Letters

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Human population growth

Rich countries need education on resource conservation

EDITOR—"Because rich countries remain the main source of new knowledge and new technologies, responsibility for finding paths to sustainability rests mainly with them."[1] Well said.

Unfortunately, the rich countries, or at least the dominant sections in rich countries, have not yet learnt to view the world as a single whole. The world continues to be a space to be dominated. It used to be domination by conquering and colonising, now it is domination through unfair economic and trade agreements. National self interest takes precedence over global good. If corporations in the rich countries do things that exacerbate the divide between rich and poor and make sustainability increasingly difficult to attain, individuals in those countries add their bit by consuming scarce natural resources at an alarming rate. Development experts talk of the great importance of education in the poor countries. I think that educating the rich countries about the need to reduce consumption and conserve resources is equally, if not more, important. Without that it would be futile to expect the rich countries to find paths to sustainability.

Subbiah Arunachalam *distinguished fellow*
M S Swaminathan Research Foundation, Chennai 600 113, India
subbiah_a@yahoo.com

1 McMichael AJ, Powles JW. Human numbers, sustainability, and health. *BMJ* 1999:319:977-80. (9 October.)

Population issue is not entirely satisfactory

EDITOR—Whether the globe can accommodate 6 billion people, each for 60-70 years, without causing damage to ecosystems, is not a question of science but of distributive justice.

Probably we could feed, dress, shelter, and protect 6 billion people, provided that we liberalise all movement of capital goods, services, information, and people. Production and consumption would need to be taxed according to the environmental damage they cause. We live in a world of barriers—religious, ideological, administrative, economic, physical, and emotional—and demographic entrapment is an observable fact resulting from rapid population growth, which has been caused by disproportionate investment in reproductive success.

Most of the 6 billion have been conceived by copulation. What proportion of the 6 billion occasions have been premeditated to "make a baby" is unknown. It seems that in a proportion the baby has been an unwanted byproduct. This situation arose because the couple did not use effective contraception, because of ignorance, unavailability, or fear of consequences. Fear is nurtured by propaganda purporting that contraception is dangerous and by asserting that it is morally wrong.

A fecundity ethos may have been justifiable at the beginning of the agricultural revolution but has become counterproductive. The proponents of this ethos are usually men.

An influential group of such men resides in the Vatican, among them the Pontiff, whose opinion in matters of sexual morality is being preached in a manner that suggests that the papal belief is an integral part of Catholic creed. The Pontiff's appointees have successfully prevented, in most parts of the developing world, the dissemination of accurate information with regard to sexuality, have opposed the distribution of contraceptives, have destroyed contraceptives, and have made untruthful claims about them.

The influence of the Pontiff reaches far beyond Catholics. In the developed world papal influence does not assert itself in matters of sexual morality; there is a schism within the Catholic Church, and the victim is the South.

In the population issue the Catholic Church is mentioned in passing.[1] I do not believe that this was due to lack of insight. I expected the lion of Tavistock Square to roar. It has not. Could this have been so because of lack of courage?

Imre J P Loefler *surgeon*
Nairobi Hospital, PO Box 47964, Nairobi, Kenya

1 Overpopulation. Overconsumption. *BMJ* 1999;319. (9 October.)

China's one child family policy is changing

EDITOR—Kane and Choi's article on the one child family policy was interesting but failed to mention important recent developments in the implementation of this policy.[1]

From the middle of 1998 in 32 counties in 16 regions of China (with a population of around 20 million in all) quotas and targets for family size have been abolished completely. Education programmes and provision of more contraceptive choice have become the cornerstone of a voluntary policy.

This programme, which is supported by the United Nations Population Fund, is being piloted for four years. If the birth rate in these counties does not increase substantially, it will be extended to more counties. It is hoped that a "small family culture" has become accepted, as it has in Hong Kong, where the total fertility rate is 1.1 despite no limits on family size.

Another important relaxation of the policy has been in effect since 1996 in cities in eastern provinces: couples are allowed to have two children if both husband and wife are only children.[2] This is affecting the many only children now entering their childbearing years. It is important, since it will reduce the phenomenon known as 4-2-1, which means that one individual could be responsible for the care of four grandparents.

Incidentally, the caption for the photograph of Mao Ze Dong, "One is ideal, said Mao," could not be more wrong. He said exactly the opposite, as is pointed out in the article. He regarded human resources as China's greatest strength. He is at least partly responsible for causing the baby boom of the 1950s-60s, which led to the imposition of the one child family policy after his death.

Therese Hesketh *research fellow*
Centre for International Child Health, Institute of Child Health, London WC1N 1EH
t.hesketh@ich.ucl.ac.uk

Zhu Wei Xing *regional manager East Asia*
Health Unlimited, Prince Consort House, London SE1 7TS

1 Kane P, Choi CY. China's one child family policy. *BMJ* 1999;3:992-4. (9 October.)
2 Hesketh T, Zhu WX. The one child family policy: the good the bad and the ugly. *BMJ* 1997;314:1685-7.

Registering trials is not enough to counter perceived irrelevance of much research

EDITOR—Horton and Smith make a cogent case for requiring all clinical trials to be registered.[1] This is not, however, a complete solution to the underlying problem—the chaotic process by which randomised controlled trials come into being. It may prevent unnecessary duplication (as distinct from desirable replication), but it will not address the real problem of health service research and development—its seeming irrelevance to managers of NHS resources, such as clinicians.

The problem arises because those who control research think like scientists rather than resource managers. Traditionally, scientists decide their own research questions, with the peer group determining the allocation of funding to projects and being the main consumer of the end product (scientific papers). Practitioners and other resource managers, however, are expected to absorb and act on the wisdom handed down to them from systematic reviews and the like. The model is knowledge changes attitude and then attitude changes behaviour; it was discredited in health promotion when it was realised that altering people's behaviour is a far more complicated process.

Horton and Smith place the systematic collection of data from randomised controlled trials at the intellectual centre of NHS research and development. Databases, systematic review, and dissemination are not at the intellectual core but merely supporting technologies. To onlookers active in resource management those engaged in these technologies seem obsessed with the finer points of their trade (tracking down elusive work and the methods of meta-analysis) to the point of being ridiculous. Also, much of the wisdom dispensed has little bearing on the questions resource managers want answered: few reported trials encompass the Peckham extended trial protocol[2] and the wider humanistic issues (often expressed qualitatively) necessary for a balanced judgment.

People engaged in research wrongly think that they are being scientists when the epistemological basis of health service research is different, though no less rigorous. In our new book Jo Walsworth-Bell and I show what research and development really is.[3] We develop a new paradigm and show how this naturally engages resource managers, who are the consumers of the research and thus those who must direct its agenda. We also discuss how elements of the paradigm have been tested in the north west of England.

Selwyn St Leger *senior lecturer in public health medicine*
Biostatistics Group, School of Epidemiology and Health Sciences, University of Manchester, Manchester M13 9PT
Selwyn.St.Leger@man.ac.uk

1 Horton R, Smith R. Time to register randomised trials. *BMJ* 1999;319:865-6. (2 October.)
2 Peckham, M. Research and development for the National Health Service. *Lancet* 1991;338:367-71.
3 St Leger AS, Walsworth-Bell JP. *Change promoting research for health services*. Buckingham: Open University Press, 1999.

Coping with winter bed crises

Crises do not just happen in winter

EDITOR—Hanratty and Robinson have provided some valuable suggestions for coping with winter bed crises.[1] Unfortunately, however, the title and content serve to reinforce the myth that bed crises are seasonal. Certainly, the pressures are even greater during the winter months, but many accident and emergency departments are overwhelmed throughout the year, with prolonged delays while patients await admission. Recent initiatives have produced only a marginal benefit, and the pressures on medical and nursing staff are unrelenting. Privacy and dignity for patients are lost, and the quality of care inevitably falls.

The fundamental issue is a failure to accept the impact of the volume and casemix of emergency patients on a health service that is still oriented towards government targets for elective priorities. The situation is unlikely to alter greatly until the inexorable rise is recognised and addressed by providing adequate capacity in the acute trusts. Priorities will not change so long as chief executives live under the threat of penalties for failing to meet waiting list targets and there is little incentive or reward in delivering a safe and efficient emergency service. Change must be directed from the centre without delay. The political advantages of such a strategy would be immense.

John Heyworth *consultant*
Emergency Department, Southampton General Hospital, Southampton SO16 6YD

1 Hanratty B, Robinson M. Coping with winter bed crises. *BMJ* 1999;319:1511-2. (11 December.)

Essence of problem is insufficient resources

EDITOR—In their editorial Hanratty and Robinson note that plans for managing winter bed crises were not introduced until 1996.[1] They continue by suggesting ways in which such crises might more accurately be predicted but seem to have totally missed the point (obvious to anyone working in an acute specialty) that the crises are not primarily caused by excesses of patients but by shortages of beds and of nursing, medical, and ancilliary staff created in the years immediately up to 1996.

Predicting such crises, which would in turn allow elective work to be suspended to create space for emergency work, does not seem to me to be time or money well spent. The elective work still has to be done, and tactics such as these simply help one crisis—the shortage of emergency beds—by worsening another—the excessive time patients with painful and life threatening conditions have to wait for elective treatment. Rather than developing short term plans to rob Peter and pay Paul, I suggest that disease surveillance data should be used to highlight the fact that the NHS simply cannot cope with the demands made on it. I also believe that the implication that the service can be improved to a satisfactory point by predicting winter crises is dangerous because it distracts attention (particularly political attention, which always seems to be on the lookout for a cheap and easy fix) from the essence of the problem—insufficient resources.

Geoff Anderson *consultant orthopaedic surgeon*
Derriford Hospital, Plymouth PL6 8DH
Andersons@longbarn0398.freeserve.co.uk

1 Hanratty B, Robinson M. Coping with winter bed crises. *BMJ* 1999;319:1511-2. (11 December.)

Answer is not data showing crises will happen

EDITOR—So Hanratty and Robinson think that the way to cope with the winter bed crises is increased surveillance.[1] I can hear the hollow laughter from accident and emergency staff up and down the country. This is like an intelligence officer in a war zone telling the troops that they are about to be plastered with high energy explosives and afterwards saying while surveying the chaos, "There you are, we said it would happen."

During one weekend in December, apart from reading this article, I received a huge book about how to use NHS Direct (endorsed by the BMA) and I have been called into the accident and emergency departments that I cover. One had 29 patients waiting for beds, the other 12.

The answer to bed crises is not data to tell us they are going to happen. We cannot pull extra beds, doctors, and nurses out of a cupboard when we are told a crisis is going to happen. The solution is not to expect hospitals to run on over 100% bed occupancy to break even financially. Let us not waste any more money on data collection and an unproved telephone advice service and give it to the places where the hard slogging foot soldiers are up to their ears in mud and gore. Data may solve the problem in 10 years' time, but patients with emergency illnesses need the help now.

John Belstead *consultant in accident and emergency medicine*
Ashford and St Peter's NHS Trust, Chertsey, Surrey KT16 OPZ
John_Belstead@compuserve.com

1 Hanratty B, Robinson M. Coping with winter bed crises. *BMJ* 1999;319:1511-2. (11 December.)

Prospective risk of stillbirth

Study's results are flawed by reliance on cumulative prospective risk

EDITOR—In their study on the prospective risk of unexplained stillbirth in singleton pregnancies at term Cotzias et al performed a secondary analysis[1] of data that we published.[2] The proposed rationale for this mathematical exercise was that no published data provide accurate gestation-specific risks of stillbirth at term. In fact, our publication evaluates risks of stillbirth and neonatal and infant mortality throughout pregnancy.[2]

The original data for the North East Thames region, with the addition of births for 1992, were used to inform the confidential inquiry into stillbirths and deaths in infancy (CESDI) concerning antepartum stillbirths (L Hilder and N Datta, unpublished data). When both fetal and neonatal causes cited on stillbirth registrations were used the proportion of stillbirths that are unexplained increased from 0.1392 at 37 weeks to 0.5000 at 43 weeks. Even if stillbirth is explainable it is not necessarily preventable and is inevitably unexpected. We acknowledge that when doctors deal with parents who have had a recent stillbirth,

Prospective risk of stillbirth by week of gestation for singleton pregnancies in North East Thames region, 1989-91

Gestation (weeks)	No of ongoing pregnancies	No of stillbirths	Risk of stillbirth/1000 ongoing pregnancies (95% CI)	Risk of stillbirth in ensuing week
35	161 638	48	0.30 (0.23 to 0.37)	1:3332
36	159 723	62	0.39 (0.31 to 0.46)	1:2536
37	155 791	47	0.30 (0.23 to 0.37)	1:3332
38	147 631	77	0.52 (0.44 to 0.60)	1:1922
39	126 448	62	0.49 (0.40 to 0.58)	1:2039
40	93 539	81	0.87 (0.80 to 0.96)	1:1148
41	39 245	50	1.27 (0.94 to 1.60)	1:786
42	10 305	16	1.55 (0.93 to 2.78)	1:644
43	1 874	4	2.13 (0.28 to 3.99)	1:486

information on aetiology is invaluable. We continue to believe, however, that when prospective risks are being estimated for clinical purposes all stillbirths should be included.

The results presented by the authors are critically flawed by the reliance on cumulative prospective risk of stillbirth. The authors total the number of stillbirths in the remaining weeks of pregnancy in order to estimate the prospective risk of stillbirth at a specific week of gestation. This methodology produces clinically implausible results, explaining the authors' paradoxical conclusion that the risk of stillbirth at 38 weeks is greater than that at 42 weeks. If this was taken to absurdity their prospective risk of stillbirth at 24 weeks would be 1 in 330 while that at 43 weeks would be 1 in 633.

We analysed data from 158 945 singleton pregnancies in the North East Thames region in which the fetuses were congenitally normal (table). Our data give clinically relevant estimates of prospective weekly risk of stillbirth, showing a sharp increase in risk after 40 weeks. Current strategies of elective induction of labour after 41 weeks seek to avert fetal death without increasing rates of obstetric intervention. Until large enough trials are performed to justify this course of management the correct interpretation of observational data from large population based analysis is vital.

Lisa Hilder *lecturer*
Department of Environmental and Preventative Medicine, Wolfson Institute of Preventive Medicine, London EC1M 6BQ

Kate Costeloe *professor*
Academic Department of Child Health, St. Bartholomew's and the Royal London School of Medicine and Dentistry, London E1 4NS

Baskaran Thilaganathan *director*
Fetal Medicine Unit, St George's Hospital Medical School, London SW17 0RE
basky@pobox.com

1 Cotzias CS, Paterson-Brown S, Fisk NM. Prospective risk of unexplained stillbirth in singleton pregnancies at term: population based analysis. *BMJ* 1999;319:287-8. (31 July.)
2 Hilder L, Costeloe K, Thilaganathan B. Prolonged pregnancy: evaluating gestation-specific risks of fetal and infant mortality. *Br J Obstet Gynaecol* 1998;105:169-73.

Randomised trials of earlier induction of labour are needed

Editor—Cotzias et al report the risk of unexpected stillbirth as a function of increasing gestational age.[1] We would like to comment on the methodology of this risk estimation.

Fetal mortality can be expressed by two measures. It can be expressed as a risk (or cumulative incidence), defined as the number of stillbirths diagnosed at or beyond a specific week divided by the number of ongoing pregnancies at the beginning of that week. Alternatively it can be expressed as a rate, defined as the number of stillbirths diagnosed at a specific week divided by the number of ongoing pregnancies at mid-week (an approximation of person time at risk).

Cotzias et al point out that the risk of stillbirth is similar across gestational weeks at term. In an earlier paper based on the same database, results were expressed as rates, increasing with gestational age.[2] Although the risk of death is similar across gestational age, the period during which the fetus is at risk is clearly much longer at 35 weeks (about five weeks) or 38 weeks (about two weeks) than at 42 weeks (only a few days). The figure shows Cotzias et al's data presented as risks and as rates.

In their comments in their electronic response about Cotzias et al's paper [3] [see next letter] Yudkin and Redman refer to their own study, where the risk during the next two weeks increased with gestational age.[4] This shows that, in the interpretation of risk estimates, the period for which the risk is present should not be ignored. Reporting fetal mortality as cumulative incidence is misleading if readers are not told the length of the period at risk. The clinician caring for a pregnant woman, and the woman herself, balance the risk of stillbirth with the probability of spontaneous onset of labour during a specific period (for example, until the next visit). The clinically relevant measure is either the rate or the risk of stillbirth during a limited period.

Cotzias et al state that at 38 weeks the risks of stillbirth near term exceed those at 42 weeks. Obstetricians may be tempted to induce labour at 38 weeks on the basis of these conclusions. This policy is not based on scientific evidence. In contrast, results of randomised trials suggest a reduction in perinatal mortality when labour is induced at 41 weeks, a time when the stillbirth rate increases.[5] Until randomised trials of earlier induction of labour or of specific tests show a reduction in perinatal mortality, we will continue to believe that gestation should continue until spontaneous onset of labour up to 41 weeks.

Michel Boulvain *epidemiologist*
michel.boulvain@hcuge.ch

Daniel Faltin *senior registrar*
Victoria Ibecheole *registrar*
Olivier Irion *senior lecturer*
Unité de Développement en Obstétrique, Hôpitaux Universitaires de Genève, CH-1211 Geneva, Switzerland

1 Cotzias CS, Paterson-Brown S, Fisk NM. Prospective risk of unexplained stillbirth in singleton pregnancies at term: population based analysis. *BMJ* 1999;319:287-8. (31 July.)
2 Hilder L, Costeloe K, Thilaganathan B. Prolonged pregnancy: evaluating gestation-specific risks of fetal and infant mortality. *Br J Obstet Gynaecol* 1998;105:169-73.
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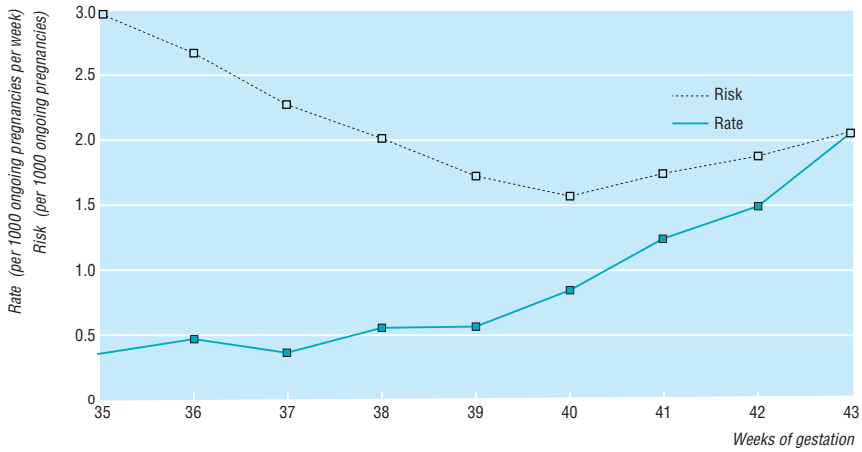
4 Yudkin PL, Wood L, Redman CWG. Risk of unexplained stillbirth at different gestational ages. *Lancet* 1987;i:1192-4.
5 Hannah ME, Hannah WJ, Hellmann J, Hewson S, Milner R, Willan A. Induction of labor as compared with serial antenatal monitoring in post-term pregnancy. A randomized controlled trial. *N Engl J Med* 1992;326:1587-92.

Impending fetal death must be identified and pre-empted

Editor—Cotzias et al estimated that the prospective risk of stillbirth at 38 weeks



Risk and rate of stillbirth according to gestational week (data from study by Cotzias et al)

(1 in 529 ongoing pregnancies) was greater than at 42 weeks (1 in 565).[1] On the basis of this finding, they question whether routine delivery should be considered at 38 weeks, rather than at 42 weeks as is usually recommended.

This approach makes sense only if the costs of delivery are equal at each gestational age. But, as the authors imply, this is not the case. Not only would one expect a higher incidence of caesarean section and iatrogenic prematurity at earlier gestational ages, but important non-clinical outcomes, such as maternal satisfaction, would differ too. Routine induction as early as 38 weeks would be unacceptable to most women. Although the risk of prospective stillbirth at a given gestation represents the greatest cost of allowing the pregnancy to continue, a knowledge of all the costs involved, including those associated with delivery at each gestation, is needed to inform a policy of routine early delivery.

The alternative form of management is to improve fetal monitoring so that impending fetal death can be identified and pre-empted. In our study of births to Oxfordshire residents at the John Radcliffe Hospital in 1978-85 we calculated the risk not of prospective stillbirth at any future gestation but of impending stillbirth—that is, stillbirth occurring in the next two weeks.[2]

The risk of unexplained impending stillbirth was very low until 40 weeks (0.2-0.4/1000 ongoing pregnancies) and increased to 1.2/1000 at 41 weeks and beyond. These results set in context the difficulty of identifying the rare impending fetal death and also (subject to cost considerations) support a policy of increased surveillance at and beyond 41 weeks. A recent update of this analysis, based on 44 450 singleton births in 1991-8, shows a similar pattern, although the peak level of risk, of 1.3/1000, is delayed until 42 weeks.

Our updated analysis suggests, contrary to the assumption of Cotzias et al, that the proportion of total stillbirths that are unexplained stillbirths is not constant between 35 and 42 weeks but rising, being highest at and after 39 weeks. Applying these changing proportions to the data of Cotzias et al would support the view that, contrary to their claim, the prospective risk of unexplained stillbirth is greatest in post-term pregnancies.

Patricia Yudkin *research lecturer*
Department of Primary Health Care, Institute of Health Sciences, Oxford OX3 7LF
pat.yudkin@public-health.oxford.ac.uk

Christopher Redman *professor*
Nuffield Department of Obstetrics and Gynaecology, John Radcliffe Hospital, Oxford OX3 9DU

1 Cotzias CS, Paterson-Brown S, Fisk NM. Prospective risk of unexplained stillbirth in singleton pregnancies at term: population based analysis. *BMJ* 1999:319:287-8. (31 July.)
2 Yudkin PL, Wood L, Redman CWG. Risk of unexplained stillbirth at different gestational ages. *Lancet* 1987;i:1192-4.

Authors' reply

EDITOR—We are surprised by Hilder et al's comments, as we invited collaboration and discussed initially the study design and later the results with their corresponding author. We are unable to understand why they view our analysis as critically flawed: of course the prospective risk of stillbirth is higher at 24 weeks than at any other subsequent gestation, because there is a longer period during which stillbirth can occur. We expressed the risk of stillbirth for the remainder of the pregnancy, whereas Hilder et al have expressed the risk as a rate over the next week[1] and Yudkin et al over the next fortnight.[2]

The fact that our figures suggest broadly comparable risks per pregnancy at 38 weeks and at 42 weeks is entirely consistent with the observation that the risk per day or week at 42 weeks is many times higher than that at 38 weeks. This issue of risk versus rate per week or fortnight is illustrated in Boulvain et al's graph. In our view what matters to women at term is the overall chance of their baby dying, not the chance in the next day, week, or fortnight. The point of our analysis was to inform women and obstetricians what the chance of a stillbirth was once fetal maturity was achieved.

Yudkin and Redman observe that the percentage of unexplained stillbirths increases with gestation. As we argued in the short report, however, the more relevant risk is that of unexpected, rather than unexplained, stillbirth, which in our view includes all stillbirths after 38 weeks. We agree that routine delivery at term would have cost implications. Obviating these risks, however, should be seen in the context of changing obstetric risk:benefit ratios and the wider debate on optimal timing and mode of delivery.

Yudkin and Redman have suggested the obvious alternative of improving fetal monitoring techniques. There are currently no data to support routine monitoring from 38 weeks, just as after 41 weeks meta-analysis clearly shows expectant management with fetal monitoring is associated with a higher stillbirth rate than is elective delivery.[3]

Nicholas M Fisk *professor*
Christina S Cotzias *research registrar*
Sara Paterson-Brown *consultant*
s.paterson-brown@rpms.ac.uk
Department of Maternal and Fetal Medicine, Institute of Obstetrics and Gynaecology, Imperial College School of Medicine, Queen Charlotte's and Chelsea Hospital, London W6 0XG

1 Hilder L, Costeloe K, Thilaganathan B. Prolonged pregnancy: evaluating gestation-specific risks of fetal and infant mortality. *Br J Obstet Gynaecol* 1998;105:169-73.
2 Yudkin PL, Wood L, Redman CWG. Risk of unexplained stillbirth at different gestational ages. *Lancet* 1987;i:1192-4.
3 Crowley P. Elective induction of labour at 41 + weeks gestation. In: Keirse MJNC, Renfrew MJ, Neilson JP, Crowther C, eds. Pregnancy and childbirth module. In: Cochrane Collaboration. *Cochrane library*. Issue 2. Oxford: Update Software, 1995. (Revised May 1994.)

Treatment of venous leg ulcers

Nice study, pity about the sample size

EDITOR—Dale et al based their calculation of the sample size (200 patients) on the minimum size required to find the large effect observed in a previous very small study.[1] The confidence interval for this large effect would have been very large.

A better strategy would have been to decide on a detectable difference that would be clinically significant and design the study to be capable of detecting this difference. The difference that they found (64% healing with pentoxifylline *v* 53% healing with placebo) would probably be clinically significant in view of the high material and labour cost of continuing treatment with pressure bandaging and the unpleasantness of leg ulcers.

The study described only had a 30% power to detect this magnitude of difference. To have an 80% power to detect this difference would require a study with 332 in each group. We need larger groupings than single hospital clinics for research into conditions like this. The rapidly developing primary care research networks are ideal structures for research into common conditions that are usually managed in the community.

Frank Dobbs *senior lecturer in primary health care*
University of Plymouth, Plymouth PL4 8AA

1 Dale JJ, Ruckley CV, Harper DR, Gibson B, Nelson EA, Prescott RJ. Randomised, double blind placebo controlled trial of pentoxifylline in the treatment of venous leg ulcers. *BMJ* 1999;319:875-8. (2 October.)

Authors' reply

EDITOR—We note with interest Dobbs's comments on our trial and agree that primary care research networks are promising vehicles with which to conduct trials of conditions normally managed in the community. We are, however, unconvinced by his comments concerning the trial size in relation to our particular study.

Our trial was of factorial design in order to compare three types of treatment within a single trial, in order to derive the maximum amount of data from the minimum number of patients. The treatments compared were: two types of dressing; two types of bandage; and a pharmacological treatment (pentoxifylline) against placebo.[1] The paper under discussion presented only the pharmacological data.

Trial size is always a compromise between sophistication of design, cost, and the difference we would like to detect. Patients with leg ulcers constitute a highly heterogeneous group. The recruitment of large numbers of adequately assessed patients is very difficult. Our trial size and 20% margin of benefit were carefully derived from the known outcomes of alternative treatments, such as bandaging regimens.

The trial required precise definition of patient groups by means of detailed patient screening including vascular laboratory

tests and duplex scanning. Altogether 525 patients were screened to arrive at our study population of 200. Also required were precise standardisation of treatment methods by specialist nurses and tight day to day supervision of the protocol. The cost of an equivalent trial in 634 patients conducted in the community as suggested by Dobbs would have been enormous, and funding would have been unlikely, even by a pharmaceutical company. Furthermore, if such a large number of patients were required to demonstrate a benefit it would raise serious questions about the cost effectiveness of such a treatment in this particular group of patients.

C V Ruckley *emeritus professor of vascular surgery*
R J Prescott *director, medical statistics*
J J Dale *consultant nurse specialist*
Vascular Surgery Office, Department of Surgery, University of Edinburgh, Royal Infirmary of Edinburgh, Edinburgh EH3 9YW

1 Prescott RJ, Nelson EA, Dale JJ, Harper DR, Ruckley CV. Design of randomised controlled trials in the treatment of leg ulcers: more answers with fewer patients. *Phlebology* 199;13:107-12.

Stages of change model for smoking prevention and cessation in schools

Authors applied adult dose for smoking to adolescents when smoking behaviour is different in the two

Editor—My colleagues and I have read the article by Aveyard et al on smoking prevention and cessation in schools, which examines the use of computer delivered expert system interventions that we have developed.[1]

For unreported reasons, Aveyard et al applied our adult dose for smoking to an adolescent population. In our standard adult protocol we provide three expert system interventions over six to 12 months. Aveyard et al provided three expert system interventions to adolescents over a comparable period of time. Our behaviour change protocol for adolescent populations calls for six to eight expert system interventions over two academic years. One of the reasons our treatment with adolescents is at least twice as long, with more expert system interventions, is that smoking increases over a two year period with adolescents, whereas it decreases with adults. Why would Aveyard et al expect an adult dose for smoking to be effective with adolescents? I know of no evidence, and Aveyard et al provide no evidence or rationale, for applying our adult dose of expert systems to adolescents.

We will soon be reporting on the important pattern of results that were produced when our two year adolescent protocol was applied to an adolescent population.

James O Prochaska *director*
Cancer Prevention Research Center, University of Rhode Island, Kingston, RI 02881, USA
jop@uri.edu

Competing interests: I am one of the developers of the expert system under discussion. The expert systems for smoking are owned by the University of Rhode Island and are licensed to Johnson and Johnson Health Services. A sublicence has been developed with Nelson Communications in the United States and Public Management Associates for commercial use in the United Kingdom. Pro-Change Behavior Systems, LLC, of which I am a principal, provides research and development services to these two companies.

1 Aveyard P, Cheng KK, Almond J, Sherratt E, Lancashire R, Lawrence T, et al. Cluster randomised controlled trial of expert system based on the transtheoretical ("stages of change") model for smoking prevention and cessation in schools. *BMJ* 1999;319: 948-53. (9 October.)

Authors' reply

Editor—Prochaska implies that there is a well known adult dose and an adolescent dose of the expert system. The only evidence on how many doses of the expert system should be used is from a trial in adults, and that evidence suggests that one is enough.[1] Prochaska and colleagues' only other published study on the transtheoretical model expert system in adolescents used three sessions.[2] There is no evidence on which to base a decision about how many sessions adolescents might need.

Prochaska advances the argument that because the prevalence of smoking in the group of adolescents is changing rapidly the individuals in that group are less susceptible to change by the intervention and need more sessions to achieve the effect that adults would achieve with fewer sessions. This does not follow. Our data show that 37% of adolescents who smoked regularly were preparing to stop smoking, compared with the 20% that is typical in adult populations.[3] Such individuals are more likely to have quit at one year than individuals in earlier stages of change.[4] This reflects itself in the high quit rates achieved by both intervention and control groups (more than 25% at one year). On this basis, it seems more likely that the expert system for adolescents, the only one that can be compared with the system for adults, should be more successful and require fewer sessions, yet we found no effect.

A better explanation for the failure of the intervention is one advanced by Reid.[5] Teenage smokers have a variety of smoking histories and do not construe their behaviour in the same way that questionnaires do. Perhaps the concept of being a regular smoker is foreign to most young teenage smokers, as is the idea of needing to go through a process to stop smoking, so the expert system's messages were lost on them.

Paul Aveyard *lecturer in public health medicine*
p.n.aveyard@bham.ac.uk

K K Cheng *professor of epidemiology*
Terry Lawrence *senior health development adviser*
Department of Public Health and Epidemiology, University of Birmingham, Birmingham B15 2TT

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Medicine to serve an ageing society

Retired doctors could have a role

Editor—With reference to Tonks's editorial about medicine in an ageing society,[1] it is surprising that there was no discussion of the possibility of recruiting retired doctors to provide direct advice and supervision to sick elderly people.

They will have had wide experience and so provide the "generalist" element required. They will have lived through the same events as their patients and been moulded by them enabling the genuine empathy of a cohort comradeship. They will be experiencing the progressive changes in outlook and capacity conferred or imposed by advancing years. They will eschew mindless and unrewarding over investigation for investigation's sake. They will also have been subjected to the automatic ageism of compulsory retirement, and the automatic condemnation of being out of date and "past it." If the NHS employers had any sense and were prepared to jettison their bias, they might just be able to persuade some veterans to help them out.

G H Hall *retired physician*
Ballamount, Broadclyst, Devon EX5 3BJ
harry.hall@which.net

1 Tonks A. Medicine must change to serve an ageing society. *BMJ* 1999;319:1450-1. (4 December.)

Profile of elderly people in society needs to be raised

Editor—I read Tonks's editorial with interest.[1] I agree that a higher profile within the undergraduate curriculum for geriatric medicine and a period of general professional training spent within geriatric medicine would improve doctors' understanding of both the presentation and the impact of diseases in elderly people.[2] My own career choice of geriatric medicine was influenced by the positive experiences of the specialty that I experienced at medical school and as a senior house officer.[3] I believe, however, that the underlying problem of ageism in the health service is more complex than this.

Elderly people have a low profile in a modern society that has become obsessed

with youthful images. Our tendency as a profession to label those over 65 years as a homogeneous group without recognising the tremendous diversity within that population does not help. The promotion of more positive images of elderly people such as the activities of "the university of the third age" would help to combat the inherent ageism that is present in our society. This would improve the attitudes of our future doctors even before their arrival at medical school. Only when elderly people are universally recognised as equals in our society can ageism be eradicated.

Susie Orme *senior registrar*
Royal Hallamshire Hospital, Glossop Road, Sheffield S10 2JF
adamanderson@compuserve.com

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Removing barriers to career development in public health

EDITOR—The white paper *Saving Lives: Our Healthier Nation* offers, for the first time, possibilities of careers in public health without glass ceilings. In other words, bright graduates will, at last, one day be able to choose public health as a career without having to study clinical medicine for six years first.

Many of us who have spent a lifetime teaching doctors how to be better public health physicians find the remarks of the BMA and the Joint Consultants Committee a little disingenuous.[1] It is due, in some important respect, to people like us that Ian Bogle can claim that the special training provided for public health physicians makes them particularly appropriate choices for director of public health posts. We agree with him.

Meanwhile, the training opportunities for people qualified in other core component disciplines in public health are, as yet, meagre and undeveloped. Their career structures are essentially non-existent. This claim of the BMA is the obvious next argument (of last resort?) after decades of effective medical protectionism. But it really is rather tedious.

Surely the debate (in the BMA and elsewhere) must now be, in the interests of public health, how can we enable the best people to enter the profession without glass ceilings and other unnecessary barriers to career development? At the moment the only chance of reaching the top of the profession is in the academic sector, which is too restrictive.

Public health is, after all, very different from clinical medicine in practice. It is about the health of populations, and it is not evident that studying the illness of individuals is necessarily the best initial training.

Klim McPherson *professor of public health epidemiology*
Cancer and Public Health Unit, London School of Hygiene and Tropical Medicine, London WC1E 7HT
klim.mcpherson@lshtm.ac.uk

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Crinkly toenails

Toenail onychomycosis can cause serious problems

EDITOR—We have read with interest and dismay the flurry of correspondence from general practitioners sent in response to your articles concerning the treatment of toenail onychomycosis.[1 2] Toenail onychomycosis seems to be regarded by many as a purely cosmetic problem and relegated to causing no more distress to the patient than a crinkly nail.[2–4]

The letters raise two important issues: firstly, the need to highlight the subsequent possible sequelae from leaving toenail onychomycosis untreated; and, secondly, the ongoing conflict between meeting this year's drug budget and the eventual long term costs to the NHS. Evans mentions that toenail onychomycosis is important in patients with peripheral vascular disease and increases the risks of conditions such as cellulitis or erysipelas of the lower leg.[5]

Toenail onychomycosis is very important in two groups of the population. The first group comprises young adults with physically active jobs or a keen interest in sport and professional sports people, who will be at risk of physical distortion of their toenails in the presence of onychomycosis. This leads to the frequent occurrence of ingrowing nails, with associated pain, risk of secondary infection, need for operations, and associated time away from work.

The second large group are elderly people. Onychomycosis has been shown to complicate up to 50% of cases of onychogryphosis. Most of these patients recall having dystrophic toenails for many years before the development of onychogryphosis, which implies that onychomycosis may have a causative role.

The effect of onychogryphosis on the quality of life of the elderly population should not be underestimated with respect to pain, decreased mobility, and increased dependence. The cost to the NHS must also not be underestimated because of the costs incurred from podiatry, adapted shoes, and transport for less mobile patients. There are many causes for toenails becoming dystrophic or discoloured, and treatment should be reserved for those nails with a proved fungal infection. Toenail clippings are, however, often inadequate, and scrapings from beneath the toenails or from the nailbed yield a higher rate of fungal culture.

A significant minority of cases of onychomycosis will still have negative mycological culture but can be diagnosed clinically and proved by other methods. Untreated onychomycosis of the toenail can have a great impact on a patient's quality of life and cause many expensive long term sequelae. It is vital that doctors look beyond the current drug budget to the future consequences of ignoring their patient's crinkly nails.

Natalie Stone *dermatology specialist registrar*
Rodney Dawber *dermatology consultant*
Churchill Hospital, Oxford, OX3 7LJ

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BMJ staff may not be representative

EDITOR—Does the high incidence of crinkly toenails among *BMJ* staff[1] represent a true reflection of the population's level of affliction, or is it just that the *BMJ* staff do not practise "safe sox?"

Justine Nanson *cardiac fellow*
Anaesthetic Department, Southampton General Hospital, Southampton SO16 6YD

1 Müllner M. Roughly quarter of BMJ staff surveyed said they had crinkly toenails. *BMJ* 1999;319:1197. (30 October.)