Letters

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Medicine information leaflets fail concordance test

EDITOR—Two initiatives about the use of medicines reflect the growing movement towards involving patients in their health care. The first is concordance, which promotes a model of prescribing based on negotiated agreements that respect the patient's perspective.[1] The second is a European directive requiring mandatory patient leaflets with all medicines.[2] Manufacturers must produce these comprehensive leaflets, including all datasheet information, in a form understandable to patients. Unfortunately, concordance and such mandatory leaflets seem to be mutually incompatible.

Concordance requires that patients be fully informed about their medicines in the widest sense. Perversely, the mandatory leaflets are in most senses narrow and undermine concordance. Firstly, patients only receive the information when they open their medicines. Any information throwing doubt on the appropriateness of the drug therefore comes too late. Secondly, patients need to know the various options for treatment. Currently leaflets include only information about the drug in question. Thirdly, patients need balanced information, but the leaflets present mainly negative information, such as side effects.

Other problems exist. Recent research found that nearly a fifth of patients failed to notice the package leaflet. Of those who recalled receiving a leaflet, only two fifths read some of it and two tenths all of it.[3] Unlike computer generated pharmacy leaflets (which are used in the United States and Australasia), the leaflets cannot be individualised.[3] Computer generation allows pharmacists to use leaflets as aides mémoire and go through them with patients. The electronic form also gives options for providing access to partially sighted people.[4]

Surgery computers are already able to print condition-specific leaflets at doctors' desks.[5] They could be adapted as decision making aids for patients and hence facilitate concordance. The doctor makes a diagnosis and supplies a leaflet, which includes all treatment options. Having read and digested this information, the patient returns to make a decision jointly with the prescriber. This may reduce misunderstandings but might increase the number of consultations. But common conditions such as hypertension already necessitate multiple consultations before long term treatment options are decided.

The introduction of comprehensive medicine leaflets is an important development in the provision of information, but the opportunity has been lost to support concordance in medicine taking, which is now endorsed by the NHS Plan. The inadequacies of such leaflets mean that they may become a legal backstop, with patients accessing more relevant, individualised, and wideranging information generated electronically. European law needs amending to reflect this.

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What have we learnt from the Alder Hey affair?

February 2001 seems to have been average month for organ donations in Newcastle

EDITOR—Bauchner and Vinci ask what we have learnt from the Alder Hey affair, in which it was found that various whole organs had been removed at necropsy from children at Alder Hey Hospital in Liverpool without their parents' knowledge and consent.[1] This affair and its aftermath highlighted many of the controversial issues of organ removal.

It has been suggested that the number of organs offered for transplantation has dramatically declined since the adverse publicity. The cardiopulmonary transplant unit at the Freeman Hospital in Newcastle upon Tyne has indeed seemed uncannily quiet recently. February is always said to be a quiet month, and we have reviewed our statistics for this month over the past four years.

In February this year we had 24 offers for heart and/or lung transplantation, which resulted in three transplants. In February 2000, 24 donations were offered, yielding seven transplants. February 1999 saw 30 offers and five transplants, and February 1998 saw 17 offers and six transplants. February 2001 therefore seems to have been an average month at this transplant unit, in keeping with the national figures discussed at the emergency transplant summit in London earlier this year.[2 3]

Although there is no strong evidence of fewer offers after the recent media interest, we hope that crisis talks led by the government will increase public awareness of the plight of the thousands of patients on the transplant waiting lists. The campaign for presumed consent, endorsed by the BMA, may go some way to address the imbalance between supply and demand.

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Lack of information on transplant procedures is disturbing

EDITOR—In 1959 our 10 month old daughter, Alice, became ill; she died within hours in Edinburgh Sick Children's Hospital. My husband and I requested a postmortem examination, and endocardial fibroelastosis was diagnosed. When Alice's body was cremated we assumed that all of her was there. It never occurred to us that organs might be retained for scientific purposes, and now that more than 40 years have passed we would hate to ask. At least the postmortem examination may have helped others.

Our youngest daughter, Sacha, died in 1985 after a cycling accident when she was 22. At Charing Cross Hospital we learnt that she was brain stem dead with no hope of recovery. Unaware of whether she had carried a donor card, we at once said that she would wish her organs to be used to help others. Once this had been agreed I asked, "When can you turn that machine off?" and was told, "As soon as we have removed the organs."

We were in no condition to question this let alone argue, having just flown in from Switzerland in a state of shock. Instead we went to Sacha's new flat just along the road and spent the day dismantling her possessions, knowing that the doctors were doing the same with her still functioning body. More than four hours passed before the ordeal was over. We were thankful to learn that Sacha had helped several people who needed spare parts, but afterwards we felt that she had been violated.

The number of donor organs is said to have declined because of adverse publicity after the Alder Hey affair, so more members of the public are being urged to carry donor cards. The donor card states: "I would like to help someone live after my death." The meaning of death is now in question. The dictionary describes it as "the end of life," and to simple folk like us that means that every physical vestige of life has ceased. After organs have been donated it is harrowing to read about the sensations of deeply unconscious people and the need to anaesthetise people who are brain stem dead because of their physical reactions while organs are being removed.

Although these things can be explained, this should be done when donor cards are issued; members of the public are not being told "everything you need to know" about organ donation, and the lack of information on transplant procedure is disturbing.

Criticism of pathologists has been unfair

EDITOR—The pathologists at Alder Hey Hospital, Liverpool, have been portrayed in an unfair and ill informed manner.[1] I am an ophthalmologist and have a large collection of clinical photographs, fluorescein angiograms, etc of my patients. I have collected these over several years; some are sorted in filing cabinets while several lie in my office drawers. Some are for immediate clinical care and some for future reference, teaching, and research. I sort them out in my own time.

I suspect that my interest in such material is exactly what a pathologist's interest in a retained body part is. A pathologist's reason for collecting and retaining a specimen of a malformed heart is exactly similar to a cardiologist's reason for keeping a collection of echocardiograms or a radiologist's reason for keeping computed tomograms.

The BMA should help to educate the public. The fact that retained organs exist in pathology laboratories does not in any way mean that pathologists or any other group of doctors are disrespectful of any patient, living or dead, whose organs have been stored. The profession and those who have chosen to represent our views should take a stand and stop apologising for everything that the media throw at us.

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Clinicians and pathologists must work as team

EDITOR—As a consultant histopathologist, I find that the current public and political response to events at Alder Hey Hospital threatens the very essence of my work.[1] Histopathologists are charged with providing a diagnostic service (surgical pathology, cytology, and requests for postmortem examinations) for a clinician or coroner. It is impossible for us to do this without access to human tissues.

Although some causes of death can be ascertained by macroscopic examination of the organs alone, in many cases examination of organs and tissues is the only possible way of accurately diagnosing the cause of death. In some instances the appropriate investigations on fresh frozen samples cannot be determined until preliminary microscopic examination has been performed. The small size of several complex organs (particularly paediatric hearts) means that it is impossible to perform a complete and thorough examination without first fixing (that is, retaining) the organ.

It is up to the people requesting our services and the public at large (as represented by our politicians) to decide if they wish for a proper investigation into deaths or for a suboptimal examination, in which vital information may be lost. Suboptimal examinations will inevitably mean that congenital abnormalities are sometimes missed, resulting in further affected pregnancies causing additional distress to parents, and that cases of homicide or infanticide are missed.

There is currently a crippling shortage of histopathologists. This inevitably results in prioritisation of work, and clearly a delay in turn-around times in clinical diagnostic work is unacceptable. The current political and public response will require us to indicate to bereaved relatives that postmortem work has been delayed because priority was given to other aspects of the workload. This will potentially engender further ill feeling and mistrust towards pathology services, which will be detrimental to the current damaged doctor-patient relationship and conceivably to the health care offered by trusts.

All people responsible for requesting cellular pathology investigations have a responsibility to consider the reasons for the request and the consequences of the investigation. As doctors we are charged with explaining both of these clearly and realistically to patients and relatives. Only with clinicians and pathologists working as a team will public trust in pathology services be regained.

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Asking for consent would halt decline in voluntary necropsies

EDITOR—The Alder Hey scandal and other recent events have brought the topic of necropsy into the news. Numbers of voluntary necropsies (those for which consent must be granted by the family) have been falling over the past decade, and this trend should be a cause for concern. These necropsies have a role in education, research, and audit.[1]

In view of this, we conducted an audit of voluntary necropsies at St Mary's Hospital, London. In 1980, voluntary necropsies were conducted on 24% of patients who died at the hospital. In 2000, this figure had fallen to 7%. In almost three quarters of deaths (excluding coroners' cases) in the first six months of 2000 permission to conduct a voluntary necropsy was not sought. In half of these this was because the diagnosis was thought to be known beyond reasonable doubt. Of the families asked for permission, 46% agreed. Therefore if we asked for permission from all the families of patients in whom we were sure of the diagnosis we could theoretically double our rate of voluntary necropsies.

We surveyed 23 consultants at St Mary's Hospital; 16 replied. Of these, 14 thought that too few voluntary necropsies were being conducted. Contraindications to asking for permission were family dynamics (37% of answers), religion (37%), and knowing the diagnosis (15%). Only 6% of answers stated that there were no contraindications. The most common reasons for the consultants' teams not actually asking for permission were knowing the diagnosis (61% of answers), the team not being present at the time of death (not on call, too busy) (28%), and family dynamics (6%). Fourteen consultants thought that it was reasonable to do necropsies on some patients in whom the clinicians were confident of the diagnosis.

Eleven of the consultants said that asking the family for consent to conduct a voluntary necropsy would be harder after

the events at Alder Hey Hospital. This will exacerbate the current decline in numbers of voluntary necropsies; if we value necropsy we must try to stop this reduction.

Half of the consultants we surveyed thought that developing a culture of asking for consent as standard practice was the best way of halting the decline in voluntary necropsies. We agree, but would emphasise the need to request necropsies in patients in whom clinicians are confident of the diagnosis—this is a simple way to increase the numbers done; assists in audit, education, and research; and is supported by the royal colleges.[1]

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α Streptococci and recurrences of otitis media

Right choice of antibiotic can decrease risk of recurrence

Editor—Roos et al showed that α haemolytic streptococci—a part of the normal flora—have a protective effect against otitis media.[1] Even though the effect was modest, this finding is remarkable and should influence antibiotic policy.

Broad spectrum antibiotics impair the normal flora. In a recent epidemiological survey we found an association between the use of such antibiotics and an increased risk of recurrence of acute otitis media (broad spectrum antibiotics *v* phenoxymethylpenicillin: odds ratio 1.8 (95% confidence interval 1.3 to 2.6)).[2] Howard et al showed that recurrences were less common among children treated with a narrow spectrum drug (penicillin or erythromycin) than among those treated with a broad spectrum antimicrobial (amoxicillin or erythromycin plus sulphonamide) (13.3% *v* 40.5%, P = 0.0125).[3]

Nowadays, broad spectrum antibiotics are increasingly used to treat otitis, although the clinical picture of this disease has become milder.[2] Broad spectrum drugs have not been shown to have therapeutic advantages over narrow spectrum drugs in uncomplicated acute otitis media,[4] but abundant use of them has ecological drawbacks in selecting resistant bacteria.[5] These facts support the idea that the first line treatment of acute otitis media should be a narrow spectrum drug.

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Tampering with microbial ecology is risky

Editor—Roos et al have chosen an unusual path[1] by not following the scientific tradition in which inquiries are usually based on the existing knowledge base: before a new hypothesis is formulated and tested a comprehensive review of the evidence is the standard. Before describing their experimental protocol the authors do not bother to provide the current evidence regarding antibiotic treatment for otitis media.[2 3]

In their hypothesis testing they ignore the fact that, for children prone to otitis, antibiotic prophylaxis is less effective than placebo treatment. The largest clinical trial (n = 194) on the subject has shown that those children who received placebo prophylaxis had fewer bouts of acute otitis media then those who were taking antibiotics.[4] Also, antibiotic treatment increases recurrence rates two to six times.[5]

As this background material is missing the authors have designed a flawed experiment. They have introduced a strong bias, since all the children were treated with recurrence-causing antibiotics before microbial cocktails were inserted in their noses; thus there was no real control group. Having a third group that received neither antibiotics nor bacterial spray is essential before we can advocate changes to children's microbial ecology. In addition, the study's conclusions are based on a limited dataset; confounding factors such as the child's age, prior use of antibiotics, number of prior episodes, and season of the year were not analysed.

The conclusions and recommendations are discredited by this study being so short (of three months' duration), having a basic experimental flaw (no true control group), and having the marginal benefit of 10 episodes of acute otitis media in the groups (22 *v* 12). I hope that it will not be taken seriously as it is not good science. Treatment of young children with nasal sprays to alter microbial ecology is a risky clinical undertaking.

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Authors' reply

Editor—Joki-Erkkilä and Pukander have emphasised the influence of antibiotics, especially broad spectrum antibiotics, on the normal flora. In a recent study they showed a higher recurrence rate with broad spectrum than with narrow spectrum antibiotics.[1] Information on the action of antibiotics on the normal flora, and especially the interfering normal flora, however, is scarce.

Cantekin asserts that we did not build our hypothesis on existing knowledge. The important biological role of the normal flora for protection against invading pathogens has been reported previously.[2 3] Our own research started about 15 years ago with studies on streptococcal pharyngotonsillitis.[4] We showed that lack of α streptococci in the throat predisposed to recurrent tonsillitis.[5] Similar observations have been reported in children prone to otitis and in children with secretory otitis media. We assume that Cantekin has not followed discussions about the importance of the normal flora for protection against potentially pathogenic bacteria in mucous membranes.

Does Cantekin believe that antibiotic prophylaxis protects children from otitis media or not? In the last sentence of his second paragraph he claims that antibiotic treatment increases the recurrence rate. If that is true it supports our results, which indicate that antibiotic treatment disturbs the normal flora and makes children who are prone to otitis even more prone to it.

Cantekin believes that a follow up of three months is too short. We do not agree as the children prone to otitis whom we studied get frequent infections, especially during the seasons when the study was carried out.

Cantekin claims that we should have had a non-treated group, but we chose not to for ethical reasons. All the children had a purulent ear infection. In these very young children complications are more common. We also wanted to avoid selecting or excluding patients according to how ill they were. Thus all children received antibiotics followed by spray or placebo.

The analysis of confounding factors that Cantekin thinks is missing is in the full version of the paper on bmj.com.

We do not alter microbial ecology. We restore it with normal interfering α streptococci, which exist in almost everyone. We have now treated about 600 patients, including patients with tonsillitis, in different studies, without serious complications. We have excluded patients with immunodeficiency and valvular heart defects, and the side effects have not differed from those of placebo (saline).

Cantekin hopes that our findings will not be taken seriously. We have to disap-

point him: our studies on recolonisation with α streptococci have generated a large response, and studies are now planned in different countries.

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Patients' preferences for patient centred approach to consultation

What is patient centredness?

EDITOR—Little et al's study seems to show patients' overwhelming preference for a patient centred approach to consultation in primary care.[1] The issue is not so much whether most patients agree that, for example, they "want the doctor to understand [their] main reason for coming" as whether a desire for the contrary would represent a belief in some other kind of approach to the consultation or just be plain odd. In other words, what kind of person could possibly say, and be thought rational, "I don't want the doctor to understand my main reason for coming"?

I invite readers to review the questionnaire, putting the opposite case in this way and asking themselves how many of the questions are of this type: "I don't want the doctor to be friendly and approachable," "I don't want the doctor to find out how serious my problem is," and so on. They might also like to consider how easy it would be to construct a mirror questionnaire, couching doctor centred values in a way that no one could reject ("I want to trust my doctor's expertise").

The difficulty is that there is an implicit contrast in the minds of everyone who works in this field between patient centredness and doctor centredness. The same contrast, though, is not necessarily in the minds of patients, who lack the context of the professional debate.

Researchers may believe that patients are expressing a preference for one type of consultation rather than the other, whereas they are simply expressing a preference for the commonsensical rather than the perverse. Patient centredness requires a more sophisticated approach than this.

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1 Little P, Everitt H, Williamson I, Warner G, Moore M, Gould C, et al. Preferences of patients for patient centred approach to consultation in primary care: observational study. *BMJ* 2001;322:468-72. (24 February.)

Authors' reply

EDITOR—Skelton questions the methodology of our study, and whether patients would ever disagree with particular items about expectations—that is, he is saying that the elicited expectations were meaningless.

We used a standard psychometric questionnaire design, asking the patients to agree or disagree with statements on a balanced seven point scale, with items based on the patient centred model. We could have limited patients' responses to how strongly they agreed. However, to assume that it is perverse and irrational to disagree is patronising to those who didn't agree that the doctor needed, for example, to deal with their worries (12%), be interested in the treatment they wanted (23%), or understand their emotional needs (30%). We too would have been surprised if most patients disagreed with the statements, but it would have been incorrect to constrain, and potentially bias, patients' responses by such a priori assumptions.

Citing dichotomised or polarised agreement or disagreement with particular items is a simplistic critique of the method; it overlooks the fact that we elicited the strength of expectations and that most of the variance in the response was in how strongly patients agreed. We showed how strongly patients wanted different aspects of the patient centred approach; how strongly this contrasted with other expectations (for example, for a prescription); and which patient groups most strongly wanted a patient centred approach.

If the measurements are meaningless then two points are important. Firstly, there would be no pattern among such meaningless measurements, which would be unreliable. In fact, we showed that there were distinct aspects of patients' expectations—for communication, partnership, health promotion, interest in the effect on life, and personal understanding. Where it was possible to construct scales (that is, more than one item related to the same area) the scales were highly reliable. This is clearly important theoretically in understanding the nature of patient centredness (is it one thing or many?) and practically (doctors need to be aware of several dimensions of care that their patients may want).

Secondly, measuring how well expectations were met (using identical scales) should also be meaningless, and unrelated to outcome. This is not so: using the same cohort we have found that how well these expectations were met strongly determines a range of outcomes important to patients and the health service.

The strength of patients' expectations that we measured is meaningful and reliable and should not be dismissed too lightly.

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BSE and variant CJD

Assumption that BSE originated from scrapie in sheep led to misjudgment

EDITOR—In espousing the scrapie origin of bovine spongiform encephalopathy Brown fails to explain adequately why scrapie outside Britain has not led to bovine spongiform encephalopathy.[1]

Does Brown believe that scrapie "has mysteriously chosen the United Kingdom as its only geographical site and the early 1980s as its only historical occurrence" to appear in cattle? He explains that the comparatively high incidence of scrapie in sheep in the United Kingdom and the changes in rendering were responsible. This seems implausible, for reasons set out in the report of the BSE [bovine spongiform encephalopathy] inquiry.[2]

I am not aware of comparative data on the incidence of scrapie in the United Kingdom and abroad. Similar changes in rendering were occurring elsewhere, and no rendering process either before or since the emergence of bovine spongiform encephalopathy has been capable of completely inactivating scrapie.

The BSE inquiry was impressed by the more plausible alternative view that the agent giving rise to bovine spongiform encephalopathy was a novel strain of unknown origin that was pathogenic to humans. This view held that the strain arose in southern England in the 1970s and that the infection spread in waves through recycled infected cattle waste in meat and bonemeal. We will probably never know the origin of the agent—whether it arose in cattle or sheep or, indeed, any other species whose waste was incorporated into meat and bonemeal.

It was the assumption that bovine spongiform encephalopathy originated from an infection in sheep that led to the judgment that the risk of transmission to humans was remote. Brown's own research has indicated that scrapie was not linked to Creutzfeldt-Jakob disease, although humans

have been exposed to scrapie for over 200 years.[3]

The uncertainties about the origin of bovine spongiform encephalopathy were not appreciated by many of those involved at the time. This is not a matter of criticism. As a result, however, the public was reassured unjustifiably.

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Humans can live with BSE as long as they do not eat brains

Editor—Brown not only understands bovine spongiform encephalopathy and variant Creutzfeldt-Jakob disease but gives a coherent account of them in plain English not embellished by jargon.[1] Even he, however, falls into the trap of writing that "epidemiological evidence indicates that scrapie does not affect humans."

Epidemiological evidence indicates that we do not know whether scrapie affects humans—a very different matter. If Brown had interrogated abattoir workers in 1989 (as I did) about their routine in dealing with sheep and cattle brains he would have been told that, whereas cattle brains were always removed to be added to "meat products," sheep's brains were routinely left inside the skulls (it wasn't worth the bother to remove them) to be cooked up with the rest of the carcase and fed to cattle in the form of "meat and bonemeal."

The reason that humans have had little trouble with scrapie over the centuries (but not none—how else could sporadic Creutzfeldt-Jakob disease have arisen?) is that we were never forced to swallow sheep's brains, some of them infected with scrapie. But we were forced to swallow cattle brains, which after 1981 contained the proliferating scrapie agent.

The rendering process changed in 1981: fat solvents, previously added to collect fat for tallow, were withdrawn, and from then on the (lipid) sheep's brains, complete with the organisms, went into cattle feed instead of into candles. It was not "inactivation," or the lack of it, that enabled scrapie to get into cattle rations after 1981 but stopping its diversion into tallow. These vital facts about fat solvents, given in the Southwood report in 1989, were overlooked by the BSE inquiry, which is why the chairman of the inquiry, Lord Phillips, postulated that bovine spongiform encephalopathy must have some other origin.[2]

The apparent preference of variant Creutzfeldt-Jakob disease for young people, which Brown finds puzzling, is surely because the victims were children between 1981 and 1989 (cattle brains were finally banned from human foods in November 1989) and therefore shedding teeth. The resultant raw areas in their gums provided the organism with direct access to the bloodstream, thus shortening the incubation period. Nobody knows how many humans are incubating variant Creutzfeldt-Jakob disease, but we can live with bovine spongiform encephalopathy, just as we have lived with scrapie for three centuries, as long as we are not forced to eat the brains.

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1 Brown P. Regular review: Bovine spongiform encephalopathy and variant Creutzfeldt-Jakob disease. *BMJ* 2001;322:841-4. (7 April.)
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Patient education to encourage graded exercise in chronic fatigue syndrome

Trial has too many shortcomings

Editor—Powell et al's controlled trial of graded physical exercise in chronic fatigue syndrome has several shortcomings.[1]

Firstly, the only tool that was used to assess the level of physical activity was entirely subjective. This was a single item (the third item) of the 11 item standardised SF-36 health survey questionnaire. Use of this single item alone as a valid measure of physical fitness is hardly acceptable in the absence of objective data.

Secondly, in a randomised study one can only compare like with like. In this case, all patients in the intervention arms had a minimum of three telephone contacts during the first three months. Patients in the control group were abandoned to primary care after the randomisation. Why did the investigators not maintain the same number of telephone contacts with the control group? They could have discussed anything but chronic fatigue.

Thirdly, frequent early contacts with patients in the three intervention groups (and not the control group) might have confounded the outcome measures by positively influencing the results. This view is supported by the maximum difference emerging as early as three months among patients who had had the illness for an average of over four years, with little change thereafter. By speaking to the patients Powell et al might have provided them with a coping strategy that the control group could not access. Furthermore, did the authors ask the patients to keep an activity diary to record the intensity (mild/moderate) and duration (minutes/hours a day) of physical exercise so that they could note any difference across the intervention groups?

Because no objective measures of physical activity (for example, exercise endurance) were included before and after the interventions for assessing outcome in this study, the reported beneficial effects of graded physical exercise are based on weak evidence. Moreover, the authors did not use the current diagnostic criteria to select patients with chronic fatigue syndrome. Why are we reading this in the *BMJ*?

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1 Powell P, Bentall RP, Nye FJ, Edwards RHT. Randomised controlled trial of patient education to encourage graded exercise in chronic fatigue syndrome. *BMJ* 2001;322:387-90. (17 February.)

Authors' reply

Editor—As the design of clinical trials involves compromises between practical and methodological considerations there is no such thing as the perfect trial. We do not think that the compromises identified by Chaudhuri undermine our conclusions.

We used the 10 item physical functioning subscale of the SF-36 as our primary outcome measure, not a single item as he suggests. Previous research had shown it to be a valid indicator of the kind of change we believed was most important.[1] Changes in other outcome measures were reported, including subjective fatigue.

Despite our concern about type 1 error proliferation, significant group differences were observed on improvement scores at final follow up for the complete SF-36 ($F(4,142) = 12.66$, $P < 0.001$ with baseline scores as a covariate; Bonferroni comparisons between each treatment group and the control group $P < 0.001$). It would have been better to include objective measures of activity, but our findings can be discounted only by assuming that patients were massively inaccurate in recording their symptoms and functioning.

The case for a placebo control (involving an equal number of telephone contacts discussing "anything but chronic fatigue") is not clear cut. There is considerable disillusionment with the placebo concept in studies of psychological interventions.[2] So called active placebos, such as non-directive counselling, may share key ingredients with the target treatment. Other strategies for equalising therapists' time may be perceived as time wasting by patients.

Comparisons with standard treatment are more informative for healthcare managers because they give a realistic indication of what can be achieved when services introduce new treatments. Research into the effectiveness of psychological treatments therefore often begins with this kind of comparison and then proceeds to the examination of key ingredients in later studies. Mere contact with a therapist is unlikely to achieve substantial changes in a chronic illness; as we noted, other studies have not found clinical benefits from this.[3 4]

We hope that Chaudhuri read our study in the *BMJ* because our findings add to the now substantial evidence that psychological

treatments encouraging graded exercise benefit most patients with the chronic fatigue syndrome.[3–5]

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Higher dose inhaled steroids in childhood asthma

Conventional doses do have side effects

Editor—We disagree with Keeley's assertion that in childhood asthma "important adverse effects are rare" with inhaled corticosteroids at doses of ⩽400 μg/day (beclomethasone equivalent).[1] Growth suppression is the most important side effect of inhaled corticosteroids in childhood, and all three preparations licensed for use in children in the United Kingdom cause appreciable suppression at these doses. A recent meta-analysis reported growth suppression of 1.51 cm/year in children receiving beclomethasone and 0.42 cm/year for fluticasone,[2] and the child asthma management programme (CAMP) study showed growth suppression of 1.1 cm/year with budesonide.[3]

We agree with Keeley's suggestion that higher doses of inhaled corticosteroids than are currently advocated are needed to affect acute episodes, but we would advise caution. It is likely that the growth suppression is relatively short lived[3 4] and that growth thereafter readjusts to baseline. Consequently, repeated short courses of high dose inhaled corticosteroids could have a comparatively larger effect on growth than lower doses. It remains important to balance the beneficial and detrimental effects of any treatment in childhood.

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Why isn't titration advocated more often in delivery of inhaled drugs?

Editor—In his editorial Keeley has highlighted an anomaly that has puzzled me for years: why is titration not advocated more often in the delivery of inhaled drugs in acute asthma exacerbations?[1]

As Keeley points out, the delivery of inhaled treatment in a particular child (or adult) with acute asthma will be influenced by the delivery method used and by the degree of constriction in his or her airways during the exacerbation. It is logical to approach the uncertainty of response to treatment by giving frequent doses at short intervals and monitoring the response.

This technique has been used extensively in randomised trials to overcome the uncertain ratio of β agonist that is delivered to the airways (in contrast to the amount of drug emitted by the delivery device). Intervals between delivery have varied and range from 10 to 60 minutes. Equivalent benefits have been found with both nebulised delivery and metered dose inhaler with a spacer device (in the latter case using four to six separate actuations of the inhaler inhaled one at a time over a few minutes).[2] The trials have mostly been carried out in emergency departments, usually with the concurrent administration of oral steroids; the few patients who did not respond to repeated treatments after two hours were admitted to hospital.

Personal experience over the past two years has shown that such an approach can work well in primary care for inhaled β agonists in adults and children (for all but the most severe exacerbations). I now tend to use four puffs of a β agonist via metered dose inhaler given through a spacer device every 10 minutes; most exacerbations respond well within about half an hour. I routinely also use oral steroids for such attacks,[3] but I agree that we need a trial of a titrated approach for inhaled steroids in the community, as the best doses, intervals, and delivery methods for inhaled steroids are not yet clear.

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Competing interests: None declared.

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Thromboprophylaxis after replacement arthroplasty

Many surgeons prefer not to prescribe chemoprophylaxis after arthroplasty

Editor—Thomas confirms that intermittent calf compression reduces the rate of pulmonary embolism to 1% after replacement arthroplasty without having the possible side effects of chemoprophylaxis.[1] He goes on to state that when the efficacy of foot pumps was compared with that of anticoagulation "the results in terms of preventing deep venous thrombosis were comparable."

The rest of the editorial is aimed at supporting the use of anticoagulation in these patients. I presume that Thomas is neither an orthopaedic surgeon nor a patient who has had a failed joint replacement; if he was he would not regard an incidence of major bleeding with anticoagulation of 1% as being an "acceptable price to pay."

His conclusion that anticoagulation is the single most effective way of preventing these complications is not supported by the rest of his editorial. It seems incongruous that he is suggesting using a method of prophylaxis with a 1% rate of major complications to prevent a complication with the same incidence.

If this article is not retracted or a counter-argument published there is a risk that litigation lawyers will use it against the many orthopaedic surgeons who avoid chemoprophylaxis in patients undergoing arthroplasty.

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1 Thomas DP. Thromboprophylaxis after replacement arthroplasty. *BMJ* 2001;322:686-7. (24 March.)

Author's reply

Editor—Macdonald seems more concerned about the hazards of chemoprophylaxis than the dangers of thromboembolism. No effective drug treatment is without some risk, and I tried not to underplay the undoubted but quite small risks of anticoagulant treatment. Thromboembolism in patients undergoing replacement arthroplasty is a well recognised hazard, and the question is, where does the balance of risk lie?

I have no quarrel with the use of foot pumps while patients are in hospital, although data concerning their effectiveness are sparse. But does Macdonald send his patients home with foot pumps? Evidence increasingly shows that the risk of thromboembolism continues well after patients have usually left hospital.

As for retracting my argument, perhaps Macdonald should be more concerned about the risk of litigation that he runs from not using effective prophylaxis. A failed joint replacement is indeed a tragedy, but I suggest that it is less so than sudden death from pulmonary embolism. A recent American-Canadian consensus statement on preven-

tion of venous thromboembolism recommended only the use of warfarin or low molecular weight heparin as prophylaxis of choice after major joint replacement.[1]

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1 Geerts WH, Heit JA, Clagett GP, Pineo GF, Colwell CW, Anderson FA Jr, et al. Prevention of venous thromboembolism. *Chest* 2001;119 (suppl 1):132-75S.

Use of Dr is perhaps even more confusing in Germany than UK

Editor—Loudon and Crisp described the confusion surrounding the use of the title of Dr.[1 2] To confuse the issue more, I will describe what happens in Germany. Here the differentiation between the titles of Dr and Mr or Mrs is quite clear. Contrary to the United Kingdom, where the title tells us something about the working field of the physician, the German differentiation is fully based on having earned an academic MD title (German term: Dr med.) or not. Therefore, German doctors are desperately trying to earn an MD title to be called Dr by the patients, as being called just Mr or Mrs is a clear sign of inferiority. People who are Mr or Mrs usually do not reach the position of a head of department.

After having passed the preliminary medical exams, German medical students are allowed to start with their MD thesis. As the research interests and abilities vary from student to student, but each individual tries to earn an MD title, the quality of German MD theses varies widely.

Furthermore, as in Germany someone with a PhD is also called Dr (of natural sciences, for example), patients may confuse the profession of a person called Dr completely—for example, when encountering a doctor in chemistry but expecting a physician.

The disaster is compounded by the title of professor. This can be only be achieved after having completed a second thesis, called habilitation, which is comparable to a high quality PhD thesis, usually covering at least the authorship in 20 peer reviewed research papers. Having received the habilitation, your job title is initially that of a private lecturer, and after about four years of continuous research you are entitled to be called an associated professor. There are about 10 000 to 20 000 medical professors in Germany. Most of them are head of departments in peripheral hospitals and have stopped doing research.

Thus the British system is not as absurd as it seems if you compare it with that in other countries.

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2 Crisp AJ. Title of Dr should be sufficient for all doctors. *BMJ* 2001;322:617. (10 March.)

Job advertisements

Applicants are not always treated well

Editor—The article by Jones on job advertisements and descriptions reminded me why I am currently not practising as a clinician but running a market leading medical software business.[1]

I qualified in 1999 at University College London Medical School. After working as a junior house officer for six months I decided to defer my second house job to devote myself full time to my then start-up business. I intended to return to medicine as soon as feasible, particularly in a part time or job sharing position so that I could divide my commitment between medicine and business.

I noticed a job advertisement last summer in the *BMJ*'s classified supplement that seemed written for me: a good hospital, job sharing or part time working welcome, and in a convenient location. The advertisement invited applicants to contact the consultant for an informal chat. I was keen to find out about the rota (not mentioned in the advertisement) and more details about the job sharing. So I rang and spoke to the consultant's secretary, who told me that she only found out about the advertisement that morning as I was the 25th person ringing up for information. She took my number and said she would ring me back the next day. I took the initiative to ring her then. The secretary told me that she hadn't forgotten, but the consultants had yet to decide among themselves about the details and rota of the job and the consultant concerned would ring me the next day for our chat. I was excited again, but of course he did not ring. I decided to leave the secretary alone for a day, but the day after I rang her again. Things had changed, she said, and the consultant was not going to ring me. Instead, I should submit my curriculum vitae and I could find out about the job at my job interview if I was shortlisted.

What a way to treat prospective staff. I can understand the frustration and irritation caused by applicants sending in "third generation photocopies" of their CVs and not turning up for interviews, but I think that an important contributing factor is the way job applications are handled. I love medicine, but my initial attempt to get back into medicine has been so off-putting and unprofessional that for now I would rather stick with the challenges and job satisfaction of running an international business. If I tried to recruit or treat my staff in a similar manner I would probably be a one-man band.

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1 Jones O. Job advertisements and descriptions. *BMJ* 2001;322:S2. (24 March.)

Guessing games are fun

Editor—I enjoyed Jones's article on job advertisements and descriptions, but I think that in some ways it would be a pity if the current pattern of advertising was changed.[1] The *Weekend Australian* and the fortnightly *Medical Journal of Australia* provide endless amusement, as readers wonder exactly which town, "equidistant from Brisbane and Sydney," might justly be described as the "Athens of the North." Does a "distinctly seasonal" climate mean merely hot in summer and cold in winter, or is it a warning that one should be prepared to be snowed in?

My career as a locum provided local knowledge of the identifiable hospitals, which allowed me to translate the following commonly used terms (box).

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Commonly used terms, with translation

Challenging	Exhausting
Newly created	Newly vacated
Generally good support from other specialties	Interdepartmental war
Enjoys the support of the local community	There is at least one community representative on every committee and probably some at mortality and morbidity conferences
Interesting and varied case mix	Half the patients do not speak English
Multicultural environment	Neither do the staff
Committed to equal opportunity	Senior executive staff entirely male
Multidisciplinary team	Headed by a nurse or a social worker
Good communication skills	For lots of reports
Leadership qualities	Of Henry Kissinger
Recently refurbished department	Still too small