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Part I.—Original Articles.

The Biological Conception of Insanity. By JOHN TURNER, M.B., Medical Superintendent, Brentwood Asylum.

I.

I often hear it said that pathological anatomy has proved a failure in the attempt to solve the problems of insanity.

But has it? Even at the present time many of those cases which one is accustomed to hear quoted as failing to show pathological changes of a nature to account for their abnormal mentality, do show anatomical changes in the brain, and appearances which I have suggested are evidence of defective development of the nerve cells.

Kidney.—Naturally, the wider our field of examination of the tissues of the insane, the rarer is it to find any without some grave pathological change. Even in such fields as I can speak of from personal experience, I have found for a large series of cases, excluding general paralysis, pathological changes generally in the nature of vascular nephritis, or arterio-sclerosis of the kidney, in 66 *per cent.* males and 78 *per cent.* females. Quoting from last year's figures of those cases whose kidneys were histologically examined, and which include chronic as well as acute, I find pathological alterations in 41 *per cent.* of males and 56 *per cent.* of females, and seven of the males and eighteen of the females were under fifty years of age. The

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kidney in my experience is the organ which is most frequently and most markedly affected in insanity, compared with those organs which I have chiefly studied. This fact was noted some ten or fifteen years ago in a paper by an American writer, whose name I forget, on kidney symptoms in insanity, and the conclusion which this writer came to was that insanity was a symptom of kidney disease. However much we may dissent from such a statement it seems to me the observations from which it was deduced at all events show the frequent implication of the kidney in insanity.

Parenchymatous changes occur to a less extent, but fatty degeneration of the tubal epithelium is very common, although often overlooked in default of using the special technique which is needed to show it.

Liver.—The liver appears to be neither so frequently nor so severely affected as the kidneys. Fatty infiltration and degeneration is found in about 50 *per cent.* of females, and 26 *per cent.* of males. Sometimes this change when of the nature of infiltration may have very little signification, but it is not always easy to determine how much of the fatty change may be infiltration, and how much degeneration. Most cases of acute delirium show pronounced fatty degeneration of this organ.

Cirrhosis is met with, but generally to a very slight degree, in 65 *per cent.* of the males and 37 *per cent.* of the females. It is usually a slight portal cirrhosis, except in cases of polyneuritic psychosis, in which it is a very marked feature.

Gallstones are much more frequently met with in the female :—In 1912, males 4·5 *per cent.*, females 15·5 *per cent.*; in 1913, males 3·3 *per cent.*, females 17 *per cent.*

Heart and bloodvessels.—Valvular lesions are infrequent among the insane. Mitral are more common than aortic, *e.g.* among the autopsies in the Brentwood Asylum, extending over a number of years, mitral lesions, generally stenosis and of slight degree, were found in 21 *per cent.* of males, aortic lesions in 14 *per cent.*, among females, mitral lesions in 14 *per cent.* and aortic in only 1·5 *per cent.*

A developmental defect found in the heart of the insane may have some significance. I refer to a patent *foramen ovale* which I found some years ago among 235 consecutive autopsies in nearly 16 *per cent.* The aperture was frequently

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large enough to put a pencil through, and although generally valvular, was in eight cases direct.

Bloodvessels.—Pathological changes in the arteries examined (including aorta, coronary arteries, basal, renal, and cerebral) were found in 77 *per cent.* of males and 81 *per cent.* of females. This is not including the periadventitial changes in general paralysis of the insane.

Naturally when the cases are divided into age periods the greatest incidence of disease is seen in the later, but even in the earlier, containing only persons under thirty, it is interesting to find that both among males and females over half the cases which die show arterial degeneration, and further among recent forms of insanity (recent that is as far as evidence obtained allows us to decide this question) the percentage of vascular lesions is not much below the general average, *e. g.* among forty female cases it was found in 72 *per cent.*

Aorta.—Taking all ages and forms of insanity, the incidence of atheroma is rather more frequent among males than females, being 58 *per cent.* and 50 *per cent.* respectively. Last year's figures gave 59 *per cent.* of men and only 26 *per cent.* of women. Under thirty years of age it was slightly affected in 37 *per cent.* males and 8 *per cent.* females. From thirty to fifty-nine it was affected in 57 *per cent.* males and 55 *per cent.* females. From sixty and upwards it was affected in 71 *per cent.* males and 93 *per cent.* females.

With advancing age the changes seem more severe among the women, and calcification was five times more frequent than in males.

Basal vessels.—Disease visible to the unaided eye is much less frequently met with than in the aorta.

Among insane males of all ages I only found it in 13 *per cent.*, and amongst females in 16 *per cent.* Below thirty years of age it is not found.

Finer cerebral vessels.—Hyaline thickening was found in 44 *per cent.* males and 37 *per cent.* females. Endarteritis only noted in 9 *per cent.* and 2 *per cent.* males and females respectively.

The pathological alterations were noted as frequently under thirty as between thirty-nine and fifty-nine, but from sixty upwards there is a marked increase in both sexes.

Renal vessels.—Pathological changes in these vessels are far

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more frequent and more severe than in the brain. About half the male cases and 71 *per cent.* of the female show marked nodular endarteritis. Generally, it is the larger vessels of the pyramids which are affected, but sometimes all the smaller arteries of the cortex show patchy thickening of the intima.

What I have to say concerning cerebral lesions I shall refer to when discussing cerebral anomalies.

Thus even in the very limited field that I have gone over, grave pathological changes are noted in the majority of all cases of insanity who die, changes so far referring only to the bodily viscera.

But consider how very limited at present are the fields which have been explored, and at best incompletely explored! We may say that practically we are ignorant of all pathological knowledge concerning the sympathetic system. We know very little about changes in the posterior spinal ganglia, and not much about changes in the peripheral nervous system in relation to insanity (except perhaps in connection with polyneuritic psychosis).

Then the whole subject of the internal secretory glands is only just beginning to receive the attention it deserves, and no doubt in a few years' time the observations recorded in diseased conditions of these glands will materially help to swell the evidence in favour of an anatomical basis for insanity.

I submit that insanity is the expression of disorder of the brain the result of an anomalous condition of this organ, to which is superadded in most cases the evil effects of bodily disease, and by this I mean disease of other viscera than the brain.

I have already lightly touched on some of the diseased conditions met with in my own experience, and drawn attention to the large gaps there are in our knowledge of diseases of certain parts of the body, more especially the internal secreting or ductless glands. Even in my limited field of observation, it is comparatively rare to meet with a decently healthy insane body, no matter how young, at death.

I propose now to refer to some cerebral appearances seen in insanity, chiefly in the nature of anomalies, aud to contrast these with those seen in the brains of patients dying in general hospitals, Guy's and London.

Before dealing with my own material, I shall very briefly

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refer to J. Bolton's researches, very briefly because the reader is as familiar with them as I am.

Bolton's classification of insanity differs very considerably from those ordinarily held, and is somewhat difficult to collate.

He divides the insane into cases of amentia and dementia.

Amentia constitutes the mental condition of patients who suffer from deficient neuronic development, and includes all idiots and imbeciles, paranoiacs, cases of recurrent insanity not liable to develop more than mild dementia (so that this class can only be placed after they have lived out the allotted span of life, and if they should die early of some intercurrent disorder, can never be placed), cases of chronic insanity without visible dementia, hysteria, and true epileptic insanity.

Dementia is the mental condition of those who suffer from the effects of neuronic degeneration following insufficient durability (of neurones).

He leaves us in the dark where to place the large class now called dementia præcox. I suppose he is one of the few who does not recognise it, or perhaps I should say *did* not recognise it, as his monograph was published some years ago.

Although Bolton's classification does not seem to me entirely satisfactory—what classification is except in its author's eyes?—I have always considered his work, and his deductions from his pathological studies, as most valuable.

In one main point I am in complete accord with him, namely that there is an inherent cerebral defect in all cases of insanity, either deficiency or defective durability of neurones. I shall not refer in detail to the appearances he studied within the skull cap in his two hundred consecutive cases, from which he deduced that the morbid changes vary in degree with the amount of dementia, and that severe grades of dementia are associated with a much more extreme degree of vascular degeneration than occurs in non-dements, or mild dements of the same age.

As regards local atrophy of the cerebrum, he found that it was "greatest in amount" in the following regions, in the order named:

(1) Prefrontal.
(2) Frontal first and second.
(3) Ascending frontal and Broca.
(4) First temporal, and superior and inferior parietal.

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(5) Remainder of the cerebrum.

Although somewhat differently phrased this substantially, I suppose, is tantamount to saying that this is the order of frequency in which local atrophy occurs in these regions. If so, I should like to contrast it with a series of 172 cases of mine, in which, although I found the prefrontal most often affected, yet my other findings differed considerably:

(1)	Prefrontal most frequently affected . .	92
(2)	Parietal	56
(3)	Frontal (1st, 2nd, and 3rd) . . .	38
(4)	Central	28
(5)	Temporal	9

Bolton has shown that in the cases which he studied of amentia there was a deficiency in the width of the pyramidal layer of the cortex as compared with its width in normal cases. This deficiency was found chiefly in the prefrontal region—the region he specially studied; and he has produced evidence, embryological and anatomical, to show that this layer is intimately connected with the highest intellectual functions, and, further, that the innermost layer or spindle layer is concerned with the more animal functions of the body.

Watson's comparative studies on small mammals' brains are confirmatory of Bolton's deductions. Unfortunately reliable micrometric observations are of such a tedious nature that it is only possible for one observer to deal thoroughly with a very few (comparatively) cases; and one has to be especially cautious in collating the results of one observer with those of another, as the personal factor is a large one. For these reasons, although it is very probable that his conclusions refer to the many cases of insanity included under his voluminous "amentia" class, it must remain for a long time only a plausible inference that in these insane there is, especially in those regions most concerned with intellectual processes, a definite lack of development or paucity of numbers of pyramidal cells, and that, with the added stress of disordered vital functions, the result of disease of the bodily viscera, we have the necessary conditions for the production of states of insanity.

My own observations on the cortical layer measurements are too scattered and unsystematised to advance either for or against Bolton's conclusions, but it has certainly struck me, in com-

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paring the histological appearances of the different cell-layers, perhaps more especially in the prefrontal regions, that the innermost cells—the spindle cells—are those which, judging by their shape, absence of shrinking, and character of nucleus, more often appear intact than the nerve-cells of the outer layers, especially that layer which Meynert terms the second, but which Bolton includes in his pyramidal (Meynert's third) layer, an appearance which I take it is confirmatory of Bolton's views concerning the animal functions which the spindle-cells subserve. This, I may here remark, is equally seen in hospital cases. But we must be cautious of drawing dogmatic conclusions on such a point, because the outer layer-cells are very small cells, and probably, from their superficial position and delicate structure, are especially liable to be injuriously affected by our fixing reagents.

Many years ago I described an axonal Betz cell appearance found in persons dying insane. The first series of cases (5 or 6) where my attention was drawn to this feature were in a state of dementia (preceded in some by agitated melancholia), with paresis, involuntary jerking of limbs, diarrhœa, and death. An account of these was published in *Brain*, 1899, and two years later, Adolf Meyer, in the same journal, published an account of similar cases under the title of "Parenchymatous Systemic Degeneration of the Brain." Looking back, I am inclined to think that the appearance of the Betz cells in my first series was due to genuine reaction at a distance, or "axonal reaction," as Meyer more conveniently terms it. I believe so, as in one at least of my cases osmic acid showed definite Marchi degeneration of the large axons in the section which showed the altered cells, and I believe in all Meyer's cases Marchi degeneration was demonstrated in some part of the course of the axis cylinders.

Later on, in the *Journal of Mental Science*, 1901, I published a brief account of sixteen cases, where somewhat similar appearances were found in the Betz cells, and this last consisted of cases of dementia, melancholia, and imbecility, and I attempted to account for the association of two such conditions as melancholia and imbecility by assuming in both cases a lack of sensory impulses to the cells in question, leading in the first case to a devolution of the cell, and in the second to a lack of development. In support of these contentions I quoted

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Warrington's experiments showing that on cutting through the posterior nerve-roots of the spinal cord, certain cells in the anterior horns of corresponding segments show axonal characters, due, he surmises, to interference with their functions from the lack of sensory impulses which usually impinge upon them.

My list of cases gradually increased, and before long every known variety of insanity, was represented by sections of the cortex showing Betz cells with axonal-like characters. But it was noted that in the great majority of instances the appearances are such as are only seen in a very early stage of axonal reaction, *viz.*, large plump cell, with remarkably distinct tigroid in branches and at the border of the cell-body, the centre of which is pale and filled with tigroid in the form of fine grains; the nucleus is large and often bladder-like, and either pushed up into the apex or against one side of the cell-body, its membrane being intact.

In the asylum cases, very many sections show every Betz cell with these appearances, but I have not reckoned any case as characterised by this form unless I could count at least six cells showing clearly the character, and only counting the cells in which the nucleus was apparent. One has to make some arbitrary rule where more than one form of cell is seen in the same section.

I am now able to quote a large number of cases of all sorts of insanity, where, by the rule above noted, I can give the percentage characterised by axonal-like Betz cells. Taking all forms together, it occurs in from 35 *per cent.* to 40 *per cent.*

It is least prevalent in general paralysis of the insane, only about 20 *per cent.*

In dementia præcox it is extremely rare, if ever, that it does not occur, and we may say that one may count upon finding it in every case of dementia præcox, katatonia, and generally in these cases every cell is affected. This refers to cases examined on the point for several years past. Of the congenitally defective without epilepsy it occurs in 60 *per cent.*; with epilepsy in 70 *per cent.*

In cases showing at one time or another melancholic symptoms, often with agitation, and in certain cases of dementia, 60 *per cent.* show this change (probably in some of these cases the change is a genuine axonal reaction, and not, as suggested of the others, the result of an inherent anomaly).

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Signification.—As I have previously stated, it soon became evident to me that this form of Betz cell was not entirely due to axonal injury or disease, in fact in only a very few cases would such an explanation be at all adequate. In the first place, although sections of the cord containing the axons of the altered cells were examined, and likewise other regions through which they passed, it was very rare to find any evidence of Marchi degeneration in these structures.

And, secondly, it was noted that in a very large proportion of all the cases the cells were in what would represent a very early stage of axonal reaction, although clinically many of the cases were of very long standing. If an axonal change was to account for the appearance, almost certainly a very much larger number would have shown more advanced stages of axonal reaction. We can scarcely imagine that nearly all the cases die just when the axonal reaction is beginning.

On studying the incidence of these cells in different classes or varieties of insanity, some further light is, I think, thrown on their significance. Thus, the congenitally defective and cases of dementia præcox yield the highest percentage, and general paralytics, that class which is the best endowed physically and mentally, the lowest. Lugaro (Riv. Sperment. di Fren., 1902), states that types of cell corresponding to the phase of reaction and repair (axonal reaction) are found in certain stages of phylogenetic development, and that the slighter form of reaction is normal in some of the nerve cells of the lower vertebrates.

Van Biervliet pointed out the resemblance of the reacting cell to an embryonic nerve-cell, and I can corroborate this statement by my own observations, for in the new-born rat I have been struck by the predominance of two of the most striking features of the axonal-like cells, characterising the young nerve cells of the cerebral cortex ; I refer to the large bladder-like nucleus, and its peripheral situation.

It was from a consideration of these facts that I was led to conclude that the form of nerve-cell I am describing was in reality not a pathological condition, but an anomaly indicating a defectively developed, and, therefore, probably a cell of deficient durability.

I am not wishing to suggest that there is a direct relation between morbid mental manifestations and this appearance. But I have singled the Betz cells out, as from their large size

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and prominent structure they are convenient objects to study, and I imagine that their development or lack of development will proceed *pari passu* with the smaller and less easily studied nerve-cells. Certainly I find that when the majority of the Betz cells show these characters the large pyramids are similar. So that they may be likened to sign-posts.

For some years past I have been endeavouring to collect and examine a number of general hospital cases in order to contrast their cells with those in the insane, but I have not been able to succeed, until quite recently, owing to the kindness of Dr. Turnbull of the pathological department of the London Hospital, and Dr. French of that of Guy's. My examination is not completed, but so far as it goes, the results so far as regards the axonal character of the Betz cells are what I anticipated, viz., that they occur, but in a smaller number of cases, and also, in those cases where they are met with, a smaller number of the cells show the change than one is accustomed to find in insane cases.

It must have been merely a matter of chance that some of these hospital cases died in a general hospital and not in an asylum, but without doubt a larger proportion of what are termed sane people will fall into the hospital class. One of my Guy's Hospital cases appeared to be a general paralytic, but he was labelled as having died from uræmia.

The following is a very brief account of the cases so far examined—*viz.*, fifty—as compared with asylum cases.

The distinctive points which have struck me most in the two series are:

(1) The much greater frequency of the form of cell I have termed "immature" in the asylum cases.

(2) The greater number of subcortical nerve cells in the insane.

(3) The greater frequency of gliosis in the white matter adjacent to the cortex in the insane.

As regards yellow pigmentation of Betz cells, the occurrence of corpora amylacea (or colloid bodies), the occurrence of halo degeneration of the nerve cells, and hyaline thickening of the cortical arteries, I did not find that in any of these particulars were the asylum brains more frequently affected than the fifty hospital cases. In fact as regards excessive pigmentation of the Betz cells, this was more frequent among the hospital cases.

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I am under the impression that in a sufficiently large number of comparable cases the insane would show both a greater tendency to arterial degeneration and to the production of corpora amylaceæ, but comparing the fifty hospital cases with fifty asylum cases not selected, as I say, I find no preponderance of this change in the asylum cases.

Among the hospital cases 22·2 *per cent.* showed more than six Betz cells with axonal-like characters, but it was very rare to find a section showing more than seven or eight. Among the hospital cases were three of 5 years or under, and if we deduct these the percentage showing axonal cells is reduced to 18 *per cent.* Probably about 20 *per cent.* of a hospital population shows the feature; very much the same proportion as is shown among general paralytics, but far below that of the general asylum population. And whereas in a large percentage of insane cases (28 *per cent.*) all the cells are affected, in only 2 *per cent.* of the hospital cases was this found to be.

Subcortical nerve cells.—The comparison can only be rough in default of actually counting every subcortical nerve cell seen, which has not been done.

They are noted as numerous in 8 *per cent.* of hospital cases against 14 *per cent.* of asylum, and among epileptics only they were (some years ago) noted by me as numerous in 75 *per cent.*

Gliosis.—The distinctive character between the two series would seem to be the greater frequency of gliosis affecting the white matter subjacent to the cortex. My figures are 6 *per cent.* of hospital cases against 28 *per cent.* of asylum, and this seems to apply to the cerebellum as well. As regards zonal or molecular gliosis, although the asylum cases are more frequently affected, yet the difference in the incidence is much less than in the white matter.

In concluding this section it seems that the evidence derived from embryology, comparative anatomy, and the study of the incidence of the changes in different forms of insanity, and in the sane community, all tends to show that *the form of cell* I have termed immature is an anomaly depending on defective development.

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II.

It is with the greatest diffidence that I venture on psychological questions.

Very likely much of what I advance will be easily refuted, but nevertheless I do not think that anything said will alter my views. Probably because one of these views is that man is not a reasoning being in the sense that his conduct is guided by reason, although he supplies reasons (later on) for his conduct, he is, it is suggested, a rationalising being. This view is impressed upon me by observation, by introspection, and by physiological evidence.

Whatever our philosophy, and however firmly we believe in it, it does not influence our conduct. I am persuaded that we do things because we want to, and not because we can show adequate reasons for doing them. Our motives are of the nature of tropisms, and our characters are more the outcome of what we eat than what we think. No one acts up to his beliefs. Any business man who believes he is a Christian would infallibly be bankrupt were he to conform to the tenets he says he believes. Reason does not enter into the carrying out of the most important of all our acts—love and marriage—we fall in love with some very ordinary female purely from physiological causes, and we invest her with all sorts of desirable attributes which probably she does not possess. This is a biological act.

Scores of people from philosophical reasons have come to the conclusion that life is a dismal failure, and that death is far to be preferred, but they do not commit suicide, because, in ordinary phraseology, they funk killing themselves, or more correctly because the instinct to live is too powerful.

Balfour, in his Gifford lectures last year, referred to atomism and the theory of conservation as illustrations of beliefs having gone before evidence, and of the evidence later on justifying the beliefs. Is not this rationalisation? The evidence supplied after the belief I suppose always seems to justify the belief, at any rate to the holder of the view.

A delightful writer contributes a short leading article to the *Times* every day, generally on some psychological question, and the other day he dealt with myths, ancient and modern. It is only a short article, and I shall quote largely from it, as it seems to me to bear out what I am saying:

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Men have always enjoyed making myths about the origins of things. For instance, nearly every race or tribe of men has its myth about the origin of evil. . . . Primitive man saw evil everywhere about him, and he asked himself how it began. In answer to this question he made a story about its beginning. The scientific investigator sees religion or the drama equally prevalent, and asks himself how it began; and in answer to his question he makes, not a story, but a theory. But he usually assumes in his theory, just like primitive man in his story, that man in some remote past had some particular reason for worshipping or making plays which does not exist now. No one now would look for a particular reason for either of these activities. Men make plays because they want to make them, just as they make love; and they worship because they feel the need of worship. . . . (He then amusingly speculates on the origin of pictures, and shows how probably they had a commercial origin.) For some thousands of years, how many is uncertain, advertisement consisted merely of the recommendation of wares by printed words, which almost entirely covered all buildings. But at some period, not yet precisely ascertained, someone, probably a vendor of soap, introduced a crude representation of a piece of soap into one of his printed advertisements. The innovation must have aroused great interest, and largely increased the sale of his soap. Then, some generations later, under the stress of competition, another soap merchant produced a representation, no doubt very imperfect, of human beings washing themselves with his soap; and this was the real beginning of pictorial art and also the explanation of the remarkable prevalence of the nude in the pictures of several succeeding centuries. . . . Now, if a theory of this sort were presented to us about the art of our time we should be surprised, not so much at its errors of detail as at the notion that any theory should be needed at all. We make pictures as the palæolithic men made them—because we like them. And this liking for pictures always has existed, and always will exist. There is a perpetual impulse to produce pictures which is not started by any particular pretext, but exists in the nature of man, and if our memory of the past could suddenly be wiped out of our minds, we should begin to make pictures again just as they have been made in the past. We cannot explain why we have this impulse to make pictures, but we know enough about ourselves to be sure that any explanation which finds some accidental and external cause for it is absurd.

The present generation interests itself a good deal in psychology and metaphysics—a few years ago there would have been, I suspect, no demand in a general daily paper for such articles as the one I have just quoted from.

The teachings of Bergson, of Freud, of Balfour, and others are much in men's minds, or, at all events, much on men's tongues. In the medical world, Freud's teachings are most to the front, and questions bearing on the use and application of "Psychanalysis," the interpretation of dreams, etc., etc.

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However much dissent there may be from particular detail of Freud's philosophy, it is certain that his teaching contains much of great value, but, like all new doctrines or new modifications, time alone is the criterion which will determine what is to remain and what must be discarded.

If we are deterministic beings, that is, beings whose acts are rigidly determined, and if all our motives boil down to motives of hunger or lust, it will be impossible to find any chance act, or any act, that has not a sexual or hunger root. It seems to me almost inconceivable, but I suppose there is a hidden sexual meaning in a run of luck at cards, or in holding a good hand at bridge.

McDougall, whilst referring to the wide connotation of the word "love" with some psychologists, contends that if it is synonymous with sex-love, then "we shall have to attribute even the love of truth, or of the differential calculus, to the sexual instinct" (*Proceedings of the Royal Society of Medicine*, Sect. Psych., March 10th, 1914).

But if you accept these postulates, there does not seem to be much room in the scheme for reason, if we have no choice in the matter, and no variety in the motive.

I think some of the deductions the Freudians make concerning the origin and evolution of normal sexual love are quite untenable, because they are anti-physiological.[1]

In certain mental disorders, such as dementia præcox paranoides, a tendency for homosexuality in its most objectionable form is met with amongst men. I have no personal experience of its occurrence in women. Freud contends it is always found. We must, however, be cautious in concluding that, because we meet with certain symptoms in involutional or devolutional processes, that therefore these symptoms have been present in some stage of normal evolution.

But, apart from observation and introspection, there is, I believe, in Pawlow's recent work on the nervous system physiological evidence which seems to show that much of our conduct is not susceptible of a metaphysical explanation. His experiments tend to support the biological conception of mind. His investigations show that the cerebrum is essentially an organ for forming reflexes—that new reflexes are continually being formed, and probably transmitted.

He has produced experimentally and studied new reflexes,

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which he terms "conditional reflexes" because of their great dependence on a multitude of conditions.

The animals on which he worked were dogs, and the reflex which he chiefly studied was the secretion of saliva. (The quotations following are from Pawlow's paper in the *British Medical Journal* of October 18th, 1913.)

"A fundamental condition for the formation of a conditional reflex is that whatever indifferent stimulus is chosen for the purpose of the reflex which it is desired to build up is given at the same time as the food or acid is introduced into the mouth. After a few such sittings it will be found that this formerly indifferent stimulus alone is now capable of calling forth a secretion of saliva. The conditional reflex has been formed; the formerly indifferent stimulus has now found a path to the requisite parts of the central nervous system. The reflex arc now has a different afferent neuron.

"Conditional reflexes can be formed not merely from indifferent stimuli, but also from a stimulus which is more or less closely connected with some special centre in the brain. A good example of this is seen in the experiments with nocuous stimuli, which in common terminology are called painful stimuli; the usual result of such a stimulus is that the animal does its best to defend itself against the stimulus and to get away from it if possible, or to attack the source of the painful stimulus. But it is possible without any great trouble to modify this response by systematically accompanying each feeding of the animal (that is to say each stimulation of its feeding centres, for there are as good reasons for the existence of the feeding centres as for that of the respiratory centre) by strong electrical stimuli applied to the skin. We now find that even the strongest electrical stimuli applied to the skin give rise merely to the feeding reaction, that is, to secretion of saliva, and to no symptoms of fright or pain at all. The skin of the dog can now be subjected to cutting, pinching, or burning, and the only result we shall obtain will be the manifestation of what, judging from our own experience, we should call the symptoms of the keenest appetite; the animal follows the experimenter about, licks himself, and saliva flows in abundance. This fact has been demonstrated very frequently to large audiences, and to many of my friends privately, and the result can invariably be obtained. Its significance seems to me to be quite clear, for what other explanation is more appropriate than that the nervous impulse resulting from the stimulus, which formerly went to a particular region of the nervous system, is now directed to a different one? In this way we have been able to divert the impulse from one path to another, according to the conditions, and we cannot avoid the conclusion that this represents one of the most important functions of the highest parts of the central nervous system."

The conditional reflex is established by specialised parts of the central nervous system which Pawlow terms analysors, comprising the—

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(*a*) Peripheral end organ of the so-called sense organ.
(*b*) The nerve fibre belonging to it.
(*c*) Its termination in the cells of the cerebral hemispheres.

Such analysors are incessantly at work controlling the reactions of the living organism to its changing external conditions, and "many results we now ascribe to metaphysical processes are really only due to the fine and accurate work of neural analytical mechanisms."

"The experiments on conditional reflexes with skin stimuli are specially striking. Since at the commencement all skin reflexes are generalised, it is easy to render mechanical stimuli of each of several large skin areas a conditional stimulus for the feeding reflex. If now certain parts of the frontal lobes of the brain be extirpated in such an animal, it will be found that the conditional reflex cannot now be obtained at all from a particular and sharply delimited area of skin; in all other parts, however, it can be obtained in the normal manner. It is a fact of some importance, also, that on stimulation of the inactive skin areas one obtains a strong *inhibition* of the effect got by the stimulation of the active areas; and that, further, such stimulation of inactive areas leads very quickly to drowsiness and sleep.

"Conditional reflexes can also be obtained from stimuli from the locomotor apparatus—for example, from the joints, when the latter have been differentiated from the skin stimuli. The proof that such differentiation is attained is furnished by the extirpation of parts of the frontal lobes. When one part is extirpated, the reflex is obtained from the flexion of the joint, but not from the skin; if a different part be removed, we can get the skin reflex, but not the reflex from the joint."

He also investigated certain conditional visual and auditory reflexes in dogs with ablation of the greater part of the posterior part of the cerebrum, from which it appeared that "the limits of accuracy of the analysor's are, to some extent, at all events determined by the cerebral apparatus, and that partial destruction of the central end of the analysors leads quite definitely to some special limitation of the activity of the analysors."

He relates a very interesting experiment on ablation of part of the brain.

"In this dog the anterior half of the brain was taken away, and all the reflexes which had been worked out on this animal disappeared. The animal appears to be completely helpless, and to have lost all its normal relations with the outer world; it cannot take food which is placed before it, it recognises no objects, persons, or animals, and when it attempts to walk it encounters every obstacle and gets into all kinds of difficulties. And yet even in such an animal there can be found a

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link with the normal complicated nervous reactions; we can train it to give that response of the salivary glands which we have called 'the water reflex.' In a normal dog when water is poured into the mouth, there is usually no secretion of saliva, or at most a few drops. But, if first of all an irritating substance—for example, a little acid—be introduced into the mouth, the subsequent administration of water will produce an abundant secretion of saliva.

"Apparently the various stimuli which accompany the forcible introduction of acid into the mouth, and which also accompany the reflex secretion which follows upon this, become converted into conditional stimuli for the reaction to acid; as such they would also come into play in the administration of water. This secretion of saliva in response to water has all the characters of a conditional reflex. In the animal which I have just mentioned it is also possible to work out the conditional reflex to water in all its normal details. This result has also been confirmed in another animal in which the centre for smell had been spared, but which in other respects resembled the previous one. In this animal it was possible to train smell reflexes also. These animals, speaking psychologically, were uneducable idiots when judged by their locomotor systems, but as judged by their salivary apparatus they were rational beings enough."

In a leading article of the *British Medical Journal* of the same date as that in which Pawlow's paper is published, the writer asks this pregnant question: "Will it be found that our every thought and deed is a reflex, and man a mere automaton conditioned by his various reflex mechanisms, and his environment?"

Until the publication of Pawlow's researches, it seemed as if the limits of knowledge by physiological experiments on the cerebrum had been reached, whilst yet the greater part of the convolutions was practically of unknown function, or at most known vaguely as an association area, but his work opens up the prospect of much further penetrating into the exact functions of the cortex. It also flings fresh light on the problem of mind.

Much of the conduct and the delusions of the insane is, I believe, susceptible of explanation on a conditional reflex basis. I will merely refer to one case typical of very many.

I was examining a woman in order to fill in her continuation certificate. She was a case of systematized delusional insanity, but, like many such patients, had an annoying way of not committing herself, although I knew she still retained her delusions. I took her over all the delusions and hallucinations referred to on the admission certificate, but she repudiated

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with scorn all my suggestions, and answered in a rational enough manner. When questioned as to "voices," she admitted that she had been troubled by them, but not for two years, and when I touched on a former statement about men coming into her room at night and threatening her, she admitted that her mind must have been unhinged at the time to entertain such absurd ideas, for, as she explained, it would have been impossible for a strange man to come to her bed here and threaten her life, "because he could not possibly know" which bed she occupied in the dormitory. I asked her other questions, especially if she was subject to any annoyance or trouble, and she answered me "No," and was apparently most candid and open.

It may have been my stupidity, but I spent quite a long time without extracting anything showing her insanity, when the nurse quietly remarked, "What about that mesmerism, Mrs. P.?" and a change came over the woman's face, and she tried to pass the matter over. But now the whole train of delusive ideas poured forth. She was tortured by a "mesmerism" left behind by some man sent from the police. A "mesmerism," she told me, was a "ball of ether," which disperses and gets into her hair, etc., etc. She seemed to be quite aware that she was giving herself away, but quite unable to help herself, and at the end of my examination she said, "I think it is very unfair of you to associate this mesmerism—which is a *fact*, not a delusion—with the state of my mind."

I think such a phenomenon, one I have frequently met with in a similar form, and which doubtless many will corroborate, is susceptible of an explanation on the assumption of a conditional reflex having been formed on a basis of some painful impression, to which a rationalistic explanation has been attached.

The stimulus to the reflex in question was the word, "mesmerism," and when this was applied, and not until then, the response came off.

If my conception of insanity is correct, and by "my" I mean merely the conception that I hold, as the idea itself is as old as the hills, then obviously the proper person to treat insanity is the physician, not the metaphysician, or psychologist, and, further, there is no necessity for him to comprehend the disordered mental states in order to correct them. He will

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endeavour to correct the disordered bodily functions, and if he succeed, the mental will also be adjusted.

In the discussion which followed the reading of this paper it was contended that I did not show the correlation between the bodily signs and the mental phenomena. Nor have I; nor do I pretend to.

I merely contend that just as we say we can account for a pain by certain bodily conditions, and just as we can relieve the pain by correcting them, so we have a similar association between bodily states and insanity, and just as it is not necessary to comprehend the real meaning of pain to relieve it, so it is not essential to study mind to adjust its disorders. I am only preaching the ancient doctrine of a sound mind in a sound body.

(1) One of the most brilliant of Freud's disciples in England tells me that this idea is not and never was held by Freud. Nevertheless, in a brief notice of a work by Freud on "Paranoia" in the *Journal of Abnormal Psychology* (vol. vii, April-May, 1912), my informant writes, expressing, I take it, Freud's views: "In the passage of the normal child from auto-erotism to object-love, there is a stage in which, when the auto-erotic impulses are being grouped into a unity so as to seek an external object, the first object utilised is the individual himself, a condition known as 'Narcissism.' The passage from this to normal hetero-sexuality leads over homo-sexuality. In dementia præcox Jung and Abraham have shown that what happens is a return to primitive auto-erotic activities. In paranoia, the arrest of development takes place at a later stage, so that the return is to a life of phantasy concerning narcissism and homo-sexuality." It seems to me that most people would interpret these sentences in the way I have in my text.

Healing and Training (*Heil und Bildung*).

By Dr. ALEXANDER NEUER, Vienna.(1) (Translated by T. DRAPES.)

"He, verily, did not hit the target of truth who took for his missile the phrase, the Desire for Existence; not the Desire for Existence—such is my teaching—no, the Wish for Power!"

OF all those who kept outside the psychology of the schools, and were psychologists in spite of it, Nietsche is one of the greatest. We may not unfairly deny the significance of the fabrications of his speculative and ethical philosophy—the present does not, as it is, overrate him in reckoning him merely as a poet—but his deep psychological intuitions, in which he towers far above such essentially kindred minds as Montaigne, Amiel, and Vauvenargue, assure for him immortality. I repeat, in

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