by two of the following three tests: standard agglutination, anti-human globulin, and complement fixation. Thirteen of the 19 patients were diagnosed during 1971-2 compared with five in 1973-4 and one in 1975. These findings indicate that the brucellosis eradication campaign has had considerable effect in Ayrshire. Moreover, it should convince clinicians that brucellosis is now unlikely in this area, and that they should rarely request laboratory tests for brucellosis.

CONSTANCE A C ROSS
ALEX MCCARTNEY
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Compensation for congenital defects

SIR,—While supporting the basic idea of extending the protection of the law to the possible liability to a child born disabled (leading article, 28 February, p 482), a number of Members of Parliament are most concerned about the weaknesses in the Bill on scientific grounds.

Already several consultants and medical practitioners concerned with obstetrics and paediatrics have voiced their concern too, but no amendments have been put forward during the committee stage in the Commons. If the misgivings which have so far been expressed by Lord Pearson, Mr Ian Kennedy and Dr R G Edwards have support, MPs would welcome the comments of the medical profession as to what amendments might be made to improve or change this Bill.

The primary concern of the Congenital Disabilities (Civil Liability) Bill must be for the wellbeing of victims of congenital disabilities. As the Bill stands at present a baby must live for 48 hours before compensation for loss of expectation of life may be recovered. The Bill also contains different liabilities in terms of negligence concerning the causation of fetal damage.

After all the problems that arose over the thalidomide disaster it is natural that the law should seek to resolve the problems of congenital liability. If there are further comments in addition to the Law Commissioners' report and the Law Society working paper on this subject, Members of Parliament and their Lordships would welcome them.

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Rib pain

SIR,—In your leading article (14 February, p 358) on rib pain, an exhaustive list is published of the more and less frequent causes of non-visceral thoracic pain. However, the most common lesion of all in civilian practice—a lower thoracic disc protrusion—is omitted. Just as displacement of a fragment of cervical disc may result in root pain in the upper limb and a lower lumbar disc protrusion can impinge against the nerve root causing sciatica, so is intercostal root pressure common at the sixth to twelfth thoracic levels.

The difference is that cervical and lumbar disc herniations often draw attention to the diagnosis by provoking a clear root palsy. This is not to be expected in thoracic root pain: a palsy or pressure on the spinal cord is seldom encountered. The signs are thus less obvious and a diagnosis of muscle strain, intercostal neuritis, or (since a deep breath hurts) "pleurodynia" may be made. Indeed, presentations of other peridiscal onset, in which root pain is present without any posterior component, may be thought of as gastritis, cholecystitis, chronic appendicitis, or a renal disorder.

It is important that these lesions should be recognised by doctors; for, quite apart from patients' relief, they afford apparent confirmation of lay manipulators' mistaken ideas that visceral disease can be put right by spinal manipulation that they suppose alters symptomatic pain. If a doctor has diagnosed a visceral disorder and, after many months of negative investigation and fruitless treatment, the patient finds a few simple twists who can blame the laymen for advancing so advantageous a notion?

For the sake of the good name of our profession and the advancement of scientific concepts I hope thoracic disc lesions will receive the attention they deserve.

J H CYRIAX
London W1

SIR,—I was surprised to find in your leading article on this subject (14 February, p 358) that the impression was given that most primary rib tumors were benign. After listing a number of benign rib tumors, including chondroma, solitary myeloma, and fibrous dysplasia, the article went on to say that primary malignant tumors are rare. This is not in accord with the literature, where, in most series, the malignant cases outnumber the benign ones.

In the chest cage generally (and often it is difficult to determine if a tumor has originated in the rib or in adjacent tissue) my own series of primary tumors includes 21 definitely malignant tumors, three plasmacytomas, which may or may not be considered benign, and other tumors which were definitely malignant. These included four haemangiomas, one subperiosteal lipoma, one eosinophilic granuloma, and four cases of fibrous dysplasia.

The malignant tumors are usually locally malignant, and the results of surgery, if it is bold and radical, are good. The results of procrastination or ineffective surgery are very bad, for the tumor will extend inexorably and is usually radioresistant. It seems to me, therefore, very important that there should be a broad understanding that chest wall tumors should be taken seriously, and, although the article mentions that chondromas should be removed, I do not think it stresses enough the frequency of malignancy.

JOHN DARK
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SIR,—Hitherto few of your leading articles have been of much positive help to clinical general practice. "Rib pain" (14 February, p 358) is a notable exception because it views the problem of an undiagnosed symptom—what is exactly what patients present to their GP's. Perhaps, following your example, the organisers of postgraduate lectures for GP's will structure their courses in similar manner rather than give us the standard rehash of revision notes for the MRCP examination.

On the specific topic of rib pain I wonder if I might add to your list two cases commonly found in general practice: Bornholm disease and pulled or torn fibres of the diaphragm. These conditions themselves give further illustration of the gulf that sometimes appears between the academic and the practising clinician. The academic might argue that neither condition has anything to do with pain specifically in the ribs. The clinician, however, starts with the words of the patient and not those of the textbook. Secondly, both diagnoses are virtually unprovable within the practical framework of clinical practice. This might upset the academic who has little insight into the fact that his own treated factual proofs and demonstrations are themselves little more than inspired guesses that may well look a bit silly in as little as 10 years from now, let alone in a hundred. The evolution of the "Medical Practice" section of the BMJ has been a notable acknowledgement of your own acceptance that not all problems can be discussed, let alone resolved, within the rigid framework of research. By which you, sir, do not yet go far enough. Perhaps your train of thought would have led you further into the exciting world of clinical general practice if you had begun your leading article with, "When a patient says he has rib pain what does he mean?".

ROBERT LEFFER
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SI units

SIR,—The recent introduction at this hospital, under protest, of SI units has been difficult and expensive and has brought, as was predicted, no advantage but instead danger and confusion. No doubt danger and confusion will abate as we become accustomed to these units but neither the scientists nor the clinicians who work here expect that any benefit will accrue from the change. The arguments in its favour are flimsy and pedestrian and an appeal to international conformity comes ill from those who have put us out of line with the United States of America in this regard.

It is not hoped that we can revert to former practice, and the damage which has been done will perhaps be minimised by not trying to do so. We are concerned rather to prevent a repetition of this unnecessary misfortune, for it is not to be supposed that SI units are the last word of the theoretical reformers. There may be some who will advocate the replacement of decimal counting by binary notation.
or one of its multiples; hours, minutes, and seconds must be a thorn in the flesh of avid rationalists; while even the SI units contain some compromises with what has been called pure time.

So threatened, we ask only that the authorities decline in future to consider any proposal for changes in notation which come to them from any individual or group, however distinguished, unless the institutions which represent the clinicians of the nation shall have had a part in its making.

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Radiology and endoscopy in acute upper gastrointestinal bleeding

Sir,—I am interested in the paper by Dr G M Fraser and others (31 January, p 270) and the reaction to this by Dr R F R Schiller and his colleagues (14 February, p 393).

As a radiologist I have stated my opinion elsewhere that “endoscopy undertaken by an experienced endoscopist takes pride of place in the investigation of the acute upper tract bleed.” However, it is important to realise that emergency endoscopic services are not available to all and to take note of the remarks on this subject expressed by Forrest et al.5 At the same time we should also remember that the patient suffering an acute bleed is admitted to the nearest acute hospital, whether emergency endoscopy is or is not available within 12–24 hours of admission. It is well known that, for obvious reasons, an upper tract endoscopy is not possible until the patient is in a stable condition. Furthermore, not all patients are available for emergency endoscopy and a patient at risk of bleeding must undergo emergency endoscopy, which is not always possible if the patient is not admitted to a hospital with an endoscopy unit.

The criticism by endoscopists that radiology may show a lesion but cannot demonstrate that this lesion is the source of bleeding is not a valid argument. Double-contrast studies are capable of showing specific features characteristic of bleeding point which are never reproduced in any other situation.3 This additional information greatly enhances the value of emergency radiology.

It is interesting that Dr Schiller and his colleagues should refer to “this most recent attack on endoscopy, written by radiologists in defence of radiology.” I recall numerous papers written by endoscopists in favour of endoscopy and questioning the role of radiology in the investigation of the acute bleed. Constructive co-operation and not empire-sustaining sharpshooting from either side would serve the best interest of the patient and give most help to the clinician responsible for the management of the case. Surely there is a happy medium based on an understanding of the needs and merits of the individual case and the facilities available at the time of admission. I agree with Mr F P McGinn and his colleagues (14 February, p 394) that “the two methods of investigation are complementary, but if a choice must lie between them endoscopy should take precedence.”

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Chemotherapy for breast cancer

Sir,—Your interesting leading article “Curbility of breast cancer” (21 February, p 414) refers to trials in America. Perhaps it would not be out of place to remind your readers that the pioneer work in chemotherapy for breast cancer was begun in Bradford in 1957 by Dr (now Professor) R L Turner and the late Mr G Whyte Watson, and their first paper was published in the BMJ in 1959.1 It is not generally appreciated how much is owed to these two pioneers.

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Immunisation against whooping cough

Sir,—In writing to you to defend the papers by Dr Christine L Miller and Mr W B Fletcher (17 January, p 117) and Dr N D Noah (p 128) Dr T M Pollock (14 February, p 396) does a capacious mantle. He says that essential data were withheld from Dr Noah’s paper, for sake of argument and the substantive point which I should have thought concerned you, Sir, and Dr Noah. And he rebukes me for adhering to a basic tenet of epidemiology when I suggested that an association, however significant, between an independent variable (immunisation) and a dependent variable (disease) cannot be regarded as causal unless allowance is made for other variables known to influence susceptibility to the disease. Since the epidemiological data at large from Colindale discount all other variables, conclusions drawn from them are at best inferential. However, even without analysis of variables other than immunisation it is clear from both papers that the protection afforded by immunisation is highly incomplete since 36%, of all patients and 44% of patients aged 1–2 years described by Dr Miller and Mr Fletcher were fully immunised, as were 38% of the entire series presented by Dr Pollock.

Dr N W Preston (14 February, p 396) seems to be in conflict with all of us. He despises notifications, so he presumably distrusts the Colindale data. But he agrees with our conclusions because he regards the new vaccine used by the Colindale workers as being effective because, in previous letters, he has said so. He considers that the decline in whooping cough is due to this new vaccine but does not say how he would explain the greater decline which occurred before it began to be used in 1968. He asks us to accept the new vaccine as being non-toxic because he says so and calls upon the world at large to provide evidence to the contrary. He asks me to provide evidence before criticising the Colindale data but does not hesitate to refute my evidence before it is published. He will find, incidentally, the desirability of bacteriological confirmation (who wouldn’t?), but he must surely know that in practice whooping cough is a disease in which an experienced doctor or patient is as likely to reach a correct diagnosis as a bacteriologist.

Mrs Rosemary Fox (21 February, p 458) draws attention to the need to investigate the possibility that the new vaccine may occasionally be neurotoxic. In my view she is correct in requesting a retrospective investigation, for it may be some years before the prospective survey authorised by the DHSS yields useful evidence.

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Primary gout affecting the sternoclavicular joint

Sir,—The short report by Dr G R Sant and Mr B Dias (31 January, p 262) cannot be allowed to go unchallenged. The authors have carefully documented two examination errors in the diagnosis and management of this disease.

It is of course unjustifiable to diagnose gout of the sternoclavicular joint in an 18-year-old girl purely on the basis of raised plasma uric acid levels obtained at a time