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*Certain Physical Signs in Melancholia.**

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THERE is but one important preliminary to my paper. It is that I wish to take this opportunity of expressing my most sincere thanks to the medical officers of Bethlem Hospital (Dr. Percy Smith, Dr. Hyslop, and Dr. Craig) for their great kindness and courtesy in putting every facility in my way while I was making the following observations, for the interest which they took in my work, and for the valuable suggestions which they so willingly afforded me from time to time.

It is now nearly a quarter of a century ago since Kahlbaum described the symptom-complex, which he called and we now know as Katatonia. This is a form of alternating insanity, in the melancholiac stage of which there is a secondary alternation. Rhythmical forms of movement and of speech alternate with rigidity and mutism. The rigidity, as Kahlbaum further pointed out, affects mostly the muscles of the neck and shoulders.

I now wish to draw attention to the fact that rigidity of this nature is not confined to cases of katatonia. It also exists in, as I think, all cases of melancholia to a greater or

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less degree; but it is especially marked in severe cases of the disease, and especially in those cases in which there is an element of stupor.

Now, if the nature of this rigidity be examined more closely, it will be found that it is most marked in the muscles of the trunk and neck, that it is less marked but very strikingly present in the muscles of the shoulders and hips, that it is again less marked at the elbows than at the shoulders—less marked at the wrists than at the elbows, and that it is practically absent from the fingers. Similarly the rigidity is less marked at the knees than at the hips, very slight at the ankles, and again practically absent from the toes.

In order that my premises may be clearly understood, I repeat that rigidity of this nature is discoverable in all severe cases of melancholia. It has appeared to me that it is most marked in those cases which suffer a large amount of mental pain, while it is difficult to detect in slight cases of the disease.

It will be observed that this rigidity is of a nature the converse of that which occurs in ordinary cases of hemiplegia, where it is more marked at the fingers than at the wrist—more marked at the wrist than at the elbow, and more marked at the elbow than at the shoulder. Similarly in hemiplegia, the rigidity is more marked at the toes than at the ankle—more at the ankle than at the knee, and more at the knee than at the hip.

It will be convenient for the sake of brevity to refer to this latter form—the form which occurs in ordinary hemiplegia—as peripheral rigidity, and to that form which I have described as occurring in melancholia as proximal rigidity.

In making observations upon this point it is merely necessary to grasp the limb and move it about, taking it segment by segment. For example, in the case of an arm, the elbow would first be grasped, the upper arm moved about on the trunk, and the amount of rigidity observed. The forearm should then be grasped, alternately flexed and extended upon the upper arm, and the degree of rigidity compared with that observed at the shoulder. The hand should then be moved upon the forearm, and so forth.

Two fallacies must be avoided, both of which are dependent upon the patient's attention being attracted to the observation. The first is that he may voluntarily resist the movement, and the second is that he may, on the other hand, so to speak, acquiesce in the attempts to move the

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limb, and move it himself in what appears to him to be the desired direction, thus masking the rigidity.

I look upon this rigidity as being more or less universal in the melancholiac, affecting also the bilaterally acting muscles; for example, among details, I regard the wrinkling of the forehead as rigidity of the frontales muscles.

My reasons for believing this proximal rigidity to be a true physical sign of melancholia are—

(1) That it does not occur in other forms of insanity.

(2) That it disappears from the patient as he gets well.

(3) That voluntary rigidity is of the peripheral type.

This is best observed in a resistent child.

Since rigidity is frequently associated with paralysis, one naturally endeavoured to ascertain whether there was any weakness of movement at those joints where the rigidity was most marked. This paralysis, I think, I have detected. There is very little weakness to be detected in the elbow or wrist movements, but if such a patient as I have described be asked to hold his hands straight above his head, he has difficulty in doing so; and it will be observed in extreme cases that the upper arm is not nearly held vertically, and that the elbow is not quite fully extended, even when the utmost persuasion is used to get the patient to assume this attitude. This symptom has again seemed to me to be most marked in those patients who suffer a large amount of mental pain, especially if associated with an element of stupor.

As a corollary to this observation, I have another one, which is this. There are usually in a large asylum one or two female melancholiacs who can be induced to knit or sew, but who cannot be induced to do housework; on the other hand, it is rare to find a patient who is willing to do housework but unwilling to perform the fine movements of knitting or sewing. It is difficult to make many observations on this point; because there are usually cogent reasons which demand that the patient must be put to one or the other, especially the disposition to suicide. Patients who are willing to perform the grosser movements of housework, but unwilling to knit or sew, are generally people who never did knit or sew. The observation of this paralysis has previously been put in other ways; I am now merely putting it in a new light. For instance, it is an old observation that the attitude of the melancholiac is one of general flexion. I now submit that this attitude, which is also seen in senility and in paralysis

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agitans, is an attitude of weakness, or in other words of slight—very slight—paralysis. There is, at least, paralysis in effect. Again, the melancholiac patient will himself tell us that he has difficulty in doing things, and Dr. Bevan Lewis bases his hypothesis with regard to melancholia entirely on this symptom. There is, as he puts it, failure in the muscular element of thought.

The condition is one of very slight double hemiplegia, and hence it would be expected that there should also be weakness of the bilaterally acting muscles. The tenderness, if I may so speak of it, of the melancholiac to noise is probably due to weakness of the tensores tympani, and I hope to show, in a subsequent communication, that the indistinctness of vision of these patients, by which people—for instance—look dead to them, is due to temporary weakness of accommodation.

Kahlbaum also described, in the melancholiac stage of katatonia, a diminution of the normal number of nictitations of the eyelids, and also a diminution of the amplitude of the movements of the chest in ordinary respiration. I have been unable to confirm these signs either in katatonia or in melancholia; and, indeed, I should not expect to find them. These movements are involuntary, and are not therefore of cerebral initiation; they are movements relegated to a lower level of the nervous system. I might expect weakness of the power of voluntarily screwing up the eyelids and of voluntarily taking a deep breath, but not of involuntary nictitation and respiration.

In the explanation of these phenomena I make use of a principle first enunciated on theoretical grounds by Dr. Hughlings Jackson, and subsequently confirmed by direct observation by Dr. Bevan Lewis. I refer to the fact that movements at the large joints are represented in the cortex by large cells, and movements at the small joints by small cells. This is now well established.

It will be best at this point to digress for a moment to consider a mathematical fact as regards solid bodies. Taking a sphere for example, it will be remembered that the content varies as the cube of the diameter, while the surface varies as the square of the diameter. The practical bearing of this is that small bodies have a larger surface relative to their content than large bodies, although of course they have a smaller absolute surface. This is why a small body cools so much more rapidly than a large body. Similarly, the large

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cells of the cortex have a smaller relative surface than the small cells.

It seems likely that some affection of these cells would account for a rigidity and paralysis which affects the joints according to their size, and again it would appear right to suppose that the change is metabolic in its nature rather than structural. Further, it is obvious that such a metabolic change must depend either on intrinsic changes within the cell itself or on changes in the environment of the cell. If we suppose some change in the environment of the cell (for instance, anæmia or some toxin circulating in the blood or lymph), the small cells would be affected more than the large cells, because they have a greater relative surface exposed to the deleterious influence. Under such circumstances the rigidity would affect the small joints more than the large joints,—that is to say, it would be of the peripheral type. Such rigidity occurs in the second stage of chloroform anæsthesia, where we have a poison circulating in the blood. I have made several observations on this point. But we have already seen that the rigidity of melancholia is of the converse or proximal type. Let us, then, suppose what appears to me to be the only other alternative, that the cells of the cortex contain within them some deleterious substance, some effete product of their own metabolism. In such a case it is clear that the large cells would be more affected than the small cells, because the large cells have a smaller relative surface from which to get rid of their products of metabolism. And this agrees with my observation that the rigidity affects the large joints more than the small joints. I submit, therefore, that in melancholia the cells of the cortex cerebri fail to some extent in the excretion of their metabolic products.

Having arrived at such a conclusion, it was in the natural order of things that, as a physician, one's thoughts should next be directed to the investigation of the effect upon the melancholiac of those drugs which have the property of causing cells to excrete their metabolic products. There is one drug, viz. pilocarpine, which we know to have this property *par excellence*. Pilocarpine is a very appropriate drug to try, because it acts, as Langley has shown, upon the cells themselves, and its action is not to any great extent dependent upon modifications of the circulation or upon the innervation of the cells.

Accordingly, observations were made on the effect of pilo-

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carpine on melancholiacs in two ways; to wit, by the administration of infusion of jaborandi by the mouth, and by the subcutaneous injection of pilocarpine.

In these observations one discovered the very striking and remarkable fact that melancholiacs are exceedingly tolerant of this drug, or, in other words, that they react very feebly to pilocarpine.

After gradually increasing doses, four patients were treated regularly with 4 ounces of the infusion three times a day, and in one case four times a day (*i.e.* 16 ounces in the twenty-four hours), and yet the reaction to the drug was practically *nil.* The skin was scarcely more than comfortably moist; salivation was not perceptibly increased, nor was there any marked contraction of the pupil. The only control observation was made upon myself, in whom a single dose of 2 ounces of the same infusion caused profuse perspiration and salivation.

In the above cases, too, the mental effect of the drug appeared to be practically *nil.* K. W—, the patient who had 16 ounces in the day, a case of melancholia attonita, was being artificially fed at the time. She was more excitable on each of these occasions than when she was not under the influence of the drug, and she used unparliamentary language, which, again, was not her wont when she was not under the influence of jaborandi. Otherwise these patients were quite unaffected. As regards the rigidity, it seemed to be very slightly, if at all, relieved.

The next series of observations was made with hypodermic injections of pilocarpine—$\frac{1}{3}$ gr. of the nitrate on each occasion. As it was desirable to have some form of measurement by which the reaction of cases of melancholia might be compared with that of other cases, and also among themselves, I devised the following method. Discs of ordinary Swedish filter-paper were used. One of these was placed upon the patient's back between the shoulders, and covered in with a piece of ordinary gutta-percha tissue, such as is used for covering in fomentations. The gutta-percha tissue was held in place over the disc of filter-paper by means of strips of lead plaster strapping. The object of covering the paper in this way was, of course, to prevent evaporation. It was then noted what interval elapsed between the time of the injection of $\frac{1}{3}$ gr. of nitrate of pilocarpine into the arm of the patient, and the time when the disc of filter-paper was uniformly just saturated all over with moisture.

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The results of the control observations were as follows:

In the case of patients who had not had an injection of pilocarpine the paper remained comparatively dry at the end of several hours. About twenty cases were observed under these conditions.

Five control observations were made in the other direction; three on cases of acute mania, one on a case of anergic stupor, and one on myself. Of these the shortest interval was in my own case, in which the paper was saturated in two and a half minutes; and the longest was in the case of stupor, where it was three and a quarter minutes. The cases of mania took about three minutes on each occasion. In all these cases the perspiration was very profuse.

Twenty-six observations were made on cases of melancholiacs. None of these were of more than twelve months' standing. In five of these cases the paper was still comparatively dry after an interval of two hours. At the end of this time one of the patients was sick.

The following are the times for the other patients:

	Min.		*Min.*		*Min.*
M. C.	10	J. C.	4	Miss B.	9
K. W.	11	Mrs. F.	10	Miss K.	13
J. L. C.	10	B. E. J. H.	10	Mrs. P.	12
A. E.	9	Mrs. Wby.	3	Miss H.	17
R. P.	12	Mrs. Wly.	10	Mrs. J.	19
E. R.	16	Mrs. B. (saliv.)	26	Mrs. J.	10
Miss P.	11	Miss C.	10	Mrs. C.	21
A. F.	12				

Leaving out, of course, the cases in which there was no reaction at all, the average of these numbers gives about twelve minutes as the time for reaction of a melancholiac to $\frac{1}{3}$ gr. of nitrate of pilocarpine as against three minutes for other people.

With regard to the case in which the paper was saturated in three minutes, it is only fair to state that the original diagnosis was acute mania, and although the patient developed melancholic ideas subsequently, she was still incoherent and deficient in self-control. This case suggests that the reaction may possibly be useful as a help in diagnosis.

Before proceeding farther, let us here pause for a moment to consider the pharmacology of pilocarpine. As Langley has shown, pilocarpine is a drug which acts upon the cells themselves, causing them to excrete their products of metabolism. Binz, of Bonn, came to the conclusion that this drug acted through the influence of the nervous system.

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The difference between the results of these two observers depends upon the difference of their methods. Langley paralysed the nerve-endings by means of, if I remember rightly, nicotine, and noted the action of pilocarpine there and then; while Binz waited for divided nerves to degenerate before making the observations. The obvious result of this latter method was that the cells degenerated before the effect of pilocarpine was tried.

Coupling, then, my observations with those of Langley, my conclusion is that in cases of melancholia the cells of the tissues throughout the body have their function of excretion diminished. I have already pointed out how the nature of the rigidity in melancholia leads to the same conclusion *quâ* the cells of the cortex cerebri.

And if we review the symptomatology of melancholia I think it will be agreed that it is in accordance with this view of the pathology of the disease.

The diminution of the quantity of saliva and the furred tongue indicate diminution of the buccal secretions. The indigestion and loss of appetite with consequent refusal of food are more than probably due to insufficient secretion of the digestive juices.* The constipation may be also explained on this hypothesis, although the partial paralysis—to which I have already referred—probably plays an important part. It is certain that the fæcal accumulation of the melancholiac is excessively deficient in moisture.

A striking example of this deficiency of secretion is further afforded by puerperal cases. Those who are attacked with insanity within the first week or so after parturition are usually cases of mania, and it is frequently necessary in these cases to treat the patient with belladonna or potassium iodide to arrest the secretion of milk. But those who are attacked later than this usually become cases of melancholia, and with these it is noteworthy that the secretion of milk is arrested by the disease, and at the time when the secretion is normally at its height. In those institutions where the breasts are treated by merely squeezing out the milk before a fire, the nurses always have more trouble with the breasts of maniacs than with those of melancholiacs.

The urine is also diminished in quantity in melancholia, but this probably has no bearing on my point. Most of the constituents of the urine have merely filtered through the

* In this connection *vide* Dr. Greenwood's paper in the January number of *The Journal of Mental Science.*

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glomeruli of the kidney from the blood into the Bowman's capsules. Very few of the constituents are of the nature of a true secretion; that is to say, they are not built up by the cells of the kidney before excretion. And those constituents which are formed in this way (hippuric acid, for example) are so variable in quantity under normal conditions that it would serve no useful purpose to estimate them in the melancholiac. The fact that the quantity of urine is diminished in the melancholiac is, I imagine, merely due to the fact that he does not drink so much as a healthy individual.

I will refer to but one other symptom, viz. the amenorrhœa. Amenorrhœa is very much more common in melancholia than in other forms of insanity. In the consideration of this interesting symptom the question arises as to whether it is the part played by the maturation of the Graafian follicle which is at fault, or whether it is the part played by the uterine mucous membrane. I am inclined to think that most physicians would ascribe the amenorrhœa to failure of the changes which normally take place in the ovary at the time of menstruation, but it is a point which remains to be scientifically settled. It seems to me that light might be thrown on this question by the study of cases of melancholia which begin shortly before marriage. These cases are not very infrequent, they are usually classed as post-connubial insanity; but closer inquiry into the history often reveals that there were some symptoms of insanity before marriage.

In looking up some past records I came across notes of thirteen cases of this kind; but in not one of them was the menstrual history sufficiently detailed to throw any light upon this point.

It must be admitted that we are here rather trespassing upon unknown land; but if the fault were proved to be at the Graafian follicle, and if the maturation of the follicle be of the nature of a secretion (as I am inclined to think it is), then this symptom is also to be explained by the pathology which I have suggested.

In submitting the above pathology it is not to be understood that I suppose these metabolic changes in the cells to be the cause of melancholia, nor do I suppose the mental changes to be the cause of the physical. Here, as in the domain of normal psychology, I adopt the view of psycho-physical parallelism. All that I submit is that the physical changes which I have described go on *co et par* with the psychical

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changes. What the nature of the connection is between the two I do not know, and it would open up too wide a question for discussion were I to enter upon this point here.

Similarly it would be opening up too wide a question if we were to discuss whether these physical changes in melancholia are mere exaggerations of what occurs in physiological melancholy (as is quite probable), or whether they are limited to the conditions of disease.

In conclusion, gentlemen, I must thank you for the kind and patient manner in which you have listened to what I have had to say. I cannot ask you now to discuss the merits of my observations on the nature of the rigidity and paralysis, or on the pilocarpine reaction. The value of these observations must depend entirely upon their confirmation or denial by other observers.

I hope, however, that I may have the great advantage of your friendly criticism of my deductions and conclusions. These may be right or wrong; but should the observations be confirmed, they cannot but add to the data from which some abler mind than my own may some day present us with a real advance to our knowledge of the constitution of mind in health and disease.

Discussion.

Dr. MERCIER—To me, sir, this paper is a very interesting one indeed. It is a very remarkable fact that this peculiar distribution of the rigidity which Dr. Stoddart has noticed is precisely the distribution that I assigned many years ago to certain other phenomena, but especially to rigidity in certain conditions. He terms it proximal and peripheral; I called it centrifugal and centripetal. There are certain phenomena, and especially there are certain rigidities, which begin at the periphery, in the fingers, and are most marked in that position, and which diminish as we approach the trunk and the bilaterally acting muscles. There are certain other rigidities again which are most marked in the great muscles of the trunk and the neck, and which diminish as we go towards the periphery. It is a corroboration of the correctness of Dr. Stoddart's observations that the rigidity he has noticed follows a classical order; because, as he has stated, it is most marked in those muscles which follow a natural order and in which rigidities have been observed to occur before. I have no doubt that he is perfectly correct in putting down the transverse corrugation of the forehead to the extension of the rigidity to another bilaterally acting muscle. He did not mention the condition of the masseters. Now I have always found in these rigidities which I call centrifugal, that is to say, which are most marked in the great trunk muscles and diminish towards the periphery, that the muscles of mastication are associated with the muscles of the trunk; whereas the muscles of the lips and the muscles of articulation are exactly at the diametrically opposite end of the scale. I have not noticed whether the masseters and other muscles of the jaws are rigid in melancholia, although now that Dr. Stoddart draws one's attention to it, it becomes manifest to one's mind that this rigidity does exist in cases of melancholia, and especially in cases of stupor. We have all noticed it, but not with that vividness of perception which has enabled us to give the importance to it that Dr. Stoddart clearly has. Then with regard to his explanation, it is

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impossible for me at present to follow him in all his conclusions, but I would point out that his description of the rigidity is that it occupies a certain definite region in a certain definite order, and that that particular order and region are complementary to another order and region. Then he puts this down to certain changes or certain want of changes in the cells. My explanation was a very different one. My hypothesis is that the movements in which the smallest muscles—the fingers, the lips, and the movements in articulation—take the lead, and are, as it were, the great motive of the movement, are cerebral movements. They are produced by the action of the cerebrum; and that the movements which are first and last, the movements of the trunk, the neck and masticatory muscles, which gradually spread, if they spread at all, away from the trunk towards the smaller muscles, are cerebellar movements. That the cerebrum and cerebellum represent movements of the body in two opposite orders, the cerebrum representing them from the smallest muscles to the greatest, and the cerebellum in the opposite direction. A classical illustration of a wave of cerebellar influence is observed in yawning. When we yawn the trunk straightens out, the masticatory muscles are thrown into movement, and gradually the wave spreads to muscles that are smaller and smaller, and further and further away from the centre of the trunk. Although the attitude assumed is not the same, this is the order in which the muscles are affected in the rigidity which Dr. Stoddart has described to us. The fact that this rigidity that he has observed does follow precisely the order, which is a classical order, and has been observed in very many rigidities, I think goes very far indeed to speak for the accuracy of his observations. As to the explanation that he has given, well, it may be correct or not. My explanation is that I believe in melancholia the cerebral influence is weakened, and when this is so the cerebellar influence is proportionately strengthened. Hence, the due balance not being maintained, the cerebellum overacts, and produces too great rigidity of the muscles which it most prominently supplies. This may be the correct explanation, or Dr. Stoddart's may be the right one; but in any case we must recognise that the observation of rigidity occurring in melancholia is very important, and one that is very likely to meet with important results.

Dr. CONOLLY NORMAN—I hardly feel competent, sir, after the remarks made by Dr. Stoddart and by Dr. Mercier, to discuss the deeper aspects of this question, or, as I might call them, the speculative aspects. There are, however, certain clinical facts that suggest themselves. We were told by Dr. Stoddart that the rigidity in melancholia occurred in a certain order, and that fact I am not prepared to dispute. I have no doubt that Dr. Stoddart's observations are more accurate than any I have ever made on the subject, and that he is in a general way correct, but rigidity in cases of melancholia is not unknown in the fingers. Not unfrequently in such cases, if the patients be neglected, the fingers become permanently rigid. Now he has spoken of a certain degree of paralysis which accompanies this rigidity, and he speaks of testing the patients by making them elevate their hands straight over their heads. I am not quite clear that this is a sufficient test by which we could say that the larger muscles of the arm and shoulder are paralysed, because we all know that the muscular system in cases of extreme melancholia is, to use a very slipshod clinical word, relaxed, and no doubt weakened, sometimes absolutely wasted; but this is a general nutritive change, and could hardly be classed as paralysis in the strict sense of the word. With regard to wrinkling of the forehead, the explanation is that it is due to a condition of tension in the frontalis muscle. If that is so in melancholia, does that explanation cover the wrinkling of the forehead which is a familiar indication of mental trouble in the physiological state? There is something further than the mere engagement of a certain portion of the brain. With regard to the state of the respiratory movements, it is a very old observation, and I have many times confirmed it clinically, that the respiratory movements in melancholia are feeble and shallow. One also certainly

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sees diminished nictitation. Cases are on record, and I remember one case which I could not distinguish from melancholia, in which the flies used to drink the accumulated secretion out of the corner of the patient's eye without producing winking. Now this is obviously diminution of reflex action, and is probably not due to any changed condition either in the cerebral or cerebellar cells. Similarly the general diminution of all the secretions must be connected with some general cause not merely due to change in particular brain cells. There is another matter upon which I should like to say a word or two. When I read the title of Dr. Stoddart's paper it occurred to me that he was about to mention that very curious condition that we are all familiar with in melancholia agitans, which is hard to fit in with any of the theories that we have heard about the condition of the brain. May be there is an explanation that does not occur to me. In the state to which I refer there is a tendency not only to general large movements, which are, of course, not very common in melancholia, but that tendency to small movements which I am in the habit of describing to my class as small pseudo-purposeful movements. A patient takes his dress and picks it thread by thread to pieces, as if he had some task to accomplish. The movement, though apparently voluntary, is, no doubt, to a large degree to be considered as an involuntary, perhaps I might almost say an automatic movement; and this condition of the muscular activity is one which I cannot reconcile with Dr. Stoddart's theories regarding the conditions in melancholia which he has described. It is, however, a condition which one clinically sees, and which needs to be accounted for in any theory which deals with the general muscular condition in melancholia.

Dr. JULIUS MICKLE—I understand the reader of the paper to say that a condition of rigidity exists in all cases of melancholia. That is a statement I should not be willing to allow to pass in this Association without raising a dissentient voice. The muscular condition of cases of melancholia varies so much that it is impossible to accept rigidity as the universal condition of the muscular system in that particular affection. It all depends upon the kind of melancholia—the clinical form it takes. There is a form in which rigidity is the chief symptom. One has been accustomed to point this out to students, and to show them that the most significant and most characteristic feature of that rigidity is the difficulty in raising the bowed head, the muscles of the neck being so strongly contracted.

Dr. STODDART—It is my misfortune and my fault that I have not read Dr. Mercier's work on the subject. I feel sure that anything written by him would throw much light on the subject, and it is *no* matter for great surprise that both Dr. Mercier and myself have turned to the fertile brain of Dr. Hughlings Jackson to obtain our explanations of the rigidity and the paralysis. The explanation of influx from the cerebellum* and complementary influx from the cerebrum is, of course, exceedingly likely, but it seemed to me that the pilocarpine experiments and also the general observations of the secretions throughout the body point to the same conclusions as I have arrived at from the observations on the rigidity. Further, the result of clinical observation goes to show that proximal rigidity occurs if cerebellar influx be cut off, and peripheral rigidity if cerebral influx be cut off. On Dr. Mercier's hypothesis, therefore, the muscular conditions associated with melancholia are referred to cerebellar mischief. This, I submit, is not probable. With regard to the masticatory muscles, I am afraid I have not come to any conclusion. I have tried to observe whether there was any rigidity of these muscles, but it is very difficult to notice without inducing resistance on the part of the patient. Of course I quite agree with Dr. Norman that rigidity of the fingers does occur, but what I maintain is that the rigidity there is nothing like so marked as higher up the limb. If

* In this connection *vide* Horsley and Löwenthal's paper (*Proc. Roy. Soc.*, 1897).

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the rigidity be greater at the fingers than it is higher up the limb, then I should conclude that there was something more than melancholia. I entirely avoided discussing the question as to whether melancholia was a mere advance upon physiological melancholy. With regard to the movements of small joints in melancholia agitans, I think it quite agrees with the explanation which I have attempted to set forth, namely, that the finer movements are still at work, while the grosser movements are more or less paralysed and rigid. Rigidity is present in cases of melancholia agitans, for while the movements are going on in the fingers it will be noticed that there is sometimes a certain amount of rigidity in the movements of the shoulders and elbows. With regard to the constancy of the rigidity, as I have said before, in the very slight cases of melancholia it is extremely difficult to observe. I think if Dr. Mickle makes the observation in the same way that I have done he will find that rigidity almost constantly varies *co et par* with the severity of the melancholia.

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*Alcoholism and Suicidal Impulses.** By W. C. SULLIVAN, M.D., and Stewart Scholar, R.U.I., Deputy Medical Officer H.M. Prison, Liverpool.

THE important part played by alcoholism in the causation of suicide has been abundantly recognised by all observers of both these social phenomena; and so far as debate now touches the question, it is merely to deal with points of detail.

In the present slight contribution to one such detail of the subject I have endeavoured to show, by the analysis of a series of cases of alcoholism associated with suicidal tendencies, in what mode and under what special conditions the intoxication determines the development of these tendencies. For this purpose I have utilised, with the kind permission of my colleagues, the clinical records of 142 cases in which persons have been charged in the Liverpool police courts with attempting to commit suicide, and have been sent on remand to Walton prison, where they have been subject to medical observation. As the practice of so remanding prisoners charged with this offence is almost invariable, the figure named represents practically the total number of futile attempts at suicide in the city of Liverpool during the period of eighteen months covered by the records.

I shall first submit in detail the analysis of these cases, and subsequently discuss the inferences which they seem to suggest.

(*a*) *Proportion of cases due to alcoholism.*—Of the 142 cases, 64 (45·1 per cent.) were in males, 78 (54·9 per cent.) in

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