varying from 14% to 53%.¹ Pleural effusion occurs in 3-17% of patients. These are often haemorrhagic, usually painless, without pleural features, and three times more likely than other effusions to be left sided.² Other forms of intrathoracic involvement are elevation of a hemidiaphragm, atelectasis, pulmonary infarction, and pneumonia with various infiltrates present.³ It is generally agreed that pulmonary complications are more frequent in fatal cases, especially if true adult respiratory distress syndrome occurs when the mortality is high.² Bronchopleural fistula is also reported, and the possibility that this complication will develop was the rationale for the removal of the enzyme-rich pleural fluid. The possible routes of pulmonary involvement have been well reviewed.¹ There is also agreement that an amylase level in the pleural fluid elevated above that of the serum amylase assayed simultaneously is diagnostic, minor elevations occurring only infrequently.¹

Acute pancreatitis can occur following trauma or upper abdominal surgery and also occurs with increased frequency in patients taking corticosteroids. It is, however, unusual for pleuropulmonary involvement to be the presenting feature, as was the case with our patient. This case illustrates the value of a simple laboratory investigation and gives some indication of the circumstances in which pancreatic involvement may be the primary pathology.

We wish to thank Mr A Lewis and Professor A V Hoffbrand for enabling us to study this patient.

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¹ Kaye MD. *Thorax* 1968;23:297-306.

² McKenna JM, et al. *Chest* 1977;71:197-204.

³ McWilliams A, Cross R. *Am Surg* 1974;40:448-52.

Comparison of continuous and intermittent papaveretum after cholecystectomy

SIR,—There is nothing revolutionary about the conclusions that Dr J A Catling and others draw from their comparison of continuous and intermittent narcotic analgesia for the treatment of postoperative pain (16 August, p 478). The effects of narcotics on the respiratory centre have been known for many years and the low therapeutic index is well documented. This paper, like all those assessing the efficacy of analgesics, suffers from the wide variability in the subjective response to pain, the total dose in each group varying more than sevenfold.

Table II records the change in breathing pattern of the seven patients in the intramuscular group with that of seven patients in the intravenous group. No conclusions can be drawn from this table without knowing the normal respiratory pattern during sleep of each patient. Case 13, a woman of 73, appears to have not only periods of prolonged apnoea but also a persistently low respiratory rate. However, over the 48-hour period she received a total dose of only 37 mg papaveretum, about 0.01 mg/kg/h, an unlikely dose to depress respiration to that extent.

The authors were fortunate in having the approval of their hospital ethical committee

for the insertion of arterial cannulas into each patient to assess the effect on blood gases of the two forms of analgesia. It was here that concrete evidence could have been obtained; unfortunately it was not. The arterial PCO_2 levels were marginally raised in the group with the better analgesia but levels around 6.2 kPa are of doubtful clinical significance. However, no conclusions can be drawn from PO_2 results owing to the enormous difference in the two groups after anaesthesia but before analgesia had commenced.

Finally, when the analgesic action of drugs in the treatment of postoperative pain is studied, the findings must be influenced by the peroperative analgesic used, particularly if given as required in supplementary doses. Would it not have been tidier to have given each patient a standard dose of analgesic at the onset of anaesthesia?

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*.*We sent this letter to the authors, who reply below.—ED, *BMJ*.

SIR,—We agree with Dr Church that there is nothing revolutionary about the idea put forward in our paper (16 August, p 478)—that apnoeic periods in the postoperative period, whatever their cause, will lead to potentially hazardous fall in PaO_2 . However, Dr Church's comments about respiratory depression are difficult to follow. On the one hand, he is already convinced of the respiratory depressant effects of narcotics. On the other hand, he searches for "concrete evidence" of respiratory depression in our blood gas data yet fails to recognise it. If he doubts the significance of a mean increase in $PaCO_2$ to 6.2 kPa what does he make of $PaCO_2$ values of 2 SD above that—that is, 8.0 kPa? There must be few traditional clinicians who fail to understand the significance of a PCO_2 of 60 mm Hg.

But this is not the real point of our paper. Our attention in this study was drawn to the respiratory patterns in our postoperative patients. Lugaresi *et al*¹ have reported that some normal subjects show apnoeic periods of not more than 5-15 seconds during light and rapid-eye movement (REM) sleep and Guilleminault *et al*² record a maximum number of 10-second apnoeas of four per hour (mean 0.5/h) in normal subjects. Because of this we chose a greater than 20-second apnoeic period to indicate abnormal apnoea and we found that these apnoeic periods were invariably more frequent than four an hour. It is remotely possible that, by chance, three out of seven subjects in our intravenous group had the very rare pathological sleep apnoea syndrome; otherwise we conclude that this was related to the drug regimens used in these patients.

We pointed out in our paper that the intravenous infusion was discontinued within 48 hours if pain had ceased. Case 13 was such a patient; the 37 mg of papaveretum was given in 24 hours, so the infusion rate was 0.02 mg/kg/h. Dr Church, while accepting that there is a wide variability in response to pain, finds the wide variability in respiratory responses unbelievable. Yet in his own paper, of three patients who had respiratory depression requiring reversal with naloxone, two were receiving an opiate dose only a factor of 1.5 greater than our patient—in whom, he claimed, respiratory depression was unlikely.

We share with Dr Church an interest in

providing adequate postoperative pain relief. Apnoeic periods, no matter how produced, result in a fall in PaO_2 no matter what the initial PaO_2 . To investigate the severity of these changes we are currently making continuous measurements of breathing pattern, oxygen saturation, and sleep staging in the postoperative period. The episodes which are life threatening last for a minute or so and cannot be recognised by intermittent measurements of respiratory rate or arterial blood gas tensions.

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¹ Lugaresi E, Coccagna G, Cirignotta F, et al. *Adv Exp Med Biol* 1978;99:35-45.

² Guilleminault C, Van den Hoed J, Mitler MM. In: Guilleminault C, Dement WC, eds. *Sleep apnea syndromes*. KROC Foundation Series, vol 2. New York: Alan R Liss Inc, 1978:1-12.

Anthropometric and dynamometric variables and serious postoperative complications

SIR,—I was most interested in the article by Mr A M Klidjian and others (4 October, p 899). May I make one comment? It does not detract in any way from the possible value of their screening test, but I ask myself whether it is really detecting malnutrition, and if so to what extent.

It is interesting that among the measurements taken weight loss, which is probably consequent on the illness, showed no significant correlation with complications; whereas measurements of muscularity and body shape do so. Among these forearm girth, which is least affected by subcutaneous fat, is the most significant, together with strength of handgrip. The relationship between body type and strength as measured by lumbar pull dynamometer was reported for healthy subjects in 1958.¹ I suggest that body type with its genetic element is a more important contributory factor than malnutrition. More muscular people have a relatively greater reserve store of protein on which to draw in time of adversity.

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¹ Parnell RW. *Behaviour and physique: an introduction to practical and applied somatometry*. London: Edward Arnold, 1958: 54-8.

Antibiotics in surgical treatment of acute abscesses

SIR,—Your correspondent Mr Roger Grace states that primary suture of an abscess after incision and curettage under antibiotic cover is not appropriate for anorectal sepsis (20 September, p 813). He seeks to establish this point from the information contained in our paper (12 July, p 111). This paper was not written to give specific guidance on the treatment of acute anorectal infection. Its purpose was to report that there is no delay in the healing time of an abscess if the customary postoperative course of antibiotic therapy is omitted, provided that the surgery has been performed under an effective antibiotic cover.

Apparently Mr Grace does not appreciate that we make a clear distinction between an

anorectal abscess and a fistula in ano. The latter condition, because it communicates with the anal canal, clearly is not suitable for primary closure; but the former condition responds extremely well to this treatment. However, we accept that this diagnostic distinction is not always easy. When reporting the late results of 100 patients treated for anorectal abscess we discovered that seven patients were subsequently found to have a fistula.¹ We suspect that this was due to errors in the original diagnoses rather than to late complications of the abscess. This same study showed that 15 patients had a recurrent abscess within the succeeding two years and, more important, 78% of patients had no recurrence or complication.

More recently we have described the details of the operative technique when reporting its efficacy in treating a consecutive series of 150 patients.² We have also published the results of a randomised trial involving 219 patients with anorectal abscesses.³ This latter study showed that treatment by incision, curettage, and primary sutures was preferable to the conventional treatment as judged by the patient's comfort, healing time, time off work, and the incidence of recurrence. We continue to use it and recommend it for anorectal abscesses and for all other acute pyogenic abscesses.

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¹ Wilson DH. *Br J Surg* 1964;**51**:828-31.

² Jones NAG, Wilson DH. *Br J Surg* 1976;**63**:499-501.

³ Leaper DJ, Page RE, Rosenberg IL, Wilson DH, Goligher JC. *Dis Colon Rectum* 1976;**19**:46-50.

Hazards of surgical glove powders

SIR,—Your leading article (4 October, p 892) indicates that the risk of granuloma formation from glove powder is small, but there is now sufficient evidence to refute the anecdotes of many surgeons that the phenomenon does not occur "in their hands." I am among those who unwittingly have inflicted this iatrogenic problem on a patient and am stimulated to recommend a method which I believe reduces substantially the amount of powder left on surgical gloves.

When the gloves have been put on the hands I continue cleansing the gloves with povidone-iodine surgical scrub cleanser (Betadine) for a further minute. This agent contains non-ionic surfactants, which mobilise loose material on the rubber, and iodine, which combines with the starch to form blue-black starch iodide. The starch iodide acts as a visible indicator, is water soluble, and is washed off easily under running sterile water. The gloves feel tacky after this procedure—which, I believe, indicates that the lubricating powder has been substantially removed. I would be interested to hear if this simple method has been put to the elaborate scientific tests¹⁻⁴ that surround this subject.

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¹ Jagelman DG, Ellis H. *Br J Surg* 1973;**60**:111-4.

² Kent SJS, Burnand KG, Owen D. *Ann R Coll Surg Engl* 1975;**57**:212-4.

³ Henderson WJ, Melville-Jones C, Barr WT, Griffiths K. *Br J Surg* 1975;**62**:941-4.

⁴ Grant JBF, Davies JD, Jones JV. *Br J Surg* 1976;**63**:867-9.

Breast cancer trials—a new initiative

SIR,—Patients selected for inclusion in the trial of adjuvant systemic therapy proposed by Professor Michael Baum and others (13 September, p 742) would presumably have received conventional local treatment. Yet much of the controversy surrounding the management of early breast cancer revolves around the extent of the primary surgical procedure. Surely it is of crucial importance both to the clinician and, not least, to the patient to determine whether breast conservation is a reasonable alternative to mastectomy.

A randomised, prospective trial of adjuvant systemic therapy in patients treated with surgical techniques that conserve the breast would answer both questions posed by these investigators. Such a study would not only provide an assessment of the risks of recurrence but also determine the impact of adjuvant therapy on patients treated by less radical procedures. This alternative proposal deserves consideration and might commend itself to those clinicians reluctant to participate in recent clinical trials.

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Parity and breast cancer

SIR,—Dr K L Woods and others (9 August, p 419) recently reported an inverse association between parity and age at presentation of breast cancer. The report prompted an examination of this relationship among 611 cases of breast cancer and 1826 patients hospitalised with other diseases (controls) in the county of Glamorgan in South Wales. These data were collected as part of a population-based case control study performed in seven areas of the world in the middle and late 1960s, the details of which have been reported.¹ Glamorgan provided the population most similar to that used by Woods *et al.*²

The women with breast cancer were interviewed within a few days of diagnosis and the controls at the time of their selection as controls. The mean ages at presentation of nulliparous and parous women were 58.86 years and 57.26 years respectively ($P(2) = 0.15$). Among parous women the mean age at presentation did not decrease with increasing parity, as shown in the table. In fact, the data suggest if anything an increase in average age with increasing parity; however a similar relationship was evident when the age at interview of hospitalised controls was stratified by parity level. Thus there is probably no meaning to the trend in the cases.

The relative risk of breast cancer associated with being parous, at successive ages at interview, was also examined. If, as suggested by Dr Woods and his colleagues, pregnancy accelerates tumour growth among women who had a malignant transformation at an early age, cases of breast cancer diagnosed among

young women should have occurred predominantly among the parous subgroup and the relative risk of breast cancer associated with parity should be greater than 1.0. In addition, the protective effect previously observed to be associated with parity would have been largely restricted to cases diagnosed at an older age. The table offers little confirmation of the proposed hypothesis in the age groups noted by Wood (χ^2 (linear) trend) = 0.07; $p > 0.90$). Subdivision into narrower intervals of age at presentation within the range 55-65 years did not reveal any trend in relative risk.

All these analyses were repeated in a comparable population-based set of data from Boston, comprising 580 cases and 1736 hospital controls.³ The results were very similar to those in the Glamorgan data and did not confirm the observation of Dr Woods and his colleagues.

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¹ MacMahon B, Lin TM, Lowe CR, *et al.* *Bull WHO* 1970;**42**:185-94.

² Lowe CR, MacMahon B. *Lancet* 1970;**ii**:153-6.

³ Salber EJ, Trichopoulos D, MacMahon B. *J Natl Cancer Inst* 1969;**43**:1013-24.

Mexiletine for supraventricular tachycardia

SIR,—The recent article by Dr Edward B Leahey and others on combined mexiletine and propranolol treatment of refractory ventricular tachycardia (2 August, p 357) prompts us to bring to your attention our experience with mexiletine in the management of resistant supraventricular tachycardia.¹

This concerns four patients in whom mexiletine corrected eight out of 10 episodes of supraventricular tachycardia. In five instances the prior administration of practolol with or without other drugs had been ineffective. In one a supraventricular tachycardia with aberrant conduction was mistaken for ventricular tachycardia and lignocaine was given without effect followed by mexiletine, which terminated it. This was our index case, which first led us to examine the effect of mexiletine on supraventricular tachycardias. In the remaining two instances mexiletine was the only drug used and successfully aborted the arrhythmia. In the two episodes in which mexiletine failed verapamil was also given in one and disopyramide in the other, but DC cardioversion was required in both.

Mexiletine until now has been advocated only for ventricular arrhythmias, but these results suggest a useful role also in supraventricular arrhythmias. It has a "class-1" anti-dysrhythmic effect on atrial as well as ventricular muscle, and has also been found in isolated rabbit atria to reduce the maximum driven frequency and the conduction velocity and to raise the electrical threshold.² It is also possible that in the five instances where practolol

Age and parity in breast cancer

Parity	..	..	..	1	2	3	4	5	>6				
Mean age at presentation	..	..	..	55.7	55.5	57.4	60.1	57.0	67.0				
No of cases	..	..	..	120	139	91	50	21	29				
Age at interview	..	<30	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	70-74	>75	
Relative risk	..	2.0	0.7	1.6	0.6	0.4*	0.6	0.7	0.6	0.8	0.4*	0.6	
No of women (611 cases and 1826 controls)	..	..	11	37	194	306	291	334	339	323	276	186	140

* $p < 0.05$.