Intal-Compound

SIR,—How does Intal (plain) become Intal Co. (Dr. P. Hugh-Jones and others, 7 November, p. 366)? All but a dozen or so of three to four hundred patients who have started treatment on Intal plain (disodium cromoglycate) in this department have returned within a few months taking Intal Co. Also, we have yet to see a patient who has started treatment elsewhere who is taking Intal plain. We understand that unless the word "plain" is included in the prescription Intal Co. is usually dispensed; neither do the manufacturers advertise Intal plain as being generally available.

We would agree that many patients are taking Intal Co. at great cost solely for the relief they get from the small amount of isoprenaline it contains. In those patients who do benefit from inhaling disodium cromoglycate there is no doubt that in many the effect is greatly improved by the prior inhalation of a bronchodilator. We have found that the long-acting salbutamol or oriprenaline is particularly effective in enhancing and prolonging the action and thereby reducing the frequency of the dose.—We are, etc.,

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Information Service on Occupational Health

SIR,—Just over two years ago an information and advisory service was established at this Institute. Its objectives were to provide doctors, trades unionists, managers, and others with factual information, informed advice, or field studies on problems in occupational medicine and hygiene.¹

Analysis of the 196 requests for information so far received shows that while industry in the form of management, unions, and medical officers provided 85 and universities and other research bodies a further 24, no less than 46 have come from National Health Service doctors. Most of these questions have come from general practitioners, but some from hospitals and local health authorities. Fifteen have been referred via the Poisons Reference Service at Guy's Hospital. Classification of the requests by their nature has shown that two-thirds have concerned a very wide range of chemicals including proprietary substances, while the remainder have screening examinations, the effects of noise included topics such as working conditions, or glare, employment problems of disabled individuals, and selected bibliographies.

The advisory service, to which queries are referred when they cannot be answered by factual information alone, has received 61 requests, 36 from industry and nine from the N.H.S., but in contrast to the information queries, most of these nine have come from medical officers of health. The problems have included an investigation of respiratory symptoms arising from the manufacture of an enzyme detergent powder,² a study of headaches and migraine in printing workers,³ surveys of organizations to assess

their needs for occupational health services, and a number of environmental dust, solvent, vapour, and other measurements.

The policy of the Institute is that these services should attempt to become financially self-supporting. A charge is therefore made whenever possible to organizations other than trades unions, who already support the Institute through the T.U.C. grant to the London School of Hygiene and Tropical Medicine. In practice however—for various reasons—146 of the information queries, amongst them those from N.H.S. doctors, have been answered without charge, and the income from this source has been £330. This compares sadly with the £1,000 spent on reference material alone. The advisory service has been rather more successful, having earned nearly £7,000 from 30 sources.

In addition to expanding the existing services, future plans include educational activities directed at the large number of non-professional people who deal with problems of occupational health in their daily life. The main problem however will be financial.

The provision of this type of service may not be generally accepted as a proper function of a university department, but it is already being done by others. Our experience indicates that it widens the scope of our teaching, produces new problems for research, and helps industry and doctors in the National Health Service to deal with problems related to the health of people at work.—I am, etc.,

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Depression and Oral Contraception

SIR,—Dr. J. T. Hart (7 November, p. 367) suggests that women who use a diaphragm as a contraceptive method are temperamentally more stable than women taking the pill. His criticism draws attention to the great difficulties of finding a suitable control population when investigating the side effects of the pill, but I do not think that it invalidates our findings (17 October, p. 142). Our results showed that 6.6% of women taking the pill were more severely depressed than the control group, and whether this was the result of a more unstable experimental group or the converse is surely less relevant than the conclusion that the pill-taking population is more likely to develop severe depression.

The need for careful supervision of women on the pill is emphasized, and doctors should look for mental changes as part of a regular examination.—I am, etc.,

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Postpartum Renal Failure

SIR,—Dr. I. Timor-Tritsch and his colleagues (24 October, p. 221) describe a patient with pregnancy toxæmia who showed evidence of intravascular coagulation and recovered normal renal function after treatment with heparin and repeated haemodialysis. On the basis of a statement that postpartum renal failure causes irreversible renal insufficiency in most, if not all, cases, and that there have been no reports of successful treatment, they suggest that treatment with heparin probably prevented permanent renal damage.

Acute renal failure is an important problem in obstetrics, and because of the current interest in intravascular coagulation as a possible aetiological factor, and consequently in heparin as a method of treatment, it seems important to question the authors' interpretation of this case. It has been our experience that acute renal failure associated with severe toxæmia, which is not complicated by abruptio placentae, nearly always recovers, provided of course the patient is not permitted to die from electrolyte imbalance in the acute phase. The following is a summary of a recent case very similar to the one described.

A 29-year-old primigravida was admitted in labour at term. The blood pressure was 140/90 mm.Hg, but she could not provide urine for examination. She had a spontaneous delivery of a healthy infant two hours later. One eclamptic fit occurred 12 hours after delivery when the blood pressure was 150/110 and the urine (300 ml.) obtained by catheter contained massive amounts of protein. Anuria persisted for eight days, during which haemodialysis was performed on four occasions. The patient was discharged from hospital 17 days after delivery, and renal function was normal at three months: serum creatinine 0.8 mg./100 ml., urea 29 mg., and electrolytes within normal limits. Mid-stream urine and intravenous pyelogram were normal.

When assessing the likely outcome of a case of acute renal failure in obstetrics it is of vital importance to advert to the clinical circumstances in which it occurred. It is essential not to evaluate treatment in cases which have a very high rate of natural cure.—We are, etc.,

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Postoperative Leg Vein Thrombosis

SIR,—Of course I agree with Mr. L. R. J. De Jode, and others (3 October, p. 56) and with Mr. G. F. Moloney and others (24 October, p. 244) that there are more causes of deep vein thrombosis than venous stasis. Broadly speaking venous thrombosis can be explained in terms of Virchow's triad: defects in the wall of the vein, a change in the chemical and cellular properties of the blood, and stagnation of the flow.

The number and anatomical sites of the defects in the walls of the deep veins of a patient's lower limbs cannot be known. We can neither discover whether there are, or are not, mural defects in the lower limb veins, let alone whether they are unilateral or bilateral. A little more can be found out about the chemical factors responsible for the clotting of blood and the increased stickiness of the platelets. The degree of

venous stasis which occurs is the only factor which can be measured. But even here there is a great difference between the rate of the axial flow, which in most people passes from the foot to the groin in 10-15 seconds,^{1,2} and the length of time pools of stagnant blood may lie in and around valve cusps and in the venous sinuses of the soleus muscle. The delay may be up to 27 minutes³ with a person lying horizontal, 45-50 minutes if manual intermittent positive pressure respiration is employed.⁴ The flow is also retarded by anything weakening the action of the heart, like halothane, fall in CO₂ pressure, etc.

In the face of this multiplicity of factors, the positive value of electrically stimulating the calf muscles during the operation needed a properly planned clinical trial making the fullest use of the 400 legs of 200 patients undergoing major surgical operations.⁵ The positive result of this trial has been confirmed by an even more carefully planned clinical trial using superior diagnostic measures.⁶ I am worried by the risk that, from the outset, the reputation of this simple procedure will be undermined by reports of trials too small to have any chance of showing a valid correlation, either positive or negative; and by other reports on improperly designed trials unlikely to yield a valid mathematical answer; and using a quite different method of stimulation, whose ability to maintain a normal rate of venous flow to the end of the operation has yet to be proved.

May I humbly ask those interested in this particular aspect of the problem to construct and conduct their trials with care or we will finish up in the same confusion as we are in over the proper treatment of early carcinoma of the breast, the correct way to repair an inguinal hernia, etc., etc.—a confusion which, in each case, is a result of neglect of basic statistical methods in clinical research.—I am, etc.,

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SIR.—The point made by Mr. G. E. Moloney and others (24 October, p. 244), concerning the use of controls in assessing the efficacy of electrical stimulation as a prophylactic against deep vein thrombosis, is well taken.

The original investigation by Doran, Drury, and Sivyver,¹ upon which most of the current work is based, did not, unfortunately, extend to a rigorous examination of the effect on the velocity of venous return in one leg of electrically stimulating the other. All subsequent work on the effects of electrical stimulation which make each patient his own control (by stimulating only one leg) are, therefore, open to doubt.

We have just concluded an investigation

into the effects on the volume flowrate in the femoral vein of rhythmic passive flexion of the foot. Passive flexion was chosen for two reasons. Firstly, because it mimics the effect of active exercise in producing compression of the calf veins. Secondly, because it is possible to exercise control over the rate and amplitude of the flexion with a precision that is unattainable using electrical stimulation. In our investigation, the volume flowrate in the femoral vein was measured proximal to the sapheno-femoral

junction using an electromagnetic flowmeter. Controlled passive flexion of the foot was produced by a specially constructed machine. In one part of the investigation flows were measured in both femoral veins while the foot of one leg was subjected to rhythmic passive flexion. In Figure 1 the sustained change in mean flow produced by passive flexion is shown as a function of the period of oscillation of the foot. The results show that the mean flow is increased by passive flexion—the faster the flexion, the greater the increase. Analysis of the points indicates that no increase in the mean flow can be expected when the period of oscillation is greater than 3 seconds. This may, in part, explain the success of some^{2,3} and the failure of others^{4,5} to prove the efficacy of electrical stimulation as a prophylactic against deep vein thrombosis. Figure 1 also shows that passive flexion of one leg produces an increase of flow in the other leg of approximately 5%, and that this

increase is independent of the period of oscillation of the flexed foot. This figure for control limb-flow change is similar to that found by us in an earlier investigation into the effects of external pressure on femoral vein flow.⁶

Another fact which emerged is that if the resting flow is greater than 180 ml./min. no increase can be expected (Fig. 2). The implication behind this result is important. If poor flow is the precursor to deep vein thrombosis then, from the results presented

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