Letters

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Integrated medicine: orthodox meets alternative

Bringing complementary and alternative medicine (CAM) into mainstream is not integration

EDITOR—Vickers's review is another example of how complementary and alternative medicine (CAM) is being brought into the mainstream rather than integrated.[1] Times are indeed changing, but what to?

The dictionary defines integration as "the incorporation of equals into society."[2] Let's be honest: there is no equality in medicine; there never was and probably never will be. The recent approval of acupuncture by the BMA is by no means an overarching endorsement of Chinese medicine as a legitimate alternative system.[3] It is simply an acknowledgement of the accumulation over time of good enough evidence that shows the effectiveness of acupuncture in some conditions. This is, to borrow a metaphor from the word processing world, a cut and paste approach. It results in the assimilation, and not creation, of a new emergent property. Combination medicine is not integrated medicine.

Two other important aspects related to the future of CAM deserve discussion.

Health services research—Currently, much of the research effort in CAM is in the form of treatment x for disease y. Almost no systematic research is taking place on the delivery, organisation, and financing of different integrative healthcare models or on the appropriateness, quality, availability, and cost of CAM modalities in the current healthcare system. At a time when there is much interest in marketing, to ignore this line of research would undoubtedly be counterproductive in the long run simply because money is easier to measure and relate to than healing. Only by combining both types of research—biomedical that looks mainly at mechanisms of effect, and health services that looks mainly at modes of delivery—will true integration beyond the mere expansion of therapeutic tools be possible.

Medical education—Tallying the number of CAM courses given in medical schools is probably the most misleading indicator of integration. It may create an illusion that CAM has already made inroads into the temples of the medical establishment. The truth is that CAM education is currently an optional dessert rather than a main course. True integration will not be feasible without shifting the medical education paradigm from disease to humanism, from cure to healing, from knowing to not knowing. Education is the key to change.[4]

By their nature, review articles deal with the past. People, however, ought to look at the future: what will follow the current trend of mainstreaming? Integrative medicine, in its true sense, has the potential to expand the horizons of medicine beyond therapeutics.

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Challenge of making holism work

EDITOR—Treating the patient as a whole is a perennial issue in medicine. As doctors, we have the necessary skills to deal with some of the factors entailed. Other factors, such as financial status and education, fall outside the scope of our training and skills. Wanting to engage with the whole patient is one thing. Being able to do so is quite another. To consider tackling the physiological, psychological, social, epidemiological, and spiritual aspects of a person all at the same time is a pretty tall order. Maybe even this is not possible. Our complex and rapidly changing environment challenges the understanding of anyone. Why should we be any better equipped than other human beings to make sense of this fragmented world or of someone in it?

Doctors often complain that patients' expectations are unrealistically high, without seeing that the medical profession has played a large part in creating these expectations. If we really want to engage with the needs of the person in front of us, a constructive first step is to admit that this is an intractably difficult task rather than continuing to persuade ourselves that scientific medicine has the answers to everything. Adopting convenient politically correct modes of practice dictated by others merely enables us to sidestep the uncomfortable nature of this truth.

As doctors, we are very privileged. Technology has given us some powerful tools. Furthermore, our patients often share information with us that they cannot discuss with other people. If we can accept our position with a little humility, we may be able to begin to get a larger and more accurate picture of the person in front of us.

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Dr Woolfson has set up a website, the Proper Doctors' Club (www.propadox.com), to act as the focal point for an informal network of doctors who are interested in addressing the issues raised in this letter and want to improve the quality of their work.

Integrated medicine is not new

EDITOR—Ancient medicine was always integrated—a mind-body medicine rooted in a social and religious matrix of a culturally defined people with a definite belief system. Ancient medical beliefs differed radically from those of modern medicine. When Milton wrote: "Of Man's first disobedience, and the fruit/Of that forbidden tree, whose mortal taste first/Brought death into the world and all our woe . . ."[1] he expressed a Christian sentiment that disease was contamination caused by sin and a punishment from God; physicians were priestly servants of Christ.

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By the end of the 17th century beliefs and superstitions began to be replaced by more practical methods. This entailed the systematic removal of metaphysical elements such as the life force. Mesmer's magnetism is akin to Hahnemann's vital force, the Qi energy of Chinese medicine, and the vis medicatrix naturae of Paracelsus—an *élan vital*. Observation based and explanatory, the concept of a life force is dismissed by modern physicians. Most integrated medical systems see disease as an imbalance of natural energies, and cure as a retuning of the whole organism—concepts long ignored by modern clinicians.

If ancient physicians were to view a clinical life dominated today by specific drugs and diseases, would they understand conditions to be distinct from the patient or regard them as constructs of the observer's mind, visible only "through the spectacles of their own hypothetical conceits"?[2] Such a viewpoint persists in most holistic therapies, a patient being regarded as a multidimensional living continuum subject to myriad influences. Stahl (1660-1734), Mesmer (1734-1815), and Hahnemann (1755-1843) were the last great vitalists.

Sundered from theology, so medicine has become secularised. Imbued with materialism, the modern view of the world has wholly displaced the ancient view. As diseases have become defined strictly in chemical or physical terms, so the spiritual side of medicine has been lost. Koch and Pasteur caused germs and vaccines to eclipse vitalism. Molecules and infectious agents are all that interest modern physicians; other possible causes of disease have been ignored or sidelined.

Much of the metaphysical element that was ejected from medicine centuries ago now queues at modern medicine's back door. A path of disintegration has separated medicine from its spiritual roots. The holistic therapies might lead medicine back towards the holism of the ancient systems. In some senses the disintegrative force of reductionism has run its course. Integration seems likely to entail retrieving some theology.

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1 Milton J. *Paradise lost*. London, 1667.
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Homoeopathy versus placebo in perennial allergic rhinitis

Statistics in study were flawed

Editor—Taylor et al come to the conclusion that their study of homoeopathy versus placebo in perennial allergic rhinitis "has failed to confirm our original hypothesis that homeopathy is a placebo."[1] Unfortunately, the statistics do not prove that.

The basis for the study was a prestudy power calculation that required 120 patients to prove the hypothesis with a 5% significance and an 80% power.[2] In fact, the authors only recruited 51 patients but analysed the results as if they had the required number. Their only conclusion was that they did not have enough data to make a conclusion.

If we accept the availability of only 51 patients at the outset, what are the relevant calculations? The power calculation is only 43%, and to maintain the power calculation at 80% the P value becomes 34%. The only conclusion is that the trial is not able to prove anything.

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Competing interests: None declared.

1 Taylor MA, Reilly D, Llewellyn-Jones RH, McSharry C, Aitchison TC. Randomised controlled trial of homoeopathy versus placebo in perennial allergic rhinitis with overview of four trial series [with commentary by T Lancaster, A Vickers]. *BMJ* 2000;321:471-6. (19-26 August.)
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Study shows dissociation between objective and subjective responses to homoeopathy in allergic rhinitis

Editor—Taylor et al show the apparent dissociation between objective responses to homoeopathy (domiciliary nasal peak flow) and subjective responses (nasal symptoms) after four weeks in 50 patients with allergic rhinitis.[1] Few randomised controlled studies have measured domiciliary peak nasal inspiratory flow rate in allergic rhinitis, which makes these results all the more intriguing.

In one of those studies, of 38 patients with allergic rhinitis, nasal symptom scores showed significant ($P < 0.01$) correlations with morning and evening domiciliary nasal peak flow after four weeks of treatment.[2] The mean overall improvement in domiciliary nasal peak flow was 25 l/min in response to four weeks of antihistamine, which is comparable to the magnitude of the homoeopathy peak flow response (20 l/min).

Perhaps a longer period of homoeopathy or a different dose might have resulted in a subjective treatment response in patients with allergic rhinitis. Moreover, we need to know how homoeopathy compares to conventional drug treatments such as intranasal corticosteroids and antihistamines, given their proved long term efficacy on symptoms in allergic rhinitis.[3]

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Competing interests: The Asthma and Allergy Research Group has received funding from Aventis, AstraZeneca, Schering Plough, and GlaxoWellcome, which make intranasal corticosteroids and antihistamines.

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Study shows double standards in evaluation of homoeopathy

Editor—The appearance of yet another high quality randomised trial in allergy raises the important question of whether homoeopathy should be treated any differently from conventional treatments in healthcare systems that are ostensibly evidence based.[1] It also brings to mind an example of the unforeseen way that double standards can rebound on those who refuse to accept any positive results for homoeopathy.

The homoeopathy meta-analysis by Linde et al was generally positive and also found a positive result in a subgroup of the most formally rigorous trials—those with quality scores ≥70%.[2] More recently, Jüni et al compared 25 quality scales, including Linde et al's, by using them to rate a sample of 17 trials of low molecular weight heparin or standard heparin in the prevention of deep vein thrombosis during surgery.[3] Trials rated as high quality with Linde et al's scale showed greater benefit from low molecular weight heparin, reversing the findings of the original meta-analysis from which the sample of 17 heparin trials was taken.

Jüni et al attribute the range of results obtained to the content of the scales themselves. Accepting this explanation, one of the authors of the earlier heparin meta-analysis has singled out Linde et al's quality scale for attack: it not only "maximally disconfirmed" his original findings but also achieved a "scientific impossibility"—a positive result for homoeopathy.[4]

As usual, an alternative explanation is possible. The content of Linde et al's scale is similar to that of others on test, such as Jadad et al's scale,[5] that did not reverse the earlier heparin meta-analysis. The main difference is the exceptionally high cut-off point of Linde et al's scale relative to its median score of 50% for the 17 heparin trials: only three reached ≥70%. Compare this with Jadad et al's scale, where the median score was 60%; nine of the 17 trials were rated as high quality, because the cut-off point was also set at 60%.

If cut-off points are as important as they seem to be here, Vandenbroucke's implication that Linde et al's scale is intrinsically unreliable because it overturned his heparin conclusions is as questionable as his assertion that homoeopathy can never work.

It would seem that the original heparin meta-analysis fell at a hurdle designed to trip up homoeopathy—one that homoeopathy sailed over.

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1 Taylor MA, Reilly D, Llewellyn-Jones RH, McSharry C, Aitchison TC. Randomised controlled trial of homoeopathy versus placebo in perennial allergic rhinitis with overview of four trial series [with commentary by T Lancaster, A Vickers]. *BMJ* 2000;321:471-6. (19-26 August.)
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Results of study were not convincingly in favour of homoeopathy

Editor—Before taking the results of Taylor et al's study as an opportunity to speculate about how homoeopathy might work we should first take a careful look at the study and state that the results were negative, the meta-analysis was (or may be) flawed, and there was no homoeopathy at all.[1]

All three preceding trials used the change in visual analogue scale scores as the sole main end point, with significant results. In this trial the calculation of sample size was also done for only one main end point—the change in the visual analogue scale score. The P value was 0.82, which means that the trial was by no means able to reproduce the positive results of the others. The discrepancy with additional "objective" data is interesting but not positive for homoeopathy. What is to be the interpretation of this discrepancy in the light of the preceding trial, in which the discrepancy was just the other way round?

The meta-analysis is founded on four trials with different indications, different treatments, flaws in design and analysis, and a significant heterogeneity in treatment effects. The heterogeneity in treatment effects is almost spectacular in a series of only four relatively small trials, considering the low power of these statistical tests. In such heterogeneous situations, researchers are advised to refrain from doing meta-analyses because these can lead to grossly misleading results.

The patients in this study underwent allergic testing according to standards of orthodox medicine. The allergens were chosen on the basis of standards of orthodox medicine. There was no homoeopathy at all. The only part of the study reminding readers of homoeopathy is the dilution procedure. This whole scenario has nothing to do with the usual practice of homoeopathy, and if the trial had been perceived to be negative this would be the unanimous justification by homoeopaths.

Do we learn anything from this study that is convincingly in favour of homoeopathy? My answer is: No.

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1 Taylor MA, Reilly D, Llewellyn-Jones RH, McSharry C, Aitchison TC. Randomised controlled trial of homoeopathy versus placebo in perennial allergic rhinitis with overview of four trial series [with commentary by T Lancaster, A Vickers]. *BMJ* 2000;321:471-6. (19-26 August.)

Did patients really have allergic rhinitis?

Editor—Can we really believe that the objective improvement in Taylor et al's randomised controlled trial of homoeopathy versus placebo in perennial allergic rhinitis resulted from the administration of 1 g of lactose-sucrose globules (impregnated with either a 30c dilution of the allergen or placebo) at intervals of eight hours for only one day?[1]

Examination of the baseline clinical characteristics given in table 1 could provide an explanation. Eight patients in the homoeopathy group and 12 in the placebo group had previously been ineffectively treated with topical steroids, while three of the homoeopathy group and five of the placebo group had been effectively treated with topical steroids. In the homoeopathy group eight had had immunotherapy (three effectively), and in the placebo group five had had immunotherapy (two effectively); but immunotherapy has been impossible in the United Kingdom since 1986. In the homoeopathy group five had had surgery (one with benefit), and in the placebo group six had had surgery (two with benefit). Thirty five of the patients were allergic to mites, but 10 to house dust, presumably not to mites, which is most unusual.

Topical steroids are effective in true allergic rhinitis, confirmed by the presence of many eosinophils in the mucosal smear or blown specimen. Absence of eosinophils goes with lack of response to topical steroids, so the diagnosis of allergic rhinitis in this group is in doubt. It would seem that these results were obtained in a miscellaneous group of volunteers, dominated by 36 women.

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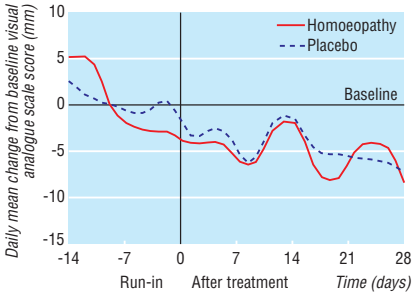
Competing interests: None declared.

1 Taylor MA, Reilly D, Llewellyn-Jones RH, McSharry C, Aitchison TC. Randomised controlled trial of homoeopathy versus placebo in perennial allergic rhinitis with overview of four trial series [with commentary by Lancaster T, Vickers A]. *BMJ* 2000;321:471-6. (19-26 August.)

Authors' reply

Editor—The authors of these letters and the other rapid responses to our paper[1] have made colourful contributions to this debate. In responding we will try to avoid opinion and concentrate on substance.

Our study was indeed underpowered, but Miller misunderstands the implications of this: the increased risk from a smaller than desirable study is of false negative results, not false positive results. That may explain the result of the visual analogue scale score in this study. In weighing this up readers should note that both the homoeopathy and placebo groups "worked" symptomatically on average; it was not neither group (a common error in interpreting placebo controlled trials).



Average change each day for each patient compared with his or her baseline average. An average improvement is evident in both groups, beginning during the single blind placebo run-in and continuing thereafter. These data contrast with the objective results shown in the paper, which improved only in the homoeopathy group

The data did reproduce the previous responses of useful symptom reductions on average in the visual analogue scale measure with homoeopathy, but this time there was also a relative average decrease in the measure with the placebo. These subjective changes began in both groups during the single blind placebo run-in (figure), and this may have further reduced the subsequent power of the study. Our discussion speculated on the origin of this possible "zeal" factor.

Whatever the causes of this symptom reduction, this study also predefined the objective measure as one of two main outcomes. The objective and subjective responses were not uncoupled in the homoeopathic group as Lipworth remarks (because both measures improved on average) but rather were uncoupled only in the placebo group, which showed a subjective but not objective average change.

Dean's analysis suggests that if the trends in the four trials had been in favour of placebo some of the protests might be more muted. Windeler dismisses this trial, and likewise our overview (we accept that it is not a meta-analysis). But a scientific dilemma of this order will not be solved by data-free opinion, only by data. Our experiments were a planned series of studies—that is, a body of work—and are best considered as such. The fundamental coherence of motive, method, and model is well described in the papers. In their responses to these papers some authors do not seem to realise that allergic asthma, allergic hay fever, and allergic perennial rhinitis are all manifestations of the atopic syndrome.

Windeler's comments on what is orthodox medicine may need revision: homoeo-

paths discovered pollen as the cause of hay fever[2] and first introduced pollen treatment in rhinitis.[3] Just as it would be wrong to use these homoeopathic roots to dismiss orthodox allergen sensitisation, so it would be wrong to use an a priori belief in the implausibility of homoeopathic action to dismiss the results of experimental testing.

In dismissing our results Morrow Brown argues that our patients could not have had perennial rhinitis, citing the previous failures of orthodox treatments. His experience varies widely from that of many patients and general practitioners, who will agree with Lipworth's remarks in his rapid response that he sees "many patients with allergic rhinitis who clearly do not benefit from conventional treatment."[4] All patients met generally accepted current diagnostic criteria (including results of skin testing). Are we to redefine these criteria rather than accept the results of this trial? And should we also do this for asthma and hay fever because of the positive results in our previous trials?

It is true that the trials are not optimal in terms of the day to day best practice of homoeopathy. This was not the point of our inquiry or the yardstick for our results. We designed the trials for clarity, simplicity, and internal validity to answer the basic question we had posed: Does a homoeopathically prepared dilution show a positive effect over and above its placebo effect? Our data take us nearer to that question's answer.

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Competing interests: MAT's salary was partly paid by the Blackie Foundation Trust, British Homoeopathic Association, and Scottish Homoeopathic Research and Education Trust administered by Glasgow University. She was reimbursed for attending a symposium organised by the Blackie Foundation Trust. DR began this research programme before using homoeopathy or developing education. He uses homoeopathy in clinical care. He accepts occasional lecture and teaching fees but has no consultancy work. He has declined all direct industry grants for research and has used intermediary regulatory organisations to ensure independence.

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Authors' reply on aspirin for primary prevention

Editor—By implying disagreements with our study[1] which are largely misplaced or non-existent, Ramsay et al may have confused doctors who are deciding about aspirin in the primary prevention of coronary heart disease.[2] We pointed out that the British Hypertension Society,[3] the hypertension optimal treatment trial,[4] and our trial[1] all say that aspirin treatment should be started only when blood pressure is satisfactory. Since both aspirin and raised blood pressure are risk factors for cerebral haemorrhage, this seems to be good clinical practice anyway.

In citing the physicians' health study from the United States[5] Ramsay et al (despite their reservations about subgroup analyses, which we also drew attention to) did not quote the non-significant trend for its finding on response according to blood pressure ($P = 0.48$) compared with the interaction term for the association of pressure on entry with response to aspirin in our trial ($P = 0.0004$). The published data from the hypertension optimal treatment trial did not show response to treatment according to pressure at entry.[4] Readers of our paper will find that the other details Ramsay et al discussed also have little bearing on the main issues.

Overall, aspirin undoubtedly reduces the risk of (mainly non-fatal) myocardial infarction by some 30% in primary prevention[6]—perhaps more in some, less in others, and in this respect we question the assumption by Ramsay et al that the benefit is necessarily constant. Our results suggest otherwise as far as blood pressure is concerned. Ramsay et al did not draw attention to the risk of serious bleeding, against which any benefit has to be balanced, although this was alluded to in the British Hypertension Society guidelines. These indicated that aspirin in primary prevention should be used only in high risk individuals.[3] We agree with this while re-emphasising the need to bring blood pressure to satisfactory levels first, whatever the degree of coronary risk. There is now evidence of an increase in haemorrhagic stroke due to aspirin in primary prevention besides the evidence we cited.[6] This evidence, together with the risk of serious gastrointestinal bleeding that we also discussed, means that the balance between benefit and hazard even in men at moderately increased risk of heart attacks is debatable and that only those at quite substantial risk should be treated in the setting of primary prevention.

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1 Meade TW, Brennan PJ, on behalf of the MRC General Practice Research Framework. Determination of who may derive the most benefit from aspirin in primary prevention: subgroup results from a randomised controlled trial. *BMJ* 2000;321:13-7. (1 July.)
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More on burns after photodynamic therapy

Might doctors performing the study have been given the wrong injection instructions?

Editor—I was surprised to read the letter from Robert Dow, chief executive of Scotia, stating that Foscan (temoporfin), a light activated anticancer drug, was originally reconstituted in water and is therefore water soluble.[1]

Foscan is highly insoluble in water and was never reconstituted in water but in a solution of ethanol, polyethylene glycol, and water. If any formulation of Foscan is injected into a saline filled needle it will precipitate. If an intravenous injection port has been flushed with saline before Foscan injection the vein is also likely to contain some saline and, on being injected, Foscan will precipitate along the vein walls, giving an excessively high concentration of drug along that vein. The photodynamic injuries reported after Foscan injection do not have a distribution which looks attributable to local extravasation of the drug.[2] They seem to follow the line of the vein and are therefore much more likely to be a consequence of precipitation of the drug along the vein resulting from a faulty injection procedure.

If there is a view in the company that Foscan is water soluble, might the wrong injection instructions have been given to the doctors performing the study, thus accounting for the excessive photodynamic toxicity along the line of the vein?

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Competing interests: DFH was formerly chief executive of Scotia.

1 Dow RJ. Burns after photodynamic therapy: manufacturer's response to second authors' reply. *BMJ* 2000; 321:53. (1 July.)
2 Hettiaratchy S, Clarke J, Taubel J, Besa C. Burns after photodynamic therapy. *BMJ* 2000;320:1245.

Manufacturer's reply

Editor—Horrobin raises two issues. The first, Dow's description of Foscan (temoporfin) powder being dissolved in water,[1] has already been raised by the editor of the *BMJ*,

Pivotal efficacy trial, study 08B. Values are numbers (percentages)

	Treatment	
	Powder (n=88)*	Non-aqueous solution (n=65)†
Burns at injection site:		
Mild	1	0
Moderate	2	0
Severe	0	0
Total No of burns	3 (3.4)	0
Other mild local photosensitivity reactions	4 (4.5)	5 (7.7)
Total No of burns and other reactions	7 (7.9)	5 (7.7)

*Reconstituted in aqueous solution.
†Identical with that used in study of Hettiaratchy et al.[1]

Richard Smith, in his letter to Dow of 10 July 2000. Dow's reply to Smith dated 26 July 2000 explained that the exact text of his letter had to be issued to the stock exchange and therefore had to be capable of being understood by lay readers. The term "aqueous solution" was therefore simplified to "water."

Horrobin's second point speculates on the impact of injecting Foscan into a saline filled injection port or needle, or flushing with saline after Foscan injection. The best database to give an insight into the influence of flushing instructions or formulation on incidence and severity of the type of adverse event described by Hettiaratchy et al[2] is our pivotal efficacy trial, study 08B. A total of 147 patients have now been treated in this study, six of whom received both formulations. The type of formulation used and the local adverse events encountered are summarised in the table.

The overall incidence of moderate to severe local adverse reactions in the injected arm is extremely low whatever the formulation. Of the 65 treatments with Foscan non-aqueous solution, 2 (3%) entailed flushing with saline, 3 (5%) flushing with water, and 60 (92%) no flushing. Of the patients who were flushed with saline, none had burns or local photosensitivity reactions. Of those flushed with water, none had burns or local photosensitivity reactions. All five local photosensitivity reactions occurred in those who were not flushed with either saline or water. Thus, in the absence of any incidence of severe burns and given only five reports of mild local photosensitivity reaction, there seems to be no evidence from clinical use to support Horrobin's opinion.

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Protective measures may help prevent *Helicobacter pylori* infection

Editor—We wish to comment on the paper by Hildebrand et al on acquisition of *Helicobacter pylori* by gastroenterologists.[1] A large cross sectional study in France has already identified an increased risk to gastroenterologists from occupational exposure, and previous negative results can be explained by the lack of a suitable control group, the study's power, or confounding by socioeconomic determinants.[2] What is not apparent is whether the stratification of risk by work practices or the period in which the exposure during endoscopy occurred alters acquisitional risk. Senior gastroenterologists encountered different conditions than those in contemporary endoscopy theatres.[3–5] Universal precautions have increased the use of protective gloves, gowns, or masks by physicians. In the United Kingdom, endoscopists invariably use video convertors and visual display monitors, and the nurse holds the mouthpiece in place or removes secretions and so is closer to potential aerosols or gastric fomites.

Nurses make better use of protective equipment and seemingly do not acquire *H pylori* infection from occupational exposure,[5] although they handle contaminated endoscopes and undertake cleaning or sterilisation procedures. These nurses care for the patient after the procedure and are potentially exposed to the same gastric fomites as the doctor. Significant aerosol generation has not been identified as common in endoscopic practice in the United Kingdom and may relate to faulty technique. Insufflated air and secretions can be expelled through the biopsy port, but to our knowledge they have never been assessed and were not described in the paper by Hildebrand et al.[1]

The major acquisition of *H pylori* relates to close contact in childhood. Occupational exposure may pose a small extra risk above childhood acquisition, but it is questionable whether further control measures are required other than those measures currently deployed. Other factors are associated with the acquisition of *H pylori*, and Hildebrand et al have not described adequately how they controlled for these factors. These include the risk from clinical exposure, housing density, and the possibility of intrafamilial spread from young children. We investigated the prevalence of *H pylori* antibodies in gastroscopy nurses in the west of Scotland (unpublished data). Our results showed no evidence of excess prevalence in gastroscopy nurses in this region with high endemicity of *H pylori*. We established that 96% of nurses in our exposed population reported always using gloves during these procedures and we concluded that this practice was protective.

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Global health research

Global health research issue was not provocative enough

Editor—The special issue on global health research[1] certainly provoked me by failing to be provocative enough and by skirting round the fundamental issues. There is throughout the issue an assumption that research, when targeted and organised effectively, will play an important part in improving international health and health care, particularly of poor people. But how will strengthening the governance for global health research improve the unequal education provision, income distribution, and access to decision making, jobs, and food, not to mention environmental mayhem, which underlie poor health as well as poor health care? There is already overwhelming evidence on the antecedents to good health, and these need to be in place before good health care can be achieved.

Is the well intentioned call for more and better health research not merely a smoke screen behind which health professionals can hide from these uncomfortable truths? The closest that the *BMJ* got to airing these issues was in the article by Bhutta,[2] an article whose penetrating analysis was tamed by the conclusion that health research might provide a solution to the problems described.

Are we all frightened of entering the political debate of the millennium—the debate on how to redress the balance between those who have and those who have not, a debate that goes under the umbrella of globalisation? Do we have to leave it to protesters at Seattle and Prague? Have we all forgotten the powerful role that health professionals can have in modifying the policies of governments and corpora-

tions? Is not the most important research that on how health professionals can most effectively change the political, military, and economic order so that all can enjoy good health?

There are already many organisations, of which Medact in the United Kingdom and International Physicians for the Prevention of Nuclear War are but two, which discuss these issues. But our voices, along with those of the poor, are drowned by the clamour of the powerful. May I suggest that those health professionals who want to make a difference use their voices to combat the clamour of the powerful and that the *BMJ* encourages a debate on the real issues?

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Internet access is not yet universal

Editor—When the internet is discussed in terms of health information[1] two issues are usually overlooked. The first is access to electronic information sources for frontline health professionals in district hospitals and clinics in developing countries. The second is the content and format of the information needed for health services with limited resources.

We are all agreed that access to the internet and email provides information support which is vital to good (and evidence based) practice, research, and teaching in the health sciences. At the same time we have to remember the likely continuing remoteness of most health professionals—the frontline doctors and nurses without access to the internet—from the kind of biomedical and health information that would be appropriate to their limited resources.

The poorest countries are unable to sustain, reliably, all the many components of information technology infrastructure even at their major hospitals, let alone at remote district hospitals and clinics. These include the essential hardware, software, printing consumables, telephone, radio, or satellite connections, electricity supplies, internet bandwidths, technical support, human resources staff with skills in information technology, and so on.

The vast majority of health workers in the poorer countries, particularly those in rural areas, urgently need information support, but it has to be appropriate to the practice of health care with limited resources and under often adverse conditions unfamiliar to most of those practising and writing in the West. This kind of information has been available up to now, it seems, only in print. It can only reach these health professionals in print.

The need for basic information, the reference sources essential to good practice, has always been acute. It is now so long standing that it is routinely overlooked—recurrent shortages of staff, drugs, and equipment have to take precedence. Yet it is more than likely that donors will acknowledge the need to provide such reference sources in print and will support responses to that need.

If some of the needs can be met through the distribution of copyright-free copies of, for example, therapeutic guidelines drawn from electronic sources in national or regional resource centres, so much the better. Electronic and print versions of these reference sources could be widely distributed, ideally to individuals, in a worldwide concerted effort. To avoid more delay, one or two regional or national pilot studies could be initiated now. In Zimbabwe we hope to do this as soon as the therapeutic guidelines currently being distributed in print to Uganda's rural hospitals as part of a programme of continuing medical education can be revised to match Zimbabwean conditions.

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1 Edejer TT. Disseminating health information in developing countries: the role of the internet. *BMJ* 2000;797-800. (30 September.)

Science aid programme could be set up to help researchers in developing countries

Editor—My experience of trying to conduct research in Syria in collaboration with scientists from developed countries is a sobering one. Although many of the scientists whom I have contacted over the years have provided the help I have asked for, many others have not had time to answer technical research questions (study design, statistical analysis) from an outsider—a scientist from a developing country. Who can blame them?

Multidisciplinary research teams, scientific journals, access to databases are a fiction for most researchers in developing countries. Even postal charges to submit manuscripts are a burden, as is the ability to pay processing fees or return proofs by courier or fax as some journals require.

High quality research in developing countries is often done by sending in qualified research teams for just a short time. Such an approach is costly, with little impact on the host country, and sometimes ethically questionable or culturally biased. International agencies concerned with the scientific development of developing countries prefer to talk with ministries and big organisations. In many developing countries, however, these bodies are plagued by bureaucracy and riddled with corruption. This may be one of the reasons why decades of international scientific aid have produced only modest results.

I do not underestimate the efforts of the international community, but what we urgently need is an easily accessible system whereby scientists in developing countries can talk to individual experts in advanced ones. Scientists in developing countries—faced with the continuous frustrations of unanswered queries, lack of institutional support and funding, and letters rejecting their manuscripts—eventually either join the Third World scientific diaspora in Europe or the United States or are forced to start earning a living. A vast number of potentially important contributions is being lost.

Science as it is practised now inevitably leads to the silencing of researchers in developing countries. This is a big waste; is there nothing we can do about it? I suggest that a science aid programme is set up, which would be a scientific humanitarian project to aid researchers in developing countries. Leaders of science could facilitate these researchers' access to specialised consultations with established scientists in developed countries—say, allocating one hour a week per scientist in participating centres of excellence for helping a research project or a researcher in a developing country. I will send a personal sketch of such a programme on request.

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Prescription pricing databases should include more details to assess prescribing rationality

Editor—Campbell et al's study on prescribing indicators for general practices in the United Kingdom showed how difficult it is to conduct quality assurance of prescribing by using the existing prescription pricing databases in the United Kingdom.[1] The authors selected 41 proxies of rational prescribing from prescribing analysis and cost (PACT) data, the English system. Only seven of these were rated as valid for economic rationality (cost) and five for scientific rationality (quality).

This shows the impossibility of assessing prescribing rationality without knowing the diagnosis or patient identification. None of the prescription pricing databases in the United Kingdom records either of these. Yet from 1987 the American equivalent of the NHS, Medicaid, has used its database containing these data to screen monthly for two things: incompatible prescribing and inappropriate prescribing.[2]

Incompatible prescribing is the concurrent use of two or more drugs that have serious interactions (a common cause of the 5% of acute hospital admissions that are prescription related)—for example, the use of an angiotensin converting enzyme inhibitor with a non-steroidal anti-inflammatory drug or a potassium sparing diuretic in the same patient. This can be shown when a

patient's identity is recorded in the database, by computer screening of current and recent prescriptions. (The master patient index used in Scotland and Northern Ireland is ideal for this purpose.)

Inappropriate prescribing is the use of a drug that is known to worsen the condition diagnosed—for example, a non-steroidal anti-inflammatory drug given to a patient with peptic ulcer or a β blocker given to a patient with asthma. This requires the diagnosis to be given in the database, and an appropriate code is required.[3] This is a matter of great importance, for the Medicaid researchers showed an incidence of inappropriate prescribing of over 2% (25 600 inappropriate prescriptions issued out of a monthly total of 1 million in one state alone).[4] Informing the doctors caused inappropriate prescribing to halve within a month, and drug related admission to hospital fell substantially.

The computer technology is available; all that is wanting is the administrative will. These simple screening processes can save much iatrogenic suffering and death, and a great deal of money, while simultaneously educating prescribers.

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Global health agencies' response to malaria

Panel should be set up to review malaria control proposals from endemic countries

Editor—A news item about the Oxford 2000 conference on tropical medicine reports my address at the meeting.[1] Readers may have been left with a negative impression of Roll Back Malaria from Yamey's précis of my remarks. I therefore wish to underscore my view that this is an extremely important and impressive global project, which has the promise to reinvigorate the global effort to control the disease. My colleagues and I at the Center for International Development at Harvard greatly applaud the effort; indeed we have worked closely with Roll Back Malaria in preparing some of the background analysis of the heavy economic burden of malaria in Africa.

The purpose of my remarks was to emphasise the need for Roll Back Malaria and the donor agencies that are partners within it to implement an independent, scientifically driven review panel under the auspices of the World Health Organization. Such a panel would screen malaria control proposals from endemic countries and would urge ample and immediate funding for those thought likely to achieve success. Such an arrangement would serve as a quality check that malaria control projects are scientifically sound. The fact that the projects have passed scrutiny would help give donors confidence that they can commit the much larger funds needed for malaria control without them being wasted.

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1 Yamey G. Global health agencies are accused of incompetence. *BMJ* 2000;321:787. (30 September.)

Agencies dispute Attaran's view of Roll Back Malaria initiative

Editor—The Roll Back Malaria initiative strives to halve the burden of malaria by 2010. It was launched in 1998 and now includes not only its founding partners (the World Health Organization, Unicef, the World Bank, and the United Nations Development Programme) but also many other organisations and, most importantly, representatives of countries and communities where malaria is endemic.

One of the initiative's aims is to ensure that results of research are used widely and wisely. We are not sure that the interests of partners in the initiative in South East Asia were adequately reflected in discussions at the conference on tropical medicine reported on by Yamey.[1]

Given the variability of malaria around the world, consultations on strategies to reduce the incidence of malaria took place in each region during 1999 and 2000. All Mekong Roll Back Malaria partners agreed on three major recommendations for the participating Mekong countries in areas with multidrug resistant *Plasmodium falciparum*:

- Rapid tests should be introduced in addition to microscopic blood slide examination to facilitate the diagnosis of *P falciparum* malaria;
- For patients with a positive result in a rapid test, combination treatment (usually including an artesunate drug combined with mefloquine or another effective drug) should be provided
- For patients in whom the result is negative (though we recognise that they might have *P vivax* malaria, which is not diagnosed by rapid tests), or if there is microscopic confirmation of *P vivax* malaria, chloroquine should be provided.

The WHO Roll Back Malaria project, which monitors malaria from country to country, has documented a 15-20% prevalence of *P vivax* malaria in Myanmar. Unicef's malaria treatment policies, supply of drugs, and diagnostic tests for Myanmar reflect the epidemiological evidence and the agreed Mekong Roll Back Malaria recommendations.

Comments regarding the use of salt, mentioned in Yamey's article, were obtained from open discussion on the Roll Back Malaria internet site (www.who.int/rbm/); there was no endorsement of what was being discussed by either host organisation. Attaran's suggestions that the World Bank and United States Agency for International Development encourage the selling of bed nets are misleading. Both organisations say that countries should encourage people who can afford bed nets to buy their own and target limited resources on subsiding costs for those who cannot afford to buy or treat nets.

Malaria prevention and control is the business of everyone, from families to communities, from government agencies to the international community. It is within this context that we would welcome Attaran and his colleagues into the partnership.

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1 Yamey G. Global health agencies are accused of incompetence. *BMJ* 2000;321:787. (30 September.)

Shooting the messenger: author's reply

Editor—I attended the African summit on malaria in Abuja, Nigeria—a high profile meeting of the Roll Back Malaria project.[1] It was an upbeat event, full of fanfares and grand promises. Nabarro expressed his optimism to me that he would find donors to furnish the $1bn needed annually to eradicate malaria.[2] The crucial questions now are whether this donor money is being used effectively and whether the project is employing the worldwide expertise on malaria that is needed for its control. On both fronts, it must be accountable.

Why, then, are Nabarro et al "dismayed" at the *BMJ* for reporting legitimate concerns about the effectiveness of the project to date?[3] This smacks of defensiveness. The *BMJ* has a part to play in fostering an open debate about international health issues, and it is reasonable for me to have reported Attaran's address to an important conference on tropical medicine.

Nabarro et al imply that I took Attaran's comments at face value, without any corroborating evidence. But I investigated all three of his assertions, and all are valid.

Respected sources in South East Asia, who unfortunately do not wish to be named, have confirmed that Unicef supported the use of chloroquine for treating malaria in

Hyperlink to article on malaria cure using common salt under epidemiological news on Malaria Network website

Burmese towns along the Thai border. They also state that there was no systematic identification of Plasmodium spp before treating patients in this region, that no other effective treatment was provided, and that chloroquine was therefore given to patients with *P falciparum*. The *Lancet*, in a lead editorial, also asked why Unicef has proposed support for chloroquine for Burmese towns along the Thai border.[4]

I read the article about the "malaria cure" that uses common salt on the Malaria Network, the collaborative web project run by the World Health Organization and World Bank. This was not part of an electronic discussion site. It appeared under epidemiological news (figure), though it mysteriously disappeared shortly after my report was published.

Why are two authoritative members of Roll Back Malaria publishing such potentially harmful advice?

Finally, Attaran is not alone in questioning the World Bank's use of cost recovery (user fees) for insecticide treated bed nets. Cost recovery remains a contentious issue, the global trend in the development community being towards abandoning its use (P Garner, personal communication). The US Congress is going further still. A new amendment to US foreign aid law opposes loans by international financial institutions—including the World Bank—that "would require user fees or service charges on poor people for primary education or primary healthcare."[5]

Roll Back Malaria will thrive by being open and responsive to criticism. I agree that it is "in principle well founded, but partners must realise that for the programme to succeed money cannot be squandered on flawed projects."[4]

Gavin Yamey *assistant editor, BMJ*

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NHS Direct

NHS Direct is a learning organisation

EDITOR—Farrer et al report a case of a 6 month old boy whose mother had rung NHS Direct and who was subsequently admitted with diabetic ketoacidosis.[1] We cannot comment on this specific case without access to the caller records, but we are perturbed by the suggestion that attempts to give feedback to NHS Direct were frustrated.

The service welcomes and encourages feedback from callers and clinicians. All general practitioners have been issued with specific feedback forms, which can be made available to other clinicians. More importantly, each site has a medical and a nurse director with key responsibilities for clinical governance, as well as a clinical advisory group of local clinicians. We invite any clinician who has reservations about the handling of a particular case to ring the medical or nurse director of their local site to report their reservations.

NHS Direct has a substantial advantage over most of the NHS in terms of reviewing cases because all calls are taped and can thus be fully analysed along with the relevant decision support guideline. Commonly, such investigation will reveal misunderstanding or erroneous reporting, themselves valuable lessons. However, if fault is found with the handling of the call, nurse advisers can learn from this by working through it with their supervisor. If fault is found with the guideline there is a clear process for central review and amendment of the decision support software. Contrary to the editor's comment, NHS Direct is a machine that is keen to learn and improve.[2]

We endorse Kempley's comment that "the primary end point of NHS Direct should be whether it meets the need of its callers for information, not whether it reduces work for any sector of the medical profession."[3] Although a reduction in inappropriate demands on the health service is a realistic and desirable aim, it is not the key purpose of the service. Restricting demand by restricting access has often seemed to be the only strategy in the health service. NHS Direct offers the opportunity to meet demand at an appropriate level. If this means reaching out to patients below the water level of the classic iceberg of illness, so be it. This may not necessarily reduce demand elsewhere in the service, but it will mean, as Kempley suggests,[3] that the increasingly information hungry population can get its health information from the NHS with the quality control that may be lacking elsewhere.

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1 Farrer K, Rye P, Murdoch L, Bain M, Hampson-Evans D.NHS Direct. *BMJ* 2000;321:446. (12 August.)
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Website does not always take symptoms seriously enough

EDITOR—I was interested to read Farrer et al's concerns about paediatric advice from the government's NHS Direct service.[1] As a senior lecturer in psychiatry I experimentally accessed the NHS Direct website (www.nhsdirect.nhs.uk/) as if I was two of the patients in my recent care to see what management the service would advise.

In one case I pretended that I had symptoms highly suggestive of raised intracranial pressure, just like one of my patients with a malignant brain tumour. I was advised to take aspirin. When I pretended that I had symptoms suggestive of paranoid schizophrenia (including command auditory hallucinations telling me to kill myself) I was given advice that effectively fobbed me off and did not meet with the severity of the symptoms.

The NHS Direct site seems to be overly ambitious and poorly trialled. Like Farrer et al I attempted to give a clinician's feedback, but who it went to or what became of it I do not know. The site never changed and probably still offers the same inaccurate advice to seriously ill patients.

It occurred to me that if a doctor was to give negligent advice there would be the possibility of legal redress, but with the NHS website is there a record of a consultation? Probably not. Even if there was, who would the patient sue? The NHS Executive or the secretary of state, perhaps.

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1 Farrer K, Rye P, Murdoch L, Bain M, Hampson-Evans D. NHS Direct. *BMJ* 2000;321:446. (12 August.)

Will website help people or scare them?

EDITOR—Just after the NHS Direct website (www.nhsdirect.nhs.uk/) was launched I tried it to see what it suggested for a recurrent sore throat I was experiencing at the time.[1] Going to "Conditions and treatment" and typing in the search box "Sore throat" I was mildly amused to find that all it could suggest was "Throat cancer." Helpfully, today it also suggests "Laryngeal cancer" as an alternative. The actual diagnosis (from my general practitioner) was more mundane: a middle ear infection.

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1 NHS Direct [letters]. *BMJ* 2000;321:446. (12 August.)