

Review Article

The Behavioural Treatment of Hysteria

By JULIAN BIRD

“The starting point of hysteria . . . is a depression, an exhaustion of the higher functions of the encephalon”.
(Janet, 1920)

“The hysteric is a liar who does not admit or recognize his lies”.
(Szasz, 1974)

“You must not imagine that very much has been gained by this (symptomatic cure) for the therapeutics of hysteria.”
(Freud, 1893)

“Cure the symptom (or rather the totality of symptoms . . .) and you have cured the disease”.
(Eysenck & Beech 1971)

Introduction

This review will attempt a description of the common behavioural treatments for hysteria and a critique of the outcome studies done on those treatments.

Accounts of any treatment of hysteria, and reviews of those accounts, are peculiarly prone to bias, as the opening quotations would suggest. These quotations illustrate a modern clash of dogma, but it is noteworthy that even before Charcot hysteria was a favourite battleground for students of human behaviour: the denigration of ‘symptomatic’ treatments (an inherently biased word) merely continues this historical process. Historical dissension among schools is mirrored by the personal dissensions among staff which hysterical patients commonly provoke (Pollack and Battle, 1963).

Behaviour therapy

Behavioural treatment, for the purpose of this review, is taken to mean any psychological management in which the problems, goals, interventions and measures of change can all properly be defined in terms of observable behaviour (including speech), (Bird *et al*, in

preparation). Typically, the therapist focuses on current behaviour itself rather than its distant historical associations: he negotiates clear goals, establishes concrete measures of progress, is explicit in his propositions, questions and feedback to the patient: he encourages deliberate practice of desired changes of behaviour without necessarily waiting for emotional ‘preparation’, and generally strives to be empirical, rather than theory-directed. Systematic exposure for anxiety-linked behaviour, aversion for unwanted appetitive behaviour, and modelling or operant conditioning for deficits of behaviour are widely accepted specific treatment principles.

This description, in which the general principles of explicitness and measurement are more important than ‘special techniques’ and in which empiricism is considered overwhelmingly more important than theory, is increasingly representative of practising behaviourists (Sloane *et al*, 1975; Marks, 1976; Chesser, 1976). Strong arguments have been presented for its being made the basic position of all clinicians (London, 1972).

Behaviour therapy has restricted scope: its practitioners should, and generally do, confine themselves to working with problems where their approach has been shown to be superior in achieving measurable results, and only expand their field of operation after careful experiment. They do not in general seek global explanatory models for psychiatric disorder or make global claims for the relevance of one particular style of treatment. Their ‘area of confidence’ only includes something over 10 per cent of the adult clinical psychiatric population (phobias, obsessions, sexual and habit problems, etc.) Hysterical phenomena would seem to sit on the borders of this area.

Hysteria

Many behaviourists doubt whether the term 'hysteria' is meaningful or useful at all, being not only vague but heavy with unsubstantiated aetiological overtones. For the purpose of this review it is interpreted narrowly. We may hope that, since the clinical problems are similar in all the selected studies, differences of treatment approach will stand out. Furthermore, a clear idea of the natural history of the problems being treated can be kept in mind. Where studies lack adequate controls, numbers or follow-up, a clear knowledge of natural history helps a little to reduce uncertainty about the meaning of results. For these reasons, the review will concentrate on studies that deal either with pseudo-neurological 'conversion symptoms' or with the 'hysteria syndrome' of Briquet and Guze, (Guze, 1970; Templar and Lester, 1974). Crudely summarized, these two entities and their natural histories are as follows:

Conversion symptoms: relatively discrete organic-style symptoms of pseudo-neurological character, such as aphonia, amnesia, astasia, fits, blindness, deafness, etc. They occur in a wide variety of psychiatric and general medical settings and have a very variable natural history; the likelihood of spontaneous remission within two years is probably about 50 per cent (Rachman, 1971).

Hysterical syndrome: relatively diffuse poly-symptomatic disorder, nearly always in women, with a minimum of 25 symptoms spread over a wide range of physical and psychological functions and starting before the age of 35. The natural history is extremely stable: up to 90 per cent will still fit the description six to eight years after presentation.

The treatment of many other phenomena which could also perhaps be called hysteria (Mayou, 1975) is not considered here: for example, the 'psychosomatic disorders' including problems of appetite, menstruation, chronic pain, asthma and so forth are not reviewed despite a very promising behavioural literature (Price, 1974). Similarly excluded is the so-called hysterical personality (shallow emotions, attention seeking, etc.) because of grave doubts about its validity (Mischel, 1968).

Types of study

Objective descriptions of psychological treatments are rare enough: methodologically sound outcome studies of such treatments are even more rare (Sloane *et al*, 1975) and the danger that choices of management in psychiatry will continue to be dictated by the swings of mere fashion is still with us (Tourney, 1967). Novelty, plausibility, and extraneous shifts of social attitude at large continue to be potent forces in the shaping of therapy in any field where hard and lasting results are not easily come by. Behaviour therapy, despite, indeed perhaps because of, its pride in 'objective' evaluation, is not exempt from these forces. Six years ago, leading authorities could say, with justice:

"We do not dispute very seriously [the common criticism] that there do not exist any satisfactory studies of behaviour therapy using adequate numbers . . . proper controls . . . and (proper) methods of evaluation (which are almost always very crude)".

(Eysenck & Beech, 1971)

and yet, at that time, in the U.S.A. at least, behaviour therapy was an already rolling bandwagon. The comment could now be contradicted with fair confidence in respect of the behaviour management of focal anxiety, obsessional rituals, and specific sexual problems. In other problem areas, however, including hysteria, the comment would still hold, not just for behaviour therapy but for many other forms of psychological treatment.

The studies reviewed here are grouped under the two clinical headings (conversion and syndrome) and then sub-divided as follows:

- (a) Group studies with controlled design
- (b) Group studies with no controlled design
- (c) Case studies with controlled design (i.e. sequential conditions including placebo or reversal treatments and or multiple base lines).
- (d) Case studies with no controlled design (anecdotes).

This rank order is not always a reliable guide to the merits of a paper. Many would argue that case studies of controlled design can be as valuable as controlled group studies (Barlow and Hersen, 1973), for reasons which can be summarized as follows:

Generality of findings: individual variations in symptoms and responses can be masked in large group results.

Size of changes: the degree of real-life impact of treatment for an individual can be masked positively or negatively by group results.

Mechanism of changes: specific 'active ingredients' of treatment are more difficult to tease out in group studies, as are the effects of coincidental environment changes.

Studies carrying superior descriptions of the problems, their history, management and follow-up can, even if anecdotal, be as useful (or relatively useless, depending on one's point of view) as controlled studies lacking such descriptions.

Studies that do not either make some formal attempt at control or give unusually good descriptions and follow-up, or both, are excluded. The largest group of studies consists of the single-case anecdotes concerning conversion symptoms: some have become very famous and are also couched in the quasi-scientific language still fashionable among some behaviourists; nevertheless, they remain anecdotes and must invoke the special doubt attached to all anecdotes, i.e. why choose to report this particular case? It can only be said in mitigation that, when and if a chronic and consistent symptom, unresponsive to other treatments, suddenly remits at the time of a specified procedure and fails to re-appear or be replaced during a reasonable follow-up period, then the story should be taken seriously. If large numbers of such cases accumulate, then they justify the investment needed for proper controlled research.

Studies on Conversion Symptoms

Group study with controlled design

Dickes (1974) retrospectively reviewed the records of 16 in-patients who between them had 18 conversion 'symptom complexes' compatible with Guze's criteria. Half the symptoms had been managed by 'traditional approaches' (unspecified) and half by a reasonably well defined behavioural approach, sometimes called operant conditioning, in which rewards and the withdrawal of rewards are used systematically:

in this 'special regime' conventional privileges and diversions can only be acquired by symptomatic improvements. All patients also, however, received 'group psychotherapy' (undefined) and had the supposed benefits of 'ward milieu' (undefined). There was no follow-up beyond discharge (maximum 121 days). Full remission is claimed for 90 per cent of behaviourally managed symptoms and for 45 per cent of those traditionally managed. It is also revealed, however, that shorter histories correlated with good results regardless of type of treatment, and it seems there were more short histories in the behaviourally treated group.

The study has major flaws: treatment allocation by unknown and possibly biased criteria, poor descriptions of both the cases and the managements and minimal follow-up are the more obvious. It amounts, at best, to only the most tentative support for behavioural methods, but must take credit for being the only controlled study available to date.

Group study without controlled design

Carter (1949) gives a lucid, elegant and common-sensical account of 100 cases with well defined conversion symptoms. It is not clear how many, if any, of these were treated collectively, and the study might perhaps be better described as a collection of single cases. His treatments were short and simple: the central strategy was to suggest heavily, directly and immediately, with or without the use of thiopentone, that the symptom would resolve. This suggestion was linked to studious withdrawal of attention from subsequent symptomatic behaviour—a behaviourist might have called this latter process extinction. There was also simple support and practical help with changing the patient's day-to-day environments. Ninety per cent of the patients were successfully followed up for periods of four to six years, at which stage 66 per cent were totally symptom-free, working and 'feeling well': a further 9 per cent were symptomatically improved and also working. Tremors, fits, and vomiting appeared to do less well than other symptoms. Design deficiencies in the study are partly compensated for by the excellence of the descriptions and follow-up. In making the very favourable comparison of his own results

with the well-known psychotherapy series of Luff and Garrod, Carter himself concedes that the shorter and more dramatic histories in his cases, which were mostly of war-time origin, may account for some of the differences. The study strongly suggests that early, brief, and concrete interventions are worth trying and that so-called symptom substitution is a rarity.

Case studies of controlled design

Mumford *et al* (1976) describe the systematic evaluation of several behaviour strategies in a case of coughing, aphonia and social withdrawal. An adequate history is given: the symptoms were chronic (four years) and had not responded to other treatments, including psychotherapy. Punishment (shocks) did not have the predicted effect on cough frequency, which rose, but contingent withdrawal of attention seemed helpful; no reversals were included to check this effect. The aphonia was tackled in several ways, but particularly by shaping (breaking voice production into a set of components and giving instruction and contingent reward for each), plus delayed major rewards (e.g. home visits) for major gains.

These arrangements were reversed separately, and stepwise deteriorations did follow, progress being resumed with renewed treatment. Practical help with work and social life was also given. Major success is reported both symptomatically and in general adjustment, and this continued through the 17 months follow-up. The long history, the clear descriptions, the element of ABAB design and the adequate follow-up make this a very suggestive study. The drawbacks are the lack of independent assessment and the doubts about the timing and impact of the general practical support.

Agras *et al* (1972) examine systematically the effects of direct instruction and increasingly 'expensive' praise on the quality and quantity of walking in a 20-year old woman with a five year history of weak legs. The elegant sequential design, the good descriptions, the long history and the rapid resolution are impressive. The absence of follow-up is a major flaw.

Hersen and his colleagues have pursued their studies of 'astasia-abasia' in a similar manner (Hersen *et al*, 1972; Turner and Hersen, in

press). The studies provide strong support for the general value of instruction and contingent reward, and also suggest a particular additive effect—i.e. removal of reinforcement is not associated with temporary relapse if it has initially been given in combination with explicit instructions. Lack of follow-up is again a major drawback in these studies.

Similar well-designed studies of spasmodic torticollis and its response to instruction, reinforcement and detailed discriminative mechanical feedback are also available (e.g. Bernhardt and Hersen, 1972). This mechanical analysis with feedback and restructuring is becoming a common theme in the better studies of habitual unwanted movements such as tics, grimacing, eye closure, occupational cramps, etc. (e.g. Hersen and Eisler, 1973). The author's own anecdotal experience is similar: furthermore, it seems that complex machinery is not necessary for conducting such a programme. The behavioural literature on 'involuntary movements' is a growing one of mixed quality. This review will not attempt to deal with it adequately, since it could well be argued that, in consideration of the common organic factors in these cases, they should, along with 'psychosomatic' problems, not be labelled 'hysteria'.

Case studies without controlled design (anecdotes)

Sears and Cohen (1933) described the resolution of anaesthesia in a woman's left hand during a complex intervention derived from Pavlovian concepts of conditioning: various stimuli to the affected hand were paired with unpleasant shocks to the normal hand: the lady started eventually to withdraw the normal hand before the shocks arrived and after that began to say that she could feel the 'warning' stimuli. This description has acquired an historical aura and is much quoted in support of behavioural theories; the paper itself, however, is mainly devoted to an academic exploration of the lady's various sensation disorders; other aspects of her history are not detailed; the symptoms had only been present for eight months; other attempts at treatment, if any, are not described, and follow-up was for only six months; there

were no deliberate reversal or placebo conditions or independent measures. It is of interest to note that one of the lady's other symptoms, astereognosis, resolved spontaneously six weeks after the experiments were over. All in all, no serious conclusions can be drawn.

Another 'historic' case (Hilgard and Marquis, 1940) is more impressive: a patient with paralysis and anaesthesia of one arm of six years duration, unresponsive to other treatments, was subjected to a similar, brief, 'conditioning' programme. Mild shocks were given to the affected hand, closely followed by strong shocks to the normal hand: again, after acting on these warnings, sensation was reported in the numb hand. The procedure was then neatly reversed, and withdrawal twitches were established in the affected hand, followed by reports and demonstrations of voluntary power. The patient was followed for two years and neither the original difficulty nor any new one seems to have arisen.

Malmö *et al* (1952) described a similar dramatic result, using a comparable procedure, in a lady with total hysterical deafness. Tones were used as warnings of finger shocks. Avoidance movements quickly appeared, but hearing as such was not conceded until after a road accident the following day. Two years later she was still symptom-free, but it has to be noted that the history was short (six weeks) and that the effects of suggestion and of the road accident are imponderable.

Walton and Black (1959) used systematic reward and punishment in the management of a lady's chronic (seven years) aphonia. The central exercise was to read aloud from a boring book: the better her vocal performance the shorter was the session and vice-versa. The frequency of sessions and the size of audience were progressively increased: some 'learning theory derived' elements were included (rest periods, amphetamines) which now have a somewhat esoteric ring. No separate or systematic evaluations of the various elements was attempted, and the elaborate theoretical discussions do not help in deciding whether the treatments were in themselves effective. The lady became symptom-free and remained so for the 20 months of follow-up, but it seems that

frequent consultations took place during that period.

Brady and Lind (1961) give a most detailed account of a man of 40 with hysterical blindness of two years duration and his management, which was again in essence a system of rewards contingent upon visual performance. In treatment sessions the basic exercise was for the patient to press a button during set time intervals: if he guessed the time intervals correctly a buzzer would sound, indicating that he was scoring points which would later be translated into privileges and other rewards. Button presses at the wrong time simply restarted the time cycle and thus reduced the total rewards that could be gained from the session. Without telling the patient, a visual cue (light) was then introduced which indicated reward opportunities. Initially, his accurate-guess rate fell instead of rising, and it was discovered that he was keeping his eyes covered. After explanation and suggestion of various kinds, scores began to improve. After further paradoxical scores, crude vision was finally reported by the patient, together with much display of emotion. The cues were then made progressively more subtle: this appeared to have the desired effect of increasing his visual discrimination and acuity. Conventional support and rehabilitation measures were then added. The result was a mixed one—during the 13 months of follow-up visual performance varied with his social situation, but in other areas he seemed consistently better adjusted. There was regular contact with the clinic throughout this follow-up period.

The Brand and Lind study is impressively detailed, and yet, partly because of this, many drawbacks become apparent on reflection: it seems likely that deliberate dissembling, not to say malingering, was a bigger factor in this patient than in many hysterics. The treatment took a very long time and was heavy with suggestion and poorly specified general supportive measures. The results were mixed, despite regular visits to the clinic during the follow-up period. Grosz and Zimmerman's (1965) further reports on this case throw serious doubt on both the diagnosis and the effects of treatment.

Case studies of comparable, but no greater, import are available concerning gagging (e.g. Epstein and Hersen, 1974), aphonia, fits, astasia with vomiting (e.g. the various cases in Blanchard and Hersen, 1976), blepharospasm and astasia (e.g. Meichenbaum, 1966). The last two authors use their anecdotes, plausibly enough, to press for a combined attack on several aspects of the patient's current problems at once. In this way, they feel, the chances of relapse or of new symptoms will be reduced: hardly a revolutionary hypothesis to a working clinician.

Lazarus (1963) describes a collection of past cases which has now become very well known. The collection is discussed in more detail under Hysteria Syndrome (below) but it seems that conversion symptoms were present in 27 of the author's 126 cases, and he reports that 71 per cent of these 27 patients, rated on global scale, were improved or recovered after fairly brief treatment. Whether the conversion symptom itself resolved, and what relation this may have had to any particular element of his complex treatment package, is impossible to say.

Other workers (e.g. Wolpe, 1958) argue from their case collections that it is important to approach hysterical symptoms associated with high anxiety differently from the rest. The cases of Dollard and Miller (1950) and Brady and Lind (above) add colour to this impression that some, but by no means all, conversions can be viewed as anxiety avoidance mechanisms which may or may not subsequently become autonomous habits. The management of the focal anxiety, where it can be identified, would seem best achieved by one form or other of systematic exposure to the anxiety-provoking situation, so that the patient can learn how to cope both with the situation and with the feelings associated with it. This, of course, is a much better established success area for behavioural methods (Marks, 1976).

Studies on the Hysteria Syndrome

Group study with control groups

The first study in this category (Scallett *et al*, 1976) is primarily concerned with electrosleep, which could only be labelled a behavioural approach if very loose criteria were used.

However, a more clearly behavioural treatment (relaxation exercises) is incidentally examined as well. In a well designed and described prospective double blind study of 24 women with the hysteria syndrome, the author compares the effects of central stimulation electrosleep, peripheral stimulation electrosleep, and placebo electrosleep. All patients had relaxation exercises. He concludes convincingly that neither form of electrosleep was any use in this condition. Relaxation exercises appeared to give modest benefit as measured by anxiety and depression scales, but there were no non-relaxed controls. Follow-up was short (50 days).

The second study (Sloane *et al*, 1975) is a massive, carefully planned and impressive work comparing psychotherapy and behaviour therapy results in a mixed neurotic population. Unfortunately, the lay-out does not allow separate direct evaluation of cases with the hysterical syndrome. One can, however, point to several indirect indications that behaviour therapy works well with such cases in comparison to interpretive psychotherapy or minimal non-specific support. In the study, 94 moderately severe neurotic out-patients were carefully and independently assessed and then assigned randomly to either behaviour therapy, psychotherapy or a 'waiting list' which actually entailed regular simple support. The therapists were free to simply 'do their best', within a set total of hours over a period of four months. The treatments were thus generally complex and multi-pronged, but they were consistently different in style and conformed well to their respective behavioural and psychodynamic conventions—this issue was independently monitored. After four months, on a global rating scale, 93 per cent of the behaviour therapy patients were improved or recovered compared with 77 per cent for the psychotherapy and control groups ($P = 0.05$.) At one year, only 65 patients could be traced for follow-up, and evaluation was confused by drop-outs and intervening treatments: there was an impression that the post-psychotherapy group were in some respects 'catching up'. One feature is of particular interest to this review: in the detailed analysis of the factors that correlated with good outcome in each of the types of therapy it is

clear that patients with severe pathology did better with behaviour therapy; this was particularly so in cases with severe 'acting out' phenomena—for example, as measured by the Hysteria and Mania Scales of the MMPI. The study is of great potential value to all students of psychological treatment and its evaluation.

Group study with no controlled design

Kass *et al* (1972) give a lucid and detailed, but uncontrolled, description of a multi-component in-patient behavioural strategy adopted with five women, all of whom had chronic and multiple 'hysterical' problems. One cannot be confident as to how many had the Briquet syndrome as such, since Kass does not use this yardstick explicitly. They were all admitted at more or less the same time, following suicidal or quasi-suicidal episodes. The programme lasted about five weeks. The central idea was to demand adherence to a tight daily schedule of items such as self-care, group meetings, record keeping, outside contacts, and certain special therapy sessions mostly involving role-play. These demands were intended to elicit, in a controllable environment, concrete examples of the maladaptive responses to stress which it was supposed their symptoms constituted. Penalties (loss of privileges) were imposed, not for hysterical behaviour as such but for schedule breaking. For the symptomatic behaviour (e.g. suicide threats, somatic complaints, sulking) explicit feedback was given, including on video: there was advice about alternative behaviours, and opportunity to practise these, for example in role-play. Multiple and concrete individually tailored measures of progress were kept. In four out of five patients symptomatic behaviour built to a peak at about three weeks and then declined to a new and acceptably low level. These four patients, but not the fifth, did pretty well, both symptomatically and generally, over the 18 months follow-up. The authors themselves, in an interesting discussion section, readily admit the flaws in the study. Nevertheless, given the usual natural history of such patients, and the messy and unpleasant failures that usually accompany their admission, these results look very suggestive at face value.

Case studies with controlled design

No study has been found that fits both the design criteria and the problem definition criteria.

Case studies with no controlled design (anecdotes)

Lazarus (1963) and Wolpe (1958) have both reported on case collections where chronic multi-symptomatic neurosis has been treated behaviourally.

The more valuable descriptions, for the purposes of this review, are those of Lazarus, though even here it is impossible to separate out for evaluation those patients who had the hysterical syndrome as such. Lazarus describes his results in terms of diagnostic dimensions (phobic, psychosomatic, conversion hysteria, etc.) and their relative responsiveness to treatment. Cases themselves are only reported in terms of a global scale, though this is well defined and conservatively applied. There are valuable descriptions of his various treatment tactics, but these are not enumerated case by case. Altogether he reports on 126 adults 'persistently handicapped by generalized neurotic disturbance', and of these 61.9 per cent were reported as markedly improved or completely recovered by the end of treatment (a mean of 14.07 sessions). High scores for hysterical, psychosomatic, and interpersonal complaints were among the factors associated with better outcome. Thus, one might reasonably guess that many of his successful cases had shown the hysterical syndrome. How far Lazarus' apparent results can be accounted for by his personal qualities, by his style of therapy, or by one or other of his special behavioural techniques, cannot possibly be determined from the data available.

Conclusions

In the area of hysteria, the comments of Eysenck and Beech (1971) about the inadequacy of outcome studies in behaviour therapy would still be justified. With hysteria cases expectancy, placebo and therapist variables are likely to be very important: thus, controlled studies are particularly desirable, yet

clearly there are not enough of them. Only tentative general conclusions can be drawn:

1. The behavioural approach is likely to be useful where therapist and patient can agree on clear definitions of both problems and goals, and do so in terms of outward behaviour.
2. The behavioural approach is likely to be useful if patient and therapist can agree to work intensively on a narrow issue, providing they recognize that there may be several such issues to deal with.
3. 'Symptom substitution', as a measurable phenomenon, is rare, and fear of it need not constrain management, even in hysteria. Like any other therapist, however, the behaviour therapist is well advised to clarify any external factors which may be maintaining the problem behaviour and manipulate them where possible.
4. In those hysterical cases where anxiety, or the avoidance of anxiety, plays a major and obvious part, the anxiety should be treated on its own merits: this is probably best done by a behavioural approach, based on systematic exposure to the feared situation.

The success, such as it is, of behavioural methods in hysteria, has no necessary bearing on theoretical formulations about the causes of hysteria, nor on theoretical formulations about the mechanisms of behavioural methods. Most, if not all, of the successes reported in this review could, for example, be simply interpreted as providing the patient with an adequately impressive 'excuse to get better'. This seems particularly true for conversion symptoms: whether the cause of a woman's aphonia be unconscious conflict, traumatic conditioning, or social anxiety plus lack of practice, it seems likely that the therapist should first try a pragmatic, concrete and directive approach: currently this is called, for want of a better term, behaviour therapy.

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Julian Bird, M.A., M.B., M.R.C.P., M.R.C.Psych., *Senior Registrar,*
The Maudsley Hospital, Denmark Hill, London SE5