

## COMMENT

### Some Comments on the Relation Between A-B Status of Behavior Therapists and Success of Treatment

In a recent paper, James and Foreman (1973) reported on the treatment of 36 enuretic children by means of the bell-and-blanket method, using the mothers of the children as "behavior therapy technicians." They predicted that mothers scoring high on the A-B status scale (Dublin, Elton, & Berzin, 1969), that is, showing risk-taking attitudes and being socially affiliative, relatively masculine and field independent, would be more successful than mothers scoring low on the scale, that is, showing the opposite characteristics. This prediction is based on the findings of McNair, Callahan, and Lorr (1962) that B therapists outperformed A therapists in the treatment of neurotic outpatients. The prediction was borne out by the data, and James and Foreman concluded "that therapist variables are associated with differential outcomes of behavior therapy" (p. 227). They even went so far as to suggest that "the present study raises the issue of the possibility of employing A-B status as a criterion measure for the selection of candidates for training as behavior therapists/technicians" (p. 228). It is suggested here that such a step would be distinctly premature.

There are several weaknesses in the study that must lead one to regard the conclusions as over-interpretations. To begin with the least important criticism, James and Foreman consistently regard enuresis as a *neurotic* disorder, and thus align it with previous work done on the relation between AB status and neurosis. It is very doubtful whether enuresis can be regarded as a neurotic illness, comparable to the various anxiety states and other neurotic conditions associated with A-B status in the literature; it is at least equally plausible to regard it as a failure in habit formation or conditioning, as a physical weakness associated with the bladder, or in many other ways. The literature does not give any kind of definitive answer, and since James and Foreman do not suggest a criterion of what does and what does not constitute a "neurosis," the matter remains doubtful at best.

In the second place, the method used makes no demands on the "technician" other than carrying out routine instructions of a more or less mechanical kind; in this it differs greatly from

psychotherapy or behavior therapy as used in cases that most judges would regard as "neurotics." It seems difficult to imagine just how the technician mother could give vent to her risk-taking attitudes, or her socially affiliative drives, when all she is asked to do is to put the device in place, wake up when the bell rings, and escort the child to the toilet. It would have been important to know whether A- and B-status mothers fell down on the job, or in what ways they differed in carrying out their duties; this information is not given. In its absence, speculation would be useless.

The most important objection, however, deals with the design of the "experiment." To establish a meaningful, causal relation between x (B status) and y (success of therapy), it is essential that the allocation of therapist to case be random; if this random element is missing, there is the strong possibility that whatever non-random elements exist might be the active ingredients in any observed correlation. In this case, the nonrandom element likely to be responsible for the observed correlation is of course the genetic one. It is well-known that heredity plays a very active part in determining the personality of human subjects (Eysenck, 1974), and it is equally well-known that the personality of the patient determines the differential success of different types of behavior therapy (Di Loreto, 1971). While the hypothesized relationship between therapist and treatment success cannot be disproved, the experimental design adopted is such that neither can it be supported by the results. One must wonder why no effort was made to investigate the personality of the children concerned, and to correlate this with the personality of the mothers; such information would at least give some evidence on which to judge the relative value of these different interpretations of the findings. James and Foreman did in fact mention the possibility of "differences in conditioning" arising "from systematic differences in children's relatively stable personality characteristics" (p. 227), but they seem to attribute this to "the long relationship of he patient-therapist dyads" (p. 227). It is interesting that James and Foreman in their article never

even raised the possibility of genetic determination of differences in respect to treatment, even though they posit a biological chain of causation (i.e., through conditioning). This failure to treat human beings as biosocial organisms, that is, as having their behavior determined *both* by genetic and by environmental factors, interacting with each other, vitiates the great majority of empirical studies in this field; assumptions about the importance of environment cannot be a substitute for proof, and the interpretation of faulty designs based on preconceptions of this kind adds little to the advance of science.

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*James and Foreman reply:*

Eysenck (1975) labels as "distinctly premature" the possibility of using A-B status as a criterion measure for selecting behavior therapists. His implication that we advocate immediate implementation of this strategy rests on removing our comment from the context of both its paragraph and the discussion section. We clearly specified that substantiation of our findings requires additional study of general behavior therapies, a range of therapist populations, and a number of target behaviors (James and Foreman, 1973). Obviously, correlations do not establish cause and effect. Therefore, we called for these as well as alternative avenues of research on relevant variables as the necessary prerequisites

for establishing the A-B scale as a criterion measure.

Eysenck's next criticism is that we consistently regard enuresis as a neurotic disorder, although we fail to offer definitive explanations for either enuresis or neurosis. Regardless of our own theoretical preferences, the A-B prediction (A-status therapists are more successful with schizophrenics than B therapists, who are more successful with neurotics) was based on psychodynamic diagnostic categories. Hence, it was necessary to interpret our target behavior, enuresis, in psychodynamic terms. Reviews by Mowrer (1950) and Coleman (1956) indicate that psychodynamic theory and research consider enuresis a neurotic versus a psychotic disorder. This categorization, along with the mechanical interests tapped by the A-B scale, led to our hypothesis that B-status technicians would be more successful than A technicians with Mowrer's treatment for enuresis. While Eysenck doubts the comparability of enuresis to anxiety states or the other neurotic conditions, our post-treatment interviews demonstrated that enuretics experience high levels of distress along with serious learning and adjustment problems.

Unlike psychotherapy or behavior therapy, the treatment conducted by the technicians is so mechanical and routine by Eysenck's description that it is difficult to visualize how the A-B dimension could mediate therapeutic success. Actually, the technicians were given complete responsibility for the treatment during the single consultation interview. They were required to purchase the apparatus, explain the project, enlist family cooperation, repair mechanical failures, sustain detailed attention to procedures, and keep daily records. Our report of lower success rates than studies with extensive experimenter involvement (i.e., Mowrer, 1938) suggests both a more complex therapy task project than Eysenck describes and a possibility that the success of more complex behavior treatments would also be related to therapist variables. Given the importance of our outcome findings, we join Eysenck in calling for process-oriented research designs to determine the specific behavioral dimensions which undoubtedly characterized the differential success of A- and B-status technicians.

Eysenck's "most important objection" centers on the failure of our design to insure a random assignment of therapist to case, which leads him to hypothesize that the genetic element in the pervasive ingredient in the observed correlations. He goes on to note, as we did, that personality investigations would have shed additional light