cholecystectomy, however, is generally done by one person, and it is difficult for anyone other than the operating surgeon to observe what is taking place. How do Baxter and O'Dwyer propose to teach the minilaparotomy procedure to surgeons in training, particularly if future trainees have little or no experience with the open operation?

Perhaps the most pressing reason for leaving minilaparotomy cholecystectomy out of the equation at this stage is that it is not yet recognised as a routine procedure.

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Low protein diets in chronic renal insufficiency

SIR,—In their meta-analysis of studies of low protein diets in chronic renal insufficiency D Fouque and colleagues state that the analysis was restricted to randomised, controlled prospective studies that did not include diabetic patients.[1] They then list 18 studies that were retrospective or non-controlled or had crossover design and two studies of diabetic patients. This list seems unnecessary. Table I lists 16 studies that seem to fit the authors' criteria for consideration, yet only six were selected and one of these is unpublished. It is not clear why the remaining 10 were excluded.

For one of the six studies selected[2] the most recently published follow up of the study population was not used.[3] The dietary prescription was heterogeneous in the studies selected, with the dietary protein intake in the treated group in one study being identical with the intake in a control group in another study. In one study no significant dietary difference was observed between the control and treatment groups[4]; any effect on renal death in this study is unlikely to have been due to this non-significant intervention.

The authors conclude in the abstract that low protein diet delays the onset of end stage renal disease and then seem to contradict themselves in the discussion when they state that the diet does not reduce the progression of renal disease. It is difficult to see how a reduction in renal deaths in the treatment group fails to represent a reduction in the progression of renal disease as the fall in glomerular filtration rate is linear with time for many renal diseases.[5]

I believe that this paper does little to help understanding of the place of a low protein diet in the management of chronic renal failure.

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AUTHORS' REPLY,—Several of James D Walker's points deserve further comment because they were probably underdeveloped in our report.[1] We found 28 trials in the literature, of which we rejected 22 because they were not randomised, leaving six for the meta-analysis. Reports of meta-analysis should give the rejected trials and the reasons for exclusion. Hence we listed the 22 studies that did not fit our selection criteria.

The most recent results from the study of Rosman *et al*[2] do not differ, as far as the end point of interest to us was concerned, from those in their previous paper.[3] They reported a trend towards fewer renal deaths in patients treated with protein restricted diets, although this was not significant.

As Walker says, the dietary prescription was heterogeneous, but the gradient of protein intake between treated and control subjects was considered to be the therapeutic factor: there is no consensus on the ideal protein restriction during chronic renal failure.

The 40% reduction in renal deaths observed in the Italian study, even if not significant,[4] was observed for a true restriction of 0·2 g/kg/day instead of the 0·4 g/kg/day prescribed, as recalculated by Gretz and Strauch.[5] Although statistical tests did not show a difference in protein intakes between the two groups, that does not prove that there was no difference (type II error). Patients' protein intakes were reduced and fewer renal deaths occurred in the low protein group. We believe that this reduction would have been greater if better compliance had been achieved.

If end stage renal disease is delayed by low protein diets this may result from a reduction in the progression of chronic renal failure or a reduction in uraemic symptoms, and from our end point it is not possible to decide. But, as Levey and Shah showed, special attention must be given to the precision of glomerular markers in measuring renal function and progression of severe renal failure.[6,7] From the patient's and the health economist's point of view, however, what is important is whether or not the patient is receiving dialysis, and thus dialysis is the marker of clinical renal death.

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Assessing resuscitation skills by video recording

SIR,—Assessment of candidates attending advanced trauma life support courses includes the use of multiple choice questionnaires and "moulage."[1] How closely theoretical knowledge and practical performance during mock events relate to the management of true resuscitation is unclear.

We have been using a video camera to monitor the management of patients with trauma and with cardiac arrest in the accident and emergency department and believe that this method is useful both in training and in audit. Video recording of care in cases of trauma has been used in the United States for several years,[2] and we have reported our experience in patients with trauma: by reviewing many such cases we identified deficiencies in the organisation and design of the resuscitation room and made improvements.[3]

We have extended the method to monitor the management of cardiac arrest in the department. This has allowed us to pinpoint both organisational and individual deficiencies. For example, in the first 10 cases the mean number of staff concerned in each attempted resuscitation was 10·6 (range 7-13) and the mean longest interruption in cardiac massage during resuscitation was 23·3 seconds. The video recording, which is erased after one week, may be viewed in the presence of a senior member of the department by the members of staff who participated in the resuscitation.

Despite initial reservations on the part of some of the medical and nursing staff the method is now well accepted, and we find it useful in assessing resuscitation in practice.

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Screening for cervical cancer in developing countries

SIR,—Veena Singh and colleagues' assessment of the efficacy of visual screening for cervical cancer[1] is valid only for women attending maternal and child health clinics run by doctors trained in gynaecology. If direct visual inspection is to be useful in developing countries, where cytological screening is not available, the countries need to overcome many problems before achieving the basic level of screening for women generally. The problems are threefold.

Firstly, women in some developing countries are not valued. In an editorial Amartya Sen explains the reasons for this and suggests that women's illiteracy is one of the causes of their relative neglect.[2] Women in developing countries need education to empower them to lead healthy productive lives. This may be a long way off during the current world recession. Without education they may not be able to decide to seek screening services, which would no doubt be concentrated in cities. Their day to day survival is more important than screening for health. In a study of whether

health education increased the uptake of screening among Asian women in Leicester, McAvoy and Raza found that written material sent by post was ineffective.[3] It is labour intensive to educate women to accept screening services.

Secondly, medical staff need training to differentiate normal and abnormal cervices by visual inspection, particularly as inspection may not be sensitive and specific enough to detect early cancer owing to the range of aberrant normal cervices. Many false positive diagnoses may result and undue anxiety be caused if definitive diagnoses cannot then be made owing to lack of resources.

Finally, direct inspection of the cervix is possible only in health centres with a room, couch, proper lighting, and speculums. Facilities and equipment are likely to be inadequate owing to lack of finance. Developing countries should invest in the health of the population instead of fighting wars and shift their priorities towards the health and social welfare of people.

Direct visual inspection of the cervix to detect early cervical cancer may be useful in some centres but is not viable for population screening of women in developing countries.

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SIR,—Veena Singh and colleagues' paper purports to show that visual inspection of the cervix may be a useful screening method for cervical cancer.[1]

Screening has been defined as "actively seeking to identify a disease or pre-disease condition in people who are presumed and presume themselves to be healthy"—that is, they are asymptomatic.[2] Most of the women "screened" in Singh and colleagues' study, and all of those found to have cervical disease, had symptoms. The symptoms were those that may arise from an early cervical cancer—for example, vaginal discharge, irregular bleeding, and backache. Cervical cancer would have been included among the differential diagnoses for such women.

Clarifying the differential diagnosis by clinical examination and investigation does not constitute screening. The paper is further confused by the grouping of carcinoma in situ with invasive disease. This report provides no further evidence over the authors' earlier paper of a role for visual inspection as a screening test for cervical disease.[3]

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AUTHORS' REPLY,—We agree with Malcolm Griffiths that screening should involve only asymptomatic women. But this definition cannot be applied for Indian women as the prevalence of gynaecological morbidity in these women is extremely high (>50%), even among those not seeking medical attention.[1] Thus any screening programme for early detection of cancer needs to be started among women who are attending for primary health care, such as those attending maternal and child health services for minor complaints.

It is not correct to say that symptoms such as vaginal discharge, irregular bleeding, and backache are due to early cervical cancer as vaginal discharge is extremely high (>80%) among Indian women because of a high prevalence of infections of the reproductive tract (U K Luthra, personal communication).[1] Likewise, irregular bleeding (>40%) may be due to dysfunctional uterine bleeding.[1] The women attending the maternal and child health services presented with symptoms of short duration. In contrast, Indian women with cervical cancer with the same symptoms had had them for a long time (over three years).[2]

This study, of a fairly broad group of women, replicated the results of our earlier study, which was carried out in a highly selected population with dysplasia.[3] Further, we did not group carcinoma in situ with invasive cancer in our paper. What we tried to highlight was the clinical staging pattern of cancer detected among the screened population. It ranged from stage O to stage IIA.

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Treatment of hypertension in older adults

SIR,—The suggestion by Martin J Kendall that lipophilic, but not hydrophilic, β blockers are able to reduce the incidence of sudden death in hypertensive patients and patients with ischaemia[1] should be treated with a great deal of circumspection. This notion was first proposed by Ablad *et al*, whose work in rabbits showed that lipophilic metoprolol, but not hydrophilic atenolol, crossed the blood-brain barrier and "switched on" vagal activity, which raised the threshold to ventricular fibrillation under acutely ischaemic conditions.[2] These findings are quite the opposite to those of Meesmann.[3]

But what of humans? Certainly atenolol greatly increases parasympathetic activity,[4] significantly suppresses life threatening ventricular arrhythmias in the acute postmyocardial infarction period,[5] and is still the only β blocker significantly to reduce mortality when given within 12 hours of myocardial infarction.[6] Atenolol has been assessed in only one small trial of late intervention after myocardial infarction,[7] where it behaved similarly to propranolol in reducing mortality by over 50% in those who continued receiving treatment, as does hydrophilic acebutolol.[8] The non-significant 18% reduction of mortality after myocardial infarction by hydrophilic sotalol[9] may be due to its pro-arrhythmic potential arising from its class III anti-arrhythmic property.

As regards primary prevention of myocardial infarction in hypertensive patients, there is, as Martin Kendall points out, evidence that lipophilic propranolol has a modest benefit in middle aged subjects (particularly non-smoking men). However, his reference to the heart attack primary prevention in hypertension (HAPPHY) study suggesting that metoprolol benefited patients whereas atenolol had an adverse effect is quite out of order. The HAPPHY steering committee concluded that any apparent differences between atenolol and metoprolol were perfectly consistent with the play of chance.[10] For the record, death rates in the HAPPHY study were less in patients receiving atenolol than in patients receiving metoprolol (6·93 *v* 7·89 deaths per 1000 patient years); however, death rates in the diuretic arm randomised against atenolol were inexplicably lower than those in the diuretic arm randomised against metoprolol (5·46 *v* 9·89 deaths per 1000 patient years).[10]

In elderly patients there is now no debate that diuretics should be first line therapy for primary prevention of stroke and myocardial infarction (unless the patient has had a recent infarction or has angina, in which case a β blocker is appropriate). Atenolol based treatment, though preventing strokes, has not been shown to prevent myocardial infarction in elderly hypertensive patients. This is almost certainly a β blocker class effect; the Swedish trial in old patients with hypertension,[11] which showed no significant decrease in myocardial infarction, used three different β blockers—pindolol, metoprolol, and atenolol. This lack of benefit of β blockers in elderly patients may be due to haemodynamic circumstances (larger hearts and low vascular compliance, in the absence of overt ischaemia) not suited to first line β blockade.

Martin Kendall's recommendations that hypertensive patients with angina should be treated with lipophilic β blockers is surprising as the anti-ischaemic efficacy of atenolol is at least as good as that of propranolol.[12]

So, in conclusion, there is not a scrap of scientific evidence in humans to suggest that a lipophilic β blocker should be preferred to a hydrophilic β blocker for "cardioprotective" purposes.

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Extensible bandages

SIR,—Charles McCollum's editorial on extensible bandages criticises a widely used, performance based classification system for these products on the grounds that it is misleading and potentially dangerous.[1]

The complex test on which the classification system depends was developed over several years by a working party comprising technical repre-