after 1952; some major factor other than bread consumption must therefore be involved. Neither can the downturn in mortality beginning about 1935-6 be explained by the rise in flour fibre content starting six years later in 1942.—We are, etc.,

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Clofibrate and Fat Embolism

SIR,—We were not surprised to learn from the report of Dr. W. G. Cole (16 October 1971, p. 148) that clofibrate did not significantly improve the morbidity of patients who developed fat emboli. The effect of clofibrate on blood lipids does not reach its maximum in rats until one or two weeks of treatment¹ and apparently even longer in man. Therefore, it would appear the only patients which one would expect to be aided by clofibrate would be those maintained on the drug for its hypolipaeic effect and who subsequently sustained a serious accident. Baker *et al.*² reported a study in animals which is open to the same question because their animals were treated only 24 hours before the production of the fat emboli, and because the drug was given intravenously, a route which could cause clofibrate embolism since the drug is water insoluble. Both these studies conclude that clofibrate is an ineffective drug for treatment of fat emboli.

We have conducted a study which overcomes these objections. Rats were fed clofibrate for three weeks before the production of fat emboli by the method of Baker *et al.*² using both triolein free of fatty acids³ and oleic acid (U.S.P.). Blood, obtained from the tip of the tail, was analysed for cholesterol⁴ and total lipids⁵ to demonstrate that the clofibrate had lowered the blood lipids in the treated group. Cholesterol was 153 ± 32 (S.D.) mg/100 ml in the controls, compared with 85 ± 17 (S.D.) mg/100 ml in the treated group ($P < 0.01$) and total blood lipid levels were 664 ± 135 (S.D.) mg/100 ml in controls compared with 430 ± 79 (S.D.) mg/100 ml in the treated ($P < 0.001$). The animals that died in the first few hours after the intravenous lipid were necropsied within one to two hours after death, and all animals

Lipid Injected	Groups	No. of animals	Cholesterol mg/100 ml Mean $\pm$ S.D.	Total Lipid mg/100 ml Mean $\pm$ S.D.	Pulmonary Surfactant Mean $\pm$ S.D.	Deaths	Haemorrhagic Lungs at 48 hr	Total with Proven Fat Emboli
Triolein	Control	10	161 $\pm$ 35	706 $\pm$ 144	0.570 $\pm$ 0.101	4 (40%)	1 (10%)	50%
	Clofibrate	11	86* $\pm$ 16	436* $\pm$ 86	0.613 $\pm$ 0.139	4 (36%)	1 (9%)	45%
Oleic Acid	Control	7	142 $\pm$ 26	604 $\pm$ 97	0.484 $\pm$ 0.076	2 (28%)	1 (14%)	42%
	Clofibrate	7	84* $\pm$ 19	421* $\pm$ 73	0.567 $\pm$ 0.094	3 (43%)	2 (28%)	71%

*Significantly different at $P < 0.001$ level from the control group.

alive after 48 hours were killed and searched for evidence of grossly haemorrhagic lungs, which was taken as positive evidence of marked fat embolism. Lung biopsies from all animals of both groups were analysed for pulmonary surfactant.⁶ The results of this experiment are as in the above table.

Our results confirm in a more realistic and therapeutic situation the previous findings that clofibrate cannot be expected to improve the morbidity or mortality of patients who develop fat emboli following trauma. We also find that clofibrate treatment does not significantly alter pulmonary surfactant in the rat and that there is no difference of significance in rats sustaining fat emboli.

We wish to thank Mr. Marion Johnson and Dr. J. Noble, Ayerst Laboratories, for the supply of Atomid-S and the National Institutes of Health, U.S.P.H., Bethesda, Maryland, for grants Nos. HE-11782 and HE-05432.

—We are, etc.,

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Wetting and Soiling

SIR,—We would like to comment on some of the points raised by Dr. A. C. Woodmansey (15 July, p. 161).

Firstly, in discussing "reactive incontinence," Dr. Woodmansey makes the common assumption that the reported association of incontinence with psychiatric symptoms is indicative of causation. It is, however, equally in accord with the evidence to regard the association as indicative solely of the expected coincidence of disorders similarly distributed in the population, or to regard the accompanying psychiatric disturbance as in a large part secondary to incontinence.¹ It is certainly not our experience to find a high proportion of emotional disorder in the enuretic or encopretic children who attend for treatment.

Dr. Woodmansey describes the enuresis alarm as "contrived" and "even traumatic," and considers that its aim "ought to be attainable by more appropriate and less dis-

tressing means." We absolutely disagree that the use of an alarm is traumatic or distressing, the number of cases in which the child fears an alarm after proper demonstration being virtually negligible. The vast majority of our patients, and their parents, are only too delighted to be relieved of so distressing a problem as bedwetting by such simple means. Dr. Woodmansey states that "many a child has suffered years of distressing tests and treatments . . . to appease a parent's unrealistic expectation of early sphincter control, or because his . . . wetting or soiling was not recognized as the 'presenting symptom' of his parent's own emotional disorder." We feel, however, that many patients could have been relieved of the constant distress of bedwetting had those they consulted been more ready to use the enuresis alarm.

Postulating that "in many cases incontinence appears to be a more indirect result of a specific fear—namely, that of actually being incontinent," Dr. Woodmansey claims that "the specific treatment of sleep wetting is to emancipate the child from his dread of waking in a wet bed." Unfortunately, there are no data supplied to indicate that such treatment may be comparable in success to treatment by an enuresis alarm, which apparently fails to satisfy the criterion of treatment by "improving relationships." Where "advice" fails, Dr. Woodmansey recommends the use of "psychotherapeutic skill to overcome their [the parents'] anxious doubts, or to alleviate a parental disturbance whose existence the child's symptoms (though undoubtedly important in themselves) have served to bring to light." However, while the success of conditioning treatment has been well demonstrated,² it has yet to be shown that psychotherapy is of significant value in the treatment of enuresis.

From the patient's point of view, rejection of conditioning treatment in consequence of a therapeutic reformulation unsupported by data relating to any more effective form of treatment cannot be acceptable. We strongly recommend the wider adoption of conditioning treatment as the most effective presently available treatment for childhood enuresis.—We are, etc.,

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Puzzling Cases of Secondary Syphilis

SIR,—I was interested in your leading article on "Puzzling Cases of Secondary Syphilis" (8 July, p. 66).

One of the most important diagnostic hall

marks is a generalized *non-itching* rash—indeed, I saw a girl of 17 recently who was pregnant, had had a sore throat a week earlier, and had developed a non-itching generalized rash. I sent her to our V.D. department with a confident diagnosis of early secondary syphilide which was confirmed by the consultant.

I was taught that all non-itching rashes should prompt one to remember this possibility.—I am, etc.,

I. ROSE

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Particles in Spleens

SIR,—Drs. M. Tavassoli and R. Judith Ratzan (1 April, p. 48) have suggested that the unusual particles described in spleens by my colleagues and myself (18 December 1971, p. 721) were platelet lysosomes. We have been reluctant to accept this explanation because our particles are much smaller than lysosomes (Figs. 1 and 2). We agree that lysosomes have a structure which resembles some virus particles and indeed pointed this out in our original communication. We used the euphemistic term "unusual particle" because of the uncertainties shared between ourselves and others over cell organelles which mimic virus. These organelles include a range of structures quite apart from the lysosomes mentioned by Drs. Tavassoli and Ratzan. They include secretory granules of endocrine glands, the darkly staining particles of the juxta-glomerular junction, and bristle coated vesicles. Another source of difficulty, which I have not so far

found in the literature, is the pinocytosis of electron dense reticular cell cytoplasm by adjacent lymphocytes (Fig. 3). Nevertheless, my first impression and indeed, persisting impression, of these particles is their noteworthy resemblance to particles described as virus by Schwartzendruber *et al.*¹ in a mouse strain with a high leukaemic incidence, by Gross² in transmissible murine leukaemia, and by Dmochowski³ in human neoplasms.

FIG. 3—"Budding" reticular cell showing extrusion of darkly stained vesicles into cytoplasm of adjacent lymphocytes. $\times 23,125$.

Some may say why show concern over virus in the spleen.³ After all virus is ubiquitous, and even when found who can say whether it is passenger or driver. It seems better to formulate a hypothesis rather than botanize amidst the aridity of structural detail. Our working hypothesis is that a diversity of auto-destructive haematological conditions may be due to virus particles which render cell membranes auto-antigenic, and manifestations depending on which particular stem cell has been infected. Furthermore, the presence of such virus in healthy people would indicate not lack of pathogenicity on the part of the particle but tolerance on the part of the host, production of disease depending on the equilibrium between tolerance and immunity to the agent.

The conclusion drawn by myself and colleagues is not that these particles are lysosomes or cell organelles, but rather that they are sufficiently intriguing to warrant a detailed investigation.—I am, etc.,

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² Gross, L., *Acta Haematologica*, 1964, 32, 44.

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Community Mental Health

SIR,—It is still frequently believed that the management and treatment of mental patients in the community has to be directed mainly from the hospital and it is disheartening that even some of our own colleagues do not seem at all well acquainted with the important contributions community psychiatrists and social workers make towards psychological medicine in general. One of the main deficiencies of the present system

appears to be the sharp division between the hospital and community mental health services and their facilities. The one is usually not responsible for the other, a system which creates tremendous difficulties especially when a patient has to be admitted and the hospital or community establishment, sometimes both, refuse to accept him with seemingly little concern for the fate of the patient.

I would like to propose the establishment of a career grade—namely, that of the community psychiatrist with the status of a consultant. His functions should include taking charge from a psychiatric point of view of all mental health facilities in the community, give advice on the welfare and needs of patients contained in them, and, equally important, counsel staff on the management of the clients under their care. This would be a most onerous task and should take the greater part of his time.

He would also advise the local authority on improvements, extensions, and new facilities required for present and future community psychiatric needs, and work closely with those responsible for planning on the hospital side. As an extension of this, he would be in close contact with colleagues in other branches of psychiatry and medicine, whether in hospital or community, in order to participate in a uniformly agreed management and placement of mental patients.

Some of his time would be set aside for research into the dynamics of psychiatric illnesses as these affect people in the community with a view to evaluating appropriate prophylactic measures and where possible suggest more suitable management and treatments. A better acquaintance with the patient's environment and a knowledge of suitable alternate placement creates for the community psychiatrist an advantageous yet impartial position from which to advise on appropriate placement, should admission to one or other establishment be refused and the patient left to the often unsatisfactory devices of his family.

To second a psychiatrist for one or two sessions a week to the community services is, in my view, of little use. The proposed appointment should allow for at least half of the sessions of a full or maximum part-time employment to be spent with the community services. It would be equally unproductive, however, if all the psychiatrist's sessions were relegated to the community at the exclusion of hospital work. The suggested ratio of commitment between community and hospital with emphasis on community work should meet the requirements of advice on psychiatric illnesses in the community. In my own area such a scheme, although not fully implemented, has been in operation for a number of years, and seems to have met with some success.

It is quite unrealistic to expect a generic social worker to undertake the very specialized and often difficult task of caring for mental patients in the community. Social work in psychiatry plays a very significant role and I can see no alternative but to amalgamate once again psychiatric social workers with psychiatrists whether engaged in the community or hospital, a suggestion which is in accord with recent recommendations of a tripartite committee on closer association between personnel working in the field of mental health.¹

Time is running out and unless some

FIG. 1—Unusual particles—virus like—in spleen from a case of ITF. $\times 23,125$.

FIG. 2—Platelet lysosomes $\times 23,125$.