

## ANXIETY AND HYSTERIA—A REPLY TO VERNON HAMILTON

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In the introduction to my book, *The Dynamics of Anxiety and Hysteria*, I pointed out that: 'To indicate the tentative nature of the hypotheses put forward, I have included in the last chapter a discussion of various criticisms to which the general theory here put forward is subject; this should have the added advantage of enabling reviewers to pass straight from the introduction to the conclusion without having to read the intervening parts.' Hamilton, in his 'methodological critique', has apparently followed this advice, which was not meant to be taken seriously; he has also taken up many of those criticisms which I have put forward myself, and which he now presents as his own. In addition, he has quite misinterpreted a number of facts in a way which is difficult to reconcile with the care required of anyone purporting to examine any body of scientific work. In dealing with his points I shall concentrate on the main issues, leaving aside the many statements of opinion which fill the pages of Hamilton's essay.

To begin with, Hamilton contends that 'Eysenck's theory of the relationship between the dimension of introversion-extraversion and the current nosological categories of the neuroses is primarily based, not on his own earliest investigations, but on the findings of Hildebrand'. He then goes on to criticize Hildebrand's Ph.D. thesis on a number of points. It is interesting to note the number of errors in this one sentence. The theory in question, as I have always been careful to point out, is due to Janet and Jung, not to myself. What I have done has been simply to make deductions from their hypotheses and to test these experimentally. These deductions are of several different kinds, ranging from factor analytic to more strictly experimental ones. Hildebrand's work is only one of many strands; if it had never been carried out, the evidence available would still have been quite sufficient, in my view, to support Jung's hypothesis. The exclusive concentration on Hildebrand's study is therefore quite misplaced.

The actual criticisms made by Hamilton of the Hildebrand study illustrate a lack of awareness of even the more simple theorems of factor analysis. Thus, in discussing his Table 1, he claims 'that the major measures of introversion-extraversion and of neuroticism are significantly correlated. These results make it clear that the Guilford scales cannot produce an undiluted measure of either neuroticism or of introversion extraversion and that orthogonal solutions of such a matrix are factor analytical artifacts rather than evidence for the unidimensionality of Eysenck's measures.' This is quite erroneous. It occurs quite frequently that factors are defined by tests which also have some variance on another factor. This merely indicates that the particular test in question is not univocal; it does not mean that the factors themselves are not orthogonal. Indeed, the term 'factor analytical artifact' in Hamilton's argument is meaningless as it stands. All statistical and methodological analyses are 'artifacts', i.e. 'a product of human art and workmanship'; it is difficult to see how they could be anything else.

Another statistical curiosity is Hamilton's argument that 'the so-called abnormally high score of Hildebrand's original control group is probably no more than 1 s.d. above the mean for tested normal control groups'. Anyone finding that a group to be used as an average control group for the purpose of intelligence testing had a mean i.q. in excess of 116 would presumably conclude that something odd had happened to his sampling, and would be justified in substituting a group with a mean nearer 100. Why this should not be permissible here, where the derivation of the group itself strongly suggested a somewhat psychopathic-extraverted character, is not quite clear to me. It might be argued that Hildebrand ought to have shown more circumspection in finding his control group; that seems to be a reasonable suggestion. It does not seem reasonable to suggest that a mistake, once made and clearly recognized, should never be made good, particularly when, as in this instance, objective evidence is available about the non-representative nature of the group concerned. This can hardly be called an *ad hoc* argument in view of the fact that a highly significant difference is found in R scores between the sample presumed to be normal, and the known British population values. Having admitted on one page that Hildebrand's normal control subjects were about 1 s.d. more extraverted (on the R scale) than large random samples in which the test was standardized, Hamilton can hardly be serious in saying on the next page that 'a comparison of Hildebrand's normal controls with other control subjects tested on the same scale does not support Eysenck's and Hildebrand's special plea that Hildebrand's normal control subjects were abnormally extraverted'. A 'special plea' can seldom have had such excellent support!

Hamilton goes on to attack the use of the Guilford R scale, and of the Extraversion Scale of the Maudsley Personality Inventory, based in part on the R scale, as measures of extraversion. His argument confuses two quite distinct points, and as it is very important that they should be kept clear, I shall discuss this point in detail. Jung's hypothesis states that 'much the most frequent neurotic disorder of the extraverted type is hysteria . . .'. On the other hand, speaking of the introvert, he maintains that 'his typical neurotic disorder is psychasthenia', or as we would nowadays say, dysthymia. Two deductions may be made from these statements. The first one is that *on any good measure of extraversion, dysthymics should have lower scores than hysterics or psychopaths*. (The term 'hysterics', as used by psychiatrists at the time when Jung wrote his famous book, included people whom we would now call psychopaths; this will be clear from the quotations I have given in chapter 6 of my book.) This is absolutely essential, and indeed has been found to be true by Hildebrand of the R scale. It has also been found to be true of the M.P.I. Extraversion Scale in a study by Sigal, Star and Franks, which Hamilton curiously enough quotes as disproving the value of this scale.\* Others, such as Jensen and Claridge, have obtained similar results, and when all the available data are summed it appears that dysthymics have a mean score of 16, hysterics one of 25 and psychopaths one of 30, on the extraversion scale. The differences between the dysthymics and the other two groups are fully significant at the 1% level. These results provide strong support for Jung's hypothesis and for the adequacy of the scale in question.

The position is slightly more complicated when we consider the position of normal

\* Hamilton bases his view on a 'personal communication', not on published evidence. The actual scores obtained in this study were 21, 24 and 30 for dysthymics, hysterics and psychopaths. He does not explain how results so clearly in line with prediction can 'establish that the E scale of the M.P.I. is not a valid measure of extraversion'.

groups in relation to the neurotic ones. My own interpretation of Jung's hypothesis had always been that dysthymics would be more introverted, hysterics and psychopaths more extraverted, than a randomly selected normal group. The data available now confirm this prediction with respect to dysthymics and psychopaths, but not with respect to hysterics, who are only very slightly more extraverted than normals, whose mean score is approximately 24. This failure of the hypothesis may be linked with a curious feature relating to the intercorrelation of neuroticism and extraversion in our various groups. In normal samples this correlation tends to be around  $-0.1$ , whereas in neurotic groups it rises to  $-0.4$ . This cannot be accounted for in terms of selection. I thought at first that possibly the most neurotic and most extraverted groups might not be found in mental hospitals at all, but perhaps in prisons. However, studies with recidivist and other prison populations have shown that these have scores of both neuroticism and extraversion very similar to the scores obtained by hysterics. Furthermore, when we compared the correlations obtained from subgroups of our normal sample, selected for high and low neuroticism respectively, we found the same phenomena, i.e. zero correlations for the group which is low on neuroticism and substantial negative correlations for the group high on neuroticism. These relations are definitely extra-chance; they have been found quite independently by American investigators using the M.P.I. The explanation may be in terms of 'response set', or in terms of a multiplicative effect of emotionality and conditioning.

This problem is being actively pursued at the moment; fortunately its solution is of no particular importance in relation to Hamilton's criticism because the use of the R scale or the M.P.I. as a measure of extraversion is not dependent on the precise position of the normal group with respect to various clinical groups, but rather on the relative positions of the different clinical groups. Hamilton, in his discussion, mixes up these two points and is thus enabled to reach a conclusion which is not in accordance with the facts.

One further point should be mentioned. He complains that 'the overlap of all the data over the groups of controls, hysterics and dysthymics, is considerable'. I find it difficult to see what else one could have expected. In *The Scientific Study of Personality*, I have given a brief review of experiments into the reliability of psychiatric diagnoses, showing it to be rather low. When criterion reliabilities are well below 0.5, it is impossible even for a perfect instrument to give high correlations with the criterion, or to avoid 'considerable overlap' between the groups. It is because of this unreliability of psychiatric diagnosis that I have preferred, in working with neurotics, to use a double selection procedure by combining diagnosis and questionnaire score. Hamilton's criticism of this procedure is based on his belief that the questionnaire does not correlate with clinical diagnosis in the predicted manner. As this belief has been shown to be false, this criticism also falls to the ground.

We must now turn to Hamilton's discussion of my 'theoretical assumptions'. Having mis-stated my typological postulate (by leaving out all mention of excitatory potentials, which form an important part of it), he goes on to say that 'it seems that in these postulates untested assumptions, hypotheses, and molar and molecular concepts are inextricably mixed'. Later on he claims that 'the basic assumptions underlying the postulates were critically examined'. All one can say in answer to this is that Hamilton quite misconceives the function of postulates in scientific theory. A scientist is free to advance any postulate he wishes without having to give any reasons, without having to justify

his assumptions or state his grounds for holding the postulate. There are only two restrictions on postulate making. In the first place the postulate must be specific enough to give rise to testable deductions or theorems, and in the second place these deductions, when tested, must not give rise to results incompatible with the postulate. As Hull points out in his *Principles of Behaviour*, 'scientific theory reaches belief in its postulates to a considerable extent through direct or observational evidence of the soundness of its theorems'. The whole discussion of scientific method given by Hull is apposite to Hamilton's critique because time and time again Hamilton complains that a particular test of my theory does not test the postulate *directly*. This, of course, is quite true; as Hull points out, such direct testing is impossible. All that we can do is test deductions and show that the 'empirical verification of theorems indirectly substantiates postulates'. Hamilton's failure to bear in mind these elementary principles of scientific methodology makes his discussion less valuable than it might otherwise be.

In turning from the postulates to the theorems and their verification, Hamilton rather arbitrarily limits his discussion to a few, while neglecting a wide range of other studies. This is an important point because the support which a postulate receives from experimentation is a direct function of the number of theorems tested. Furthermore, in dealing with those few theorems which he does discuss, Hamilton again arbitrarily limits this discussion to a few experiments. This, again, is not a useful method of procedure, because one's faith in the verification of a theorem rests in part on the number of studies successfully testing this particular deduction.

However, worst of all is Hamilton's tendency to misquote the research papers which he cites, and to give quite erroneous impressions on matters of fact. Thus he maintains that 'the graphs presented by Eysenck and Franks indicate that at the beginning of the conditioning experiment there was already a large difference in strength of c.r. acquisition between dysthymics and hysterics. It is held here that the data from the experimental groups are comparable only if they start from a common base-line, or if comparable points of origin are first obtained.' Even the most cursory reading of Franks's procedure would have shown Hamilton that exceptional care was in fact taken to arrive at a common base-line for the different groups, and that this endeavour was entirely successful. Hamilton's error arises from the fact that in the plotted curves the first point is a measure of the amount of conditioning on the first *test trial*. This test trial was preceded by several *conditioning trials*, and the superiority of the dysthymic group on the first test trial was precisely in line with prediction. Such carelessness in a 'methodological critique' is unusual; it becomes inexcusable when it is realized that this point was clarified by a letter sent in reply to an earlier draft of the paper.

Equally reprehensible is Hamilton's habit of making statements regarding the lack of availability of certain data when these data are in fact available. Thus, in connexion with Franks's conditioning experiment, he states that 'the hypothesis required the exclusion of the alternative hypothesis that conditionability of the eyeblink response is related to degree of neurosis or neuroticism'. He goes on to say that 'no unidimensional measures of neuroticism were available to Franks. No conclusions with regard to the relationship between conditionability and degree of neurosis or neuroticism can therefore be reached.' That this statement is simply not true, can be checked by anyone who will look at fig. 2, on p. 124, of volume 48, of *The British Journal of Psychology*, or at Franks's statement of results on p. 147, of volume 52, of *The Journal of Abnormal and Social*

*Psychology*: 'When the normals are compared with the neurotics as a combined group, there are no significant differences in the number of C.R.'s produced.' Where facts are asserted or denied in this manner in direct contradiction to the actual sources quoted, it would be a task of supererogation to enter into any more detailed argumentation.

It may be worth while, however, to mention one further point. Hamilton discusses the problem of using one-tail tests of significance, and says: 'it would seem that two-tail tests of significance are used almost arbitrarily depending on the mere technicality of hypothesis formation. . . there would appear to be as much (or as little?) justification for linking by hypothesis conditionability and introversion, hysteria and "Rhathymia", as for linking intelligence and conditionability'. The whole argument suggests an unusual degree of confusion. If a particular postulate generates a theorem, and that theorem is tested experimentally, then the use of a one-tail test for the estimate of significance is obligatory. The general postulate with which we are concerned generates a theorem that normal introverts should condition better than normal extraverts, and that dysthymics should condition better than hysterics. Although not required to achieve adequate significance, the one-tail test is the correct one to use. On the other hand, there is no theory linking intelligence with conditioning, and an overwhelming amount of evidence from the literature suggesting an absence of any relationship. This distinction would seem to be so obvious as not to require mention if it were not for the fact that Hamilton has raised this point.

Apart from conditioning, the only other prediction discussed in detail by Hamilton relates to satiation. I had postulated a functional identity between reactive inhibition and satiation, the similarity of which two functions Köhler himself had drawn attention to, and which had independently been remarked on by Duncan, who discusses in very great detail the remarkable experimental similarities obtaining between these two phenomena. Hamilton's comments are: 'In general it would seem that a theory is being presented that requires a reasonably precise definition of its parameters which would allow the equating of inhibition and satiation, but none is offered. . . the identity of reactive inhibition, conceived as a variant of cortical conductivity by Eysenck, and satiation as defined by Köhler, remains an assumption for which there is little positive evidence.' Hamilton does not mention the very careful attempt of Duncan, quoted *in extenso* in my book, to establish experimental similarities, and he again contravenes the rules of scientific logic which allow us to formulate any kind of postulate we like provided it generates testable deductions which are then experimentally verified. The evidence here is less clear-cut than in the case of conditioning. Several investigations have verified my original demonstration of the relationship between kinaesthetic figural after-effects and extraversion, although Broadbent has recently reported data suggesting an alternative explanation. Results with respect to visual satiation phenomena have failed to support the prediction. Experimentally the task of testing the theorem is quite a complicated one, particularly as such factors as eye movements interfere in the direct measurement of satiation in the visual sector. We have recently succeeded in eliminating this difficulty and the conditions for performing a proper test of the theorem in the visual area are now available.

Near the end of his paper Hamilton turns to another theorem relating reminiscence effects to extraversion. His treatment of the results there is in many ways typical of his general attitude. The predicted relationship has now been found by a number of different

investigators both in this department and elsewhere, and in view of the low *a priori* likelihood of discovering such relationship one might have expected an impartial reviewer to consider this as evidence favourable for the theory under investigation. Hamilton has this to say: 'The relationship postulated . . . has now been shown to be only very limited and slight in terms of correlations for normal extraverts, while a relationship between neurotic introversion-extraversion and reminiscence has not been found.' For the second part of this evaluation Hamilton refers the reader to a footnote on p. 254 of my book. This footnote does not refer to neurotic introversion-extraversion at all; indeed, neither we nor anybody else have ever carried out reminiscence tests on neurotics of any kind. The footnote does refer to certain time relationships, stressing the point that the expected phenomena are observed after five minutes of practice, but not after 90 seconds. The reader is urged to compare this footnote with Hamilton's statement as a final proof of the total lack of relationship between the writings to which Hamilton refers, and his statements as to what these writings contain. It should also be noted that this error of fact was pointed out to him by letter in connexion with the first draft of his paper, and that nevertheless the same incorrect statement is found in the published article.

It will be noted that I have not taken up all of Hamilton's points. The reason is twofold. In the first place I do not wish to extend this reply to an unreasonable length. In the second place Hamilton's statements bear too little relationship to the facts to make lengthy discussion worthwhile. Furthermore, most of these points have already been discussed in detail in the book itself. The reader who is in any doubt as to whether a given criticism is well taken or not is advised to read Hamilton's presentation of what my colleagues or I said or did in conjunction with what our books and papers show to have been really said and done by us. It will, I think, be found that the relationship is too tenuous to justify any more lengthy reply.

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