basis. Platelet counts are not included in the reported data and in our experience this investigation is a useful index of intravascular coagulation. In the two cases in our own series in which we suspected the syndrome clinically before death there was a profound drop in the platelet count and this was associated with an elevated level of serum fibrin degradation products and evidence of defective fibrinolysis.

Dr. Anderson discusses various explanations for his findings, including dehydration, cerebral anoxia, and acidosis of the cerebrospinal fluid. He implies that metabolic factors are probably more important than infarction or haemorrhage as the latter are not evident. It is clear from our own studies that this is not the case and that multiple ischaemic lesions are seen in the brain which are not always easily recognized macroscopically. Recovery from the abnormal neurological state is explicable on the basis of removal of the intravascular fibrin by fibrinolysis.

Dr. Anderson's first patient had a history of head injury just before admission and in Mr. Maurice-Williams's paper a patient is described with a history of mild head injury followed by progressive neurological deterioration and death. We have previously drawn attention² to the dangers of intravascular coagulation occurring as a complication of head injury. The vascular thromboses are not confined to the larger vessels and microscopical examination will often reveal thrombi within the microvasculature of the brain and other organs.

We wish to reiterate that involvement of the brain by intravascular coagulation is now known to complicate a variety of clinicopathological states; it is important that it should be recognized as treatment should include measures to combat it.—We are, etc.,

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¹ Timperley, W. R., Preston, F. E., and Ward, J. D., *Lancet*, 1974, i, 952.

² Preston, F. E., *Journal of Neurology, Neurosurgery and Psychiatry*, 1974, 37, 241.

Drugs for Gastric Ulceration

SIR,—I write again in defence of your excellent leading article on this subject (27 April, p. 186).

May I ask your correspondent Dr. S. M. M. Karim (20 July, p. 169) to reread your article? He will then find that the "carefully conducted Scandinavian trial which showed no demonstrable effect" refers to deglycyrrhizinized liquorice (Caved-S) and not to carbenoxolone as he has stated in his letter. This may have misled some of your readers who were not already familiar with this excellent study by Engqvist *et al.*¹ This inaccurate quotation undermines confidence in some of his other references which are not yet available for verification.—I am, etc.,

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¹ Engqvist, A., von Feilitzen, F., Pyk, E., and Reichard, H., *Gut*, 1973, 14, 711.

Oral Contraceptives and Health

SIR,—I am grateful for Dr. Ellen C. G. Grant's letter (13 July, p. 115) because she reveals a misunderstanding about loss rates which may be widespread among workers who are accustomed to prospective evaluation of contraceptives in clinic subjects. The situation in a general practice study is entirely different. Here all but a tiny proportion of the losses are due to the patient moving out of the district and registering with a new doctor. There is no reason to believe that this event is likely to be associated with the onset of an illness or the decision to stop the pill. In the Royal College of General Practitioners study¹ it was concluded that the loss of subjects, though substantial, was unlikely to have resulted in any material bias (chap. 3).

I do not know how Dr. Grant has determined that "only" 4,711 women were still taking the pill at the time of the analyses. In fact the correct number, in round figures, was 9,000. Here again, Dr. Grant's implication that the high proportion of women who stopped the pill invalidates the study must be based on her clinic experience. These women remained under observation as extanters.

The College's study is not "blind." Doctors and patients both know whether or not the pill is being used. In these circumstances biased reporting must be expected. In chapter 3 we give extensive and detailed consideration to the effect of this bias and show that it leads almost exclusively to over-reporting in takers. The average bias in all categories of illness is estimated to lead to a 19% excess reporting in takers. In some morbidity categories there are good reasons for believing that the bias is very small and in others we must expect it to be much above average. Depression is particularly likely to be subject to above-average biased reporting, since it is common, subjective, and frequently never reported to doctors. In these circumstances it came as a surprise to find that the excess reports in takers were as little as 30%. We were unable to show a relationship in pill users to the progestogen or oestrogen content of the pill, or to the duration of use. In these circumstances we concluded that "there is approximately a 30% maximum increase in pill users as compared with the controls. . . . However, a substantial bias is likely to be present, and a pharmacological influence of the pill may be much less than these observed differences."

Dr. Grant has misinterpreted the results of the statistical test. The test can indicate that an observed difference is unlikely to be due to chance. It gives no guide as to the likelihood of the difference being due to bias, since bias is not a chance phenomenon. It is presumably because of this misunderstanding that Dr. Grant suggests that we have found that the pill adversely affects 60 conditions, whereas we list only 25 morbidity categories on which we believe the pill has a possible adverse effect and several of these are likely to be subject to substantial biased reporting (fig. 13-1). In any case it is meaningless simply to count categories without taking into consideration the severity of the conditions and the excess incidence in pill users attributable to oral contraceptives. It is remarkable that Dr. Grant should be so concerned about the bias due to loss of subjects, which is trivial, and ignores completely

the very important influence of biased reporting.

Whenever a doctor prescribes the pill, and whenever a woman uses it, both parties are accepting that the advantages of oral contraceptives outweigh the risks. If a doctor does not believe this he has no right to prescribe the pill. Our report is based on the observation of 35,000 woman-years of pill usage and monitors the whole range of reported morbidity. It is the most comprehensive report on the effects of the pill yet published. While it confirms most of the previously reported adverse effects of the pill, it shows that some of these are of less importance than was previously supposed. It also shows that it has some beneficial effect on health. It provides very reassuring data on the outcome of pregnancy in previous oral contraceptive users and on subsequent fertility. Putting all this evidence into perspective the report must be interpreted as confirming the long-held view that the advantages of pill usage outweigh the disadvantages. This must, however, be subject to the proviso that "longer observation might reveal other untoward effects." I believe that the press very accurately reported these implications, though I must agree that some of the headlines were less objective than I would have wished. Nowhere does the report suggest that the pill is better than other contraceptives, and it is my personal belief that couples must be offered a range from which they can choose the method most acceptable to them. Nor does the report imply that doctors need now be less alert about the possibility of adverse effects amongst pill users.—I am, etc.,

CLIFFORD KAY

Recorder,

R.C.G.P. Oral Contraceptives Study

Manchester

¹ Royal College of General Practitioners, *Oral Contraceptives and Health*. London, Pitman Medical, 1974.

Conversations on the Social Services

SIR,—I feel sympathy for Mary S and her colleagues (6 July, p. 36) and, as a general practitioner, I feel guilty. However, I think there is a tendency for social workers to wring the last ounce of pathos out of dismal social situations and to discuss at length cases where neither they nor anyone else can suggest a remedy. Practical care tends to take second place to talk and correspondence.

What does Mary S say to this?—I am, etc.,

DENIS GLYN

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SIR,—My interested reading of this series has provoked the remarks enshrined in this letter and I hope these will stimulate further discussion on this most topical and important subject. I suggest that the eternal problem of communications or their lack is fundamental to the vexed question of relations between the old profession of medicine and the relatively new one of social services.

Having learnt from the advantages derived from the attachment of nurses and health visitors to general practitioners, we in the medical field tend to advocate a similar procedure aimed towards the improvement of relationships. I suggest that a study of the psychology of the situation demands a