evidence has been presented that anaesthetics, especially halothane, may interfere with immune responses.¹⁻³ The evidence is based on studies of antibody formation in vivo¹ and mitogen-induced lymphocyte transformation in cultures with added halothane,² though normal responses have been reported with lymphocytes of anaesthetists exposed to chronic inhalation of various anaesthetics.⁴ Anaesthetists appear to have a higher death rate from malignancies of the lymphoid and reticuloendothelial tissues than many other doctors.⁵ There are also reports of an increased incidence of stillbirth, abortion, and fetal malformation among women anaesthetists.^{6,8} Anaesthetic-induced immunosuppression or cytotoxicity may have a role in all these phenomena.

We have studied the effect of halothane anaesthesia on the function of B and T lymphocytes in chickens. The parameters used include the humoral antibody formation in vivo and the mitogen-induced transformation of lymphocytes from birds after halothane anaesthesia. The mitogens used, phytohaemagglutinin (PHA) and concanavalin A (Con A) are known to activate T lymphocytes selectively in the chicken.^{9,10}

Twenty 4-week-old White Leghorn chickens were injected intraperitoneally with 2×10^8 sheep red blood cells (S.R.B.C.) and 2×10^7 killed *Brucella abortus* organisms on day 0. A second similar stimulation was given on day 7. Ten of these chickens were anaesthetized on days 4, 5, 6, 11, 12, and 13 for one hour on each day and the other 10 served as a control group. Halothane was given in a concentration of 1% v/v in a mixture of 30% oxygen and 70% nitrogen at a flow rate of 2.0 l/min. Bleedings for antibody titration¹¹ were carried out on days 7 and 14. For the studies on the mitogenic response, half of the chickens in both groups were killed by exsanguination from the heart on day 14. Spleen lymphocytes isolated separately from each chicken were cultured for 72 hours in humidified air and 5% CO₂ at a density of 10^6 cells/ml in medium RPMI 1640 with the addition of penicillin 100 IU/ml and streptomycin 100 µg/ml. Special attention was paid to washing the cells free from the autologous plasma. Fresh normal chicken plasma (10%) was used to supplement the culture medium. PHA M (Difco) was used in a concentration of 50 µl/ml culture and Con A in a concentration of 50 µg/ml culture. All cultures were made in triplicate. To determine the mitogenic response 0.5 µCi of 5-iododeoxyuridine-¹²⁵I (specific activity 0.51-1 mCi/mg) was added to each culture for the final 18 hours incubation.¹² A stimulation index for each chicken was obtained by dividing the mean incorporation (counts/min) of 5-iododeoxyuridine-¹²⁵I in the experimental cultures with the mean incorporation in the control cultures.

The results, log₂ titres for antibody formation and stimulation indices for the mitogenic response, are given in the table as mean values $\pm$ S.E.M.

Halothane had no clear effect on humoral antibody response to a thymus-dependent antigen (S.R.B.C.) or a thymus-independent antigen (*Brucella*). A significant depression in the lymphocyte response to Con A was caused by halothane. PHA-induced uptake of iododeoxyuridine was also reduced, but the reduction was statistically less significant.

Our results indicate that halothane anaesthesia is able to suppress the function of T lymphocytes in the chicken. The effect was clearly detectable on the day following the final anaesthesia. Studies on human patients now in progress seem to support these results. The present findings do not reveal any effect on B cell function. However, it remains to be seen whether different timing of anaesthesia and antigenic stimulation will reveal an effect on the antibody response to thymus-dependent antigens.—We are, etc.,

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Psychiatric Nurse as Therapist

SIR,—I read the article by Dr. I. M. Marks and others (21 July, p. 156) with interest. The article does, I feel, open up some important matters.

There is little doubt that non-medically trained workers, be they nurses, clinical psychologists, physiotherapists, or Uncle Tom Cobbleigh, can be trained in treatment procedures and, given a modicum of intelligence, can carry these out effectively. Whether they should do so is another matter. The same argument has been applied to osteopaths, who no one will deny are often good at manipulating bones and joints, and in other countries people without full medical training have been employed as medical auxiliaries and taken over much of the family doctors' role—namely, that of treating the patients (as in Russia and China). Does the medical profession really want this—or indeed do we consider it ethical?

Surely the answer lies in pressing for adequate medical staff to deal with our proper role—namely, that of treating the patients under our care—rather than unloading treatment procedures on to people with relatively little background knowledge, paid at a lower rate of pay, and who have, as it were, sworn no Hippocratic oath.

This is the thin end of a very dangerous wedge from which the Government will be able to turn round and say, "You do not need more medical staff—this work can be done just as well without all your

expensive and long training." Just as well? Then why are we spending 10 years or so to qualify as experts in a field?—I am, etc.,

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Nerve Conduction Velocity and Hypertension

SIR,—Viskoper *et al.*¹ have claimed that a reduction in motor nerve fibre conduction velocity can be directly correlated with the level of the diastolic blood pressure in hypertensive patients. This association was particularly noticeable in patients with a diastolic pressure above 120 mm Hg. They suggested that nerve fibre conduction velocity studies would provide an objective measurable index of the severity of hypertension. In view of the importance of this suggestion we undertook further studies of a group of hypertensive patients and of a similar number of matched controls.

Fifty patients with hypertension and 50 normal controls were investigated in pairs which were matched for age, sex, weight, height, and occupation. There were 17 females and 33 males in each group. For the purpose of this study a patient was regarded as being hypertensive if the diastolic pressure was 100 mm Hg or greater. Thirty-five patients were being treated with methyldopa and the rest with guanethidine. Diabetes and impairment of renal function were excluded by blood urea and two-hour postprandial blood sugar estimations. Patients with absent pulses in the lower limbs and those with any neurological deficit were also excluded from further study. The fundoscopic findings were noted and the blood pressure was taken on three occasions in both the standing and lying positions, the mean reading then being calculated. To ensure against the effect of lowered temperature on slowing the nerve conduction the skin temperature of each limb was recorded with an Ellab thermocouple.

To study sensory conduction rates metal ring electrodes were placed around the fingers and the sensory potentials recorded at the wrist and elbow using metal disc electrodes 0.5 cm in diameter. Motor velocities were studied with similar recording electrodes placed over the appropriate muscle. The nerve trunks were supra-maximally stimulated at the wrist and elbow for the medial and ulnar nerves and at the ankle and knee for the lateral popliteal and posterior tibial nerves. The stimulus was provided by an American Electronics Laboratories stimulator using a fixed voltage of 250 V and a variable stimulus duration of between 0.01 and 0.03 msec.

The mean age of the patients studied was 54.35 (range 21 to 72) years, the mean diastolic pressure was 122.7 (range 100 to 180) mm Hg, and the mean systolic pressure was 183.3 (range 130 to 260) mm Hg. The conduction velocities for the corresponding nerves are given in the table and show no significant differences in the two groups studied. There was no correlation between either the systolic or diastolic pressure and the grade of hypertension. There was, however, a statistically significant negative correlation of nerve conduction velocities with the age of the patient; this feature was particularly well marked with respect to the

Antigen or Mitogen			Anaesthetized	Controls	P Value
S.R.B.C.	—1st stimulation		2.10 $\pm$ 0.75	1.78 $\pm$ 0.33	>0.10
	—2nd stimulation		3.30 $\pm$ 0.51	3.88 $\pm$ 0.71	>0.10
Brucella	—1st stimulation		7.50 $\pm$ 1.44	6.44 $\pm$ 1.09	>0.10
	—2nd stimulation		17.20 $\pm$ 2.27	14.33 $\pm$ 2.04	>0.10
PHA			7.45 $\pm$ 2.63	14.40 $\pm$ 2.63	<0.10
Con A			2.71 $\pm$ 1.46	16.10 $\pm$ 3.47	<0.01

Mean Values ($\pm$ S.D.) of Motor and Sensory Conduction Velocities (in m/sec.) in Hypertensive Patients and Controls

	Median Nerve		Ulnar Nerve		Posterior Tibial Nerve	Lateral Popliteal Nerve
	Motor	Sensory	Motor	Sensory	(Motor)	(Motor)
Hypertensive	58.89 $\pm$ 3.86	66.56 $\pm$ 4.03	57.37 $\pm$ 4.55	65.14 $\pm$ 4.24	49.56 $\pm$ 4.04	51.35 $\pm$ 5.28
Controls	59.19 $\pm$ 3.36	66.10 $\pm$ 3.95	57.37 $\pm$ 4.39	63.71 $\pm$ 4.51	51.44 $\pm$ 4.61	52.04 $\pm$ 4.64

rates in the lateral popliteal and posterior tibial nerves ($P < 0.001$).

The results of our investigation do not support the finding previously reported that hypertensive patients suffer from a sub-clinical peripheral neuropathy. Even in those patients with markedly elevated diastolic pressures (< 150 mm Hg) no significant slowing of conduction was discovered.

We are very grateful to Mr. Basil Hellman for his assistance with the statistics and to Dr. D. Taverner for his advice and encouragement with the study.

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¹ Viskoper, R. J., Chaco, J., and Aviram, A., *Archives of Internal Medicine*, 1971, 128, 574.

Lomotil Intoxication in Children

SIR,—I am glad you have pointed out in your leading article (23 June, p. 678) the dangers of Lomotil. In the last two years I have seen several children who have been treated with Lomotil for diarrhoea. They have had a variety of puzzling clinical patterns, but the most disturbing has been the abdominal distension. When faced with a two-year-old with this situation my immediate suspicion is now Lomotil intoxication, not appendicitis. It is still a very disturbing situation to all concerned.

I support your contention that Lomotil should not be given to young children, but where it has to be prescribed the doctor should be aware that it is by no means a bland bowel sedative. Very few children who reach my department with diarrhoea have escaped either powerful antibiotics or Lomotil, or both. Our routine (and usually effective) treatment is to stop all medicines. Many children in this country are being seriously overtreated for diarrhoea and vomiting.—I am, etc.,

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Fibre Optics in Medicine and Surgery

SIR,—Your recent report describing the work of our department (9 December 1972, p. 604) has provided us with a considerable amount of feedback from the profession at large, an example being Dr. J. de Swiet's letter (21 July, p. 169) in which he has suggested a novel use for our fine-calibre endoscope and also sympathized with our financial problems. With regard to the latter, we are pleased to say that the Rank Trust has generously agreed to support our work in this field (applying fibre optics to medi-

cine and surgery) for a period of two years. Secondly, as a novel application for a fine-calibre probe, Dr. de Swiet has suggested that the diagnosis of bacterial (infective) endocarditis may be achieved using our probe for the visualization of both mitral and aortic valves and that a Valsalva manoeuvre may adequately clear blood from the visual field.

The problem of blood in the visual field is indeed significant and difficult to overcome, and we think that a simple modification of our instrument's distal tip will provide the easiest solution to the problem. This would be accomplished by fitting a transparent inflatable latex balloon around the endoscope's distal tip, which could then be inflated when the instrument is in situ and hence displace sufficient blood from the visual field to enable the inspection to be completed. The addition of a Valsalva manoeuvre may further enhance the visualization.—We are, etc.,

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Smoking Hazards to the Fetus

SIR,—The assertion in your leading article (17 February, p. 369) that "no reasonable doubt now remains that smoking in pregnancy has adverse effects on the developing fetus" overlooks several reports¹⁻⁶ that disagree. That the birth weights of children of cigarette-smoking women are lower, on the average, than those of children of non-smoking women is not in question. However, the inference from statistics⁷ that smoking causes low birth weight ($< 2,500$ g) is invalid.^{8,9} Causation cannot be inferred from statistical association; unfortunately this error has occurred in a number of studies.^{7,9,10} For example, a 1964 report¹⁰ states that "the causal significance of an association is a matter of judgement which goes beyond any statement of statistical probability" (our italics). Scientific method requires objectivity; but this "method" fails to satisfy such a criterion.

At least two hypotheses may explain the smoking birth weight statistics: (1) cigarette-smoking causes low birth weight, and (2) "the smoking behaviour of women and the birth weights of their children are influenced by a common cause—the individual genotype"¹¹ or constitution. In a study reported by Yerushalmy² the percentages of low-birth-weight infants for women smokers were 6.4 (white) and 12.3 (black) and for non-smokers 3.2 and 5.8 respectively. Thus the presumed "harm" of low birth weight was absent for over 87% of all infants. Hypothesis² asserts that infants born to women before they become smokers will generally be smaller than those of non-smokers. This was confirmed ($P < 0.01$) for

white and weakly confirmed ($0.05 < P < 0.1$) for black women. Further, hypnosis² predicts a lower incidence of low birth weight among the infants of ex-smokers, while smoking than for those of habitual smokers. This was confirmed for both black and white women ($P < 0.02$). These findings are incompatible with hypothesis¹.

The restricted bioenergetic potential of high altitude,¹¹ and possibly of smokers,^{6,12-14} could lead to low birth weight. But since infants of both high and low birth weights are at elevated perinatal mortality risk,¹⁵ the problem is clearly complex.

Endocrinological anomalies influence ethological homeostatic mechanisms^{12,13,16} affecting dietary self-selection¹⁶ and probably self-selected use of coffee¹³ and tobacco.^{12,14} The biological basis of the coffee and tobacco use association probably involves biogenic amine physiology and bioenergetics.^{12,14} Smoking behavioural subgroups (smokers, non-smokers, ex-smokers) are self-selected rather than selected at random, creating special problems for hypothesis testing.⁶ If smoking is, for some, a symptom of deficient bioenergetics, such a deficiency may cause a limitation of the metabolism or detoxification of chemicals that nicotine can alleviate.^{6,12-14} To the extent that environmental mutagenic hazards are involved, such as HSO₃ and HNO₂ from atmospheric SO₂ and NO₂,^{6,13,17} the degree of non-alleviation should be proportional to the risk of accumulation of mutations of germinal DNA.⁶ Such mutations can affect development and therefore birth weight.¹⁸

In summary, it appears that the question of causal relationships between smoking and low birth weight is far from settled.—We are, etc.,

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