

# A DYNAMIC THEORY OF ANXIETY AND HYSTERIA

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## I

It has often been said that America and Britain are separated by a common language and a similar statement might, with equal justification, be made with respect to some of the misunderstandings between psychiatrists and psychologists. Particularly in the personality field, it is quite obvious that many of the terms used are similar, that some at least of the aims of the two groups are identical, and that methods are freely exchanged. Yet, on the whole, it cannot be said that satisfactory integration has been achieved between these two disciplines.

In principle, many practitioners in both camps would agree that as far as psychological illness is concerned the science of psychology is as fundamental to the art and practice of psychiatry as the sciences of physiology, neurology, anatomy, and so forth, are to the art and practice of physical medicine. However, like many statements of principle, such agreement would have to be qualified immediately by the statement that because of the lack of development of a well organized, empirically derived, and theoretically well founded set of principles in the field of psychology, it is at present impossible to base psychiatric procedures on this non-existent science of psychology, or to discuss the aetiology of mental disorders in terms of concepts derived from it.

Such an objection, while fairly widely held among psychiatrists, would not, in fact, give quite a fair picture of psychology as it is today. There have been very rapid strides in the last ten years or so; and in at least one fundamental part of psychology, namely, that concerned with learning theory, there has come about the beginning, at least, of a theory which might be useful in accounting for abnormal mental phenomena. This development, based as it is on the fundamental researches of Pavlov at the beginning of the century, and on the development along theoretical lines of many of his concepts by men such as Hull, Tolman, Guthrie, Spence, and many others, has now led to an impressive area of factual agreement and theoretical synthesis on many important points; there is inevitably still a great deal of argument, but as a recent survey by Osgood (1953) shows, much of this argument deals with points a good deal less fundamental than is often imagined by the protagonists. The question, therefore, arises whether an attempt should not be made at this stage to bring together in one general framework the theory of learning and the theory of personality in the hope that the dynamic laws of the former may be able to account for the derivative principles of the latter.

Two such attempts have recently been made, one by Dollard and Miller in their book *Personality and Psychotherapy* (1950), and the other by Mowrer in his book *Learning Theory and Personality Dynamics* (1950). Both are concerned in the main with the theory of neurosis, and both follow Freud in holding that neurosis in some way has its inception in the clash between the young child and those who attempt to socialize him. As Mowrer puts it, "the child has problems,

needs, drives; he engages in variable, exploratory behavior, and he finds satisfying solutions. However, some of the solutions hit upon by the child will not be acceptable, or will be only temporarily acceptable, to his elders; and if the child persists in his 'immaturities', naughtiness, and 'perversity', he will be *punished*. The tacit intent of this punishment is to establish new drives—notably fears—which are specifically associated with, or conditioned to, the objectionable problem-solving behavior and thus made to compete with the underlying motivation for such behavior. Or, said more precisely, the punishment is designed to produce drives that will elicit responses which will be incompatible with the responses instigated by the original impulses or motives. In one type of situation, punishment may be designed merely to block certain types of solutions and to encourage the learning of other, socially more acceptable ones. This is likely to be true, for example, of habits of eating and elimination. In other areas, however, the punishment may be designed to block solutions to drives more or less completely, as in the case of sexuality and aggression." (1953).

Up to this point there would be considerable agreement between Mowrer and the Dollard–Miller group. However, when it comes to accounting for neurotic behaviour, there is a striking difference between their views. Dollard and Miller argue that the predisposition to neurosis is laid down whenever parents punish children so severely that they abandon overt behaviour in response to certain drives and even go as far as using their problem-solving abilities to avoid being reminded of or thinking about these drives. Normally, the resultant repressions and dissociations do not last beyond puberty; sometimes, however, the disciplinary conditioning has been so severe that it persists into later life, and as a consequence the now intensified drives of sex and aggression, while still unable to gain full expression and gratification, will intrude into consciousness sufficiently to reactivate old fears and thus produce neurotic anxiety and symptom formation. Therapy on this theory should consist of two things—on the one hand, a deconditioning and finally extinction process of the fear-anxiety combination, and on the other hand a building up, through reinforcement, of the habit of facing the drives which had been repressed and dissociated (insight acquisition).

In Freudian terms, this theory might be phrased as follows. Through parental and environmental pressure, the child develops an excessively severe *super-ego*, which in due course gains ascendance over the *ego*, which in turn is thus forced to repudiate or repress the forces of the *id*. This conflict between the too-severe powers of repression and the too-weak and unintegrated drives of the individual cause neurotic symptoms and behaviour problems. Mowrer's theory is the exact opposite of this. In his view, "neurosis arises . . . when the ego, which is initially under the *complete* sway of the *id*, remains essentially *id-dominated* and directs repressive action *against the super-ego* . . . This alternative holds that the neurotic individual is one in whom the primary drives not only have had but still have major control over the problem-solving processes and cause these to be directed toward the blocking, inhibition, or nullification of the secondary, acquired drives of guilt, obligation, and fear. The problem-solving activity which is usually referred to clinically as self-protectiveness or defensiveness thus functions in the interest of the primary drives or *id*, rather than, as Freud posited, in the services of the socially derived forces of the *superego*."

As Mowrer puts it briefly elsewhere, "it is rarely, if ever, a repression of primal impulses which initially predisposes human beings to anxiety; instead

it is the socially inculcated drives of fear, gratitude, and obligation that fall under repression and which, in the act of threatening to break through and again become conscious produce anxiety". "We come to see that neurosis involves, basically, a learning deficit rather than a learning excess . . . We are also enabled to conceive the task of therapy, not as that of attempting to stay or actually reverse the process whereby the human animal is converted into a full-fledged member of his society; rather do we see therapy as the more promising venture of reinstating and, if possible, in some measure completing the education of the laggard learner."

It would appear that the attempts of Mowrer and of Dollard and Miller to integrate learning theory and personality theory have come to grief because of the contradictions contained in their assumptions regarding personality structure. The theory espoused by one group of workers derives neurosis from the dominance of a tyrannical and over-strong super-ego, aided by the ego, over the forces of the id; the theory espoused by the other group of workers, conversely, derives neurosis from the dominance of an over-strong id, aided by the ego, over an underdeveloped and weak super-ego. In algebraic notation, Mowrer holds that  $Id + Ego > S.E.$ , while Dollard and Miller hold that  $S.E. + Ego > Id$ , where  $>$  means "larger, more powerful than", or even "overpoweringly strong". This impasse has several interesting features.

In the first place, the same set of concepts taken from learning theory can apparently account with equal ease for quite antithetical theories of personality. It does not seem possible in terms of learning theory to discriminate the correct from the incorrect theory on the personality side. Learning theory does not appear to be specific enough to make divergent predictions according to divergent theories.

In the second place, the type of personality theory we are dealing with is on quite a different level from the type of learning theory built up by psychologists. The evidence it is concerned with is clinical evidence, not experimental evidence. Now clinical investigations are important in suggesting hypotheses; they cannot in themselves provide acceptable evidence for the correctness of the theories thus suggested. This widely recognized fact is particularly well illustrated by the present position as outlined above. Both protagonists claim to base their theories and their therapies on clinical observation and experience; both claim that therapeutic success in some way supports their claims. Yet these therapies are engaged in doing exactly opposite things; where Dollard and Miller advocate weakening the super-ego, Mowrer suggests strengthening it! If Mowrer were right, then patients treated according to the precepts of Dollard and Miller should actually get worse; conversely, if Dollard and Miller were right, then patients treated by Mowrer should actually get worse. In actual fact, a review of the available evidence on the effects of psychotherapy suggests that no support can be found in favour of the hypothesis that any form of psychotherapy has an ameliorating effect on neurotic disorders. Such a finding suggests that perhaps both sides claim therapeutic successes on the basis of a *post hoc ergo propter hoc* kind of argument; they have not ruled out the obvious possibility that their patients might have got better without any psychotherapy at all. Figures to show the remarkable improvement of neurotic patients not undergoing psychotherapy have been published elsewhere, and lend colour to this possibility. Indeed, a comparison of the effects of psychotherapeutic treatment led to the conclusion that "the figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder" (Eysenck, 1952).

## II

The great weakness of the two attempts at the integration of learning theory and personality theory which we have examined in the previous section has been the failure on the part of the authors to realize that, before a bridge can be built between the two, the alleged facts on the personality side must be established as firmly and with the same degree of experimental care and sophistication as are the facts on the side of learning theory. As long as writers are content to base their views on the personality side on nothing better than clinical insight, psychoanalytic beliefs, and evidence collected from the patient on the couch, the whole process of integration will remain purely at the semantic level and will inevitably lack any possibility of proof or disproof. The two works quoted bring out this impasse in a particularly marked manner. When Miller and Mowrer deal with issues in learning theory, they appeal to facts and perform experiments designed to prove or disprove their particular points; when it comes to personality theory, however, they immediately have recourse to pre-scientific modes of argumentation, to persuasion, and to reference to "therapies".

For the present writer, it has always seemed necessary in the investigation of personality to proceed in two stages. In the first place, what seemed to be required was an objectively established dimensional framework which could accommodate the main facts and features of behaviour relevant to mental abnormality. Experiments along these lines have been summarized in *Dimensions of Personality* (Eysenck, 1947) and in *The Scientific Study of Personality* (Eysenck, 1952), and the results have been shown to be congruent with work carried on by many other workers in the field in *The Structure of Human Personality* (Eysenck, 1953). The main findings of these various researches which are relevant to our present theory may be summarized in the following six points:

1. Human conduct is not specific, but presents a certain amount of *generality*; in other words, conduct in one situation is predictable from conduct in other situations.

2. Different degrees of generality can be discerned, giving rise to different levels of personality organization or structure. It follows that our view of personality structure must be *hierarchical*.

3. Degrees of generality can be operationally defined in terms of correlations. The lowest level of generality is defined by test-retest correlations; the next level (trait level) by intercorrelations of tests purporting to be measures of the same trait, or the same primary ability; the highest level by correlations between different traits defining second-order concepts like "g" in the cognitive field and "neuroticism" in the affective field, or type concepts like extraversion-introversion.

4. Mental abnormality (mental deficiency, neurosis, psychosis) is not qualitatively different from normality, in the sense that a person with a broken arm, or a patient suffering from haemophilia, is different from someone not ill; different types of mental abnormality constitute the extreme ends of continuous variables which are probably orthogonal to each other.

5. It follows from the above that psychiatric diagnostic procedures are at fault in diagnosing categories, such as "hysteria" or "schizophrenia"; what is required is the determination of the main dimensions involved, and a quantitative estimate of the patient's position on each of these dimensions. (Cf. example below.)

6. The main dimensions involved in the analysis of personality for which sufficient experimental data are available to make possible a theoretical formulation are neuroticism and extraversion-introversion.

The relationship between these dimensions and current nosological psychiatric categories is well pointed out in an unpublished research by Hildebrand (1953). Proceeding on the hypothesis that the test differences between hospitalized "neurotics" and non-hospitalized "normals" (i.e. people without psychiatric involvement) would provide us with an outside criterion of "neuroticism", and that test differences between hysterics (Jung's prototype group for the concept of "extraversion") and dysthymics (patients suffering from anxiety, Jung's prototype group for the concept of "introversion") would provide us with an outside criterion of "extraversion-introversion", a battery of objective tests of persistence, suggestibility, and other traits was administered to groups of hysterics, psychopaths, depressives, obsessionals, anxiety states, mixed neurotics, and normals. Retaining the hysterics, anxiety states, and normals as criterion groups, inter-correlations were calculated between tests for the subjects in the remaining groups, and a Lawley-type factor analysis performed. Three clear-cut simple structure factors emerged, corresponding to intelligence, neuroticism, and extraversion. Intelligence tests had high loadings on the intelligence factor; the tests differentiating between the normal and neurotic groups had high loadings on the neuroticism factor; the tests differentiating between the hysterics and anxiety states had high loadings on the extraversion-introversion factor.

Factor scores on the introversion-extraversion and the neuroticism factors were then calculated for the persons in the various groups. Figure 1 gives a

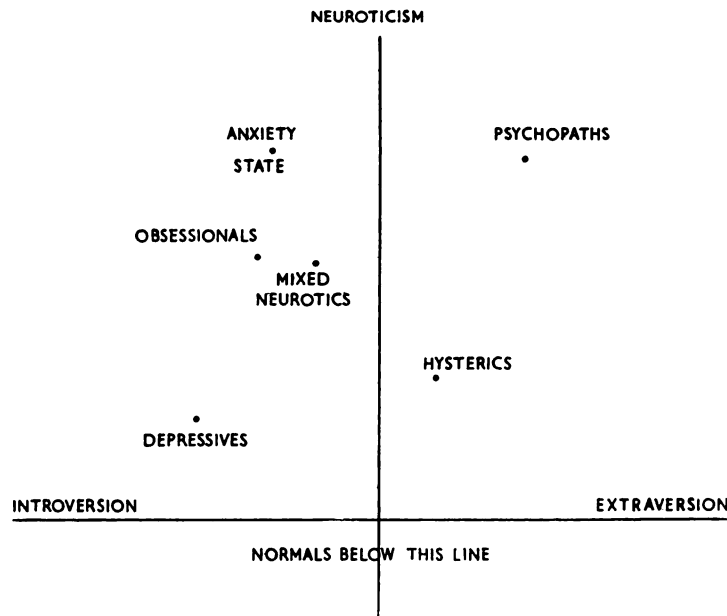


FIG. 1

diagrammatic indication of the results obtained. The line separating the neurotic groups from the normal subjects was drawn so as to put 10 per cent. of the normal group on the neurotic side, this being the percentage found by R. Fraser

(1947) to show debilitating neurotic tendencies in a normal working-class population. It will be seen that psychopaths are slightly more extraverted than hysterics, and that obsessionals and depressives are about as introverted as anxiety states. Differences between extraverted groups and introverted groups are fully significant. Mixed neurotics are intermediate between the other groups; normals are very significantly lower on "neuroticism" than any of the neurotic groups. These results allow us to use the hysteric/psychopath group on the one hand and the dysthymic group on the other as criteria for any predictions made in terms of a theory of extraversion-introversion.

So much for the first stage of investigation, which had been envisaged by us right from the beginning of this whole programme of research. However, it was also appreciated that this type of static, taxonomic, nosological, dimensional, or classificatory approach would require to be supplemented in due course by a more dynamic or causal type of investigation. If, as the author has argued, statistical factors can be regarded under certain circumstances as causal agents, then it is incumbent upon the investigator not to rest content with the extraction of statistical factors, but to go on to find the psychological or physiological causes indicated by the factor analysis (Eysenck, 1953). Consequently, in recent years our concern has been to discover the causes underlying individual differences in extraversion-introversion and in neuroticism, or, to put it in what philosophically may be a more acceptable terminology, to discover a system of relations in which the factors discovered would be the dependent variables, and certain unknown physiological or psychological properties of the nervous system would be the independent variables. In this paper, we shall be concerned almost exclusively with the dimension of extraversion-introversion; it is hoped that in due course a similar account may be possible of the causal factors underlying neuroticism. For the moment, it must be sufficient to say that we regard neuroticism as a form of drive related to the over-excitability of the autonomic nervous system, particularly the sympathetic branch. The evidence regarding the close relationship between these concepts has been discussed at some length in *The Structure of Human Personality* (Eysenck, 1953) and there is no cause to review it again here in any detail.

When we come to the construction of a theory of extraversion-introversion which, as shown in Figure 1, would also be adequate to explain the differences between the hysteric, psychopathic, criminal group on the one hand, and the anxious, depressed, obsessional group on the other, we must have in mind two objectives. In the first place, such a theory must account for the known facts. In other words, we should be able to derive or deduce these facts from our explanatory principle. In the second place, our theory should be capable of predicting facts hitherto unknown. In other words, it should enable us to verify the theory by carrying out experiments in which certain events are predicted to occur by deduction from the theory which, on other grounds, or in terms of alternative theories, would not be predictable. Such a theory will be presented in section III of this paper.

### III

The theory to be presented here was adumbrated by Pavlov in his well known discussion of "experimental neurosis" (1927). Having discussed the methods by means of which so-called experimental neurosis can be produced in dogs, he goes on as follows: "It has been seen that the above-mentioned method may lead to different forms of disturbance, depending on the type of

nervous system of the animal. In dogs with the more resistant nervous system it leads to a predominance of excitation; in dogs with the less resistant nervous system, to a predominance of inhibition. So far as can be judged on the basis of casual observation I believe that these two variations in the pathological disturbance of the cortical activity in animals are comparable to the two forms of neurosis in man—in the pre-Freudian terminology *neurasthenia* and *hysteria*—the first with exaggeration of the excitatory and weakness of the inhibitory process, the second with a predominance of the inhibitory and weakness of the excitatory process.” This acute observation on the part of Pavlov was not developed along experimental lines by him and has been largely neglected by psychiatrists and psychologists alike. We shall formalize it in terms of a general psychological law proposed by Hull, namely his law of *reactive inhibition*.<sup>\*</sup> This law reads as follows: “Whenever any reaction is evoked in an organism, there is left a condition or state which acts as a primary negative motivation in that it has an innate capacity to produce a cessation of the activities which produce the state” (1943). Hull expands this statement as follows: “All responses leave behind in the physical structure involved in the evocation, a state or substance which acts directly to inhibit the evocation of the activity in question. The hypothetical inhibitory condition or substance is observable only through its effect upon positive reaction potentials. This negative action is called *reactive inhibition*. An increment of reactive inhibition ( $\Delta I_R$ ) is assumed to be generated by every repetition of the response (R), whether reinforced or not, and these increments are assumed to accumulate except as they spontaneously disintegrate with the passage of time.”

It is easy to see what Pavlov and Hull had in mind in advocating this concept of inhibition. Whenever a stimulus-response connection is made in the central nervous system there are created both excitatory and inhibitory potentials. The algebraic sum of these potentials determines the amount of learning that takes place, and through it the particular reaction the organism makes whenever the stimulus in question is presented again.

Excitatory and inhibitory potentials have ascertainable properties which are in many ways different from each other. To take but one example, inhibitory potentials dissipate more quickly in time than do excitatory potentials. It has been found quite impossible to account for the observed effects of conditioning and learning in terms only of excitatory potential; the effects of extinction and recovery, of disinhibition, of inhibition with reinforcement, and of conditioned inhibition in the Pavlovian system, and the effects of reminiscence, massed versus spaced learning, serial learning position effects and so forth, in the Hullian learning system equally point to the absolute necessity of some such concept as expressed in Hull's law of inhibition quoted above. There is no space here to quote the evidence in any detail, and the interested reader must be referred to the excellent summary given by Osgood (1953).

Having taken Hull's law of inhibition as our point of departure, we must next propose what may be called a *postulate of individual differences: Human beings differ with respect to the speed with which reactive inhibition is produced,*

\* While this law is usually associated with Hull and is therefore quoted in the form which he has given it, it should be noted that a very similar law was proposed by Spearman in 1927. This, his so-called law of *fatigue*, reads as follows: “The occurrence of any cognitive event produces a tendency opposed to its occurrence afterwards.” Spearman does not remark on the similarity of his law to Pavlov's conclusions and Hull does not quote Spearman in connection with his own formulation. It would be interesting to know the views of these authors on the relevance of the work of their colleagues, but unfortunately they seem to have been too pre-occupied with building up their own systems to pay much attention to evidence collected outside.

*the strength of the reactive inhibition produced, and the speed with which reactive inhibition is dissipated. These differences themselves are properties of the physical structures involved in the evocation of responses.*" This postulate is implicit in Pavlov's account but has been curiously neglected by Hull, who hardly ever deals with individual differences of this kind.

To make our theory complete, we must add one further postulate which may be stated as follows: *Individuals in whom reactive inhibition is generated quickly, in whom strong reactive inhibitions are generated, and in whom reactive inhibition is dissipated slowly are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterico-psychopathic disorders in cases of neurotic breakdown; conversely, individuals in whom reactive inhibition has developed slowly, in whom weak reactive inhibitions are generated, and in whom reactive inhibition is dissipated quickly, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in cases of neurotic breakdown.*" These two postulates, added to the law of reactive inhibition, are, it is suggested, sufficient to account for the observed facts on which the dimension of extraversion-introversion is based, and enable us to make predictions which are experimentally verifiable. We shall deal with the second of these points first, in section IV, and then, in section V, undertake to show how known facts can be accounted for in these terms.

#### IV

The first prediction, made on the basis of our theory, relates to the establishment of conditioned responses. It follows directly from the Pavlov-Hull theory that with individuals in whom the excitation-inhibition balance is tilted in the direction of strong excitation and weak inhibition, conditioned responses should be formed quickly and easily and should be difficult to extinguish. Conversely, in individuals in whom the excitation-inhibition balance is tilted in the direction of weak excitation and strong inhibition, conditioned responses should be formed slowly and with difficulty and should be easy to extinguish. No work along these lines appears to have been done in Pavlov's laboratory, but some corroborative evidence has been produced by several workers. Unfortunately, most of this work has consisted merely in relating anxiety as a symptom to conditionability. This is in line with our hypothesis in view of the fact that symptoms of anxiety, as shown in Figure 1, are located on the dysthymic-introverted side of our dimension. However, the proof is far from complete as symptoms of anxiety also show projections on to the dimension of neuroticism, so that it might be possible to account for the conjunction between anxiety and conditionability in terms of neuroticism rather than in terms of extraversion-introversion. Such an attempt has indeed been made by Taylor (1951) and Spence and Taylor (1951) who, working on University students, showed that eye-wink conditioning occurred more quickly and more easily in students who, according to questionnaire responses, showed evidence of anxiety as compared with students who failed to show such evidence.

Making use of Hull's formula  ${}_sE_R = {}_sH_R \times D$ , where  ${}_sE_R$  represents excitatory potential,  ${}_sH_R$  habit strength, and  $D$  drive strength, they argue that anxiety is related to drive level, and that consequently higher status of anxiety should lead to quicker conditioning ( ${}_sE_R$ ) because of increases in drive strength ( $D$ ). Their experiments do not provide crucial evidence with respect to the two theories involved (their own and the one outlined in section III) as the same prediction would be made in terms of both hypotheses. The same ambiguity



is found in the work of Bitterman and Holtzman (1952), of Welsh and Kubis (1947) and of others who have carried out experimentation in this field. In every case the concern of the experimenters has been with the relationship between dysthymic personality and conditionability, and in every case it is possible to account for the greater conditionability of the dysthymic in terms of his *introversion* or in terms of his *neuroticism*.

The crucial experiment, therefore, requires the examination of three groups, namely, a group of dysthymics, a group of normals, and a group of hysterics. If the easy conditionability of the dysthymics is related to their neuroticism, then hysterics also should be easily conditionable and the normals should be less conditionable than both neurotic groups. If, on the other hand, the easy conditionability of the dysthymics is related to introversion then it would follow that the hysterics should be the least conditionable group, with the normals (who, being selected on a chance basis, should be on the average neither introverted nor extraverted) in between the two neurotic groups.

This crucial experiment was carried out in this laboratory by Dr. C. Franks (1954). Using the eye-wink to a puff of air as the response, and a tone as the conditioned stimulus, he plotted the acquisition and the extinction of the conditioned response to the tone in eighteen conditioning and ten extinction trials for groups of dysthymics, normals, and hysterics, respectively. The main results of his study are shown in Figure 2 where it will be seen that, as predicted in terms of our theory, the dysthymics condition more quickly, condition more strongly, and extinguish less quickly than do the normals, while hysterics condition less quickly, condition less strongly, and extinguish more easily than do the normals.

These results suggest very strongly that conditionability is related to extraversion-introversion and not in any way to neuroticism. We thus find that our first prediction is borne out by the facts (Dr. Franks also used P.G.R.

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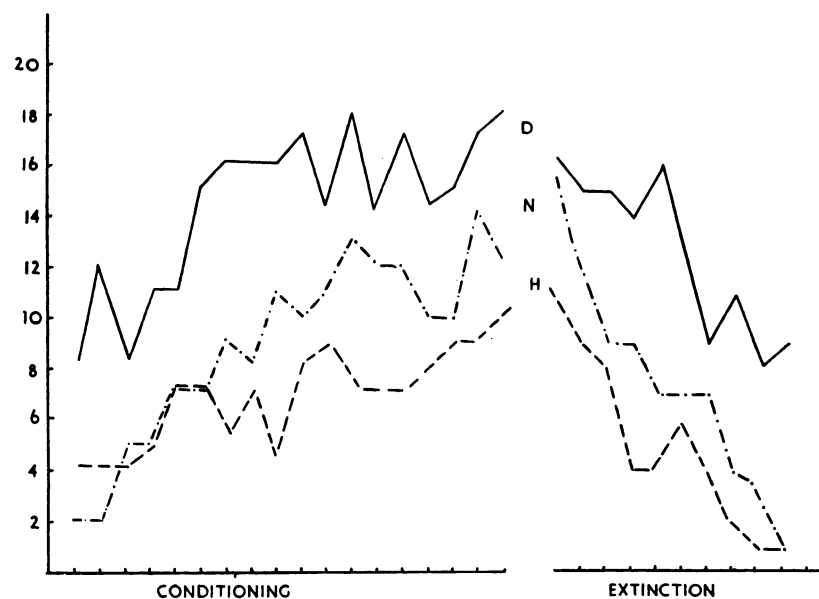


FIG. 2

conditioning and found similar results there. There is no space here, however, for going further into his well designed and carefully controlled experiment.)\*

It would be erroneous to think that the law of inhibition, which was quoted earlier in this paper, is restricted entirely to the phenomena of learning and conditioning. Pavlov is often misrepresented as having considered his conditioning procedure as a paradigm for learning. This interpretation and the developments stemming from it is almost entirely due to American learning theorists. In actual fact, Pavlov was far more interested in using the conditioning procedure to establish the laws of cortical functioning; these laws he considered would be of universal applicability and would account, he thought, just as much for perceptual phenomena as for those of learning.

Consequently, it appeared desirable to attempt a proof of our general hypothesis in a field not directly connected with learning and conditioning, and accordingly a search was made for phenomena in the perceptual field which would lend themselves to a crucial experiment. The particular phenomenon decided on was the so-called figural after-effect, discovered by Gibson (1933) and by Köhler and Wallach (1944). In essence, the phenomena studied by these writers showed beyond doubt that constant stimulation of parts of certain sensory surfaces, such as the retina, sets up states of inhibition in corresponding areas in the cortex which have measurable effects on the perception of stimuli later presented in the same region. As an example, consider Figure 3. In the first part of the experiment, the subject is instructed to fixate a small cross to the left of which is a circle, as shown in 3a. After five minutes of constant stimulation of part of the retina by this circle the stimulus is withdrawn and two squares, separated by a fixation point, as in 3b, are substituted.

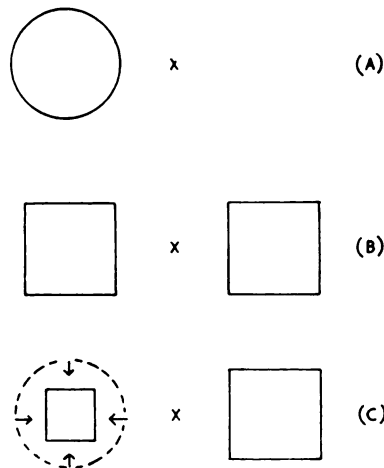


FIG. 3

According to Köhler's theory the inhibitory after-effects produced by the inspection of the circle originally presented (Köhler calls these "satiation effects") should lead to a displacement of the contours of the square now seen in the same part of the retina as the circle was originally, away from the contour

\* Group averages do not tell us much about the adequacy of a test for differentiating different groups. Eye-wink conditioning alone was found to differentiate hysterics from dysthymics with a classification error of only 12 per cent.; a combination of eye-wink and P.G.R. conditioning, using a pre-arranged weighting system, i.e. not capitalizing on chance errors, separated out the two groups with no misclassification whatsoever.

of that circle. This effect is illustrated in Figure 3c and it will be seen that the observable effect would be a displacement of the sides of the left square, and consequently a shrinkage in size of this square as compared with that on the right side. Thus, inhibition or satiation effects produced by inspection of the circle would show themselves in displacement or shrinkage effects of a figure presented in the same part of the visual field. These theoretical effects are actually observable, are of considerable size, and show marked individual differences.

Now effects of this kind appear to be very much in line with the law of inhibition, as quoted above, and are indeed predictable from it. Köhler's notion of "satiation" appears to correspond exactly to the Pavlov-Hull notion of "inhibition". It is interesting to note that in this molar law of satiation-inhibition the arch-Atomist Pavlov and the arch-Gestaltist Köhler appear to have arrived at identical concepts or laws. There is, indeed, a further similarity between these two workers. Both attempt to account for their molar principles in terms of molecular (physiological) principles which, because of their unorthodox nature, have not found ready acceptance among physiologists and neurologists. It should perhaps be pointed out, therefore, that in accepting the reality of the phenomena of inhibition and of satiation, which can hardly be gainsaid, we do not wish to imply acceptance of the physiological theories associated with Pavlov and Köhler respectively. Konorski (1948) has discussed the relationship between Pavlov's physiological and neurological theories and those of Sherrington and other orthodox physiologists in some detail, and Osgood (1953) has attempted to do a similar service for Köhler's figural after-effects. While these theoretical contributions are interesting and important, they are not strictly relevant to a psychological theory of the kind developed here, which remains throughout at the molar level and is not concerned with physiological details.

If we agree, then, to accept the hypothesis that satiation effects of the kind described are the product of reactive inhibition as defined, then it follows immediately from our postulates that figural after-effects should appear earlier among extraverts, they should appear more strongly in extraverts, and they should disappear more slowly in extraverts as compared with introverts. If we take hysterics as a prototype of our extraverted group and dysthymics as a prototype of our introverted group, then clearly these predictions admit of an easy disproof. An experiment along these lines has been reported by the writer elsewhere (1954).

The test used for this purpose was the kinaesthetic figural after-effect measure, originally described by Köhler and Dinnerstein (1947), and adapted by Klein and Krech (1952), whose procedure was followed. Figure 4 shows the test in detail. The apparatus consists of a comparison scale, marked A, a stand, marked B, a test object, marked C, and a stimulus object, marked D. These are all made of wood and movable riders are affixed to all three objects in such a way that the position of thumb and forefinger is kept constant as the subject moves these two fingers up and down along the sides of the objects. The apparatus is so arranged as to present the comparison scale to the left of the seated subject, and either the test or stimulus object to his right. The task of the subject is to adjust the position of the rider on the comparison scale in such a way that the width of the wood between his two fingers is subjectively equal to the width of the wood between the fingers of his right hand, i.e. to arrive at a point of subjective equality between the width of the test object and the width of the comparison scale. Having determined this subjective equality, the experimenter

then instructs the subject to put the fingers of his right hand into the rider of the stimulus object, which is somewhat broader than the test object ( $2\frac{1}{2}$  inches as compared with  $1\frac{1}{2}$  inches), and to stroke the sides of the stimulus object for varying periods of time (those employed in this experiment were periods of 30, 60, 90, and 120 seconds). After this process of constant stimulation, which corresponds to the inspection of the circle in the visual experiment described before, the subject goes back to the test object and again establishes a point of subjective equality. The prediction is that there should be a shrinkage of the test object corresponding in principle to the shrinkage of the square shown in Figure 3 as a result of the constant stimulation produced by the stimulus object. Thus, our prediction would be that hysterics should show a greater degree of shrinkage than would dysthymics.

- A. Comparison Scale
- B. Fixed Stand for Standard Test Object and Stimulus Object
- C. Standard Test Object ( $1\frac{1}{2}$ " wide)
- D. Stimulus Object ( $2\frac{1}{2}$ " wide)

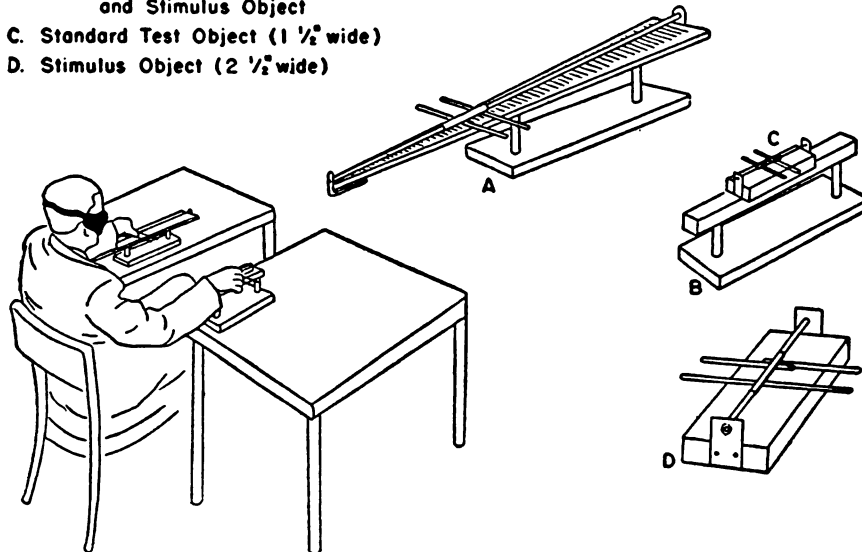


FIG. 4.—Diagram of kinesthetic figural after-effect test.

Results of such an experiment are given in Figure 5. The ordinate depicts the amount of decrement, the abscissa shows the various rubbing periods. Also shown on the abscissa are determinations made after rest periods of five and a further ten minutes respectively. It will be seen that our prediction is borne out at a statistically significant level in every detail.

Hysterics develop satiation effects more quickly, they develop them more strongly, and the effects persist longer. Thus, it will be noticed that even after a rest period of 15 minutes hysterics still show satiation effects which are stronger than those shown by dysthymics immediately after a stimulation period of 120 seconds. These results are very encouraging and suggest that our hypothesis may not be altogether erroneous in its main assertions. The results are particularly important because they are predicated on the correctness of two independent hypotheses, (1) that extraverts show more cortical inhibition than introverts, (2) that the laws of satiation in the perceptual field are formally identical with the laws of inhibition in the learning field.

The experiments described in this section were carried out especially in order to provide evidence regarding the power of our theory to make possible testable deductions. Large numbers of other deductions could, of course, be made in terms of the phenomena of learning and perception respectively which are associated in the literature with the concepts of inhibition and satiation. Thus, it would seem to follow from our hypothesis that reminiscence effects

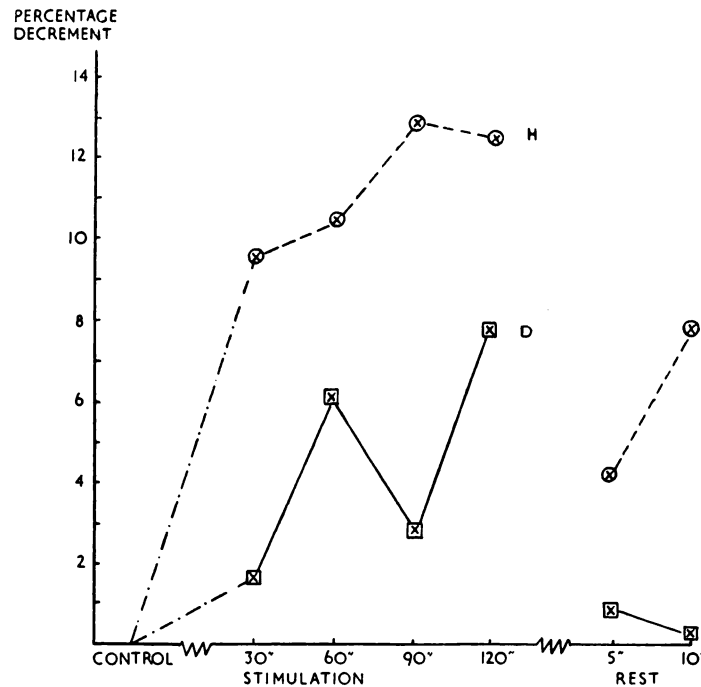


FIG. 5

should be stronger in hysterics, that serial learning position effects should be more marked, and that superiority of spaced over mass learning should be far more clearly marked in hysterics than in dysthymics. On the perceptual side, the "extinction" effects noted by Bender (1951) should be more strongly marked in hysterics, thresholds for apparent movement phenomena should be shifted further for hysterics than for dysthymics by constant stimulation, and the increase in the length of after-images as a function of the length of exposure of the original stimulus should be less marked in hysterics than in dysthymics. There is little point in adding further predictions; anyone familiar with the literature will be able to make his own deductions from the principles enunciated in the previous section.

## V

We must now make an attempt to account for the known facts relating to extraversion and introversion in terms of our general hypothesis. In doing so we shall make use of the concept of *socialization*. By this term we shall understand the process by means of which the values and standards of the society in which a person lives are inculcated into that person. In our society this process of socialization is concerned particularly with the development of

certain accepted rules regarding the suppression of overt aggressive and sexual impulses.

The process of socialization, i.e. the acquisition by the individual of a certain set of standards of conduct and behaviour prescribed by society (in different terminology, the acquisition of a conscience or of a super-ego), is considered to be mediated by a process of conditioning. In evaluating such a statement we must bear in mind a distinction drawn by many theorists between *learning* and *conditioning*. This distinction was originally proposed by Konorski (1948), in Pavlov's laboratory, who labelled the phenomena covered by these two terms conditioning of the first type and conditioning of the second type. Skinner's differentiation between S-type conditioning and R-type conditioning emphasizes a similar differentiation (1938). The distinction made by Hilgard and Marquis (1940) between classical and instrumental conditioning also refers to the same facts. Probably the clearest account, however, is that given by Mowrer (1950) in his advocacy of "the dual nature of learning". In what follows we shall make use of his account without necessarily endorsing the totality of his system.

In this account, Mowrer starts by pointing out that in all mammals the individual organism is divided into two great response systems, that of the *skeletal muscles* and that of the *smooth muscles and glands*. The responses mediated by the latter he calls *physiological*; those mediated by the former he calls *behavioural*. This fundamental dichotomy he links with the fact that mammals have not one nervous system but two. "Responses of the skeletal muscles are mediated by the *central nervous system*, whereas responses of the visceral and vascular parts of the organism are mediated by the *autonomic nervous system*. In terms of structure and organization, as well as mode of functioning, these two nervous systems are radically different; and it is by no means unreasonable to suppose that the responses which they mediate are subject to very different learning processes. As a further parallel to this basic dichotomy we may note the familiar differentiation between *voluntary* and *involuntary* responses. Without exception, the visceral and vascular responses are beyond direct voluntary control, whereas all of the skeletal responses . . . are or may be brought under voluntary control."

Normally, visceral and vascular responses subserve homeostatic functions. They may, however, be made to occur not only in response to actual physiological needs but also in response to conditioned stimuli of various kinds. When autonomic responses occur on this latter basis, as anticipatory states, they produce rather than eliminate physiological disequilibrium and are consciously experienced as emotion. "As such, they play enormously important motivational roles, roles so important to the survival of the organism that it is easily understood why the learning of these responses should be automatic, involuntary, distinct from the type of learning whereby ordinary habits are acquired. Biologically, it is clearly necessary that living organisms be equipped with a nervous system which will cause to be fixated those skeletal responses which reduce drives and give pleasure. But it is equally evident that living organisms must also be equipped with another nervous system which will cause emotional responses to be learned, not because they solve problems or give pleasure in any immediate sense, but because without such responses the organism would have slight chance of survival. There are grounds for believing that all emotions (including fear, anger, and the appetites) are basically painful (i.e. all have drive quality); and it is hard to see how they could be acquired by the same mechanism which fixes those responses (of the skeletal musculature)

which are problem solving, drive reducing, pleasure giving. The latter are learned when a problem is resolved, ended; whereas it is often necessary that emotional responses become conditioned to signals which are associated with the *onset*, not the termination, of a problem."

This distinction between conditioning mediated by the autonomic nervous system and learning mediated by the central nervous system is likened by Mowrer to the difference between Freud's *pleasure principle*, which would correspond to learning, and the *reality principle*, which would correspond to conditioning. "In other words, living organisms acquire conditioned responses, or emotions, not because it is pleasant to do so, but because it is *realistic*. It is certainly not pleasant to be afraid, for example, but it is often very helpful, from the standpoint of personal survival."

Lastly, Mowrer goes on to link the distinction between conditioning and learning with that between *training* and *teaching*. He points out that the acquisition of new responses by an individual comprises two different kinds of response. In the first place, there are those responses which are intrinsically rewarding and which help the individual to solve problems which without teaching he would have had difficulties in solving for himself. There are also responses, however, which the individual has to learn, not because they are useful to him in any direct sort of way but because these responses are required by society. "By and large, the solutions to individual problems involve the central nervous system and the skeletal musculature, whereas the solutions to social problems involve the autonomic nervous system and the organs which mediate emotional responses. Intrinsically, it is hardly helpful to the individual to be told, 'Thou shalt not do thus and so', but it may be socially very necessary, and, in the long run but not in any immediately discernible psychological sense, also advantageous to the individual."

This acquisition of socially useful responses is equivalent to our concept of *socialization* and is called *training* by Mowrer. "Teaching may be defined as the process whereby one individual helps another learn to solve a problem more quickly or effectively than would be likely on the basis of that individual's own unaided, trial-and-error efforts. Here we are dealing with 'items of culture' which are individually helpful. Training, by contrast, may be thought of as involving learning whose primary objective is social rather than individual. In this connection one naturally thinks of 'items of culture' which are associated with such words as 'morality', 'character', 'social responsibility', etc."

We cannot go further into Mowrer's account of the process of socialization as mediated by a process of conditioning; the reader who wishes to assess the evidence on this point for himself must be referred to the original publication. Instead, we must now trace the consequences which follow from this theory with respect to the personality patterns of extraverts and introverts respectively. It will be clear that if the process of socialization is based on a conditioning procedure, then, other things being equal, the extreme introvert, subjected to a standard process of cultural indoctrination, should become over-socialized as compared with the average sort of person, while the extreme extravert, subjected to the same process, would become under-socialized as compared with the average person. We shall consider in a moment what happens when other things are not equal; for the time being, let us consider whether in fact the hypothesized consequences can be observed.

The facts seem to fit the pattern remarkably well. It is one of the characteristics of the psychopathic, hysterical group that they transgress the morals of society in many ways (lying, stealing, sexual delinquency, avoidance of

responsibility, etc.), while dysthymics, on the other hand, are over-scrupulous, over-concerned with ethical and similar problems, and as over-inhibited in the behavioural field as the hysterical, psychopathic group is under-inhibited.\* This clinical pattern is apparent even in children and is shown very strongly marked in Figure 13 in *The Structure of Human Personality* (Eysenck, 1953), where the extraverted group of symptoms, as derived from Ackerson's study of *Children's Behavior Problems* (1942) includes such notations as fighting, swearing, lying, stealing, truant, violent, rude, destructive, disobedient, and egocentric, while the introverted group includes such notations as sensitive, absent-minded, day-dreams, depressed, seclusive, inefficient, and so forth.

On the experimental level we again find that many of the facts can be accounted for in these terms. The excessively high level of aspiration of the dysthymics as compared with the excessively low level of aspiration of the hysterics appears to be a direct consequence of over- and under-socialization respectively (Eysenck and Himmelweit, 1946; Miller, 1951). The preference of hysterics for sex humour as compared with the preference of dysthymics for cognitive humour also follows directly from the hypothesis (Eysenck, 1947). The disregard for rules shown by criminals, psychopaths, and hysterics on the Porteus Mazes Test when infringements of rules against cutting corners, lifting pencils, etc. are scored independently of intelligence, again show the lack of socialization of the extraverted group (Hildebrand, 1953; Porteus, 1945; Foulds, 1951). On tests measuring propensity towards cheating, Biesheuvel (1953) has found psychopaths obtaining much higher scores than other groups. The failure of extraverts to develop a vocabulary equivalent to their abstract intelligence; the tendency to lay stress on speed rather than accuracy in their work; the lack of the socially valued quality of persistence (Eysenck, 1947)—these and many other facts established in experimental work are all directly deducible from the hypothesis of under-socialization of extraverts and over-socialization of introverts, respectively.

Of particular interest in this connection is a study of social attitudes recently published by the writer (1954). From our hypothesis we would deduce that the attitudes of extraverts and introverts would differ sharply with respect to a variety of aggressive and sexual practices. When this hypothesis was put to the test it was borne out at a very high level of significance indeed. While the picture was somewhat complicated by the inescapable conservative-radical dichotomy, it nevertheless provided strong support for our hypothesis. Extraverts who, politically, were situated to the left of centre particularly frequently endorsed attitudes favourable towards a less restricted and less inhibited sex life. Thus, they were in favour of relaxing divorce laws, abolishing abortion laws, legalizing companionate marriage, abolishing licensing laws, and so forth. Extraverts to the right of centre, politically, were characterized by attitudes having a strongly aggressive cast. They tended to favour war, flogging of sex criminals, the death penalty, and the harsh treatment of transgressors generally. Compared with these extraverted beliefs introverts strongly supported the main social restraints against aggressive and sexual tendencies, i.e. religious and

\* It is unfortunate that the term "inhibition" is commonly used in so many different senses. As far as the usual physiological meaning of the term is concerned, as discussed, for instance, by Eccles (1953), little confusion is likely to arise. Experience has shown, however, that some confusion is caused by the fact that cortical inhibition as conceived here is considered to be a causal agent of *lack* of behavioural inhibition, whereas lack of cortical inhibition, on the other hand, is accompanied by inhibited behaviour. This semantic difficulty needs only to be pointed out in order to become innocuous.



ethical beliefs and practices. These results fit in too well with our hypothesis to be accountable for in terms of chance.

So far, we have assumed that the environmental influences which society brings to bear on the growing child in order to make him socially acceptable are roughly equal for all the people concerned. This is almost certainly not true and what we are dealing with in the grown-up person is obviously a balance of the socializing influences society has exerted on him and the innate receptivity (conditionability) of the individual towards these influences. Proof of this proposition has been given again in connection with social attitudes, and the main outcome may briefly be quoted here.

We have noted above a set of beliefs held by extraverts of radical or conservative views respectively. Taking all these attitudes together and contrasting them with those held by right and left wing introverts, we arrived at a distinction between tough-minded (extraverted) and tender-minded (introverted) beliefs. By carefully balancing right and left wing attitudes, a measuring scale for tough-mindedness was finally produced which was quite independent of radicalism-conservatism, and which correlated with extraversion. Using this scale and basing ourselves on the theory developed above, the prediction was made that middle-class groups would be less tough-minded than working-class groups. The grounds for making this prediction were as follows. There is no reason to expect that any differences exist between classes with respect to conditionability. It is known, however, from the work of Allison Davis (1944) and his colleagues in the United States, and of Himmelweit and her colleagues in this country (1953) that middle-class children on the average are exposed to a stronger and more definite socialization process, particularly with respect to the control of aggressive and sexual impulses. (The work of Kinsey is also relevant here (1948, 1953).) In terms of our theory the impingement of a strong and a weak socialization process respectively on groups not differentiated with respect to conditionability should lead to differential attitudes and behaviour, and this prediction was, in fact, borne out among all the groups studied and at a very high level of significance.

Another study along rather different lines, making the same point, is that of Hewitt and Jenkins (1946), quoted in some detail in *The Structure of Human Personality* (Eysenck, 1953). They showed that over-inhibited behaviour in children was found in conjunction with repressive attitude on the part of the parents, while aggression and delinquency in children were found linked with attitudes of parental rejection and negligence. The correlations are in the neighbourhood of .6 and the findings certainly are such as would have been predicted in terms of our theory, i.e. the more strongly socializing the pattern of discipline, the more introverted the behaviour pattern of the child, and the less socializing the pattern of discipline, the less introverted the behaviour pattern of the child. (The proof furnished by this study is not absolute, of course, because hereditary influences are not taken into account. A more complex study along similar lines would be required to weigh the respective influence of these two factors.)

We have only been able to touch on a few points where the theory outlined above enables us to account for the observed facts; a full discussion of the whole evidence must await a later and more extensive publication. Before closing this section, however, we would like to draw attention to the fact that this account also succeeds in reconciling the contradictory views of Mowrer (1953) and Miller and Dollard (1950) quoted in our first section. If we can equate the concept of socialization with that of super-ego, as used by these writers, then it becomes

clear that Mowrer identifies the neurotic entirely with the hysteric, psychopathic type of person, while Miller and Dollard are concerned entirely with the dysthymic group. The under-socialized extraverted neurotic is described very well in Mowrer's formula:  $Id + Ego > S.E.$ , while the over-socialized introverted neurotic is described equally well in Miller and Dollard's formula:  $S.E. + Ego > Id$ . Ironically enough, then, it turns out that the theories of these men, while ostensibly concerned with neurosis as such, are really relevant to the extraversion-introversion dichotomy. This illustrates particularly well the importance which attaches to a thorough taxonomic or dimensional analysis of the field before an attempt is made to go beyond this to the dynamic or causal type of analysis.

## VI

The present section is devoted to an extension of the theory outlined above to the field of brain damage. There appears to be little doubt that clinically the pattern of symptoms of the hysteric often shows considerable similarities to the pattern of symptoms of the brain injured person. This casual observation would seem to lend some support to a hypothesis which was never formally stated by Pavlov but which can be derived from his writings and those of other experimentalists in this field, namely, that brain damage in general results in an increase of inhibitory potential as compared with the pre-injury phase. This theory received strong experimental backing from the work of A. Petrie (1952), which has been outstanding in this field. Making use of objective tests which were known to be measures of the extraversion-introversion dimension, and applying these to leucotomy cases before and after their operation, she showed that on each of the tests there was a marked shift in the direction of greater extraversion. This shift is exactly in line with the hypothesis that brain injury (in this case surgical interference) produces an increase in inhibition, and consequently an increase in extraversion, which is measurable in terms of the objective tests used. The findings, which have since been duplicated in several countries, provide a firm support for this theory.

Further support comes in terms of the two tests of inhibition which we mentioned in section III, namely, those of conditioning and figural after-effect. If brain injury produces an increase in inhibition, then we should expect brain injured persons to be less easily conditionable than comparable groups of non-brain injured persons. Reese and his associates (1953) have recently shown that this is indeed so, and that lack of conditionability is a direct consequence of brain damage.

In the field of figural after-effects our prediction would, of course, be that the brain injured would show greater figural after-effects than the non-brain injured. This prediction has been verified by Klein and Krech (1952) who, using the same apparatus illustrated in Figure 4, obtained the results given below in Figure 6. It will be seen that their brain-injured group showed reactions very similar to our hysterical group, while their normal group showed reactions intermediate between our hysteric and our dysthymic groups. These results also agreed very well with our prediction.

Another strand of evidence which should not be overlooked is the work of Bender, as summarized, for instance, in his book *Disorders in Perception* (1951). He makes use of the concept of extinction, which appears to be closely related to the concepts of inhibition and satiation. His method essentially consists of touching the face and the hand of his subjects simultaneously while the latter's eyes are closed. In brain damaged patients a considerable amount

of extinction is found, i.e. the patient reports only the facial touch and not that on the hand. With normal people this phenomenon is decidedly rare. Of particular interest in this connection is Bender's observation that "extinction on one half side of the body may be found in patients with psychoneurosis who show hysterical features, but in these cases the sensory changes do not

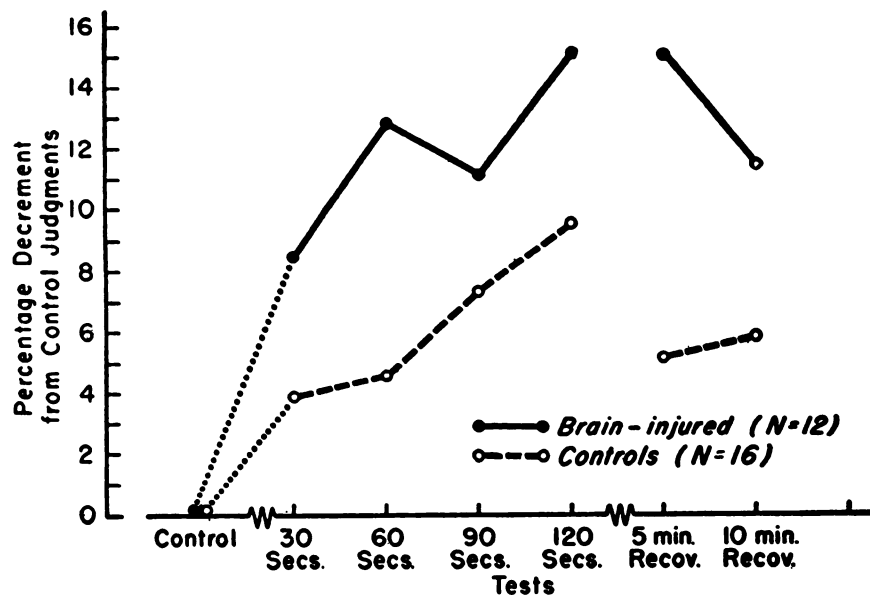


FIG. 6.—Comparisons of rate of development of figural after-effect in brain-injured and controls.

conform to the type or pattern usually found in somatic disorders of the nervous system". This occurrence of extinction in hysterics is well in line with our hypothesis.

It is also interesting to notice that extinction phenomena could be facilitated or produced through the use of inhibiting drugs such as alcohol or sodium amylal. The general extraverting effect of these drugs is well known, and it appears safe to venture the prediction that quite generally *inhibitory drugs will lower the conditionability and heighten the satiation effects* in experimental subjects, whereas *excitatory drugs will increase the conditionability and lessen the satiation effects* in experimental subjects. Experiments along these lines should be of particular interest as they enable us to actually vary the degree of extraversion-introversion, and thus treat this as an independent variable capable of experimental manipulation.\*

Another point of contact between our theory of extraversion-introversion

\* Since the above was written, an independent proof has been provided in favour of predictions of this type by C. Shagass (personal communication). Having succeeded in providing an objective sedation threshold for sodium amylal in terms of electroencephalographic changes, Shagass showed that sedation thresholds in terms of Mg./Kg. were low for hysterics (average 2.8) and high for dysthymics (5.0), with mixed neurotics intermediate (3.8). These highly significant findings are exactly in line with our theory; hysterics already near the inhibitory extreme require only a small amount of sodium amylal to reach the threshold, whereas dysthymics, being at a considerable distance from the inhibitory extreme, require a large amount of sodium amylal to reach the threshold.

and the effect of brain damage can be found in the exemplary work of Shapiro (1951, 1952, 1953, 1954), who has made use of a model, also derived from Pavlov, which is closely similar to ours. Basing himself on the negative induction phenomena observed by Pavlov, in which excitation of one part of a sensory surface would lead to inhibition in other parts, or in which excitation of one stimulus-response sequence led to inhibition of other stimulus-response sequences, Shapiro carried out a detailed investigation of such phenomena in brain injured and non-brain injured patients, showing that negative induction phenomena were very much stronger in the former than in the latter. Unfortunately there is, at the moment, no experimental evidence regarding the identity or lack of identity of reactive inhibition and inhibition produced by negative induction, and consequently it would be premature to claim that Shapiro's results support the general theoretical framework outlined here. None the less, it seems to the writer that clarification of the relationships obtaining between these different types of inhibition will result in a close integration of these two sets of phenomena. Further research in this sphere appears to be urgently required.

In pointing out the similarities between extraverted behaviour patterns and those resulting from brain damage, it was almost inevitable that the theory of brain damage developed should be simplified almost to the point of caricaturing the real complexities of the situation. We have discussed this question as if the actual location of the brain damage played no part at all in the determination of the reaction pattern, and while the results of Klein and Krech suggest that severity of damage rather than location is the important consideration, this is almost certainly too simple a view to be acceptable. In some unpublished work, Petrie has shown, for instance, that operations involving Brodman's areas 9 and 10 invariably produce more extraverted behaviour patterns in patients operated on, while operations not involving these areas, such as cingulectomy and orbital undercutting, do not produce such extraverted tendencies to anything like the same extent, if at all. Clearly, a theory of this kind will be required to be made much more specific before it can be clinically useful. However, even at the present stage of knowledge, and even using what is patently an over-simplification, none the less it has proved possible to make variable predictions in this field and to support or disprove hypotheses experimentally. In so complex and unexplored a field as this, it would be unreasonable to expect more of any hypothesis in its early stages.

## VII

An effort has been made in this paper to develop a hypothetico-deductive theory of anxiety, hysteria, and brain damage. This type of theory is characteristic of scientific endeavour which attempts to go beyond pure empiricism and simple induction to the postulation of laws and general theories, and the deduction from these of both observed and, as yet, unobserved consequences. Such a theory in its early stages is almost inevitably a gross over-simplification of the very complex relationships included within it. Nevertheless, from the point of view of science, it is more useful than more complex and more sophisticated theories, which do not enable one to make clear-cut and testable deductions. It is only in making such deductions and testing them that the value of a theory can be assessed, and it is only through experimental procedures of this type that improvements can be made and greater complexity achieved without loss of rigour.

In addition to presenting the theory, this paper has also presented a

number of facts and it should be the task of those who hold alternative theories to that developed here to see to what extent these results could have been predicted in terms of these other theories. To take but one example, psychoanalytic writers lay little stress on the differentiation between the hysterical and the dysthymic types of disorder, declaring that the differences are largely accidental, and that the one may easily turn into the other in the process of therapy. Such a view does not agree with the very profound differences observed in respect to physiological processes, such as conditioning and satiation. If a theory fails to predict observed facts of this nature, then it becomes the duty of those holding the theory to discard it or to amend it in such a way that the new facts are included within it. It would present a particularly interesting test of these psychoanalytic notions if, in terms of the Freudian system, predictions were made as to some of the facts likely to be found in experiments on reminiscence, apparent movement, and so forth, and if these predictions were compared with those made in this paper, and also with the eventual outcome of the experiments which are now in progress. Scientific advance has usually been particularly rapid when alternative theories were available, claiming to account for the same set of phenomena, and when crucial experiments became possible in the sense that phenomena predicted by one theory could not be predicted by the other, or were predicted to occur in a different manner.

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## APPENDIX

For the sake of completeness we should add to our account of attempts to link learning theory and personality theory the work of Spence and his colleagues at Iowa. We have already mentioned their demonstration that anxiety, as measured by the Taylor Anxiety Scale, a questionnaire especially constructed from items of the Minnesota Multiphasic Personality Inventory, was correlated with conditionability, and also their hypothesis that this result was

due to the high drive value of anxiety. We have also shown that this theory is untenable in view of the lack of conditionability of hysteric subjects.

More recently, Spence and his colleagues have gone on to consider the possible effects of anxiety as a variable in more complex learning situations than are presented by simple conditioning. In an admirable review of all this work, Child (1954) has indicated quite clearly the main line of argument followed by them: "The theoretical distinction they make among various learning tasks has to do with the number of competing response tendencies aroused. In simple conditioning, the situation is so controlled that a single stimulus-response tendency is aroused, with no appreciable competition; here the effect of increased anxiety is simply to improve performance through increasing the drive which, by Hullian theory, enters as a multiplier in determining the strength of this single S-R tendency. But in a more complicated learning task, such as serial rote learning, there are likely to be several competing tendencies present at any one point, and heightened drive will, according to Hullian postulates, have a multiplicative effect upon the strength of all these tendencies. If the correct response is not the one with greatest habit strength, the result will be to increase the advantage in response strength of those incorrect responses which are above it in the response hierarchy. Thus the probability of evocation of the correct response will be lowered, and the persistence of the dominant incorrect responses will be more protracted, so that during this period performance of subjects with high anxiety will be inferior to that of subjects with low anxiety."

The prediction that in situations where numerous incorrect response tendencies are aroused, high anxiety should make for poorer performance than low anxiety, has been borne out in several studies reported by Taylor and Spence (1952), Spence and Farber (1953), Lucas (1952), Montague (1953), and others. In each case, anxiety was measured by the Taylor Scale and the results seem to point quite definitely to some such relationships as that predicted by Spence. There are, however, a number of difficulties on the theoretical level, which make interpretation difficult. Some of these difficulties derive from the same error already noted in connection with Mowrer and with Miller and Dollard, namely, a failure to duplicate on the personality side the exact and careful work carried out on the learning side.

In the first place, then, it must be pointed out that the interpretation of a high score on the Taylor Anxiety Scale is difficult as the derivation of this Scale was largely subjective and psychiatric. Holtzman, Calvin and Bittermann (1952), as well as Deese, Lazarus and Keenan (1953) have found high correlations between the Taylor Scale and scales measuring general neuroticism, and, more recently, C. Franks (1954) has obtained a correlation of .92 between the Taylor Scale and the Maudsley Medical Questionnaire, which is a measure of general neuroticism. From correlations reported by him between the Anxiety Scale and a variety of other scales, it would appear that, in terms of the two-dimensional framework shown in Figure 1 of this article, the Anxiety Scale has considerable projection on to the neuroticism axis and a slight projection on to the introversion axis. This accounts for the fact that only slight correlations are found between the Taylor Scale and conditioning by Spence and his colleagues, and the fact that Hilgard, Jones and Kaplan (1951) and C. Franks (1954) failed completely to obtain significant relationships between the Scale and conditioning. To obtain such correlations a scale having considerable projections on the introversion scale and slight or negligible projections on the neuroticism scale should have been used. Such a measure is available in the form of Guilford's rathymia, or R-scale, as shown in *The Structure of Human Personality* (Eysenck, 1953), and our prediction is clearly borne out by the results reported by C. Franks, who found a correlation between eye-blink conditioning and the Taylor Anxiety Scale of .151 (insignificant), while that between conditioning and rathymia was  $-.483$ . (This correlation is negative because rathymia is a measure of extraversion; it is, of course, fully significant.)

If this argument be admitted, and if it be agreed that the Taylor scale is largely a measure of neuroticism, then it would seem to follow that whereas ease of conditioning is related to introversion, learning difficulties in complex situations are related to neuroticism, in the sense that the more neurotic a person, the more difficulties will he have in learning the correct one out of a number of possible responses having roughly equal probabilities of occurrence. Again, as in the case of conditioning, the crucial proof of this hypothesis as opposed to that of Spence will lie in the performance of a group of hysteric subjects as compared with normals and with dysthymics. Our prediction would be that hysterics should perform less well than normals, and no better than dysthymics; Spence's prediction would presumably be that they should perform much better than dysthymics (due to absence of anxiety).

A second objection to the theories of Spence has been presented by Child in connection with the experimental studies of Mandler and Sarason (1952). These investigators also used questionnaires of anxiety, but relied for their experimental design mainly on the arousal of anxiety through different test instructions, thus manipulating the independent variable. On the basis of the data collected by them, they arrived at a rather different type of hypothesis from that developed by Spence. In Child's summary, they argue that "high anxiety subjects evidently have habits of responding to anxiety with various responses, internal and external, which are incompatible with efficient pursuit of a complex task; hence these subjects do worse in a situation which evokes much anxiety (through ego-involving instructions, announcement of failure, etc.) than in a situation which does not evoke much anxiety. Low anxiety subjects, on the other hand, evidently lack strong habits of responding to anxiety with task-irrelevant responses, so that anxiety-arousing instructions have as their main effect on these subjects an

improvement of performance through increase in drive. (The increase in drive in an anxiety-arousing situation must of course be supposed to occur in the high anxiety subjects also, but its direct effect in improving performance to be obscured through interference by the task-irrelevant responses made to it.)”

Interesting as these Mandler and Sarason studies are, neither they nor a similar one by Waterhouse and Child (1953) can be considered to be in any sense definitive. Again, the failure seems to be largely due to a lack of a taxonomic framework in terms of which results can be interpreted. We are shown that different persons react differently to anxiety producing stimuli, but unless we can specify in terms of some such framework as that given in Figure 1 the position of groups responding thus differently, it becomes exceedingly difficult to generalize the findings or to link them with personality theory as a whole. It is to be hoped that a rapprochement may be achieved in the near future which would enable such a link to be made.

If it be true that the personality variable relevant to the Spence and Sarason studies is that of neuroticism, then we may be able to find an explanation of the findings in terms of the Yerkes-Dodson law (1908). This law, as is well known, specifies that *the more complex a task, the lower the optimum drive level required for its execution*. If we regard a person high on the neuroticism continuum as one in whom autonomic drive levels are particularly high, and particularly easily aroused, whereas in a person low on the neuroticism continuum autonomic drives are at a lower level, and less easily aroused, then it would seem reasonable to predict that increase in autonomic drive level would lead to a decrement in performance on complex tasks in the more neurotic, while it would lead to an improvement in performance in the less neurotic. More exact predictions could only be made in terms of accurate measurement, both of the degree of neuroticism of the subjects taking part in the experiment, and of the degree of drive leading to improvement or decrement of performance respectively. A beginning has already been made in both these directions, but a great deal more experimental work is required before the truth or falsity of this hypothesis can be demonstrated in the requisite quantitative detail.