Letters

Methods to identify increased risk of coronary disease in the general population

Conclusion is oversimplification

EDITOR—Wilson et al assert that measuring cholesterol concentration only in people of at least 50 efficiently identifies those at high risk of coronary heart disease.[1] This oversimplification results from a study with important defects.

Firstly, Wilson et al determined absolute risk of coronary heart disease by using the Sheffield tables and the underlying Framingham algorithm. However, in the German prospective cardiovascular Münster (PROCAM) study and the Augsburg cohort of the World Health Organization's monitoring trends and determinants in cardiovascular disease (MONICA) study, Framingham overestimated coronary risk about twofold.[2] Even allowing for the higher incidence of coronary heart disease in Britain compared with Germany,[3] Wilson et al should have calculated risk by using either a British algorithm or a corrected Framingham formula.

Secondly, Wilson et al considered men and women together, even though risk of coronary heart disease in women is two to four times less than in age matched men aged up to about the age of 60. This error dilutes male risk of coronary heart disease, which is substantial even in early middle age. In the PROCAM study, for example, no fewer than 8% of men under 50 had a 10 year risk of coronary heart disease of more than 15% with the Framingham formula.

A particular aim in preventing coronary heart disease is to stop the early events that produce an especially high burden of morbidity and cost. Moreover, since the risk increases sharply with age, patients at borderline risk below the age of 50 require attention if they are not to proceed inexorably into a high risk group. Limiting cholesterol measurements to the over 50s will increase costs and disease burden.

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1 Wilson S, Johnston A, Robson J, Poulter N, Collier D, Feder G, et al. Comparison of methods to identify individuals at increased risk of coronary disease from the general population. *BMJ* 2003;326:1436-8. (28 June.)

2 Hense HW, Schulte H, Lowel H, Assmann G, Keil U. Framingham risk function overestimates risk of coronary heart disease in men and women from Germany—results from the MONICA Augsburg and the PROCAM cohorts. *Eur Heart J* 2003;24:937-45.

3 Conroy RM, Pyörälä K, Fitzgerald AP, Sans S, Menotti A, De Backer G, et al. Estimation of ten-year risk of fatal cardiovascular disease in Europe: the SCORE project. *Eur Heart J* 2003;24:987-1003.

Authors' reply

EDITOR—Assmann et al have misunderstood the pragmatic question our study answered. In the United Kingdom treatment decisions for the primary prevention of cardiovascular disease are based on the Framingham 10 year coronary risk equation.[1] Measuring the cholesterol of the entire population is not currently recommended.[1] Given that the NHS has a finite budget we asked how general practitioners can target cholesterol measurement, and hence accurate risk assessment, to those people most likely to benefit from drug treatment to reduce their chance of a stroke or heart attack.

We chose to combine the results of our analyses for men and women because, despite sex differences in risk, the optimum age cut-off point to maximise both sensitivity and specificity is similar for both sexes. Our strategy of screening everyone over the age of 50 identified 91% of men and 98% of women at 15% or more 10 year coronary risk.

Finally, Assmann et al are concerned about people under 50 being neglected because they are not offered a cholesterol test. Although a clinician may not measure cholesterol routinely, this does not imply that they ignore other important risk factors, such as smoking, obesity, and raised blood pressure.

Our study has shown that routine cholesterol measurement, and hence accurate risk assessment, in everyone aged 50 and over is a simple and efficient method of identifying people at high risk of coronary disease in the general population.

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Competing interests: None declared.

1 Department of Health. *National service framework for coronary heart disease.* London, Department of Health, 2000.

What becomes of junior doctors in non-training positions?

EDITOR—There is an increase in the number of vacancies advertised for non-training positions for junior doctors variously classified as clinical fellows, trust doctors, etc. Increasing staffing is just one strategy used by NHS trusts to reduce junior doctors' working hours.

My questions are: Who fills these vacancies? What becomes of them?

Higher specialty training distinguishes clearly between training and non-training positions. Requirements for specialty examinations are met only in positions recognised for higher specialty training. Non-trainees are without the respect, training, or recourse to complaint that trainees have. They have to be very productive (and lucky) to have any chance of progressing properly.

However, insufficient training positions exist to accommodate all non-trainees, raising the standard for entry into higher specialty training (another bonus?). Most non-training vacancies are filled by foreign doctors trying to get a foot in the door and take it from there. Few doctors who have received career advice and intend to specialise spend time in these positions—at least not for very long.

Prospective employees should identify their career requirements and ensure that a vacancy will help in meeting their objectives. Unfortunately, the need to remain in continuous employment or avoid getting into trouble with the Home Office may lead to the taking up of unsuitable employment.

I do not claim to have a solution. The national shortage of junior doctors seems likely to produce a class of doctors in non-training positions who will not be able to progress, and I wonder what is expected to become of them.

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Are clinical negligence and legal action related?

EDITOR—The chief medical officer's report *Making Amends* is welcome if only because it recognises that the system of compensation for clinical negligence is in need of radical reform.[1 2] Legal action is expensive, inefficient, and involves delay. However, the report does not address the system's central failure: the relation between negligence and legal action is unclear. Most cases of alleged negligence are unsustainable, most cases of actual negligence are uncompensated. The failings of the system are due to the faults of legal aid, which funds most cases but is available only to a small section of the public.

Legal aid lacks independence, fairness, and accountability. It lacks independence because funding is granted on the advice of the applicant's lawyer—a clear conflict of interest. It lacks fairness because a blameless health service defendant cannot recover legal costs. It lacks accountability because the reasons for decisions involving public funding are by law confidential and privileged.

All patients injured by negligent treatment are rightly entitled to compensation; there must be affordable access to justice. Instead of proposing reform of access to justice so that deserving cases are compensated properly and inappropriate cases are not advanced, the report suggests impractical alternatives to the tort system. It avoids critical consideration of legal aid and so does not confront powerful legal vested interests. Moreover, there is no reason why doctors should not be accountable in court for their negligence, as are other professions.

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Competing interests: AB is a solicitor practising in pharmaceutical product liability and clinical negligence.

1 Department of Health. *Making amends*. London: DoH, 2003. www.doh.gov.uk/makingamends (accessed 28 Aug 2003).
2 Dyer C. NHS staff should inform patients of negligent acts. *BMJ* 2003;327:7. (5 July.)

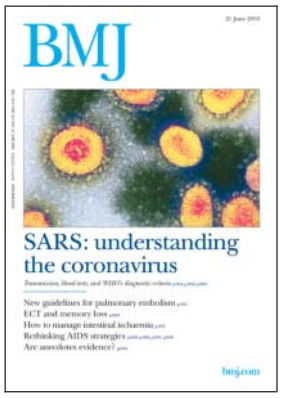
SARS: understanding the coronavirus

Accuracy of WHO criteria was similar in a "non-SARS" hospital in Singapore

EDITOR—We report a similar experience at the National University Hospital, Singapore, to that of Rainer et al in Hong Kong's New Territories.[1]

We screened patients for severe acute respiratory syndrome (SARS) by using criteria from the World Health Organization and transferred suspect and probable cases to Tan Tock Seng Hospital, the country's designated SARS hospital, for further management. Patients with undifferentiated fever or respiratory symptoms who did not meet WHO criteria (and would not be accepted at Tan Tock Seng Hospital due to the limited resources available) were monitored in isolation rooms with daily blood counts, chest radiography, and temperature monitoring every four hours without antipyretic drugs until an alternative diagnosis was established.

From 17 March to 16 May 2003 we isolated 909 patients and transferred 47 patients directly to Tan Tock Seng Hospital for evaluation (table). Thirteen of the 18 patients with SARS treated at our hospital did not initially meet the WHO criteria. WHO criteria when applied at presentation had a sensitivity of 27.8% (95% confidence interval 9.7% to 53.5%) and a specificity of 95.5% (94.0% to 96.8%).



We found a much lower positive predictive value (10.6%) than Rainer et al, as expected given the lower prevalence at a general hospital rather than a specialised SARS screening centre. Our data reaffirm that even during an epidemic, many patients may have atypical presentations[2] and require a period of careful observation under isolation until the clinical course is manifest or WHO criteria are met. They also highlight the urgent need for a rapid diagnostic test that can be applied early in the course of the infection especially if SARS reappears next winter.

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Competing interests: None declared.

1 Rainer TH, Cameron PA, DeVilliers S, Ong KL, Ng AWH, Chan DPN, et al. Evaluation of WHO criteria for identifying patients with severe acute respiratory syndrome out of hospital. *BMJ* 2003;326:1354-8. (21 June.)
2 Fisher DA, Lim TK, Lim YT, Singh KS, Tambyah PA. Atypical presentations of SARS. *Lancet* 2003;361:1740.

Accuracy of WHO criteria for SARS in a general teaching hospital. Values are numbers of patients

	SARS (n=18)	Non-SARS (n=938)	Total (n=956)
Initial WHO criteria met	5	42	47
Initial WHO criteria not met	13	896	909

Sensitivity 27.8% (95% confidence interval 9.7% to 53.5%), specificity 95.5% (94.0% to 96.8%), positive predictive value 10.6% (3.6% to 23.1%), negative predictive value 98.6% (97.6% to 99.2%).

Apoptosis may explain lymphopenia of SARS

EDITOR—In their review of the severe acute respiratory syndrome (SARS) Wong et al emphasise lymphopenia as a hallmark feature.[1] Panesar suggested that glucocorticoids or stimulation of the hypothalamic-pituitary-adrenal axis leads to lymphocyte margination and that patients without lymphopenia may have adrenal insufficiency.[2]

Apoptosis may also explain the lymphopenia of SARS. In severe paramyxovirus infections in humans such as measles, lymphopenia is commonly present and associated with more severe disease. One of us with Carrington recently reported that lymphopenia is also seen with another paramyxovirus infection: respiratory syncytial virus, which causes bronchiolitis in young children.[3]

Children with more severe bronchiolitis from respiratory syncytial virus infection have significantly lower absolute lymphocyte counts than those with mild disease. Bronchiolitis is ubiquitous and, in the developed world, the commonest reason a child under 1 year of age is admitted to hospital. Studies in mice show that not only is the lymphocyte immune response to virus essential in controlling the virus but it also causes disease.[4] The fact the immune response is both saint and sinner is believed to be why the use of ribavirin has proved less effective in bronchiolitis than was first hoped.

SARS, respiratory syncytial virus disease, measles, and sepsis show parallels in the occurrence of lymphopenia. In sepsis and measles apoptosis is believed to be the mechanism of lymphopenia. In models of sepsis, for example, inhibitors of apoptosis ameliorate illness and prevent death.[5] There may be important therapeutic implications for patients with SARS from these new research areas.

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Competing interests: None declared.

1 Wong RS, Wu A, To KF, Lee N, Lam CW, Wong CK, et al. Haematological manifestations in patients with severe acute respiratory syndrome: retrospective analysis. *BMJ* 2003;326:1358-62. (21 June.)
2 Panesar NS. Lymphopenia in SARS. *Lancet* 2003;361:1985.
3 O'Donnell DR, Carrington D. Peripheral blood lymphopenia and neutrophilia in children with severe respiratory syncytial virus disease. *Pediatr Pulmonol* 2002;34:128-30.
4 Openshaw PJ. Immunopathological mechanisms in respiratory syncytial virus disease. *Springer Semin Immunopathol* 1995;17:187-201.
5 Hotchkiss RS, Chang KC, Swanson PE, Tinsley KW, Hui JJ, Klender P, et al. Caspase inhibitors improve survival in sepsis: a critical role of the lymphocyte. *Nat Immunol* 2000;1:496-501.

Electroconvulsive therapy

Conflicting advice confuses prescribers

EDITOR—Carney and Geddes highlight the conflicting advice on the use of electroconvulsive therapy by two influential bodies, the National Institute for Clinical Excellence (NICE) and the Royal College of Psychiatrists.[1] The NICE guidelines restrict the use of electroconvulsive therapy while the royal college argues for wider inclusion criteria. The NICE guidelines also emphasise that no patient should be coerced into treatment.

So what is the reason for the disagreement between NICE and the royal college? NICE deliberately wants to curb the use of electroconvulsive therapy because of unresolved concerns about side effects, particularly memory loss.[2] Studies into its long term effects on cognitive function are lacking, but in their systematic review Rose et al say that 29-55% of patients report persistent memory loss.[3 4] However, the validity of this figure is questionable because of the poor methodological design of the studies included.

Faced with such a lack of convincing evidence NICE's approach has been to restrict the use of electroconvulsive therapy on the one hand while emphasising the need for fully informed consent on the other. How is the latter possible if we are still uncertain about the long term effects on memory?

Psychiatrists will now have to decide which expert opinion to accept. In the current medicolegal climate the implications of operating outside the NICE guidelines are likely to trump other considerations such as patient choice. We would advocate that the Royal College of Psychiatrists and NICE meet to resolve their differences and provide unified guidelines for patients and clinicians.

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1 Carney S, Geddes J. Electroconvulsive therapy—recent recommendations are likely to improve standards and uniformity of use. *BMJ* 2003;326:1343-4. (21 June.)
2 White C. New guidance on ECT looks set to curb its use. *BMJ* 2003;326:1003. (10 May.)
3 UK ECT Group, Geddes J. Efficacy and safety of electroconvulsive therapy in depressive disorders: a systematic review and meta-analysis. *Lancet* 2003;361:799-808.
4 Rose D, Fleischmann P, Wykes T, Bindman J. Patients' perspectives on electroconvulsive therapy: systematic review. *BMJ* 2003;326:1363-5. (21 June.)

NICE guidance may deny many patients treatment that they might benefit from

EDITOR—Carney and Geddes predict that most parties will be reasonably satisfied with the appraisal of electroconvulsive therapy by the National Institute for Clinical Excellence (NICE).[1 2] Our clinical experience is that many patients who may benefit from electroconvulsive therapy will be denied it under these guidelines.

Apparently, the NICE appraisal panel did not include a single psychiatrist, which may partly explain why clinical experience of the potential benefits of maintenance electroconvulsive therapy, as described in many reports,[3] seems to have been discounted.

The recommendations from NICE have also not acknowledged the different potential for memory disruption and cognitive side effects arising from bilateral as opposed to unilateral electroconvulsive therapy.[4] Given the concerns about memory disturbance as a side effect of bilateral electroconvulsive therapy, this is a surprising omission.

NICE recommends that the use of electroconvulsive therapy in depressive illness should be restricted to patients with severe depressive illness or catatonia in whom an adequate trial of other treatment options has proved ineffective or when the condition is considered to be potentially life threatening.[5] In severely depressed patients who have previously shown a good response to electroconvulsive therapy, it may be appropriate to consider it as a first line treatment.

The NICE guidelines do not recommend electroconvulsive therapy as a treatment for moderate depressive episodes. As discussed in the response to the appraisal from the Royal College of Psychiatrists' special committee on electroconvulsive therapy and the Scottish electroconvulsive therapy audit network (SEAN),[5] the randomised controlled trials that form the evidence base for electroconvulsive therapy were carried out mainly on moderately or moderately severely depressed patients, excluding those with severe depressive episodes who were unable to give informed consent.

We consider the NICE guidelines to be unduly restrictive and limiting, encouraging clinicians to deny patients potentially beneficial treatment.

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1 Carney S, Geddes J. Electroconvulsive therapy—recent recommendations are likely to improve standards and uniformity of use. *BMJ* 2003;326:1343-4. (21 June.)
2 National Institute for Clinical Excellence. *Guidelines on the use of electroconvulsive therapy*. London: NICE, 2003.
3 Gagne G, Furman M, Carpenter L, Price LH. Efficacy of continuation ECT and antidepressant drugs compared to long-term antidepressants alone. *Am J Psychiatry* 2000;157:1960-5.
4 Abrams R. *Electroconvulsive therapy*. 3rd ed. New York: Oxford University Press, 2002.
5 Royal College of Psychiatrists' Special Committee on ECT and Scottish ECT Audit Network (SEAN). *Statement on ECT practice*. 25 May 2003. www.sean.org.uk/appraisal.php (accessed 5 Aug 2003).

Patients must be confident that evidence of efficacy is compelling

EDITOR—I was a member of the review group that produced the guidelines on electroconvulsive therapy for the National Institute for Clinical Excellence (NICE).[1] I have personal experience of the treatment and have spent a large amount of my own time researching (qualitatively) patients' experience of it. With the recent recognition and acknowledgement that patients' expertise in relation to their lived experience is valid, the climate for partnership and inclusive working has improved to the benefit of patients and practitioners (and you may of course be either at different times in life).

The new guidelines were produced after a full and thorough assessment of all available data both clinical and experiential, and representation on the group included all key stakeholders. Everyone, including the Royal College of Psychiatrists, thought that the new guidelines were appropriate and workable and were a great step forward.

- Much information for patients and carers (which is scant) denies the possibility of long term cognitive impairment
- There is no long term follow up of live patients in terms of cognitive abilities and therefore no reliable evidence of long term effect
- It is openly acknowledged by the Royal College of Psychiatrists that practice within any previous guidelines has not been to standards that the college set for itself in terms of safety, consistency, environments, and expertise of professionals who carry out treatments
- Patients (myself included) have sought help to live with the consequences of treatment as opposed to the consequences of illness.

Although electroconvulsive therapy remains an accepted and useful treatment for some people, they should be able to have the greatest possible confidence that they are receiving it only when the evidence of likely efficacy is compelling from the best available data and experience. The NICE guidelines desire to achieve only this.

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Competing interests: None declared.

1 Carney S, Geddes J. Electroconvulsive therapy—recent recommendations are likely to improve standards and uniformity of use. *BMJ* 2003;326:1343-4. (21 June.)

Energy intake in pregnant women carrying boys or girls

Energy intake may correlate with prebirth knowledge of sex of child

EDITOR—Tamimi et al report that energy intake during pregnancy is affected by the sex of the child.[1] As an idea for further study, a dummy variable might be included in the regression analysis which indicates whether the mother knew the sex of her child before birth (or at the beginning of the study). One possible explanation for mothers with male embryos who eat more could be connected to socialisation.

An alternative study might compare three groups of pregnant women, one aware that their unborn child is male, another aware that their child is female, and the last unaware of the sex of their child until birth.

Differences in diet and overall food intake could be but a few maternal behaviours compared and correlated to

prebirth knowledge of the sex of the embryo.

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Competing interests: None declared.

1 Tamimi RM, Lagiou P, Mucci LA, Hsieh CC, Adami HO, Trichopoulos D. Average energy intake among pregnant women carrying a boy compared with a girl. *BMJ* 2003;326:1245-6. (7 June.)

Difference is chance observation

Editor—Tamimi et al report that women carrying male fetuses consume more energy and macronutrients than those carrying female fetuses.[1] The marginal difference in energy intake is, however, likely to be less than the error inherent in the food frequency questionnaire used to obtain the data.

The paper does not consider important factors that affect both diet and fetal growth, such as ethnic origin. It is incorrectly said that the increased energy intake of the women carrying boys meets increased demand for growth. The total energy cost of pregnancy is 289-340 MJ, which is rarely met through increases in dietary energy[2] and is instead achieved by adaptive reductions in energy expenditure.

Furthermore, our recent survey of the diets of 300 British women in early and late pregnancy, using five day food diaries,[3] found that nutrient intakes were similar in women carrying boys and girls, with the exception of energy and saturated fat in the first trimester. Here, in contrast to Tamimi et al, we found that women carrying female fetuses consumed 5% more energy ($P<0.05$) than women carrying male fetuses.

To follow the authors' hypothesis through would lead to the conclusion that with their faster growth rate and increased energy requirement, male fetuses are more vulnerable to growth retardation and effects of maternal undernutrition. There is no evidence that this is the case. The data presented in this paper look like the outcome of random chance and have no clinical significance.

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1 Tamimi RM, Lagiou P, Mucci LA, Hsieh CC, Adami HO, Trichopoulos D. Average energy intake among pregnant women carrying a boy compared with a girl. *BMJ* 2003;326:1245-6. (7 June.)
2 Durnin JVGA. Energy requirements of pregnancy. *Acta Paediatr Scand* 1991;373(suppl):33-42
3 Langley-Evans AJ, Langley-Evans SC. Relationship between maternal nutrient intakes in early and late pregnancy and infants weight and proportions at birth. *J R Soc Promotion Health* (in press).

Authors' reply

Editor—Shay's suggestions are worth considering. It is unfortunate that we did not have information on maternal knowledge about fetal sex. However, since our study included educated American women in Boston in the 1990s, a psychosocial interpretation seems less likely than a biological one.

Langley-Evans and Langley-Evans point out that the difference in energy intake between pregnant women carrying a boy rather than a girl is of marginal statistical significance, as recognised in our paper. However, the arguments they rely on to categorically conclude that our finding is a chance observation are unfounded. Firstly, non-differential exposure misclassification tends to attenuate an association, except in very unusual situations. Secondly, all women in our study were white, as indicated in the original study and referenced in our paper; therefore ethnicity cannot be a confounder. Thirdly, their argument that the total energy cost of pregnancy is rarely met through increases in energy intake is irrelevant, since our point refers to differential maternal energy intake by fetal sex. Lastly, we realise that the findings of their yet unpublished study are apparently not in agreement with those of ours, but random misclassification can negate significant associations more often than generate them.

Chance is always a possible explanation for any particular finding. Conclusive statements concerning associations should be based on a body of knowledge, rather than one or two studies. Additional investigations are necessary in order to confirm or refute these results, before statements regarding clinical significance are made.

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Electronic patient records in primary care

Study has serious flaw

Editor—There is a serious flaw in the design of the study by Hippisley-Cox et al on electronic patient records in primary care.[1] The authors say that they intended to differentiate between manual (all records kept on paper) and combination (part electronic and part paper record keeping) but actually differentiated between paperless (electronic) and paper based (combination or manual) records. The findings are therefore questionable.

For example, given that most general practitioners routinely prescribe electronically it is difficult to believe that paperless records were more likely to specify the drug dose unless Hippisley-Cox et al were reviewing only the paper based components of their paper based group, as opposed to the full record.

Additionally, Hippisley-Cox et al conclude that paperless records compare favourably with manual records. This is an extremely positive conclusion given that they specify one of the main reasons as to why general practitioners prefer to use paper based records during the consultation—diagrams. The lack of drawings observed in paperless records is surely due to the ineptness of electronic systems rather than because their value is not important? Although paperless records offer much from a medicolegal perspective, I wonder, from a patient perspective, how much more valuable that little drawing is? Can such drawings be disregarded so easily?

Finally, Hippisley-Cox et al say that the doctor-patient relationship may not be as personal as many suppose based on a textual analysis of references to specific patients. This is an erroneous observation given that doctors, like many of us, respond very heavily to visual cues as opposed to verbal recall.

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1 Hippisley-Cox J, Pringle M, Cater R, Wynn A, Hammersley V, Coupland C. The electronic patient record in primary care—regression or progression? A cross sectional study. *BMJ* 2003;326:1439-43. (28 June.)

Author's reply

Editor—In her first point Shaw misunderstands our methods. We looked at the entire medical record of the consultation whether all electronic, all manual or a combination.

The second point concerns the value of diagrams. We know of no evidence concerning this and can only observe that they are often used as expressive shortcuts, rather than as unique expressions that cannot be replicated in text. However, we would welcome evidence on this issue.

Lastly, Shaw draws attention to the finding that surprised us—namely, that so few general practitioners seemed to remember either the patient or the consultation, rather than just comment on the record entry. The assertion that this was due to an absence of visual cues should mean that the manual record group recalled patients and their consultations more than the electronic record group. However, this was not the case.

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Sharing patient information electronically throughout NHS

Change of culture is needed

Editor—Booth's editorial highlights once again the gulf between information technol-

ogy in primary and secondary care.[1] The key factor accounting for this lies in the evolution of information systems in these two parts of the NHS.

Primary care computing has been led by general practitioners, initially by enthusiastic entrepreneurs, trying to develop an electronic alternative to paper records that will provide fast and reliable answers to clinical problems. As a result, general practitioners and primary care trusts can now access quite advanced quality data on the care of patients. The new contract for general practitioners could not have been conceived without this.

In contrast, information in secondary care has generally been led by managers, with an emphasis on the needs of management, such as waiting times and bed states. This has left clinicians in secondary care unable to answer simple questions easily, such as, how many diabetic patients have had their blood pressure checked and how well controlled it is—information that is now readily available in my primary care trust. Interestingly, management information is also available to general practitioners once the clinical record is electronic; it seems you can have clinical information followed by management data but not the other way round.

Until clinicians in secondary care take control of information technology and focus it on their clinical needs the current situation will continue and make an integrated electronic record extremely difficult to achieve. Equally, managers need to relinquish control of information, risky though that may feel. Once again primary care has shown the way ahead—will secondary care follow?

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Competing interests: None declared.

1 Booth N. Sharing patient information electronically throughout the NHS. *BMJ* 2003;327:114-5. (19 July.)

Patients must be involved too

Editor—We agree with Booth that it is time to involve clinicians in the evolution of shared electronic information.[1] We would go further and argue for the involvement of patients as well. In support of this, we have two points to make, one ideational, one academic.

Firstly, in our study we showed that accuracy will be improved by reflecting on and discussing summary information with patients.[2] We also considered what patients had to say about their electronic summaries. Our study of 19 patients was not designed to provide an accurate and representative assessment of the number of errors that occur in electronic summaries in primary care. Our reason to report the figures was to show that there are worrying inaccuracies, not to quantify them. We also believe, from talking to our patients, that they would provide useful and important ideas when considering the design of a universal record.

Secondly, in referring to our qualitative study, Booth has selected the only quantitative element of this report to show the inaccuracies of electronic summaries in primary care. In selecting this part of our paper, we fear that Booth has missed the main point from our research—namely, the importance of involving patients. If data from qualitative studies are handled as they might be for quantitative studies, they can lead to misrepresentation. In Booth's editorial this manifests itself as a conclusion that is good but that does not go far enough.

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1 Booth N. Sharing patient information electronically throughout the NHS. *BMJ* 2003;327:114-5. (19 July.)
2 Ward L, Innes M. Electronic summaries in general practice: considering the patient's contribution. *Br J Gen Pract* 2003;53:293-7.

Ability to provide seamless decision support will be key factor

Editor—Booth's editorial is timely and raises several important issues.[1] Although clinicians have to assume a prominent role in planning and implementing a new integrated care record service, they should also be convinced why keeping patients' records electronically is useful.[2]

Booth details the advantage of being able to share patient data, improve clinical and workflow efficiency, and reduce medical errors. Clinicians are also interested in how it will help with their clinical workflow. Safe care for patients now requires a degree of individualisation that is impossible without computerised clinical decision support.[3]

As active clinical practitioners involved in the development of a decision support system for paediatrics (ISABEL, www.isabel.org.uk),[4] we believe that the ability to provide seamless decision support in practice will be the key to clinician satisfaction with the new system. This approach allows the implementation of evidence based decisions supported by the National Institute for Clinical Excellence, reduces medical error rates, and enables easy dissemination of lessons learnt from error into clinical workflow.

Advanced decision support will also allow knowledge delivery from textbooks and journals to the point of care. By using sophisticated pattern analysis software, data captured in the spine and the integrated care record service can be used for real time epidemiological surveillance. Public health problems, adverse events, and emerging clinical paradigms can be monitored and evoke swift remedial responses. The realisation of this vision will satisfy clinicians and promote successful implementation of the new integrated care record service.

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1 Booth N. Sharing patient information electronically throughout the NHS. *BMJ* 2003;327:114-5. (19 July.)
2 Littlejohns P, Wyatt JC, Garvican L. Evaluating computerised health information systems: hard lessons still to be learnt. *BMJ* 2003;326:860-3.
3 Bates DW, Gawande AA. Improving safety with information technology. *N Engl J Med* 2003;348:2526-34.
4 Ramnarayan P, Tomlinson A, Rao A, Coren M, Winrow A, Britto J. ISABEL: a web-based differential diagnostic aid for paediatrics: results from an initial performance evaluation. *Arch Dis Child* 2003;88:408-13.

Patient confidentiality may not be guaranteed

Editor—As general practitioners in the brave new world of sharing patient information electronically throughout the NHS,[1] can we still promise our patients that we will keep their secrets confidential? With the threat of central servers for our computers, practices would no longer be the keepers of their own data. The NHS would like easy access to our information—but who else would have such access?

If out of hours and accident and emergency departments are able to tap into our computers, then no one would have any medical secrets. Unlike hospitals, histories in general practice contain very deeply personal information about patients (mental illness, marital problems, alcoholism, abortions, impotence, etc). Does anyone really believe that it would remain secret for long that a local politician (or doctor) had been mentally ill?

Access for primary care trusts would mean that managers may be able to tap in and see the notes of their employees who are our patients. We will, of course, be promised that this will never happen. We may be promised that general practitioners can decide who has access and at what level, but for how long will the promise be kept when we don't own the equipment? Let no one tell me that lawyers and insurance companies will not hack in.

I believe that we should fight to retain control of our own data since confidentiality and trust are the cornerstones of general practice, and not an optional extra. The job is impossible without it, and if we lose this, then general practice is truly dead. Can I trust my computer?

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1 Booth N. Sharing patient information electronically throughout the NHS. *BMJ* 2003;327:114-5. (19 July.)