CORTICAL INHIBITION, FIGURAL AFTEREFFECT, AND THEORY OF PERSONALITY[1]

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THE formulation of a complete theory of personality must be based on the discovery of invariances of two rather different types. In the first place, what is required is *static* or *descriptive* invariance, i.e., the taxonomic, nosological, or dimensional analysis of personality. Work of this kind would result in a descriptive system of personality in terms of a limited number of abilities, traits, and attitudes; in the exact sciences the most obvious analogue to this system would be the discovery of the Periodic Table of Elements. The statistical methods involved in studies of this kind would be those making use of analysis of interdependence (correlational analysis, component analysis, association and contingency analysis, factor analysis).

In the second place, what is required is *dynamic*, or *sequential* invariance, i.e., the analysis of lawful sequences of behavior and the discovery of their causes. Work of this kind would result in a causal system of laws in terms of concepts such as conditioning, inhibition, oscillation, etc.; in the exact sciences the most obvious analogue to this would be the discovery of the laws of motion. The statistical methods involved in studies of this kind would be those making use of analysis of dependence (analysis of variance and covariance, regression analysis, and confluence analysis).

As has been pointed out elsewhere (5), a logical case can be made out for maintaining that the *static* type of analysis should precede the *dynamic*; before we can discover dynamic laws responsible for extraversion, say, or neuroticism, we must demonstrate that these concepts do in fact refer to measurable and operationally definable entities. Work summarized in *The Structure of Human Personality* has shown that many different investigators holding divergent points of view and making use of a great variety of test procedures can be found to agree with respect to their main conclusions in the taxonomic field. The following six points present a brief summary of the main areas of agreement:

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 of 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 orectic 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. (See example below.)
6. The main dimensions involved in the

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analysis of personality for which sufficient experimental data are available to make possible a theoretical formulation are neuroticism and extraversion-introversion.

While the congruence of empirical findings in this field is welcome, it should not be allowed to disguise from us the fact that the task of personality theory cannot stop halfway. We would be well advised to regard traits, types, abilities, attitudes, and "factors" generally not as the end products of our investigation, but rather as the starting point for a more causal type of analysis. Thurstone (44) has pointed out that a coefficient of correlation is a confession of ignorance; it indicates the existence of a relation but leaves the causal problem quite indeterminate. Much the same is true of a statistical factor; based, as it is, on an analysis of a set of correlations, it still does not in itself reveal to us anything about the causal relations at work. In this paper, therefore, an attempt is made to go beyond the purely descriptive studies which have so far engaged the main attention of our laboratory and to attempt the construction of a causal hypothesis with respect to at least one of the main personality dimensions.

EXTRAVERSION AND THE CORTICAL INHIBITION HYPOTHESIS

A brief summary of an experimental investigation will indicate the type of fact calling for an explanation. Proceeding on the hypothesis that the test differences between hospitalized neurotics and nonhospitalized "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, reactive depressives, obsessionals, anxiety states, mixed neurotics, and normals (13). Retaining the hysterics, anxiety states, and normals as criterion groups, intercorrelations were calculated between tests for the subjects in the remaining groups, and a

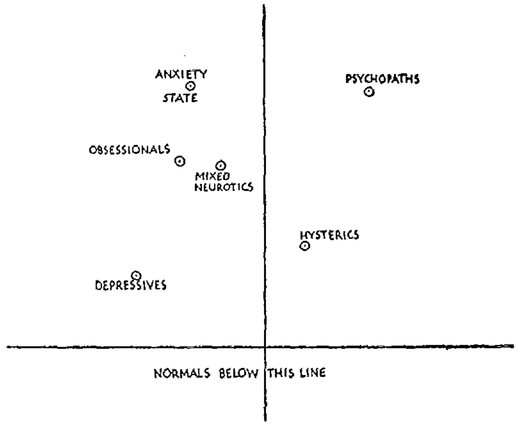

FIG. 1. EMPIRICALLY DETERMINED POSITIONS OF GROUP MEANS FOR NEUROTIC PATIENTS DIAGNOSED AS REACTIVE DEPRESSION, OBSESSIONAL, ANXIETY STATE, MIXED, HYSTERIC, AND PSYCHOPATH, RESPECTIVELY

Lawley-type factor analysis was 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 factor were then calculated for the persons in the various groups. Figure 1 gives a 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 (8) 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 (anxiety state, reactive depression, obsessionals) on the other as criteria for any predictions made in terms of a theory of extraversion-introversion.

One further fact is relevant in connection with any hypothesis regarding extraversion-introversion. In a study of monozygotic and dyzygotic twins, McLeod (24) has shown that a factor of extraversion-introversion (in addition to other factors) could be obtained from the intercorrelation of a large number of objective tests; he also found that the intercorrelation of factor scores was very much higher for the monozygotic than for the dyzygotic twins. This indicates that extraversion is strongly based on an inherited disposition. If we are willing to use Holzinger's coefficient h^2 as a very rough index of the contribution of heredity to the variance of our extraversion measure in this sample, we would have to conclude that the contribution of heredity is very much stronger than that of environment.

This finding suggests that our search for a causal factor responsible for extraverted behavior should be concentrated on properties of the central nervous system, and more particularly the cortex, as it is unlikely that peripheral factors could be responsible for the far-reaching and complex differences observed between extraverts and introverts. Historically there have been several attempts in this direction; we need only mention the work of Gross (10, 11) on the primary and secondary function, and that of Spearman (41) on perseveration. Experimental evidence is not lacking to show that these early attempts were quite unsuccessful; the recent work of Rim (34), for instance, has shown not only that there is no one general factor of perseveration, but also that none of the twenty or so tests of perseveration used by him succeeded in differentiating at a reasonable level of significance between hysterics and dysthymics.

More acceptable, perhaps, is a theory proposed by Pavlov (26), who considered the phenomena of hysteria to be closely linked with his concept of *inhibition*. Postulating excessive concentration of excitation in a weak nervous system, Pavlov argues that in the hysteric the process of negative induction should give rise to intense inhibition effects. His theory is difficult to follow in detail, and testable deductions cannot easily be made with any confidence. Further, Pavlov did not extend his tentative hypothesis to the typological field, nor did he himself carry out any experimental work on human beings to support or refute it. Nevertheless, the theory here presented is essentially a development and simplification of his. It bases itself on the concept of *reactive inhibition* developed by Hull (17), rather than on that of *negative induction* developed by Pavlov (27), because the evidence in favor of the former appears more conclusive than the evidence in favor of the latter, and also because the former seems to lend itself more easily to the formulation of exact and testable predictions.

We may state this theory in three parts, dealing respectively with the general law, the postulation of individual differences, and the typological postulate. The general law reads as follows:

A. Whenever any stimulus-response connection is made in an organism (excitation), there also occurs simultaneously a reaction in the nervous structures mediating this connection which opposes its recurrence (inhibition). This hypothesis is a more general formulation of Hull's first submolar principle; it states in effect, as he puts it, that

> all responses leave behind in the physical structures 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 (ΔI_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.

The second part of the hypothesis deals with the problem of individual differences adumbrated by Pavlov but almost completely neglected by Hull. A statement of this part of the hypothesis might be as follows:

B. Human beings differ with respect to the speed with which reactive inhibition is produced, the strength of reactive inhibition, and the speed with which reactive inhibition is dissipated. These differences themselves are properties of the physical structures involved in the evocation of responses.

The third part of the hypothesis relates A and B to the results of taxonomic work summarized above and states:

C. 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 behavior and to develop hysterical disorders in cases of neurotic breakdown; conversely, individuals in whom reactive inhibition is generated slowly, in whom weak reactive inhibitions are generated, and in whom reactive inhibition is dissipated quickly, are thereby predisposed to develop introverted patterns of behavior and to develop dysthymic disorders in cases of neurotic breakdown.

Comparatively little work has been done in this field since Pavlov's original fragmentary hypotheses were formulated. The experiments by Welsh and Kubis (45, 46) have lent some support to hypotheses of this type. In one experiment these investigators used PGR conditioning on 82 control subjects and 51 neurotic patients. They found, as could be predicted from the inhibition theory, that their patients, most of whom were of the dysthymic type, conditioned very much more quickly than did the controls (average number of repetitions required for the production of a conditioned response was 8.6 ± 3.1 in the patients and 23.9 ± 8.2 in the controls). Among the patients an attempt was made to rate the degree of anxiety from which they were suffering; it was found that the average number of repetitions required to produce a conditioned response in those with great and moderate anxiety was 7.1 and 8.4 respectively; in those with mild or no anxiety the number of repetitions required was 22.2 and 26.3. (Correlations between conditionability and age and intelligence were quite insignificant; test-retest reliability was .88 in the normal group.)

In another experiment 24 dysthymic patients were contrasted with 22 controls. Again the mean number of repetitions required was significantly different for the two groups, being 7.5 ± 2.31 for the dysthymic patients, and 21.86 ± 7.97 for the controls. Some hysterics were also tested and were found difficult to condition.

The only investigation, however, to put the hypothesis to a proper test by including a matched group of hysterics as well as normal and dysthymic groups was carried out at the Maudsley Hospital by Franks (7). Using the eyewink reflex to a puff of air as the response, and a tone as the conditioned stimulus, he obtained unequivocal evidence that dysthymics condition more quickly than normals, and normals more quickly than hysterics. (The normal group, being a random sample of the population, would include extraverts and introverts in roughly equal proportions and would therefore be ambivert on the average, and consequently intermediate between the extravert-hysteric and the introvert-dysthymic groups.)

Among several other investigators who have succeeded in relating speed of conditioning to dysthymia, the work of Taylor (42) and Taylor and Spence (43) is of particular interest, as these investigators advance an explanation of the phenomenon which is somewhat different from our own.

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 states 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 as the same prediction would be made in terms of both hypotheses.[2] It seemed necessary, therefore, to choose a prediction which would produce positive effects in terms of our hypothesis, but where no such prediction could reasonably be made in terms of the Taylor-Spence hypothesis. An attempt to formulate such a deduction will be made in the next section.

[2] Spence and Taylor (43) use the Taylor Scale of Manifest Anxiety as a measure of anxiety in spite of the fact that little evidence is brought forward to support any assumption that it correlates with clinical estimates of anxiety. The work of Holtzman (2), as well as that of Sampson and Bindra (35), in which an attempt is made to link up scores on this scale and independent criteria, fails to support Taylor's hypothesis. Franks (7) has shown that contrary to the Spence and Taylor hypothesis hysterics, whose scores on the Taylor scale are about as high as those of dysthymics, are more difficult to condition than members of a normal group, whose scores on the Taylor Scale are very much lower. He also failed, as have other investigators (2, 14), to obtain a significant correlation between conditioning and score on the Taylor Scale. These findings throw considerable doubt on the Spence-Taylor hypothesis.

Cortical Inhibition and Figural Aftereffect

In searching for a phenomenon which would avoid the ambiguity of results encountered in the work of conditioning, it was found necessary to go back from Hull's development of learning theory to Pavlov's somewhat more fundamental position. Pavlov regarded the conditioned reflex as a tool for investigating the dynamics of cortical action rather than as a paradigm of learning. He considered that the laws discovered by him had perfectly general validity and were not restricted to the very special circumstances of the conditioning experiment; indeed, he suggested explicitly that perceptual and other phenomena could find an explanation in terms of inhibition, excitation, disinhibition, etc. It seems possible, therefore, that we may be successful in our search if we look for perceptual phenomena to which our general theory may be found applicable.

The phenomenon chosen for this purpose was the figural aftereffect discussed by Köhler and Wallach (20), Gibson (9), Luchins (23), and others, in a series of articles. Essentially, the effects observed 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. If, for instance, a circle is fixated for a period of one or two minutes, and is then withdrawn, other stimulus objects, such as a small square, appearing within that part of the retina and the cortex which had previously been surrounded by the circle, will appear smaller than a square of precisely the same size appearing elsewhere on the retina and the cortex.

Effects of this kind appear to be exactly in line with the statement quoted in explanation of Part A of our hypothesis to the effect that "all responses leave behind in the physical structures involved in the evocation, a state or substance which acts directly to inhibit the evocation of the activity in question" (17). It should be noted that in accepting the fact of the occurrence of figural aftereffects, we need not necessarily accept Köhler's theory regarding the origin of these aftereffects, just as in accepting the fact of Pavlovian inhibition we need not accept his theory of cortical inhibition. There is, indeed, a curious resemblance between the arch-atomist Pavlov, on the one hand, and the arch-Gestaltist Köhler, on the other, in that both have proposed what are strictly physiological, molecular theories of brain action to account for their findings, and that both theories are well outside orthodox neurology. Konorski (21) has discussed the relationship between Pavlov's physiological and neurological theories and those of Sherrington and other orthodox workers in some detail and has attempted to account for Pavlov's experimental results in more acceptable terms; Osgood and Heyer (25) have attempted to do a similar service for Köhler's figural aftereffects.

While we need not deal in detail with Köhler's theories, we must note the terminology used by him, which is in part at least bound up with his theory. The reader will find a more extensive discussion in a recent paper by Luchins and Luchins (23). Briefly, then, Köhler assumes that every visual figure is associated with currents in the visual sector of the nervous system, the currents being the results of a difference in density and brightness between figure and ground. (For ease of discussion we are presenting an example from the visual field, but the same arguments apply to all other sensory fields, and the experiment to be described shortly was, indeed, done in the kinaesthetic field rather than in the visual.) The visual sector is considered as a volume conductor, and figure currents are assumed to polarize all surfaces through which they pass. This polarization and certain aftereffects in the affected cells are called *electrotonus*, and it is known that this condition of electrotonus may proceed for some time after the polarizing current has ceased to flow. Köhler uses the term *satiation* to describe electrotonic effect of figure currents on the cortical sector; the term *figural aftereffects* is used to denote the alterations which test objects may show when their figure currents pass through a satiated region.

Satiation has as its main effect a localized inhibition in the sense that polarization of the affected cells increases their resistance to the passage of an electric current, thereby making the appearance of figure currents in that region more difficult, i.e., acting as an inhibiting agent. The main observable fact mediated in this way is the *displacement* of test objects from the affected region. This displacement is measurable, shows pronounced individual differences, and may be used both as a measure and as an operational definition of cortical inhibition in the perceptual field.

The importance of these satiation phenomena in their own right will be obvious to anyone familiar with Köhler's highly original and brilliant work in this field. From the point of view of general psychology, they are of particular interest in that they form a bridge between two large fields of study which have hitherto remained either out of touch or else frankly antagonistic to each other. One of these is the field of conditioning and learning theory; the other is that of perception. In this work on figural aftereffects we find at long last a *rapprochement* between these large groups of workers and the sets of facts unearthed quite independently by them, and it is encouraging to note that the general law of inhibition enunciated by Pavlov, and more explicitly by Hull, appears to be formally identical with that advanced by Köhler in terms of perceptual satiation.[3] As long as we regard only the speculative brain theories of these writers, we will tend to miss the essential similarity of their formulations; once we concentrate on the molar rather than on the molecular parts of their theories the similarity will be striking.

The main aim of this section, however, is not to point to similarities between Pavlovian and Gestalt theories, but rather to link both of these with personality theory. From what has been said above it follows immediately that if our argument is sound and if the "reactive inhibition" of Pavlov and Hull is indeed essentially identical with the factors involved in Köhler's "satiation," then it would follow directly from Parts B and C of our theory that hysterics should show satiation effects more markedly than dysthymics. In fact, three quite specific predictions can be made. In the first place, satiation effects should appear *earlier* in the hysteric group; in the second place, they should appear more *strongly* in the hysteric group; and in the third place, they should disappear more *slowly* in the hysteric group. These are quite specific

[3] Several workers in Great Britain (1, 3) have recently shown interest in attempts to bring together into one framework these two great fields.

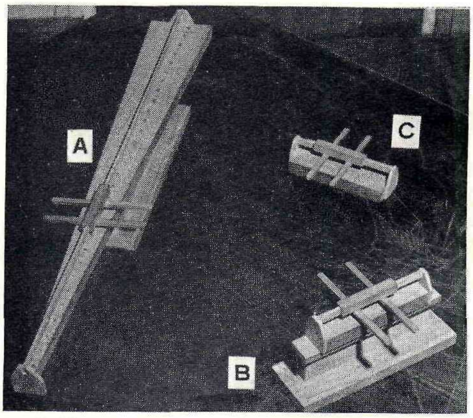

FIG. 2. PHOTOGRAPH OF APPARATUS USED FOR THE INVESTIGATION OF THE KINAESTHETIC FIGURAL AFTEREFFECT

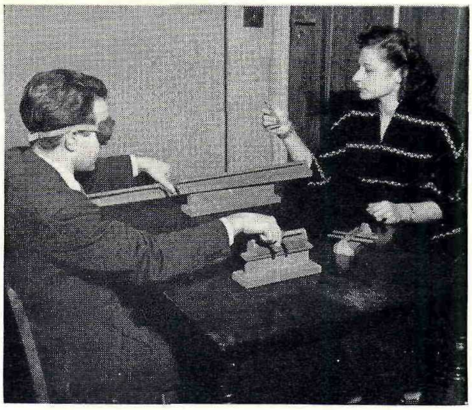

FIG. 3. SUBJECT UNDERGOING TEST TO DETERMINE THE EXTENT OF KINAESTHETIC FIGURAL AFTEREFFECT

predictions which can be tested experimentally, and it is only through such experimental verification that the theory can show its acceptability and usefulness. Our next section will, therefore, be concerned with certain empirical results obtained in comparing a group of hysterics and a group of dysthymics with respect to figural aftereffects.

AN EXPERIMENTAL TEST OF THE CORTICAL INHIBITION HYPOTHESIS: METHOD

Apparatus

The apparatus used in this experiment is an adaptation of that described by Köhler and Dinnerstein (19); the exact form of apparatus and procedure was taken from Klein and Krech (18), who used it in their work on cortical conductivity in the brain injured. As a full description and rationale are given by these authors, our own will be brief; Figs. 2 and 3 show the apparatus set out on a table, and the test in progress, and may serve to facilitate comprehension.

The apparatus consists of a comparison scale (marked "A" in Fig. 2), a test object (marked "B" in Fig. 2), and a stimulus object (marked "C" in Fig. 2). Movable riders are affixed to all three objects in such a way that the position of thumb and forefinger is fixed as the subject moves these two fingers up and down along the sides of the object. All objects are made of unpainted, smoothed hardwood. 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 (see Fig. 3).

Procedure

The subject (*S*) is blindfolded before he has an opportunity of viewing any part of the equipment. Having taken his seat in front of the apparatus, his task is explained in detail and a demonstration given. Then the experiment proper commences. Putting thumb and forefinger of his right hand into the rider on the test object, and thumb and forefinger of his left hand into the rider on the comparison scale, *S* is required to adjust the position of the rider on the comparison scale until the distance between the fingers of his left hand feels equal to the distance between the fingers of his right hand. This is the point of subjective equality, and all changes are measured from this point as the baseline. Four separate determinations are carried out, and the results averaged, to make this baseline more reliable.

The next step in the experiment consists in providing *S* with varying periods of constant tactile stimulation. For this purpose he is instructed to put his fingers into the rider on the stimulus object, which is slightly broader than the test object (2½ in. as compared with 1½ in.), and to rub the sides of the stimulus object at an even rate for periods of 30 sec., 60 sec., 90 sec., and 120 sec., respectively. Four determinations of subjective equality are made after each period of rubbing, in order to obtain more reliable measures. In this way the effect of rubbing the stimulus object on the perception of the test object is ascertained. Finally, after a five-minutes rest and again after another ten-minutes rest, the subjective width of the test object is again ascertained in order to establish the perseverative effects of the stimulation periods. These two sets of judgments are again obtained four times each in order to increase reliability.

Scoring

The predicted aftereffect consequent upon the rubbing of a stimulus object *broader* than the test object is an apparent shrinking of the test object, which should manifest itself in terms of a decrement in the width on the comparison scale judged equal to the test object. For each subject this decrement is expressed in terms of his own original baseline, so that individual differences in perceived equality are taken into account in the score, which thus is essentially a percentage decrement score, i.e., an estimate of the shrinkage that has occurred as a percentage of the original width of the object as perceived by each subject.

The following scores will be reported in this paper: 1. average percentage decrement after 30 sec.; 2. average percentage decrement after 60 sec.; 3. average percentage decrement after 90 sec.; 4. average percentage decrement after 120 sec.; 5. sum of the above four scores; 6. maximum single percentage decrement obtained from any subject. In addition to these poststimulation aftereffects, the following recovery period scores were obtained: (*a*) average percentage decrement after 5-min rest; (*b*) average percentage decrement after (10 min. + 5 min. =) 15 min. rest; (*c*) sum of these two scores.

Subjects

The *S*s used in this investigation were selected on the basis of two criteria. The first of these was that they should fall into the diagnostic groups of hysterics and dysthymics respectively. Diagnoses of conversion hysteria, hysteria, and psychopathy were accepted as falling into the former group; diagnoses of anxiety state, reactive depression, obsessional and compulsive disorders were accepted as falling into the latter group.

In view of the known unreliability of psychiatric diagnosis, which has been demonstrated, for instance, in *The Scientific Study of Personality* (4), it was considered advisable to have a second criterion which was independent of diagnosis. For this purpose a questionnaire was used which had been shown by Hildebrand (13) to be a good measure of extraversion. This questionnaire is Guilford's Rathymia scale (12), and the reader will find evidence regarding the adequacy of this scale as a measure of extraversion discussed elsewhere (6). The procedure followed was that no one with a score below 31 was accepted as extraverted, and no one with a score above 39 was accepted as introverted.[4] While it would have been desirable to have no overlap at all in the questionnaire scores of the two groups, it proved impossible to find a large enough group of subjects in the time available to

[4] Hysterics were found by Hildebrand (13) to have an average score of 37 ± 12, dysthymics one of 28 ± 10. In our sample the means were 40 ± 12 and 25 ± 10 respectively.

TABLE 1
POSTSTIMULATION FIGURAL AFTEREFFECTS—DYSTHYMICS

Source	*df*	MS	*F*
Between times	3	116.0082	0.7752
Between people	13	2050.0241	13.7010
Residual	39	149.6416	
Total	55		
$r_{11} = 0.9270$			

TABLE 2
RECOVERY PERIOD AFTEREFFECTS—DYSTHYMICS

Source	*df*	MS	F
Between times	1	1.9137	0.0541
Between people	13	642.1574	18.1429
Residual	13	35.3945	
Total	27		
$r_{11} = 0.9449$			

TABLE 3
POSTSTIMULATION FIGURAL AFTEREFFECTS—HYSTERICS

Source	*df*	MS	*F*
Between times	3	38.5519	0.3895
Between people	13	443.9761	4.4860
Residual	39	98.9703	
Total	55		
$r_{11} = 0.7771$			

TABLE 4
RECOVERY PERIOD AFTEREFFECTS—HYSTERICS

Source	*df*	MS	*F*
Between times	1	53.8013	1.1749
Between people	13	319.8050	6.9837
Residual	13	45.7933	
Total	27		
$r_{11} = 0.8568$			

reach this ideal. We can only suggest that the results found with the present groups would probably have been improved somewhat if a stricter criterion could have been employed.

There were fourteen subjects in each group, all of them males. The average ages of the two groups were 29.14 (hysterics) and 34.23 (dysthymics), an insignificant difference. Matrix IQ's were 100.86 and 104.92, Mill Hill Vocabulary IQ's 102.79 and 110.25; these differences also were insignificant.

Reliability of Scores

Granted that our methods of scoring, which we have taken over from Klein and Krech (18), are the most obvious ones, we must first of all ask ourselves questions regarding their reliability and consistency. To our knowledge, there are no reports in the literature dealing with this question, which is of crucial importance whenever test results are to be used as psychometric scores. Consequently, two analyses of variance were carried out for each of the two groups with whom we are concerned, i.e., the hysterics and the dysthymics. The first analysis deals with the scores which we have called average poststimulation figural aftereffects; the second analysis deals with the scores from the two recovery periods. With the formula suggested by Hoyt (16), reliabilities of .93 and .94 were found for the dysthymics; both of these were significant at the .001 level. For the hysterics the reliabilities are somewhat lower, being .78 and .86 respectively. Both of these, however, are significant at the .01 level. Full details are given in Tables 1, 2, 3, and 4.[5]

Having found dysthymics to be more consistent in their test performances than hysterics, we would expect to find the correlations among the six scores (four poststimulation scores and two recovery period scores) to be higher for the dysthymics than for the hysterics; this is indeed so. On comparison of the two sets of 15 correlations pair by pair, it was found that in 13 cases the dysthymic correla-

[5] This consistency in itself poses certain problems for the theoretical analysis of the figural aftereffect phenomenon. Some subjects consistently over-rate rather than under-rate the size of the test object after stimulation. This is very difficult to account for in terms of either the Gestalt or the statistical type of hypothesis. A survey of the literature, on other types of inhibition phenomena (massed and spaced learning, reminiscence, etc.) indicates that while most people act in conformity with prediction, some consistently go counter to prediction, i.e., learn better with massed rather than with spaced practice, etc. Theorists usually deal with averages rather than with individual cases and traditionally disregard aberrations of this kind. It seems reasonable to ask that any adequate theory should be able to account for discordant cases as well as for the admittedly large number of concordant ones.

TABLE 5

	Poststimulation Figural Aftereffects						Recovery Period		
	30 sec.	60 sec.	90 sec.	120 sec.	ε	Max.	5 min.	10 min.	ε
				Hysterics					
Means	9.68	10.58	13.09	12.78	46.13	20.74	4.35	8.00	12.35
Variances	9.48	13.57	14.22	16.30	1775.90	97.58	14.65	12.94	645.70
				Dysthymics					
Means	1.70	6.21	2.98	7.96	18.85	15.32	0.89	0.36	1.52
Variances	12.97	21.95	32.83	27.77	8200.10	190.12	15.77	20.59	1284.31

tion was higher; in one case the two were equal; in one case the hysteric correlation was higher. Thus, our expectation is borne out that dysthymics would be more consistent than hysterics.

RESULTS

We must next turn to the main differences between the groups. Means and variances for hysterics and dysthymics respectively are given in Table 5 for the four poststimulation aftereffects, the sum of the poststimulation aftereffects, the maximum poststimulation aftereffect, the five- and ten-minute recovery period aftereffects, and the sum of the rest period aftereffects. Four poststimulation aftereffects and the two rest periods are plotted in Fig. 4. All the results will be seen to be in line with prediction. Figural aftereffects in the hysteric group appear more quickly, are more strongly marked, and disappear more slowly than in the dysthymics.

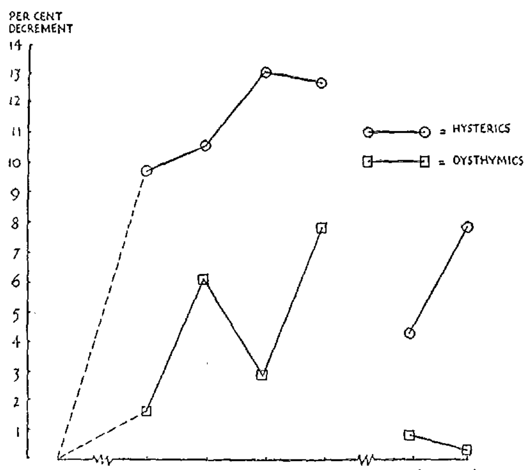

FIG. 4. AMOUNT OF FIGURAL AFTEREFFECT SHOWN AS PERCENTAGE DECREMENT AFTER FOUR DIFFERENT PERIODS OF STIMULATION AND TWO DIFFERENT PERIODS OF REST

The significance of the differences between the two groups was tested by means of Hotelling's T test (15). This over-all test invalidated the null hypothesis at between the .01 and .05 levels of significance. Individual one-tail t tests applied to the nine separate scores disclosed that only the 30-sec. period gave results significant at below the .05 level of significance; the other scores were significant at approximately the .10 level only. It is suggested that in future work more attention be paid to short periods of stimulation (between 10 sec. and 30 sec.) as longer periods of stimulation appear to increase variability without increasing differentiation. It might also prove useful to make use of more prolonged rest pauses; times of 15 min., 20 min. and 30 min. might give improved differentiation.

The calculation of differences between groups gives little idea of the strength of the relationship discovered. Accordingly product-moment correlations were calculated between scores on the figural aftereffect test and the R scale. In addition to the 28 hysterics and dysthymics used for the group comparison, an additional seven neurotics were included in this calculation. These *S*s had shown a discrepancy between diagnosis and score on the R scale, and had therefore not been included in the group comparisons. Correlations for this group of altogether 35 neurotic subjects were as follows: .374 (30 sec.); .252 (60 sec.); .236 (90 sec.); .218 (120 sec.); .321 (5 min.); .237 (10 min). It will be noted that with increasing periods of stimulation, correlations tend to fall off in a regular progression. As regards significance, the correlation for the 30-sec. period almost reaches the .01 level; of the others only the correlation for the 5-min. rest period passes the .05 level of significance. The remaining correlations just fall short of the .05 level. Significance levels were of course calculated by using one-tailed tests, as follows from the logic of the experimental design.

It is interesting to note the fate of the seven individuals in whom diagnosis and R score disagreed. In each case where a patient was diagnosed hysteric but had an R score which put him on the introverted side, relatively small aftereffects were found. In each case where a patient was diagnosed dysthymic but had an R score which put him on the extraverted side, relatively large aftereffects were found. In other words, when diagnosis and questionnaire disagree, agreement of the experimental test is much closer with the questionnaire than with diagnosis. In view of the widespread habit of heaping contumely upon questionnaires, this fact may deserve stressing.

DISCUSSION

It will not require much discussion to establish the relevance of the results of our experiment to a theory of neurotic disorder. Psychoanalytic theories have usually played down differences between the various types of neurotic symptomatology as accidental, unimportant, and variable; usually the implication has been that hysteria and the dysthymic disorders both lie close to each other along one single dimension of *regression*, and that hysterical symptoms are in a sense merely a defense against the overt anxiety shown by the dysthymic. On the basis of this type of theory, no fundamental differences would be expected on psychophysiological measures of conditioning or of figural aftereffects. The fact that such differences are observed considerably weakens the Freudian theory, and supports the dimensional theory outlined at the beginning of this paper. Another advantage of the dimensional theory appears to be that it can account for the similarities observed in the behavior and the symptomatology of hysterics, brain-injured, and leucotomized patients, a task not even attempted by psychoanalytic writers. A discussion of such an extension of our theory may be in order.

The experimental procedure adopted in section four was taken over directly from Klein and Krech, and a comparison of our results with theirs may be of some interest. They were concerned with differences between brain-injured patients and normals, and found that figural aftereffect was much more strongly marked among the former than in the normal control group. The average size of the over-all figural aftereffect was 12.08 per cent for the brain-injured and 6.25 per cent for the controls. The maximum degree of effect for the brain-injured averaged 19.50 per cent, for the controls, 13.00 per cent. Corresponding figures for hysterics and dysthymics are: 11.53 per cent and 4.71 per cent for average over-all effect, 20.74 per cent and 15.32 per cent for maximum effect. There is thus a distinct similarity in the behavior of the brain-injured in Klein and Krech's study and the hysterics in our own. The normal controls tested by Klein and Krech give results intermediate between our hysteric and dysthymic groups, though somewhat closer to the dysthymics.

These figures would seem to indicate similarities between hysteria and brain injury which are important from a theoretical point of view. In a series of studies (28, 29, 30, 31), A. Petrie has shown that one of the psychological aftereffects of leucotomy is an increase in extraversion, as measured by objective tests of personality similar to those used by Hildebrand in his factorial study (13). The theory on which the prediction of a change toward extraversion after leucotomy was based was essentially one of increased cortical inhibition following brain injury.[6] Such an hypothesis is much too broad and general to account for all the known facts and will presumably require a good deal of detailed modification, particularly with respect to the differential activity of various parts of the brain, and the effect of specific incisions and ablations. Thus, recent unpublished work by A. Petrie has shown that a change in the direction of increased extraversion is produced by all prefrontal operations involving the convexity (standard leucotomy, Rostral leucotomy, and

[6] Here again Pavlov's theory of negative induction has also been used to account for some of the observed effects (36, 37, 38, 39, 40); it is not clear to what extent negative induction and reactive inhibition can be identified with each other at the phenomenal level.

selective surgery of areas 9 and/or 10). On the other hand, cingulectomy and orbital undercutting, i.e., operations not involving the convexity, do not have aftereffects involving a shift toward extraversion on the tests used. If these results were to be confirmed, they would clearly indicate the need to make this general hypothesis much more specific.

Nevertheless, as a first approximation, this general hypothesis has led to the prediction of the phenomena observed by Petrie (28) and it does account similarly for the results of the Klein and Krech experiment. It would appear worthy of further investigation, particularly as it gives rise to very clear-cut predictions. Thus, we may predict that the formation of conditioned reflexes would be more difficult in the brain-injured than in the intact individual. Some evidence supporting this prediction has just been published by Reese, Doss, and Gantt (33). After leucotomy, we would predict that inhibitory effects would be more strongly marked than before, and we would also be able to make a number of predictions regarding the reactions of leucotomized patients on certain perceptual tests similar to those made in the concluding section of this paper with respect to hysterics.

Klein and Krech, in their paper, advance a somewhat different theory which, however, in most essentials appears to deviate but little from that used in our own work. They assume that

> . . . transmission rate of excitation patterns varies from individual to individual, from time to time within the same individual, and from area to area within a single cortical field at any time. With this assumption it is possible to appeal to *differential* cortical conductivity as a parameter which will help us understand inter- and intra-individual differences in cortical integration and therefore in behavior (18, p. 118).

It may be worth-while to indicate in just one sentence the essential difference between the conductivity hypothesis and the one advocated here. Klein and Krech postulate neural conductivity as a basic personality dimension, assuming that it may be high or low *prior to any stimulation*. We assume that individuals differ not with respect to conductivity, but with respect to the rate at which inhibition is aroused along cortical pathways by the passage of a neural impulse. The latter hypothesis seems to be more securely based on experimental findings, less subject to unprovable assumptions, and more easily testable. It is for these reasons that it has been preferred in this paper. It should be added, however, that both the conductivity and the inhibition hypothesis give rise to similar predictions in the case of the degree of satiation to be expected in the brain-injured and hysterical patients, and that the data reported here do not in any way disprove the conductivity hypothesis, any more than they prove the inhibition hypothesis.

It may be worth while, however, to indicate very briefly the type of prediction which our theory makes possible, and to suggest lines along which it could be disproved.

1. If we accept Köhler's demonstration that the rate of disappearance with time of the Müller-Lyer and other illusions is a consequence of figural aftereffects, it can be predicted that the rate of disappearance of the illusion should be more rapid with hysterics than with dysthymics, and in the brain-injured as compared with the normal.

2. If we accept Klein's interpretation of the phenomenon, it can be predicted that when the persistence of an afterimage is measured as a function of the duration of stimulus exposure, the duration of the afterimage in hysterics should fall off significantly as compared with dysthymics. Klein has already shown that this is so when the brain-injured are compared with normal subjects (18).

3. Phenomena of apparent motion may be reformulated in terms of the inhibition theory and it may be predicted that the optimal time interval for the perception of apparent movement would be decreased more in hysterics than in dysthymics after the introduction of some form of continuous stimulation in the path of the apparent movement. Shapiro (40) has shown, in experiments using continuous stimulation in order to produce experimental inhibition effects in the occurrence of apparent movement, that under conditions of inhibition the time-interval threshold was 140 sigma, as compared with 250 sigma under noninhibition conditions.

4. If a theory of satiation or inhibition be acceptable as accounting for reversal of perspective, then one would predict not only that the rate of reversal would increase in time as it is known to do, but also that this increase in

rate of reversal should be more marked among hysterics than among dysthymics. Our prediction here, as in the case of the experiment described in this paper, would relate more to a change of rate than to the initial rate of reversal, although the latter also should show differences in favor of the hysterics and the brain-injured.

5. Perceptual disinhibition phenomena of the type studied by Rawdon-Smith (32) and others might be presumed also to show differences between hysterics and dysthymics. On the hypothesis that we are dealing with the inhibition of an inhibition in these cases, it might be predicted that disinhibition should be more pronounced among hysterics than among dysthymics.

6. Critical flicker fusion would be expected to be observed at different frequencies in hysterics and brain-injured, as compared with dysthymics and non-brain-injured. This follows directly from our interpretation of the law of reactive inhibition. Some empirical data are available to support one of these predictions at least (22).

7. Rotation phenomena, such as have been described by Shapiro (40), have been explained by him in terms of inhibition (negative induction). If this hypothesis, which has led to important discoveries in the field of brain injury, should prove acceptable, then we would expect a greater degree of rotation among hysterics than among dysthymics.

In making these predictions, we have purposely kept within the perceptual field, but it is clear that many other predictions could be made in the fields of learning, memory, and motor behavior. Phenomena of reminiscence, of massed and spaced learning, of vigilance, of blocking, and many others have been interpreted in terms of inhibition. While it remains possible, of course, that in each separate case we must have recourse to a different type of inhibition, this does not seem a likely contingency, and the hypothesis certainly appears worth testing that it is the same type of cortical inhibition which causes all these phenomena, as well as the perceptual ones discussed above. The obvious method of testing this hypothesis appears to be in terms of individual differences, i.e., in postulating that a person found to show a high degree of inhibition with respect to any one of these phenomena should also show a high degree of inhibition with respect to all the others. It is hoped to provide evidence with respect to this generalized inhibition hypothesis in the near future.

SUMMARY AND CONCLUSIONS

An attempt has been made in this paper to work out a dynamic theory to account for a number of experimental findings in the field of personality related to the concept of extraversion-introversion. Following Pavlov and Hull, a theory of cortical inhibition was developed to account for observed differences in behavior and a deduction from this principle was made by extending it to the perceptual field. It was predicted that hysterics (as a prototype of the extraverted personality type) would be differentiated from dysthymics (as a prototype of the introverted personality type) in the *speed of arousal, strength,* and *length of persistance* of figural aftereffects. A comparison of two groups of carefully selected subjects showed that (*a*) hysterics developed satiation and figural aftereffects more quickly than did dysthymics; (*b*) that hysterics developed stronger satiation and figural aftereffects than did dysthymics; and (*c*) that hysterics developed more persistent satiation and figural aftereffects than did dysthymics. The differences are statistically significant and are in complete accord with prediction. In the discussion, certain parallels were drawn between hysteria and brain injury in terms of the theory outlined, with particular reference to the aftereffects of leucotomy. Lastly, a number of predictions were made from the theory which should permit of an experimental decision as to its worth-whileness.

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