you say in your leading article (24 May, p 1240), but absolutely vital. In fact, high-titre antibodies to pseudomonas, in particular to exotoxin A (and lipid A), provide very good protection even in patients who are neutropenic or who have serious underlying diseases, as reported by Pollack and Young.¹ Why was this essential reference not quoted? Taken in conjunction with the vaccine production work of Jones *et al*² it really is the promise for the future. Similar principles apply to other Gram-negative infections.³

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¹ Pollack M, Young LS. *J Clin Invest* 1979;63:276-86.
² Jones RJ, Roe EA, Gupta JL. *Lancet* 1979;ii:977-83.
³ Wardle EN. *Br J Hosp Med* 1979;21:223-31.

Distinguishing direct and indirect inguinal hernias

SIR,—If Professor N L Browse (24 May, p 1270) finds it necessary to employ all the signs used traditionally to distinguish a direct from an indirect inguinal hernia in order to exclude other causes of a swelling in the groin, so be it. It is disheartening, however, that he should claim justification for guesswork. Our point (12 April, p 1039) is that as the distinction between indirect and direct hernias is fallible and as "from the practical point of view this differentiation is quite irrelevant"¹ there is little to be said for elaborating the diagnosis beyond that of an inguinal hernia.

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¹ Browse NL. *Symptoms and signs of surgical disease*. London: Arnold, 1978.

The teaching of anatomy

SIR,—Mr S Mottershead has written a most interesting personal paper on the teaching of anatomy (31 May, p 1306). I hope I may add rheumatologists to neurologists and orthopaedic surgeons as possessing a reasonable amount of the anatomical knowledge which is essential for working out the causes of pain around the shoulder joint. He has certainly used his knowledge to explain the problem of the young woman with weakness of the trapezius muscle and save her from unnecessary surgery. I think it likely, however, that her symptoms were due to a post-infectious neuropathy affecting the spinal accessory nerve¹ and that recovery was unrelated to physiotherapy.

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¹ Cyriax J. *Textbook of orthopaedic medicine*, vol 1, 7th ed. London: Baillière Tindall, 1978:233.

Is it really a cerebrovascular accident?

SIR,—In these days of metrication, SI units, computerisation of records, and other sophistications in medicine, I am concerned at the total absence of scientific accuracy in the use of "cerebrovascular accident" as a cause of death. It commonly appears on death

certificates and cremation forms. Although the condition may be spontaneous and unexpected it is certainly not an accident. The profession know what is implied but relatives do not. On several occasions I have been asked, "Does it mean that my parent fell out of bed while in hospital?" Admittedly it may be difficult to differentiate between cerebral haemorrhage and cerebral infarction, but is there any objection to the simple and scientifically honest diagnosis of "acute cerebrovascular disease"? Let us stop perpetuating the misuse of the word "accident" when there is no evidence of trauma.

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Research and risk

SIR,—Your leading article (14 June, p 1391) draws together nicely several strands in the current debate on medical research. You question how typical are the members of the Association of Researchers in Medical Sciences, and whether medically or scientifically qualified. *The Case for Careers in Medical Research* was at pains to point out that in terms of age distribution, seniority, and employment history the members are representative of contract research staff in biomedicine and in all disciplines, in individual institutions and nationally. Whether their opinions are typical is a more difficult question; but the fact that the membership of ARMS has considerably more than doubled since the published analysis was done (without changing the findings of that analysis) and now comprises about 10% of all contract researchers in biomedicine in universities, medical schools, and the Health Service suggests that they are representative of at least a considerable minority. The problem, though not ARMS membership, is confined to non-medical staff, since medical practitioners have multiple and well-remunerated alternative career pathways, in which they are in demand.

The recommendations of the CIBA group and of ARMS are not mutually exclusive or opposed but rather obverse and reverse. Although not part of ARMS policy, it is felt that "mad" money is an indispensable element in research support. ARMS's analysis and proposals are set out in a forthcoming publication—*Detailed Proposals*. The key element in these proposals covering research staff organisation is the divorce of duration of contracts of employment from the duration of research grants. Tenure does not mean a researcher studying only one project during 40 years; a small donation from a local charity group does not mean a researcher being given the Noble Order of the Boot after nine months. If this principle is accepted by employers, and the principle of possible periodic redeployment accepted by researchers, it should be possible to institute a strong category of career researchers recruited on a proper progression of training, probation, and tenure, with suitable selection procedures and possibilities of progression from one stage to the next. Further, flexibility of funding should thereby be retained or enhanced, with the confidence that the ideas funded, whether mainstream or wayout, will be properly investigated. Finally, the possibility is raised of being able to arrange limited periods when clinical or particularly

teaching staff might be able to devote themselves full time to research, as so many claim they wish to do so.

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Epidemiology and clinical practice

SIR,—Public health doctors have always been proud of a historic tradition, extending over 150 years, of using simple facts and figures in a logical manner to demonstrate very elegantly, time and again, solutions to very complex problems. But the focus of attention was largely control of outbreaks of infection and environmental problems with essentially an engineering solution. Profound changes have taken place in the pattern of morbidity and mortality in the twentieth century. Newer diseases are dependent on less environmental causes and more on personal characteristics, often identified either by clinical observation or measured in the laboratory.

Association of clinical practice and epidemiology has resulted in spectacular medical discoveries not unlike those of the public health in the nineteenth century. I therefore find it distressing to note it has even become necessary for Professor E D Acheson and colleagues (26 April, p 1122) to point out the obvious and make a plea for epidemiology to continue and develop in clinical practice as a recognised entity. It is in the very nature of the job of a community physician to collect statistics and try to draw conclusions from them just as it is the job of an accountant at the end of the day; but whether that makes all community physicians epidemiologists or accountants economists must surely be stretching the definitions of epidemiology and economics.

I believe that the specialty of epidemiology is not the prerogative of community medicine (though they generally need to have a greater appreciation of it than others). It is just one of the fields in which a community physician is more likely to be interested as a necessary tool in his job.

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Non-fatal lung disease due to inhalation of nebulised paraquat

SIR,—I would like to comment on the case reported in the *BMJ* of 29 March (p 902) of alleged non-fatal lung disease from inhalation of nebulised paraquat.

On the basis mainly of pulmonary function tests the patient was diagnosed as having interstitial lung damage. Such a diagnosis based on these results alone seems to be beyond the limits of the tests, especially as her chest radiograph was clear, she was obese with a wheezy chest, no transfer factor was available, and there were no previous recordings with which to compare those taken two months after the incident. It is understandable that clinicians faced with pulmonary function impairment should relate these effects to the spraying incident. However, there does not appear to be any real evidence that the lady was significantly exposed to paraquat. The

first symptoms of exposure to paraquat spray are irritation to the eyes and throat and nasal irritation, followed in severe cases by nose bleeds.¹

There is no mention in the report of the patient showing any of these symptoms. Furthermore, agricultural ground spray machinery does not produce significant amounts of respirable droplets. Even mist-blowers, which are seldom used with herbicides in England, do not produce significant numbers of droplets in the "respirable range." Consequently people exposed to paraquat spray may well experience nasopharyngeal irritation due to droplet impaction in that region, but would never incur lung effects due to inhalation. It is not surprising therefore that a computer search for an association between paraquat inhalation and pulmonary damage was fruitless. Pulmonary damage from inhalation of paraquat has never been reported in 20 years of worldwide usage of this herbicide. Published cases of pulmonary damage are nearly always associated with ingestion of paraquat with suicidal intent. The reference quoted by the authors is to a general description of the lung damage which follows ingestion, and not inhalation, of paraquat.

Further evidence that the reported lung damage was not due to paraquat comes from the chest radiographs, which were normal at all times, even when the patient exhibited signs of respiratory distress. Cases of pulmonary damage from paraquat ingestion are associated almost invariably with changes of the chest radiograph.² Thus the evidence presented does not support the conclusion that this patient had experienced lung damage as a result of paraquat exposure.

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¹ Swan AAA. *Br J Ind Med* 1969;26:322.

² Croft J, Douglas A. *Respiratory diseases*. 2nd ed. Oxford: Blackwell, 1975:691.

Critical evaluation of urinary oestriol estimation

SIR,—Mr E J Shaxted's critical evaluation of urinary oestriol estimation (8 March, p 684) highlighted some of the misconceptions that many clinicians still have about the value of urinary oestriol in antepartum fetal monitoring.

During the late 1960s and early 1970s biochemical studies of the fetoplacental unit received a lot of attention, as evidenced by the multitude of papers in various obstetric and gynaecological journals during this period of time; and oestriol studies (urinary or plasma, conjugated or unconjugated) have since emerged as the most widely used test for antepartum fetal monitoring. However, while automated analysers for urinary oestrogens are springing up in numerous hospitals, there has yet to be a paper published that can show convincingly that tests such as oestriol studies contribute to any fall in perinatal mortality, and their cost-effectiveness has never been established.

Instead, obstetricians have become more aware of the fact that these placental function tests are but "static" measures of events that occurred more than 24 hours ago and normal values do not exclude the possibility of intrauterine fetal demise—as Mr Shaxted has found out with his two cases of intrauterine death. It is evident, therefore, that to refine our methods of antepartum fetal monitoring

tests of the dynamic functions of the fetoplacental unit are required. Among these tests, the most useful ones are antepartum cardiotocography (or non-stress test) and the oxytocin challenge test, and a lot of work has also been done on fetal movements and fetal breathing.

A review of the four main obstetric and gynaecological journals (*British Journal of Obstetrics and Gynaecology*, *American Journal of Obstetrics and Gynecology*, *Obstetrics and Gynecology*, and *Obstetrical and Gynecological Survey*) during the two years 1978-9 reveals that, while there are 31 papers published in these journals on biochemical tests used for antepartum fetal monitoring, there are even more (64) papers on dynamic tests of fetal wellbeing. The papers on biochemical tests include three on α -feto-protein, 16 on oestrogen assays, and 12 on other placental proteins. The papers on dynamic tests include 34 on antepartum cardiotocography, 12 on fetal breathing, 11 on fetal movements, and seven on oxytocin challenge tests.

Perhaps the cheapest dynamic test of all is to give the patient a pen and a chart to record the fetal movements. This is probably the only one test so far that does not need cost-effective scrutiny.

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Pericardial aspiration

SIR,—Drs R J Warrell and R W Portal (29 March, p 919) have produced a number of practical and valuable suggestions in their piece on pericardial aspiration. I must take exception, however, to certain features of that paper.

For one thing Kussmaul's sign does not occur in tamponade unless it is complicated by underlying epicardial constriction. The right atrium accelerates its filling during inspiration in pure tamponade but this is restricted in constrictive pericarditis and manifested as jugular vein distention.¹ Whereas we formerly believed that this sign could be observed in uncomplicated tamponade (pulsus paradoxus being the same as "Kussmaul's double inspiratory phenomenon"), both experimental and clinical data indicate that this is not the case.^{1,3}

The use of an electrocardiography monitor during pericardial aspiration has proved in experienced hands to be of little or no value, and in some instances it can be misleading in that penetration of the heart can occur without any registration on the electrocardiogram. At two meetings in the United States of "pericardiologists" (at Lexington in 1969 and at Pittsburgh in 1980) a poll of those attending disclosed that the procedure has largely been abandoned in the USA.

In any discussion of pericardiocentesis I am slightly surprised that the use of a pericardial catheter has not been mentioned. For some of us this has for long been common.^{2,4} A polyethylene or polyvinyl catheter is passed through or over an appropriate needle (in some techniques utilising a guide wire) and the aspiration is carried out through the catheter. The advantages of this are that a relatively soft catheter tip contacts the heart as fluid is removed rather than a sharp needle, and that the catheter is also left in situ for repeated aspiration should fluid reaccumulate.

This is of greatest value in patients with malignant pericarditis and other forms in which reaccumulation is common.

These remarks are meant more to supplement than to criticise the authors' material.

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¹ Shabetai R. In: Spodick DH, ed. *Pericardial diseases*. Philadelphia: FA Davis Co, 1976:67-90.

² Spodick DH. *Acute pericarditis*. New York: Grune and Stratton, 1959:65.

³ Spodick DH. In: Levine HJ, ed. *Clinical cardiovascular physiology*. New York: Grune and Stratton, 1976:621-34.

⁴ Spodick DH. In: Conn HF, ed. *Current therapy*. Philadelphia: W B Saunders, 1974:214-6.

Pleural aspiration and biopsy

SIR,—The recent article by Dr P B Iles and Dr Colin Ogilvie (8 March, p 693) was an excellent description of the technique of pleural aspiration. Pleural and peritoneal effusions are a frequent complication of advanced malignant disease, and are treated by aspiration of the serous cavity affected. Recurrence of such effusions is often rapid, however, and repeated tapping presents a problem in the management of the cancer patient.

In one study, for example, 94 patients were treated by thoracentesis alone, and the average time to reaccumulation of the effusion was 4.2 days, with a 97% chance of recurrence at one month after treatment.¹ Because of this various sclerosing and chemotherapeutic agents have been instilled into the serous cavity following aspiration of the fluid in an attempt to prevent or delay recurrence. Sclerosing agents are poorly tolerated because of pain; and thiotepa, which is the most commonly used chemotherapeutic agent, carries the risk of bone marrow depression owing to systemic absorption. Paladine *et al*² reported that intracavitary bleomycin was well tolerated and was effective in preventing the recurrence of malignant effusions.

Following this report a British multicentre study was set up to investigate the use of bleomycin in a larger number of patients. Response to treatment was defined as no recurrence of effusion, or asymptomatic recurrence not requiring tap, for at least 30 days. A total of 153 patients were treated in 32 hospitals by aspiration of their effusion, followed by intracavitary injection of 30-150 mg bleomycin in 100 ml saline. The most commonly used dose of bleomycin was 60 mg. There was an overall response rate of 58% in 117 assessable patients, pleural effusions responding better (63%) than peritoneal (49%). Pleural effusions associated with carcinoma of the breast showed the best response to treatment (70%). Side effects were mild and transient, and there was no pulmonary or mucosal toxicity. The two most common side effects were fever (8%) and pain at the site of injection (10%). The incidence of the latter was principally following paracentesis abdominis.

These results are in agreement with those of Paladine *et al* and confirm that the response to bleomycin is comparable with that obtained with the more commonly used agents. Trotter *et al*³ reported two possible drug-related deaths in their series of 23 patients treated with intracavitary bleomycin, which otherwise