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Suggestibility and Hypnosis—an Experimental Analysis

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THEORIES of suggestibility fall into three main classes. The first group of theories is based on certain permanent or semi-permanent characteristics of the person who accepts the suggestion—the suggestee. An explanation may be sought in his neurotic constitution, in his submissive instinct, in his tendency to dissociation, or in his ignorance, credulity, or stupidity.

The second group of theories is based on certain relations obtaining between the suggester and the suggestee. Prestige, for instance, is an often-quoted explanatory concept in this connexion; rapport and transference are two other relations which have frequently been suggested. Sometimes an explanatory concept from this second group of theories is combined with one taken from the first group; the relation of prestige may be combined with the submissive instinct, for instance, as was done in McDougall's theory (1926), and so on.

A third type, the ideomotor theory, has lost much of the hold it once gained through William James' brilliant advocacy (1902). According to this theory, the idea of a motor act tends to be followed directly and without fail by the execution of that act, through some direct, reflex kind of connexion. This view was advanced more particularly by the advocates of the motor theory of thinking, and lost in favour because of the exaggerated claims of that school (Max, 1934; Gibson and McGarvey, 1937).

Only experimentation can decide between the claims of these three groups of theories, and a variety of tests of suggestibility have been used by research workers in this field. Often these tests are simply quantifications of familiar clinical tests; sometimes they are more original, such as Binet's *Progressive Lines* and *Progressive Weights* tests (1900). The question immediately arises as to whether all these tests measure the same thing, i.e. whether tests designed on a *priori* grounds to measure suggestibility actually succeed in doing so, and whether suggestibility is one definite, general mental mechanism which remains constant in any one person, or whether there are different kinds of suggestibility, measured by different kinds of tests.

In a research involving eight standard tests of suggestibility, I showed that these tests fall into two sharply distinguished groups; in other words, that they measured two essentially different kinds or types of suggestibility (1943). These two kinds of suggestibility were called "primary" and "secondary". Secondary suggestibility was exhibited in tests of the Binet kind, i.e. *Progressive Lines* and *Weights*. In these tests, a suggestion is conveyed to the subject that certain weights or lines, which are objectively equal, differ in weight or length, and his suggestibility is measured by the extent to which he shows himself amenable to this suggestion. Both direct, personal suggestion and indirect, impersonal suggestion can be used in this test.

Primary suggestibility was exhibited in tests such as the *Chevreur Pendulum* test, the *Arm Levitation* test, and the *Body Sway* test. In these tests the subject is told, either once or repeatedly, that a pendulum which he is holding will swing to and fro, that the arm which he is holding out sideways will move up or down, or that while he is trying to stand perfectly still, with his eyes closed, he will fall forward or backward. The actual movements resulting from these various suggestions are measured by suitable recording devices, and the maximum amount of movement consequent upon the suggestion is taken as the subject's score on the test. It is this type of suggestibility with which this paper is concerned, because it is related very closely to hypnosis (White, 1930; Jenness, 1933; Hull, 1933). In particular, the *Body Sway* test was subjected to a good deal of experimentation: a person who sways very much in this test, or who falls altogether, will nearly always fall into a hypnotic sleep very easily, while a person who remains entirely unaffected by this test will almost certainly remain unaffected by the hypnotist. The *Body Sway* test, therefore, enables us to answer questions which are of crucial importance for any theory of suggestibility and hypnosis. If, for instance, suggestibility is due to an individual's hysterical constitution, and therefore to his tendency to dissociation, as Janet (1907) maintained when he said: "Suggestibility . . . presents itself only with hystericals, and inversely, all hystericals present this same phenomenon in a higher or lower degree"—then if we test a number of hystericals on the one hand, and a number of people free from any signs of hysteria on the other, the two groups should be differentiated very definitely by the amount of sway they show in response to the suggestion. When I carried out this experiment, thirty non-hysterical men and women were found to be as suggestible as thirty men and women with pronounced conversion hysteria. (A number of cases of dysmnesic hysteria were also tested, but were not

included in the above group because Janet's definition of hysteria would not cover them; they also were found to be no more suggestible than the non-hysterical group.) Other tests of suggestibility were also given to these groups, all of which tended to bear out the conclusion that hysterics are not markedly more suggestible than are non-hysterical neurotics (Eysenck, 1943). Consequently, Janet's theory found no support in this investigation. This conclusion is well in line with other studies which failed to find any correlation between "tendency to dissociation" and suggestibility (Messerschmidt, 1927; Mitchell, 1932).

The next theory to be considered is McDougall's view that suggestibility is connected with the "instinct of submissiveness". If this were correct, women should prove more suggestible than men on the average, because, as he also maintains, women tend to be more submissive on the whole. Fig. 1 shows the distribution of scores on the Body

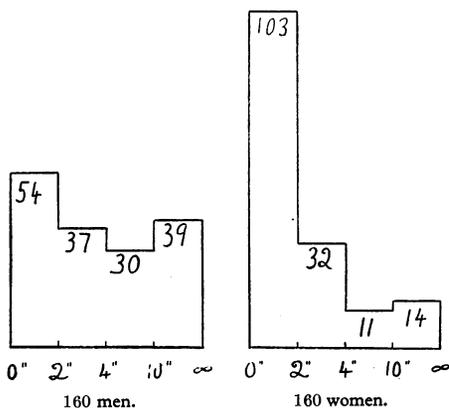


FIG. 1.—Suggestibility scores.

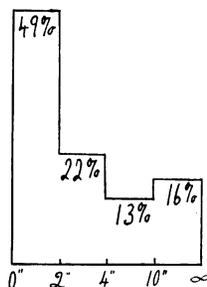


FIG. 2.—Total distribution —320 suggestibility scores.

Sway test of 160 male and 160 female neurotics, tested at Mill Hill Emergency Hospital. (Both the men and the women were very largely army patients.) The base-line of the diagrams is divided into four parts, in accordance with the maximum amount of sway noted. The first division contains cases swaying less than 2 in.; the next division those who swayed between 2 and 4 in. The third division contains the cases swaying between 4 and 10 in., and the last division cases swaying more than 10 in., or falling outright. (Those falling outright are recorded by means of the infinity sign = ∞.) The absolute numbers of cases falling into each of these divisions are given in the diagram.

If we call all those subjects who sway more than 2 in. suggestible (this value is of course arbitrary, and probably slightly underestimates the number of subjects showing suggestibility), we find the following percentages of men and women to be suggestible (Table I).

TABLE I.

160 male neurotics	66% ± 4% S.E.
160 female neurotics	38% ± 4% S.E.
320 neurotics	51% ± 3% S.E.

TABLE II.

	Sway in inches		
	Men	Women	Average
Male suggestor	5.5	3.3	4.4
Female suggestor	4.4	2.9	3.7
Record	5.8	2.8	4.3
Average	5.5	2.9	4.2

These figures show that roughly two-thirds of the men are suggestible, while only one-third of the women are suggestible according to our criterion. This difference is definitely statistically significant and shows that contrary to McDougall's theory *women tend to be less suggestible than men*. A theory which may account for this sex-differentiation will be suggested presently. Of all the 320 patients tested, almost exactly one-half were suggestible. The total distribution of our cases is shown diagrammatically in fig. 2.

Next we may test the various prestige and transference theories, all of which are based fundamentally on the face-to-face relation of experimenter and subject, and usually invoke a reinstatement of infantile attitudes inappropriate to the situation. These attitudes may or may not have a sexual basis, depending on the exact variant of the theory which may be held. If it can be shown that the person and the sex of the experimenter has little influence on the success of the experiment, and may in fact be quite irrelevant, these theories must lose much of their superficial plausibility.

The following experiment was arranged to test this hypothesis. Group A, consisting of 30 men, was tested by me in person. Group B, consisting of 30 men, was tested by a young lady at the hospital. Group C, consisting of 100 men was tested by means of a grammo-

phone record played over a pick-up relay by a nurse or by a patient. A similar programme of testing was arranged for 160 women; 30 were tested by me in person, 30 by the young lady, and 100 by means of the record. This record was made by me, and copies of it are obtainable from Mill Hill Emergency Hospital.

The results of this experiment are set out in figs. 3 and 4, for males and females respec-

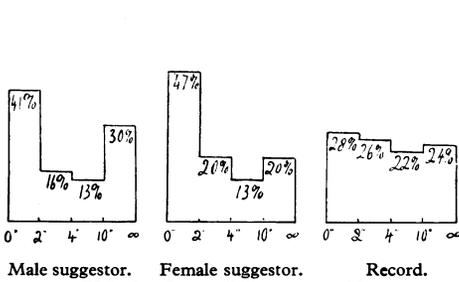


FIG. 3.—Suggestibility scores : Men.

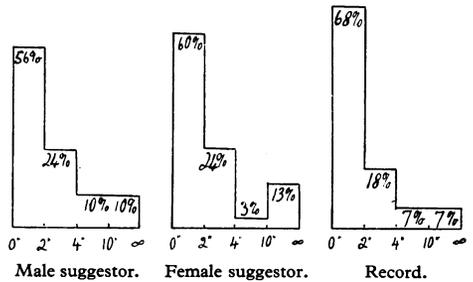


FIG. 4.—Suggestibility scores : Women

tively. It will be noticed that the differences between the three ways of inducing suggestibility are very small; on the average a male suggestor, a female suggestor, and a record produce much the same amount of sway. In Table II are given the data for the various groups, recorded in the form of "average sway in inches"; these figures show clearly that the record on the whole is almost exactly as successful as I am in person, while for both men and women the young lady is slightly less successful.

It should be noted that while for the men the record is more successful than either male or female suggestor in person, the record is less successful for the women than either male or female suggestor. This seems to indicate that while on the whole the personal relationship is not essential for the induction of suggestibility, such relationship is an aid to suggestibility in the case of women. It should also be noted that there are no such intersex effects as the theory of transference would make plausible; the female suggestor is slightly less successful with both the men and the women in roughly the same proportion. This shows that the sex of the experimenter plays little or no part in the induction of suggestibility. On the whole, then, our results tend to argue strongly against any type of prestige or transference theory.

Ignorance, credulity and stupidity have been at times cited as accounting for suggestibility (Janet, 1927). Hull (1933) on the other hand concludes a survey of experiments by stating that the relation between intelligence and suggestibility is positive. Neither view is borne out by the facts. Table III shows the average sway in inches on the Body Sway test of patients who were found to be in Grades I to V on the Matrices Perceptual Test of Intelligence. The very intelligent (Grade I) and the very dull (Grade V) sway on the average only about half as much as do those of average intelligence (Grade III). This curvilinear relationship between intelligence and suggestibility is brought out quite clearly in fig. 5. Again, experimental findings discredit the theory the experiment was designed to test.

Intelligence	Fall in inches	Number tested
Grade I	2.3	28
Grade II	3.9	76
Grade III	4.8	124
Grade IV	4.5	74
Grade V	2.7	18
Average	4.2	320

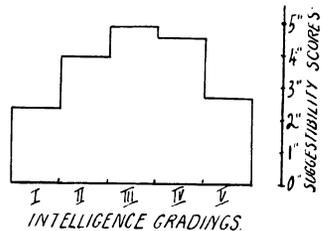


FIG. 5.

The last theory to be tested was the ideomotor theory. According to this theory, there exists a tendency of a certain strength for every motor idea to be carried into execution. It is this tendency which is being measured by such tests as the *Body Sway* test. We can subject this view to an experimental test in the following way. One group of 30 men was tested, and immediately retested. A second group, consisting of 40 men, was tested and retested the next day. A third group of 30 men was tested and retested after four weeks' stay in hospital. Similarly, three groups of altogether 100 women were tested and retested immediately, after one day, and after four weeks. There is no reason to suppose that stay in the hospital would make any difference to the strength of the

ideomotor tendency; on the other hand, it would make a great deal of difference to the attitude and mental health of the patient. If then the retests show that stay in hospital does not influence the retest fall of our subjects, we may regard this result as favouring the ideomotor theory; if there is a marked difference, however, the results would require an alternative explanation.

The results are set out in Tables IV and V. The average fall (in inches) in the original test is given in brackets; also given in each case are the average fall in the retest, the ratio initial test/retest, the average difference between the subjects' sway on the initial test and on the retest (corrected for group trends, i.e. test/retest differences), and lastly this difference expressed as a percentage of the average total sway.

TABLE IV.

	Immediately (4.7") 4.7"	Retests : Men	
		After one day (4.0") 3.6"	After four weeks (5.2") 3.7"
Average fall ...	(4.7") 4.7"	4.0"	5.2"
Test/retest ...	1.0	1.1	1.4
Average diff. ...	1.6"	1.9"	2.0"
Diff. as % of fall	34%	53%	45%

TABLE V.

	Immediately (2.3") 2.2"	Retests : Women	
		After one day (1.8") 1.8"	After four weeks (2.5") 1.2"
Average fall ...	(2.3") 2.2"	1.8"	2.5"
Test/retest ...	1.0	1.0	2.1
Average diff. ...	0.4"	0.3"	0.6"
Diff. as % of fall	17%	17%	33%

The results show (1) that the reliability of the test is affected to some extent, but not as much as might have been expected, by the passage of time, and (2) that the difference in average amount of sway from test to retest is negligible when the test is repeated immediately or after one day, but that it is very considerable after four weeks' stay in hospital. This is true of men and women alike. This result, then, requires some alternative explanation to the ideomotor theory; we must in some way account for the fact that an improvement in the condition of the patient is accompanied (or followed) by a tendency to be less suggestible.

Having shown that none of the theories examined can account for the experimentally obtained evidence, we may pause and consider another body of evidence, viz. introspections of both patients and trained psychologists to whom this test was given. When this is done, it becomes apparent that we must take into account two factors, both of which determine the outcome of our experiment. These two factors are *aptitude* and *attitude* (cf. Eysenck, 1943).

People differ greatly in aptitude. Some subjects do not experience the least urge to fall forward when they are told that they are falling forward; others experience an almost irresistible urge to fall. This aptitude appears to be distributed roughly in accordance with the normal probability curve. But aptitude alone is not sufficient to account for the results of our experiments. Some patients of great aptitude clench their fists, shake their heads vehemently while the suggestion is going on, and may even audibly say: "No, no!" They are thus able to control to some extent their tendency to fall, and may only sway a few inches, or may even sway backwards. Others fall quite easily, without struggling overmuch. But attitude alone, again, is powerless without aptitude. Many patients whose attitude is highly positive are found hardly to sway at all, while others, with a very negative attitude, may fall altogether.

We have, then, the following picture: First, there is a tendency for the idea of falling forward, once it is introduced into the mind, to be carried into execution immediately—in conformity with the theory of ideomotor action. Secondly, this action may be inhibited by the higher brain centres. The outcome of this conflict is measured by our suggestibility score.

It is interesting to review the variety of ways in which the original idea can be implanted in the mind. We have seen that it can be done either by male suggestor, by female suggestor, or by means of a record. It can also be done, with similar effect, by unconscious imitation of someone reaching forward, by imagination of the act of falling, by autosuggestion, and by indirect heterosuggestion. In this last method, two experimenters talk to each other about the patient, who stands by with his eyes closed; they pass remarks such as: "I think he is falling forward, don't you?" and "There he goes—he is falling." It is possible by means of these indirect methods to cause the more suggestible subjects to fall outright.

In all these different ways, however much they may differ from traditional suggestibility experiments, ideas can be implanted in the mind of the subject, and will then seek to be translated into action. Jacobson has provided us with direct experimental proof of this ideomotor connexion (1929, 1932). He measured the action-currents in the muscles of the right arm while his patients were imagining that they were bending that arm, but under instruction *not actually to bend it*. He found definite evidence of action currents which continued as long as the patient continued imagining, and ceased immediately after. He found considerable variation in the strength of the action currents between different people, and concluded that motor impulses are at the basis of our imagery.

If this view be correct, we can explain quite simply the fact that men are more sug-

gestible than women on this test. It has been shown that men show more frequently the motor type of imagery, while women tend to be visiles and audiles (Brittain, 1907). Consequently men should on the whole have a stronger tendency to fall, and assuming that the power to inhibit such tendencies is equally divided between the sexes, men should actually show a greater average fall than women.

There can be little doubt that this tendency to fall is nearly always counterbalanced by an inhibition arising from the higher brain-centres. On questioning the patients after the experiment, it is found that they nearly always try to counteract the suggestion; they "fight against it", as they usually put it. The only ones who do not fight against the suggestion are those who are not at all affected in the first place, i.e. those who show no sign of ideomotor action. They feel no urge to fall, and consequently need not inhibit any movement.

This theory that suggestibility is the resultant of two forces, ideomotor action and inhibition, can be subjected to a crucial experiment. If a number of suggestible and of non-suggestible subjects are given a drug, such as scopolamine hydrobromide, which inhibits the action of the higher brain centres, then on retesting them we should expect two things to happen. In the first place, we should expect that those who were suggestible in the original test would prove even more suggestible now, since the drug has the effect of lessening the inhibitive power of the higher brain centres. Secondly, we should expect that those who in the first place showed no effect of the suggestion would still show no effect, as in the absence of any ideomotor tendency the degree of inhibition is immaterial to the outcome of the experiment. In carrying out this experiment, Baernstein (1929) found that suggestible subjects tended to become significantly more suggestible after being given the drug, while non-suggestible subjects remained unaffected. Consequently, we may consider our theory confirmed in this important corollary.

We can now see why hospital treatment should lead to a reduction in the amount of sway of neurotic soldiers. It is reasonable to assume that treatment will result in an increase of control of the higher brain centres over the motor mechanism—at least in those cases where treatment is partly or wholly successful—and consequently in an increase in the effectiveness of inhibition.

The explanation of the curvilinear relation between intelligence and suggestibility is less obvious. It is reasonable to assume that high intelligence will go, on the average, with more control of the higher brain centres over the motor mechanism. Conversely, we must assume that ideomotor action is less strong in the very dull—though why this should be so is by no means clear. Here we have a wide field where much experimental work could with advantage be done.

If our theory be true that a person's score on the *Body Sway* test is the product of two relatively independent forces, viz. ideomotor action and inhibition, then clearly the test as used at the moment is not a good test. No test which reports only the product of two independent forces, but does not give us any indication as to the relative part played by these forces, can be considered acceptable. Future work, therefore, must be directed towards measuring at least one of these two factors, aptitude and attitude, separately; if we know, for instance, the relative strength of the ideomotor component, as well as the result of the product (ideomotor action times strength of inhibition), then we can determine within very narrow limits of error the other factor, viz. strength of inhibition. It is clearly possible to measure separately the strength of ideomotor action by means of an extension of the Jacobson technique, i.e. of recording muscle currents by means of the string-galvanometer; consequently we may hope in time to achieve independent measurements of the components of *Body Sway* suggestibility.

Thus far I have dealt only with suggestibility. But although I believe that hypnosis can be understood only through its relation with suggestibility, I also believe that we must posit some principle of "progressive" or "cumulative" suggestibility if we want to understand hypnosis. In other words, if we want to transform simple suggestibility into that form of heightened suggestibility which we call hypnosis, we need some kind of mechanism which will "step up", as it were, or amplify this original basic suggestibility. This mechanism, I think, is to be found in the central nervous system, and can be explained with reference to the properties of the synaptic nerve junctures, in conformity with McDougall's well-known theory (1908).

According to this theory, the resistance of a synapse is inversely proportional to the current passing through it. Hypnotic suggestion restricts the field of consciousness by means of the mechanisms which we have discussed, thus directing the whole force of the available nervous energy through a smaller number of nervous channels. This automatically reduces the synaptic resistance in these channels, thus in its turn facilitating the passage of the nervous energy. This reciprocal reinforcement would account satisfactorily for the heightening of suggestibility which is found in hypnosis.

Many psychologists are inimical to any type of physiological theory, because physio-

logical events are even more speculative than are psychological happenings. But as Koehler points out (1942), we can test physiological explanations of psychological mechanisms by deducing certain consequences implied in the premises, and then conducting experiments to prove or disprove these deductions. One deduction which may be made from the premises of McDougall's theory is the following: Under hypnosis, there should be an improvement of various mental functions due to the greater concentration of mental energy in the relevant nerve channels. This improvement should be greatest in the simplest tasks, because there the concentration of energy is greatest due to its damming up in a very small number of channels, and least in very complex tasks, because there the sum total of nervous energy is distributed over a large number of channels, a condition not very different from that of waking life.

When testing this hypothesis experimentally, I found an average improvement in the hypnotic state over the normal of some 30% in thirty different tasks; for very simple tasks, such as tapping, dotting, &c., the improvement was in the neighbourhood of 70%, while for very complex tasks improvement was very small or even non-existent (Eysenck, 1939). In this important corollary, then, we find McDougall's theory confirmed. It should not be difficult to deduce various other corollaries from his premises, and by either confirming or disproving them show the essential correctness or incorrectness of his view.

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Dr. J. A. HADFIELD: Many of the conclusions drawn by Dr. Eysenck are justified, but in other cases the conclusions drawn go beyond what the facts warrant. Taking "transference" as an illustration: the mere fact that a man or woman gives the suggestion does not necessarily imply a transference; it may be quite as impersonal as the gramophone record, which indeed produced the same results. But the experiment does prove something; for, if the suggestions are impersonal, the fact that suggestibility was produced disproves the Freudian theory in so far as it asserts that all suggestion is due to transference. A distinction must be made between suggestion and suggestibility: suggestion is the process of transmission (as in McDougall's definition), whereas suggestibility is the state of mind which makes the subject receptive to suggestion (as emphasized in Freud's definition). In the "transference" test, the suggestion is given, but the lack of suggestibility in the patient may induce indifferent results. An individual may be suggestible to one person and not to another; he may be suggestible to one theme and not another, or he may be only partially suggestible. The same criticism therefore applies to the experiments designed to prove that hysterics are no more suggestible than others. The hysteric may respond to indifferent suggestions like swaying, but may be no more suggestible to these indifferent tests than others; but since the hysteric has a tendency to resort to illness as a means of getting sympathy and an escape from responsibility, a suggestion of illness will be responded to far more readily than in the case of another man in whom it would have no effect, since he does not happen to be suggestible to that subject. The experiment therefore is not a true test of the point at issue.

The tests do prove, however, that the hysteric is not *universally* suggestible to any form of suggestion: he is only hypersensitive to suggestions corresponding with his tendency of mind.

The scope of these tests is very limited, and to claim that they abolish the theories of Janet, McDougall, James and Freud at one fell swoop is to put too great a strain upon our logic. Nor can I bring myself to believe that the theory put forward explains all the many facts of hypnosis, such as its physiological effects like the absence of bleeding during the hypnotic state, the hypersensitivity to time, and post-hypnotic phenomena.

It would be extremely valuable if Dr. Eysenck in his experiments could discover some simple and more certain method of producing deep hypnosis. Not only is hypnosis of great therapeutic value for suggestion, but it is easily the best form of anaesthetic, and under its influence we should be able to make many experiments throwing light on the relations of mental and bodily processes, including the psychosomatic disorders.