Letters

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Medical error reporting must take necropsy data into account

EDITOR—Responding to correspondence relating to medical error, organ retention, and death certification,[1] we wish to communicate the results of our study comparing causes of death on death certificates with those at 440 hospital necropsies, in which substantial discrepancies were identified.[2]

The sensitivity of the death certificate in predicting cause of death was 0.47, with a range from 0.90 in the neurological system to 0.28 in the cardiovascular system. Sensitivity for malignant causes of death on the certificate was 0.65, and in 35% of over-diagnosed malignant deaths, no tumour was shown. Our data are in accordance with similar studies showing divergence in cause of death recorded on the death certificate and at necropsy, including rates of up to 75% for previously undisclosed and clinically important findings.[3]

Despite improvements in diagnostic technology, necropsy is still considered to be the gold standard for determining the cause of death.[4] Nevertheless the rate of hospital necropsy has fallen dramatically, and in many hospitals is well below the recommended rate of 10% of all hospital deaths. It follows that the accuracy and reliability of any current mortality data must be viewed with caution.

The decline in necropsies requested in hospital has resulted from a combination of factors, which must include negative perceptions of both the medical profession and the general public. Matters have been made notably worse by an overburdening consent process and media hyped exploitation of issues relating to the retention of organs. The lack of willingness of the public and medical profession alike to acknowledge the continuing benefit of hospital necropsies, as shown by their widely understated decrease in number, should be addressed urgently. This is especially true if there is to be reassurance in a system where discrepancies and medical errors can be discussed, with clinical performance monitored openly.

In neither the paper by Vincent nor the editorial by Alberti is the role of necropsy mentioned in the collection of corroborative evidence in reporting medical error.[5] Vincent says that 8% of medical errors contributed to death, with data obtained by review of patient notes alone. Before instigating the huge and expensive administrative schemes suggested, it seems foolhardy to let the gold standard of necropsy disappear. If the current decline continues, we risk losing a crucial tool for auditing clinical practice and accurately compiling epidemiological data.

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Is it time to abandon the term mental illness?

Mental illness is descriptive term

EDITOR—Some years ago the disciples of Thomas Szasz, R D Laing, and others in the antipsychiatry movement were telling us that we should free ourselves of belief in "the myth of mental illness." Now we have Baker and Menken telling us that the term mental illness must be abandoned, not because it is a sociopolitical construct, as Szasz et al would have us believe, but because it is an erroneous label for brain disorders.[1]

Surely the term is neither of these things: it is simply a descriptive term that recognises that the primary disturbance in someone who is mentally ill will be in high level brain functions, such as cognition, volition, orientation, comprehension, reasoning, and affect. It is thus distinct from the functional disturbances in conditions such as Parkinson's disease or epilepsy, where the primary disturbance is at a more mechanical level. One could draw an analogy in terms of a comparison between osteoarthritis and a fractured neck of femur: both can affect the legs but do so in different ways and for different reasons.

Of course major mental illness is likely to be due to brain disease and of course it is wrong to stigmatise those who have mental illness, but in acknowledging this one should not bow to the latest form of political correctness. Mental illness is a useful descriptive term, and I can see no logical reason why it should not be as useful in the future as it has been in the past.

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Terminology should focus less on mind and more on matter

EDITOR—I have dissociative difficulties and thus have a personal interest in the term mental illness. I think that Fisken's rapid response commenting on Baker and Menken's personal view [www.bmj.com/cgi/eletters/322/7291/937#EL1, and published here as letter above] is fair; the term is an accurate descriptor. But the fuller meaning of Baker and Menken's view that it should be abandoned is bypassed.[1]

Although the most erudite and objective among us will understand that the phrase refers to problems in higher brain functions and that physiological dysfunction is often involved, this is not how it is understood by many people. Within the lay community stigmatisation is strongly evident and harmful. To many people the term mental illness still means a moral flaw or a weak character simply because it suggests the intangible mind (if it's a problem of the intangible mind then there is no physical problem, the reasoning goes, and all that's left is the person as a self, as a personality). Although some connection between brain and mind is

understood, if a problem is distinguished as "mental" rather than as "of the brain" then surely this means that it is not of the brain? Otherwise why would it be so characterised? I describe a simplified and extreme position, but it is nevertheless real. But should the academic and clinical community bow to this ignorance and change what is, after all, an objective description? I am reluctant to endorse that path. Rather than bow to ignorance we can recognise the reality of the patient's reception in the broader community and attempt to educate that community. A patient with a physiological problem should not be stigmatised by a label that many take to indicate a difficult personality or malingering if other accurate terminology is available.

Perhaps the best answer is to avoid the term mental illness wherever any use of brain illness would suffice or is substantiated by evidence. We do not refer to people with cryptococcal encephalitis as mentally ill even though they will show symptoms of higher brain function irregularity. With recent evidence consistently indicating physiological abnormality in mental illness we would be wise to prefer terminology that focuses less on mind and more on matter.

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Actions speak louder than words

Editor—I was astonished that Baker and Menken would write something as ill considered as their personal view proposing that it was time to abandon the term mental illness—and, too, that the *BMJ* would publish it.[1] I conclude that the publication of such a statement is an ominous symptom of scientific medicine's willingness to abandon logic for demagogy, and truth for social betterment.

If disease is defined in materialistic terms then, from a logical and scientific viewpoint, there can be no such thing as a mental illness. Accordingly, I suggested, more than 40 years ago, that mental illness is a myth (the behaviours called mental illnesses are not diseases). Some phenomena called mental illnesses are brain diseases (toxic psychosis) and some are not (pyromania).[2 3]

The authors state, "It is harmful to millions of people to declare that some brain disorders are not physical ailments." Claiming that some brain disorders are not physical ailments would be asserting a falsehood harmful to truth, not necessarily to people.

If mental illnesses are brain diseases then treating them by psychotherapy is quackery. Treating brain diseases, such as glioblastoma or subdural haematoma, with talk therapy would be medical malpractice of the worst kind. As well as regarding mental illnesses as metaphorical diseases (not diseases), I have also maintained that psychotherapies are metaphorical treatments (not treatments).[4]

The authors declare, "Neurology and psychiatry must end the 20th century schism that has divided their fields." They ignore the fact that neurologists treat people with parkinsonism with the patients' consent, whereas psychiatrists treat people with paranoia against the patients' clearly expressed wishes. People with bona fide brain diseases are not imprisoned in neurological hospitals; nor, when they commit crimes, are they declared not guilty by reason of neurological illness. Baker and Menken avert their eyes from psychiatrists' paradigmatic interventions—depriving innocent people of liberty (civil commitment) and excusing guilty people of responsibility (the defence of insanity).

The authors approvingly cite the claim of James F Toole, president of the World Federation of Neurology, that "brain dysfunction among world leaders [is] one of the greatest threats to global peace, and therefore the health of populations." This is a manifestation of a deplorable and dangerous penchant for pathologising politics. I fear that we are in the process of building a therapeutic state—a modern, scientistic totalitarianism, resting on replacing democratic-political governance with pharmacratic-bureaucratic regulation.[5]

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Psychiatrists need skill in both minds and brains

Editor—Baker and Menken argue that the term mental illness should be replaced with brain illness.[1] This is not the first time that neurologists have attempted to reclaim some of the territory of nervous disorders they have lost to psychiatry over a century or more. In the 1970s the neurologist Henry Miller described psychiatry as "neurology without physical signs." Perhaps one should view such retrogressive and reductionist suggestions charitably as a product of the erosion of neuroanatomical skill by neuroimaging, so that neurologists and their clinical skills are no longer regarded with the awe that was once their due.

Baker and Menken justify their suggestion on the grounds that it would reduce stigma. Certainly stigma is a major problem in psychiatric illness, but if neurologists were to question their patients with epilepsy, movement disorders, or paraplegias they would find that it is far from confined to psychiatry. Eliminating the notion of mind would mean ablating one of psychiatry's most essential contributions to medicine—the idea that illnesses, especially mental illnesses, cannot simply be understood as the malfunctions of a biological machine but are equally subject to intention and desire, and that people can actively contribute to their own health or disease rather than relying passively on doctors and their medicines.

The bankruptcy of a narrow biomedical approach is shown in the authors' suggestion that "brain dysfunction among world leaders is one of the greatest threats to global peace." This ignores social, political, economic, cultural, and interpersonal factors—all precisely the province of mind rather than brain.

We are beginning to understand the two-way interactions between brain and mind. We know that emotional trauma can have direct effects on the brain, and brain dysfunction can affect the workings of the mind. By championing the concept of mental health and ill health, psychiatry helps avoid the twin dangers of a mindless medicine and a brainless psychiatry. In most mental illnesses brain processes are either poorly understood or only trivially relevant. Psychiatry would be ill served by the simplification that Baker and Menken advocate. We need skill in both minds and brains.

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Reducing deaths among drug misusers

General Medical Council may be destroying the British system

Editor—Gabbay et al argued that tightening controls by extending the licensing system to all controlled drugs is likely to bring about adverse consequences.[1] Drug related deaths will increase in number rather than decrease. The hidden message in the editorial, written by four doctors experienced in treating addiction, was equally important. There is a growing and ultimately destructive schism in the United Kingdom's medical profession regarding the proper controls on doctors treating drug misusers.

On the one hand, there are doctors such as Gabbay et al who argue that the British approach to treating drug misusers has enduring value. This means that the judgment of an individual doctor should be trusted in tailoring treatment for each patient. Thus, each drug misuser is treated as a patient with unique needs, and drug misusers in general as constituting a heterogeneous, not a homogeneous, population. Trust in doctors extends to decisions to prescribe narcotics. Doctors operating from the British system assume that there is no specific treatment of drug abuse. This realistic concept has encouraged experimentation and innovation by British doctors, including general practitioners, in taking on and treating difficult patients. Medical practice based on the British system has worked to hold down the spread of addiction and disease.

On the other hand, there are doctors wielding considerable power in the drug misuse establishment who view the clinical freedom accorded by the British system as both an anachronism and a threat to public health. This politically dominant group of doctors has, over the past several decades, imposed increasing control on the clinical freedom of doctors abiding by the British system. This control has been implemented through several editions of clinical guidelines and also through an activist role for the General Medical Council: the GMC disciplines and erases selected doctors who abide by the approach of the British system to helping drug misusers. These actions by the GMC have been viewed as arbitrary and unpredictable by respected medical experts. Many doctors are afraid to accept drug misusers as patients because they worry about being irrationally persecuted by the GMC.

The planned extension of the licensing system to all controlled drugs would be an illness masquerading as a cure. Stricter controls signal the death knell for a humanitarian and efficacious system of addiction prevention and treatment. The GMC has a duty to keep that tragedy from happening. Yet it almost seems intent on creating it.

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Competing interests: AST is an adviser to John Patrick Hickey of Cornwall; an expert witness to the court in various legal matters involving Dr Hickey.

1 Gabbay MB, Carnwath T, Ford C, Zador DA. Reducing deaths among drug users. *BMJ* 2001;322:749-50. (31 March.)

Standard of care in Britain was not addressed

Editor—Despite criticising American treatment practices, the editorial by Gabbay et al does not address the standard of care in Britain.[1] The United Kingdom reportedly has the highest death rates from opioids in Europe, at 22 per million,[2] and a proportion of these are from methadone.[3]

Other European countries have reported substantial decreases in such fatalities in the 1990s. Most have used both carefully prescribed opioids as well as other public health measures.[2] It is unwise to prescribe unsupervised supplies of a strong medicine to unstable addicted patients. Doses may be taken early, they may be injected, or they may be used by others because of theft or on-selling. Most published addiction outcome studies have employed supervised dosing. With increasing stability, less frequent attendance is necessary and more flexibility possible. Despite widespread circulation of the British dependency guidelines,[4] self regulation has apparently failed to encourage British doctors to follow the advice on supervision and dose levels. To avoid cravings, most dependent patients require 60-120 mg methadone daily.[4] Initial doses, however, should not be higher than 40 mg, with prompt increases after careful assessments in the following days to avoid treatment dropouts. Inadequate dose levels, a lack of supervision, and poor access to treatment can all restrict treatment outcomes.

Such deficiencies in the United Kingdom may have sabotaged a potentially positive public health achievement. This could yet be attained, utilising the twofold British attributes of the profession's freedom to prescribe and universal access to treatment under the NHS. Although clinic induction is ideal for severely dependent patients, it is possible that general practitioners, with adequate support, can implement such treatment successfully, as practised in Scotland for over a decade.[5] After stabilisation, any sympathetic, knowledgeable general practitioners should be able to manage patients having methadone maintenance treatment by using community pharmacies and established professional support systems. In rejecting government interference for dependency management, these authors confuse evidence based treatment (for example, methadone maintenance) with the practice of continuing to prescribe to known addicts under harm reduction principles (as for benzodiazepines, stimulants, and perhaps cocaine). Some have termed this "the British system," although this ambiguous term should be discarded.

As with heart disease, diabetes, or depression, patients with dependency deserve a careful history and physical examination plus special tests if required. Predictably, favourable outcomes should follow judicious prescribing when necessary, with appropriate safeguards and psychosocial supports. The threat of licensing should encourage British doctors to re-establish themselves as providers of best practice in the field of addiction as they have long done in other fields.

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Authors' reply

Editor—Mortality and morbidity of problematic drug users reduce substantially when the users are receiving treatment.[1] Methadone treatment on the basis of reduction of harm, in which the use of illicit drugs is tolerated, is strongly related to decreased mortality from both natural causes and overdose.[2] Any intervention that prevents people who want and require treatment for their drug problem from receiving that treatment is thus likely to be detrimental to their health and fails to address the impact of their addiction on society.

Licensing could act as a barrier to providing readily accessible treatment. We are pleased that Trebach agrees with our view. Byrne has, however, missed this point in our editorial. Byrne focuses his reply on methadone diversion, supervised consumption, and opiate related deaths. These are not the main point of our editorial and would require another article to be discussed fully.

We do not believe that reducing methadone diversion would have a significant impact on opiate related deaths. We believe that supervised consumption has benefits and costs, neither of which has been properly evaluated. Its advantages must be balanced against other important issues such as accessibility of service, retention in treatment, and convenience for the patient.

The United Kingdom has the highest death rate from opioids in Europe because it also has the highest rate of consumption of opioids. Recent figures from the UK's Office for National Statistics show the number of methadone deaths falling for the past three years, with deaths from heroin continuing to rise. The number of cocaine related deaths is continuing to rise steeply, which suggests that deaths are a result of increasing misuse, not irresponsible prescribing.

We do not advocate inadequate dose levels or poor access to treatment—quite the reverse. We hope for a wider range of treatment options, the steady improvement of services through clinical governance, and a wider availability of high quality treatment. Supporting general practitioners through shared care schemes and training, rather than encumbering them with additional bureaucratic mechanisms that do not improve care, will achieve this.

We agree with Trebach that the population using drugs is no more homogeneous than any other group of people with a particular condition, and we treat them as such at our peril. We also agree with Byrne that all patients with a dependency deserve a careful history and examination, and favourable outcomes should follow judicious prescribing when necessary, with appropriate safeguards and psychosocial support. Where we disagree is that licensing will help this process. Prescribing policies for people who undertake problematic drug use should also be supported by available evidence. What little available evidence there is suggests that limiting treatment options and availability through further licensing restrictions will have adverse effects on the quality and availability of evidence based treatments. Furthermore, there is no evidence to support the notion that restricting prescribing in this way will necessarily increase community and patient safety.

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Competing interests: None declared.

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Predictive genetics and predictive morphology have certain similarities

EDITOR—Evans et al may be exaggerating the distinction between genetic predictive testing and predictive testing based on various phenotypic abnormalities.[1] Phenotypic predictive testing, such as grading the histopathological abnormalities in breast ducts, is often performed on tissue from clinically well patients.

Genetic and histopathological tests use different methodologies to search for the presence of cellular structural abnormalities that are useful for risk assessment in asymptomatic patients. Both types of test, though, carry a degree of uncertainty about whether an illness will develop, when it will develop, and how severe it will be. As with genetic testing, the utility of histopathological predictions varies widely for different illnesses, depending on factors such as the severity of the predicted illness and the costs of early intervention.

Predictive testing has been carried out for decades in several areas of pathology, including breast biopsies showing non-invasive morphological deviations from normal, prostate biopsies showing tiny cancers, colon biopsies showing adenomas, and skin biopsies documenting dysplastic changes in naevi. Geneticists must not conclude that because they use new methodologies they should ignore the substantial experience that has already been accumulated with predictive testing.

Pathologists have found that the speed with which predictive tests become widely available tends greatly to exceed the speed with which risk data from externally valid studies become available. In addition, both patients and doctors often misunderstand the uncertainty and complex cost benefit calculations that arise out of risk data and will conclude that a positive test result shows the presence of a life threatening disease that should be treated aggressively. One consequence of the artificial distinction between genetic and histopathological prediction is that doctors—who require additional training before explaining the implications of genetic predictive testing to patients—are mistakenly thought to be adequately trained to explain equally complex histopathologically based prediction.

One substantial difference between predictive genetics and predictive morphology is that patients who undergo a histopathological predictive test often have not decided to obtain a predictive test result. Patients who have breast biopsies are seldom aware of the complex range of non-invasive findings that might be found.[2]

Finally, I have a word of advice for geneticists: don't label a predictive mutation a cancer gene, or you will create an almost intractable communication barrier similar to that which pathologists have seen with non-invasive breast cancer.

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Greater awareness and education are needed to help prevent acute mountain sickness

EDITOR—Cerro Aconcagua (6962 m) is the highest mountain on the South American continent. Despite being described by early explorers as "the most gruelling ordeal known to climbers,"[1] in 2000-1 it was visited by 4197 people.[2]

Aconcagua is usually climbed by one of two routes, with over 80% of visitors favouring the "normal route." This is a straightforward route and attracts many trekkers with little experience of altitude. The second approach is usually chosen by more experienced climbers and mountaineers. Last year, for the first time, medical posts along both routes were staffed by doctors trained in high altitude medicine and supported by a helicopter rescue service provided free to permit holders.

This season, 130 of the 839 visitors given permits for the route chosen by more experienced climbers and mountaineers attended the medical post at the base camp. Thirty three had symptoms of acute mountain sickness that warranted treatment and descent. A further 14 had high altitude pulmonary oedema and three had high altitude cerebral oedema and were evacuated to hospital by helicopter (JS Diaz, Plaza Argentina base camp medical logbook, 2000-1 season). Although statistics are not yet available for the normal route, local reports suggest that up to three doctors attended at the base camp during the busiest months, seeing up to 40 patients a day.

These figures do not include the many visitors who continue to climb with symptoms of acute mountain sickness, not recognising or ignoring their importance. In commercial trekking groups, guides often cannot allow sick clients to descend, rest, and return later to the main party. Inflexible itineraries mean that clients either have to abandon their trip prematurely or hide their symptoms in order to continue.

Climbers and trekkers must be aware of the need for acclimatisation and the problems of acute mountain sickness before spending time at altitude. They need to understand the symptoms and treatment (including use of acetazolamide as prophylaxis) and the life threatening consequences of high altitude pulmonary oedema and cerebral oedema. The Lake Louise scoring system for acute mountain sickness, found in several medical handbooks and local guides, helps with this.[3]

Large, inexperienced parties can employ an expedition doctor to help tailor acclimatisation, recognise and treat early symptoms, and manage their life threatening consequences. Doctors undertaking this work will be liable for care they provide and will be expected to have training in high altitude medicine. At present no formal accreditation exists in the United Kingdom.

Only by improving awareness and education among health professionals and the growing numbers of people visiting mountainous regions can the inherent dangers of altitude be reduced.

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Checklists for improving rigour in qualitative research

Never mind the tail (checklist), check out the dog (research)

EDITOR—Barbour's article is tantalising and mystifying in equal measure.[1] She is right to counsel qualitative researchers from shielding behind a protective wall of checklists and quasi-paradigmatic research techniques—although the same should be levelled at epidemiologists, statisticians, and health economists, with all researchers being charged with the responsibility of ensuring that the research tools and analysis fit the question to be addressed. Yet, and this is where the tantalising becomes mystifying, she twice (once in the second paragraph and again in the last) tells us that our research strategies need to be informed by the epistemology of qualitative research, without giving us an inkling as to what she believes this to be. Although she rightly espouses the importance of context for qualitative researchers, she denies us the context in which to assess her own critique.

As a champion of applied social science, particularly action research and qualitative research in public health, I think that the biggest threat to this growing area of work is not so much overadherence to prescriptive checklists and sampling strategies but rather the over-reliance on self reports and verbal representations of the world. For many, qualitative research has become synony-

mous with the semistructured interview, the self report, and the ubiquitous focus group. The roots of qualitative research are in anthropology and ethnography, where direct observation of events is central. Much of the contribution of qualitative research in the understanding of social aspects of health issues, notably HIV/AIDS, has been through direct observation of the nuances of social behaviour. Questioning the validity of checklists and the prevailing methodological orthodoxy in qualitative research is useful, but of greater relevance is the need to promote (and teach) a more observational paradigm for qualitative health research.

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Including personal reflections might help

Editor—Barbour has been brave enough to place her head above the parapet.[1] Over the past few years, researchers who use qualitative methods have received greater esteem, obtained funding from sources previously out of bounds, and published in journals that may have dismissed their efforts a decade ago. As a result it has been in the interests of many researchers to keep some of these views to themselves. I believe that Barbour has done a service to the integrity of the methodology of qualitative research, but she stops short of offering many solutions. I sympathise. Solutions to any of the problems highlighted are themselves likely to be added to the checklists. For example, Barbour seems to advocate the "constant-comparative" method of analysis, but should this simply be added to current lists?

The constant search for rigour simply results in longer and longer checklists. One only has to compare the assessment and design guidelines for clinical trials 20 years ago with those of today to see the point. Will qualitative research go the same way? Perhaps it does not matter, and longer checklists simply reflect advances in knowledge of the scientific method. My concern, however, is that, although checklists are a quick and easy way of facilitating the appraisal of a paper, they simply set up rules that researchers play to and get around, in rather the same way that they find ways of getting round tax legislation.

Historically, qualitative researchers have addressed this issue, not simply through technical fixes but the more important process of documenting reflection. They constantly reflect on the research question, their role, attitudes, feelings, the impact of the researcher on the people being studied, and so on. Although the personal reflections of the researcher may seem rather out of place in many academic journals, it would at least provide a way of covering some of the known and unknown blind spots of checklists. This applies to quantitative as well as qualitative research. In reading the report of a clinical trial, do we really know everything that happened? For example, patients were not told what drug they were receiving but were they really "blind"? Rigour may lie in the unreported details, peculiarities, and idiosyncrasies of studies as much as in the overarching issues contained in a checklist. The challenge is finding a way of making it possible and acceptable to report these openly.

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1 Barbour RS. Checklists for improving rigour in qualitative research: a case of the tail wagging the dog? *BMJ* 2001;322:1115-7. (5 May.)

Diagnosing genitourinary chlamydial infection

Vaginal swabs alone may not be sufficient

Editor—Gilson and Mindel in their article on recent advances in the management of sexually transmitted infections emphasised some important diagnostic issues.[1] As Gilson and Mindel say, many studies have shown that DNA amplification tests are now the gold standard for the diagnosis of genital *Chlamydia trachomatis*. But we are concerned with the unreferenced statement that a vaginal swab is a better alternative for the detection of genital chlamydial infection.

We found two studies that have examined the utility of vaginal swabs, collected either by healthcare personnel or patients themselves.[2 3] Both found high sensitivity for vaginal swabs, but this was matched by the sensitivity of sampling both urine (as a surrogate for the urethra) and the cervix. In some cases of genital chlamydial infections (cervical swab positive) vaginal swabs were negative.[2] In women the sensitivity of testing for *C trachomatis* by the polymerase chain reaction is increased by about 12% if both cervical swab and urine specimens are examined as opposed to urine alone.[4] This approach is expensive, however, particularly considering the comparatively high cost of the tests compared with enzyme immunoassays.

We found that combining a cervical swab with a urine specimen in the clinic setting is acceptable for testing for genital *C trachomatis* infection by the polymerase chain reaction[5] and has the potential to increase further the cost effectiveness of DNA based screening for genital infection with *C trachomatis*. Thus, this approach has the advantage of sampling both the main sites of genitourinary chlamydial infection and being more cost effective. Also, the problem of DNA amplification inhibition by endogenous substances present in urine, and more commonly in cervical secretions, is not increased by combining the two sample types.[5]

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Authors' reply

Editor—Wilcox and Subramanian question our referring to a vaginal introital swab as a better alternative for chlamydia testing. We were considering developments in testing in the context of chlamydia screening, which will mostly be undertaken in settings other than specialist clinics. In clinics for genitourinary medicine or sexually transmitted disease, an examination by speculum is the norm and allows other sexually transmitted diseases to be diagnosed. A cervical sample can be taken easily, with or without other samples. Sampling from multiple sites may increase the sensitivity of detection of chlamydia infection. Although combining a cervical swab with a urine sample, as suggested by Wilcox and Subramanian, is of interest, it requires further validation and is still restricted to those situations where a cervical sample can be collected easily.

In situations where examination by speculum is either not feasible or may be declined, alternative, self collected samples such as a vaginal swab or urine sample have obvious advantages. Testing a vaginal swab sample alone has a similar or higher sensitivity than urine testing alone.[1–3] Compared with a urine sample, vaginal swabs have the advantage that they can be sent to the laboratory in transport medium at ambient temperature[4] rather than having to be kept cool, and they are simpler to process. They have recently been shown to be highly acceptable to adolescents in a high school.[5] Nonetheless, further work is required to establish the most practical and cost effective testing strategy for chlamydia testing in the community.

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Accessing emergency test results on ward computers

Results indicating that lifesaving treatment is needed should probably be telephoned

Editor—Kilpatrick and Holding report an audit of the introduction of computer terminals to two wards so that emergency test results could be accessed.[1] They have opened the debate on the presumption that computerised results are superior to telephoned results for emergency tests. Their study highlights several important issues.

In each of the two busy clinical areas audited only one terminal could be used to access results. This may have led to limited access to results at certain times. The audit was carried out one month after the computer terminals were activated. Whether staff had a period of learning and familiarisation before using the system is unclear. Also, teething problems would have to be overcome before such a new system was implemented. It would be interesting to see the results of an audit carried out today.

The attitude of junior doctors in practice must be considered. With the high workloads and numerous patients, they are often anxious to get through the admissions that need to be seen rather than to formulate a differential diagnosis, arrange tests, and, most importantly, review the results requested. Frequently, inappropriate blood tests are requested for patients seen in accident and emergency departments, or those requested are not noted down or the results examined, so that effort is duplicated.

Possibly the reporting system used in this audit could be improved. Abnormal results could be highlighted by a colour, not an asterisk. The system used is not particularly user friendly for novices or inexperienced staff: this could be improved with more modern software facilities.

In many centres results are not routinely telephoned; in some hospitals paper is obsolete for reporting results. We would suggest that results indicating that lifesaving treatment should be initiated rapidly should always be telephoned—otherwise a system failure will result.

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Electronic transmission is generally the way forward

Editor—I was disappointed at the negative tone of Kilpatrick and Holding's paper on using computer terminals on wards to access emergency test results.[1] As a chemical pathologist and someone who often has to contact the laboratory for results, I have no doubt that electronic transmission of all authorised results to computer terminals in wards, clinics, and general practitioners' surgeries would be a great advance. This assumes that this method of transferring results is part of a strategy that recognises the laboratory's responsibility to interact with clinicians when abnormal results are obtained.

Anybody who contacts a busy NHS laboratory for results regularly will agree that this is frustrating and time consuming. Equally, laboratory staff are upset, and their efficiency is compromised, when their work is repeatedly interrupted by phone calls for results—often results that have already been telephoned, faxed, or delivered in report form. Computer terminals on wards can be used to access all results, both routine and emergency, and those pertaining to the current and previous patient episodes. The electronic transmission of results greatly improves efficiency and reduces frustration.

I was surprised to read in Kilpatrick and Holding's paper that some laboratories had dispensed with the telephoning of results in the knowledge that they can be accessed on the wards by clinicians at their convenience. Surely the communication of abnormal laboratory results is too important to be left to chance. The results of this audit were, in my opinion, entirely predictable. It is not all surprising that the busy staff in an accident and emergency department and an acute medical admissions unit did not look at a considerable proportion of laboratory results.

The authors mention that there might be legal and financial consequences for the clinician responsible and the hospital trust if it transpired that harm to a patient occurred because abnormal results had not been viewed. I believe that the onus lies with the senior laboratory staff to ensure that all abnormal results outside agreed limits are telephoned immediately, preferably to the doctor looking after the patient. With this safeguard in place, I would be much more upbeat than the authors and conclude that electronic communication of laboratory results is far superior to traditional communication methods.

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Introduction of electronic communication alone would not improve clinical care

Editor—The audit by Kilpatrick and Holding highlighted two important issues of interest to the NHS[1]: the need for the evaluation of new technology and to understand and redesign systems.[2] The central law of improvement says that every system is perfectly designed to achieve the results it achieves.[3] When laboratory staff telephone results to a clinician it prompts a review of the patient. The new system removed this prompt, and in this audit over a third of emergency results were never seen before they were printed.

It is interesting that the laboratory staff were not satisfied that they had transferred the clinical responsibility for an abnormal result to another person so that he or she could act on it. This could have medicolegal implications. The difference of synchronous (telephone) and asynchronous (messaging) communication is a fundamental issue. How existing systems have evolved over time, the behaviour of clinicians, and system features such as the doctor's shift arrangements are other design issues.

When communication systems need to be improved there must be, as Berwick pointed out, a change of a system, not a change within a system.[3] Direct booking systems for general practitioners are now being presented as solutions for current NHS referral problems.[4] These initiatives are essential. Their full implications are apparent only when the whole system is in place. It is time to look at how these systems are developed before it is too late.

Technology is sufficiently advanced to make real changes to the lives of patients. Without a holistic approach to the development of information systems, however, it is unlikely to achieve this.

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Written information for treating minor illness

Authors did not consider patients' view of information they received

Editor—Two papers conclude that information booklets are unlikely to influence consulting rates.[1 2] One criticism of these papers that we have is their failure to report on the quality of the booklets used and how this might be judged, or to comment on the factors other than factual content that might affect patients' reactions to the materials. This failure comes despite the resources provided by the NHS Centre for Health Information Quality[3] and despite comments made in an accompanying editorial.[4] More important, however, is the way in which patients are characterised by these papers.

The outcome chosen in both studies—that of reducing consultation—represents a prioritising of professional and biomedical agendas over the patients' agenda, in common with many previously published papers of this type.[5] The preoccupation with effecting behavioural changes in patients implies that the main purposes of printed information are to manipulate patients and remedy their information "deficiencies" rather than to provide a resource for patients.

Little et al conclude that the failure of their booklet to change consulting behaviour gives rise to questions about whether such booklets justify the use of NHS funds.[2] Even though their research showed that patients found the materials useful and were more confident in managing illness as a result of them, the researchers place no value on these outcomes of satisfaction to citizens. Heaney et al do seem to recognise that a more sophisticated approach to understanding the reflexive nature of patients' responses to information is required, but they did not investigate this.[1]

A more appropriate approach would have emphasised the role of booklets as resources for patients and in creating responsible partnerships with health professionals. It is disappointing that such patronising views of the role of printed information for patients continue to be published—and particularly when they are reinforced by an accompanying editorial with the subheading "Alone, [such information is] not very valuable, but we shouldn't expect it to be." Such studies do a disservice to everybody, not least those patients who participated in them.

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Patients find printed advice written with easy-to-find format helpful

EDITOR—Heaney et al's and Little et al's papers both report that health information on minor illnesses did not reduce the number of consultations for those illnesses.[1 2] Colleagues and I came to the same conclusion when our health authority widely distributed a health leaflet to the local population. It covered some 30 conditions and, although comprehensive, was difficult to navigate. Finding a specific condition was fairly confusing, especially for a member of the public trying to manage an urgent situation.

Subsequently, in 1997 Tilehurst Surgery undertook an audit (sponsored by Berkshire Health Authority), in which we specified three childhood conditions (earache, fever, and diarrhoea and vomiting) which constituted one third of requests for out of hours visits and surgery consultations. The participating mothers (105) were surveyed in the school grounds and the surgery waiting room about their confidence in managing the three conditions. They were then given a leaflet explaining the management, and the survey was repeated eight (winter) weeks later. We found that confidence in the mothers' ability to manage the conditions improved by up to 60% with empowerment by authoritative medical advice on a narrow range of conditions.

If the advice concentrated on three or four commonly occurring conditions in a quick and easy to find format, the outcome might be more favourable than the findings reported.

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Smear tests: déjà vu

EDITOR—I refer to Goodman's review on cancer screening and misunderstanding by the media.[1] In an editorial on the rising risk of litigation in Pap smear interpretation, DeMay gives the following analogy: You are a firefighter. You arrive at a burning house and hear screaming. There are 10 people inside. You run in and save nine lives. Despite your best efforts, one person perishes. So, should you be cited for heroism—or indicted for homicide?[2]

The tabloids reporting the Leicester smear audit seem to have made up their minds in favour of the latter. They do not go on to explain exactly how the zero error standard can be achieved. Many of the improvements recommended after previous scandals were presumably in place in Leicester for at least part of the period covered by the audit. New technology has yet to prove itself in large trials. There are failures in all systems, and all we can do is attempt to reduce them but we are unlikely to eliminate them. As DeMay proceeds to say, not only are mistakes normal, they may even be necessary for the success of this screening test. Trying to eliminate these "mistakes" could make the test so costly as to be unaffordable.

The cancer czar should be congratulated for trying to defend the Leicester results, but I think that this is too little, too late. There have been numerous opportunities in the past to explain in simple terms to the public about the risks and benefits of screening.[3 4] They are rarely exploited, hence yet another scandal.

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Conscription of children in armed conflict: clarifications

EDITOR—My coauthor and I read the clarifications requested on bmj.com by readers of our article, among them the correspondence by Bandaranayake, entitled "Conscription by whom."[1 2] Our study was done among children conscripted by armed groups (rebel "armies") in Sri Lanka. We are sorry that readers have interpreted army as that of the government. There is no direct evidence that the Sri Lankan government recruits children. We had no intention to twist the responsibility.

This article was sent some time ago, and some changes have taken place in the stand taken by the United Nations. At the General Assembly held on 26 June 2001 the optional protocol to change the Convention of the Right of the Child on the involvement of children in armed conflict was successfully adopted after several years of rejection.

Of special interest is article 3[1], which states that state parties shall raise the minimum age of voluntary recruitment of persons into their national armed forces from that set out in article 38[3], of the Convention of the Rights of the Child, taking account of the principles contained in that article and recognising that under the convention persons under the age of 18 years are entitled to special protection.

Article 4 mentions that armed groups that are distinct from the armed forces of a state should not, under any circumstances, recruit or use in hostilities persons under the age of 18 years. We are sorry for any misunderstandings created.

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