cerning those seeking to combat disease and underdevelopment.

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¹ *Lancet*, 1976, **1**, 730.

² *Alternative Approaches to Meeting Basic Health Needs in Developing Countries*, ed V Djukanovic and E P Mach. Geneva, World Health Organisation, 1975.

³ Gish, O, *World Development*, 1973, **1**, No 12, p 37.

⁴ Department of Employment, Statistics Division.

⁵ The British Volunteer Programme comprises the Catholic Institute of International Relations, International Voluntary Service, United Nations Association, and Voluntary Service Overseas.

Adjuvant therapy of lung cancer

SIR,—We read with interest your leading article on adjuvant therapy of lung cancer (22 January, p 187) and in particular the statement attributed to Professor R Baldwin that "the original concept that human tumours are antigenic and that immunotherapy may aid their elimination by an immune reaction was no longer tenable." We feel that this quotation without further comment is misleading and disappointing. Misleading because it appears to deny firstly the antigenicity of human tumours, secondly the therapeutic possibilities of manipulating host immunity, and thirdly, by implication, the possibility that immune reactions may be involved in the host responses to tumour cells. Disappointing because, as elsewhere in the article, it represents a note of pessimism which is based, presumably, on the failure of clinical trials to demonstrate specific immunological responses to active immunotherapy.

There is considerable evidence that human lung tumours express neoantigens.¹ By immunising a goat with cultured cells derived from a human oat cell carcinoma we have demonstrated both anti-normal and anti-tumour antibodies. This antiserum contained high titres of lymphocytotoxic, haemolytic, and haemagglutinating antibodies. Removal of the anti-normal antibodies by absorption with human spleen preparations did not affect the anti-tumour antibody titre in an indirect immunofluorescence test. The absorbed antiserum contained extremely high titres of antibodies to the onco-fetal antigen CEA. These findings of lung tumour antigenicity are similar to those of others.^{2,3}

The evidence that such tumours are immunogenic in the host is less convincing, although several authors have described, in vitro or in vivo, specific sensitisation to lung tumour antigens.⁷ At presentation cancer patients are immunosuppressed and have already proved themselves incapable of responding effectively to tumour antigens. They are therefore unlikely to respond to further stimulation with tumour antigens—for example, irradiated tumour cells. It is not

easy to believe that human tumours are completely different from those investigated in animal models, especially in spontaneous tumour systems. If they are is the enormous expenditure of resources and effort on research in such models justified? If they are not should we dismiss the possibility that human tumour elimination may involve an immune reaction? Combination of active and passive specific immunotherapy either alone or together with chemotherapy would probably be the ideal immunological treatment for cancer. The assessment of claims made for antibody drug synergism (ADS)⁸⁻¹⁰ and immune RNA^{11,12} may eventually enable us to reinforce the ineffective immune system of cancer patients.

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¹ Baldwin, R W, and Price, M R, *Annual Review of Medicine*, 1976, **27**, 151.

² Frost, M J, Rogers, G T, and Bagshawe, K D, *British Journal of Cancer*, 1975, **31**, 379.

³ Bell, C E, jun, and Seetharam, S, *International Journal of Cancer*, 1976, **18**, 605.

⁴ Hollingshead, A C, Stewart, T H M, and Herberman, R B, *Journal of the National Cancer Institute*, 1974, **52**, 327.

⁵ Wells, S A, et al, *Journal of Thoracic and Cardiovascular Surgery*, 1973, **66**, 557.

⁶ Boddie, A W, jun, et al, *International Journal of Cancer*, 1975, **15**, 823.

⁷ Roth, J A, et al, *Journal of Thoracic and Cardiovascular Surgery*, 1975, **70**, 613.

⁸ Davies, D A L, and O'Neill, G J, *British Journal of Cancer*, 1973, **28**, suppl 1, p 285.

⁹ Davies, D A L, Manstone, A J, and Buckham, S, *British Journal of Cancer*, 1974, **30**, 297.

¹⁰ Davies, D A L, Buckham, S, and Manstone, A J, *British Journal of Cancer*, 1974, **30**, 305.

¹¹ Pilch, Y H, Veltman, L L, and Kern, D H, *Archives of Surgery*, 1974, **109**, 30.

¹² Schlager, S I, Pague, R E, and Dray, S, *Cancer Research*, 1975, **35**, 1907.

SIR,—Your leading article on this subject (22 January, p 187) reminded me of the well-known phenomenon that new acquisitions need a considerable lapse of time before they reach the medical news media; their appearance in medical textbooks even takes several years. Therefore I should perhaps not have been surprised to read that "the enthusiasts [whoever they may be] for levamisole are now promoting a new image for this drug . . . as an 'immune-restorant,' whatever that may be, having failed to make the grade as an immune stimulant" and that "the initial disappointing results . . . in cancer may now, it seems, be explained as the consequence of getting the dose wrong" (my italics). I would like to make the following comments in that regard:

(1) More than a year ago it was concluded¹ that "levamisole is an antianergic chemotherapeutic agent. It restores to normal the function of phagocytes and lymphocytes when this is depressed. Cell functions are not stimulated above normal. The effect seems to be confined to cells involved in the cell-mediated immune reactions." This, at the same time, gives a reply to your question of what an immune-restorant may be. Finally, I, too, may wonder how much better immune stimulants may be as opposed to immune-restorants which have failed to make the grade as immune stimulants.

(2) As early as July 1975 it was realised² that the dosage used in our lung cancer study was probably too low for patients with a higher body weight. This observation for the first time clearly suggested that animal data pointing to an undeniable dose-

dependency of the levamisole effect have their clinical counterpart. I may add that the initial clinical results^{3,4} with levamisole as an adjuvant therapy of cancer were not disappointing at all, except, perhaps, for those people who expected levamisole to be a miracle drug. For the latter people I would like to make clear that we do not know about any miraculous effect of levamisole on top of its anthelmintic and antianergic properties.

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¹ Symoens, J, Paper presented at the 2nd Conference on Modulation of Host Immune Resistance in the Prevention or Treatment of Induced Neoplasias, Bethesda, Maryland, 1-3 December, 1975.

² Amery, W K, in *Chemotherapy*, vol 8, Cancer Chemotherapy II, ed K Hellmann and T A Connors. New York and London, Plenum Press, 1976.

³ Rojas, A F, et al, *Lancet*, 1976, **1**, 211.

⁴ Study Group for Bronchogenic Carcinoma, *British Medical Journal*, 1975, **3**, 461.

Whooping cough vaccination

SIR,—A survey of encephalitis among patients discharged from hospitals in Scotland in the two-year period 1968-9 revealed not a single case of brain damage due to whooping cough vaccination.¹ No such cases have been encountered in the Glasgow area by our two senior infectious diseases physicians with a combined experience of 60 years² or by the staff of the city's immunisation clinics.³ No such case has been confirmed at the regional assessment unit for paediatric neurology⁴ and none has been found in the register of handicapped children of the Greater Glasgow Health Board, although one child was noted to have suffered brain damage as a consequence of having contracted whooping cough itself.⁵ Is this experience shared by other parts of Britain?

Prensky's remark of 1974 is highly pertinent: "This 25-year old controversy, punctuated by limited clinical studies and testimonials, will remain a monument to the ineffectual nature of anecdotal reports and retrospective studies."⁶ It is high time that properly detailed evidence concerning the allegedly vaccine-damaged children was put on record for critical scrutiny by those able to evaluate it.

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¹ *Communicable Diseases, Scotland, Special Report 77/6*, Edinburgh, Scottish Home and Health Department, 1977.

² Lawson, J H, and McKenzie, P, personal communication.

³ Wilson, T S, personal communication.

⁴ Stephenson, J B P, *Lancet*, 1977, **1**, 357.

⁵ Forwell, G D, Note to Community Health Staff, Greater Glasgow Health Board, 10 February 1977.

⁶ Prensky, A L, *Developmental Medicine and Child Neurology*, 1974, **16**, 539.

Increased incidence of poliomyelitis

SIR,—We would like to draw attention to the recent increase in the number of notifications of paralytic poliomyelitis in Britain.

For the period 1972-5 there were 3-6 cases on average per year. During 1976 eight cases were notified, of which five occurred in October. In Addenbrooke's Hospital there are at the moment two further cases and there have also been two in London.

The total of nine cases during the four months October-January is equivalent to an annual incidence of 27 cases per year, or 4-9 times the incidence before the long hot summer of 1976. It indicates that there exists a higher level of poliomyelitis virus in the population than formerly, and as almost all those paralysed were unvaccinated we feel that more cases are likely to occur.

It seems to us important that an attempt should be made to reach the 15-20% or more of the population which is said not to receive poliovaccine nowadays, a group which may be increased by the current adverse publicity incurred by other vaccines.

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Backache

SIR,—I must respectfully disagree with some of the views expressed by Dr John Mathews (12 February, p 432). General practitioners and hospital doctors in training, for whom presumably this series of articles is designed, will have been misled by some of the statements and omissions.

I wish there could have been more emphasis placed on the commonest form of backache—namely, postural or occupational—and on the fact that less than 10% of backache results from disc prolapse. In my experience these patients usually complain that their backache is worse with rest and eased by exercise. My own backache, for example, responds to vigorous spinal exercises, especially flexion, and is much worse in the recumbent position. Many of these patients, myself included, respond well to rotary manipulation. Generally speaking, it is this type of backache that the osteopath treats so well, but unfortunately they tell their patients that they are replacing their “slipped discs.” Anyone who has observed a protruding or prolapsed disc at the time of surgery will know that such statements are nonsense.

It is unfortunate that these patients are often kept off work for long periods and are told to go to bed. In fact this is the worst possible treatment for them. Almost as bad is to give them a surgical corset. The only useful purpose for this over-used appliance is in elderly patients; it is occasionally useful as a temporary measure in patients with spondylosis or spondylolisthesis, an important mechanical problem not mentioned in the article. In all other circumstances the corset simply helps to keep the back warm, considerably weakens the back and abdominal musculature, and assists the neurotic man when he has to explain to his wife or employer that he cannot do certain jobs because “I have a bad back.”

No mention was made of the common condition of low lumbar instability secondary to previous disc derangement. It is also a pity that weight reduction was not mentioned as an essential part of treatment whatever the cause.

I submit that in an article of this type controversial treatments of unproven value and not without risk—namely, chemonucleolysis and sclerosants—should not have been mentioned. Instead it would have been more useful to extend the section on the forgotten art of history taking.

Finally, sir, may I plead that the totally meaningless neologism “osteoarthrosis”

(dreamed up by rheumatologists, I believe) should be dropped from the medical literature.

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SIR,—I was pleased to find that Dr John A Mathews (12 February, p 432) gives due importance to the apophyseal joints as a source of backache. He rightly says that it is not easy to distinguish an apophyseal joint lesion from a minor disc lesion, but his predecessor at St Thomas's (Dr J Cyriax) dismissed the apophyseal joints as unimportant and an insignificant cause of back pain. Manipulation when properly applied is an effective measure in many cases of back pain, and, while it is possible that manipulation can have a reducing effect on a small disc herniation, the explanation that is much more likely is that manipulation releases fixation in an apophyseal joint.

If a disc protrusion is ever reduced by manipulation it would inevitably be reproduced as soon as weight-bearing took place again by the sheer mechanics of compression, whereas an apophyseal joint lesion corrected by manipulation would not be so reproduced; and as so many patients obtain immediate and lasting benefit from a simple manipulation, then surely the explanation lies in the apophyseal joint rather than the disc.

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SIR,—It is encouraging to see that backache is at last being taken seriously enough to be the title of an article by Dr J A Mathews (12 February, p 432). His article is an excellent summary of current thought on this subject. I was a little disappointed, however, by his rationale for treatment. I had thought that we had progressed from the idea that all “mechanical” backache was due to a swelling impinging upon dura or nerve root and that all treatment must be directed to “restoring anatomy.”

As one who spends nearly all his time manipulating, I feel that to talk of the rationale for this kind of treatment as being to “coax a bulge back into shape” is a gross oversimplification and misrepresentation. That improvement in the disc contour can occur has been shown by Dr Mathews's own work, but I would venture to suggest that in most cases it is irrelevant. Aside from this effect, numerous other effects of manipulation have been claimed—for example, modification of the topographical relationship between a disc hernia and nerve root; decreasing compression and shearing stress on the disc; opening of the intervertebral foramen; increase in posterior joint mobility; muscle relaxation; normalisation of proprioceptive impulses; vasomotor and visceral effects; and psychological effects.

To my mind, by concentrating too hard on any one of these effects of manipulation one is missing the most important and perhaps the most obvious: movement. A spinal joint by careful intersegmental movement palpation is found to be locked. One manipulates. It moves. Gradual elimination over a number of sessions of all such locked segments leads to disappearance of backache in the vast majority of cases. Is it unreasonable to assume that restoration of function in the spinal joints and therefore in the spine as a whole is more important than restoring anatomy?

If one thinks more of function and less of anatomy “the unlikelihood of persuading a soft-tissue lesion to heal more rapidly than by rest alone” seems to be a false assumption. Rest is one thing, immobility or abnormal mobility is another. I would venture to suggest that restoration of function is more important to the healing of cartilaginous structures than rest. Apart from this, by manipulation one is restoring a normal balance of movement to the spine as a whole. How frequently are the locked segments not at the site of root compression? Has Dr Mathews not seen manipulation of the atlas remove signs of upper cervical nerve root irritation and at the same time improve sciatica? There is no disc either above or below the atlas.

The discovery of disc herniation may have been an advance, but, like thoracic outlet syndrome and carpal tunnel syndrome, does it not seem that the concept is blinding us to other and perhaps more general problems in the aetiology of referred pain syndromes?

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SIR,—I fear that little progress will be made in coping with the difficult problem of backache as long as doctors share the view of Dr J A Mathews (12 February, p 432) that “recumbency and analgesics remain the basic treatment for most mechanical spinal lesions.” In the experience of doctors who routinely apply the various active medical measures for which Dr Mathews evidently has little enthusiasm good results are consistently obtainable. Indications for manipulation and traction,¹ epidural local anaesthesia,² ligamentous sclerosis,³ and peripheral nerve blockade⁴ have been set out. Short of a major new approach, the selective application of these methods constitutes the only hope for effective treatment for “mechanical” backache (apart from surgery). All such patients should therefore be given the chance of treatment along these lines rather than, as a primary treatment, being consigned to bed.

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¹ Cyriax, J, *Textbook of Orthopaedic Medicine*, 6th edn. London, Baillière Tindall, 1975.

² Coomes, E W, *British Medical Journal*, 1961, **1**, 20.

³ Barbor, R, *Proceedings of the IVth International Congress of Physical Medicine*. Amsterdam, Excerpta Medica Foundation, 1964.

⁴ Rees, W S, *Annals of General Practice*, 1971, **16**, 126.

Drug promotion

SIR,—I recently received through the post three Mexican jumping beans together with promotional material from Napp Laboratories Ltd for Bradilan (tetranicotinoyl fructose). The envelope carried the following intriguing message in red: “Contains live jumping beans” and inside I was told that the beans should remain active for several months, particularly if exercised regularly. The advertisement stated (among other things) that “we can't promise to make your patients jump but your claudicants will be able to walk further.” Admittedly there is a dearth of good objective evidence for the efficacy of this type of vasodilator in peripheral vascular disease,¹ but to give doctors Mexican jumping beans as an inducement to prescribe Bradilan is a sad