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AUDITORY HALLUCINATIONS IN A CASE OF HYSTERIA

DEAR SIR,

That hallucinations are experienced by hysterical patients is well known, but when Dr. Levinson (*Journal*, January 1966, pp. 19–26), asserts that the auditory hallucinations of his patient were "... disclosed to be the result of an unconscious wishing and yearning to resurrect and rejoin her mother", he is drawing a conclusion not supported by his data. In fact, the patient states: "I've had these thoughts before, so I know you're right. The voices were my mother. When you first told me I didn't really believe it. I thought you were just telling me things to satisfy me, as I do to my daughter just to keep her quiet." Clearly, it was Dr. Levinson who "disclosed" the source of her hallucinations to the patient.

The origin of hallucinations remains unknown, and it is difficult to see how psycho-analysis, building and rebuilding upon the same old theories, can add any new knowledge in this area.

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GENESIS OF HOMOSEXUALITY

DEAR SIR,

My previous correspondence, critical of Dr. Eva Bene's papers "On the Genesis of Male Homosexuality" and "On the Genesis of Female Homosexuality" was published in the January, 1966, issue along with Dr. Bene's reply. At the risk of prolonging a controversy beyond tolerance, I am compelled to reply to the reply.

Dr. Bene disapproved of my referring to her data-gathering vehicle as essentially an item check list. She prefers to call it a "semi-projective test". In this test a series of statements, printed on cards, is presented to the subject, including such items as "This person in the family was very pleasant". The cards are sorted into a group designated "mother" or one designated "father". For practical purposes this is essentially the same as listing the statements in a column on a piece of paper and having the subject check off items which pertain to mother or to father. Even the author has previously (*J. ment. Sci.*, 1957, 103:541) described the test as one "which could be reproduced, it is true, in a paper and pencil form ..." However, as it was originally designed for children it was felt to be less an inhibiting reminder of school if paper and pencil were not used. Presumably, the adult subjects in Dr. Bene's more recent studies were no longer labouring under the same handicap.

I emphasized that the test is essentially an item check list in order to highlight the source of potential bias implicit in any study which must rely on the validity of a subject's reply to a question about his past. Dr. Bene has also recognized this potential bias. Her hypotheses regarding female homosexuality were wisely prefaced by "according to their childhood recollections". For male homosexuality, however, reference was made to the utilization of recollections as the investigative tool, but the actual hypotheses did not include the vital words found in the "female" paper. Unfortunately, in the study on female homosexuality, between the statement of the hypotheses and the interpretation of the results, the vital qualifier distinguishing recollection from fact tended to become less distinct.

In support of my contentions, consider first the statement taken from the "male" paper's summary: "The results confirm those of previous studies according to which homosexual men more frequently than heterosexual men had bad relations with their fathers, and had fathers who were ineffectual as parents ..." More precisely, this study can only confirm other studies in which subjects also *reported* bad paternal relationships. If the study "confirmed" did not rely on retrospective reports, but utilized a more reliable measure of the father-son relationship, then Dr. Bene's study cannot confirm the latter, for a poorer measure should not confirm a better one, rather it ought to be the other way round. Consider next, from the "female" paper, "... and so was hypothesis 5 (confirmed) to the effect that the fathers of homosexual women more frequently had weak personalities". How do we know their personalities were weak? Dr. Bene's hypothesis was that lesbians would *describe* their fathers as weak, not that the fathers *were* so. Finally, to the specific point of contention in my previous correspondence—whether the fact that more lesbians than heterosexuals report their parents as having wanted a son can be accepted as indicating that such a phenomenon truly occurred. "Hypothesis 7, which states that the parents of homosexual women more frequently want a son,

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instead of a daughter, than is the case with the parents of heterosexual women, was confirmed by the findings." Again, the hypothesis only stated that lesbians would *report* the phenomenon to a greater degree. We do not know that the parents of homosexual women wanted a son more frequently.

I stress these points for the following reasons. The hurried professional, flooded by a sea of medical literature, finds it impossible to read all the articles he would like. Much reading is reduced to summaries of papers, or to abstracts which quote statements from the original paper succinctly summarizing the results. Furthermore, full-length papers frequently refer to the past related publications of other authors also by quoting one or two of the results. Thus it behoves all of us who publish to be ever scrupulous in the wording of our material. Dr. Bene's study was a carefully conducted one in an area which demands sophisticated research. Undoubtedly, it will be widely cited. Thus, I consider it important to emphasize its potential for overinterpretation.

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DEPRESSION: PSYCHOTIC/NEUROTIC; ENDOGENOUS-EXOGENOUS

DEAR SIR,

Your correspondent (Foulds, *Journal*, November 1965) begins his letter with a misunderstanding of my report. He states that "Mendels (. . .) seems to regard it (viz. the separation of endogenous and neurotic depression) as neither possible nor useful."

As far as the usefulness of the separation is concerned, I wrote (*Journal*, 1965, p. 683), "We found a striking difference in response to ECT between the groups of patients designated as endogenous and reactive". Further on, in discussing the symptomatological overlap I stated ". . . the response to treatment was significantly associated with a small difference in balance between the two groups of symptoms, in spite of the marked overlap"; and, at the risk of overstating the point, I wrote, in conclusion, "Using symptomatic definitions of endogenous and reactive, a clear-cut difference in response to ECT of two syndromes was demonstrated."

With regard to Foulds's contention that I claimed that the separation was not possible, it appears that he has been selective in his interpretation of the paper. One of the major points made is that (using the single criterion of response to ECT) these are two distinct syndromes. I emphasized the need to consider the overall clinical picture, rather than reach conclusions based on the presence or absence of individual clinical features. While this would seem axiomatic, the fact is that there are many published reports in this field which do emphasize the importance of the individual signs and symptoms (references in the original articles).

Foulds's next point (paragraph 2) also quotes the paper out of context. To correct the erroneous impression he creates, I again quote from the paper. In the course of discussing some of the implications of the results, I stated 'This lends support *to the concept* that there is always an endogenous element to a depressive illness, and that the reactive element is more variable. Furthermore, the response to ECT is *perhaps* related most intimately to the extent of the reactive components . . . " In the conclusion I reiterated that the results and conclusions were based on defined symptomatic definitions of the two depressive syndromes (i.e. that this was not meant as an exhaustive study of the problem with final conclusions); I stated that "an endogenous component" *appears* to be present in *most* of the patients; the diagnosis as well as the response to ECT is *more closely related* to the "reactive features" present. To my mind this does not support the dogmatic interpretations made by Foulds.

The typing errors in Table II, while unfortunate, were not crucial. The Table should have read: A/E, Steady, Course, 10; B/E "Adequate Premorbid Personality" 17; B/R "Adequate Premorbid Personality" 10. This criterion does not significantly alter the distribution of the symptoms. Chi Squared analysis bears out most of the original interpretations as well as the general argument (viz. the apparent "dominance" of "over reactive" and "endogenous" symptoms in the context of this experiment). Chi Squared analysis, using the figures in Column B as Foulds has done (it is much more arbitrary than Column A) and ignoring the dichotomized personality and course variables, reveals that certain "reactive" features are highly significant in their distribution (neurotic traits in adulthood p ·001; precipitating factors, p ·001; and emotional liability, p ·005). Two of the six "endogenous" factors shows less significant variation (diurnal variations (worse a.m.), p. ·05; and psychomotor retardation, p ·01). Foulds also ignores the supporting findings in the first paper: that when the factors were considered individually, emotional liability, precipitating factors, neurotic traits in early life, and inadequate premorbid personality (as a single factor) were significantly associated with poor response to