while they were on a gluten-free diet, but we believe that even these can be explained on the natural history of the disease. After all, the average duration of dermatitis herpetiformis without a gluten-free diet is four years, and we have patients who have had spontaneous remissions (without a gluten-free diet) for periods of up to four years. More cases who needed no dapsone while on their diet and relapsed when the diet was stopped, like case 17 in the series of Drs. Marks and Whittle, are needed to establish that gluten has an effect on the rash, and such case histories have not yet been published. We believe indeed that none of the published evidence shows that the rash, in contrast to the enteropathy, of dermatitis herpetiformis is improved by gluten withdrawal.—We are, etc.,

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Marrow Suppression from Mercury Poisoning?

SIR,—Workers exposed to mercury in industry are found to excrete increased amounts of mercury in their urine. The World Health Organization figures suggest that more than 300 µg./l. of urine should be taken as an index of over-exposure. Nevertheless, figures well over this level, even amounting to several milligrams of mercury per litre of urine, may be found in many mercury workers without any clinically apparent ill effects.¹ The diagnosis of mercury poisoning as opposed to simple over-exposure has traditionally been taken to depend on finding such symptoms as stomatitis, erethism, and tremor. There are but few references in the literature on the effects of chronic mercury exposure on the haemopoietic system. Thrombocytopenia has been described in man,² and Kossmann *et al.*³ have reported a reduction in the platelet counts of rabbits experimentally exposed to mercury vapour. Wilson⁴ reported death from aplastic anaemia in a patient taking Hydrarg. cum creta tablets for 33 years, whose urine contained 674 µg. of mercury in 24 hours, but in whom there were none of the classical symptoms of mercury poisoning. The following report is of a mercury worker who died of aplastic anaemia, with a high level of mercury in the urine.

A 59-year-old male developed a purpuric rash after working for 6 months as a mercury filler in a factory making thermometers. When first seen his blood count showed a haemoglobin of 11.8 g./100 ml., white cells 900 per cu. mm., neutrophils 20% (180 per cu. mm.), platelets less than 10,000 per cu. mm. Iliac crest bone marrow showed acellular fragments, severely suppressed granulopoiesis, and absent megakaryocytes. The urine contained 1,010 µg. of

mercury per litre. Intractable diarrhoea and a rapidly falling haemoglobin level necessitated admission to hospital, where he was transfused and treated with high dosages of steroids. Later, 2, 3-dimercaptopropanol (BAL) (800 mg. daily) was given. He died with signs suggesting cerebral haemorrhage 20 days after his first attendance. None of the traditional signs of mercury poisoning were detectable. Careful inquiry indicated that he had had no contact with any other drugs or chemicals likely to injure the marrow. Post-mortem examination (Dr. R. E. Cotton) confirmed the marrow aplasia and showed, in addition, haemorrhagic "colitis" and multiple small haemorrhages in the brain, endocardium, and renal calices.

In most cases of aplastic anaemia, the question of whether a particular substance is responsible is answered by an assumption based upon previously documented relationships. Similarly, in this case no dogmatic conclusion can be drawn, but the findings are reported to draw attention to the possibility that some degree of marrow suppression might occur with high body concentration of mercury.

We are indebted to Dr. M. H. P. Sayers, of the Medical Branch of the Department of Employment and Productivity, for the urinary mercury estimations.—We are, etc.,

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Voluntary Sterilization

SIR,—In his helpful letter (13 December 1969) Dr. P. H. Addison, secretary of the Medical Defence Union, stresses the principle that vasoligation may not be carried out under the National Health Service unless there is a medical indication for the operation. There is, he says, no distinction between men and women. In either case the operation must be necessary to protect the physical or mental health of the patient.

Two questions arise here: What, in this context, is meant by the terms physical and mental health; and to what extent do women differ from men in their eligibility for sterilization under the N.H.S. on grounds of physical and mental health respectively? There is little doubt as to what is meant by physical health. On this ground tubal ligations, mostly connected with foreseen complications and aftermaths of future pregnancies, are widely performed under the N.H.S. But manifestly there are no equivalent grounds for performing vasectomies. Mental health, however, is less easy to define. The World Health Organization has described it in the following terms: "Health is a state of complete physical, mental and social well-being and not merely the absence of disease or infirmity."

This statement, which has been understandably criticized, is quoted here because, in spite of its over-comprehensiveness, it

gives substance to the view that a person's mental health can fall short of what it ought to be without that person being overtly ill. In respect of mental health, however defined, the two sexes are here more on a par than in respect of physical health. If a couple has lost confidence in contraception because, despite precautions, one or more unwanted pregnancies have occurred, misgivings and anxieties will be focused by both on the coital act. The more numerous the failures the greater and more natural the anxiety of the couple.

Dr. Addison mentions the Simon Population Trust. A noteworthy finding of a recently completed follow-up, organized by the Trust, of over 1,000 vasectomized subjects (a report of the follow-up is now being prepared) was that the commonest reason prompting men to seek vasectomy was their dislike of contraception, though most gave several reasons. Nearly 80% of the respondents to a questionnaire gave this answer. The next most common reason was the husband's wish to spare his wife the more serious operation of tubal ligation. Coitus interruptus remains one of the most commonly practised and least reliable methods of birth control. In my experience coitus interruptus is harmless to most couples, but not to all. More readily than other methods it generates stress-causing anxieties about the possibilities of failure. In the above-mentioned follow-up questions were asked about the effects of vasectomy on the general health and on the sexual lives of both the respondents and their wives. In respect of their sexual lives more than three-quarters of both sexes reported improvements. Men do not apply for vasectomy for frivolous reasons. They apply because they think that they and their wives would be happier and their marriages more harmonious after it than without it: because, in short, it would conduce to their mental health.

It is desirable that a man wishing to be vasectomized should consult his family doctor. If the latter is of the opinion that vasectomy would spare the applicant and his wife coital anxieties and other worries, and accordingly recommends it, then, I suggest, a surgeon would be justified on grounds of mental health in performing the operation under the N.H.S.—I am, etc.,

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Epidemic Malaise

SIR,—I am glad that Drs. C. P. McEvedy and A. W. Beard have had the grace to say that their opinions (3 January, p. 7), on the Royal Free Hospital epidemic of 1955 are their own, as their paper is an example of the errors that one is apt to make in retrospective assessments of case notes made by people one doesn't know well. It also betrays considerable lack of historical awareness of the situation obtaining at the Royal Free Hospital at that time and of the full history of the epidemic. This includes the physical separation of the preclinical medical school from the hospital, which itself was split into six main divisions with the result that clinical students were pretty scattered for much of the course. Nobody who really knew the physicians who looked after most of the cases would ever suggest that they would overlook hysteria in the differential diagnosis of an obscure and bizarre illness, and indeed the tendency would

have been to put this higher on the list for students and nurses than for the general hospital population.

It was precisely because of this that the epidemic appeared to break out in such an explosive fashion. Clinical students who were smitten by such an odd assortment of unpleasant symptoms which fitted nothing in the textbooks were strongly motivated to keep them to themselves and struggle on as best possible, hoping for a rapid end to them, particularly as none of us wanted to delay taking finals. It was only when the bulk of the class who qualified in 1956 were gathered into residence for the midwifery post at Liverpool Road at the beginning of July, 1955, that the epidemic really broke out, and it was because of the sore throats, which were the usual presenting symptom, that the illness could not be concealed any longer. I should perhaps remind readers that this coincided with an exceptionally hot, dry summer and students' quarters were rather cramped—absolutely ideal conditions for an explosive outbreak of virus infections such as anterior poliomyelitis. This was possibly aggravated by the previous post having been the "E.N.T. and Eyes," which involved the least interstudent contact of all, was the least demanding, and which had enabled the pre-epidemic victims to suffer in silence without attracting any attention to themselves.

As psychiatrists, Drs. McEvedy and Beard should be aware that diagnosis of hysterical illness should be made on positive grounds of finding evidence of both primary and secondary gain, and not just on negative grounds of not being able to explain the symptoms in another way.

However, I should like to deal with the details of the paper in order.

(1) In 1955, a very high proportion of the staff and student population and all the nurses at the Royal Free Hospital were females. One would therefore expect most of the people to be affected to be women and at that time, most of the male students were preclinical. Even in term, there was minimal contact between clinical and preclinical students, and this was during the long vacation, so very few male students were at risk.

(2) As I have already pointed out, it is quite as erroneous to base any conclusion, even statistical, on figures obtained from people treated only as inpatients as it would be to deduce the true incidence of dysentery (or depression) on returns from those admitted to hospital.

(3) The hypoaesthesia was difficult to define because of its odd quality. The nearest I can get is to say that thermal sensation was impaired, and for light touch the difference from normal was comparable to the feel of swansdown as against feathers. There was a loss of precision.

(4) Surely it is only if a paralysis is due to a pyramidal-tract lesion that exaggerated tendon reflexes develop, and, the longer I am in practice, the less convinced I am of an absolute correlation between muscle tone and strength and tendon reflexes. Particularly in mental deficiency practice, it is quite common to find hypotonia, sometimes extreme, associated with normal or grossly exaggerated reflexes and extensor plantars. Hypotonia is often associated with relative disuse, and pain is a powerful inhibitor of movement. The pain of benign myalgic encephalomyelitis could be quite intense.

(5) Anybody who believes that the female population of the Royal Free Hospital was segregated just doesn't know the Free. I can only think that Drs. McEvedy and Beard must have led a very sheltered student life.

(6) Disproportionate depression and emotional lability are common concomitants of most viral infections (for example the well-known post-influenza depression), especially those affecting the C.N.S. Disturbance of sleep was a common feature, and disruption of normal sleep-rhythm is now recognized as a factor productive of emotional instability.

(7) As I have mentioned above, sore throat was a usual presenting symptom. The cervical lymph nodes, especially posterior groups, were usually mildly enlarged and disproportionately tender. In this context it may be of interest to record that when I came round after a tonsillectomy in 1957, two years after the onset, I found that I had severe impairment of light touch and temperature in my right thumb, which indeed felt as if it had been completely frozen, and it took nearly three months for this to clear up.

(8) Because of the functional reserves of the liver, jaundice is an exceedingly crude index of hepatic function, so one would not expect a high incidence in an infection without a high predilection for liver. Even now, liver function tests are also pretty crude indices, and they are more refined now than they were in 1955.

(9) Throughout most of the C.N.S. also there are tremendous functional reserves and considerable overlap, so that, with the exception of certain areas such as deltoid, and tibialis anticus, it takes a pretty large lesion to produce changes gross enough to be detected objectively by the still very crude methods of examination at present available. Widely diffused but very small lesions are not likely to be detected easily either clinically or histologically.

Drs. McEvedy and Beard state that encephalitis is a serious illness; this is an example of cyclical thinking. If one requires features such as disturbance of consciousness before making such a diagnosis then one automatically excludes relatively minor illnesses, but it is not logical to postulate that brain tissue is the only one to make an all-or-none response to infection.

I must apologize for writing at such length, but having been personally involved in the epidemic, and having the rather unusual combination of postgraduate experience in both infectious diseases and psychiatry, I feel I am better qualified than most people to point out the fallacies in this paper.—I am, etc.,

PAULA H. GOSLING.

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SIR,—I have followed the papers of Drs. C. P. McEvedy and A. W. Beard (3 January, p. 7) and the subsequent correspondence—irate and otherwise—with much interest.

I can find no immediate reference to long term follow up of any of these cases, and would therefore like to briefly describe the features presented by an ex-patient of mine (Caucasian), whom I saw in Rhodesia over the period 1965-6. She had been quite severely affected at the time of the original Wentworth/Addington Hospital, Durban, outbreak described early in 1955.¹

She was a nurse. Throughout the above period of observation, she worked as a medical receptionist in the hospital out-patients department. She was happily married with four children. Her husband, a miner, was, at this time, more or less out of work—the result of a series of hernia repairs.

After leaving Durban she had become prone to attacks of severe "tetanic-type" spasm at irregular intervals. There was no obvious precipitating factor except possibly

prolonged muscular contraction. I was frequently in a position to witness the onset of these. They occurred at hospital—in the course of her duties, at home, and on one memorable occasion in a crowded stadium during the visit of the Prime Minister. Attacks were initially of a carpo-pedal type, painful, and rapidly became generalized, almost to opisthotonus, causing great distress to the patient. I can recall them lasting over a period of up to three hours. They were unrelieved by intravenous calcium, opiates, or rebreathing. In extreme attacks a few ml. of intravenous sodium pentothal was found useful, as later was intravenous diazepam. I would stress that at no time can I recall witnessing over-breathing, overt or otherwise, in the early stages. With increasing distress gasping respirations did, not unexpectedly, occur.

Maintenance therapy with various calcium supplements did not appear to diminish the frequency of the attacks. Serial serum calcium and magnesium readings, etc., were always normal. Reference to specialist centres in Johannesburg shed no real further light on the diagnosis. Hysteria and over-breathing were often hinted at, though again the latter was not witnessed at onset. A detailed psychiatric assessment was not, as I recall, obtained, but to all outward appearances this lady did not seem the "hysterical type"—in so far as such a description applies.

Today, some years later, with hind-sight and current thinking to hand, I am still unable categorically to state that this lady's symptoms were functional. It would be interesting to know if others have any similar experiences in the follow up of those patients involved in the various "epidemics."—I am, etc.,

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SIR,—I feel compelled to write in reply to the letter by Dr. J. R. Wray (7 February, p. 363) who suggests that the outbreak of illness at the Royal Free Hospital, which has evoked so much interest, was due to epidemic winter vomiting.

The main feature of winter vomiting is, as its name implies, vomiting, which is often sudden in onset, and nearly always settles within 48 hours.¹ In an outbreak involving 50 cases² and in numerous cases seen in general practice²³ I have not found meningism or any other abnormal neurological signs. I therefore cannot believe that the epidemics described by Ash¹ and Haworth *et al.*⁵ quoted by your correspondent were due to epidemic winter vomiting, though they have been referred to in the past in discussions on this disease. I also cannot accept that the epidemic at the Royal Free Hospital, in which only 12% of the patients vomited, could have been due to epidemic vomiting.

Dr. Wray also suggests that in epidemic vomiting it is normal for all the patients to be ill on the same day and that the incubation period is sometimes less than one day. I believe the main feature which enables one to distinguish this condition from various forms of "food poisoning" is that cases occur in batches. In the boarding school epidemic described by myself² cases occurred over a