Brit. J. Psychiat. (1968), **114**, 1593–1597

Correspondence

CONCEPTS OF HYSTERIA

DEAR SIR,

Will you once again allow me space in your columns to act as Devil's Advocate against the Farley-Guze concept of hysteria? I outlined some of my main objections in previous correspondence to the *Journal* (May, 1968, pp. 644–645). Now the latest papers in the *Journal* by Woodruff (1), and Farley *et al.* (2) seem to finally destroy their own attempts to set up the concept of hysteria as an independent clinical entity, at any rate as so defined by Farley and Guze. In these papers they show that the incidence of hysteria in their female populations is 1–2 per cent. They base this conclusion on the finding that only that percentage of women had suffered symptoms in nine of the ten groups, "without other medical explanation".

But, as could have been predicted by anyone with the slightest knowledge of the practice of medicine, they also show just how common all these symptoms are. In fact if anything they seem to underestimate the incidence of the symptoms. Taking Group 6 in Farley *et al.*'s paper as an instance, they found that 76 per cent. of women had never suffered from either abdominal pain or vomiting. My own experience is that about 99 per cent. of people (of any age, population or sex) have had abdominal pain or vomiting at some time in their lives. Even if no "formal" medical explanation or diagnosis was handed to them at the time this does not mean to say that there was none; mild alimentary infections are very common. Exactly the same remark could with justice be made about every one of the ten so-called groups.

Accepting, however, their level of estimation of the symptoms, they are all very common. One would naturally expect, therefore, by the operation of chance laws, that a certain percentage of people would have experienced symptoms from nine of the ten groups. Does this logically establish the concept of hysteria as a diagnostic entity? Obviously not.

What about people who have suffered from one or more symptoms in say seven, instead of nine, of the ten groups? Are they to be considered to be suffering from minor forms of hysteria falling just short of the full syndrome? The authors remain silent on this point. But on their own estimates about 60–70 per cent. of the population would have "minor hysteria", and, in my estimation, 100 per cent. This obviously makes their concept of hysteria quite meaningless.

And can the authors, if they reply to this letter, tell us what they consider to be the value of their diagnosis, once made (apart from the incidence of similarly affected relatives which they have pointed out in previous articles and correspondence)? Hysteria has become such a pejorative term to both the lay and the medical public; it seems to imply to most people a state compounded of the elements of incurability and malingering.

For myself, I am very wary of the term hysteria, whether in Farley and Guze's sense or in any other. I think the American Classification was right to drop the term altogether and retain only the term Conversion Reaction. I prefer to recall Walters's (3) eloquent plea for abandoning the word: "It is time the old label 'hysterical' was killed and buried. For there is magic in words, the magic of meaning, and we as physicians should avoid terms that can mislead or harm."

Finally, can the authors tell us why they picked on the age of 35? Why not 15 or 55 or any other multiple of 5? Is it perhaps that the magical number 7 when multiplied by 5 helps them to decide whether or not a person is a hysteric?

R. P. SNAITH.

Stanley Royd Hospital,
Aberford Road,
Wakefield, Yorks.

REFERENCES

1. WOODRUFF, R. A. (1968). "Hysteria: an evaluation of objective diagnostic criteria by the study of women with chronic medical illnesses." *Brit. J. Psychiat.*, **114**, 1115–1119.
2. FARLEY, J., WOODRUFF, R. A., and GUZE, S. B. (1968). "The prevalence of hysteria and conversion symptoms." *Ibid.*, **114**, 1121–1125.
3. WALTERS, A. (1961). "Psychogenic regional pain alias hysterical pain." *Brain*, **84**, Part 1, 18.

DEAR SIR,

We agree with Dr. Snaith that the term "hysteria" does seem to anger some psychiatrists. On several occasions we have indicated that we recognize its ambiguity (2, 3). Likewise, we have discussed the historical precedents for the term and our decision to continue to use it.

Dr. Snaith asks what is the value of our diagnosis, once made. Dr. Snaith has missed the point; that, perhaps, is our fault. In each of our reports we have attempted to stress the value of the diagnostic criteria in prediction of clinical outcome. We consider our findings useful, first of all, to physicians who are confronted with patients who report symptoms which are unexplainable. These patients are frequently labelled "hysteric", "psychogenic", or "functional". Physicians need a systematic way of approaching such patients in order to organize management. Our studies represent an attempt to provide such a systematic approach. We need not recapitulate the result of the various studies here, except to say that the more closely a patient meets the objective criteria for hysteria the more his physician may be confident that the symptoms are medically benign. On the other hand, unexplained symptoms among patients who do *not* meet the objective criteria for hysteria are associated with a medical outcome involving the most diverse medical, neurological, and psychiatric illnesses (1, 3, 4).

The question of "mild hysteria" needs clarification. We have not been particularly vocal about mild hysteria because our understanding of it is incomplete. If mild hysteria exists, it will be demonstrated by data provided by the follow-up of patients who have symptoms fewer than we require for our present diagnosis; also by the study of the family members of those patients. We are engaged in such studies at this point.

We have commented previously on the importance of family data (2). We refer the reader to those comments, rather than recapitulate them in the same journal after this brief period of time.

It is characteristic that hysteria begins early in life. The age of 35 as a formal requirement is somewhat arbitrary. The ages of 30 or 25 might have been chosen just as well. In fact, we suspect that the choice of a younger age of onset might improve our criteria; probably even fewer false positives would occur. Preliminary data also suggest that a requirement for younger age of onset may allow us to dispense with the awkward business of deciding when individual symptoms are or are not medically explainable. Out of context, the diagnostic criteria appear arbitrary; they were actually derived from studies of the natural history of hysteria (5). It was on the basis of those studies that our more quantitative approach to diagnosis was organized.

ROBERT A. WOODRUFF, JR.
SAMUEL B. GUZE.
Washington University School of Medicine,
Barnes and Renard Hospitals,
4940 Audubon Avenue,
St. Louis, Missouri 63110, U.S.A.

REFERENCES

1. GATFIELD, P. D., and GUZE, S. B. (1962). "Prognosis in differential diagnosis of conversion reactions (a follow-up study)." *Dis. nerv. Syst.*, **23**, 623–631.
2. GUZE, S. B. (1968). "Correspondence." *Brit. J. Psychiat.*, **114**, 645–646.
3. —— (1967). "The diagnosis of hysteria: what are we trying to do?" *Amer. J. Psychiat.*, **124**, 77–84.
4. PERLEY, M. J., and GUZE, S. B. (1962). "Hysteria—the stability and usefulness of clinical criteria. A quantitative study based on a follow-up period of six to eight years in 39 patients." *New Eng. J. Med.*, **266**, 421–426.
5. PURTELL, J. J., ROBINS, E., and COHEN, M. E. (1951). "Observations on clinical aspects of hysteria. A quantitative study of 50 hysteria patients and 156 control subjects." *J.A.M.A.*, **146**, 902–909.

UNILATERAL ELECTROCONVULSIVE THERAPY

DEAR SIR,

I should like to comment on the double-blind trial of unilateral E.C.T. by Raymond Levy (*Journal*, April, 1968, p. 459).

Clinical psychiatrists may agree that the duration and intensity of memory impairment after each E.C.T. varies a great deal from treatment to treatment, and although temporary may last more than six hours. It becomes a serious matter, however, if it lasts more than 24 hours. Perhaps there were practical reasons for retesting only six hours after the last E.C.T., but it would have been more interesting to have waited for 48 hours. The author finds significantly less impairment on "general orientation" and "memory for general events" of the Gresham Test Battery with unilateral treatment, and more relief from depression with bilateral treatment (but not significantly more).

By a curious coincidence, three days after reading this paper, one of my patients (male, aged 45, suffering from endogenous depression with obsessional features) volunteered the information that his present E.C.T. (three treatments in one week) had caused much less memory disturbance than his treatment two and a half months ago (two treatments) which was discontinued because of severe memory impairment. He had come back for completion of the course of E.C.T. as he felt that in spite of the memory disturbances the previous two treatments had done him more good than antidepressant drugs.

He did not know it, but the previous E.C.T. was given bilaterally and the present treatment unilaterally. Subjective relief of depression is now almost complete.

I for one shall continue to use unilateral E.C.T. as I have done in the past, not as a routine procedure,