This would then facilitate greater mobility of medical staff in the years to come and help iron out unwanted differences in medical training within the European Community.

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Understanding the other's decision

SIR,—The juxtaposition of the papers on the proposed changes to preregistration training¹ and differences in the perception of patients' needs between primary and secondary care physicians² has an important lesson.

I have travelled on "the road to hell" on many occasions. I know only too well what it is like to be in the situation of the junior hospital doctor and the general practitioner in the anonymous article.² Truly, I can empathise. Surely the answer is simple: a compulsory period spent in the community as a preregistration trainee. Just as prospective general practitioners train in hospitals, so prospective hospital doctors should train in general practice. Sometimes there is no right or wrong to management decisions, but there can be understanding of how such decisions are made.

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Rice based oral rehydration solutions

SIR,—In their meta-analysis of results of trials of rice based oral rehydration solution Sheila M Gore and colleagues confirm a decade of observations that when rice and other cereals replace glucose or sucrose as the source of cotransporting substrate in oral rehydration solutions, fluid loss and the duration of acute watery diarrhoea are reduced.¹ This is most easily observed when the fluid loss is greatest, in cholera.²

The conclusion that there is no persuasive advantage of rice oral rehydration salts over the glucose based salts recommended by the World Health Organisation, however, is premature. In addition to the issue of total stool volume and the oral rehydration therapy needed to replace it, even what are apparently only minor diarrhoeal episodes may seriously interfere with absorption of nutrients, often for a prolonged time.³ Early in field studies of glucose oral rehydration therapy children who were consistently given this treatment for their diarrhoeal episodes were found to have improved nutrition compared with children receiving alternative treatment.⁴ More recently, field studies with rice oral rehydration salts solutions⁵ and maize solutions (P Kenya *et al*, personal communication) in Bangladesh and Kenya respectively have indicated an appreciable advantage—both a more rapid recovery from diarrhoea and improved nutritional status—when cereal based solutions were used consistently. Surely better child nutrition is an important outcome variable to

be considered in any judgment on treatment for diarrhoea.

Cost and convenience are important but will vary depending on local practices. In fact, most countries now have cereal based soups or gruels that are used for children with diarrhoea, which with an educational effort could become effective oral rehydration solutions. In this case there would be no added cost over solutions based on glucose, which is not produced by many poor countries. The cost of fuel needed to heat all cereal solutions has to be considered, but there is also a substantial benefit as the water used will be boiled, making it less likely to propagate disease.

It is more than a decade since the first reports that rice based oral rehydration solutions reduced fluid losses and shortened illness in cholera, and half a decade since field trials of the solutions showed the reduced severity of other diarrhoeal diseases and improved nutrition in children in two continents. I hope that we do not have to wait another decade before this knowledge is implemented enthusiastically. There is a point at which an inexpensive, safe, more effective version of a treatment that has the potential to reduce further the need for hospital treatment and costly intravenous fluids should be widely implemented rather than ignored through the inertia of large scale existing programmes.

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Laparoscopic cholecystectomy

SIR,—The Royal Australasian College of Surgeons is collating the results of this procedure throughout Australia and New Zealand, but so far they are only anecdotal. Deaths and severe damage to the common bile duct have occurred. Some deaths from heart problems and pulmonary embolus have been considered not to be relevant to the procedure, but I challenge that. The increased pressure in the abdomen from infused gas raises the diaphragm as well as compressing the vena cava. This may predispose to cardiac and thrombotic problems. Because of the short stay in hospital after laparoscopic cholecystectomy patients are not protected against thrombosis in the same way as they are after open cholecystectomy, and I believe that they should be. The operation is prolonged, and pressure on the vena cava may well increase the likelihood of clotting in the lower limbs and pelvis.

Another aspect needs to be addressed. A patient in her 70s had a laparoscopic cholecystectomy last October, which did not alleviate her symptoms. Further investigation led to another laparotomy early this year, when a carcinoma of the pancreas with early invasion of the peritoneum was found. Minilaparotomy and laparoscopic cholecystectomy may well be suitable for young patients who want

to get back to their families, or work, quickly and are less likely to have more than one disease. Multiple diseases, however, are much commoner in older people, for whom early return to work is not relevant and who, for other reasons, may need to stay in hospital longer. During cholecystectomy I have found an early carcinoma of the stomach (the patient is still alive after 20 years); a carcinoma of the kidney (the patient is still alive after 15 years); a carcinoma of the duodenojejunal flexure; several duodenal ulcers; and various benign and malignant ovarian conditions. Most of these would not have been detected by laparoscopic examination because it is not possible to feel around the abdominal cavity.

Laparoscopic surgery is exciting but must find its true level of use not only in gall bladder disease but in the many other conditions for which it is used—for example, repair of inguinal hernias, vagotomy for duodenal ulcer, and surgery for hiatus hernia and of the colon. In these conditions refinement of the open technique to produce the best results has taken many years. Until laparoscopic techniques can be shown to be as effective and safe in the short and the long term I agree with J N Baxter and P J O'Dwyer that they should be used only in clinical trials.¹

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1 Baxter JN, O'Dwyer PJ. Laparoscopic or minilaparotomy cholecystectomy? *BMJ* 1992;304:559-60. (29 February.)

SIR,—J N Baxter and P J O'Dwyer correctly state that the best means of evaluating laparoscopic cholecystectomy is by randomised controlled trials comparing it with an established surgical technique.¹ Their suggestion that it should be compared with minilaparotomy cholecystectomy rather than conventional cholecystectomy is less convincing because minilaparotomy cholecystectomy is not widely performed; it requires special instruments and techniques; and the current operative "gold standard" is open cholecystectomy.

The higher rate of injury to the common bile duct with laparoscopic cholecystectomy is cited as the main indication for subjecting the procedure to critical evaluation. Exactly what the incidence of ductal injury will be once surgeons are beyond the learning phase is unknown. The references cited by the authors do not support their claim that the rate of ductal injury is as high as 7%. Cameron and Gadacz refer to a possible incidence of 1%,² and Peters *et al* report one ductal injury in 100 consecutive patients.³ Injuries to the common duct may be due partly to the learning curve for this new operation. In a large multicentre study of 1518 laparoscopic cholecystectomies the incidence of ductal injury fell from 2.2% to 0.1% during the learning phase; the overall incidence was 0.5%.⁴

Operative cholangiography in laparoscopic cholecystectomy is tedious without appropriate instruments, but specially designed instruments for this part of the procedure and use of image intensification can make laparoscopic cholangiography as quick as open cholangiography.

The argument that surgeons' skills in operating on the biliary tree would be lost if open cholecystectomy was abandoned does not stand up to scrutiny. Trained general surgeons should be able to convert laparoscopic to open cholecystectomy. Training may be more of a problem with the minilaparotomy than with laparoscopic cholecystectomy. With laparoscopic cholecystectomy surgeons in training begin as assistants and progressively perform more of the procedure. As a teaching exercise video projection allows all present to learn the procedure. In future, as more abdominal procedures are performed laparoscopically, difficulties in learning laparoscopic techniques will be lessened. Minilaparotomy

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 selected² 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 groups⁴; 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 cancer¹ 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