Outcome of pregnancy in diabetic women

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Rectal bleeding and colorectal cancer

Inclusion criteria of study need clarification

Editor—Wauters et al report on the diagnostic value of rectal bleeding in terms of subsequent development of colorectal cancer.1 We feel that this study requires clarification for several reasons.

Firstly, the authors do not report the pre-test probability of colorectal cancer in age specific categories in their population. The diagnostic value of a symptom such as rectal bleeding and the impact on post-test probability and subsequent referral threshold are maximised when the pre-test probability of the disease is known.

Secondly, they fail to mention that less than half of patients with rectal bleeding have no other symptoms.2 More often it is associated with other bowel symptoms that have higher diagnostic value than rectal bleeding alone.3

Thirdly, the reported positive likelihood ratio of 68.3 "rules in" a diagnosis of colorectal cancer, irrespective of the pre-test probability of the disease.4 The reported specificity of 99.5% has the same effect of ruling in the target disorder of colorectal cancer. These findings imply that any patients attending their general practitioner with rectal bleeding need referral and further evaluation. Our own clinical experience and other community based studies of rectal bleeding indicate that such a high specificity and likelihood ratio is unlikely and may well be misinterpreting.5

Finally, the most likely explanation for the results relates to general practitioners underreporting rectal bleeding in the prospective arm of the study. Wauters et al chose “rectal bleeding as the reason for visiting a general practitioner” as the inclusion criterion for their study. They should clarify whether this means that patients in whom rectal bleeding was not the primary reason for consulting their general practitioner were excluded. A prospective study in the Netherlands showed that among patients presenting with rectal bleeding, 51% stated this as the primary reason for consulting their general practitioner; 49% consulted for a different reason, but rectal blood loss was subsequently mentioned during the consultation.2 Another prospective study found that 3% (95% confidence interval 1.4% to 5.8%) of patients with rectal bleeding subsequently developed colorectal cancer, and even in this study patients with “clinically relevant rectal bleeding” were overrepresented.2 In Wauters et al’s study, 27 patients with rectal bleeding (7%, 4.6% to 10%) developed colorectal cancer.

In summary, there may have been a systematic bias in excluding less severe forms of rectal bleeding which may have not been the primary reason for consulting a general practitioner. This has resulted in inflated values for specificity and positive likelihood ratio. Before their results are incorporated into clinical practice Wauters et al should clarify their inclusion criteria and provide age specific two by two tables so that readers can judge for themselves the diagnostic value of isolated rectal bleeding in general practice.

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Results of study were incorrectly interpreted

Editor—The diagnosis of colorectal cancer is an important subject, but the paper by Wauters et al is flawed on several counts.1

To determine the value of rectal bleeding in the diagnosis of colorectal cancer the authors correctly analysed retrospectively all patients with a diagnosis of colorectal cancer in 1993-4 to calculate the sensitivity and analysed prospectively all patients presenting with rectal bleeding in 1993-4 to calculate the positive predictive value. They then claim to have estimated the specificity and negative predictive values from these results. However, the retrospective and prospective parts of the study were carried out on different populations of patients, and the data collected did not give a measure of the relative size of the population who neither had colorectal cancer nor presented with rectal bleeding. Hence it was not possible to make valid estimates of the specificity and negative predictive values. For the same reasons, valid estimates of the likelihood ratios cannot be made.

The authors stated that the probability of colorectal cancer increases greatly in association with fatigue and weight loss. However, the figure of 7.1% for the positive predictive value associated with fatigue must be wrong as the mean is outside the 95% confidence interval (8.3% to 15.8%). The positive predictive value associated with weight loss of 16.0% (4.5% to 36.1%) is not statistically different from the overall positive predictive value of 7.0%.

The authors argued that “people should be better informed and encouraged to seek medical advice if bleeding occurs.”2 Although this advice might be appropriate, the conclusion was not justified by their results, which did not show that any of the patients failed to seek appropriate medical advice.

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Ferrero—The diagnosis of colorectal cancer is an important subject, but the paper by Wauters et al is flawed on several counts.

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References


Possible recall bias results in a small difference in the left upper cell of the two by two table (27 in the prospective arm and 31 in the retrospective arm). However, this difference does not affect the results of our calculations.

Finally, we concluded from our data that most cases of rectal bleeding do not require referral in connection with possible colorectal cancer. This does not apply, however, to patients aged over 60 or to those with additional signs or symptoms. These conclusions still hold.

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All epidemiological evidence is important in colorectal cancer

Entwror—to Boyle and Langman summarised descriptive features and risk factors of colorectal cancer in their article.1 They seem, however, rather selective (or not well informed) of recent literature regarding dietary and nutritional factors in the aetiology of colorectal cancer. Boyle and Langman state that the intake of dietary fat and meat is positively related to risk of colorectal cancer. A high intake of meat is probably associated with increased risk of colorectal cancer, but the epidemiological evidence for fat and colorectal cancer is not as strong as they say. They referred in detail to the results in the nurses’ health study published by Willett et al in 1990,2 a single prospective study that showed an increased risk of colon cancer associated with high intake of total or animal fat after adjustment for total calorie intake.

Epidemiological evidence should not be relied on for the result of a single study, but the total evidence must be considered. At least seven large prospective studies were reported in Europe and the United States up till 1999. None of these studies found a clear, positive association between fat and colon or colorectal cancer; reported relative risks for the highest versus lowest intake ranged from 0.5 to 1.2 with adjustment for total calorie intake. Six of these studies also addressed the relation between saturated fat and colorectal cancer and found no material association, with relative risks of 0.7-1.4 for the highest versus lowest intake. Giovannucci et al noted that the increased risk associated with animal fat intake in the American nurses disappeared when red meat intake was taken into account.3 Furthermore, in the combined analysis of 13 case-control studies, Howe et al showed no measurable positive association between either total fat intake or intake of saturated fat and colon cancer with adjustment for total calorie intake.4 While animal studies have suggested an aetiologic role for high fat intake in colorectal carcinogenesis, such evidence is very hard to extrapolate to humans living freely.

Boyle and Langman also say that both vegetables and fruits may be protective against colorectal cancer. There is, however, little evidence regarding the protective effect of fruit against colorectal cancer; readers will relish a more accurate and succinct review article regarding dietary factors and colorectal cancer,5 which Boyle and Langman missed in the list of further readings.

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Competing interests: None declared.


Impact of NHS Direct on demand for immediate care

NHS Direct must be better marketed and deal with problems more effectively

Entwror—in their responses to the paper by Munro et al,1 who found that NHS Direct had no appreciable impact on the use of ambulance services and accident and emergency departments, McNerney et al2 and Lawson et al3 addressed two important points: do the patients know about NHS Direct; and does NHS Direct make any difference to the use of emergency services anyway? At the moment, the answer to both questions seems to be “no.”

We are studying consultations with our out of hours general practitioners’ cooperation (Bridgewater Out-of-hours and Night Emergency Service, BONES), comparing the outcomes for two groups of patients who have called our service: those who have previously contacted NHS Direct about their problem and those who have not.

Preliminary results show that, of the 1153 consultations with BONES over four weeks in October, in 1005 cases (87%) the patients said they had not tried NHS Direct. We had a similar number of contacts over the same period in 1997, before NHS Direct became operational. Even if NHS Direct is preventing a small upward trend in calls out
of hours, the fact therefore remains that most patients do not use NHS Direct.

But would it make any difference to the outcome if they did? The purpose of NHS Direct is to deal effectively with problems that can be dealt with on the telephone, and pass on to the emergency services those problems that are likely to need some kind of intervention. Therefore, those who call NHS Direct and then consult the emergency services should end up needing more face to face consultations, on the spot treatment, visits, and hospital admissions, and fewer consultations by telephone alone. On the contrary, we found that 53% of the problems that had already been presented to NHS Direct could still be dealt with over the telephone by BONES, compared with 47% of those that had not involved NHS Direct.

Furthermore, the NHS Direct callers ended up needing fewer treatments or admissions to hospital. NHS Direct has the potential to alleviate some of the increasing demands on primary care, both in and out of hours, but if the government wants it to be useful it must be better marketed and must deal more effectively with the problems presented to it.

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Meaningful review is still outstanding

Editor—The comments by Munro et al and the responses by McNerney et al and Lawson et al relate to a time when the volume of calls to NHS Direct and their impact were very small. Volume of calls to NHS Direct and their responses by McNerney et al and Lawson et al have been unable to divert telephone calls for clinical advice from their accident and emergency department to NHS Direct.1 Such a scheme has been in place in Portsmouth for over a year now and was recently extended to Southampton. As Lawson et al noted, call diversion to NHS Direct offers significant advantages in quality of service, including staff trained specifically in telephone advice, computerised protocols, and improved documentation. It can also increase time for direct patient contact. In Portsmouth we estimate that the removal of the need to respond to telephone calls has freed up the equivalent of two whole time equivalent senior nurses, enabling them to improve the quality of service to patients requiring face to face advice.

Along with other published evaluations,2 Lawson et al comment on the lack of impact of NHS Direct on numbers attending established healthcare services. Such evaluations oversimplify the objectives of the service. NHS Direct was set up to improve access to healthcare services, which it has achieved, with over 3 million callers to the service already. Many callers indicate a prior intention to call their general practitioner or attend accident and emergency wards, and yet they are advised about self care, potentially saving a visit. It is, however, evident that other callers would not otherwise have accessed healthcare services. Some of these patients, who would normally fall “below the water level” of the “iceberg of illness,” are advised to seek further clinical advice and will thus move into the system.

The net numerical effect of these flows on existing services may be neutral. But those accessing services should be doing so more appropriately. Evaluations of NHS Direct must tackle this challenge of measuring appropriateness. This is the third side of the triangle of evaluation—increased access to healthcare information and advice, and demand on existing services being the others.

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Comparative data for two of the largest areas of general practice cooperative and adjacent accident and emergency departments in September 1999 and 2000

<table>
<thead>
<tr>
<th></th>
<th>September 1999</th>
<th>September 2000</th>
<th>Percentage change</th>
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<tr>
<td>Out of hours contacts with two largest divisions of cooperative.</td>
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<td></td>
<td></td>
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<tr>
<td>Telephone advice</td>
<td>1 948</td>
<td>2 948</td>
<td>+32.1</td>
</tr>
<tr>
<td>Visits to centre</td>
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<td>842</td>
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<tr>
<td>Home visits</td>
<td>748</td>
<td>656</td>
<td>-12.4</td>
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<tr>
<td>Contacts with local accident and emergency departments (24 h)</td>
<td>8 397</td>
<td>8 153</td>
<td>-3.8</td>
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<tr>
<td>Calls to NHS Direct North East</td>
<td>10 122</td>
<td>17 559</td>
<td>73.5</td>
</tr>
</tbody>
</table>

Should NICE's advice be handled centrally or locally?

Editor—Smith’s editorial on the failings of the National Institute for Clinical Excellence (NICE) addresses the right issue but attacks the wrong target. It is not the recommendations emanating from the institute’s deliberations that have an impact on the NHS but the subsequent executive letter that issues from the Department of Health.

The institute has many strengths, but its remit is weak in at least four areas:

- It often has to rely on evidence from studies whose criteria on patient selection exclude people at the extremes of age, non-compliant people, and those with comorbidity.
- Economic appraisal is a factor in its assessments. In complex interventions, widening generalisations have to be made about typical costs in typical hospitals and typical clinical outcomes in a range of treatment centres and populations. At local level the costs (especially marginal costs and opportunity costs) and outcomes can be very different from the norm. Briggs, in the editorial preceding Smith’s, warns of trials that are underpowered with respect to economic variables.
- The institute usually looks at a single link in a patient pathway, and the Department of Health translates its recommendations into requirements. But in a local health economy the more pressing bottlenecks in the patient’s journey might be at the diagnostic stage or in palliative care.
- The institute does not have to consider trade-offs within whole programmes of care, such as women’s health. The forthcoming review of infertility treatments will be a fascinating worked example. In east Norfolk last year we lengthened screening intervals for cervical cancer from 36 months to 54 months, at a small but tangible health loss. But we are now able to sustain our substantial in vitro fertilisation programme as well as allow sterilisations on the NHS, giving a favourable net health gain; this has been supported at public consultation. A neighbouring health authority claims that in vitro fertilisation and sterilisations are unaffordable and continues its 36 month cervical screening cycle.

The job of the National Institute for Clinical Excellence is not to ration but to advise. The institute works best at arm’s length. Smith’s Committee for Honest and Open Rationing is the Department of Health. The real debate is whether openness, honesty, and responsibility should be handled centrally (on the assumption that the centre always knows best and one size fits all) or delegated to local level, where we would celebrate the differences that emerged as an indicator of a lively and responsive NHS. I prefer the latter.

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Competing interests: The author has participated in several public and professional educational events on health economics, including support for the case for health funding authority of infertility within the NHS, sponsored by a variety of pharmaceutical companies, notably Serono, Bristol Myers-Squibb, Zeneca, Novartis, and Janssen-Cilag.

1 Smith R. The failings of NICE. BMJ 2000;321:1365-4. (2 December.)

Catheter ablation for cardiac arrhythmias

Ablation should not be denied to elderly patients on basis of age

Editor—Peters in his editorial fails to justify his conclusion that older patients with longstanding atrial fibrillation can be managed by controlling their ventricular rate and by giving them antiocoagulation treatment without need for input from a specialist.1 Elderly people are underrepresented in clinical trials of cardiovascular disease and are less likely to have appropriate cardiological investigations.2, 3

The reason for valuing the life of younger patients over that of older ones is not made explicit. Harris argues that what is valued by each of us is the rest of our lives, the duration of which is unknown.4 An anti-ageist argument is of particular relevance to catheter ablation, a procedure that is applicable to patients of all ages and considered by some to be first line treatment in selected elderly patients.5 If catheter ablation is denied to elderly patients on the basis of age then they are subject to the same injustice as if the procedure had been denied to the young on some equally spurious premise.

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Author’s reply

Editor—Bourne seems to make an assumption that is common in clinical practice, of considering older patients with atrial fibrillation to encompass all elderly patients with an arrhythmia. Not all complaints of palpitations and irregularities of the pulse in elderly people can be assumed to be atrial fibrillation.

Atrial fibrillation is recognised to be common and a major clinical challenge among elderly people (affecting 9% of those older than 70 years), but other arrhythmias must be considered, particularly atrial flutter or the possibility of ventricular tachycardia, if there is evidence of previous myocardial infarction. Although they are considerably less likely than atrial fibrillation, other arrhythmias such as these for which radiofrequency ablation may be appropriate are not uncommon in this age group. For older patients with longstanding atrial fibrillation, Bourne’s assertion that the paper by Van Gelder et al concludes that radiofrequency ablation should be first line treatment is incorrect. In chronic atrial fibrillation, radiofrequency ablation cannot be considered first line treatment to abolish the arrhythmia in any age group. If, however, control of the ventricular rate cannot be achieved by other means, radiofrequency ablation of the atrioventricular node accompanied by implantation of a ventricular pacemaker may provide appropriate symptomatic relief.

I thank Bourne for the opportunity to raise these important issues in response to his drawing against conclusions from the editorial. In so doing, however, he seems to have missed the point.

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Surveillance of Haemophilus influenzae infection

Surveillance data for assessing impact of vaccination are valid

Editor—Olowokure et al suggest that routine surveillance of Haemophilus influenzae is incomplete, that completeness declined after the vaccine was introduced, and that the effectiveness of vaccination programmes is overestimated.1 We question this.

The authors suggest that the same weakness may affect the surveillance of the group C meningococcal vaccination programme. We accept that routine reporting is often incomplete, but, because of this, all vaccine preventable infections are under enhanced surveillance. The 15-fold reduction in the incidence of H influenzae type b disease after the introduction of the H influenzae type b vaccine was observed in an enhanced active surveillance scheme operating in five NHS regions,2 not by the use of routine laboratory reports to the Public Health Laboratory Service (PHLS) Communicable Disease Surveillance Centre.

This surveillance scheme was established before the vaccine was introduced and was continued until 1995, when an enhanced national scheme was implemented.3 Completeness of data can be maximised by reconciling reports to the Communicable Disease Surveillance Centre, isolates referred to the PHLS Haemophilus Reference Unit, and notifications of H influenzae type b meningitis, and by active reporting of cases to the British Paediatric Surveillance Unit.

A similar scheme—reconciling reference laboratory reports, notified infections, cases known to consultants in communicable disease control, and laboratory reports to the Communicable Disease Surveillance Centre—was used to determine the burden of infection before the introduction of meningococcal group C vaccine1, this scheme has now been extended nationally.

Olouwokure et al do not mention the most important weakness of surveillance systems for vaccine preventable disease. When the incidence of a disease is being ascertained after the introduction of a vaccine, the specificity of clinical case definitions and laboratory tests is critical. If specificity is low, then the true incidence of an infection declines the predictive value of the case definition falls and the proportion of false positive diagnoses increases.

Since 1990 in five regions, and since 1995 in the whole of England, all reports of confirmed invasive haemophilus infections have been followed up by referral of the isolate to the PHLS Haemophilus Reference Unit, where additional confirmation is carried out with molecular typing techniques. Between 1995 and 1999, of 136 putative type b isolates referred, only 108 were confirmed as type b.

The evaluation of surveillance data for *H influenzae* type b vaccine should be based only on confirmed type b infections. The collaboration between the Communicable Disease Surveillance Centre and the national reference laboratories for meningococcal and haemophilus infections ensures that national surveillance data for England and Wales for assessing the impact of vaccination are valid.

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**Authors’ reply**

**Editor**—Ramsay et al misrepresent the conclusions of our study. We suggested that routine surveillance was incomplete, that the underascertainment worsened after the vaccine was introduced, and that if routine surveillance data were used the effectiveness of the vaccine was overestimated. Thus we made the argument for the introduction of enhanced surveillance before the introduction of the intervention (to set baselines) and its continuation, with the same methods, after the introduction (to detect change).

At the time that we performed our analysis the relevance to the introduction of meningococcal type C vaccine was that no funding had been agreed for ongoing enhanced meningococcal surveillance, even though the immunisation programme had started. Enhanced national surveillance has now been set up, although there are methodological differences to the sub-national system that was in place before the vaccine was introduced.

Because our paper was published as a short report (maximum 600 words) we did not have space to mention several weaknesses in surveillance systems. The use solely of laboratory confirmed cases obtained by testing routinely generated clinical specimens presupposes that no changes occur in the clinical practice that generates these isolates. Experience with meningococcal infection suggests that preadmission antibiotics and an increased reluctance to perform lumbar puncture in recent years have reduced the likelihood of obtaining an isolate of the infecting organism2 and that other methods of case ascertainment are required.

Ramsay et al, and others, expend enormous effort in improving surveillance data. They should interpret our report as supportive of the need for their efforts.

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**Outcome of pregnancy in diabetic women**

**Authors did not define criterion for case selection**

**Editor**—Hawthorne et al claim to show that women with diabetes have a much more unfavourable outcome of pregnancy in England than Norway.1 What does their study really show?

Their criterion for case selection (“diabetes”) was not defined. But the prevalence differs hugely between the countries: 1 in 335 pregnant women in northeast England and 1 in 90 in Norway were known to consultants in communicable disease control, and laboratory reports to the Communicable Disease Surveillance Centre—was used to determine the burden of infection before the introduction of meningococcal group C vaccine1, this scheme has now been extended nationally.

Olouwokure et al do not mention the most important weakness of surveillance systems for vaccine preventable disease. When the incidence of a disease is being ascertained after the introduction of a vaccine, the specificity of clinical case definitions and laboratory tests is critical. If specificity is low, then the true incidence of an infection declines the predictive value of the case definition falls and the proportion of false positive diagnoses increases.

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4 Enhanced surveillance of meningococcal disease. Commun Dis Rep 1998;8:1

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**Authors’ reply**

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cases of more severe uncontrolled diabetes; the Norwegian data have a higher rate of keying or editing errors (which will serve to increase the apparent numbers with a rare disorder and dilute any effect); the Norwegians do have a higher rate of diabetes in young women; or the criteria for diagnosis differ between the two countries. We hope that these four possibilities will be investigated before any conclusion is reached on poorer control of diabetes causing an excess of problems in the United Kingdom. We have strong reasons to think that the English data on prevalence are accurate since data from the prospective Avon longitudinal study of parents and children show a similar rate (0.4%) of pregnancies in women with non-gestational diabetes to that in Hawthorne et al’s study.\(^1\)

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\end{itemize}

**Authors’ reply**

\begin{itemize}
  \item Editor—Johnstone suggests that the case selection for diabetes was not defined and that this invalidates the finding that the outcome of pregnancy is better in Norway than in England. He acknowledges that diabetes was confirmed in most cases from northeast England before entry into the study, but he questions the data from Norway. Golding et al draw attention to the contrast in prevalence of maternal diabetes between the two countries and suggest possible explanations for this.
  \item We accept that there are pitfalls in using centralised data. However, data collection by the medical birth registry in Norway has documented a decline in perinatal mortality for diabetic pregnancy, from 155.1/1000 for 1967-72 to 18.1/1000 for 1986-92.\(^2\) During this time the number of births delivered in hospitals with more than 3000 births a year has increased from less than 10% to 34%.
  \item During the 1990s gestational glucose intolerance was a particular focus in clinical care as well as in the registration. This has ensured the option of removing cases of gestational glucose intolerance from the analysis. The fact that there has been no secular decrease in the occurrence of macrosomia in the infants adds to the validity of the diagnoses registered by the medical birth registry.\(^3\)
  \item There are clear clinical guidelines recommending centralisation of clinical care of diabetic pregnancy. These guidelines distinguish the management of diabetes and glucose decrease during pregnancy.\(^4\)
  \item The improved mortality figures are thought to relate to an updated intensified follow up module for pregnant diabetic patients, improved diabetes care in general, home glucose monitoring, and measurement of haemoglobin A\(_1c\) concentrations.
\end{itemize}

Norway has a documented high and increasing incidence of diabetes in children\(^5\) and young people.\(^6\) In 1991 Joner and Sovik showed that there was a twofold increase in the incidence of diabetes mellitus in the 15-29 year age group.\(^7\) The finding that there is an increased prevalence of diabetic pregnancies in Norway compared with northeast England is consistent with these data.

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**Ultrasonography in diagnosis of acute appendicitis**

**Diagnostic laparoscopy is often more useful than ultrasonography**

\begin{itemize}
  \item Editor—Douglas et al’s trial of ultrasonography in the diagnosis of acute appendicitis, and the accompanying editorial, highlight the importance of an accurate diagnosis of acute abdominal pain in the right iliac fossa and the need to avoid unnecessary appendicectomy.\(^1\) The results indicate that ultrasonography has little practical value in the diagnosis of acute appendicitis because of false positive and false negative results and the inability to identify alternative diagnoses.\(^2\)
  \item Neither article mentioned the increasing use of diagnostic laparoscopy in these cases. This technique makes an accurate diagnosis clear; this is especially useful in female patients of any age and in elderly men, in whom diagnostic doubt is common. As well as preventing inappropriate appendicectomy, diagnostic laparoscopy defines the correct operative intervention if an alternative diagnosis necessitates surgery. If surgery is not required a definitive management plan is usually clear. A further advantage of laparoscopy is that if surgeons have appropriate training and experience the appendix can be removed laparoscopically, with advantages in patient recovery.\(^3\)
  \item If the abdominal signs are sufficiently clear to indicate peritonism in the right iliac fossa in elderly patients or female patients of reproductive age there is little to be gained from ultrasonography. Laparoscopy should be undertaken and surgical intervention proceeded to as appropriate.
\end{itemize}

**Active observation is often sufficient to make diagnosis**

\begin{itemize}
  \item Editor—Douglas et al have added to the debate on the role of imaging in the diagnosis of acute appendicitis. Reasons for using ultrasound, and perhaps computed tomography, need to be viewed against the background of the epidemiology of acute appendicitis.
  \item When all children admitted with acute abdominal pain were studied in the 1960s it was found, surprisingly, that in 30-40% (mostly referred as having “acute appendicitis”) the condition settled without treatment.\(^4\) This syndrome, which for 12-24 hours closely resembles acute appendicitis, was named acute non-specific abdominal pain and was soon found to occur equally commonly in adults.\(^5\)
  \item The knowledge that as many as one third of patients with acute abdominal pain will prove to have a self limiting condition must have an effect on management. At the time of admission about one third of patients clearly need emergency surgery. Then the task among the remainder is to distinguish, with the minimum of delay, those with suspicious signs who are developing surgical or medical disease requiring treatment from those with non-specific abdominal pain.
  \item When the frequency of non-specific abdominal pain is not allowed for there is a tendency to regard every doubtful case as a possible perforated appendix, and this still leads to as many as 15-30% of appendicectomies being unproductive. If a policy of active observation is adopted, experience over 25 years in many centres has shown that repeated bedside examination is a safe method of separating the two groups; an unproductive appendicectomy rate of 14% in the 1960s had fallen to 3-5% in the 1990s.\(^6\) Concern is expressed that delay during observation allows perforation to occur, but most patients with a perforated appendix show appreciable signs on admission; recovery has been good in the few cases that have been recognised during active observation.\(^7\)
  \item The existence of imaging (especially computed tomography, with its high dose of radiation) is not a reason for using it if a simpler method is effective and safe, and experience with active observation suggests that it is rarely required. Douglas et al and

\begin{itemize}
  \item 1 Douglas CD, Macpherson NE, Davidson PM, Gani JS. Randomised controlled trial of ultrasonography in diagnosis of acute appendicitis, incorporating the Albright sign. BMJ 2000;321:919-22. (14 October.)
  \item Bruland SN. Can we improve diagnosis of acute appendicitis? BMJ 2000;321:917-8. (14 October.)
\end{itemize}
Weyant et al have found that reliance on ultrasound and computed tomography produces problems from false negative and positive results that have to be resolved at the bedside; both groups emphasise the central role of “more precise patient selection by clinical criteria.”

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1 Douglas CD, MacLennan NE, Davidson PM, Gami JS. Randomised controlled trial of ultrasound and computed tomography in diagnosis of acute appendicitis, incorporating the Alvarado score. BMJ 2000;321:1959-62. (14 October.)
2 Jones PF. Acute abdominal pain in childhood, with special reference to cases not due to acute appendicitis. BMJ 1972;i:284-6.

If possible, antiretroviral treatment should not be interrupted in patients undergoing general anaesthesia or other procedures for which they must be nil by mouth for a period. When an interruption is unavoidable all antiretroviral drugs should be stopped and restarted together, except for those with a long half life, which should be stopped two to four days before the others. We recommend that specialist HIV advice should be sought about antiretroviral management in individual patients.

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Smoking and use of mobile phones

Data have been wrongly interpreted

Enntror—Charlton and Bates’s use of data was breathtaking in its inaccuracy.1 For a start, the chart has no data points before 1996, so we have no way of judging how large the downturn in teenage smoking has been. But more importantly, the chart shows clearly that teenage smoking was falling before the sharp rise in mobile phone ownership. And, even worse, at the point where phone ownership sharply increases, the decline in smoking actually levels off. This is a classic case of not letting the facts get in the way of an interesting hypothesis. It is even more regrettable that the letter caught the national headlines. This sad misuse of numbers is a great deal worse—because it is so obvious—than the more technical statistical liberties I often find in BMJ articles.

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Proportion of population that smokes and ownership of mobile phones in Switzerland 1992 and 1997

<table>
<thead>
<tr>
<th>Year</th>
<th>Smokers of all ages (%)</th>
<th>Smokers aged 15-24 (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1992</td>
<td>30.1%</td>
<td>31%</td>
</tr>
<tr>
<td>1997</td>
<td>32.7%</td>
<td>43%</td>
</tr>
</tbody>
</table>

No correlation in Switzerland either

Enntror—the link between smoking and use of mobile phones by young people suggested by Charlton and Bates is not supported by Swiss data.1 The prevalence of smokers aged 15 and 24 in Switzerland increased disproportionately between 1992 and 1997 (table). The number of mobile phone subscribers increased from 215 000 in 1992 to 1 044 000 in 1997. Even though I do not have the specific numbers for the age group 15 to 24 year olds, it is likely that there was at least a proportional, if not even more rapid, growth in this age group.

Italian data don’t show the same pattern

Enntror—The hypothesis of a beneficial effect of mobile phones on adolescents’ attitudes to smoking is appealing, and the explanations suggested by Charlton and Bates are attractive.1 Italians lead the European Union in ownership of mobile phones, so the influences of this particular social habit on teenage behaviour could be studied appropriately in our country.

This question should be resolved through a prospective study in which other known promoters and demoters of smoking in various countries are considered together with use of mobile phones.

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Percentage of smokers among Italians aged 15-24 years in 1995 and 1998

<table>
<thead>
<tr>
<th>Year</th>
<th>Male</th>
<th>Female</th>
</tr>
</thead>
<tbody>
<tr>
<td>1995</td>
<td>27.7</td>
<td>26.2</td>
</tr>
<tr>
<td>1998</td>
<td>15.4</td>
<td></td>
</tr>
</tbody>
</table>
Hospital mergers may be painful but have positive aspects too

Errone—Most of us would agree with Harvey’s comments in her personal view about the problems of hospital mergers. A merger is more like a divorce than a marriage. It introduces vulnerabilities, sensitivities, suspicion, and redirections that often seem inappropriate.

The developing paranoia that Harvey expresses—concerning the perceived low morale and poor standards of the interloping hospital—is only too common. It is overcome by communication and joint decision taking, which, in itself, is extremely difficult, not only because of the separation of the units but also because of the lack of time in one’s clinical practice.

There is, though, a positive side to a merger. The larger population that we serve allows a more efficient use of resources, particularly in smaller specialties. Merged departments allow a more efficient use of junior hospital doctors, especially specialist registrars; this is a bonus in view of both the European directive to reduce their hours and the reduction in Calman numbers. The junior hospital doctors, especially specialist registrars; this is a bonus in view of both the European directive to reduce their hours and the reduction in Calman numbers. The Royal College of Obstetricians and Gynaecologists has considered the increasing risk and the reduction in Calman numbers. The European directive to reduce their hours and the reduction in Calman numbers.

Men should follow example of women’s health movement

Errone—As Kraemer points out, being a man can seriously damage your health. The Men’s Health Forum (www.menshealthforum.org.uk), with a membership of over 180 organisations ranging from the BMA and the Department of Health to the Royal College of Nursing, and from the Post Office to Marks and Spencer, has campaigned on this issue for six years. Politicians now seem to realise that men’s health is often a contradiction in terms and urgently needs more attention and resources.

Extensive research shows that the health of both sexes is often inextricably entwined; this is clearly shown by chlamydial infection. A joint approach to the health of women and men is required, rather than a “them and us” confrontation.

It is no coincidence that men’s health is increasingly highlighted in areas where women are taking their place as policymakers. Half of the elected representatives of the Men’s Health Forum are female, with a woman as deputy chair. It was two women—Tessa Jowell and Yvette Cooper (minister for health, public health and minister for health, public health and minister for health, respectively)—not Frank Dobson or Alan Milburn (previous and current health secre-

caries) who brought men’s health as an issue to the attention of parliament. Perhaps men should take a leaf from the women’s health movement’s book rather than begrudge women their success. With spending on women’s health in the United Kingdom the lowest in Europe, it would serve both sexes well to improve spending generally in the NHS.

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Title of Dr should be sufficient for all doctors

Errone—I applaud Loudon’s questioning of the title of Mr for surgeons, but he is much too gentle with our sensitive colleagues. My history genes could grudgingly accept Mr for male consultant surgeons, but the absurdity is complex.

Married multiparous surgeons are usually referred to as Miss, a title more appropriate to an actress than a doctor. Gynaecologists and ophthalmologists are Dr in Scotland but Mr or Miss in England and Wales. Patients, who erroneously consider that all consultants are Mr or Miss, become confused when a young surgical senior house officer or registrar converts overnight from being addressed as Dr to being addressed as Mr or Miss on passing the fellowship of the Royal College of Surgeons.

I have no doubt that some patients fret with anxiety over getting the great man’s (or woman’s) title right. I am a physician, and I well recall one of my elderly patients whispering to her husband as she left my consulting room, “He was Dr Crisp, not Mr Crisp. He must be one of their junior doctors.”

British medicine makes itself ridiculous with its multiplicity and inconsistency of titles for practising doctors. The title Dr should be sufficient for all doctors regardless of age, sex, specialty, and distinction. I applaud the North American practice of preferring to use the title Dr rather than Professor when treating patients.

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Letters

The fragile male

Male zygotes are often formed at suboptimal times in fertile cycle

Errone—Kraemer added large quantities of data to substantiate the proposition that, from conception, males are more vulnerable than females. Postnatal vulnerability has some social causes, which are known, but the

causes of the prenatal vulnerability of males are not established, so I wish to suggest one.

During the menstrual cycle women have a fertile window lasting several days. There is strong direct and indirect evidence that the regression of the sex ratio of the offspring (proportion male) on the time of fertilisation is U shaped across this fertile window. In other words, females are formed disproportionately often in the middle of this window and males at either end of it. There is also strong evidence that zygotes formed at either end of the window are more likely to be spontaneously aborted.

Thus it would seem that, compared with female zygotes, male zygotes are formed disproportionately often at suboptimal times in the cycle. This suggestion would explain three factors: the additional prenatal vulnerability of males, the suspected higher sex ratio in spontaneously aborted fetuses than in live births, and the male excesses in those adult diseases suspected of being related to suboptimal intrauterine environments.

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