

CONDITIONING AND PERSONALITY

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The application of learning theory to the study of personality presents many problems, some of which are here discussed in reply to a critique by Champion of the author's particular contribution to this field. It is suggested that such discussions are relatively fruitless unless they take into account the differences between 'strong' and 'weak' theories, and unless they bear in mind the many different ways along which predictions may be mediated from postulates.

Champion's (1961) recent comment on the writer's use of learning theory in relation to personality makes a number of points which could all be argued at length. Similarly, there have been other critics who have been concerned with other points of the general theory put forward in *Dynamics of Anxiety and Hysteria* (Eysenck, 1957). Some of these criticisms arise from a certain confusion which is almost inevitable when certain notions originally advocated by Hull have to be used with an alteration in meaning and content made necessary by more recent experimental findings. 'Reactive inhibition' in the writer's theoretical framework is clearly not peripheral and work-produced as it is for Hull, but central and not crucially related to the actual amount of physical work done. Similarly, Hull makes I_R , conceived as a negative drive additive with ${}_S I_R$, a habit; the writer has followed Gwynne Jones (1958) in subtracting I_R from D instead. The term 'excitation' has been used in the Pavlovian sense, i.e. as a facilitating factor in neural transmission, perception, and learning rather than in Hull's sense of ${}_S E_R$. Failure to take account of these and other changes in meaning make some of the criticisms levelled at *Dynamics of Anxiety and Hysteria* inapplicable.

A more important source of misunderstanding may be the conception of the role of theory in science held by the writer (Eysenck, 1960*a*). Hull, Champion and many other learning theorists appear to regard learning theory as what has been called a 'strong theory', capable of mediating precise quantitative deductions along rigorous lines. While this is no doubt a most desirable type of theory to have, it seems to the writer that at present the only theories available to psychologists are 'weak' theories which at best suggest areas of exploration, possible lines of advancement, and the outlines of experimentally testable nomological networks (Eysenck, 1960*a*). Weak theories of this type cannot reasonably be criticized along lines which would be appropriate for strong theories, and confusion between the two may account for a good deal of the rather futile warfare between those who label themselves as 'pro' theory and those who regard themselves as 'anti' theory.

The main advantages of the weak theory are (1) that it may lead to the discovery of new and unexplored facts, and (2) that it may raise new problems which had not previously been considered at all. In due course weak theories may turn into strong ones by pursuing the paths suggested by these new discoveries, and by the carrying out of experiments along these new lines; in this process there will inevitably occur a transmutation which will leave very little of the original theory intact, except

perhaps as a way of looking at the field, and in the way of certain general concepts. In the theory put forward in *Dynamics of Anxiety and Hysteria* the concepts of inhibition and excitation were linked in certain ways with extraversion and introversion, and used to make certain predictions, some of which were later verified experimentally; it was not the writer's intention that inhibition and excitation should be regarded as concepts having a perfectly rigorous definition and meaning, but it was his hope that the relations indicated by the theory would help in clarifying the nature and meaning of these concepts (Eysenck, 1962).

It may be possible to exemplify the way in which the writer has attempted to use learning theory by taking as a particular example the relationship between conditioning and personality. Starting with a theory about the existence of two important personality dimensions, neuroticism or emotionality and extraversion-introversion (Eysenck, 1960*b*), we went on to seek for causal factors to account for a given individual's position on these dimensions. Emotionality may be identified, with some misgivings, as a consequence of an over-labile autonomic system; extraversion was conceived of as being related to the notions of inhibition and excitation as used and operationally defined by Pavlov and Hull. It seemed that in order to mediate predictions from the conceptual and experimental levels to the behavioural level it was necessary to postulate some such mechanism as 'defective conditionability' in extraverts, and 'enhanced conditionability' in introverts; as will be shown later there are many different ways in which predictions of this kind could be derived from the hypothesis linking extraversion-introversion and the excitation/inhibition balance.

There was available at the time no single experimental report of any such connexion, although there were several experiments purporting to verify the Spence-Taylor hypothesis relating conditionability and scores on the Manifest Anxiety Scale. This scale is a good measure of neuroticism, correlating very highly with such scales as the MPI Neuroticism scale, or the Maudsley Medical Questionnaire; it also correlates positively, although much less highly, with introversion (Eysenck, 1957). When correction is made for attenuation due to unreliability of the scales, it can be shown that scores on the MAS can be predicted fairly exactly from scores on the MPI, the Neuroticism scale contributing some 80% and the Extraversion scale (reversed) contributing less than 20%. The Spence-Taylor notion of anxiety as a drive seemed to identify their predictions of higher conditionability with the dimension of neuroticism; the writer's theory would account for their findings in terms of the (small) introversion content of the MAS. We thus have two clearly different predictions, relating 'conditionability' respectively to *neuroticism* or to *introversion*; work done on the MAS is largely irrelevant in this connexion as this scale partakes of both these orthogonal dimensions. (It would of course be relevant if the findings had been consistently negative, as this would have contraindicated both theories.)

At the writer's suggestion, the crucial experiment was carried out by Franks (1956, 1957) who used both normal and neurotic introverts and extraverts. Taking both groups together, he obtained results which show that in the eighteen test trials interspersed among the conditioning trials introverts show a proportion of conditioned responses over twice as large as that shown by extraverts; the scores of 35 subjects in each group are given in Fig. 1. Similar results have been obtained with the GSR

by Vogel (1960); she found that introverts required a mean of 5·18 trials to conditioning, while extraverts required 12·25 trials. Franks failed to find any relationship between conditioning and neuroticism in either of his groups; Vogel found a barely significant one. Halberstam (1961), using PGR conditioning on normal controls, hysterics and psychasthenics, found that the introverted neurotics conditioned in half the number of trials (19·61) needed by the extraverted neurotics (40·94); the normal controls were intermediate (23·33). These findings 'fully agree with the similar results obtained by Franks'. Hysterics were also found to extinguish more quickly, both under 'informed' and 'uninformed' conditions. (Cf. also Barendregt, 1961,

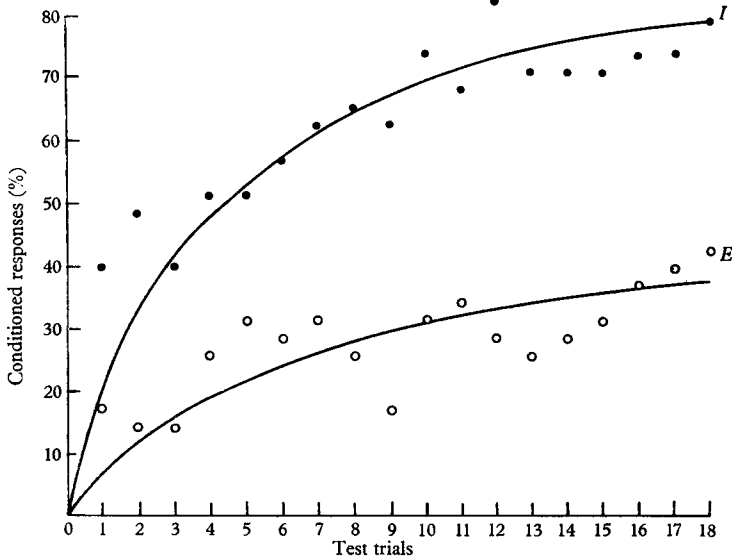


Fig. 1

p. 211, who found a correlation of 0·29 between eye-blink conditioning and introversion.) These findings seem to indicate that the predicted relation between conditioning and introversion is not entirely absent, and that if these results can be duplicated by other research workers, then our theory has passed the main test of a scientific theory, to wit, *the mediation of prediction leading to new and previously unexpected facts*. It also fulfils a second test, that of accounting for the known facts, such as the correlation between conditioning and the MAS. It is not necessary at this point to discuss the now voluminous literature bearing on this point; most studies give results in the predicted direction, but these are not always significant. Thus Das (1957) failed to find a significant correlation between introversion and conditioning, but his subjects were partly white, partly coloured and non-European; inspection of the questionnaire responses of the two groups showed that they could on no account be regarded as coming from the same universe. Field (1960) obtained rather lower correlations between introversion and conditioning than had Franks, but he was working with a prison population to whom the ordinary MPI with its stress on sociability questions does not apply particularly well.

When it comes to the precise mediation of our hypothesis in terms of learning theory, a large number of possibilities must be considered, and it is quite impossible to make a rational choice at present. Consider these possibilities. (1) Introverts have greater excitatory potential, facilitating sH_R acquisition. This would directly lead to the correct prediction regarding the higher conditionability of introverts; some evidence in favour of this hypothesis is given by Eysenck (1960*c*). (2) Extraverts have greater inhibitory potentials (I_R). This could be made to lead to the correct prediction of higher conditionability of introverts along several different lines: (a) I_R , being a negative drive, subtracts from D according to the Gwynne Jones formula (Gwynne Jones, 1958), leading to lower effective drive \bar{D} . This deduction would bring drive into the picture, but along quite a different route from that favoured by Spence and Taylor. (b) I_R , attaching to the stimuli used in the experiment, leads to habituation and consequently to a lowering of the *effective* strength of the UCS. (c) I_R , generalizing over the cortex, leads to 'sleep-inhibition', or loss of 'arousal'; there is some direct evidence for both (b) and (c) in a recent paper by Voronin, Sokolov & Bao-Khua (1959). (d) The autonomic responses associated with the stimuli become habituated due to I_R attaching to them and thus lower the total effective drive-arousal combination. This deduction also would lend to the prediction of a lessening of drive, again quite different in origin to the Spence-Taylor hypothesis. Certain subjective observations made by Franks (1956) and by other investigators in our laboratories indicate that one or all of these possibilities may be true; thus Franks remarks that 'the poor conditionability of the hysterics, their more rapid PGR adaptation to the air puffs, their subjective reports that the air puffs were not very disturbing, and perhaps their reports of feeling sleepy all support the hypothesis that hysterics are in a state of cortical inhibition'.

It should not be assumed that these possibilities exhaust the supply; many others will occur to the reader. It will be a long time before the precise chain of causation is known, and much precise and detailed work will be required. It is unlikely, as Champion apparently believes, that results such as those plotted in Fig. 1 can be used neatly to separate our rate of learning and drive strength. If the reader will consult Champion's Figs. 1, 2 and 3, which illustrate the consequences on conditioning of variations in rate of learning, in drive strength, and in both jointly, he will notice that a decision in any particular case only becomes possible (if then) when the curves are known to have reached their respective asymptotes. If now the reader will draw a line parallel to the ordinate intersecting the abscissa about $\frac{1}{2}$ in. from the origin, and consider the curves up to this point, he will find that they all look pretty much alike, and indeed that they all look rather like those given in Fig. 1. In other words, where the curves clearly fall short of the asymptote, no decision between these various explanations is possible. Eysenck (1957) did provide a discussion and some tentative mathematical manipulations of the factual observations, but as clearly pointed out on p. 119, these depend on extrapolations which of necessity are very uncertain indeed. Champion has taken these *jeux d'esprit* too seriously; they were not intended to prove anything, but merely to indicate lines along which research might profitably proceed.

While thus there are many possibilities of interpretation of the observed results, it would seem that the original Spence-Taylor hypothesis is not strongly supported.

The evidence suggests a definite relationship between conditioning and introversion, little or none between conditioning and neuroticism. (More recent work by Becker & Matteson (1961) may require a reappraisal of this conclusion.) Champion says that the members of the Iowa group 'have not, as Eysenck suggests, seriously attempted to relate learning theory to personality functioning'; but it has never been maintained that they had. Quite the contrary; Eysenck has criticized the Spence-Taylor group for failing to consider the well-substantiated knowledge regarding personality structure now available (Eysenck, 1957), and thus arbitrarily settling on a questionnaire (the MAS) without any knowledge of its dimensional structure. They thus failed to perform what must be regarded as the crucial experiment in the argument between them and the London group, to wit, the examination of subjects *high* on neuroticism (and the MAS) and *low* on introversion. The hysterics in Franks's group fulfilled this qualification, and as will be remembered, showed low conditioning in spite of their high MAS scores. Similar findings are reported by Lykken (1957), working with psychopaths and sociopaths, as well as by Tong & Murphy (1960), who refer to the low conditionability of psychopaths as 'an accepted laboratory fact' (p. 1285). The evidence against the Iowa group, and in favour of the London group, seems sufficient to indicate that the suggestion of a significant relation between introversion and conditioning may not be altogether mistaken.

To say this does not, of course, rule out the possibility that under certain circumstances neuroticism too may be found to be related to conditionability, or that introversion may fail to be so related. It would be naïve to believe (although it is often useful and time-saving to write and talk as if this were so) that there is a specific and permanently demonstrable relation between 'conditioning' and a given personality trait. The term 'conditioning' is not closely enough defined to carry any predictive burden, and any proposition stated so broadly cannot in the nature of the case be disproved or supported. (1) In the first place, different types of conditioning (eye-blink GSR, heart rate, hand-withdrawal, skin temperature) do not usually intercorrelate very highly, suggesting the existence of considerable response specificity (perhaps analogous to Lacey's (1950) concept of autonomic response specificity.) (2) Within a given type of conditioning, there are many important parameters which must be explored in detail in order to discover their relevance to the hypothesized relationship. Thus in eye-blink conditioning the CS-UCS interval is usually taken at the population optimum of about 450 msec.; it is quite in line with theory to assume that introverts and extraverts have different optima, so that variations in this interval from experiment to experiment may change the nature of the relationship between conditioning and personality. (Some indirect evidence for this view is offered by Eysenck (1962).) (3) Both Franks and the Spence-Taylor group use a fixed strength of puff to the eye as the UCS. But individuals differ considerably with respect to their lid-closure thresholds, and such individual differences should be taken into account; our more recent studies along these lines support the postulation of considerable individual differences in this threshold. (4) Experiments may differ in the degree of *awareness* the subject is allowed of the purpose and the nature of the experiment. We have recently compared the traditional type of eye-blink experiment with one in which subjects were instructed to respond with a depression of a key to the CS; UCS was introduced as a punishment for slow reactions. Awareness is thus

reduced, and altogether higher rates of conditioning achieved (Issa, unpublished data). (5) Experiments differ in the degree of *spatial inhibition* allowed (distraction, etc.). If extraverts, as postulated, show greater degrees of spatial inhibition, then manipulation of this variable may be important in accounting for different experimental results. (6) The strength of the UCS differs from experiment to experiment; it is not impossible that with *low* values introversion shows higher correlations, with *high* values neuroticism. (7) Experiments differ in length of total trial, length of inter-trial pauses, spacing of such pauses, as well as in the use of separate test trials, interspersed with training trials. It may be surmised that experimental arrangements will favour introverts more the more massed the trials are, and the greater is the number of unreinforced test trials, as compared with reinforced training trials. These are only some of the parameters which may distinguish one 'conditioning' experiment from another, and it is by no means reasonable to assume that correlations between conditioning and personality achieved with one combination will be duplicated with another. Clearly, the search for a satisfactory conception of 'conditionability' and its relation to personality variables is only at the beginning; it may be surmised that the outcome will throw important new light, not only on personality development and breakdown, but also on the very concept of conditioning itself.

In conclusion, it seems worth while to draw attention to Champion's reticence in refusing to deal with 'the fairly extensive sections of Eysenck's book which relate to inhibition, work decrement, reminiscence, and similar factors'. The theory put forward derives whatever interest it may have from the fact that it tried to relate a large number of experimental facts to a limited set of explanatory variables, and these in turn to personality and drug action; in each particular case many different alternative explanations of the experimental findings are possible. It is when the total set of related data is viewed as a whole (which as we all have learned long ago is more than the sum of the parts) that such alternative hypotheses, which never cover more than one, or at most a few of the experiments, cease to be attractive. One's judgement of the possibility of explaining the lesser conditionability of extraverts in terms of cortical inhibition is probably increased by the fact that such inhibition can be demonstrated in many other circumstances, and in connexion with many other types of experiment. To isolate one phenomenon and treat the rest of the evidence as non-existent is not likely to give the reader the correct perspective for judging the theory under discussion; '*natura in reticulum sua genera connexit, non in catenam*' (Haller, 1768).

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