autosomatognostic feeling, for although the feeling of the limbs concerned was more vivid and evidently nearer the normal in the synchronic stage than in the allochiria, yet the patient greatly preferred the latter on account of its definiteness and feeling of security and controllability. The sensory features also accord with this point of view.

A cutaneous stimulus evokes two sensations in synchronia whether this is unilateral or bilateral, but in allochiria only if it is unilateral. The explanation of this was in part given when the two forms of allochiria were contrasted. An hallucinatory contralateral sensation is necessary to arouse the abnormal chirognostic association only when the memory of such a contralateral normal sensation might compete with the homolateral (in synchronia) or sideless (in allochiria) phrietopathic on one, and hence prevent the consummation of the illusion. This is so in both forms of synchronia and in the unilateral allochiria; not, however, in bilateral allochiria.

4.—*Review of Dyschiria.*

Before reviewing the pathogenesis of the dyschiria manifestations as a whole it will be convenient shortly to consider two further questions here.

(a) *Relation of the Dyschiria Stages to One Another:* It will be sufficiently evident from the description given of Case A, or from that of Janet's case (16), that achiria represents the stage of most advanced disaggregation and synchronia that of least, so that the arguments that might be brought in support of this statement may be omitted.

At first sight there seems a perfectly even course of events between the normal in the one direction and complete anaesthesia in the other, and I have frequently watched the evolution of all the dyschiria stages take place in as short a time as a couple of minutes. On closer examination, however, it can be observed that there is a point at which this apparent evenness shews a sudden break,

a phenomenon that has an important bearing on the theory of allochiria. When in the synchiric stage of Case A the right side felt as though it were drifting in an indefinite medium, being neither on the right side nor on the left, the patient's general condition of disagreeable insecurity was about the same in all the sub-stages, as now, it passed over to reach its safe anchorage on the left, instantaneous superposition would occur in a way that almost suggested the snap of a lock, and the patient, having found security in allochiria, would sink back with the profound sigh of relief given by one who again exchanges doubt for certainty. In passing from allochiria to the normal it was so difficult to maintain synchiria for the time necessary to progress to the normal that often the patient would slip back again, as with a click, into the stable resting stage of allochiria. This evanescent character of synchiria probably explains why the condition has been previously observed in only two of the twenty-nine cases of allochiria on record.

Similar features are observable in the case of achiria. The passage from achiria to allochiria occurred with the same dramatic suddenness as that from synchiria into allochiria. Further, during the passage through the achiric stage the patient shewed the same tendency to slip back into complete anæsthesia as he did to slip back again into allochiria when traversing the synchiric stage. Achiria is thus also a temporary stage that has to be traversed in order to pass from complete anæsthesia to allochiria, though it is more unpleasant than either of these. This explains the rarity of the condition, which no writer other than Janet has previously recognised.

The conclusion is thus reached that allochiria is the only stable variety of dyschiria, so that if a permanent defect in assimilation of chirognostic feeling exists it will find expression in allochiria. This accounts for the long duration that may be seen in cases of allochiria; in Janet's case the syndrome was present at different times over at least twelve years, and patient A spent seven years of his life fluctuating between the anæsthetic and allochiric

stages, the latter occupying the greater part of the time. Achiria and sychiria are essentially transitional stages of an unpleasant nature that have to be traversed in order to reach more satisfactory states. Whether they are necessary stages is not known, but it is certain that they may last for a very brief period, as short as a few seconds. It is highly probable, therefore, that if achiria or sychiria is observed in a given case, allochiria will surely be observed at some other time, but the contrary by no means certainly follows.

(b) *Difference between Unilateral and Bilateral Dyschiria:* In the discussion in connection with allochiria the conclusion was reached that the essential difference between the unilateral and bilateral cases lay in the illusion of displacement, whereby one-half of the body seemed to be folded on a vertical axis until it coincided with the other half, being conscious in the former case, whereas the illusion of interchange between the two halves of the body in the latter case was throughout unknown to the patient. The question immediately arises as to whether this difference is one that holds good in other than the present cases. Unfortunately I know of only one instance in the literature where allochiria is more than mentioned; in that, the bilateral case published by Janet, there is no evidence that satisfies me on the points in question. It is not certain even whether the patient merely named the side of a touch wrongly or whether she felt it on the opposite side. She seems to have recognised the error as soon as she looked at the limb touched, in which respect she perfectly resembles patient B, but it is not mentioned whether she could then continue to feel the allochiric error and introspectively discover what was wrong, as could patient A, or whether she became so confused, as did patient B, by the intolerableness of the situation that she had to end it one way or the other.

Pending further experience, therefore, judgement must be reserved on this question, but if the differences noted above are found to hold good in further cases, then we

may have in them a clue to the obscure problem of why unilateral allochiria occurs in one case and bilateral in another. The two facts I would bring together in this relation are that: (1) The illusion in the unilateral case violently conflicted with previous experience, that in the bilateral not at all; and (2) neurasthenia complicated the latter case and not the former.

With regard to the first point it is unnecessary again to discuss the striking contrast between the two cases in this respect. Patient A, it is true, was exceedingly puzzled by the bizarre phenomenon of feeling two distinct halves of the body on the same side, but he accepted it, discussed it at length, and lived on the assumption of it for seven years. Patient B, on the other hand, was unaware of his error, and at the least indication that anything was wrong, such as by his catching sight of the hand he was incorrectly naming, he immediately found the situation impossible. Thus A accepted a situation that grossly conflicted with all past experience, while B was quite unable to reconcile the two.

Taking next the second point, we recognise that clinically there is a gradation in cases of hysteria according to the extent to which symptoms may arise that conflict with previous experience. It is familiar how amazingly resistant of contradictory experience certain hysterics are, an observation especially often made on patients in the state of hypnosis. If, for instance, a fixed idea is produced in the patient's mind that he cannot see a given individual, this individual may shout in his ear, come into physical contact with him, and adopt any means to demonstrate his presence, all in vain; no experience, however grossly obvious, will be accepted which contradicts the fixed idea. The fact that such a patient will entertain the presence of two contradictory and mutually exclusive views, and yet not allow the contradiction to have the least influence on his attitude, is equally striking. At the other end of the scale from this is the neurasthenic, whose symptoms are always to him comprehensible enough. Insomnia, tiredness, dyspepsia and

headache are facts more easily reconcilable with past experience than feeling both halves of the body on one side. I have tried elsewhere (28) to define these differences in terms of the psychological disaggregation, describing that present in hysteria as "massive," and that in neurasthenia as fragmentary or "molecular."

One is thus led to speculate as to whether the two forms of allochiria depend on the clinical variety of hysteria present, the bilateral one occurring in the variety that most approximates to nearasthenia. In support of this the observation may be referred to that whenever patient B's neurasthenic symptoms diminished the allochiric manifestations were present even in the waking, more conscious, state, provided he was not actually looking at the limbs in question. If this suggestion be valid, it will make intelligible the strikingly greater facility with which introspective analysis could be carried out in the clear definite mind of patient A; this was done to an extent that was hopeless to attempt in the confused and neurasthenic mind of patient B.

It will be noticed that in this argument it has been assumed that bilateral allochiria conflicts with experience to a greater extent than does the unilateral variety. The justifiability of this assumption will, I think, be apparent from reflection on the introspective aspects of the conditions. It is difficult, if not impossible, to conceive of a case of unilateral allochiria in which the condition could escape direct introspective observation. On the other hand, bilateral allochiria must totally escape introspective observation until some accessory circumstance, such as sight, reveals the error; patient B, for instance, was almost always allochiric in the dark, but only rarely in the light.

Whatever may be the final explanation, however, the distinction between the two classes certainly seems to be very sharp. In an experimental study, an account of which I hope later to publish, it was found that all attempts to induce bilateral dyschiria in a unilateral case, and *vice versa*, met with only the most limited success.

As far as my experience goes, any given case definitely belongs to one or the other class throughout its whole course, and never shews any tendency to pass from one class to the other. When the dyschiric process starts it is immediately decided to which class the case in question will belong, once and for all.

On the other hand, it is likely that in both classes the defect is primarily a unilateral one, a fact of some importance for the analysis of the underlying complexes and for the necessary re-educative treatment. The ground for this view is that even in cases of bilateral dyschiria evidences of asymmetry are usually, and perhaps always, manifest. That this was so, for instance, in Case B, which was carefully investigated from this point of view, was put beyond doubt by the following observations.

(1) Introspectively the patient was certain that one side, which he erroneously called the right, was far less distinct in feeling than the other; he would say that it was more "bloodless and numb," and less "distinct and alive" than the other.

(2) Of two simultaneous stimuli, one on each side, only one was felt; this was the one actually applied to the left side. It will be remembered that the sensibility was of equal acuity on both sides.

(3) Of two successive touches, one on each side, one was felt more distinctly than the other; this also was the one applied to the left side. This observation as well as the last was made by means of the numbering test described in the account of the case.

(4) The abnormal association of left-sidedness was more firmly fixed than that of right-sidedness, as was shown, for instance, in Observation IV.

There is therefore a consistent body of evidence showing that the defect was in this case more pronounced on one side, viz., the left. Janet unfortunately makes no reference to any kind of comparison between the two sides in his bilateral case, nor apparently did he apply

any of the above tests. In one of Sollier's cases (38), however, there was clear evidence of asymmetry in distinction of feeling.

Dyschiria as a Whole: The evidence gained from the observations here recorded seem clearly to demonstrate that in dyschiria we have essentially to do with an affection not of sensorial acuity, but of synthesis of chirognostic feeling. In achiria certain mental processes fail to arouse a normal association of chirognostic feeling, in allochiria they arouse only an abnormal one, and in synchiria they simultaneously arouse both a normal and abnormal one.

The varying distribution of dyschiria can be readily described in these terms. Cases of allochiria occur in which the erroneous reference of sensation concerns only certain parts of the body, and only certain kinds of stimuli; for instance, as in Dobie's case (4) only painful stimuli may be referred, and not tactile. This may be expressed by saying that painful sensations failed to arouse the normal chirognostic association, whereas tactile ones succeeded, a dissociation highly characteristic of hysteria.

As read in the light of the observations recorded and the hypothesis offered in this paper, the pathogenesis of a case of dyschiria may be summarily described as follows. Dissociation occurs of the memories of one or more members on one side of the body, this, like all hysteric disaggregation, being due to the presence of a repressed complex. The suppression of chirognostic feeling is in this instance more intense than that of the other members of the autosomatognostic group. The æsthetic sensibilities may also be suppressed, but if so, they are subsequently recovered. It is impossible properly to recover the autosomatognostic memories so long as they are associated with the customary chirognostic feeling, but they can be recovered provided they are associated with the complemental chirognostic feeling, i.e., provided allochiria occurs. At this moment it is decided whether the allochiria be unilateral or bilateral, this perhaps depending,

as was above suggested, on the type of hysteria present. Achiria and synchronic stages occur in the transitory manner above described.

All dyschiric manifestations, therefore, are typically hysteric forms of reaction to the suppression of the chirognostic feeling of a given part of the body. In no affection is it truer than in hysteria that diagnosis should precede treatment, and elucidation of a case of allochiria, and therefore rational treatment of it, is impossible without a knowledge of the problems of dyschiria, a partial answer to which it is the object of this paper to contribute.

V.—Summary of Conclusions.

1. The anatomical hypotheses concerning allochiria are contradicted by all the facts bearing on the subject, and should be discarded. No psychological hypothesis concerning either dyschiria or allochiria has hitherto been given, for those offered by Head, Janet, and Spearman treat of allochiria as though it were a variety of alloæsthesia, i.e., an error in localisation due to a certain kind of hypo-æsthesia, and apply solely to the latter condition, the fundamental distinction between dyschiria and alloæsthesia not having been recognised.

2. Dyschiria is a form of psychological disaggregation of the variety characteristic of hysteria. It is primarily a defect in the synthesis of the feeling of "sidedness" (here called chirognostic feeling).

3. Allochiria is the only stable variety of dyschiria, achiria and synchronia being temporary and transitional stages, the former between allochiria and anæsthesia, the latter between the normal and allochiric. A patient who has dyschiria usually fluctuates between the conditions of anæsthesia and allochiria.

4. Achiria is an amnesic failure of association between certain mental processes (concerning different bodily members) and the chirognostic feeling that normally is associated with them; allochiria is achiria in which an abnormal association has been formed between

these mental processes and the complementary chirognostic feeling; synchiria is the co-existence of these normal and abnormal associations.

5. The mental processes relating to a given part of the body are divided into (a) the æsthetic group of perceptions, and (b) the autosomatognostic group of memory feelings, of which chirognosis is one. When disaggregation implicates the second group and the first is relatively or absolutely spared (a phenomenon here referred to as "the paradoxical type of cleavage"), then achiria—or any other secondary dyschiric manifestation—results, and any sensation following stimulus of the part concerned shows the phrictopathic attributes previously described.

6. When the paradoxical type of cleavage occurs, and return to the normal is prevented by the forces of the repressed complex causing the disaggregation, then the autosomatognostic memories concerned can be recovered only at the expense of replacing their constituent chirognostic feeling by its complement. It is thus suggested that allochiria subserves the junction of enabling certain autosomatognostic memories to be once more apprehended in consciousness, a consummation which is of marked benefit to the personal well-being of the patient. This teleological hypothesis of allochiria regards it as being akin to the transference phenomena of hysteria, in which one symptom is replaced by another less detrimental one.

7. It is probable that all cases of dyschiria are primarily unilateral, but immediately pass permanently into one of the two classes, and show no subsequent tendency to change. It is tentatively suggested that the class chosen depends on the type of mind concerned: unilateral dyschiria will occur in pure hysteria, bilateral dyschiria in hysteria complicated by neurasthenia. The essential difference in the mechanism of unilateral and bilateral allochiria is that in the former the illusion of displacement of the part of the body in space is accessible to intro-

spection, and thus involves a much grosser contradiction of previous experience than does the latter condition.

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FREUD'S THEORY OF DREAMS.*

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I should be setting myself an impossible task were I to attempt in the short time at my disposal to give even a mere description of this theory, let alone any account of the evidence on which it is based or any discussion of possible objections and rival views. This Freud has done fully, and yet concisely in his *Traumdeutung*, and no expository article can replace the study of this volume, nor can the theory be adequately stated more briefly than it is there. The aim of this paper, therefore, is a more modest one, in that it is intended merely to serve the purpose of bringing the theory to the notice of those psychologists who have not already examined it. Two especial claims that the theory has for the con-

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sideration of psychologists may perhaps be mentioned. In the first place the chief respect, and often the only respect, in which Freud's supporters and opponents agree is this: that if the theory developed in the *Traumdeutung* is true it carries with it a revolutionary change in our knowledge of the structure and functions of the mind, a change that has a considerable bearing on our conceptions of sociology, history, and the allied normative sciences; the importance then of carefully investigating this theory is obvious. In the second place no investigator, in any country, who has learnt the technique of the psycho-analytic method employed by Freud in the study of dreams, has yet reached any conclusions that fail to confirm the theory in all particulars; this fact in itself speaks for the finished state in which Freud gave the theory to the world.

Of psycho-analysis itself no account can here be given; it essentially consists in the collecting of the *free* associations that occur to the subject when he attends to a given theme and abrogates the selecting control over incoming thoughts that instinctively exercised by the conscious mind. If this method is applied to any component part of a dream, however senseless it may appear on the surface, mental processes are reached which are of high personal significance to the subject. These mental processes Freud terms the "dream thoughts"; they constitute the "latent content" of the dream in contrast to the "manifest content," which is the dream as related by the subject. It is important to keep distinct these two groups of mental processes, for on the appreciation of the difference between them rests the whole explanation of the puzzling riddles of dreams. The latent content, or dream thoughts, is a logical and integral part of the mental life of the individual, and contains none of the incongruous absurdities and other peculiar features that characterise the manifest content of most dreams. This manifest content is to be regarded as an allegorical expression of the underlying dream thoughts, or latent content. The distortion of the dream thoughts into the dream proper

takes place by certain well-determined psychological laws, and for certain precise reasons. The core of Freud's theory, and the most original part of his contribution to the subject, resides in his tracing back this distortion to a "censor" which interposes an obstruction to the becoming-conscious of unconscious psychical processes. This conception he arrived at through the analysis of various abnormal psychical manifestations, the symptoms of hysteria, *Zwangsneurose*, etc., which he found to be constructed on a plan analogous to that of dreams.

A dream is thus not a confused and haphazard congeries of mental phenomena, but a distorted and disguised expression of highly significant psychical processes that have a very evident meaning, although in order to appreciate this meaning it is first necessary to translate the manifest content of the dream into its latent content in the same way that a hieroglyphic script yields its meaning only after it has been interpreted. The mechanisms by means of which the manifest content has been formed from the underlying latent content are mainly four. (1) The first of these is called *condensation*. Every element of the manifest content of the dream represents several dream thoughts; it is, as Freud puts it, "over-determined." Thus the material obtained by analysis of a dream is far richer and more extensive than the manifest content of the dream. The condensation may shew itself in several ways. For instance, in a dream a figure may appear that is built up of traits some of which belong to one actual person and some to another, rather like a Galton's composite photograph. In this way a certain feature in common between several people or places may be expressed in the manifest content of a dream by the occurrence of a composite person or place built up in the way just mentioned; the feature in common, which in this case is the essential constituent in the latent content, need not be present in the manifest content. The feature in common may be one actually present in real life, it may represent another common feature, or it may re-

present the wish that there were such a common feature. Further, not only is every element in the manifest content connected with several elements in the latent content, but every element in the latter is connected with several in the former. In addition to this frequently associations exist between the different elements of the entire structure, so that this has often the appearance of a tangled network until the full analysis brings law or order out of the whole. At various points in the network the associations are especially close as though they formed particular points of junction. These points are intimately related to the most significant elements of the underlying dream thoughts. Lastly it may be remarked that the elements in the dream that shew the greatest sensorial vividness are those that have the most associations in the dream thoughts; they are in other words the best-“determined” elements. (2) The second distorting mechanism is that termed *displacement*. In most dreams it is found after analysis that there is no correspondence between the psychical intensity of a given element in the manifest content and the associated elements in the latent content. An element that stands in the foreground of interest in the former, and seems to be the central feature of the dream, may represent the least significant of the underlying dream thought; conversely an apparently unessential and transitory feature in the dream may represent the very core of the dream thoughts. Further the most prominent affect in the dream, hate, anxiety, and so on, as the case may be, frequently accompanies elements that represent the least important part of the dream thoughts, whereas the dream thoughts that are powerfully invested with this affect may be represented in the manifest content of the dream by elements of feeble affective tone. This disturbing displacement Freud describes, using Nietzsche's phrase, as the transvaluation of all values. It is a phenomenon peculiarly frequent in the psycho-neuroses, in which a lively interest or an intense affect may be found associated with an unimportant *idea*. In both cases a transposi-

tion has occurred whereby a highly significant idea is replaced by a previously indifferent and unimportant one. Often the association between the primary and secondary ideas is a very superficial one, and especially common forms of this are witty plays on the speech expression for the two ideas, and other kinds of clang associations. As is well known, this superficial association is usually, as Jung has well demonstrated, the cover for a deeper hidden bond of high affective value. This mechanism of displacement is the cause of the puzzling fact that most dreams contain so many indifferent and hardly noticed impressions of the previous day. It also explains much of the bizarreness of dreams, notably the remarkable incongruity between the intensity of the affect and the content of the dream; a person may in a dream be terrified at an object that is usually indifferent, and quite at ease in the presence of what should be alarming danger. (3) The third mechanism is that termed *dramatisation*. It is a familiar observation that the manifest content of most dreams is predominantly of a visual nature, so that it may be said that in this respect a dream resembles a theatrical presentation. This fact that the representation operates principally by means of visual pictures exercises a selecting influence on the mental processes that have to be presented in this special way. Under the same heading may also be included the special technical means employed by a dream to present the logical relations, casual connections, judgments and the other intellectual operations to be found in the underlying dream thoughts. These will be further mentioned when we speak of the "dream-making" proper. (4) The fourth mechanism, that of *secondary elaboration*, fundamentally differs from the other three in that it arises from the activity, not of the underlying dream thoughts, but of the more conscious mental processes. To it we owe whatever degree of ordering, sequence and consistency there may be found in a dream.

Having mentioned some of the mechanisms that bring

about the distortion of the dream thoughts into the dream itself we may now shortly consider the material and sources from which a dream arises. Again we have sharply to distinguish between the manifest content of the dream and the underlying latent content; the latter will presently be dealt with apart. Three peculiar features exhibited by the memory in dreams have especially struck most observers; first, the preference shown for recent impressions, secondly, that the experiences are otherwise selected than in our waking memory, in that subordinate and unnoticed incidents seem to be better remembered than essential and important ones, and thirdly, the hypermnesia for previously forgotten incidents, especially for those of early childhood life. In every dream, without exception, occur incidents experienced by the subject in the last waking interval; the explanation why this is so cannot here be given, as it would lead us too deeply into the subject. Other recent experiences, however, that have not occurred on the day immediately preceding the dream are treated in just the same way as ancient memories. The selection of incidents of subordinate interest applies only to incidents of the day before the dream. Older incidents that at first sight appear to be unimportant can always be shown to have already become on the day of their occurrence psychically significant through the secondary transference to them of the affect of significant mental processes with which they have got associated. The material of dreams may, therefore, be either psychically significant or the opposite, and in the latter case always arises in some experience of the preceding day. The third feature mentioned above, namely, the hypermnesia particularly for infantile experiences, is only sometimes to be found in the manifest content of the dream, but always in the latent content. The origin of some dream material is to be found in somatic stimuli during sleep, though not so frequently as many writers maintain. They are, however, in no case the cause of the dream, but are merely woven into

its fabric in exactly the same way as any other psychological material, and only when they fulfil certain conditions. This is easily shewn by, for instance, the following considerations: A sleeper may react to a given somatic stimulation when this is of a lively nature, such as bad pain, in one of several different ways. In the first place he may altogether ignore it, as often occurs in disease, in the second place he may feel it during sleep without dreaming, in the third place he may be awakened by it, and fourthly he may weave it into a dream. Even in the last instance it enters into the dream only in a disguised form, and it can be shewn that this disguise depends not on the stimulus but on the nature of the dream. The same stimulus may appear in different dreams in quite different forms, and analysis of the dream regularly shews that the form adopted is altogether determined by the motive of the dream. The dream makes use of the somatic stimulation or not according to its needs, and only when this fulfils certain requirements.

We have now further to consider the construction of the manifest content out of the latent content, or what Freud calls the *dream-making*. Freud lays great stress on the fact that in the formation of a dream no intellectual operation of any sort is carried out; the dream-making is solely concerned with translating into another shape underlying dream thoughts previously in existence. The dream-making proper is a process more distant from waking mental life than even the most determined detractor of dreams would maintain; it is not merely more careless, incorrect, illogical and incomplete than waking thought, but it is something that is qualitatively absolutely different from it, so that the two cannot be compared. In dream-making there is no thought, calculation, judgment or any other intellectual operation; there is nothing but transformation of previously formed mental processes. Any speech phrases or ciphers in a dream, and anything that appears to indicate judgment, calculation or argument,

are taken bodily from the underlying dream thoughts, either directly or in a distorted form. Even some of the waking judgments passed on a recent dream belong to the original dream thoughts. In the dream-making a number of devices, grouped above under the heading of dramatisation, are employed to present the manifold intellectual operations and logical relations present in the latent dream thoughts. It is, however, impossible here to do more than mention a few of them. Such parts of speech as "if," "although," "either," "because," etc., which occur in the latent thoughts, and make them comprehensible, are expressed in the manifest content by special devices. For instance, logical connection is expressed by simultaneousness, casual relation either by presenting the cause in a fore-dream and the consequent effect in a main dream or, more rarely, by producing a gradual transformation of one scene into another; an alternative is expressed by synchronously presenting as existent both possibilities, similarity by identification, and so on. Obvious absurdity in a dream signifies the existence of mockery or scorn in the dream thoughts. The affect in dreams has many interesting features. The incongruous manner in which it may be present when not explained by the ideas of the dream, or be absent when it might have been expected, has already been noted above, and is quite elucidated by psycho-analysis, which reveals that in the dream thoughts the affect is logically justified and congruous enough. The affect investing the latent content is always more intense than that present in the manifest content, so that although strongly affective dream thoughts may produce an indifferently toned dream, the reverse never occurs, that is to say an affective manifest content never arises from an indifferently toned latent content. The effect of the dream-making on the original affect is different from that on the rest of the dream thoughts, in that no distortion of it takes place. The affect appears in the same form in the latent as in the manifest content, though of course through the

mechanism of transference it is otherwise associated than in the latter.

We have last to consider the most important problems of all, those concerning the latent dream thoughts. The first thing that strikes one about these is their intense psychical significance. A dream never proceeds from trifles, but only from the mental processes that are of the greatest moment and interest to the subject. More than this, they are processes of the greatest *personal* interest, so that the dream thoughts are invariably egocentric. We never dream about matters that concern others, however deeply, but only about matters that concern ourselves. It has already been mentioned that the underlying dream thoughts are perfectly logical and consistent, and that the affect accompanying them is entirely congruous to their nature. Freud, therefore, not only agrees with those writers who disparage the mental quality of dreams, holding as he does that the dream-making proper contains none of the nobler intellectual functions, but proceeds only by means of "lower" mental processes, but he also agrees with those other writers who maintain that dreams are a logical continuance of the most important part of our waking mental life. We dream at night only about matters that have most concerned us by day, though on account of the dream distortion this fact is not evident. The dream thoughts shew certain differences in the young child and in the adult. In the young child no distortion takes place, so that the latent content and the manifest content are identical. In such cases it is easy to see that the dream represents the imaginary fulfilment of an ungratified wish. Now Freud maintains that the latent content of every dream represents nothing else than the imaginary fulfilment of an ungratified wish, and it has sometimes been alleged by his opponents that this generalisation is the outcome of observing a few child dreams, and that his analyses merely consist in arbitrarily twisting the dream until a wish can be read into it. We have

seen that this is historically untrue, for Freud came to the analysis of adult dreams from the analysis of adult psycho-neuroses. He found that his patients' symptoms arose as a compromise between two opposing wishes, one of which was unconscious, the other conscious, and that they allegorically represented the imaginary fulfilment of these two wishes. He further found that an essential factor in their production was a conflict between the two systems, of such a kind that the unconscious one was forcibly prevented from becoming conscious; it was unconscious because it was "repressed." It frequently happened that the psycho-analysis led to the patients' dreams, and on submitting these to the analysis in exactly the same way as any other mental material he discovered that the structure of them showed close resemblances to that of the neurotic symptoms. In both cases the material examined proved to be an allegorical expression of deeper mental processes, and in both these deeper mental processes were unconscious and had undergone distortion by the censor of consciousness. The mechanisms by means of which this distortion is brought about is very similar in the two cases, the chief difference being that representation by visual pictures is much more characteristic of dreams, though in psycho-pathology we are also familiar with it in the form of hallucinatory visions. The unconscious mental processes always in both cases arise in early childhood and constitute a repressed wish, as do all unconscious processes, and the symptom or dream represents the imaginary fulfilment of that wish in a form in which is also fused the fulfilment of the opposing wish. Dreams differ from psycho-neurotic symptoms in that the opposing wish is always of the same kind, namely, the wish to sleep. A dream is thus the guardian of sleep, and its function is to still the activity of unconscious mental processes that otherwise would disturb sleep. Freud couples with his investigations on dreams a penetrating enquiry into the nature of uncon-

scious mental processes, the function of consciousness, and many allied subjects that I cannot here consider. I would conclude with a sentence of his that "Dream interpretation is the golden way to the knowledge of the unconscious in mental life."

PSYCHO-ANALYTIC NOTES ON A CASE OF
HYPOMANIA.*

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The psycho-analytic methods developed by Freud in the past fifteen years have been singularly neglected by workers outside of German-speaking countries, as is illustrated by the fact that no psycho-analysis carried out in any other country has up to the present been published. Yet these methods are unquestionably destined to have a far-reaching influence not only in the case of the psychoneuroses but in much wider fields, and particularly in that of insanity. Already in dementia præcox Jung¹ has applied Freud's principles to the elucidation of the mental changes present in this obscure malady, and in spite of the great technical difficulties encountered has met with such brilliant success as greatly to encourage workers in this and allied spheres of investigation. Up to the present no psycho-analysis of a case of manic-depressive insanity has been recorded, for the only one published under that name, by Otto Gross,² was almost certainly, as Jung and others have pointed out, a case of dementia præcox. The same objection may be open as regards the

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¹Jung: The Psychology of Dementia Præcox. Translated by Peterson and Brill. Journal of Nervous and Mental Disease, Monograph Series, 1909. See also two excellent analyses by Brill, Journ. of Abn. Psychol., vol. III, p. 219, and Amer. Journ. of Insanity, Vol. LXVI, p. 53.

²Otto Gross: Das Freud'sche Ideogenitätsmoment und seine Bedeutung im manisch-depressiven Irressein (Kraepelin's). 1907.

case presently to be described, but in the writer's opinion the evidence seems here definitely to point against the diagnosis of dementia præcox.

Before detailing some of the results of the study carried out by the Freudian methods, I will first give an account of the case as observed on ordinary lines.

Previous History.—The patient, S. T., is a woman aged 39. Her father died of pneumonia at the age of 55. Her mother, aged 78, is still alive; she is said to have suffered from "hysteria." One brother died of an aortic aneurism at the age of 35; two others are alive and well. There was no instance of insanity known in the family.

The patient was brought up in the country, in a Northern American State, and was at home until she married. At school she was unusually quick in learning. She began to menstruate at the age of 14, and had no dysmenorrhœa. She married at the age of 19, and in the next year bore a son, who is still alive and well. A year or two later she had a miscarriage; this was followed by pelvic complications, evidently of a septic nature, which kept her in bed for five months. In 1892, when aged 23, she underwent double ovariectomy. This operation had no effect on her menstruation or on her sexual feeling. Ill-health persisted, and she suffered much from painful sensations in the top of the head. Three years later her uterus was curetted for this. A period of depression followed, and she was given electrical treatment for six months. In the succeeding years she was well for varying periods, and in the intervals suffered much from weakness, malaise and pelvic pain. In the summer of 1904 her health again badly broke down, and she was in hospital for three months of that year and for six months of the next. In June of the latter year (1905) she had a broad ligament cyst removed; it was at the time noted that the ovariectomy had been complete. Since this last operation she has rarely been free from sleeplessness, lack of appetite and other symptoms, though these greatly varied in intensity from one time to another. Two

months before her admission to the asylum a marked exacerbation of them took place, as will presently be detailed.

At different times during her married life she had entered into irregular relations with men, which lasted for varying periods. Her husband, who was a commercial traveller, was aware of this. He attributed it to irresponsible eccentricity caused by her markedly erotic temperament, and took the view that his duty was to forgive, and, wherever possible, to guide her. His attitude towards his wife was one of exceptional patience and fondness, and domestic scenes between them were rare and insignificant. It should be mentioned that the patient had always from a girl been religious, but only to a reasonable extent.

Present Illness.—For two months before admission the patient had been restless, irritable, depressed and excited, varying greatly in mood from one day to the next. She had conceived an aversion towards her husband and son, and thought they were conspiring to harm or destroy her. She imagined that various clergymen were in love with her, that people were discussing her reputation and conspiring to injure it. Her food was being poisoned, and she refused to eat it. Electricity was being pumped up through the floor to hurt her, and there were strange odours in the room. The house was full of men and spirits. The front of the house was transparent, so that everything could be seen from the street.

She tried to throw herself out of the window, and to run out into the street undressed. She became violent, uttered loud screams, and had to be forcibly restrained. Throughout, however, she seemed to have considerable insight into her condition; she realized that her mind was sick and that she was going insane.

First Admission.—She was admitted to the asylum on January 30, 1907. From the note taken during her stay the following points may be added to the account

given above. Her attention was easily gained, and was well maintained. There was no clouding of consciousness. She was well oriented. She cried readily. She was fretful and, later on, depressed. She made a good recovery and was discharged on March 27, 1907.

Second Admission.—The patient was readmitted on September 27, 1908, with the story of having suffered from a very similar attack to the previous one. The symptoms had developed only a week ago, and had ensued on another gynæcological operation to which she had recently been submitted. They began with considerable depression, which was soon followed by excitement. The same erotic delusions were present as on the previous occasion. She was exceedingly restless and excited, gesticulated wildly, swore, sang, wandered from room to room, and threw things out of the window. She would toss herself about in bed until she reached an ecstasy of excitement, banging the walls with her fists, and so on. She would eat the most curious things, such as stove polish, etc. Her first act on being brought to the asylum was to smash a window pane with a chair.

On examination the patient was seen to be still in this excited condition. She was in continual movement, made frequent attempts to climb out of the window, constantly flourished her arms and declaimed at a high voice. Her hands, when she was not gesticulating, were busy crumpling or tearing fragments of paper. At times exacerbations of great violence occurred, in which she needed restraint. Her hair was dishevelled, and her personal appearance neglected. Her expression was distinctly lascivious, though her behaviour was never indecent.

On studying her logorrhœa one found that it consisted of a series of rapidly uttered words and short phrases, mostly related one to another by sound associations. The following are examples: "What a pretty tie. I wish I were tied to someone who was pure, and had pretty eyes. I'm fond of pretty eyes. Fond of lies.

Not fond of the kind of lies I've been accused of. Oh, yes, I was. 'There are different kinds of lies, lie up, lie down.' The significance of the last words will be seen later. "I don't trust him. New York trusts. Roosevelt is all right. Except when he talks about race suicide. Bearing children is all very well when you have no bearing-down pains. There are too many panes in this window; I should like to open one." We see how prominent is the impulsive and rapid transition, along the most superficial associations, from idea to idea, so characteristic of the manic flight of ideas.

The patient was perfectly oriented in time, place and personality. She retained insight into her condition, knew exactly why she had been confined, and that she would soon be better and about again. After the early stage no hallucinations occurred, except of smell. Her delusions have been mentioned above, and will be discussed in the course of the analysis. Her memory was unimpaired, as was—so far as could be tested—her power of retention. There was no sense deficiency, no contraction of the visual fields or disturbance of sensibility, and no other stigmata of hysteria. Her judgement in everyday matters was sound, and there was no evidence of intellectual deterioration. No mannerisms, catalepsy or stereotypies were ever noted.

There was but little sleep for the first few days. The tongue was furred and rather dry. The pulse rate and temperature were slightly raised. There was much constipation and the appetite was at first very deficient. There were no abnormal physical signs in the nervous system.

Course.—The excitement gradually subsided, and in a couple of weeks had practically disappeared. Synchronously with this the delusions became less prominent, and the patient gradually realised the falsity of them. In January, 1909, she was allowed to go out on probation, and did so for a couple of days at a time at frequent

intervals until her final discharge in April. Further details of the course of the case will be given in the analysis.

Diagnosis.—There are only three diagnoses that come into serious consideration, manic-depressive insanity, dementia paranoides, and hysteria. The last-named can with the greatest probability be excluded, on account of the absence of physical signs or symptoms of this malady, and the non-correspondence of the mental condition, either clinically or psychologically, with that characteristic of hysteria. The reactions were never infantile, there were no amnesias, abouliias or phobias, and the word-associations were not of the kind found in hysteria.

To exclude the diagnosis of dementia præcox is more difficult, especially as the thought of this malady is especially raised by the fact of the history extending so far back, by the chronicity of the symptoms with their occasional exacerbations, and by certain features of the delusions present. However, on the one hand there was none of the peculiar "shut-offness," or loss of contact with the immediate environment, that is the most constant accompaniment of dementia præcox, nor were there any somatopsychic perversions, stereotypies, verbigerations or mannerisms such as we might expect to find by the time the case had developed so far; on the other hand, the alternation of depression and excitement, the vehemence of the latter manifestation, the logorrhœa, the suggestibility during the excited period, the retained insight into the condition, the morbidly sharp definition of the subsequent recollection of all the events in the illness, the form of association reactions, and the typical manic flight of ideas, all strongly go to show that we are dealing with a case of manic-depressive insanity. As will presently be shown, the delusions that occurred bore a distinctly paranoid stamp. This feature may be found in a variety of psychoses, in general paralysis, dementia præcox, alcoholic "dementia," etc. It is becoming more and more doubtful whether true paranoia exists as a separate entity,

and Specht³ has recently advanced excellent reasons to show that a large number of cases in which this diagnosis has been made are really cases of chronic mania, i.e., of manic-depressive insanity.

Psycho-Analytic Observations.—Up to this point the case has been considered on strictly Kraepelinian lines, and the diagnosis arrived at by observing and weighing the import of the external objective manifestations of the malady. Of fundamental importance as this route is in teaching us so much about our cases, the grouping of them, the separation of one form from another, the outlook on prognosis and the general review of the disease, yet in its very merits lie its limitations. It definitely aims at giving us a conception of the disease *as seen from the outside*, in other words from the point of view of the clinical observer. It does not pretend to lead us to an appreciation of the morbid phenomena *as seen from the inside*. We thus never reach the patient's point of view, never realise what a given external manifestation represents to *him*, and thus never approach a true understanding of the meaning and significance of that manifestation.

It is precisely here that Freud's psycho-analytic methods supplement the usual modes of study, and, thanks to them, we are for the first time beginning actually to penetrate into the patient's mind, and to learn something about the pathogenetic mechanisms by means of which the different symptoms of the disorder are brought about. The psycho-analysis of an individual case of any psychosis is so difficult that it is rarely complete. In the present case only a certain number of indications were obtained, which, however, yielded clues of considerable value to the interpretation of the most prominent symptoms. We may begin by noting the results of a few association tests.

In these associations the following points are noteworthy: the high percentage of "complex" indicators; the high percentage of superficial associations, particularly

³G. Specht: Ueber die klinische Kardinalfrage der Paranoia. Zentralbl. f. Nervenhellk. u. Psychiatr. 1908. S 817.

CASE OF HYPOMANIA.

III

of clang and motor-speech forms; the markedly erotic assimilation shown in regard to most of the stimulus

Stimulus.	October 23, 1908.			November 28, 1908.			
	Time.	Reaction.	Remarks.	Time.	Reaction.	Remarks.	Reproduction.
Carpet.	2	pet.		2.0	pet.		✓
Window.	1.5	deagh.			O		O
Cup.	2.5	cut.		8.0	saucer, cup	fellatorism	✓
Blue.		O		9.0	of kindness	complex.	✓
Grass.	4.0	rasped.	seuaction in orchard rememb'd.	8.0	white. stubble.	purity. As before.	O
Red.	3.0	green.		2.0	blue.		green. fields.
Hit.	2.0	blow.		4.0	hat.		blow.
Gain.	3.5	win.		2.0	loss.		✓
Tree.	4.0	fruit.	childbirth recalled.	2.0	shrub.		plant.
Wind.	4.0	wind.		4.0	wind.	winding clock.	✓
Spent.	15.0	it.	orgasm evoked.	4.0	spend.	orgasm.	✓
Cloud.	4.0	clod.		12.0	clown.		O
Hat.	3.5	cat		4.0	cap.		O
Pot.	1.5	pan.		1.5	pan.		✓
Come.		O	orgasm.	2.0	go.		O
Dress.	4.5	rest.		4.0	rest.		habit.
Heel.	2.0	toe.		2.0	toe.		✓
Truth.	4.0	Ruth.		3.0	true.		liar.
Fast.	6.0	asked.	erotic memory.	2.0	present.		✓
Rail.	4.0	ence.		3.0	log.		✓
Way.	2.0	right.		4.0	side.		wayside.
Black.	1.5	white.		1.0	white.		✓
Street.	5.0	pillow.	mediate assoc. via sheet.	2.5	stream.		O
Cow.	4.0	calf, no, bull first.		1.5	calf.		O
Face.	1.0	form.		1.0	form.	" should prefer it to face if I were a man."	O
Spirit.	4.0	proof, the best proof of a fruit is the eating of it.	orgasm.	3.5	move.		O
Life.	3.0	death.		1.5	death.		✓
Clothes.		O		2.0	closer.		habit.
Sent.	1.5	smell.		1.0	smell.		✓
Sheet.	2.0	sheep.		2.0	towel.		✓
Form.	1.5	habit.		1.5	habit.		✓

✓=correct reproduction.
O=nothing reproduced.
OO=memory even for stimulus word forgotten.

words; and the striking similarity between the results of the two examinations, although a month apart.

They may be contrasted with the following associations, made on January 21. In these the number of superficial associations is much fewer, and approximates

Stimulus.	Time.	Reaction.	Remarks.	Reproduction.
Post.	2.2	pole.		✓
Rich.	1.6	wealthy.		✓
Work.	1.0	play.		poor.
Run.	7.4	stop.		walk.
Talk.	1.2	laugh.		✓
White.	0.6	black.		✓
Spend.	6.4	spent.	orgasm.	✓
Old.	0.6	heat.		✓
Field.	1.6	felt.	assimilation to sexual complex.	✓
Car.	4.8	people.		✓
Mother.	0.6	father.		✓
Copper.	0.6	penny.		✓
Pole.	5.2	post.		✓
Part.	5.6	the part.		✓
Lamp.	1.6	oil.		✓
Boy.	0.6	girl.		✓
Black.	0.8	white.		✓
Bag.	3.0	bush.		✓
Watch.	0.4	clock.		✓
Mouse.	1.0	rat.		✓
Shoot.	0.8	shot.		✓
Jump.	1.8	spring.		✓
Can.	1.0	must.		✓
Hat.	1.0	cap.		✓
Lace.	1.8	corsets.		✓
Rain.	1.8	snow.		✓
Coal.	1.8	furnace.		✓
Tool.	3.0	toy.		✓
Coat.	1.2	dress.		✓
Brother.	0.8	sister.		✓
Mount.	5.0	mounted.	orgasm.	✓
Tea.	2.2	milk.		✓
Pin.	3.0	needle.		✓
Drop.	2.0	fallen.	"from grace."	✓
Come.	1.4	came.		OO
Cow.	1.4	horse.		✓
Blood.	2.2	white.	mediate via "washed in blood, whiter than snow."	✓
Tree.	0.6	branch.		✓
Rail.	1.6	fence.		✓
Bed.	1.0	couch.		OO
Snow.	1.6	white.		rain.
Prick.	1.2	pin.		O
Nut.	1.0	cracker.		✓
Lie.	1.0	truth.		OO
Plate.	2.6	pudding.	refers to a ward joke.	✓
Touch.	1.6	play.		feel.
Train.	1.8	people.		✓
Roof.	2.0	ceiling.		✓
Rub.	2.4	brush.	refers to "spots on clothes."	O
Horse.	0.6	cow.		✓

✓ = correct reproduction.

O = nothing reproduced.

OO = memory even for stimulus word forgotten.

to the normal. The erotic trends are, however, still marked, and the number of "complex" indicators is almost as great as before (about 50% of the tests).

On studying the patient's logorrhœic utterances, and certain clues which will presently be mentioned, obtained from her association reactions, one of the first groups of ideas that impressed itself on the observer was that concerning the "impurity" of her past life, particularly of her sexual life. Already in her delusion that clergymen were announcing from the pulpit the fact of this impurity, and were urging her to take up a better life, this complex revealed itself. Other manifestations of the same idea were: the belief that she had some unclean contagious disease, and that on account of this the nurses refused to touch various vessels she had used and even feared contamination by touching her; also that her body, especially her vagina, emitted an evil odour. The following flight referred to this: "I haven't a single cent; no one has sent me anything; oh, here is that scent again; shall I never get rid of it?" (indicating the genital region).

This "impurity complex" arose from more than one source. In the first place she had masturbated to an unusual extent, and suffered much remorse in consequence. Many of her association reactions already indicated this fact. Thus 10% of her responses on November 28 contained the word "habit," which was always linked in her mind with the idea of masturbation. Free association from the word "dress" gave "riding-habit, I was fond of riding as a child, it started in me the habit of self-abuse." On another occasion the word "can" gave the association "candle, can't."

Another root of this complex lay in the idea that she had contracted some "filthy" complaint from her husband. About seventeen years ago she had suffered from a vaginal discharge, to which she attributed the septic complications that followed the miscarriage. The medical practitioner in attendance developed a finger infection which he said he had contracted from her. This may have been one of the grounds for believing that the nurses were now afraid of being contaminated by contact with her, for her pride had naturally been much hurt by the occurrence in question. That the memory was still rankling

in her mind was illustrated by a free association, taken on December 1; "run—fast—drop to a walk—tap till the sap runs—running tree—root—penis—running sore." The idea that her husband had made her unclean became associated with similar ones about him that we shall later discuss.

The external source of her uncleanness, and the relation of it to sexual matters, was probably one cause of her delusion that she was being poisoned by someone, particularly by her husband (she once accused him of putting a powder in her drink). She also maintained that there were men in the cellar who kept rapping on the pipes, drugged her with gas, and then had sexual intercourse with her. The gas was sometimes coal gas, sometimes sewer gas. It had a cloudy appearance. *In the morning everything was sticky from the stuff deposited by it.* It was on account of this gas that she would rush to the window to avoid suffocation (an action misinterpreted as a suicidal attempt). The tap water would start running and then stop for a moment while someone put poison in it. The sound of running water, and even the patter of rain drops, did in fact always evoke sexual sensations, probably from its association with micturition.

Another curious symptom in connection with the same complex was the following: During the examination she bent down and insisted on inspecting the heel of my boot. The result appeared to be satisfactory, for she exclaimed: "I see you polish the heel as well as the toe (see association test for 'heel'); you must be pure. All parts that one can't see ought to be clean. If the heel is clean, the sole (soul) must be clean. I am a heeler, a healer (spelt), heal-err. To err is human, love is divine. One cannot heal and err at the same time. Thou shalt bruise his heel. I once healed an impotent man, simply by touching his hand." (The subject of impotence will concern us later, as also that of personal cleanliness.) Other manifestations of the same train of ideas were her constant habit of picking up anything she considered clean, pieces of paper, pins, etc., and her exaggerated personal ablutions,

teeth-cleaning, etc. The latter symptom is a very common one, especially in the "*Zwang*" neurosis, where it always indicates a reaction against sexual "impurity," particularly masturbation.

We must now further pursue the relation of the patient to her husband. She often maintained that she was not legally married to him. One day she burst into tears at the sight of a photograph of her son, and called him an orphan because he had no father. "Our marriage was not a real one. We were giggling before the clergyman. Mr. T. didn't know which finger to put the ring on, and I had to put it on myself. That hurt me dreadfully." The seizing at trumpery pretexts to prove that her marriage was not binding of course indicates the presence of some deeper reason for wishing this. Two other pretexts, though less trumpery, served the same purpose. The first of these was that she was already two months pregnant when she married her husband, and would never have married him had it not been for that. She seriously thought of evading the marriage, and her brother had offered to help her in that event. The marriage ceremony was in some respects therefore a farce in her eyes. The second pretext was that she had before marriage already had sexual relations, at the age of sixteen, with her music master. This affair had lasted three months, and he had made her swear that she would never marry anyone else. The notion that her "second" marriage was not holy in the sight of heaven had for years haunted her, and only recently she had taken the advice of a clergyman on the question whether it was lawful for her to continue her relations with her husband. Some four years ago she put a stop for some months to conjugal relations, because she thought they were "wicked."

There were, however, deeper reasons why she was unconsciously striving against the idea of a binding marriage. Her sexual needs had apparently only increased as she grew older, and indeed were approaching a stage when they might almost be called nymphomaniac, so that the demands made upon her husband in this respect were

exceeding his capacity. She stated this quite unequivocally, and contrasted him in this respect with her first lover, who had on several occasions had sexual intercourse with her over a dozen times in one night. We find here a second cause of her refusal to take food. She said: "I nearly died of starvation, and yet I wasn't trying to commit suicide. However much I ate I couldn't get *satisfied*. Satisfied has two meanings, hasn't it?"

Further, although, as was above mentioned, her husband had taken a very lenient attitude towards her sexual irregularities, still she naturally felt her impulses in this direction restrained. An instance of how this powerful wish for freedom caused her to read obscure meanings into simple actions might here be given. Her husband brought one day to the asylum a watch which she had left behind her at home, and handed it to her with the words, "This is yours, my dear." She felt convinced that his words contained an inner meaning, namely, that the "watch" stood for the female genital organs, and that his giving it to her indicated his agreement that these organs were her own, to do with as she willed. I asked her why a watch should represent the female genital organs, although I knew that the association between the two is not a rare one in dream processes, etc., and she answered: "I suppose because it has works inside that keep regular time(s), or else because it needs a 'key' to wind it up." (See association to "wind" on November 28.) It is not without significance that the particular watch in question had been given to her by her first lover, the music master.

Her love, that could not adequately be gratified by her husband, had, therefore, to seek another direction. Several causes combined to make this a religious one. Ministers appealed to her from the pulpit to forsake her evil ways and follow the true path. She interpreted this very literally as referring to her sexual life, and meaning that she had previously been indulging in sexual gratification *in the wrong way*, and that she must now find *the right way*. One minister publicly made to her in veiled language the great revelation of what was the right way.

Before eliciting this I had already noticed the plainest signs indicating the nature of her discovery. When speaking of religious observances, particularly of holy communion, she broke off, and slowly and reverently went through a perfect pantomime of the whole ceremony. This culminated in her taking a glass of water, which she had placed on a Bible, and gradually raising it to her lips, where she beatifically sucked the rim, slowly revolving the glass as she did so. During the latter part of the performance a complete and exhausting sexual orgasm took place. I pointed to the glass, and asked her if it was the communion cup; she answered, "Do you call it a cup? It has another name," and later remarked, "This is *the Way*, the Truth and the Life." (See associations to "way," October 28 and November 28.) Again, "I prayed earnestly to God to let me have what I most wanted with Mr. X. (the minister). He refused, and I had to submit to His will, and drink the cup of sorrow (communion) alone."

It was manifest that the act of partaking of holy communion, performed in a very sensual way, had for the patient replaced that of normal sexual intercourse. There was ample evidence to support this conclusion. I asked her at what age she had first taken communion. She answered, "At sixteen (referring to her first sexual relations; she had partaken of communion two years before this). Oh, do you mean the outward and visible sign of true communion? That was two years before." The religious ceremony was thus to the patient an outer symbol of some actual physical mode of sexual gratification. On another occasion she told me how puzzled she had been at a statement of her minister's to the effect that no one should partake of holy communion until after having two weeks' preparation; later she "discovered" that he had referred to the necessity for previous purification—by thoroughly washing the genitals with soap and water.

After this revelation it now became plain to her that all her life she had been enjoying sexual pleasures in the

wrong way, and that the true way was to admit the male organ not into the vagina but into the mouth (fellatorism). The seed was in this way to enter into the body—had not Christ said “Take and drink”?—where it would perform its function of creating and nourishing the child. She would thus be able to bear another child to replace the one she had lost, in spite of the ruin of her internal genital organs. Perhaps her deepest grudge against her husband was the fact that his “uncleanness” had put an end to her child-bearing at the early age of 22. Her belief that “the new way” would bring to fruition her maternal desires, and secure both the creation and nourishment of the child was confirmed by a piece of advice given to her by her doctor to the effect that “she ought to swallow as much *milk* as possible.”

One naturally next enquired into the source of the patient's knowledge of the perverse pleasure just described. She had on a few occasions performed cunnilingus with her husband, but never actually fellatorism. “He was not clean enough; he did not bathe so often as Mr. X” (the music teacher). She had, however, on several occasions performed the act in question with another man. There were further evidences to show that the patient was one of that frequent type to whom the excitation of the lips and mouth is capable of yielding as intense sexual pleasure as that of the genital regions. In these people one might say that in a certain respect the cavity of the mouth is an equivalent of that of the vagina, and can in fact replace it (Freud's *Verlegung von Unten nach Oben*). This is, of course, as a rule, accompanied by marked sucking movements, and the earliest source of this abnormality has been clearly traced by Freud to the sucking movements of the infant at the nipple. Children destined later to show this abnormality are morbidly fond of sucking various objects, particularly their own toes or fingers. This simple pleasure had still persisted in the patient in question. On several occasions I witnessed her develop an obvious orgasm by vigorously sucking her thumb. When she was first

seduced, at the age of sixteen, by the music teacher, he aroused her passions by warmly kissing her and at the same time moving his tongue round and round in her mouth ("like I am now turning the cup," she said, carrying out this action). On one occasion, when referring to some high words she had had with a doctor, she said, "I gave him a good tongue-ing. That's a word that has two meanings, you know. I mean it here in the innocent sense."

The patient had thus by devious routes come to the idea that fellatorism was the "true way" of obtaining sexual gratification, and that the conception of a child would follow on this act. "The seed of a man must enter into the woman—into the woo-man—not into the womb-man, but into the mouth" was a remark that, amongst many others, exemplified this. The form of gratification just mentioned she identified with the partaking of the holy sacrament; a vital fluid was swallowed in both cases, the name communion she used indifferently for both, and the penis she referred to as the "cup of kindness."

A series of consequences followed from this belief. Swallowing became for her an act of the highest significance. In a number of articles of diet, particularly in a peach, she saw a resemblance to the genital organs and entered into an orgasm when sucking and swallowing them; she would frequently keep one hand on the communion service in the prayer-book during this process. Drink had always to be taken in a certain way, following a ritual resembling that of the holy sacrament. Her husband had poisoned her with his uncleanness; therefore, the food at home, i.e., belonging to him, which she had to swallow was also poisoned. On the other hand, she could not obtain enough food to "satisfy" her. It was, therefore, not only injurious in quality, but also inadequate in quantity.

If we now make a short synthesis of the order of development of the psychosis it will run somewhat as follows: A woman, of passionate temperament and

strong religious training, had at the age of sixteen been seduced, and at the age of nineteen had married another man by whom she was already pregnant. After bearing one child she had a miscarriage, which she attributed to a gonorrhœa contracted from her husband, and underwent a number of gynecological operations and other treatment for the relief of subsequent pelvic complications: her ovaries were removed at the age of twenty-three. As the years went by, her desire to have more children was strong, and her sexual inclinations increased in intensity; at the same time her husband's capacity to gratify these grew less, and she contrasted him unfavourably in this respect with her former lover. She thus blamed her husband twice over for her lack of children. She had illicit relations with other men, which caused her much remorse. Religious appeals to forsake her evil ways and lead a new life she interpreted as a revelation indicating the error of her past sexual life and advocating a new form of sexual life. For a number of reasons this idea of a new sexual life took the form of the fellatorism perversion. She tenderly loved her husband, so that there arose in her mind an intense conflict between this feeling of love and duty, and the forces impelling her to turn from him to a new kind of life. The compromise between the two sets of forces was found in identifying, for a number of reasons, the act of fellatorism with the partaking of the holy sacrament. A number of abnormal mental processes were the direct outcome of this; such were delusions of poisoning, refusal to take food, intense excitement evidently of erotic origin, belief that various ministers were in love with her and eager to lead her into the "new way" of sexual life, etc. These abnormal processes clinically constituted recurrent attacks of mania.

It was impossible to perform a complete psychoanalysis of the case, and I have contented myself here with giving a few examples out of the rich material of observations made, together with the main conclusions to which the study led. By means of the knowledge

gained by psycho-analytic methods one was able to render intelligible the abnormal mental processes in a way otherwise impossible, and to obtain most valuable clues into the significance and origin of the symptoms of the psychosis. No generalisations as to the nature of manic-depressive insanity are offered from the observation of this case, but it is maintained that studies undertaken by means of the psycho-analytic method promise better than any others to give us in time an understanding of the mechanism, and perhaps the nature, of the malady.

THE DIFFERENCES BETWEEN THE SEXES IN THE DEVELOPMENT OF SPEECH.*

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It is universally recognised by those who have worked at the subject that the articulatory capacity develops in girls earlier and better than in boys. No proof, therefore, need here be quoted of this statement which I have thoroughly confirmed by my own investigations. Up to the present no satisfactory explanation has been given of the fact, most writers confining themselves to the remark that girls are in general more adept and skilful in their movements than boys. This popular opinion has not been established by exact psychological investigations, so that it cannot be accepted as a satisfactory explanation of the greater lingual dexterity of girls. Further, it is certain that girls' speech surpasses that of boys in other respects than in mere muscular dexterity, indicating that some more recondite factor must be at work. Girls show, for instance, a much smaller tendency than boys to suffer from defects in fluency, particularly stam-

*Read before the International Congress of Psychology, Geneva, August 6, 1909. Published in the *British Journal of Children's Diseases*, Sept., 1909, p. 413.

mering (*Stottern*), which are now known to originate in complex psychical causes.

It has seemed to me possible that a clue to the problem might be obtained by making a detailed study of the precise respects in which the articulatory capacity of girls *most* surpasses that of boys. This is best done by determining the frequency of the various articulatory defects presented by average children. So far as I can find out from the literature, previous studies of this kind have all suffered from one of the following fallacies: Either they have been based on the study of too few cases, as in the speech clinics of Gutzmann, Neumann, Oltuszewski and others, or else when the frequency of the various defects in large numbers of children has been recorded, as by Mehnert,¹ Rouma,² Sarbo³ and others, the observations have been made by untrained school teachers. Observations made in this matter by school teachers are of hardly any scientific value, especially when they are made by a number of different individuals, of different sexes, whose standard of comparison possesses no uniformity.

The present paper is based on the examination of 450 normal school children, and as the observations were all made personally it was possible to employ a uniform standard. Fifty children from each school year were examined, the sexes being equally represented. The details of the method employed, and the results obtained, have been elsewhere recorded,⁴ so that I need here quote only a few of the most important conclusions. The enquiry was limited to the consonantal sounds; 227 of these were tested in every case, and marks allotted according to a conventional notation.

One hundred and thirty-two of the sounds tested were better enunciated by the girls, sixty-three by the boys. We shall first consider the respects in which the girls most excelled, and then those in which the boys did.

1 Mehnert. Ueber Sprachstorungen. 1904. S. 18.

2 Rouma. Internat. Arch. f. Schulhygiene. Bd. II. S. 165.

3 Sarbo. Monatschr. f. Sprachheilk. Bd. XI. S. 65.

4 Ernest Jones. Internat. Arch. f. Schulhygiene. Bd. IV. S. 186, and Bd. V. S. 137.

The former may be divided into two groups: (1) Linguo-dentals and (2) sibilants; the superiority of the girls was most pronounced in the first of these groups. (1) There are in English two linguo-dentals, voiceless and voiced Th. The girls excelled with nineteen of the sounds containing a linguo-dental, the boys with only one. The superiority of the girls was often very marked, the number of defects being in one instance five times as great with the boys as with the girls. (2) Of the sounds containing one of the four sibilants, S, Z, Sh, Zh, the girls excelled with sixty-five, the boys with only four. The boys replaced the S sibilant by a peculiar lateral sound (oral signatismus) in a hundred and forty instances, the girls in only three; they replaced sibilants in general by it in two hundred and twenty-three instances, the girls in only thirteen.

The relative capacity of the girls was least developed in the case of the posterior linguo-palatals, and was here actually inferior to that of the boys. Of the sounds containing one of the three posterior linguo-palatals, K, G, Ng, the girls excelled with twenty, while the boys excelled with twenty-one. The greatest difference between the sexes here was found with Ng, where nearly twice as many defects occurred with the girls as with the boys. Next to this the respect in which the boys most excelled was with explosive linguo-palatals combined with liquids, particularly the sounds Dn, Kn, Gl.

If we now survey these results we note that the sounds in respect to which the girls excelled differ from those in respect to which the boys excelled in one important matter, namely, in that they are sounds more easily taught to deaf children. They are therefore sounds which a child can learn to imitate not only by hearing, but also by the process of lip-reading. The inference thus seems plausible that the development of the articulatory capacity may proceed in girls by the aid of unconscious lip-reading to a greater extent than in boys.

With the children investigated a fairly close cor-

respondence was found¹ in each sex between the degree of articulatory development and the amount present of nasal obstruction, and therefore presumably of deafness. Deafness occurs to about the same extent in the two sexes, so that the effectiveness of it in retarding articulatory development is greater with boys than with girls. This one would attribute to the fact of hearing being with boys the almost exclusive channel of education of the articulatory capacity. With girls, on the other hand, the effect of partial deafness is not so serious, because they can make use of the other educative channel, vision, to a greater extent than can boys.

If this hypothesis is substantiated by more extended investigations, then it will become necessary to proceed a step further and enquire into the reason why lip-reading should play a more important part in the case of girls than of boys. There is no corresponding difference in the visual acuity of the two sexes, so that some influence must be at work allowing girls to profit to an unusual extent from their visual perception *in the particular respect in question*. In other words, for some reason girls must be able in intercourse with their elders to learn more from the faces of their audience than do boys, probably because they watch them more calmly and with less embarrassment. This is, I think, in accord with every-day observation, for it will be generally conceded that, before the age of mental puberty—that is, before nine or ten—boys on the whole more often show awkwardness, abashment and shyness in the presence of their elders than do girls. This may well be due to the more frequent and intense feeling of shame, or even guilt, that boys experience as a consequence of the nature of their early sexual emotions.

1. Fifth Report to the Medical Officer, London County Council Education Department, 1908. To be published in detail later.

A SIMPLIFIED TECHNIQUE FOR ACCURATE CELL ENUMERATION IN LUMBAR PUNCTURE.*

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The practical utility of many methods of clinical investigation is greatly dependent on the facility with which observations can be quantitatively estimated, and so the results of different workers compared. This is strikingly seen in the case of such methods as Widal's test, where an enormous accession of utility at once followed on the discovery of how to standardise the results; without this discovery the method might still have been to-day of only theoretic interest. Consequently, when a new method has been proved to be of great value, it behoves workers to try to find a means of so stating their results that they can be compared quantitatively with those obtained by other workers. The method of lumbar puncture is still in a state of incomplete evolution in this respect, in spite of the extensive investigations that have been made by it. Of the valuable information that it yields only some can be stated in quantitative terms. For instance, the pressure of the fluid can be accurately measured by the procedure described by Quincke (1); the amount of albumin can be determined by Reiss' process (2) or, if it is very small, by the more delicate one of Nissl (3). With the important question of the cell content of the fluid, however, only the roughest methods of cell estimation are in general use, and the object of this paper is to consider whether we have not at our disposal the means of making this estimation in a more accurate and ready manner.

The technique for the cytological study of the cerebro-spinal fluid is usually carried out according to the lines

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laid down in the classic writings of Widal, Sicard and Ravault. The three cubic centimetres withdrawn are centrifugalised for ten minutes in an instrument that revolves 3,000 times per minute, the supernatant fluid is completely decanted and a film is made from the deposit. The film is fixed with alcohol-ether, stained by Ehrlich's or any other suitable stain, and examined under an oil-immersion lens. By this means the relative proportion of the various cells present can be accurately determined by a differential count, which, of course, is of the greatest service in making a diagnosis. The absolute number of cells can however be only roughly indicated thus, and the estimation of it is largely a matter of individual judgment. When a great number of cells is present in each field, the existence of an abnormal leucocytosis, or lymphocytosis, as the case may be, is obvious, but when this number is small it is frequently very difficult to say whether it is of pathological significance or not. Personal experience is here of great importance, but even then it is impossible to give any data that can accurately be used for comparison. Ravault (4) has suggested the following convention: he calls it a "pronounced" reaction when more than twenty cells are present in an oil-immersion field (exact size not stated), a "moderate" reaction when seven to twenty cells are present, a "mild" reaction when four, five, or six are present, and a "negative" reaction when there are less than four. Although this convention has proved of some service in default of a more accurate procedure, it is clearly only a rough makeshift and is open to many criticisms. Nissl (5) has indicated the fallacies implicit in the procedure, and has formulated five precise objections to the use of such a method as a quantitative observation. He concludes that, even with the most careful technique, it may occasionally be possible to find eight or ten cells in a field without any pathological significance attaching thereto.

The only serious advance on this technique was that made by Laignel-Lavastine (6), who suggested that after centrifugalisation only some of the fluid be decanted, leav-

ing enough to make an emulsion with the deposit. He pipettes this emulsion into a Manassez chamber and estimates the number of cells per cubic millimetre. Then the number of cells per cubic millimetre of the original fluid is clearly $\frac{N \times D}{V}$, where N is the number of cells

per cubic millimetre in the emulsion, D the quantity (in cubic millimetres) of fluid and deposit left to make the emulsion, and V the total amount (in cubic millimetres) of the original fluid. De Buck (7) has also employed this procedure, but he uses a Thoma-Zeiss chamber with a modification of the Prague method of counting.*

The theory of the procedure is therefore quite straightforward: the fluid is concentrated till all the cells are in a conveniently dense solution, the percentage of them in this solution is estimated, and the percentage in the original fluid calculated by knowing the relation of the solution examined to the original fluid. In practice, however, the procedure is more cumbersome than might appear, which probably accounts for its being rarely adopted. The main reason for this is that the actual number of cells is much smaller than that present in blood, even when this is diluted to the usual extent, so that the enumeration is both tedious and fallacious. For instance, De Buck counts the cells present on 256 squares of an Abbé-Zeiss slide, that is in about a fifteenth of a cubic millimetre; in a case of tabes or general paralysis, even if the fluid is concentrated by centrifugalization, the number of cells in the emulsion examined is frequently less than twenty in this amount, so that the possible error is very considerable (over 30 per cent.).

It thus becomes specially advantageous to adopt some of the more modern methods of using the Thoma-Zeiss chamber by means of which large areas can be rapidly inspected. The best of these is the "field" method, first described by Stengel (8). In this the squares marked on the slide are ignored, and the number of cells in each

*The Fuchs-Rosenthal method of counting gives accurate results, but necessitates the use of a special counting chamber.

field of vision of the microscope counted, a larger unit being thus obtained. As usually employed this method entails a rather elaborate calculation, which, however, can be avoided in a way I shall presently indicate. The area of the field must of necessity be calculated by the formula πr^2 , where r is the radius and π a constant, 3.1428571. The radius can most accurately be determined by adjusting the slide and the magnification of the microscope till a square formed by the ruled lines—and containing several little squares—touches at its four corners the circumference of the field; the radius is then equal to the length of one side of this square divided by $\sqrt{2}$ (corollary to Euclid, Book i., Prop. 47). The length of the line in question, and of similar lines, is easily found, because it is made up of a number of marked sections, each of which constitutes the side of a little square and measures one-twentieth of a millimetre. An easier though rather less accurate way of finding the length of the radius is by moving the Abbé-Zeiss slide until one of its ruled lines coincides with the maximum diameter of the field. The line is measured as just indicated, and, of course, the radius is half its length.

The field method is commonly employed in the following way. Recognising the fact that the diameter, and therefore the area, of the field can be varied at will by adjusting the magnifying powers of the microscope, a diameter is chosen which contains an integral number of the marked sections measuring each one-twentieth of a millimetre. The cubic capacity of the underlying space is then calculated as above, and its relation to the standard, a cubic millimetre, thus ascertained. This relation is usually a complicated fraction. The average number of cells per field is then found by counting a number of fields, and the result is divided by the above fraction, and multiplied by the degree of dilution of the original fluid. In the case of the cerebro-spinal fluid the number would have to be multiplied by the Laignel-Lavastine formula given above.

It will thus be seen that the process is quite cumbersome, and the trouble taken is disproportionate to the accuracy of the results, on account of the error due to the smallness of the number of cells. It has occurred to me, however, that the process might be greatly shortened by the following device, which also adds considerably to the accuracy of the results. Instead of choosing a diameter which contains an integral number of marked sections, let one be chosen in such a way that the cubic capacity of the space constitutes a simple factor of a cubic millimetre. For instance, if this space occupies exactly $\frac{1}{100}$ of a cubic millimetre, evidently counting the cells in 100 fields gives us the number of cells per cubic millimetre without any further calculation whatever. All that then remains to do is to calculate the number per cubic millimetre of the original fluid. The only preliminary work that has to be done is the adjustment of the magnification until the diameter of the field is of the requisite length. This takes only a minute or two, and, once it is done with a given microscope, the knowledge is gained for good that the use of such and such lenses gives a field of the required dimensions, so that the diameter need never be again measured. The diameter I recommended when I described the method in connection with hæmatological work (9) was equal to $6\frac{2}{5}$ of the marked sections; a field of that size is obtained with the magnification of Leitz Oc. 1 and Obj. 7 lenses, the draw-tube—in a Leitz II. A microscope—being at 170, and the space thus obtained is $\frac{1}{125}$ of a cubic millimetre. In this case the counting of the leucocytes in twenty-five fields gives, provided the dilution is 1 in 20, the exact number of leucocytes per cubic millimetre merely by the addition of two terminal ciphers.

The next problem is to decide what size field will be the most convenient to employ in the case of cerebrospinal fluid. This decision is arrived at only as the result of practical experience, and I would suggest the following technique as being the most advantageous.

We may divide the cases in which a cytological study

is necessary into two fairly defined groups: those with a great excess of cells, causing obvious turbidity of the fluid, such as is often seen in cases of meningitis, and those with a much smaller number, such as is seen typically in tabes or dementia paralytica.

In the former group the number of leucocytes per cubic millimetre is frequently as great as several hundred, in other words as great as that in normal blood when diluted twenty times, as it usually is in the "white" pipette of the Thoma-Zeiss hæmocytometer. In this case previous centrifugalisation is unnecessary, and it is sufficient directly to count the cells present in half a cubic millimetre, and then simply to multiply the result by two. The readiest means of doing this is to choose a field corresponding to a cubic space of $\frac{1}{8}$ of a cubic millimetre. This is found to be so when the diameter of the field covers eight sections (sides of little squares). All that need be done then is to count the cells present in twenty of these fields, and repeat the count in a second drop. We then know the number of cells present in half a cubic millimetre. This procedure is equivalent to counting the cells present in 2,000 little squares, and is thus far more accurate than that employed by De Buck, who counts the cells in only 256 squares, though it occupies very little more time. If the turbidity of the fluid is specially pronounced, it is sufficient to count the cells in twenty fields, using one drop only, and then to multiply the total by four, which gives us the number per cubic millimetre.

In the second group Laignel-Lavastine's manœuvre has to be adopted in order to obtain a sufficiently great number of cells; this is important, for the accuracy of the results depends on, among other factors, the number of cells counted. The readiest way of carrying out the manœuvre is to employ a centrifugalisation tube that has the lowest centimetre graduated in divisions of tenths. Then enough liquid is retained after centrifugalisation and decantation to form an emulsion with the deposit, and the exact relation of the liquid retained to the original

quantity is obvious at a glance. Even then the number of cells obtained may be so small that special manœuvres have to be adopted. For instance, out of thirty-five cases of G.P.I. that presented a meningeal reaction, Laignel-Lavastine (10) found that in twenty-eight cases the number of cells per cubic millimetre was under ten. When this is so, greater accuracy is obtained by counting the cells in three separate drops, thus ensuring a more uniform distribution. In doing this it is convenient to employ a field corresponding to $\frac{1}{100}$ of a cubic millimetre and to count the cells in thirty fields in each of the three drops. This is equivalent to counting the cells in four thousand squares, more than fifteen times the number recommended by De Buck for the same purpose. A field of this size is obtained by using a diameter containing 7.5 sections.*

An actual example, from an advanced case of tabes, will best demonstrate the simplicity and accuracy of their procedure. Three cubic centimetres of cerebro-spinal fluid were withdrawn and centrifugalised. A slight deposit was then visible, and the supernatant fluid was decanted only until the level of the remainder stood at the sixth division; the remainder was thus one-fifth of the original quantity. The deposit and small amount of fluid were thoroughly stirred by rotating the tube rapidly and evenly. A drop was pipetted with a platinum loop on to each of three Abbé-Zeiss slides, covered and allowed to settle. The microscope magnification was adjusted until seven and a half little sections formed the diameter of the field, a procedure that takes a minute or two. The cells were counted in the thirty fields, and

*The odd half, like most of such fractions, can be accurately measured by the following device. It will be remembered that on an Abbé-Zeiss slide every fifth section is divided by a line the function of which is, in conjunction with three other similar lines, to enclose sets of sixteen little squares. If now the end of the line that is to be measured is placed at the right-hand corner of a little square, so that the line from right to left traces first the sides of two unbisected squares, then that of a bisected square, and lastly those of four unbisected squares, we have seven sections measured, and the remaining half is already marked off by the vertical line that bisects the square that comes next in order to the left.

this was repeated with the other two drops. The total number of cells in the ninety fields was forty-three, which therefore was the number per cubic millimetre of the emulsion. Consequently the average number of cells per cubic millimetre of the original cerebro-spinal fluid was 8.6. The whole time occupied, including ten minutes for centrifugalisation, was twenty-five minutes, and in the same time a film was made for microscopic examination. It will be observed that there was no step in calculation that could not be instantaneously performed mentally.

A few further remarks might be made on some points in technique. When the fluid is quite turbid centrifugalisation is not necessary, except of course for film purposes. When, however, there are clinical grounds to suspect that the number of cells will be small, the fluid must not only be centrifugalised, but this must be done thoroughly, preferably for ten to fifteen minutes. Nissl and Devault (11) have demonstrated that after centrifugalisation for only a couple of minutes, even with an instrument that rotates 3,000 times a minute, there may be an appreciable number of cells still in the supernatant fluid. Again, when the emulsion containing all the cells is obtained, it is important to mix this thoroughly so that the drops taken shall be representative of the whole.

The presence of blood is sometimes a practical source of embarrassment. If this is due to the puncture of a small vein, clear fluid can generally be obtained by discarding the first portion that flows. Blood that is originally present in the cerebro-spinal fluid is frequently laked, so that only shadow erythrocytes are visible. It is sometimes difficult to distinguish erythrocytes from lymphocytes in the stained film, whatever technique be used, as Nissl (12) has pointed out. In the process of making the total count the difficulty due to the presence of blood can be avoided by adding a minute quantity of 30 per cent. B. P. dilute acetic acid, care being taken to allow in the calculation for this dilution. The acid has the further advantage of making nucleated cells more easily visible.

There are also some practical matters to be attended to in the actual counting of the cells. The most important point here is to be sure that empty fields are fairly counted in with fields containing one or more cells. It is a matter of great difficulty, which is appreciated only by practical experience, to avoid the habit of unconsciously moving the field until the eye catches sight of something positive. It is unquestionably better to glance away every time that the slide is moved. I need hardly say that a mechanical stage is practically essential, if only to ensure that the same region is not inspected more than once. Another rather important matter is the question as to whether cells should be included when they are only partly in the field. Clearly in a case where the excess of cells is only slight, two observers would get very different results if one counted every cell any part of which was in sight in the fields examined, while the other counted only those cells that were entirely in the fields. Some time ago I suggested (13) that Lyon and Thoma's (14) rule relating to the inclusion of erythrocytes in the Abbé-Zeiss squares be adapted to the "field" method of counting cells in the following manner. Imagine a line forming a vertical diameter of the field parallel to the observer's sagittal suture and let all corpuscles seen to the left-hand side of this line be counted as being within the field, whether in fact they are wholly so or only partly; on the opposite right-hand side only the corpuscles which are wholly in the field should be counted, whilst those touching the circumference should be ignored.

Finally, the pre-eminent importance of securing data adequate in amount cannot too often be insisted on. Especially is this so when it is a question of deciding whether the borderland between physiology and pathology has been crossed, as, for instance, in cases with a small number of cells in the cerebro-spinal fluid. Laignel-Lavastine (15) states that one should attach pathological significance only to liquids containing more than 0.5 cells per cubic millimetre. Even if a liquid

containing 0.5 cells to a cubic millimetre is concentrated to one-tenth and 4,000 Abbé-Zeiss squares be inspected, only five cells will be seen. Now the possible error due to paucity of data alone is here over 60 per cent.; in other words, the number really present might prove in a larger count to be anything between two and eight. If we accept twice this number, that is one cell per cubic millimetre of the original fluid, as being the lowest indicating pathological significance, then if this is the exact number of cells present its existence can be established with certainty only by counting the cells present in over eight million squares. Put in another way, by using ordinary procedures we can be sure of the existence of one cell per cubic millimetre only when there are really more than this number present. As a matter of fact, if we count only the cells present in 4,000 squares, that is in one cubic millimetre—a sufficiently extensive procedure for routine use—there must be actually present at least 1.5 cells per cubic millimetre before we are justified in stating that the number present exceeds the pathological limit of one per cubic millimetre.

I append a few instances bearing on this subject, and also a summary of the procedures suggested, though from the principles indicated above it is easy to modify these according to the number of cells that may be anticipated in different cases.

Summary of Methods Suggested.

Method.	Diameter of Field.	Cubic Capacity corresponding to Field.	No of Slides prepared.	No. of Fields examined on each slide.	No. of Abbé-Zeiss Squares inspected.
I.	8 sections	$\frac{1}{30}$ c.mm.	2	20	2,000
II.	7.5 "	$\frac{1}{30}$ "	3	30	4,000

Examples Illustrating the Above Methods.

Method.	Total No. of Cells in Fields examined.	No. of Cells per c.mm. of Emulsion.	Degree of Concentration.	No. of Cells per c.mm. of original Fluid.
I.	75	—————	1 : 1	150
	90	—————	1 : 1	180
II.	200	200	1 : 10	20
	75	75	1 : 5	15

Examples Illustrating Influence of Extent of Data on Accuracy of Conclusions.

No. of Cells actually counted.	Possible Error due to Paucity of Data.
300	8 per cent.
200	10 "
100	14 "
75	16 "
50	20 "
25	28 "
20	31 "
15	37 "
10	45 "
8	50 "
5	63 "

Examples Illustrating Data necessary to justify Statements as to Actual Number of Cells present.

Degree of Concentration of Fluid examined.	No. of Cells (average per c.mm. of original Fluid) the presence of which it is desired to confirm.	No. of Cells (average per c.mm. of original Fluid) the presence of which is necessary to justify the foregoing statement.					
		1 : 1		1 : 10		1 : 5	
No. of Squares inspected.		2000	4000	2000	4000	2000	4000
			1	6	1	2	1.6
	2	8	5.2	3.1	2.8	3.8	3.1
	5	12	9.4	6.3	6.1	8.3	6.6
	10	18.8	15.6	12.2	11.5	13.3	12.2
	20	31.2	27.8	23	22	24.5	23
	25	37.6	33.2	27.4	27.5	30	27.4
	50	66.4	61.2	55	53.5	57.2	55
	100	122.4	115.2	107	104.6	110	107
	200	230.4	221	209	206.5	214	209
	300	340	325.5	311.5	308	316	311.5

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THE PROTEID CONTENT OF THE CEREBRO-SPINAL FLUID IN GENERAL PARALYSIS.*

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The subject of the proteid content of the cerebro-spinal fluid in general paralysis has received a considerable accession of interest within the past year or so from the discovery of the part played by certain forms of proteid in the Wassermann reaction. Testing for the total proteid present has for some five or six years past been generally recognised as an important step in the examina-

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tion of the fluid in this disease, but of late it has been seen that the determination of the *kind* of proteid that is increased is a matter not only of diagnostic significance but also one of great theoretic interest in relation to the special substances that are provisionally termed the Wassermann anti-bodies.

The history of the development of our knowledge on the subject may first be shortly related, and the question of technique in examination separately considered. Only the albumin and globulin proteids will be dealt with in this paper. That proteid is a normal constituent of the cerebro-spinal fluid has been known for some forty years. Soon after the introduction of lumbar puncture as a clinical procedure it was found that the amount of proteid is greatly increased (often tenfold) in inflammatory conditions, particularly of the meninges, but that it is increased also in metasphilitic affections has been generally recognised only within the past half dozen years. The discovery that this increase occurs almost constantly in general paralysis was independently made and published on four separate occasions, and it was not until the last of these—seven years after the first—that general attention was called to the observation. Babcock, in 1896, found that the proteid was pathologically increased in every one of twelve cases of general paralysis he examined, and Nawratzki, in 1897, found the same in six cases. Schaefer, in January, 1902, refers to the work of these writers, which he confirmed; he was the first to appreciate the high diagnostic value of the observation. The same conclusion was independently stated in two papers published simultaneously in April, 1903, by Guillain and Parant, and Widal, Sicard and Ravaut. The former of these, in reviewing the literature, referred only to a case of tabes, recorded in 1901 by Achard, Loeper and Lanbry, one each of tabes and general paralysis recorded in the same year by Wolf, and to a statement made by Léri, in 1902, to the effect that there was *occasionally* an increase in proteid in cases of metasyphilis; their claim, therefore, that no one prior to

observation holds true only so far as French writers are concerned. They found that the increase occurred in the great majority of cases, that it was generally, though by no means always, parallel in extent to the degree of lymphocytosis, and that it bore no definite relation to the stage of the disease. Their conclusions were within a year decisively confirmed by Coriat, Donath, Marchand, Nissl, Schoenborn, Siemerling, Skoczynski and others, and at the present time observation of the proteid present is, of course, a routine step in the examination of the cerebro-spinal fluid. The value of the sign is enhanced by the facts that on the one hand, as Kutner and Liebscher pointed out, the lymphocytosis due to metasyphilis is more constantly accompanied by an increase in the proteid content than is the lymphocytosis due to tertiary syphilis of the nervous system, so that we have here a differential point of considerable value between the two conditions, and on the other hand, as shewn especially by Zilinakis, an increase in proteid may occur in cases of general paralysis at a time when no lymphocytosis is present and may thus supply an important clue to the diagnosis. Rénon and Tixier have similarly reported cases of meningitis in which there was present an increase in proteid but not in the cell count.

The amount of proteid present in normal cerebro-spinal fluid is but small. It is given as 0.25 grammes per litre by Gumprecht, as 0.2-0.5 by Quincke, as 0.2-1.0 by Siemerling, as 0.5 by Léri, and as 0.5-1. by Riecken. Some of these figures are based on the examination of the fluid in abnormal conditions, such as of hydrocephalus, spina bifida, etc., and it is doubtful if the amount ever exceeds half a gramme per litre in absolutely normal fluid. In general paralysis the average amount per litre is given as one gramme by Marie and Viollet, 0.9 by Nawratzki, 1.1 by Marchand, 1.6-2.5 by Cimbal, 0.75-3.5 by Schaefer, and 1-3 by Zilinakis. Nissl, without giving absolute figures, finds similar amounts, and Decoubaix says the amount varies up to 2-4 grammes. We

thus see that there is commonly four times as much proteid in the cerebro-spinal fluid of general paralysis as in that of the normal. It is therefore evident that in this affection the proteid will be manifest to coarser tests than are necessary to detect its presence in the normal. In practice it is found that valuable diagnostic indications may be got from a study of the reaction of the fluid to a few simple tests without the necessity arising of a quantitative estimation being made. In such quantitative estimations as have been made the methods employed have usually been Kjeldahl's (Schaefer, Cimbal) or some modification of Esbach's, such as Nissl's (Henkel, Kutner, Nonne and Apelt, Siemerling) or Nageotte's (Rous). Sufficiently accurate information may however be obtained by the application of the ordinary tests for proteid, such as heat, nitric acid, or picric acid. In normal cerebro-spinal fluid simple boiling, or boiling after slight acidification, produces only a faint cloudiness. This observation, made first by Hoppe-Seyler, may readily be confirmed, although some authors, including Donath, have maintained that no cloudiness occurs except in cases of general paralysis and tabes. In four cases of non-nervous disease I have found the reaction positive in every examination, though the opalescence is sometimes exceedingly faint. It is advisable, as recommended by Guillain and Parant, to boil the whole of the fluid, two or three centimetres, and not the upper part only, as is usually done. With a little experience it is easy to determine whether the opalescence formed passes beyond the normal limits. A more suitable test, however, is the nitric acid one, applied in Heller's way, so that a ring forms at the junction of the two fluids. This reaction is, in my experience, always positive, in health and disease. By gauging the density and thickness of the ring one can readily appreciate deviations from the normal, and, if necessary, the extent of the deviation can be measured by finding out the dilution of the fluid at which the ring forms only after three minutes' time (Brandenberg's method).

While at first interest was mainly concerned with the

question of the amount of total proteid present, of late years increasing importance has been attached to the study of the form of proteid. There is a tendency in present-day physiological circles to restrict the term globulin to a much smaller group of proteids than heretofore, but the definition of globulin afforded by the various salt-solubility tests will here be adhered to as being the better known and therefore more generally comprehensible one.

There is some divergence of opinion as to which form of proteid is present in normal fluid, a fact which illustrates how minimal is the quantity there. Whereas Arthus, Guillain and Parant, Halliburton, Mott, Hoppe-Seyler, Nawratzki, Rénon and Tixier, Sicard, Siemerling and Sollmann state that normally globulin is present but not albumin; Dircksen, Nonne and Apelt, Rous, Sabrazès and Schoenborn find both forms, and Nissl only albumin. The first-mentioned view is more probably the correct one, for it is based on very exact investigations made by some of the most experienced physiological chemists. At all events, if any albumin be present, it must be in exceedingly small amount. Halliburton states further that the proteid consists of serum-globulin, not fibrinogen or cell-globulin, and that it coagulates at 75° C.

In regard to metasyphilis, a similar divergence of opinion prevails as to the variety of proteid present, a fact that is to be attributed to the same cause as in the above case, namely, to the difficulty of deciding the point when the amount of proteid is so small—usually under five grammes per litre. Different results have been obtained by the use of different tests, so that some writers, as Cimbald and Nonne and Apelt, have wisely chosen to state their results empirically in terms of the tests employed, thus abstaining from any expression of opinion as to the nature of the proteid revealed by these tests. All writers on the subject are unanimous on at least one point, namely, that considerable quantities of serum-albumin are to be found in general paralysis. Nissl and Schoen-

born are the only two who consider that the proteid present in this affection is mainly albumin, and that globulin is rare. All other workers find that globulin is present in relatively large quantities, and most of them, including Widal, Sicard and Ravaut, Cimbäl, Pegna, and Nonne and Apelt, find that the amount of globulin exceeds that of albumin.

This question, however, cannot be considered as being at present decided, and practical interest is confined more to the question as to which tests are reacted to in cases of general paralysis in contrast with the normal. In other words, investigations have been empirically directed towards determining which chemical reactions are the most characteristic of the cerebro-spinal fluid in general paralysis. As will presently be pointed out, there is much evidence to show that in this disease certain peculiar qualitative changes take place in the proteid present, so that we are encouraged to believe that chemical tests may be found which respond to the changed form of proteid more constantly and characteristically than to the unaltered form present in the normal.

The technique that in this connection has been most widely employed is that introduced for the study of the cerebro-spinal fluid in cases of meningitis by René Monod. It was first applied to cases of general paralysis by Guillain and Parant, and is the one that was used in the extensive studies made by Henkel, E. Meyer, and Siemerling. The globulin is precipitated with magnesium sulphate, the fluid is filtered, and the filtrate heated; a precipitate then indicates the presence of albumin. Widal, Sicard and Ravaut used the same technique, except that they slightly acidified the filtrate with acetic acid before heating it. Cimbäl replaced the sulphate of magnesium by that of zinc, and allows it to stand for from four to twenty-four hours before filtering. The precipitate thus obtained he terms Fraction I., and that obtained by heating the acidified filtrate Fraction II.; the former is presumably globulin, the latter albumin.

It is clear from the results given by all these workers that these reactions, though often enough positive in cases of general paralysis, are not sufficiently characteristic to be of any value in differential diagnosis. This was thoroughly demonstrated by Nonne and Apelt, who repeated the various techniques in a large number of cases, and from personal experience I can fully confirm their conclusion. The reactions are frequently given by fluids taken from the most diverse cases, whether these are of a syphilitic nature or not.

In 1904 Nissl replaced the sulphate of magnesium by that of ammonium, finding that he could thus easily get a clear filtrate which could be more accurately tested. As was mentioned above, Nissl obtained a precipitate with ammonium sulphate (indicating the presence of globulin) only very exceptionally. Nonne and Apelt employed the same method, but introduced another standard in that they declared a case positive if cloudiness arose within three minutes after the addition of the ammonium sulphate. This stage they designate as Phase I., and the precipitate subsequently obtained by heating the acidified filtrate as Phase II. They found the Phase I. reaction positive in all cases of general paralysis, and not often in other cases. In a later contribution, Nonne disclaims any contention that the reaction is pathognomonic, but holds that it has considerable diagnostic value.

Last year another reaction was introduced by Noguchi, which he maintains specifically indicates the presence of syphilitic changes. He applies it in examination of both the blood serum and of the cerebro-spinal fluid. When it is positive in the latter then the presence of general paralysis or tabes is very probable, though it is sometimes positive also in cases of tertiary syphilitic affection of the nervous system. The technique of the procedure in the case of the cerebro-spinal fluid is as follows: To 0.1 c.cm. of clear fluid is added 0.5 c.cm. of a 10 per cent. solution of butyric acid in physiological saline. The mixture is heated to boiling, and then 0.1 c.cm. of 4 per cent. sodium hydrate is added, and the

mixture again heated to boiling. The test should be carried out without any pause. An opalescence frequently follows even in the normal, but the reaction is considered positive only when peculiar light greyish flocculi occur. This should come about within half an hour, and often does within a couple of minutes. Noguchi holds that the reaction is given only by globulin, and especially by the changed form of globulin present in syphilitic and metasyphilitic cases.

Before relating my personal experiences with these and other chemical tests, I wish briefly to indicate the theoretic interest that the subject has recently acquired; for fuller information on the matter the reader is referred to a review I have recently published elsewhere. The substances present in the blood-serum of syphilitics, and in the serum and cerebro-spinal fluid of general paralytics, and on which the Wassermann reaction depends, may provisionally be called anti-bodies, though it is now certain that even if they owe their origin to the presence of syphilis, they are not specific anti-bodies in the ordinary sense of the term. They were thought by Levaditi and Yamanouchi to be of a lipid nature, but more recently they have been demonstrated beyond doubt to be proteids, and, indeed, globulins. Further, thanks to the very careful work of Elias, Neubauer, Porges and Salomon, Gross and Volk, and especially of Noguchi, we now know that the proteid in question is a euglobulin. They possess in a high degree the capacity to bind lecithin, and on their proclivity to produce a flocculent precipitate in lecithin emulsion depends the well-known Porges-Meier reaction for syphilis. Levaditi and Yamanouchi, Bauer, and Gross and Volk have proved that they exist in small quantities in normal blood-serum. In other words, *the Wassermann reaction depends on an increase of some special form of euglobulin that is normally present in the serum.* The reaction is defined by Elias, Neubauer, Porges and Salomon as an interaction, of the nature of a precipitate formation, between certain hydrophile colloids, notably lecithin and a changed

form of globulin. These authors, as well as Pappenheim, Weil and Braun, and Much, consider that in syphilis some change occurs in the globulin which is of too fine a nature to be revealed by the ordinary salt tests. The change is therefore both a qualitative and quantitative one, and in the latter connection it is interesting to note that Noguchi has shewn that in syphilis there is a marked increase in the globulin of the blood serum. The source of the added substances in syphilis is not yet known. Weil and Braun find the origin of them in cell disintegration, Pappenheim and Marie and Levaditi in white corpuscles; this would agree with Feuillé's suggestion that the increase of the proteid of the cerebro-spinal fluid in metasyphilis is due to leucolysis.

We therefore see that it is highly probable that in syphilis, and especially in metasyphilis, there is a considerable increase in some particular form of euglobulin, and that in this euglobulin is contained the "*anti-body*" which is responsible for the positive reaction in the Wassermann test. Noguchi maintains that the existence of this increase is better demonstrated by his test, carried out as described above, than by any other. The test was discovered quite empirically by experimenting with a number of allied methods. With the cerebro-spinal fluid taken from patients with general paralysis he obtained a positive reaction in thirty-seven cases out of forty-three. These results were published in January of this year, and in the same month Dr. G. W. Ross and I published an account of the examination by the same method of fifteen syphilitic and ten non-syphilitic cases. Of the former cases we found the Noguchi test positive in three cases of tabes, five of general paralysis, and four of tertiary syphilis of the nervous system; it was negative in three cases of syphilis under treatment, in one of which the nervous system was implicated. Of the ten control cases the test was negative in all, with the exception of one case of tubercular meningitis. We found it advisable to view the test-tube by reflected light, and have had constructed a box blackened on the inside into which the

test-tube and a glazed electric bulb may be inserted. The fluid can then be viewed through a lateral opening, and the light nicely controlled. This device is useful in other similar tests in which it is necessary to determine the presence of slight cloudiness, of a faint ring, etc.

Since this time I have been able further to apply the test in two cases of cerebro-spinal lues, in one of which it was positive, in ten cases of general paralysis, in all of which it was positive, and in four cases of dementia præcox, in all of which it was negative. Up to the present, therefore, I have tested the reaction in fifteen cases of general paralysis and have always found it positive. Apart from metasyphilis, I have not found the test positive in any chronic disease, except sometimes in tertiary syphilis of the nervous system (gummatous meningitis, diffuse cerebro-spinal lues, etc.).

Recently Gay and Fitzgerald have recorded the results of Noguchi's test with the cerebro-spinal fluid in thirty-three cases. It was positive in eight cases of general paralysis, one of diffuse cerebro-spinal syphilis, and thirteen out of fourteen cases of syphilitic disease of the meninges. It was negative in two cases of congenital syphilis and eight non-syphilitic cases. I can confirm their conclusion that the test is positive in cases of syphilis of the nervous system far oftener than the Wassermann reaction, so that it is of much less aid than the latter in enabling one to distinguish between general paralysis and cerebro-spinal syphilis. It is chiefly of value in connection with the diagnosis between general paralysis and other non-syphilitic mental affections.

Noguchi considers that the test is a much sharper and clearer one than the Nissl-Nonne ammonium sulphate one described above. In my experience this is certainly true, and I would attribute the conflict between the results recorded by Nissl and those recorded by Nonne and Apelt to the difficulties of the test they employed. It seems to be much less delicate an indicator of small quantities of globulin than either the Noguchi test or the one now to be described. This consists in modifying the

Nissl-Nonne test so as to perform it in the same way as the Heller test. The fluid is taken up in a fine pipette, and allowed gently to flow on to the surface of the ammonium sulphate solution. The solution must be quite saturated, and this is best attained by dissolving the salt in boiling water, and keeping an excessive amount of salt in the vessel containing the solution. Care must be taken that the salt is pure, so that the solution is neutral in reaction. When the test is positive a ring forms at the junction of the two liquids. The ring is rather characteristic in having a clear white appearance, and, in being fine, compact and sharply cut. After about half an hour it assumes a mesh appearance as of a delicate cobweb. It must be viewed in an indirect light. Some idea of the quantitative intensity of the reaction can be obtained by the Brandberg method—that is, by forming a standard estimate according to the length of time before the ring appears; the fluid can be tested for this purpose at different dilutions. It is hard to say what are the actual physical conditions present at the junction of the two liquids. It may well be that penetration of the ammonium sulphate solution occurs so gradually as to permit the formation of a layer in which the concentration is only a third—namely, the concentration which is most favourable for the precipitation of euglobulin as distinct from other globulins.

The results obtained by the ring test agree almost exactly with those obtained by Noguchi's test, and it offers the advantage of being more readily applicable. It is never positive in the normal, and up to the present I have never found it positive in any chronic affection except active syphilis, or metasyphilis, of the nervous system. The cases mentioned above, therefore, need not be again enumerated.

SUMMARY.

An increase in the globulin content of the cerebro-spinal fluid almost invariably occurs in general paralysis; the proteid concerned is a euglobulin. A peculiar quali-

tative change takes place in this euglobulin, and is associated with the formation of the "anti-body," which is the active agent in the Wassermann reaction. It is here maintained that the two most accurate tests for this euglobulin are the Noguchi butyric acid test and the ammonium sulphate ring test above described. These tests are of the greatest value in distinguishing general paralysis from non-syphilitic affections of the nervous system.

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