other after heparin prophylaxis. Both women were over 75 years and had malignant disease. This incidence (0.48%) of fatal pulmonary embolism is not experienced in Wolverhampton, where 17 139 gynaecological operations of all types have been performed during the past five years, of which approximately 36% were major pelvic procedures, a fifth of which was radical cancer surgery. No specific routine prophylaxis was used.

In the last five years only three patients died in the first four weeks after operation from pulmonary embolus (confirmed at necropsy), an incidence of 1 in 5713. One patient died from peritonitis and basal pulmonary thrombi following a small bowel resection for intestinal obstruction associated with pelvic metastases. One died from massive pulmonary embolism and advanced corporal carcinoma seven days after drainage of a pyometra. The third died from pulmonary embolus after laparotomy for an inoperable pelvic tumour, later found to be bladder carcinoma at necropsy.

Whereas a constant watch should be kept for venous thrombosis and pulmonary embolus, specific chemical prophylaxis, with all its known (and in this paper demonstrable) hazards, should be avoided in gynaecological patients. However, age greater than 65 years, gross obesity, severe lower limb varicosities, pelvic cancer, and a history of thromboembolism will lead to a consideration of specific pharmacological prophylaxis, especially if the patient is on the operating table for over 60 minutes. Otherwise post-operative patients with suggestive symptoms should immediately be considered for intravenous heparin therapy.

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Intravascular thrombosis during air travel

SIR,—I was very interested to read the case report by Mr R E C Collins and others (8 December, p 1478) on femoral artery thrombosis after prolonged travel. Initiation of intra-arterial thrombosis during travel by air, especially in older subjects, may not be so uncommon.

Recently, I saw an Asian woman of 76 who felt her right leg going colder than the rest of her body a few hours after take-off on board a scheduled flight from Karachi to London. On arrival, she did not experience ischaemic pains or sensory changes. Her right foot was cooler and arterial pulses below femoral were weaker on the right side than the left. There was an audible bruit over the femoral artery. During the following four months, there was no marked change in the state of her ischaemic limb. Arteriography or surgery was not performed.

The interesting features of this case are that the woman developed the ischaemic symptoms within a relatively short period of travel and that, apart from her age, there were no predisposing factors. She was in excellent health prior to her journey and had no history of pre-existing arterial disease. She had been normotensive, a non-smoker, and a teetotaler. Apart from the occasional anti-inflammatory agents for moderate osteoarthritis of her knees, she was not on any medication and had never taken any steroids. Having lived all her life in the tropics, she was accustomed to keeping herself well hydrated.

What proportion of all cases of intravascular thrombosis develop the pathology during or soon after air travel? Is flying in itself a factor? Sudden changes in pressure,

oxygen and carbon dioxide concentrations, and temperature occurring inside aircrafts on ascent and descent may possibly affect the haemodynamics. The effects of low temperature and oxygen lack on sympathetic activity are well known. Blood pressure may also be affected by the altered plasma levels of catecholamines due to temperature changes¹ or to the apprehension of flying. Furthermore, it has been shown that a sudden drop in atmospheric temperature is associated with the initiation of arterial thrombosis in the elderly.²

Air fares are cheaper and many more people travel by air now than ever before; and if flying, with or without other predisposing factors, does promote the formation of intravascular thrombus we should be aware of this hazard.

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¹ Johnson DG, Hayward JS, Jacobs TP, Collis ML, Eckerson JD, Williams RH. *J Appl Physiol* 1977; 43:216-20.

² Bull GM, Morton J. *Age Ageing* 1975;4:19-31.

Massive arterial thrombosis and oral contraception

SIR,—We were interested to see the case report "Massive arterial thrombosis and oral contraception" by Drs P M Holt and D Hollanders (5 January, p 19), and would like to make the following comments.

Firstly, the authors state that arterial thrombosis is less often encountered in association with oestrogen-containing contraceptives than abnormalities of coagulation or venous thromboembolic diseases and that "these events [arterial thromboses] are relatively rare." However, in the report of the Royal College of General Practitioners study¹ there were 10 deaths attributed to arterial thrombosis (seven myocardial infarction, one acute myocardial insufficiency, and two mesenteric artery thrombosis) compared with one death attributed to venous thromboembolism (pulmonary embolus) in women who had used oral contraception, either currently or in the past. Similarly, of the nine cardiovascular deaths reported in the Oxford-FPA study,² all of which were in women who had at some time used oral contraceptives, five were directly attributable to arterial thrombosis (three myocardial infarction, one chronic ischaemic heart disease, and one precerebral artery occlusion), while there was none due to venous thromboembolism. Thus although, as Drs Holt and Hollanders suggest, clinicians may still be relatively more aware of the association of oral contraceptives with deep venous thrombosis and pulmonary embolism, arterial thrombosis accounts for a much greater proportion of the increased mortality which may be attributed to oral contraception.

Secondly, although the occurrence of massive arterial thrombosis may be unusual, in a study of all deaths during 1978 in England and Wales in women aged 16-44 years we have identified three women dying of widespread large artery thrombosis without venous involvement, including the woman reported by Drs Holt and Hollanders. The accompanying table summarises the presence of risk factors in these three cases (the information being derived from coroners, hospital records, general practitioners, and family planning clinics).

As can be seen, only one patient was using oral contraceptives (Eugynon 30) at the time of death. Case 2 had stopped using oral contraceptives seven years previously and case 3 had never used them. The only characteristic which these women appear to share is that they were all moderate or heavy cigarette smokers at the time of their death.

Thirdly, anecdotal case reports play an important role in suggesting new aetiological hypotheses and in directing the focus of subsequent epidemiological research. However, they do not allow the estimation of relative and attributable risks. Nevertheless, Drs Holt and Hollanders do attribute risk to the current use of oral contraception—both implicitly in the title of their paper and explicitly in their penultimate sentence: "Massive arterial thrombosis . . . represents another major complication of oral contraception." We do not believe that there is sufficient evidence yet available to justify this statement. The data from our study indicate that cigarette smoking may play at least as important a role as the use of oral contraception in the mortality from generalised arterial thrombosis.

We would like to thank Drs J O Davies, A D Gordon, and J R Leslie for providing information and for giving us permission to report their patients; the coroners and medical records officers for their help with our study; and Dr J I Mann for his advice. The study is supported by the medical Research Council.

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¹ Beral V, Kay CR. *Lancet* 1977;ii:727-31.

² Vessey MP, McPherson CK, Johnson B. *Lancet* 1977; ii:731-3.

Oral contraceptives and the prothrombin time

SIR,—Dr E De Teresa and others (17 November, p 1260) report that in 12 patients on long-term anticoagulation with warfarin the mean prothrombin time ratio was significantly higher, or the dose of oral anticoagulants required to get the same prothrombin time ratio was significantly lower, when they were also taking oral contraceptives. An opposite effect could be expected, on the assumption that some clotting factors tend to

Risk factors in three cases of massive arterial thrombosis

	Case 1 (reported by Holt and Hollanders)	Case 2	Case 3
Age	34	43	43
Smoking at death (cigarettes/day)	40	20	20
Hypertension	Present*	Absent	Absent
Nutritional state (from postmortem report)	"Very obese"	"Nutrition: good"	"Obese"
Hyperlipidaemia	Absent	Absent	Absent
Diabetes mellitus	No	No	No
Use of oral contraceptives	Current user	Ex-user	Never-user

*Blood pressure not recorded as abnormal until 11 months before death—then 125/85 mm Hg)

be increased during oral contraceptive treatment¹; the authors conclude that the level of the observed drug interaction remains to be clarified.

We have recently studied prothrombin complex activity in female rats treated with an oestrogen-progestogen combination (lynestrenol, 5 mg, and mestranol, 0.3 mg/kg body weight) at the lowest concentration which gave 100% infertility in this species. The Normotest clotting time was already significantly ($p < 0.001$) longer in oral-contraceptive-treated (37.7 ± 0.5 sec) than in control rats (31.0 ± 0.4) after treatment for only one oestral cycle, and the difference was even more marked after treatment for 10 cycles.²

Although, to our knowledge, a similar finding has not been reported in women on the pill, it could well be that only in special conditions, such as during anticoagulation, does the oestrogen-induced "lability" of the prothrombin complex, clear as such in rats, become apparent in humans. Alternatively, liver dysfunction, known to occur in women on the pill,³ could account for reduced metabolism of warfarin, thus making the anticoagulation more effective. Clinical pharmacology studies are warranted to substantiate hypotheses about the interesting interaction between oral anticoagulants and oral contraceptive drugs.

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² Roncaglioni MC, Pangrazzi I, Donati MB. *Thromb Haemost* 1980; in press.

³ Anonymous. *Br Med J* 1974;iv:430-1.

Psychoanalysis, psychotherapy, and health services

SIR,—Professor M Shepherd in his article "Psychoanalysis, psychotherapy, and health services" (15 December, p 1557) ends by commenting on the difficulty of validating the therapeutic claims of psychoanalysis. I suggest that this is not difficult but impossible and that the impossibility is inherent in any system of medicine that is exclusively dynamic in outlook.

This is an age-old problem and is illustrated by the experience of the German scientific school in the mid-nineteenth century. The Germans revolted against the sterile nihilism into which the French school had fallen. True, this school had classified many chronic illnesses on the basis of morbid anatomy and displayed a prognosis where there was little or none before. But it offered so little hope of cure that it was summarised by the sour comment that the acme of medicine was a diagnosis by Skoda confirmed at postmortem by Rokitansky. The Germans' slogan was "We want to cure, not classify." They pointed out that a knowledge of the pathogenesis of a patient's illness would reveal a chain of events in which some links might be seen to be breakable, and thereby suggest remedies. The last 100 years have shown how right they were; but success depended on one condition they soon found they had to accept, and that was that a classification had to be preserved if chaos was to be avoided. Psychoanalysis is a similar dynamic system, but the discipline of classification was ignored.

The argument is as follows. The effect of a cure cannot be judged unless it is known what would have happened without it. The only way of knowing what treatment has done to patient Z is to compare his state with patients A to Y, untreated, who had the same thing. "Having the same thing" is, of course, a classification, separating one group of patients from all others on the basis of criteria presumed to have some biological meaning. Hence the paradox that a nosological system can validate cures it cannot suggest while a dynamic system can suggest cures it cannot validate. The sterility of pure nosology is obvious enough to be a source of public mockery. The danger of the second is that hopeful therapy slides into unchecked fantasy, all the more beguiling because of its activity and seeming rationality.

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Antenatal diagnosis

SIR,—Your contributors Barbara J Culliton and Wallace K Waterfall, discussing antenatal diagnosis (8 December, p 1488), state that the alarming increase in the caesarean section rate in the USA must be attributed not to fetal monitoring but rather to more cases of cephalopelvic disproportion.

This does not sound plausible unless (a) obstructed labours were widely neglected prior to the increase in the section rate (surely unlikely); (b) an environmental factor has suddenly caused a considerable increase in the prevalence of acquired pelvic contraction (surely unlikely); or (c) the criteria for making a diagnosis of cephalopelvic disproportion have changed. It is naive to suppose that such a change in criteria would not have been influenced by fetal monitoring.

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An ABC of medical prejudice

SIR,—It seems Dr A Paton (22-29 December, p 1652) needs a geriatrician to rehabilitate patients he has treated. So why does he want to abolish geriatricians?

I wish him a happier New Year.

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Unwillingly to school

SIR,—Those of us who work with children will have been struck by the sense and sensibility manifest in Dr G F Goddard's special article (5 January, p 21). That most children don't like school most of the time has been obvious since Shakespeare drew our attention to the phenomenon ("creeping like snail unwillingly to school"); that their dislike may be based on something more than laziness and original sin has been pointed out by our greatest imaginative writers, Wordsworth ("shades of the prison house"), Blake, and Dickens among them; and that schools themselves are more than partly to blame for their inadequacies is obvious from Rutter's recent study.¹

Could I make three comments? The first is that before extending the school age down-

wards as we have—often disastrously—upwards we should make sure that our schools can cope with their present load. There are sensitive, imaginative, inspiring, and competent teachers but the supply is limited, as it is of adequate baby minders. The second is that, given ordinary good parents and potentially normal children, there is no place like an ordinary good home for rearing young children and that those who don't relish the commitment involved don't have to take on the responsibility; there is always contraception or, for those who feel that the world needs their DNA, adoption in an already overpopulated world. Freedom to reproduce as the fancy takes one should be limited by consideration of the limitations on the freedom of the resultant children which irresponsible parents unwittingly impose, and which ultimately limits the freedom of all of us. Thirdly, should we now not give some consideration to Winnicott's suggestion² that we create two kinds of school—one for the deprived, whatever their intelligence quotient, which would be primarily therapeutic, and one for ordinary children, which would be primarily concerned with education in its academic sense? There would then, I hope, be no question of removing children from good homes for the sake of their academic education.

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¹ Rutter M. *Fifteen hundred hours: secondary schools and their effects on children*. London: Open Books, 1979.

² Winnicott D. *The child, the family and the outside world*, ch 30. Harmondsworth: Penguin, 1964.

SIR,—Devotees of the apparently realistic BBC series *Grange Hill* could not fail to identify the source of "sarcasm, rudeness, . . . threats of violence, and physical violence" not only with children's peers, as Dr G F Goddard describes in his interesting personal paper (5 January, p 21), but also with the controlling adults—whether teachers or others. While we must not neglect the commercial implications, perhaps the words of the present most popular record may have some relevance to Dr Goddard's theme: ". . . no dark sarcasm in the classrooms . . . teachers leave those kids alone (Pink Floyd, "Another Brick in the Wall").

Given the contrived phenomena of schools it seems inevitable that the actions of adults—unchecked by normal social restraints, suspended for adults only on the threshold of the schools—could provide in some instances a more influential, even if less feared, model of poor behaviour, which cannot assist the management of children en masse. It was this latter aspect which Dr Goddard identified as the major problem associated with his referrals.

It is said that teachers receive little practical advice on achieving discipline in the classroom¹; in 1965 the majority of those teaching psychology in colleges of education had not experienced formal teaching themselves in that subject.² One can only commiserate with the individual teacher, and also wonder whether the still-entrenched reliance on physical punishment in British schools has compromised the attempts of young teachers to learn (with appropriate higher support) the art of control without resort to violence—the implied use of which, as a last or first resort, renders sterile the communication