but with the exception of the two patients who did show a little dizziness we could not show any significant postural drop in blood pressure in the series.

Prazocin was not continued in all of our patients since this was not part of the experimental design of this study. We have previously published longer-term studies on prazocin, one of which was a controlled double-blind crossover study of 26 weeks' duration. I do not think one needs to try and defend not continuing the therapy in some cases where this was not part of the experimental design; in any case, it was related to some very mundane things such as tablet supply, transfer of patients to other clinics, and the like. The same answer could be given to the next question, which was, Why did only seven volunteer for rechallenge with a smaller dose of the drug? The reason is that only seven were asked to do so; the other five were lost to follow-up through a variety of reasons, none of which was related to the previous prazocin therapy. Therefore the rather sinister assumptions made by Dr Bendall are completely unjustified and the suggestion that these patients must be regarded as "failures" in this study is beyond comprehension.

Dr Bendall suggests that the whole study is in any case ethically unacceptable. This must of necessity be a highly subjective judgment; I do not regard it as ethically unacceptable to rechallenge patients with a very much smaller single dose of potentially valuable antihypertensive agent, under careful observation in a hospital environment, in order to test the hypothesis that the side effects are dose-dependent. Rechallenge is an accepted technique widely used in diagnostic and therapeutic as well as experimental situations. If we were to stop these studies, not only would it inhibit a large area of experimental therapeutics, but also many studies designed to determine the needs of individual patients could not be done. In any case, the study was completely consistent with the final paragraph of Dr Bendall's own short report in the *BMJ*¹: "We conclude that the recommended starting dose of 2 mg is too high and that further investigations are needed to establish a safe starting dose . . ."

I am happy to say that I can agree with one remark in Dr Bendall's letter. That is that the general acceptance of prazocin must await adequate clinical trials in which the 0.5-mg starting dose is compared with other, established, antihypertensive agents.

C ROSENDORFF

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¹ Bendall, M J, Baloch, K H, and Wilson, P R, *British Medical Journal*, 1975, 2, 727.

Chemotherapy in breast cancer

SIR,—Your leading article on this subject (9 October, p 832) does less than justice to the item on cancer treatment broadcast in the BBC Radio 4 "World Tonight" programme on 7 July. This programme, quite properly, included statements by Dr Frank Rauscher, Director of the National Cancer Institute in the USA, in which he made certain claims about the success being achieved in treating breast cancer by chemotherapy. Because the claims were so surprising the programme gave roughly

equal time to a member of the British Breast Group, who gave the background to the claims and who stressed the caution with which they should be regarded. The programme was both accurate and balanced and did not transform the American claims "into a triumphant fanfare" as your article suggests.

JAMES WILKINSON
Science correspondent,
British Broadcasting Corporation

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* * James Wilkinson misses the point. Cancer sufferers will always clutch at straws, and—as we know from the episode of the Issels clinic—if they hear of a new form of treatment they will want it. What has happened in the USA, and what we hope to avoid here, is clamour by patients for adjuvant chemotherapy before it has been evaluated. The presence of a cautionary spokesman to "balance" the enthusiasm of Dr Rauscher did little to diminish the impact of his claims on an uncritical audience.—Ed, *BMJ*.

Compulsory seat belts

SIR,—It is all too easy to fall into uncritical agreement with Dr T H S Burns (16 October, p 946) and to believe that wearing seat belts increases carelessness on the grounds that the consequences of careless driving are mitigated by the wearing of a seat belt.

Having myself always worn seat belts since the British Standard was published in the early 'sixties and talked to many people since, I am convinced that wearing a seat belt heightens awareness of the consequences of careless driving and leads to safer driving rather than the reverse.

M J W BARKER

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Neonatal electrocardiogram

SIR,—How strange that Dr D P Southall and his colleagues (11 September, p 615) should state that their preliminary observations of the neonatal electrocardiogram have identified the need to define more clearly the range of normal variation without telling us what yardsticks they have found wanting.

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* * We sent a copy of this letter to Dr Southall, whose reply is printed below.—Ed, *BMJ*.

SIR,—We indicated in our article (11 September, p 615) that several workers had previously given yardsticks of normality for interpretation of the neonatal electrocardiogram. Since these studies are quoted in most textbooks of paediatric cardiology, including Dr Watson's own,¹ we felt it would be tedious to list these textbooks as references.

The usually quoted source references for tables which appear in textbooks are those of Walsh² and Leibman.³ The former had a relatively small number of patients, while the latter recategorised previously published data. We found that many of the values in normal neonates fell outside their criteria for normality,

particularly in respect of voltages and axis. However, until more data are at hand we do not feel justified in producing new tables; we merely wish to alert cardiologists to the possible deficiencies of the present tables such as those published in Dr Watson's textbook.

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¹ *Paediatric Cardiology*, ed H Watson. London, Lloyd-Luke, 1968.

² Walsh, S Z, *British Heart Journal*, 1963, 25, 784.

³ Leibman, J, *Electrocardiography in Infants and Children*, pp 79-98. New York, Grune and Stratton, 1966.

"Press that bruise"

SIR,—Surely the argument used by Dr Michael Bennett (23 October, p 1010) is fallacious. The application of pressure in controlling haemorrhage is related to but not dependent on the bleeding time. When the test is performed no pressure is applied to the wound site, and the time of the spontaneous cessation of bleeding recorded.

Using Dr Bennett's logic, pressure in excess of 7 min should be applied to the site of every venepuncture. Mr B M Tracey (2 October, p 814) is right to "press that bruise" for 3-5 min.

PETER KIRK

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Hyposensitisation with house dust mite vaccine in bronchial asthma

SIR,—In commenting on our article (4 September, p 561) Drs J O Warner and J F Price (16 October, p 945) point out that trials of treatment in bronchial asthma are difficult because of the heterogeneity and intermittent nature of the disease, a point of which we are well aware. Only patients who satisfied the accepted criteria for the diagnosis of asthma¹ were included in the trial, and all the patients had a rise in the forced expiratory volume in 1 s (FEV₁) of at least 15% 14 min after the inhalation of an aerosol bronchodilator, so that they all had bronchial lability. Their sensitivity to the house dust mite was based on criteria used by other authors—namely, a positive prick test reaction to *Dermatophagoides pteronyssinus* and also either an unequivocal history of symptoms being provoked by contact with household dust or a positive reaction to nasal challenge with *D pteronyssinus*.² This latter test has been found to provoke positive reactions in 86% of house dust mite sensitive subjects, as does bronchial challenge in the same proportion, but nasal challenge does not lead to attacks of asthma and is therefore safer.³

Before treatment total and specific *D pteronyssinus* serum IgE levels were measured, but the range was wide and this test was not helpful in identifying those who might benefit from therapy. We have found the FEV₁ to be as good an indicator of changes in bronchial calibre following a bronchodilator in patients with airways obstruction as the specific conductance (SGaw) measured in a body plethysmograph.⁴ Moreover, changes in the FEV₁ probably reflect changes in both small and large airways better than SGaw which is more affected by changes in the larger airways.⁵

The complete analysis of the results consists of 27 tables, but obviously a selection had to be made for our paper.

JOHN GADDIE
C SKINNER
K N V PALMER

¹ Ciba Foundation Guest Symposium, *Thorax*, 1959, 14, 286.

² D'Souza, M F, *et al*, *Clinical Allergy*, 1973, 3, 177.

³ McAllen, M A, Assem, E S K, and Maunsell, K, *British Medical Journal*, 1970, 1, 501.

⁴ Skinner, C, and Palmer, K N V, *Thorax*, 1974, 29, 574.

⁵ Pride, N B, *British Journal of Diseases of the Chest*, 1971, 65, 135.

should distinguish vomiting due to thyrotoxic hypercalcaemia, vomiting due to thyrotoxicosis per se, and their coincidences. In the series of Gordon *et al*² vomiting was found with equal frequency in both normocalcaemic and hypercalcaemic groups (about 30% each). In fact, the only distinguishing symptoms of the hypercalcaemic group were apathy and constipation.

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¹ Rønnev-Jessen, V, and Kirkegaard, C, *British Medical Journal*, 1973, 1, 41.

² Gordon, D L, *et al*, *American Journal of the Medical Sciences*, 1974, 263, 31.

³ Baxter, J D, and Bondy, P K, *Annals of Internal Medicine*, 1966, 65, 429.

⁴ Rose, E, and Boles, R S, *Medical Clinics of North America*, 1953, 37, 1715.

⁵ Twycross, R G, and Marks, V, *British Medical Journal*, 1969, 2, 701.

⁶ Harper, P S, and Osborne, Hughes, R, *British Medical Journal*, 1970, 1, 213.

⁷ Guyer, P B, *British Medical Journal*, 1965, 1, 169.

⁸ Richards, A J, *Postgraduate Medical Journal*, 1970, 46, 440.

⁹ Adams, P H, *et al*, *Quarterly Journal of Medicine*, 1967, 36, 1.

Risk of venepuncture

SIR,—We have recently had a peculiar complication of antecubital venepuncture. A 67-year-old man was admitted because of suspected pulmonary emboli. He received intravenous heparin intermittently. On the sixth day of therapy, after an antecubital venepuncture for diagnostic purposes, a hematoma of 6×8 cm developed, and he began to complain of severe pain with median distribution. In two weeks he presented a clear-cut carpal-tunnel syndrome without any signs of bleeding in this region. After six weeks he was operated on and the nerve was found to be compressed by a tiny blood clot. His state improved but permanent damage remained. It must be assumed that this small amount of blood wandered along the nerve from the antecubital region to the carpal tunnel, where it coagulated and showed its adverse effect.

DAVID KOHN
ASIA BUSH
ISIDOR KESSLER

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Israel

Thyrotoxic vomiting

SIR,—I wish to dispel two myths regarding vomiting in thyrotoxicosis.

Firstly, it would appear from the paper by Dr F D Rosenthal and others (24 July, p 209) and from the subsequent correspondence that vomiting in patients with thyrotoxicosis is rare. This is not so. Rønnev-Jessen and Kirkegaard¹ reported vomiting in seven out of 28 patients over 60 years of age. Gordon *et al*² in a prospective study found thyrotoxic vomiting in 35 of 121 patients. The omission from textbooks of any mention of vomiting as a symptom of thyrotoxicosis is to be blamed for the general unawareness of this symptom, since diagnosis is based on mental processes trying to find the closest match between the patient and the textbook "disease."

The second point, touched upon by Dr P H Sanderson (4 September, p 584), is the association of thyrotoxic hypercalcaemia with vomiting. Although hypercalcaemia in thyrotoxicosis is as common as in sarcoidosis (about 17%),^{2,3} it has had the reputation of a rare complication for the same reasons as for vomiting. Rose and Boles⁴ and Twycross and Marks⁵ each reported three patients with thyrotoxic hypercalcaemia, and all six patients also had vomiting. Single cases of combined hypercalcaemia and vomiting in thyrotoxicosis were described by Harper and Hughes,⁶ Guyer,⁷ and Richards.⁸ While both hypercalcaemia and vomiting in thyrotoxicosis are unrelated to the severity of the disease,^{2,3,9} vomiting may also be a symptom of hypercalcaemia. Thus we

SIR,—I wonder if the vomiting described by Dr F D Rosenthal and others (24 July, p 209) might be due to too rapid ingestion of large amounts of food. I have observed this mechanism in one patient with gross thyrotoxicosis. Treatment with carbimazole resulted in more leisurely eating *pari passu* with cessation of vomiting.

B I CHAZAN

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Coronary artery disease and coalworkers' pneumoconiosis

SIR,—I write to support Dr Dewi Davies (16 October, p 925). It seems that the DHSS have drawn sweeping conclusions from the articles quoted, which are by no means proved. Even the authors themselves only postulate certain possible explanations. Unfortunately the DHSS loftily expresses its views as based on "authoritative opinions," suggesting that nobody else has any authority or is worth considering, and by incorporating these in "case law" wrongly turns a theory into a proved fact, failing to realise that one of the conclusions which could be drawn from the articles quoted is that death from coronary thrombosis can be avoided by tying a ligature around one's neck to obstruct the airway and then arranging to die from something else at the earliest possible moment.

We do not know all about the cause of chronic bronchitis or emphysema or why some cases of pneumoconiosis progress to progressive massive fibrosis. We do not know the cause of coronary atheroma or why some cases develop thrombosis which may or may not lead to myocardial infarction. How, in the midst of all this ignorance, can we produce "authoritative statements" on the interaction of the two conditions?

In practice these statistical surveys mean little. One is faced with the problem of a patient who has in fact developed both myocardial infarction and pneumoconiosis and died. I do not think that anybody can say that pneumoconiosis did not play some part in the cause of death or that it did not have any effect on the heart because I do not believe that anybody can say whether a patient with myocardial

infarction will recover or not. I do a large number of post-mortem examinations and it is quite obvious that many, many people, often dying from unrelated conditions, have had previous extensive myocardial infarction which has completely healed without the patient having made any serious complaints or requiring treatment.

It seems to me, therefore, that in the light of our present proved knowledge Dr Davies's opinion is the only reasonable one and, indeed, the only humane one. There is something rather objectionable about a government department using the whole of its weight to sweep aside the claim of a widow whose husband probably died as the result of working and at the same time hastening to distribute money to drug addicts and the like.

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Professional standards

SIR,—As a practising surgeon in a district hospital I find myself becoming increasingly concerned about the professional standards of my recently qualified colleagues. I get the impression that their undergraduate training has concentrated on the "scientific" rather than the clinical and practical aspects of medicine. Many do not appear to have learnt how to take a proper history, how to perform a proper clinical examination, and most have virtually no experience of practical procedures.

Perhaps even more serious is their lack of "professionalism." I do not refer solely to the matter of dress and address of colleagues (and other staff) in front of patients, but mainly to their attitude towards their job and patients. Too often they seem to lack interest, inquisitiveness, and a sense of personal responsibility to patients under their care, even if their contact with the patient is of brief duration. Many do not understand that every doctor has a legal responsibility to take "due care" within the limit of his knowledge and competence: they do not appear to have been taught what constitutes "due care."

The practice of medicine is based on compassion and caring; all possible and foreseeable risk to the patient—not the doctor—should be eliminated so far as is practical. In other words, as is implied in the word professionalism, we must strive for, even if we cannot achieve, perfection. We cannot condone any fall in the standard of care given to our patients. There is only one yardstick by which such care should be measured. Would we like to be on the receiving end?

M C T MORRISON

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Traumatic rupture of ear drum

SIR,—Recently I saw an overseas student who had just sustained a traumatic rupture of his ear drum from a cause I have not met before. His girl friend had inserted her tongue into his auditory meatus for his delight and when she removed it he experienced a sharp pain which had persisted. Examination showed a traumatic perforation of the drum with haemorrhage. Presumably the tongue had