of greater or lesser degree associated with (*unter*) the symptoms of an acute or subacute mental disturbance." Examples of this *Schwäche* follow: "The mental capacity of the patient is decidedly reduced; he may show the same industry or even more . . . but he can no longer grasp matters correctly, cannot follow complicated expositions of a subject, cannot concentrate his attention; he is absentminded, dreams and broods without any deeper interests or recognizable aim. . . . The elements of his experience no longer influence each other, no longer lead to any conceptions, judgments or conclusions. . . . In his actions, the patient is either slow and sluggish, or shows a peculiar childish silliness. . . ."—and much more to the same effect.

It is *these* symptoms of the early and florid stages that Kraepelin believed to be manifestations of *Schwäche* or *Verblödung* (the last term, incidentally, ought to be translated simply as "dementia", rather than by the question-begging "deterioration", just as *Altersblödsinn* is "senile dementia"). Of course, we may disagree with Kraepelin and hold that these symptoms do not indicate dementia; but that is another matter.

Now we come to the prognosis (p. 429), and here we find that, so far from forecasting progressive and inexorable deterioration in every case, Kraepelin says quite definitely that "in these milder cases dementia can be arrested at very different stages. . . . In favourable cases the disorder comes to an end with a moderate degree of mental enfeeblement (*Schwachsinn*) which generally remains unaltered, but occasionally, it seems, some part of the mental impairment may actually disappear. . . . There must be many people whose mental shipwreck through dementia praecox has passed unnoticed, because they have been able to rescue enough mental capacity to carry on the struggle for life in modest spheres of activity"—other examples of this diversity of outcome are, on the one hand, scholars who fail to fulfil their early promise, and, on the other, persons who drift into vagrancy and eventually arrive at the asylum via the workhouse.

Finally, diagnosis. Do we find Kraepelin warning us that a diagnosis cannot be made until the course and outcome of the case are known? Not at all; on the contrary, he tells us that an early diagnosis can and should be made (p. 440); and in differentiating from early periodic psychosis one should pay attention to the more insidious onset, the lesser vehemence of the symptoms, and *the signs of acquired mental weakness* without any profound disturbance of consciousness. Once again, we have "mental enfeeblement" used to denote an aspect of the early symptoms, not to indicate a terminal condition.

To sum up: Kraepelin did not rest his concept of dementia praecox on the course or prognosis of the disorder, or regard incurability as its criterion. He rested it on a definite clinical picture, of which he gave a masterly description, and, rightly or wrongly, he considered that certain of its features could from the outset be summarized under the heading of "a peculiar kind of mental enfeeblement". He recognized that the disorder could be arrested at any stage, although the majority went on to severe dementia, and he emphasized the importance of early diagnosis.

It should be added that, contrary to what is sometimes alleged, Kraepelin was not acquainted only with asylum cases, but knew all about the *formes frustes* patients who remained in the community; also that he realized the importance of "guarding the still remaining mental faculties against the threat of atrophy through disuse, by means of careful and well-planned exercise of those faculties so far as may be practicable" (p. 441).

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FAMILY AND MARITAL HYSTERIA

DEAR SIR,

The paper by Woerner and Guze (*Journal*, February, 1968, p. 161) draws attention once again to the concept of hysteria as a diagnostic entity. Few terms in medicine have adapted themselves so readily in the Carrollian sense of meaning just what we want them to mean. Few terms, beside hysteria, have had to do duty for so many different concepts.

Hysteria may be used to mean that the patient is considered to be deriving some subtle "gain" from his illness. Hysteria may be used to imply that we believe the patient's symptoms are directly derived from emotional conflict and so translated by the theoretical mysteries of conversion and dissociation. Hysteria may be called upon to "explain" any psychological complaint that is considered contagious, such as folie-à-deux or the "epidemic hysterias" of schoolgirls. Hysterical "overlay" is a favourite formula of physicians who consider that the patient is exaggerating his symptoms. Hysteria may be applied to the importunate patient, or to any patient with chronic neurosis who "refuses" to get better (and so applied gives the physician a retrospective bonus of solace for his therapeutic failure). Laymen and many medical men consider that hysteria and malingering are scarcely distinguishable, and the term "hysterical attack" is beloved by the nursing profession as a way of conveying in their reports that

the patient is upset in a somewhat noisy and alarming way. As if this were not enough, a collection of ill-defined personality traits have been arbitrarily banded together (probably without any natural desire to form a cluster!); persons who possess one or more of these traits before or during their illness are then considered to have an "hysterical personality" which has predisposed them to develop hysteria.

Whitlock (1) in his paper on the aetiology of hysteria has decided to define his population as those showing clear-cut conversion or dissociation symptoms. The pitfalls of the diagnosis on the basis of such symptoms has been stressed by Slater (2); moreover, many such symptoms which have a time-honoured label as "hysterical" may well have a basis in some other pathology; for instance Stengel (3, 4) has shown that many "hysterical" fugues are depressive in origin; Walters (5) has made a plea for the abandonment of the term "hysterical pain" and the substitution of the term "psychogenic regional pain"; and I, for one, never see patients with so-called "globus hystericus" except in the setting of fairly severe anxiety states.

Woerner and Guze now attempt to "define" hysteria in a different way. Do they really believe that they have delineated a clinical entity by rating the patient for a whole list of symptoms, most of which are manifested by all patients with chronic neuroses? The authors state that a patient suffers from hysteria if he has at least 25 different symptoms drawn from ten "groups" of symptoms, in association with "a complicated medical history beginning before the age of 35, and the absence of any other diagnosis to explain the symptoms". The authors support their contention that they have defined a clinical entity on the basis of previous work (6) that patients, so defined are consistent in the subsequent course of their illness. But surely the authors must agree that the more severe and chronic any condition, organic or psychological, the less likely the patient is to get better.

If the authors had limited their conclusions to the statement that patients with more severe and prolonged neurotic states were more likely to have relatives who also suffered from some form of psychological instability, there would probably be few who would have disagreed with them. But their present conclusion that they have defined a clinical entity with a unique constitutional basis (in terms of the type of psychological disorders of their relatives) is of dubious value.

Medical science is now irrevocably "saddled" with this confusing term hysteria. We are really no nearer to defining what we mean by it than were the ancient Greeks. It is a pity that medical men did not eject the term from their vocabulary when it was proved that the uterus was not in the habit of wandering around the body. Whitlock (1) recalls the warning given by Charcot 80 years ago, and since ignored: "Bear well in mind—and this should not exact too great an effort—that the word hysteria means nothing."

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Dear Sir,

I welcome an opportunity to reply to Dr. Snaith's letter.

Dr. Snaith seems to be saying that because the label hysteria has been used in so many different ways in the past, often confusing and contradictory, it should be abandoned. In the final analysis perhaps he is right. Nevertheless, diagnostic names in medicine tend to persist when they have been in use for a long time. As I have indicated elsewhere, the syndrome that we have been studying was described by Briquet under the label of hysteria in 1859 so there is a precedent going back that far at least. Furthermore, I believe that most psychiatrists would agree that hysteria is the correct diagnosis for the patients we have been describing. Their usual argument is not that these patients are improperly labelled hysteria, but that they wish to use the term for other patients whom we are not prepared to label hysteria.

If Dr. Snaith believes, as his letter suggests, that follow-up and family studies do not serve to validate clinical diagnoses, I strongly disagree. I think that the results of our work indicate, subject to confirmation by others of course, that certain diagnostic criteria will predict the subsequent course and response to treatment of a group of patients, and that a similar disorder will be found in the families of these patients. Dr. Snaith may believe that this kind of observation is unimportant. While there is no arguing about taste in these matters, I cannot refrain,