SCIENTIFIC METHODOLOGY AND '*THE DYNAMICS OF ANXIETY AND HYSTERIA*'

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In their recent critique of *The Dynamics of Anxiety and Hysteria* Eysenck (1957), Storms & Sigal (1958) have made a large number of comments and criticisms. I do not intend in this brief reply to take up all the points they mention. In the first place, many of them have already been dealt with elsewhere (Eysenck, 1958*a–d*). In the second place, anyone replying to a critique of his work is faced with an awkward choice which appears to be inescapable. If he wishes to answer each point separately, his reply would become impossibly long as he would have to recapitulate what he said or did in the first place, and what the criticism was, before being able to answer it. The alternative method of answering only what would appear to be the main points easily lays him open to the charge of evasion. I have chosen the second course, namely to discuss a few major points rather thoroughly, in preference to discussing every point raised in the short and unsatisfactory manner necessitated by space requirements. In the third place, it seems to me that Storms and Sigal go wrong not so much in point of detail (although here they also make some curious mistakes and errors), but rather by having an altogether unrealistic notion of scientific methodology in general, and the question of proof in particular. Such points can best be brought out in a more general discussion.

To begin, I will take a point made early on by Storms and Sigal. They say that my statement about individual differences in excitation and inhibition being properties of the physical structures involved in making stimulus-response connexions, 'implies that measures of personality dimensions based on these differences should be relatively stable over time within the same individuals. However, no longitudinal studies of stability or shift of his dimensions have been conducted'. It is difficult to see quite what it is that Storms and Sigal are criticizing. They have stated my hypothesis correctly and they have also stated the deduction from it in a manner which is at least not incorrect. This is one of many hundreds of deductions which can be made from my theory, and it undoubtedly deserves to be taken up and tested. That no such test has hitherto been performed is surely no criticism of the theory, which in any case was put forward only a relatively short time ago, thus making it quite impossible for any proper longitudinal studies to have been executed. What Storms and Sigal appear to imply is that no theory should be put forward until all the possible deductions from it have been tested. This, of course, is equivalent to saying that no theory should ever be put forward, as at the beginning no, or only very few deductions, have usually been verified, and as at no time will more than an infinitesimally small proportion of all possible deductions have been submitted to scrutiny. Storms and Sigal mention several other deductions which they consider to follow from my theory, and at various points take me to task for not having tested these deductions; this appears to be a rather unrealistic type of critique. In spite of the large-scale efforts that have gone into the testing of Hull's postulates, only a very small number of deductions have in fact been submitted to the experimental test. To pick out one or two of those not yet verified, and use the fact that they had not yet been tested to discredit the Hullian system would quite rightly be regarded as absurd.

Nor is this all. The history of science shows that it is often possible for a vital deduction from a scientific theory to be apparently falsified by facts for a long time, although the theory was in fact correct. Thus it follows from Copernicus's heliocentric theory that parallax should be observable among the stars, yet for hundreds of years after his death astronomers failed to discover any evidence of parallax. The *ad hoc* hypothesis that the stars might be so far away as to make the observation of parallax with existing instruments impossible was duly laughed out of court by his opponents. Yet both theory and explanation of contrary findings turned out to be correct. Thus it is by no means essential for a scientific theory at its point of inception to predict all phenomena observable at the time; certain matters may have to be left for further study because methods for correctly evaluating them are not yet available. This may, for example, be true of visual figural after-effects. My theory predicts that these should be greater for extraverts than for introverts, *provided both receive an equal amount of stimulation*. The theory would also seem to predict, however, that visual fixation for any lengthy period of time (which is an essential part of ensuring equal stimulation) will be more difficult to maintain for extraverts than for introverts. Thus at present, we appear to have a deduction here which is difficult to submit to any proper test. It may be possible in the future to do so by paralysing the eye muscles, by taking a film record of eye movements, or by the short-time exposure methods at present being experimented with by H. Holland in my laboratory. Until a proper test of the hypothesis is carried out, it is impossible to evaluate the prediction scientifically. Storms and Sigal make much play with Nichols's (1955) failure to observe differences in visual figural after-effects, but as he failed to control for eye movements it is difficult to see how his results can be conclusive one way or another.

When it comes to the verification of deductions actually attempted by various experimenters, Storms and Sigal commit a further methodological error. To put it briefly, they argue as if *failure to disprove the null hypothesis* were equivalent to *proving the null hypothesis*. They regard results which are in the predicted direction, but which fail to be significant, as actual disproof of the hypothesis in question. This is never admissible, and the only conclusion which can be drawn is that the issue is still open. However, even this conclusion ceases to be very meaningful when the number of cases used is so small as to make the attainment of a significant correlation almost impossible. As an example, a study by Sigal, Star and Franks (1958) may be mentioned, in which they reported a failure to find significant differences between certain neurotic groups on a test of extraversion. The number of psychopaths tested by them, to give but one example, was only eight, and the likelihood of a group of that size being discriminated significantly from other groups, all selected on the basis of highly unreliable psychiatric ratings, is rather small.

A proper test of deduction from a theory must ensure that the number of cases is large enough for a negative result to have some meaning. Storms and Sigal never mention the fact that particularly in correlational research the smallness of the groups employed heavily loads the dice against significant positive findings, and largely invalidates the failure to obtain significantly positive results to be significant as evidence against the theory.

Even more curious is a related complaint by Storms and Sigal, to wit, that there is, on the part of the writer, 'frequent failure to mention relevant test or population characteristics when making a case for the practical utility of his measures or methods. There is notably a failure to mention test-retest reliabilities, which are known to be very low for some tests such as eye-blink conditioning.' It is, of course, true that in my work I have been more concerned with validity than with reliability, particularly as with many of the measures, such as eye-blink conditioning, it is impossible to calculate any very meaningful test-retest reliability. However, granted that the test is valid (and I shall return to this point in a minute), it is inevitable that the test must also be reliable. Storms and Sigal are therefore faced with a dilemma. If the test is valid but also unreliable, then its true validity when corrected for attenuation must be very high indeed. It is in fact an axiom in psychometrics that the test to go for is that which is valid but has a relatively low reliability, the reason being, of course, that it is reasonably easy to increase reliability, and that any such increase is reflected in increased validity. The only alternative open to Storm and Sigal would be to admit that high validity implies high reliability, and that they were mistaken in the criticism.

We come now to the question of the validity of the conditioning technique. It will be remembered that Franks (1956) used both eye-blink conditioning and PGR conditioning measures on groups of hysterics and dysthymics, finding a total misclassification of only 15 % with the one measure, and 30 % with the other. He also found that 'when a double criterion is used... then the two groups may be separated with no misclassification whatsoever.' What do Storms and Sigal have to say in criticism of this work? Their main criticism appears to be related to initial blink sensitivity, and to a possible higher spontaneous blink rate among dysthymics. These criticisms are difficult to inderstand. Franks's selection procedure involved giving each subject 'three tone stimuli, followed by three air puff stimuli (not paired with the CS) and then three more tone stimuli. Only S's who did not give PGR or eye-blink responses to the last three tone stimuli were included in the conditioning study. The purposes of these trials were to eliminate those S's who showed any evidence of pseudo-conditioning or original sensitivity to the tone.' It might be thought that Franks was in fact loading the dice against the hypothesis under investigation, because pseudo-conditioning has in fact been shown (Wickens & Wickens, 1942) to be identical in principle with true conditioning, so that by his method Franks would eliminate the most readily conditioned subjects, i.e. dysthymics according to the hypothesis. However, this is not what Storms and Sigal protest against. They say: 'All subjects and therefore all groups had initial scores of zero blinks. However, these zero scores were arbitrarily forced to be so. It may be that more persons of one group than of the other would blink at least once in another series of three tone presentations. It should also be pointed out that the tone trials previous to conditioning should have consisted of eighteen presentations in order to provide comparability to the test series scores'. These requirements are quite arbitrary and quite unlike anything that has ever been done by other workers in this field. An investigator has to decide at which point safeguards and controls against a particular artifact are sufficient, and he is usually guided in this by what is known about the phenomena and by common practice. If Franks had given eighteen presentations of the tone alone, as Storms and Sigal demand, they might have advanced exactly the same argument as before and demanded 36 or 200 or 5000 presentations. Clearly they are going beyond the limits of rational criticism in this, and making it quite impossible for any study to be acceptable in this field.

Much the same must be said about another suggestion of theirs, namely, that 'at least some of the difference between hysterics and dysthymics in total number of blinks during the test series could be due to a higher spontaneous blink rate amongst dysthymics.' Such evidence as is available from the literature (Meyer, Bahrick and Fitts, 1953) does not suggest any positive relationship between blink scores and data from personality inventories, and Franks himself (1956) found no significant difference between hysterics and dysthymics over a 1-minute period following the conditioning and extinction trials, a test which in fact loads the dice in favour of the Storms-Sigal hypothesis because of the possibility of reminiscence effects.*

* I have not dealt with the Storms and Sigal analysis of variance of rate of growth of conditioning. It is not clear to me what precisely it is they are trying to prove or disprove. The undoubtedly significant differences between extraverts and introverts in total number of conditioned responses, as well as the significant correlation between introversion and conditioning, are sufficient to verify the deduction from my general theory.

It is interesting to contrast the treatment Storms and Sigal give to these conditioning data with their treatment of data relating to visual satiation. They dismiss the very highly significant results of Franks (1956) on the basis of criticisms which are far-fetched and unrelated to current practices of experts in this field. Conversely, they severely criticize the writer for refusing to accept Nichols's (1955) equivocal data on visual satiation because of the very serious doubt relating to the question of fixation. They say that 'no evidence was presented to support this as a valid objection to the acceptance of his (Nichols's) findings'. Again, therefore, Storms and Sigal are attempting to have it both ways. Data favourable to the writer's hypothesis can be dismissed on the basis of highly speculative and unlikely objections, and without any evidence being presented 'to support this as a valid objection'. On the other hand, the writer's refusal to consider data subject to very serious criticism as being crucial to his hypothesis is merely greeted with demands for direct evidence that the suggested objection is in fact true. This certainly is an unusual request to make; in scientific criticism it is usually considered sufficient to raise a reasonable doubt, particularly when this is in line with theoretical consideration, to leave the question *sub judice*; it is not considered essential that the critic himself should provide direct evidence that his objection can in fact be sustained. If this is indeed what Storms and Sigal are suggesting, then clearly most of their criticisms would have to be withdrawn as having no factual basis or support.*

* They fail to mention that the writer's criticism of Nichols's failure to control for fixation differences has also been extended by him to such phenomena as after-effects of rotating spirals (Eysenck, 1957) p. 154, where the reported results are *favourable* and the criticism would invalidate this support of the writer's theories. It is thus far from being made simply *ad hoc*, and in order to explain away a contrary result. Storms and Sigal do not mention this important fact, but prefer to pretend that this criticism was specific to an inconvenient finding.

One of the most important and, if true, damaging criticisms of Storms and Sigal relates to the validity of the Maudsley Personality Inventory. This questionaire was constructed to measure neuroticism and extraversion, and a considerable amount of work has been done in relation to it (Eysenck, 1956, 1959). Of particular interest at the moment is the relationship of the extraversion scale to different psychiatric groups. The essential analogy here stems from Jung's hypothesis and may be put in the following form: Extraversion/Hysteria and Psychopathy = Introversion/Dysthymia. In other words, on the extraversion-introversion continuum dysthymics will be found towards the introverted side of the other groups, hysterics and psychopaths towards the extraverted side of the dysthymics. This is in fact what Sigal *et al.* (1958) found in a study specially carried out to test this hypothesis, although because of the small number of cases not all their differences were significant. Dysthymics had the highest introversion scores, psychopaths the highest extraversion scores, and hysterics were intermediate with a mean score on the extraversion scale higher than that of the dysthymics. The writer has collected records from a large variety of normal and neurotic subjects, and these have completely borne out the findings of Sigal *et al.*, all the differences being fully significant statistically (Eysenck, 1958*e*). The prediction about the relative position of the dysthymics at the introverted end, and the other two groups towards the extraverted end, is therefore fully confirmed. It is interesting, but not relevant, to add that normal groups, criminal groups and patients suffering from psychosomatic disorders all had scores on the extraversion scale very similar to those obtained by the hysterics. The interpretation of this finding is open to discussion. It is possible that Jung's hypothesis regarding the high degree of extraversion of hysterics may have been mistaken; they may be *relatively* more extraverted than dysthymics, but not *absolutely* so in comparison with the normal population. This would be an interesting finding if true, but it in no way affects the usefulness of the M.P.I. as a measure of relative extraversion in normal or neurotic groups. In any case, other hypotheses are tenable regarding this finding, but will not be discussed here as they are not really relevant to the point at issue (Eysenck, 1958*f*). The interesting point in this connexion is the attempt by Storms and Sigal to give the impression that the M.P.I. is not a good measure of the hysteric-dysthymic dichotomy, and has been shown not to be so. This is untrue, and Storms and Sigal have treated this point in a very cavalier fashion in view of the fact that they were fully aware of the points raised in the last few paragraphs.

This failure to mention evidence and arguments contrary to their view (*suppressio veri*), is one which recurs throughout their paper and is, in view of their criticism of the writer on precisely this point, a curious verification of the Freudian mechanism of projection. One or two further examples of this tendency must suffice. One of their first criticisms is the writer's 'failure to cross-validate before considering a measure useful or adequate. An example of the relevence of this is the Eysenck & Prell (1951) study of the inheritance of neuroticism. In this study, the only measures with appreciable loadings on neuroticism were tests of body sway suggestibility, static ataxia and autokinetic movement. Only the first two of these tests have been used in later relevant research, and they have not held up as discriminators of neurotics from normals' (S. B. G. Eysenck, 1955; Hildebrand, 1958). The reader would not guess from this account that a special study was done by Eysenck & Prell (1952) demonstrating highly significant differences between normal children and neurotic children with the use of the factor scores on neuroticism derived from the tests so slightingly referred to by Storms and Sigal. Nor would one gather from their account that the work of Ingham (1954, 1955), Connor (1952), Cattell (1957) and many other authors has in fact strongly supported the relationship between neuroticism on the one hand, and static ataxia and body sway suggestibility on the other. By suppressing the relevant evidence, Storms and Sigal succeed in giving an impression which is quite contrary to fact.*

Much the same is true of their treatment of reminiscence. They criticize the writer for quoting the Treadwell study (1956) as independent duplication, but make no mention of the fact that other duplications, such as that by Star (1957), exist and are familiar to them. In addition to this, they confuse the issue in a number of ways which are of some interest to students of the uses and abuses of communication. Storms and Sigal state that 'Eysenck found a correlation of 0·29 between the E scale of the M.P.I. and the first reminiscence score. This correlation, significant at the two per cent level, he reports in the book; the insignificant correlation of 0·10 between the E scale and the second reminiscence score is not reported.' The impression this may convey to the reader is undoubtedly that there has been some sharp practice here. The reader will not realize from their account that the theory under investigation actually predicted that the second reminiscence score would correlate lower than the first with extraversion, so that this additional finding could in fact have been quoted as further verification of the theory. The failure to do so, as well as the failure to repeat in detail findings reported previously in articles, is due simply to the fact that books have a finite length, and that it would be absurd to repeat all details once these are available in published form elsewhere.

Throughout their review, Storms and Sigal attempt to give the impression that the writer

* Cattell (1957, p. 252) alone quotes *seven* independent experiments of his own in which body sway suggestibility appears as part of his neuroticism factor; there is no other test of his battery which performs as consistently as this. Storms and Sigal may have been ignorant of these facts, in which case they can hardly be considered as well qualified to write a critique of this kind; if they were aware of the facts, but misrepresented them, an even harsher verdict would have to be passed on their qualification for the task they set themselves.

has selectively omitted evidence contrary to his hypotheses, while adducing all the available evidence in favour. The truth of the matter, of course, is that *The Dynamics of Anxiety and Hysteria* was not written as a comprehensive text-book, and that much material both *pro* and *con* had to be omitted for reasons of length. Storms and Sigal always draw attention to omissions of material which might be considered as being *contrary* to the writer's hypotheses, whereas they never draw attention to similar omissions of material *favourable* to the writer's hypotheses such as, for instance, the work of Nichols (1955) on thresholds for apparent movement, which they mention near the end of their paper. This work supports the writer's theory quite strongly, but was not mentioned in *The Dynamics of Anxiety and Hysteria.* No hint of this omission is given by Storms and Sigal, who have noted every other omission which could possibly be construed as being concerned with material not supporting the writer's hypothesis. Such selectivity is fortunately unusual in scientific criticism.

Perhaps a further example of this tendency on the part of Storms and Sigal to make insinuations having little point to them may be in order. In dealing with one of my drug studies in which I tested certain predictions relating stimulant and depressant drugs to pursuit rotor learning curves, they say: 'However, he does not provide data for reminiscence scores, which could easily have been obtained. Inspection of the curves he does provide indicate that the introvertizing drug (dexedrine) produced more reminiscence than the extravertizing drug (amytal). This part of the data is more directly relevant to the theory, though inconsistent with it, than the parts discussed by Eysenck. Yet he fails to report the result or to indicate whether it is statistically significant.' The statistical test was carried out and failed to indicate any difference between the groups. It was not reported for the simple reason that no prediction can in fact be made from my drug postulate on reminiscence effects where both pre- and post-rest testing is carried out while the subject is still under the influence of the drug. Storms and Sigal nowhere state precisely how they would derive any such prediction as theirs, and consequently it is difficult to argue the point which appears entirely notional. On the Kimble hypothesis of reminiscence, inhibition rises until it equals drive; thereafter certain involuntary rest pauses are enforced which inhibit work. Drugs may affect the speed of generation of inhibition, or the drive level, or the speed of dissipation, or any combination of these variables; they may also affect $_{S}H_{R}$. My theory does not permit any choice between these possibilities, which have to be investigated empirically; consequently no prediction can be made about reminiscence effects. The only statement that can be made is the one actually tested, namely, that performance should differentiate significantly between drug effects. This is so because the facts would be identical regardless of the particular variable affected by the drug. This is not true of the reminiscence score, and it is for this reason that no prediction was made. A brief statement of this point will be found on p. 238 of '*Dynamics of Anxiety and Hysteria*'; it is not mentioned by Storms and Sigal. Storms and Sigal do not document their views that reminiscence effects are more directly relevant to my theory than are the effects reported, and that they are inconsistent with it; they rest content with the simple insinuation that the data were supressed because they were inconsistent with theory. This is a good example of *suggestio falsi*; the alert reader will find many others in the original paper by Storms and Sigal.

One of these examples at least must be mentioned before closing. As no. 7 of their list of 'general deficiencies', Storms and Sigal say that 'when an article is quoted or a figure reproduced items which are not fully consistent with Eysenck's hypotheses may be altered or omitted.' The reader may be interested in looking at one of the instances given by Storms and Sigal. In connexion with the discussion of conditioning, Storms and Sigal say: 'It may be noted that the graph presented by Eysenck contains certain errors which make the groups appear to differ more than was actually found by Franks.' The figure in question was photographed from Franks' (1956) article for reproduction, but was not found satisfactory by the publisher, who had it redrawn by his own artist. In doing so, the artist made two minute and quite unimportant errors. One was in the direction of increasing the apparent differentiation of the groups during extinction, the other in the opposite direction, during acquisition of the conditioned response. Neither of these two minor errors affects the issue in the slightest, and indeed it takes considerable time in checking the figures to note any differences at all. It is characteristic of Storms and Sigal that they mention the one error but not the other. The reader is invited to verify for himself the completely negligible nature of the error in question.

A similar comment applies to the other example given by Storms and Sigal, to wit, the figure presented by the writer of the two factors emerging from the Trouton and Maxwell (1956) study. It is perfectly true that for reasons of legibility and clarity a number of items have been omitted in figure 3 of *The Dynamics of Anxiety and Hysteria*; Storms and Sigal do not mention that a full list of all the items involved was given on a previous page so that the reader could be in no doubt about the facts of the case, and could consult the original publication if interested in any of the missing items.

One more remark may be added relating to the last sentence of Storms and Sigal's review. They say that 'it is clear that at least Eysenck's typological postulate and probably the one about individual differences require considerable modification to cover the available evidence. This necessity is not suggested by the book....' The reader who will have a look at what I have to say on pages 250 to 259 will see that I have no doubt myself about the necessity of considerable modification. As I have pointed out there, a good deal of the difficulty of making precise predictions from personality postulates lies in the backward state of so many of the fields to which these predictions could be applied. Even in the most advanced fields like conditioning, reminiscence and so on, there is much that remains doubtful and debatable; as I point out in my Introduction, theorizing in science is an uncertain business at the best of times, and never more so than in the early stages of development of a science. I have stated quite clearly the purpose of presenting my theory. 'In the first place, it may be regarded as a first feeble attempt to achieve the aims of a properly quantified and rigorous system. In the second place, it may serve to bring together, in one set of generalizations, large numbers of more or less certainly established facts. In the third place, such a system may give rise to predictions whose main interest will be not only the verification of the system they make possible, but also the fact thay they point to areas of investigation which might otherwise have been overlooked.' It will be clear from this quotation that I am far from regarding my postulates as immutable and certain parts of knowledge. They were put forward as aids to further research and as possible generalizations; I shall be quite content if Storms and Sigal are right in saying that they require considerable modification, rather than complete replacement. I would, however, express a doubt as to whether such one-sided, partial, non-factual and inaccurate critiques as that of Storms and Sigal will be of any great assistance in improving either the accuracy or the coverage of the theory in question.

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