promoting legislation, a task which will now be pursued vigorously from within Wessex.

Change is the first to invite criticism and we would agree with Dr Draper and his colleagues that if anything in the health field needs reorganising the stagnant backwater of "social" health education is crying out for a fresh sluice.

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¹ Wessex Positive Health Team. *Promoting the use of seat-belts in Wessex—an evaluation of integrated health education*. Winchester: Wessex Regional Health Authority, 1980.

SIR,—In the article by Mr M O'Hanrahan and others (30 August, p 597) attention is rightly drawn to the value of distributing booklets and leaflets on cardiovascular disease to the lay public, and their recommendations regarding future booklets on primary prevention, secondary prevention, and management should certainly be given full consideration.

I note, however, that their classification of the 130 publications that they received includes six publications on stroke, all of which are listed as belonging to type 3—that is, management. This gives the impression of non-availability in the UK of any publications dealing with the prevention of stroke, whereas one of the publications sent to the authors by our association was entitled *Reducing the Risk of a Stroke*, which quite definitely comes under the heading of primary prevention—that is, type 1 in the classification table.

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Retroperitoneal fibrosis associated with atenolol

SIR,—In his response (23 August, p 564) to an earlier letter from us (26 July, p 311) commenting on his views (5 July, p 59) of structure-activity patterns and the fibrotic syndrome of certain beta-blockers, Dr D W Bullimore selects three items for criticism.

(1) His former view that the syndrome relates to the molecular mass of a substituent side chain, which we contested, is now replaced by the better-known lock-and-key model used to illustrate drug-receptor interaction, a change which we would support (except his startling premise that "not all Yale keys open all Yale locks . . ." which implied that many do). However, in pursuing this analogy he still insists that the "size" of the key—that is, its mass (Dalton value?)—takes equal place with "design" in determining specificity of effect, which must be news to locksmiths.

(2) Dr Bullimore had supported his mass hypothesis with some equally imprecise views on "electron clouds" and, in turn, criticises our interpretation of these not only as dipoles but as dipoles having opposed polarity. Rotation about bonds is clearly possible, as he suggests. However, our diagrams related specifically to practolol and atenolol, which Dr Bullimore had himself selected and classed together as being derived from acetamide. On this basis, it does not require what would otherwise be a complicated configurational

and vectorial analysis to restate and re-establish our original contention. Quite simply, it is self-evident that the aromatic moieties (R) of these drug molecules are attached to "opposite" ends of the acetamide residue—that is, R-NH-CO-CH₃, and NH₂-CO-CH₂-R—and will be influenced accordingly. Such reversals (for instance, of single units in a polypeptide chain) can have a profound effect on biological properties.¹

(3) Dr Bullimore suggests that partition coefficients (π values) have little correlation with biological activity, noting in support of his views figures for drugs of widely disparate types. To quote these unrelated values in this way, and in isolation, shows a total misunderstanding of the part this single parameter plays in a multiterm linear free-energy expression for activity. He might as well have quoted melting points. Moreover, it is only a partial truth to claim that partition coefficients mainly influence "pharmacokinetic handling," meaning presumably drug absorption, distribution, etc, in the animal. It is no less important in determining intrinsic activity than is any other factor that governs drug access, whether at organ, cell, receptor, or molecular level. Finally, in general terms, it seems very illogical for Dr Bullimore to discard the precision of the π -value concept, yet wholeheartedly embrace the comparatively crude notion of molecular mass as being of greater significance.

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¹ Rudinger J. In: Arien EJ, ed. *Drug design*. Vol 2. New York: Academic Press, 1971:319-419.

. This correspondence is now closed.—Ed, *BMJ*.

Postoperative deep-vein thrombosis: identifying high-risk patients

SIR,—Mr A J Crandon and his colleagues (2 August, p 343) rightly draw further attention to the problems associated with postoperative deep-vein thrombosis in gynaecological practice. Rather than offer prophylactic subcutaneous heparin to all their patients the authors have sought to select a high-risk group. They are understandably reluctant to prescribe a drug which may be unnecessary for the individual patient and which may have adverse effects. We have here two separate issues. Firstly, is the "predictive index" truly discriminatory and, secondly, are the risks of increased perioperative bleeding associated with the use of heparin in low dosage real?

Selection of high-risk patients is inevitably a hazardous exercise since one first has to be able to demonstrate that by offering prophylaxis only to the selected high-risk patient one can reduce the incidence of perioperative pulmonary embolism to the remarkably low levels achieved with the non-discriminative use of low-dose heparin. Furthermore, one must surely also demonstrate that such low levels are also seen in the operated patients who are not offered prophylaxis. In order to demonstrate such apparently simple points one must study very large patient groups, and in my view the use of stepwise logistic discriminative analysis is insufficient for this purpose.

With regard to the "risks" of low dose heparin, this point again is not adequately clear since the work of Gallus and his colleagues¹ did not use adequately objective indications of postoperative blood loss; and the information obtained from the international multicentre trial, in which a large number of patients from this city were entered, can again be criticised on these grounds since the incidence of postoperative blood loss was again very much an estimate on subjective grounds only. Investigations from our laboratory using labelled fibrinogen and red cells, combined with scanning of the abdominal wall after laparotomy, does not in fact support the view that subcutaneous heparin leads to excessive postoperative bleeding.

One of the major objections to the widespread use of prophylactic low-dose heparin is in fact its cost, a point that was not made by Mr Crandon and his colleagues. While applauding any efforts to identify patients at particularly high risk, one is anxious that doubt may arise concerning the immense value of low-dose heparin as a prophylactic since widespread use of this during the past 10 years must surely have saved the lives of many hundreds of our patients.

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¹ Gallus AS, Hirsh J, Tuttle RJ, et al. *N Engl J Med* 1973;288:545-51.

Following up patients with rheumatoid arthritis

SIR,—Your leading article on following up patients with rheumatoid arthritis (12 July, p 97) must be unique in dealing with a common chronic disease and yet completely dismissing any contribution general practitioners might have to make to continuing care. It is a very sad reflection on the state of relations between hospital and general practitioner and it is to be hoped it does not reflect British practice as a whole.

The first sentence opens the way with a mixture of condescension and guesswork. "The clinician's impression is that the true prevalence of rheumatoid arthritis is rather less than the epidemiologists' estimate of 1-2%." One general practitioner clinician (or are clinicians only those who do clinics?) gives an incidence of rheumatoid arthritis of 2.7 cases per 1000, or 2.7% in 10 years, deaths and incurability permitting.¹

This opening sets the tone for the whole article. One solution, you say, is to refer patients back to general practitioners with a contingency plan; but "Most physicians believe that this is inadequate—nor is it popular with the general practitioners, because their experience of the disease is usually small." Perhaps the Royal College of General Practitioners could help out with a few figures to refute this astonishing statement. When a paper is quoted showing no important differences between GP and hospital care following discharge,² this finding is explained away. The answer suggested is to have clinical metrologists working in hospital with the specialist comfortably sitting on top of the growing pyramid, further and further away from patients.

Please could you remember that colleagues exist outside hospital; they deplore long waiting lists for outpatient appointments, especially when the clinics are cluttered up

with patients being followed up who would be just as well managed by their general practitioners—who are responsible for their care for the rest of the time anyway. And, who knows, if the consultant returned to true consulting practice the standards of treatment of rheumatoid arthritis in his GP colleagues' practices might just possibly improve.

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¹ Hodgkin K. *Towards earlier diagnosis*. Edinburgh: Churchill Livingstone, 1958.

² Mowat AG, Nichols PJR, Hollings EM, Haworth RJ, Aitken LC. *Ann Rheum Dis* 1980;39:12-7.

Perinatal and neonatal mortality

SIR,—I would like to congratulate Mr R Garry and A F J Atkins (23 August, p 562) on the organisation of the maternity services in their area. The basic principles of hospital delivery and immediate return home where there were no clinical or social contraindications were laid down by Dr Sarah Walker and myself in 1956.¹ Although this received wide publicity at the time, such is our innate resistance to new ideas that it took Professor Duncan over seven years to establish such a unit in Cardiff. This was followed a few years later by Salford, and no doubt similar facilities exist elsewhere.

I have subsequently developed my ideas and now feel that it can no longer be argued that midwifery is part of ordinary general practice. I would like to have the obstetric list restricted to a relatively few dedicated general practitioner obstetricians, who with the midwives, district or hospital, would be part of the maternity team under the consultant of their choice. They would be GPs for, say, three days a week and general practitioner obstetricians for the other days. All deliveries would be in hospital with immediate discharge home where suitable. All antenatal care would be given in the health centre by this same team, the consultant coming out on special sessions in difficult cases to assess the risk. Transport of the mother in labour would be relatively easy in most of this country, but in very remote areas what is wrong with using helicopters?

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¹ Sluglett J, Walker S. *Lancet* 1956;i:684-6.

Anastomotic integrity after operations for large-bowel cancer

SIR,—The figures on anastomotic leakage given by Mr L P Fielding and others (9 August, p 411) are interesting and important. However, it would be a pity if surgeons were to interpret a leakage rate of 25% for tumour resection performed in the presence of obstruction when there was a protecting colostomy, compared with only 14% where this was not employed, as a condemnation of a relieving colostomy in this situation.

Two factors may account for these rather surprising figures. Firstly, we are not told how many resections in the unprotected group were obstructions due to right-sided cancers, where a relieving colostomy would not, in any case, be indicated. Secondly, we are not informed of the severity of the obstruction; a surgeon might resect a minimally obstructed "easy" case without a colostomy but might protect a really nasty occlusion by this means

and not be too surprised if the leakage rate was higher in the second category.

Until there is more news, I shall continue to protect left-sided anastomoses performed in obstructed cases of colonic cancer by means of a transverse colostomy.

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* * * We sent this letter to Mr Fielding, who replies below.—ED, *BMJ*.

SIR,—Thank you for asking me to reply to Professor Ellis's letter. In the group of patients undergoing anastomoses with stoma (127 patients), 32 had tumours up to the splenic flexure (right-sided lesions), of whom five (15.6%) died in the postoperative period. Thus the remainder of this group (95 patients) had obstructed lesions of, or distal to, the splenic flexure, of whom 14 (14.7%) died. In the other two comparative groups the figures were: anastomoses with transverse colostomy—28 patients, of whom six died (21%) and delayed anastomoses—29 patients, of whom one died (3%). I note that Professor Ellis makes no mention of this last group—the data suggest that it is this policy which might be achieving the best results—but I agree with him that we need information about the comparability of patients in these three groups for the degree of obstruction and severity of illness.

Sadly, there are no yardsticks by which these factors can be measured. In the past I have attempted to provide some information by using a clinical score and recording the delay from admission to surgery.¹ It seems that a choice of a transverse loop colostomy to accompany a left-sided obstructive resection after an anastomosis has been constructed is one of personal clinical bias—as indeed it seems to be for Professor Ellis—but that a decision to delay an anastomosis is different. Here it seems that clinical severity is greater than in the other two groups, and that surgeons, accepting the principle of immediate tumour resection, have been unwilling to risk an anastomosis. Taken in this context, the outcome for this group is interestingly good. In summary then, my overall clinical view depends on two levels of assessment by the surgeon—firstly, of his own ability to construct a secure anastomosis and, secondly, of the degree of the patient's sickness.

Thus, on the assumption that a fully trained surgeon is present for all of these operations, a resectable tumour should be resected after bowel decompression.¹ Then, if the patient is relatively unfit or has "significant" systemic effects from the obstruction or if the bowel wall is friable, an anastomosis should be delayed and the ends brought out as stomas. I believe that this policy should be used for both left and right-sided lesions (in the case of a distal tumour in which a mucous fistula is not possible then the end can be oversewn—a Hartmann procedure). Alternatively, if conditions are more favourable and if the surgeon is certain that he has a low rate for anastomotic leakage, then reconstruction should be considered: an ileocolic anastomosis for lesions up to and including the splenic flexure (or even beyond) and a colorectal anastomosis for tumours of the sigmoid after "on-table" orthograde colonic and distal rectal irrigations.² The combination of which allows reconstruction between clean and empty segments of gut. Neither of the policies just described includes the construction of a transverse loop colostomy.

Currently these views may seem unconventional. However, over the last decade we have seen a shift in our approach to the management of patients with large gut obstruction, in that immediate tumour resection is far more widely practised than formerly,¹ when staged tumour

resection was the order of the day. We now need to turn our attention to the timing and methods of reconstruction in relation to the factors just listed, accepting two principles: firstly, that right hemicolectomy for bowel obstruction with immediate reconstruction is not the simple uncomplicated operation once thought^{1,3} and, secondly, that transverse loop colostomy is of unproved value to militate against the septic complications of a leaking left-sided anastomosis.

Clearly, these are not subjects suitable for an orthodox randomised trial⁴ and we will need to proceed slowly on the basis of discussion and reporting of results to establish the most effective treatments. In the meantime it would be helpful for us to stop using the words "protecting" or "relieving" to qualify the procedure of transverse loop colostomy until such time that this function has been established; my guess is that we will not be able to find such evidence. Even if these thoughts do not constitute the "news" sought by Professor Ellis, perhaps the small amount of evidence supplied will cause him to reconsider again the role of concomitant transverse loop colostomy.

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¹ Fielding LP, Stewart-Brown S, Blesovsky L. Large bowel obstruction caused by cancer: A prospective study. *Br Med J* 1979;ii:515-7.

² Anonymous. *Br Med J* 1980;280:882-3.

³ Fielding LP, Stewart-Brown S, Dudley HAF. *Lancet* 1978;ii:778-81.

⁴ Debas HT, Thompson FB. *Surg Gynecol Obstet* 1972;135:747-52.

Unprocessed bran in idiopathic hypercalciuria

SIR,—In their brief article on the effect of unprocessed bran on urinary calcium excretion in idiopathic hypercalciuria, Mr P J H Shah and others (9 August, p 426) identify three possible mechanisms: the binding of calcium to phytic acid; prevention of calcium absorption by bran; or decreased intestinal transit time reducing the time available for calcium absorption. A fourth possible mechanism merits discussion.

Vaughan and Filer¹ reported that lactose and certain other sugars increase calcium absorption from the gut; Hodgkinson and Heaton² showed that a carbohydrate meal increases calcium excretion without producing any change in plasma calcium level; and Leman *et al*³ found that subjects prone to develop stones in the renal tract tend to exhibit bursts of hypercalciuria after ingesting carbohydrate-rich foods. Many other papers in the scientific literature indicate that calcium is absorbed along with monosaccharides. If absorption of the latter is complete in the jejunum then little calcium is absorbed lower down the gastrointestinal tract, but if for any reason disaccharides or more complex carbohydrates are not broken down to absorbable monosaccharides until food reaches the ileum or large bowel, absorption of calcium may take place over a much longer stretch of the intestinal tract and consequently overall calcium absorption may be excessive. In the rat it is easy to produce various forms of nephrocalcinosis by feeding lactose in high concentration (V J Feron, personal communication). In young growing animals enhancement of calcium absorption by lactose in milk is doubtless advantageous.⁴

Lower consumption of carbohydrates that