tives, usually poor palliatives at that, and in these circumstances any dosage carrying a 16% mortality rate is far too high. During 1959-61 I treated well over a hundred cases with "thiotepa." Roughly one-third were given the high dosage (along with testosterone propionate) recommended by Whyte Watson and Turner,¹ the remainder receiving about half this dosage. This experience convinced me that maximal doses do more harm than good. Dr. Basil A. Stoll is right in stressing the harmful effects of injudicious chemotherapy.² He writes of the "dangers of piling misery upon misery in the late cancer case."—I am, etc.,

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REFERENCES

- ¹ Whyte Watson, G., and Turner, R. L., *Brit. med. J.*, 1959, 1, 1315.
² Stoll, B. A., *ibid.*, 1962, 2, 1128.

Never on Sunday

SIR,—For the last ten years I have made a practice of giving most hospital patients on continuing doses of such drugs as chlorpromazine ("largactil") and thioridazine ("melleril") a "drug holiday" every Sunday.

The original idea was to give the patients' bodies a break from continual bombardment with drugs which, though producing valuable benefits, are far from being entirely benign.

Experience has not indicated that any of these patients suffered any resulting increase of disturbed behaviour on this day or other days; and this was to be expected in view of the evidence for continuing effect of these drugs for considerable periods after they have been entirely stopped. Likewise stabilized epileptics on more than one anticonvulsant have had one of the anticonvulsants stopped Sundays and the other on Wednesdays to try to reduce undesirable cumulative, etc., side-effects of the drugs. These patients did not show any notable increase in fits, due to this, or extra disturbance, on these days. None of the group developed jaundice or other serious complications (except Parkinsonism), although, of course, this may be pure chance.

In view of the present economy drive one wonders whether others have had the same experience and whether this practice might legitimately be worth extending. The practice is, of course, long established in digitalis therapy.—I am, etc.,

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H. JACOBS.

Pseudoagglutination

SIR,—It was necessary to crossmatch a group A rhesus-positive 2-day-old female child prior to laparotomy for intestinal obstruction due to a congenital abnormality of the gut. The child was mildly jaundiced but there was no evidence of haemolytic disease, the direct Coombs test was negative, and the maternal serum failed to haemolyse the baby's cells. The mother was group O rhesus positive and her serum was absorbed according to the procedure described by Dunsford and Bowley,¹ using 1:1, 1:2, 1:3 amounts of maternal serum to AB substance. It was found that the donor cells were compatible using the 1:1 absorbed maternal serum, but the 1:2 and 1:3 absorbates showed pseudoagglutination in albumin and saline most marked in the 1:3 absorbate.

It would appear that this phenomenon of pseudoagglutination was due to anti-B substance retained with

the donor cells which precipitated the B substance in the absorbed maternal serum, producing pseudoagglutination of the donor cells. The increased amount of pseudoagglutination present in the 1:3 absorbate reflected the increased amount of B substance present. This phenomenon was not obtained when the donor cells were washed three times with saline before being used in the compatibility test. The indirect Coombs test applied to the donor cells using maternal absorbed serum was in all cases negative.

It would appear advisable to wash donor cells three times in saline before being used in a compatibility test against absorbed maternal serum.—We are, etc.,

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DIANA HORTON.

REFERENCE

- ¹ Dunsford, I., and Bowley, C. C., *Techniques in Blood Grouping*, 1955. Oliver and Boyd, Edinburgh.

Aetiology of Peptic Ulcer

SIR,—With regard to the review of my book on *Peptic Ulcer* in your issue of January 26 (p. 250) the only criticism the reviewer appears able to make is that the work does not take cognizance of other possible explanations of the causation of the disease, based on the same epidemiological evidence. This evidence derives not only from Europe, Africa, India, Indonesia, China, and Japan, but also from such widely different sources as the Japanese prisoner-of-war camps in the Far East, the German army before Stalingrad, and the German prisoners of war in Russia itself. The whole point in referring in the work to this evidence and to the exceedingly complicated pattern of incidence that results from it was to show that the only cause that *can* be reconciled with it is the consumption of refined, protein-stripped carbohydrates, ranging from refined grains to refined sugar. Directly these appear the disease becomes common; directly they disappear it vanishes.

It is submitted, in short, not only that no other cause for the disease could have been advanced, but also that no other can be advanced. For example, if a "stress" cause be put forward, how is it that peptic ulcer disappeared from the 6,000 prisoners of war in Tokyo during the 18 months when that city was almost annihilated by air raids? These men were fed on crude, unpolished grains, unlike the Japanese themselves, who were fed on polished grains and in whom the incidence of the disease doubled. Or why did peptic ulcer disappear from the German army before Stalingrad, where in the severest conditions of climate and violence half a million men perished? These men were reduced to so primitive a diet that it often consisted of raw turnips and potatoes, taken out of the fields. And much the same applies to the German prisoners of war in Russia, where the disease also disappeared on a crude, unrefined food structure. Meanwhile, in Singapore, where calmer conditions prevailed but where the prisoners were fed on polished rice, duodenal ulcers, to quote from the official history of the war, became a plague. And just as a stress cause is quite incompatible with the extraordinary incidence pattern, so, it is submitted, is any alternative cause other than the one given. If this is not the case, can such cause please be advanced?

As regards the reviewer's suggestion that further epidemiological studies be instituted, what on earth would be the value of these, observing the vast amount of material already available, compared with the value of extending to ulcer sufferers right away the advan-