There is, however, a much more sinister aspect to this brainwashing process by the new media and by noise affecting young people, and that is loss of critical faculties and increased suggestibility. These two phenomena are well known to psychiatrists and may play their part in the various violent protest movements we are witnessing to-day. We have created a society where for some young people stimulation of the mind by the mass media and by other undesirable aspects of our society, such as many films, comics, loud pop music, noise at work, and traffic has already reached saturation point, and the cult of drug addiction may be one way of escape for them.

It is high time our politicians and social scientists recognized these facts, as a reversal of the trend at this stage will prove very difficult.—I am, etc.,

ERIC FRANKEL

Wanstead Hospital,
London E.11

Hyperbaric Oxygen and Radiotherapy

SIR,—As you observe in your leading article (13 May, p. 368) the exact value of hyperbaric oxygen in radiotherapy is still subject to debate. Apart from the question of the effectiveness of changing malignant tissue from the anaerobic to the aerobic state before irradiation, the complexity of the technique and the expense of apparatus are two further deterrents to its wider use.

Recently it has been shown that glucose loading greatly increases the heat emission of tumours of the breast.¹ This is presumed to be due to increased metabolism stimulated by the excess carbohydrates. Normal tissue does not appear to react in the same way.

Although the reduced sensitivity of some malignant tissue can be attributed to lack of oxygen, counter measures to increase radiosensitivity need not necessarily be confined to increasing oxygen tension, and it is possible that increased metabolic activity can bring cells into a more radiosensitive state.

Could it be that the relatively simple perfusion of glucose prior to radiotherapy would be an equally effective "dose-multiplying agent" as hyperbaric oxygen? It would be inexpensive to try, and if effective, tiresome expertise and the paraphernalia of hyperbaric oxygen both obviated.—I am, etc.,

G. B. YOUNG

Radiodiagnostic Department,
Royal Infirmary, Edinburgh

¹ Chiricuta, I., Bucur, M., Opris, I., and Bolaga, S., *Oncologia si Radiologia*, 1970, 9, 453.

Termination of Pregnancy

SIR,—Mr. Desmond Bluett (22 April, p. 228) raises some important points. Not the least of these are the long-term complications of induced abortion. It can be inferred, not only from Mr. Bluett's figures, but also from other sources, that abortion is being increasingly used as a contraceptive method, a procedure which is acceptable only if an abortion is considered as a reasonably safe and simple procedure. However, "there is now ample evidence to show that abortion is neither safe nor simple. The long-term complications alone condemn its use as a contraceptive method."¹

The facts about mortality and morbidity are not likely to have consequences to family life, the stability of marriage, or financial cost in the same proportion as the risk of infertility and the risk of harm to unborn children. The British Perinatal Mortality Survey showed that women who had one or more abortions had on average a reduced reproductive capacity and subsequent children were at higher risk.² There are also papers from Japan, Hungary, and Czechoslovakia showing that induced abortion increases perinatal mortality, subsequent spontaneous abortions, subsequent ectopic or extrauterine pregnancies, sterility, the proportion of premature births, and a variety of other complications affecting subsequent pregnancies.^{3,4}

It is indeed surprising that the consequences of induced abortion have not yet been the subject of a systematic longitudinal study of a large cohort of women. As full knowledge is needed for doctors and social workers to give women responsible advice on therapeutic abortion only an appropriate study will provide the necessary facts, and the consequences of induced abortion on subsequent pregnancies and to subsequent children should be one of the main purposes of such a survey.—I am, etc.,

PAUL MOXON

Leeds

¹ Gordon, H., *Royal Society of Health Conference Proceedings*, 10 February 1972, p. 7.

² Butler, N. R., and Bonham, D. G., *Perinatal Mortality*, E. & S. Livingstone, Edinburgh and London, 1963.

³ Kotasek, A., *Journal of Gynaecology and Obstetrics*, 1971, 9, 118.

⁴ Klinger, A., *Journal of Gynaecology and Obstetrics*, 1970, 8, 680.

Cardiac Enlargement in Jamaicans

SIR,—Professor K. L. Stuart and others (1 April, p. 21) reported that many of their Jamaican outpatients with mildly elevated blood pressure had cardiac enlargement on chest x-ray (cardiothoracic ratio >50%) and left ventricular hypertrophy on electrocardiogram (Sokolow Lyon criteria). The significance of these observations may be misjudged by readers who are unaware that, by these standards, a high proportion of healthy, normotensive Jamaicans have cardiac enlargement and left ventricular hypertrophy.

Cardiovascular surveys have been carried out involving subjects aged 35-64 years living in communities in rural¹ and suburban² Jamaica and in a village in Guyana³ among persons of similar ethnic (African) and socio-economic backgrounds. The data have been re-analysed using the same criteria for cardiac enlargement, left ventricular hypertrophy, and hypertension as were used by Professor Stuart and his colleagues. (Mild hypertension was regarded as a diastolic pressure in the range 95-109 mm Hg in those under 45 years and 100-119 mm Hg at 45 years and over; normal

blood pressure and severe hypertension were diastolic pressures below and above these values respectively.)

The data could not be closely compared with those of Professor Stuart because no details of age, sex, and blood pressure of their patients were reported but the combined results from the three surveys show (Table) that cardiac enlargement and left ventricular hypertrophy, although more prevalent in hypertensives, were also common in normotensive subjects.

One of the reasons for the surprisingly high rates of apparent cardiac enlargement in healthy West Indians of African origin is that their thoracic diameters are, on average, smaller whereas their cardiac diameters are similar to those of Caucasians of the same height.⁴ This ethnic difference may also partially explain the greater amplitudes of their R and S waves in E.C.G. precordial leads⁵ (on which the Sokolow Lyon criteria for left ventricular hypertrophy are based).

The high prevalence of apparent cardiac enlargement and left ventricular hypertrophy in mild hypertension might lead to its overestimation as a cause of cardiac changes if the common presence of these arbitrarily defined states in healthy normotensive Jamaicans is not appreciated. The use in medicine of standards and criteria based on studies of white populations should be scrutinized before being applied to other ethnic groups.—I am, etc.,

M. T. ASHCROFT

M.R.C. Epidemiology Unit,
University of the West Indies,
Jamaica

¹ Florey, C. du V., and Miall, W. E. M., *American Journal of Epidemiology*, in press.

² Miall, W. E. M., *Proceedings of the 5th International Scientific Meeting of the International Epidemiological Association*, Belgrade, 1970, 277.

³ Ashcroft, M. T., Beadnell, H. M. S. G., Bell, R., and Miller, G. J., *Bulletin of the World Health Organisation*, 1970, 42, 205.

⁴ Ashcroft, M. T., and Miall, W. E., *American Journal of Epidemiology*, 1969, 89, 161.

⁵ Ashcroft, M. T., Miller, G. J., Beadnell, H. M. S. G., and Swan, A. V., *American Heart Journal*, 1971, 81, 467.

Herpes Encephalitis Revisited

SIR,—The interesting letter from Dr. A. R. M. Upton (22 April, p. 226) concerning the treatment of herpes encephalitis deserves comment.

I have examined the results of 369 cases of acute necrotizing encephalitis either published in the world literature or the subject of personal communications (Table). These suggest that any treatment is better than no treatment at all. However, in such a retrospective study the biases and obscurities are numerous, and a full report on that subject has been prepared.¹ Most of the reported cases represent the severer form of the disease, while it is probable that the least severe cases will have been overlooked, and may sometimes be merely diagnosed as a "stroke." The true incidence and natural history of the disease

	Normal Blood Pressure		Mild Hypertension		Severe Hypertension	
	M.	F.	M.	F.	M.	F.
No. of Subjects	474	619	64	104	9	23
Cardiac enlargement %	31.3	52.0	42.2	76.0	66.7	87.0
Left ventricular hypertrophy %	34.8	10.5	50.0	19.2	66.7	41.7

therefore remains largely unknown. On the other hand, the terms "alive" or "dead" do not give an account of the whole phenomenon. For example, the patient described by Dodge and Curie² benefited from decompression and lobectomy, and was the only one of the 37 cases reported by Haymaker *et al.*³ which survived. Nevertheless, the death of the same patient was later reported⁴ and I wonder whether this case could be used to illustrate the success or failure of surgery.

	No. of Cases	Alive	Dead
Untreated	268	63	205
Treated	101	49	52
Surgery only	28	11	17
IDU* only	25	13	12
ARA C** only	8	7	1
Steroids only	19	9	10
Steroids + surgery	2	1	1
IDU or ARA C + surgery	12	4	8
IDU or ARA C + steroids	3	2	1
Steroids + surgery + IDU or ARA C	3	2	1
Poly-I.C.***	1	0	1

*Idoxuridine.

**Cytosine arabinoside.

***Semisynthetic RNA which induces interferon production.

It must be stressed that in the untreated group recovery is usually slow and often takes weeks, if not months. In many cases in the treated group, however, the first signs of recovery appeared within 72 hours, an improvement noted by Dr. Upton himself in his case no. 1.⁵ It is true, nevertheless, that a careful reading of the literature shows that there is no valid evidence that any one treatment is much better than the others.

In a patient described by Gurwith *et al.*⁶ there was marked deterioration while on dexamethasone, but there was rapid recovery after idoxuridine therapy. In other cases (personal communication) idoxuridine is of no help, but external decompression and lobectomy may have a striking beneficial effect. I also have seen patients who deteriorated despite surgical intervention but responded within 30 hours to idoxuridine infusion.

The importance of brain oedema as a contributory cause of death has been clearly recognized.⁷⁻⁹ One must therefore agree with Dr. Upton when he asks that chemotherapy be combined with either surgical or "medical" decompression. For reasons which have already been put forward^{10,11} the use of steroids alone appears hazardous, and I would like to make a plea that if dexamethasone is to be used it should always be combined with antiviral chemotherapy.

Acute necrotizing encephalitis is the most frequent type of encephalitis in adults encountered in our latitudes; the problem is not only, therefore, of academic interest. At the symposium on herpes encephalitis held in Manchester earlier this year a working party was established with a view to encouraging multicentre retrospective and prospective studies into the problems inherent in the treatment of this disease. I hope that such a co-operative undertaking will help us in the future to treat our unfortunate patients more successfully.—I am, etc.,

MARC RAPPEL

Clinique Neurochirurgicale,
Université Libre de Bruxelles,
Brussels, Belgium

¹ Rappel, M., *Postgraduate Medical Journal*, in press.
² Dodge, P. R., and Cure, C. W., *New England Journal of Medicine*, 1956, 225, 849.

- ³ Haymaker, W., Smith, M. G., van Bogaert, L., and de Chenar, C., in *Viral Encephalitis* ed. W. S. Fields and R. J. Blattner, p. 95, Springfield Thomas, 1950.
- ⁴ Booth, C. B., Okazaki, H., and Gaulin, J. C., *Neurology*, 1961, 11, 619.
- ⁵ Upton, A. R. M., Barwick, D. D., Foster, J. B., *Lancet*, 1971, 1, 290.
- ⁶ Gurwith, M. J., Harman, C. E., and Merigan, T. C., *California Medicine*, 1971, 115, No. 6, 63.
- ⁷ Pierce, N. F., Portnoy, B., Leeds, N. E., Morrison, R. L., and Wehrle, P. F., *Neurology*, 1964, 14, 70.
- ⁸ Page, L. K., Tyler, H. R., Shillito, J., *Journal of Neurosurgery*, 1967, 27, 346.
- ⁹ Delmas-Marsalet, P. M., Bergouignan, M., Castaing, R., du Pasquier, P., and Vital, C., *Bulletin de l'Académie de Médecine (Paris)*, 1968, 192, 316.
- ¹⁰ Longson, M., and Beswick, T. S. L., *Lancet*, 1971, 1, 749.
- ¹¹ Rappel, M., *et al.*, *Journal of the Neurological Sciences*, 1971, 12, 443.

Trimethoprim-sulphamethoxazole in Folic Acid Deficiency

SIR,—Drs. I. Chanarin and J. M. England (11 March, p. 651) have drawn attention to the dangers of trimethoprim-sulphamethoxazole therapy in megaloblastic anaemia. We also have observed these dangers and the following case may be of interest.

A 72-year-old woman was admitted because of a severe urinary tract infection. She had a previous history of diabetes mellitus, rheumatoid arthritis, and primary hypothyroidism, which had been treated by diet, analgesics, and thyroxine respectively. To this was added trimethoprim-sulphamethoxazole (Septrin) for the urinary infection, because the patient claimed to be allergic to ampicillin. While this therapy continued, investigation was instituted for the severe megaloblastic anaemia which had been discovered on admission. In view of the previous history, pernicious anaemia was considered the most likely diagnosis, but B₁₂ deficiency was excluded by the presence of a normal serum level (170 pg/ml), free acid in the gastric juice (pH 2.3), and the lack of response to vitamin B₁₂ therapy (Fig.).

Folate deficiency was confirmed as the cause of the anaemia by a low serum folate (1.7 µg/ml) and a dietary history showing a daily intake of only 4.1 µg per day approximately. We were very disturbed to find a marked fall in the haemoglobin and platelet count after ten days of Septrin (Fig.). It seemed very likely that the patient's folic acid deficiency anaemia had been aggravated to a dangerous degree by the Septrin. It would

seem that caution is indicated in using this drug in any patient in whom folic acid deficiency is suspected.—We are, etc.,

PATRICK J. ROONEY
E. HOUSLEY

Royal Infirmary,
Edinburgh

Anticonvulsant Drugs, Folate Deficiency, and Metabolic Bone Disease

SIR,—Dr. Roger Williams and his colleagues (23 October 1971, p. 202 and 29 January, p. 297) reported similar significant inverse correlations between urinary glucaric acid excretion and (1) the level of serum calcium and (2) the degree of folate deficiency in drug-treated epileptic patients. Both sets of data were interpreted as supporting the hypothesis that anticonvulsant bone changes and anticonvulsant folate deficiency are due to enzyme induction.

There are some difficulties in explaining the anticonvulsant folate changes on the basis of enzyme induction. There is evidence that diphenylhydantoin is a more powerful "antifolate" drug than phenobarbitone.¹ This would imply, according to the induction hypothesis, that diphenylhydantoin is a more powerful enzyme inducer than phenobarbitone, which does not appear to be the case. Indeed, the data presented by Drs. G. H. Spray and D. G. Burns (15 April, p. 167) clearly suggest that phenobarbitone has a more marked enzyme inducing effect than diphenylhydantoin. Despite this the only group of their animals in whom liver folate fell was that on diphenylhydantoin.

There is no convincing evidence yet that folate is required for drug hydroxylation. If this were true one would expect an increase in the urinary excretion of 5-(p-hydroxyphenyl)-5 phenylhydantoin following the administration of folic acid to folate-deficient epileptic patients being treated with diphenylhydantoin. However, Olesen and Jensen² indicated that the excretion of this metabolite actually decreases after the administration of folic acid.

Alternative theories of anticonvulsant-induced folate deficiency for which there is some evidence are impairment of folate transport³ or inhibition of co-enzyme function.^{4,5}

The explanation for the drug-induced