episodes, dyskinetic movement disorder, and adoption as a drug of abuse, may all be added to the record of a drug which has already been associated with a small number of fatalities. As with other drugs introduced as substitutes for the amphetamines, claims have been made that fenfluramine is a drug of low toxicity which does not stimulate the central nervous system. These assertions can no longer be sustained, and it is imperative that the same caution be exercised in the use of this drug as would be appropriate to other central nervous stimulants.—I am, etc.,

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1 Ashcroft, G. W., Eccleston, D., and Waddell, J. L., *British Medical Journal*, 1965, **1**, 57.
2 Sigwald, J., Bouttier, D., and Courvoisier, S., *Revue Neurologique*, 1959, **100**, 553.
3 Crane, G. E., *American Journal of Psychiatry*, 1968, **124**, Supplement 40.

SIR,—I read with interest Dr. M. Y. Alvi's report of nightmares described by a patient who had been taking fenfluramine as an aid to reducing weight (25 October, p. 237).

Today I saw a female patient aged 45 for whom I had prescribed fenfluramine to be taken morning and evening five days ago. She complained bitterly of extreme sleepiness and lethargy during the three days that she persisted in taking it, and indeed she claimed that apart from brief waking spells she slept for 24 hours after the first day's tablets, and was totally unable to get up to prepare her husband's breakfast and lunch on the other two occasions. On stopping the tablets at her own discretion she experienced a normal night's rest with a return of her normal daytime alertness. I would point out that she is a well-balanced, rather phlegmatic individual and has never exhibited any symptoms of anxiety, depression, or hysteria.—I am, etc.,

GERALD ELLIS.

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Avian-Battey Mycobacteria Infection

SIR,—In view of the increasing interest in atypical mycobacterial infections and the recent article on Avian–Battey group infection in England and Wales (17 May, p. 412) and the evidence from Western Australia (2 August, p. 300) our considerable experience with Battey infection in Florida may be of some interest, and I enclose a Table derived from our register of excreters since 1962.

The marked increase in the number of excreters was due to the replacement of the old sputum digestion technique by the much less abrasive Zephiran trisodium phosphate sputum digestion technique in June 1964. In 1965, the first full year with the new technique, a 200% increase in the total number of excreters occurred. It will be noted that there was only a small increase in the number of *M. kansasii* group I excreters, as indeed there was in the number of *M. tuberculosis* excreters. The increase in atypical mycobacterial cultures was from 1·6% to 6·2%, and in *M. tuberculosis* cultures from 3·8% to 4·8%. An additional significant finding was that 8·4% of specimens yielding negative smears gave positive cultures, indicating the unreliability of the negative smear report. Battey infection is not uncommon in Florida and accounts for some 4% of tuberculosis hospital admissions, although many cases are treated privately. Battey infections are largely resistant to normal therapy, but in-vitro tests suggest that Rifampin may be more effective.

It is of course true that thanks to chemotherapy the community now includes old tuberculous cases, many with a residual fibrosis, itself a predisposing factor to the acquisition of atypical infections. A recent analysis of Battey cases in Florida revealed that, apart from *M. tuberculosis* infection, emphysema, chronic bronchitis, asthma, bronchiectasis, diabetes, and gastrectomy were predisposing causes actually diagnosed in well over 30% of cases. In some areas silicosis would increase this percentage. Similar figures were obtained for the *M. kansasii* cases.

It would seem obvious, therefore, that although *M. tuberculosis* infection may be a considerable predisposing cause to the acquisition of atypical infection it is so in only some 30% of atypical cases. The other causes of pulmonary fibrosis, idiopathic and otherwise, together account for the majority of cases. The aetiological relationship of the atypical infection to the pulmonary fibrosis is largely undetermined, and the incubation period for these infections is quite unknown. Schaefer (personal communication) has recently confirmed two strains of *M. intracellulare* (Battey bacillus) isolated from Florida soil as Boone and Yandle serotypes, both frequently implicated in cases of human pulmonary mycobacteriosis.

Further information on the biochemical tests differentiating the potential pathogens and common saprophytes in each of Runyon's groups of atypical organisms will shortly appear in the *Southern Medical Journal*.

These concluding Florida studies have been supported in part by Grant 5R01, CC00062-10, Department of Health, Education and Welfare.

—I am, etc.,

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Florida State Board of
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Management of Infants with Cerebral Palsy

SIR,—The balanced discussion by Dr. J. Wilson on the management of children with cerebral palsy (18 and 25 October, pp. 152 and 211) is obviously intended for the guidance of the family doctor. May I ask for clarification of several points arising from his discussion?

What immediate plans have been made for setting up and medically staffing the urgently needed district assessment centres (as distinct from one or two research centres)? It is obvious that highly skilled specialists are essential to appraise the complex neurological and mental problems which are so commonly found associated with the palsy.

Where does one find the centres he mentions, where it is "usually possible to make arrangements for . . . a preschool programme of treatment" to provide "passive stimulation and movement and an awakening of interest in under-used limbs"?

Where neither of these facilities are available, why are parents of such infants discouraged (as Dr. Wilson admits they usually are) from seeking the highly specialized treatment methods which provide sensory stimulation, play, and functional patterning? Such treatment must, at the very least, provide general stimulation of a child whose "inability to explore his environment may add sensory and social deprivation to his physical problems." I am sure that Dr. Wilson, like myself, must have seen good results from two well-known methods of such treatment, which aim at return to normality in some cases with the help of intensive therapy by the parents. (Neither of these methods is unrealistic enough to claim return to normality as its usual goal.)

Does objection to the methods exist as a consequence of the involvement of parents in the treatment? A sense of guilt may indeed be felt by parents who have been persuaded to undertake a laborious method and become discouraged. In other parents the advice of folded hands while awaiting spontaneous progress or institutionalization may fit in with deep-seated rejection feelings. Nevertheless, there is a considerable number of parents (particularly those who have seen a capacity to learn in their child) in whom a policy of inactivity encourages a deep sense of suspicion of their advisers, and of impotent unease. This can be even more frustrating than the sense of guilt felt by those who have tried and failed. Surely we should assess the emotional attitudes of both parents as carefully as we should make a realistic neurological assessment of the infant's capacity to progress.

May I take the liberty of quoting Dr. Wilson's words in a somewhat different context: "exchange of correspondence, even if faithfully undertaken, is only second best, and

Identified Florida Excreters of Unclassified Mycobacteria. Analysis by U.M. Group and Year

U.M. Group	1962		1963		1964		1965		1966		1967		1968	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
I	24	4·3	26	5·1	22	2·6	26	1·7	46	2·8	39	3·7	66	10·0
II	138	25·0	135	26·3	391	45·5	728	46·2	880	53·1	364	34·6	228	34·8
III	274	49·7	264	51·5	249	28·9	510	43·4	410	24·7	225	21·5	155	23·6
IV	99	17·9	72	14·0	161	18·7	263	16·7	276	16·7	365	34·7	175	26·7
Unknown ..	17	3·1	16	3·1	37	4·3	47	3·0	45	2·7	58	5·5	32	4·9
Total ..	552	100·0	513	100·0	860	100·0	1,574	100·0	1,657	100·0	1,051	100·0	656	100·0

June 1964, Zephiran trisodium phosphate sputum digestion technique instituted.
January 1, 1967. Excreters of Tween hydrolysis positive organisms of Group II and Group III dropped from register.

does not provide a forum for the discussion which is essential for managing complex conditions." There is increasing professional and public interest in this subject, and it is to be hoped that my questions will stimulate open discussion by other practitioners of what has hitherto been an esoteric field.—I am, etc.,

BASIL A. STOLL

London N.W.11.

Labelling of Poisonous Commercial Preparations

SIR,—I was pleased to read (*Supplement*, 4 October, p. 1) that the B.M.A. Public Health Committee had taken up the question of labelling of commercial preparations with the Home Office, and that despite its adverse reply they were going to pursue the matter further with the Home Office.

The three factors which were put forward by the Home Office as influencing its decision not to carry out the B.M.A.'s recommendations are quite indefensible. Some answers to these factors are as follows. The extent to which labelling is already required under statutory schemes is extremely limited. There are many poisonous substances present in household preparations which are exempted from the Pharmacy and Poisons Acts, particularly by Schedule III but also by Schedule II of these Acts. In addition the majority of the common poisoning agents such as ethylene glycol, carbon tetrachloride, turpentine, paraffin, strong sodium hypochlorite solutions, and methyl ethyl ketone peroxide, to mention only a few, are not even mentioned in these Acts. For example, ethylene glycol (L.D. adults 3–4 fl. oz. (100 ml.) and therefore much less for a child) is a common constituent of motor-car anti-freeze preparations and brake fluids, and these must be present in millions of homes throughout this country with nothing on the labels to warn people that these preparations are dangerous. The same goes for many other poisonous substances present in common commercial preparations which are routinely used in millions of homes in this country. Voluntary schemes of labelling are useless. There was recently[1] a report even in the non-medical journal *Which* highlighting the lack of implementation of the so-called voluntary scheme of labelling which had been agreed to for insecticidal preparations.

The "excellence" of the service made available to doctors in this country by the poisons information services, whether national, regional, or any other,[2] does not even begin to cope with the problem of accidental poisoning of children by commercial products in the home. These services come into the picture only after the poisoning has occurred. Parents in this country should receive the same consideration given to parents in other countries such as Sweden and the U.S.A., where the labels of the containers of commercial preparations containing dangerous substances have this fact clearly stated on the label in one form or another so that care can be taken to keep these particular preparations locked away well out of the reach of young children. These countries also put the name of the dangerous substance present on the label, while in addition Sweden adds a "poisons symbol" in the form of a skull.

In answer to the third point that the Home Office put forward it should be clearly stated and understood now that *no* practical difficulties would be presented if legislation requiring all toxic substances to be labelled is contemplated. It has been done in several countries including Sweden and the U.S.A., and in fact Sweden implemented its own legislation relating to this matter as recently as July 1967.

If the Home Office and its advisers find difficulty in producing lists of the dangerous substances which require cautionary labelling, then I for one, and I am sure many other doctors, would be very willing to help compile such lists. Incidentally, when firms export commercial products containing dangerous substances to such countries as Sweden and U.S.A. the firms comply with the rigid cautionary labelling requirements of these countries without any trouble. I have also had an example of a commercial preparation which was put out on to the British and American markets at the same time by one firm and found that those on the American market had a cautionary label on them while those on sale here had no cautionary label at all.

I sincerely hope that the Public Health Committee will continue to pursue this matter further with the Home Office until a satisfactory form of labelling is agreed to. My experience and that of my colleagues indicates that there would be overwhelming medical and public support for such a course of action by the B.M.A.—I am, etc.,

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1 Davidson, S., *The Principles and Practice of Medicine*, 9th ed., 1968, p. 91. Edinburgh, Livingstone.
2 *Which ?*, July, 1969, p. 202.

Authorship of Medical Publications

SIR,—In recent years the standards of scientific ethics of authorship of medical publications has fallen. There are two common sins. One is for the name of one or more of the major participants in the research to be either omitted completely from the paper or to be acknowledged at the end with a mere "thanks for help." The other is for the names of one or more authors to be attached to a paper when they may neither have taken an active part in the research nor collaborated in the writing, nor even seen the typescript submitted for publication. Even the order of names (which often does have significance) may not truly reflect the relative contributions of the authors. Occasionally papers are seen which obviously lean heavily on the work of other departments, but where no acknowledgement is made.

Unjustifiable investigations on humans have become fewer since editors of medical journals made the specific decision to refuse to publish articles which did not specify the informed consent of the subject. I am writing to suggest that medical editors could equally diminish, if not eliminate, the problems of authorship attribution. They should require at the submission of a paper for publication a declaration signed by *all* authors that they have approved the submitted typescript, and the names and order of the authors, and that they have not omitted the names of any participants in the work.

The victims of these offences usually find it difficult to complain publicly (or even privately) without grave embarrassment.—I am, etc.,

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Contraceptives and Cervical Carcinoma

SIR,—I am obliged to Dr. Myron R. Melamed (1 November, p. 302) for pointing out my misinterpretation (23 August, p. 471) of Table I of the paper by him and his colleagues (26 July, p. 195). His explanation does indeed clarify the matter, which, I submit, was not put as clearly as it might have been, and which I suspect may well have been misinterpreted by others than myself.

However, even allowing for the doubts which could exist about the accuracy of recalled information on previous contraceptive practice by women before they entered the study, data on prevalence rates of carcinoma in situ at the time of entry into the study would still be of considerable interest. If, for example, they showed a significant difference between the two groups (those choosing oral contraceptives and those choosing the diaphragm), they would raise the suspicion of heterogeneity, such as I had, apparently mistakenly, inferred. Perhaps Dr. Melamed could provide the figures for prevalence, at the time of entry to the study, for the two groups of women concerned.—I am, etc.,

G. I. M. SWYER.

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Sugar and Ischaemic Heart Disease

SIR,—Some of the points raised in the letter of Professor J. Yudkin and Dr. R. H. J. Watson (11 October, p. 110) demand an answer, notwithstanding the fact that 12 years after the publication of Professor Yudkin's hypothesis the only support he cites from trials in man is restricted to two papers from his own team. On the other hand, five very recent papers[1–5] have failed to substantiate this hypothesis.

Professor Yudkin and Dr. Watson appear to attach great importance to the question of changing sugar habits with age. We considered that age-standardization would have introduced complications for readers, unjustifiably, in view of its insignificant effect in our series, where intake tended to fall slightly with age in both groups. A study of Table V in our article (19 July, p. 145) supports this view, and the Fig. giving mean weekly sugar intake by age again shows no pattern of increased consumption in the ischaemic heart disease (I.H.D.) group. The experience of those with unchanged sugar habits is also not significantly affected by age-standardization. Similarly, there were minor differences in sugar intake by social class, but since class-standardization did not substantially affect the results we again refrained from complicating the Tables. The Table overleaf quite clearly supports this view.