transferred and not transferred? This would be of great value to many medical and geriatric services who do not have the facilities of a stroke unit and who regularly indulge in transferring stroke patients, albeit at times it seems in a wholly random fashion.

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*.*We sent this letter to Dr W M Garraway and his colleagues, whose reply is printed below.—Ed, *BMJ*.

SIR,—The limitations inherent in conducting controlled trials of methods of providing health services, which we refer to in our paper, mean that we have no information on the criteria which were used to transfer a selected group of patients from medical units during the acute phase of rehabilitation. But we have been able to examine the results of standard clinical tests which were designed to predict outcome following acute stroke, and which will form the subject of a future publication.

No difference was found in the pattern of neurological impairment present in the transferred group compared with those patients remaining in medical units throughout. Therefore it may not be surprising that there was no difference in outcome between the two groups. Transferring stroke patients from medical units for further rehabilitation appears to be a common practice in Britain, and on the basis of our results these transfers should be arranged earlier rather than later if the full benefits of intensive therapy are to be realised. But, as Dr Powell correctly infers, conclusive evidence to support this statement can come only from further randomised controlled trials.

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Preventing infection in laboratories

SIR,—As we were not consulted on the drafting of the original report perhaps you will permit us to make several observations on the article by Sir James Howie and Mr C H Collins (19 April, p 1071) entitled "The Howie Code for preventing infection in clinical laboratories: comments on some general criticisms and specific complaints."

It is important to realise that the code relates to clinical laboratories, a point which is often not appreciated by authorities responsible for ensuring safety standards in non-clinical laboratories. While this does not imply that the code is irrelevant to non-clinical laboratories, it does mean that its interpretation in these non-clinical situations needs careful consideration and possible modification to meet local requirements.

In particular, we have in mind the use of *Salmonella typhi* in the Rideal Walker test for disinfectants. While we would not disagree with the general comments regarding the dangers of *Salmonella typhi* as isolated in a clinical situation we do believe that the *Salmonella* strain NCTC 786 as used in the

Rideal Walker test presents a different picture. The strain is a virulence-negative variant and while this is not positive proof of the non-virulence of the organism it does differentiate it from the normal virulent strains. The strain has been in use in members' laboratories from as early as 1904 with well over 450 000 Rideal-Walker tests conducted during this time, involving more than 200 laboratory personnel. There is no recorded case of infection resulting from the use of this strain during this period. These facts speak for themselves and need to be taken into account in reacting to the use of *Salmonella typhi* NCTC 786 in the Rideal-Walker test.

All too often the applicability of *Salmonella typhi* NCTC 786 to the testing of modern disinfectants clouds the issue of the safety aspect. Manufacturers of disinfectants sell products on an international basis and the Rideal-Walker test forms an integral part of this operation. Naturally, the imposition of excessive restrictions will place UK companies in an unfavourable competitive position.

In seeking to clarify the situation with regard to *Salmonella typhi* NCTC 786 we support those who oppose the unnecessary use of the Rideal-Walker technique using *Salmonella typhi*—for instance, for teaching purposes or evaluating disinfectants for hospital use, where because of the readily availability of *Salmonella* cultures alternatives to strain NCTC 786 may be inadvertently used.

While we agree with the Howie Code's recommendation that the use of exhaust protective cabinets (class 1) is necessary for most category B pathogens, which cause airborne infections, we do not agree that it is appropriate for organisms causing infection by the alimentary route such as *Salmonella typhi*. To cover this case the association had drawn up its own code of practice for the handling of *Salmonella typhi* NCTC 786 in members' laboratories.

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Why has Swann failed?

SIR,—In your leading article (17 May, p 1195) you state that legislation enacted as a consequence of the Swann Report has failed to prevent the emergence of multiple drug resistance in *Salmonella typhimurium* in bovines in Britain despite the fact that it was precisely intended to do so. The legislation was not designed to do this. Its purpose was to stop the feeding of pigs and table chickens continuously on diets containing antibiotics that are commonly used in human and veterinary medicine, because the practice had given rise to enormous populations of antibiotic-resistant bacteria in these animals. The feeding of such diets to bovines in Britain had never been permitted.

The Swann legislation, in essence, has now been adopted by many other countries and I think Britain can take some pleasure in having initiated it.

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SIR,—In the excellent leading article "Why has Swann failed?" (17 May, p 1195) I was surprised that attention was not directed also

to the current agricultural practice of spreading raw fluid slurry on the fields. This practice could be a possible source for dissemination of these organisms.

If the fluid comes from stock some of which is infected with or is a carrier of salmonella it may reasonably be supposed that when the field so treated is next grazed the grazing stock are at risk of infection. It must also place anybody who may have contact either with the fluid or with the surface of the field at some risk of infection. This is less likely to occur if all the manure is allowed to rot down fully to compost in a dung heap, for the simple reason that the heat of a well-made heap will inhibit if not destroy bacteria. The broadcasting of fluid slurry in this way is also socially unacceptable to those unfortunate enough to live to leeward of the fields so treated.

The epidemiology, both animal and human, described in your article suggests that this practice requires careful scientific study.

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Possible cancer hazard associated with 5-methoxypsoralen in suntan preparation

SIR,—With regard to the current discussion (3 November, p 1144; 1 March, p 648; 29 March, p 940) on the safety of cosmetics containing 5-methoxypsoralen (bergapten) I would like to add a number of points.

(1) Although the current controversy concerns the suntanning aids containing bergapten oil, they are not the only cosmetics involved. Of a total of 108 perfumes investigated, 57.4% contained bergapten in concentrations ranging from 0.00004 to 0.0108%.¹ Perfumes are therefore likely to produce phototoxicity unless the psoralens are removed or the content of 5-methoxypsoralen reduced to 0.001%.² Subjects known to be sensitive to small concentrations of 5-methoxypsoralen and patients suffering from poikiloderma of Civatte should therefore be warned about all these preparations.¹

(2) The suntanning aids of current concern contain up to 0.003% (30 ppm) (Chefaro Proprietaries Ltd, personal communication). The amount of 5-methoxypsoralen necessary for the achievement of an enhanced tan has, however, been reported to exceed 1%.³

(3) Not only is there a possible risk of human carcinogenicity associated with the use of methoxypsoralens in combination with ultraviolet A light, but also there may be as yet unknown effects on the microbial ecology of human skin. Methoxypsoralens are known to induce mutations in various bacteria and fungi.⁴ Any genetic changes in skin organisms may be clinically important with regard, for example, to acquisition and transfer of antibiotic resistance.⁵

(4) Many substances are photosensitisers. They include several fluorescent dyes (erythrosin B), some metabolites (for example, bilirubin and porphyrins), and drugs like phenothiazines, tetracyclines, sulphonamides, thiazides, nalidixic acid, frusemide, quinine, and anthracene.⁶ The topical application therefore of preparations containing another phototoxic compound, 5-methoxypsoralen, could lead to an increased photosensitivity in individuals already sensitised to solar radiation.

(5) All tanning aids contain sunscreens, which are substances designed to prevent

erythema by absorbing ultraviolet B rays. The efficacy of some of these compounds, however, is being questioned.³ Some sunscreens may themselves be phototoxic⁷—for example, amyl dimethylaminobenzoate (Padimate A, Escalol 506). Some can prevent erythema, but not premalignant changes in the epidermis⁸—for example, mexenone (2-hydroxy-4-methoxy-4-methylbenzophenone). Furthermore, a 5% ethanolic solution of para-aminobenzoic acid (PABA), though an effective sunscreen, cannot protect against drug-induced phototoxic reactions.⁹ Broad-spectrum sunscreens combining a PABA ester with a benzophenone ester appear currently to provide the most protection from ultraviolet light.⁹

The Council of Europe is currently considering sunscreens and one hopes that in the future proper standardisation and toxicological tests will be required for all sunscreens and tanning aids, studies which would dispel much concern regarding their safety.

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- ¹ Zaynoun ST. *J Soc Cosmet Chem* 1978;**29**:247-63.
- ² Marzulli FN, Maibach HI. *J Soc Cosmet Chem* 1970;**21**:695-715.
- ³ Greiter F, Doskoczil S, Bilek P. In: Breuer MM, ed. *Cosmetic science*, vol I. London: Academic Press, 1978.
- ⁴ Scott BR, Pathak MA, Mohn GR. *Mutat Res* 1976;**39**:29-74.
- ⁵ Weissmann A, Noble WC. *Br J Dermatol* 1980;**102**:185-93.
- ⁶ Moore DE. *J Pharmaceut Sci* 1977;**66**:1282-4.
- ⁷ Emmett EA. *Photochem Photobiol* 1979;**30**:429-36.
- ⁸ Pearce AD, Wolska H, Marks R. *Br J Dermatol* 1979;**101**, suppl 17:22.
- ⁹ Akin FJ, Rose AP, Chamness TW, Marlowe E. *Toxicol Appl Pharmacol* 1979;**49**:219-24.

Distalgesic poisoning—cause for concern?

SIR,—Dr Robert Wall and others are right to draw attention to the dangers of dextropropoxyphene (17 May, p 1213).

Some years ago I became aware that patients for whom Distalgesic had been prescribed frequently requested further supplies in circumstances which, in my opinion, did not warrant this level of analgesia. It was also noticeable that the difficulty of weaning patients off this preparation increased in proportion to the duration of medication. I now rarely prescribe Distalgesic or similar drugs, and never for more than a short period.

In my view any preparation containing dextropropoxyphene should be controlled under the Misuse of Drugs Act, and the dangers of dependence should be publicised widely.

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SIR,—Any Glasgow graduate of my vintage will have clear memories of the depressing remark by our brilliant professor of "materia medica," the late Professor Ralph Stockman, when at the end of nearly an hour's discourse on some drug he would sum it all up with a deprecating "But it is of no use whatever."

Younger generations can hardly realise how few were the really effective drugs that were at our disposal in these presulphonamide days of the early 1930s. The corollary that the highly effective drugs of today result in many

unexpected and dangerous side effects appears to occasion surprise and sometimes what seems undue alarm. The correct action is surely to accept their benefits while retaining an alert outlook for any unwanted reaction.

With regard to the specific warnings on the dangers resulting from Distalgesic, although I do not rank as a "controlled experiment" yet I can speak from personal experience, having suffered from severe disability from back pain and sciatica, which started a year ago and which eventually put me on my back for some weeks. A slipped disc and any more sinister cause having been excluded, it may be guessed that almost all the available range of analgesics was tried (hiatus hernia and a tendency to peptic ulceration being a problem). I can assure Drs R J Young and A A H Lawson (12 April, p 1045) that Distalgesic, in association with the peripherally acting aspirin in the form of Nu-seals, was quite the most effective, avoiding the disastrously constipating effect of codeine. Certainly the pain was such that I at one time required a daily total of 390 mg of dextropropoxyphene, 3.9 g paracetamol, and 3.9 aspirin, in addition to various physical forms of treatment in combination with the anti-inflammatory indomethacin.

Despite this large dosage, there were no ill effects and I had no difficulty in gradually reducing it as the condition improved, until now I am down to single capsules a day with no withdrawal symptoms or apparent development of dependence. Is it not true, with perhaps the exception of the hard drugs (although after a month of codeine I stopped it with alacrity), that much of addiction depends as much on a patient's personality as on the particular drug? The patient of Dr Robert Wall and others (17 May, p 1213) seems to be a case in point.

Let us not throw out the baby with the bath water but retain Distalgesic for what it can do, without forgetting the caution in the use of any drug that Stockman would have approved.

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Investigating constipation

SIR,—Your leading article (8 March, p 669) "Investigating constipation," which described the pathological and iatrogenic causes of the condition, contained some questionable statements—for example: "From infancy the British are brought up to regard a daily bowel action almost as a religious necessity and to believe in autointoxication from the cesspool of the unemptied colon; so it is little wonder that their doctors see vast numbers of patients obsessed with the frequency, consistency, diameter, and appearance of their stools." This, surely, is an exaggeration and carries with it the unfortunate implication that regular bowel habits are of little importance.

This illustrates once again that physiological yardsticks, in numerical terms, for human bowel function are badly needed.¹ Recently, a number of investigators²⁻⁶ have compared the effects of high- and low-fibre diets on bowel function. Their data suggest that in healthy subjects good fibre-containing diets produce at least one motion daily, stool wet weight of 150-200 g/day, and a transit time of approximately 48 hours. Multicentre studies are now necessary to confirm these findings; and close co-ordination is essential to secure

uniformity of methodology, which should include an objective method of measuring consistency.⁷ Reference values established in this way would have wide applications in pharmacological research, clinical practice, and health care and would also provide the basis for a scientific definition of constipation. In the mean time Painter⁷ has provided a useful working definition, which states: "Any patient who strains to defecate and does not pass at least one soft stool daily without effort is constipated."

With the present enthusiasm for the "bulk" principle it has become somewhat fashionable to denigrate chemical laxatives as unnecessary evils—as is evident in your leading article. That there has been gross abuse of laxatives in the past and is some continued misuse today is unquestioned. However, there is ample evidence to show that a chemical laxative, carefully selected and used, has a valuable place in many cases of disordered gut motility.⁸ Painter⁷ illustrates the complementary actions of a chemical laxative and dietary fibre when he states: "Idiopathic slow transit constipation, megacolon and megarectum are a persistent problem. Besides dietary fibre they require standardised senna tablets (Senokot). Usually 2-4 tablets daily are sufficient, but the dosage must be found by trial and error. It may be possible to reduce the initial dose when the high-fibre diet has taken effect."

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- ¹ Godding EW. *Lancet* 1976;iii:1294-5.
- ² Harvey RF, Pomare EW, Heaton KW. *Lancet* 1973;ii:1278-80.
- ³ Eastwood MA, Kirkpatrick JR, Mitchell WD, Bone A, Hamilton T. *Br Med J* 1973;iv:392-5.
- ⁴ Payler DK, Pomare EW, Heaton KW, Harvey RF. *Gut* 1975;**16**:209-13.
- ⁵ Cummings JH, Jenkins DJA, Wiggins HS. *Gut* 1976;**17**:210-18.
- ⁶ Cummings JH, Southgate DAT, Branch W, Houston H, Jenkins DJA, James WPT. *Lancet* 1978;ii:5-9.
- ⁷ Painter DK. *Practitioner* 1980;**224**:387-91.
- ⁸ Avery Jones F, Godding EW, eds. *Management of constipation*. Oxford: Blackwell, 1972.

Diagnosis of infectious mononucleosis

SIR,—Your leading article (10 May, p 1153) dealt with a common problem—the accurate diagnosis of infectious mononucleosis—in which there is much misunderstanding. Pfeiffer's¹ "glandular fever" was a different disease from that of Sprunt and Evans.² In the former the disease occurred in infants, was very contagious and of short duration, and atypical lymphocytes were not described. The infectious mononucleosis of Sprunt and Evans occurred in adolescents, was not very contagious and lasted longer, and atypical lymphocytes were present.

The diagnosis of infectious mononucleosis formerly depended on finding clinical, haematological, and serological evidence of the disease. Recently, the presence of specific IgM anti-Epstein-Barr virus antibody has been accepted as diagnostic of the disease. This test is of particular value in children and in the elderly, where the Paul-Bunnell-Davidsohn test may be negative and the illness atypical. However, many laboratories use only commercial screening kits (for example, Monospot), which are associated with a number of false-positive results. We have studied the serology of 100 patients with clinical and haematological evidence of infectious mononucleosis.³ All patients had atypical lymphocytes constituting over 20% of the total leucocyte count. Positive