Letters

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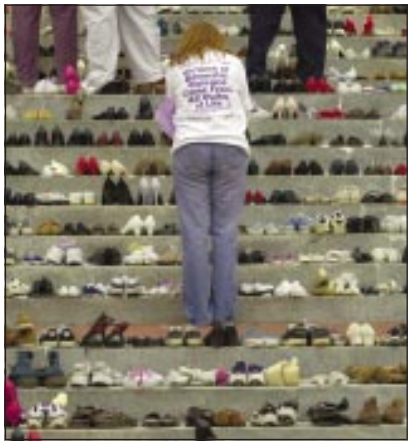
Screening for domestic violence



AP/ED REINKE

Hundreds of pairs of shoes belonging to victims of domestic violence, placed on steps of capitol building in Frankfort, Kentucky, October 2002

Cultural shift is needed

EDITOR—The systematic review of Ramsay et al makes a valuable contribution to the debate on whether to screen for domestic violence.[1] This debate also needs to consider some of the wider cultural issues influencing the acceptability of the existence of domestic violence in society.

The taboo of recognising, acknowledging, and bringing into the open issues surrounding domestic violence has led to resistance by the health profession in dealing with what is increasingly becoming understood as an important influence on the health of women.[2–5] Domestic violence is not unique: the recent history of the denial of the existence of child sexual abuse has undergone a major societal and cultural shift in the past 20 years, resulting in a heightening of awareness and recognition by health professionals and society at large.

A similar cultural shift is starting to take place in attitudes towards domestic violence—for example, with its inclusion within the community safety plans of local authorities. Although clear needs exist for research in determining the effectiveness of interventions for the prevention of domestic violence, part of the resistance towards screening for domestic violence seems to be related to negative attitudes held by health professionals.

To address this, more work needs to be done in assessing the training needs of health professionals in relation to domestic violence. Furthermore, the approach to dealing with domestic violence in the health sector may benefit from creating an environment whereby health professionals are seen not to support the use of violence as a means to deal with interpersonal conflict in any setting. A stronger emphasis needs to be placed on becoming a part of the cultural shift towards non-tolerance of violence in relationships in a similar way that health professionals have been able to contribute to the prevention of child abuse.

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Screening for partner violence makes a difference and saves lives

EDITOR—Ramsay et al say that implementation of screening programmes for domestic violence in healthcare settings cannot be justified, although such violence is common with major health consequences for women.[1] Many healthcare organisations have professional statements on violence from intimate partners and support routine screening, including the American Medical Association, American College of Obstetricians and Gynecologists, American Academy of Family Physicians, Family Violence Prevention Fund, and Physicians for a Violence-free Society. Partner violence continues to gain recognition by the healthcare community as one of the most prevalent current public health issues.

Screening for intimate partner violence in medical settings is effective in identifying victims and providing interventions.[2 3] People are not offended when asked about current or past violence in their lives,[4 5] although research design is sometimes suboptimal.

The attitudinal surveys quoted by Ramsay et al are taken to mean that women do not favour screening. We calculated percentages by using the criteria provided by the authors, and when all four studies were combined 708 out of 1117 (63%) patients favoured screening. We therefore conclude that people favour screening.

Even if patients disliked screening, we do not accept the argument that it would be detrimental or possibly harmful to screen for partner violence. A comparison can be made historically with screening for cigarette use.

Many smokers do not favour screening. For years the harmful effects of cigarette smoking were not documented by observational studies. Now, well done, long term epidemiological research has documented multiple adverse outcomes associated with cigarette smoking and universal screening for cigarette use is standard.

Similarly, evidence of the poor physical and mental health outcomes of people exposed to violence continues to grow. Universal screening for violence must be taught as a healthcare imperative before millions more die from its adverse effects, as happened with smokers in the 1950s and 1960s.

Ramsay et al did not find any randomised controlled trials of interventions in healthcare settings to improve outcomes. In the absence of optimal research, we recommend universal screening. We challenge medical and sociological researchers everywhere to conduct government funded research to follow up people throughout their lifetimes so that the effects of screening, long term health consequences, and death rates from intimate partner violence can be brought to light.

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Doctor's duty of confidentiality may not be in patient's or community's interest

Editor—With reference to the paper by Ramsay et al on screening for domestic violence, I cannot see that asking about domestic violence is a screening test as there is no agreement on effective subsequent intervention, even if there is a statement confirming domestic violence.[1]

The difficulty in these cases is the conflict between the doctor's duty of confidentiality to the patient and the doctor's common law responsibility to report a crime that has been committed. Currently the duty of confidentiality is ranked far higher than the doctor's duty to society.

If we as a society are to tackle domestic violence it needs to move from being treated by doctors, the police, and legal services as a personal matter and instead be treated as seriously as any other crime. In particular the police and prosecuting authorities need to stop asking victims whether they want the prosecution to go ahead. If the crime has been committed the prosecution should go ahead anyway as a domestic crime strikes as much at society as it does at the immediate victim.

Perhaps it is time to look at whether the doctor's duty of confidentiality is really in the patient's (and the community's) interest in cases such as these. I do not necessarily know the answer to these questions, and I find these areas where law and medicine mix difficult territory to navigate. A clearer map would be useful.

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Safe healthcare system needs to be in place

Editor—Webster and Creedy argue that screening has improved diagnosis and the provision of health services and information to women who experience domestic violence.[1] The domestic violence initiative in Queensland, Australia, found high acceptability of screening, although only 10% of the 6.5% of women disclosing abuse accepted help. A recent case-control study of acceptability of screening to women found complex differences in attitudes depending on the question, and only 54% of abused and 48% of non-abused women supported routine screening.[2]

Furthermore, there have been no studies discriminating between participants who disclose current and those who disclose past abuse. Being accepting of being asked a particular question is not the same as being accepting of or confident about the follow up response.

There is some evidence that brief interventions in the antenatal setting can increase abused women's safety seeking behaviour, which is important in light of the new finding that women abused in pregnancy are three times more likely to become a victim of attempted or completed femicide.[3]

Davidson et al conducted a systematic review of the evidence for which domestic violence interventions work in health systems in the context of possible screening policies.[4] They concluded that no systematic evaluations have taken place, and therefore what could be achieved in the current healthcare setting in the United Kingdom is subject to serious limitations. They noted the challenges of staff time and workload, lack of sustainable training, lack of privacy for women, and the problems when partners are present. Their recommendations included further evaluation and improved provision for confidentiality, privacy, time, links to child protection, and weekend provision of support for victims who disclose, before any major policy shifts. We agree.

No studies have been conducted into the longer term outcomes for women of routine inquiry and disclosure to healthcare providers. Some evidence shows negative outcomes for women disclosing to general practitioners. Limited evaluation of health provider training indicates that problems remain, including negative attitudes from health providers.[5]

Routine inquiry in healthcare settings is a critical goal, but sustainable staff training, safety, and coordination in health systems and interventions with women should be rigorously evaluated so that the outcomes for women are optimal. Case finding should continue, but before routine inquiry is implemented a sensitive, safe, and effective health system needs to be in place so that no harm is done.

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Routine questioning of patients attending emergency departments may help in assessing local violence

Editor—We agree with Ramsay et al that domestic violence is an important problem, with major health consequences for women.[1] But the wider issue of violence in society (not just domestic violence) also deserves consideration, particularly since violent assault is most commonly directed against men.[2]

Police data provide an incomplete picture of violent assault in a community,[3] and public health action to reduce the levels of violence in society will require the use of health service data to identify priorities and monitor change.[4] The routine questioning of patients attending hospital emergency departments is one option for assessing the levels of violence in a local community.

We have recently reported on the acceptability of the routine questioning of patients (both men and women) attending emergency departments in England.[5] In our questionnaire survey of a representative sample of 281 adults, 67% (95% confidence interval 60% to 74%) supported routine questioning about violent assault, with similar levels of support from both men (66%, 59% to 73%) and women (68%, 59% to 76%). The proportion of respondents supporting routine questioning increased with age (52% of 16-24 year olds; 65% of 25-44 year olds; 85% of people older than 45). Overall 89% (85% to 93%) believed that healthcare staff should actively encourage victims of violence to inform the police, and 74% (68% to 80%) believed that health professionals should routinely inform the police—as is the case in some American states.

Patients attending emergency departments support a far more active approach from healthcare professionals in identifying victims of violence than is currently the case in the United Kingdom. But we agree with Ramsay et al that further evidence is required to assess the effectiveness of both population based and individual based interventions intended to reduce violence.

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Balanced approach is needed

Editor—We agree with Ramsay et al that insufficient evidence exists to justify routine screening for violence in healthcare set-

tings.[1] The finding, however, seems to contradict current recommendations of numerous professional organisations in the United States that urge clinicians to screen routinely.[2]

Such contradictions are not uncommon in prevention and intervention research. Recent summaries of evidence on routine screening for both breast and prostate cancer cite concerns that for some people the potential harm of mammography and prostate cancer screening could outweigh the benefits.[3 4] Compared with these two widely used types of screening, the evidence base for violence screening in healthcare settings is far more tenuous.

Ramsay et al base their assessment on studies that do not include a single randomised controlled trial. The Centers for Disease Control and Prevention recently funded a randomised controlled trial to test screening and intervention in primary care settings. Research questions will address screening effectiveness. Do screening and identification decrease violence? Do women use referrals and find them helpful? No single trial, however, will provide all the answers; additional researchers and funders must design and support similar studies. Evaluation will require complicated and expensive methods and replication in diverse settings to gather rigorous scientific evidence.

Meanwhile, recommendations for routine screening will probably remain, and screening for violence will continue. Acceptance of screening may be more widespread in some settings than Ramsay et al's findings indicate. A recent survey of public family planning clinics in the United States reported that among 665 clinicians (75% nurses; 78% responding), 30% always conduct face to face screening for violence with new patients, and an additional 40% reported that another staff member screens new patients routinely (Centers for Disease Control, unpublished data).

Although these screening levels are unlikely to become widespread while scientific evidence is lacking, a balanced approach is needed towards the future. Routine screening should continue when appropriate systems are in place. Alternatively, some institutions and people will choose not to institute routine screening until stronger evidence exists.

Regardless of the approach, however, healthcare providers and institutions cannot ignore the problem of violence and must attempt to intervene whenever violence is disclosed or discovered. Scientific evaluation of screening and interventions must advance, and so must the critical need for institutions and healthcare providers to adopt the most appropriate responses and to receive the most complete training currently known.

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Review is not an excuse for clinicians to ignore abuse

EDITOR—From the responses to the review on screening of women for domestic violence, routine questioning of all women in healthcare settings about abuse is clearly still considered a justifiable strategy by many, but not all, correspondents.[1 2] The interpretation of research evidence and use of public health criteria to judge effectiveness of routine questioning remains controversial.

The correspondence reflects a healthy debate on different public health models for addressing domestic violence. Consensus is strong on the importance of training healthcare professionals in responding appropriately to women who have been abused, with or without routine questioning. There is no disagreement on the urgent need for further research on interventions in healthcare settings.

We are dismayed that the review is being cited in the United Kingdom and the United States as a reason for health services not to develop projects or programmes for women experiencing abuse and as a reason for health professionals not to participate in existing projects. This is emphatically not the message of the paper, which argues that domestic violence is a crucial issue for healthcare professionals.[1]

We believe that all acute and primary care organisations need to develop policies and procedures for responding to domestic violence. Evaluation of these programmes and robust research on interventions based in health services is a priority. We urge the health departments in the United Kingdom to show their commitment to improving the response of the health service to women experiencing abuse by commissioning research in this area.

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Competing interests: If research funding becomes available for health services research on domestic violence, some of us work for organisations that might benefit financially.

Also signed by Katie Cosgrove, Public Health Department, Greater Glasgow NHS Board; Victoria Lavis, University of Huddersfield and Eastern Wakefield Primary Care Trust; Kate Mulley, Victim Support; Jo Nurse, Health Policy Unit, London School of Hygiene and Tropical Medicine; Katrina Smith, Greenwich Primary Care Trust; Judy Shakespeare, Summertown Health Centre, Oxford; Ann Taket, South Bank University; and Judy Watson, Forest/Redbridge Domestic Violence Health Project, London Borough of Camden and Waltham—all on behalf of the United Kingdom Domestic Violence and Health Research Forum.

1 Ramsay J, Richardson J, Carter Y, Davidson L, Feder G. Should health professionals screen women for domestic violence? Systematic review. *BMJ* 2002;325:314-26. (10 August.)
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Summary of responses

EDITOR—Twenty five of the 28 responses to the paper by Ramsay et al were from 25 individual or groups of respondents writing from the United Kingdom (11), North America (10), Australia (2), and Israel and France (one each). Over half of them agree that screening for domestic violence requires a cultural shift in thinking, is necessary, or is a healthcare professional's responsibility.[1]

Screening is seen as necessary but not enough—a start to addressing the problem of violence, if just to raise awareness. It highlights what becomes a matter for private or public concern. It may be the first link in activating the chain of survival, requiring healthcare professionals to refer victims on to suitable agencies.

Kathy Shore from the United States was personally affected and asks for domestic violence to be rated as a crime rather than a health problem. Jane Dimer (also from the United States) reports that the lack of screening for violence in Europe nearly resulted in her death while she was living in Germany.

Chris Carlsten from Seattle University criticises the fact that only male to female violence was under investigation, and Elspeth Webb and Rachel Brooks from Cardiff University warn that the impact of violence on children should not be overlooked. Teresa Goodell from Portland, Oregon, thinks that the ultimate solution may lie in interventions targeting perpetrators rather than victims, which is echoed by Judy Watson from the London Borough of Camden.

Protagonists on both sides of the Atlantic argue for a multiagency response, however difficult it might be to implement. Models from London and the United States underline this requirement. The training needs of general practice registrars and healthcare professionals at all levels will have to be identified and programmes implemented to ensure that these needs are taken care of. Women's groups and the police are mentioned as necessary parties in discussing and deciding how healthcare systems could best respond to the problem so that women do not avoid going to specific agencies.

The evidence found in Ramsay et al's systematic review may be insufficient to back up screening, but it is also insufficient to

reject it out of hand, say even some of the critics of screening. As Robert Gribble from Marshfield, Wisconsin, puts it: no action is not an option.

Birte Twisselmann *technical editor, BMJ*

1 Electronic responses. Should health care professonals screen women for domestic violence? bmj.com 2002. www.bmj.com/cgi/eletters/325/7359/314 (accessed 4 Dec 2002).

Association between competing interests and conclusions

See also editorial by Smith

Denominator problem needs to be addressed

Editor—During my personal exchange of material with Kjaergard and Als-Nielsen (I provided them with the equipoise scale, our data extraction forms, etc), I commented that their association between authors' competing interests and conclusions could have been explained by at least two additional types of bias. The first is pervasive authors' self selection bias (the authors tend to send their best pieces to a high impact journal such as the *BMJ*) and the second is the bias of the *BMJ* 's editors.[1]

The major problem with this type of research is that its results can never be completely reliable until the problem of denominators is addressed. The journal editors' decision accepting or rejecting a given paper for publication can potentially seriously skew the distribution of studies with negative and positive results. I believe that the editors of the *BMJ* have a unique opportunity to inform this important debate by publishing data on the number of studies that they reject (and accept) for publication (ideally as a function of the funding source, quality of the studies, and comparator intervention). Only in this way we can learn what really affects the published body of knowledge. I hope that the *BMJ* 's editors, who have been highly vocal on the issue of publication bias and competing interests, will accept this challenge.

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1 Kjaergard LL, Als-Nielsen B. Association between competing interests and authors' conclusions: epidemiological study of randomised clinical trials published in the *BMJ*. *BMJ* 2002;325:249. (3 August.)

Djulbegovic is right that the results of this study might be explained by bias on the part of the *BMJ*'s editors. We have no evidence of such a bias, and my suspicion would be that any bias would operate in the opposite direction. In other words, we might be more suspicious of positive results in studies funded by pharmaceutical companies. I agree, however, that it would be good to do some research on studies submitted to us rather than simply those published. The *BMJ* now has an active research programme, and we have completed around a dozen studies and have about 20 under way. We also have a list of some 20 proposed studies, and we will add Djulbegovic's suggestions to the list. One snag, I'm sure, would be that many authors do not give the source of funding or declare competing interests when submitting their studies. These only emerge when we ask them for the information. As we accept only around 7% of all the studies submitted to us and send only 40% out for external review, it would be a large undertaking to get all submitting authors to declare sources of funding and competing interests. It is perhaps worth adding that we would be happy for external researchers to use the *BMJ* as a test bed for research. There are many more studies to do than we have capacity to get done.—Richard Smith, editor, *BMJ*

Reasons for relation are also interesting

Editor—Kjaergard and Als-Nielsen present interesting data on the relation between competing interests and conclusions, but I think that they have missed an opportunity to tell us something even more interesting about the reasons for that relation.[1]

The observed association could be because authors of industry sponsored publications are more likely to draw inappropriately positive conclusions, or it could be because industry sponsored research is more likely to reach positive results. This could be for many reasons, perhaps because pharmaceutical companies are more likely to fund trials if they believe the results are likely to be positive, or perhaps because industry sponsored studies, usually being better funded than independent studies, are more likely to be adequately powered and therefore less likely to reach a false negative conclusions. The difference between these potential causes of the relation between funding and conclusions is important.

The authors' attempt to address this by statistical correction for sample size seems very crude. What is important is not the sample size of the study, but its power to detect a difference between treatments. A study with 200 patients that needed only 150 to have 90% power to detect a clinically important difference between treatments is clearly a more highly powered study than one with 500 patients that should have had 1000, but this would not be captured by the analysis used in the paper.

The study would have given us much more interesting information if we had been told not only the conclusions of Kjaegard and Als-Nielsen but how positive the results of the studies really were, as assessed independently by someone blinded to the authors' conclusions and the source of funding. Perhaps Kjaergard and Als-Nielsen could consider doing this for a future publication.

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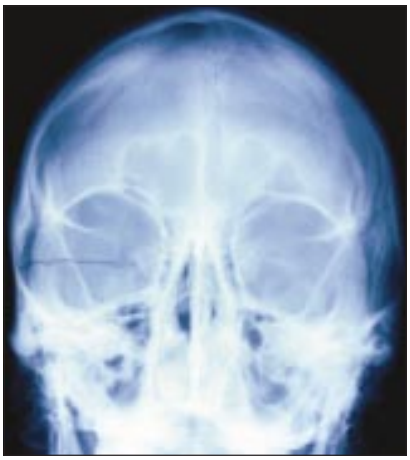
1 Kjaergard LL, Als-Nielsen B. Association between competing interests and authors' conclusions: epidemiological study of randomised clinical trials published in the *BMJ*. *BMJ* 2002;325:249. (3 August.)

Treating head injuries



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Skull fracture

Outcomes in specialist units using protocols may not be better

Editor—Mortality and morbidity from head injury seem to have fallen, presumably with use of organised trauma care systems and adequate critical care.[1] In his editorial on treating head injuries Wasserberg said that evidence now shows an overall improvement in the outcome of head injury from treatment in a specialist unit that uses protocol driven treatment.[2]

This statement is not based on a randomised controlled trial but a retrospective survey showing that in the whole referral population the tendency to increased favourable outcome after institution of protocol driven treatment did not reach significance and the overall mortality did not change significantly.[3] Only patients with severe head injury showed an increase in favourable outcome, without a difference in mortality. Wasserberg's statement therefore seems unsubstantiated.

All protocol driven treatments are based on successive introduction of hyperventilation, drainage of cerebrospinal fluid, infusion of mannitol, hypothermia, barbiturates, and (rarely) decompressive craniotomy—all treatments lowering intracranial pressure. Two studies cited by Raj and Narayan (by Roberts et al, reference 11, and Dickinson et al, reference 5) concluded on the basis of randomised controlled trials that it was impossible to refute either a moderate increase or a moderate decrease in the risk of death or disability from the use of hyperventilation, drainage of cerebrospinal fluid, mannitol, barbiturates, or corticosteroids.[1]

Wasserberg quotes a Cochrane review, concluding that no evidence exists that hypothermia is beneficial in head injury, forgetting that a recent randomised controlled trial was halted by the patient safety and monitoring board because the treatment was not effective and in fact worsened the prognosis in patients older than 45.[4] As hypothermia did reduce raised intracranial pressure but outcome did not improve, surrogate markers of efficacy (such as intra-

cranial pressure) have been deemed unreliable substitutes for clinical outcomes in determining the value of treatment.[1]

Protocol driven treatment and guidelines might be valuable tools in treating head injury, but, although guidelines for the management of severe head injury assembled by the United States Brain Trauma Foundation did take randomised trials into account, the methods used would not satisfy the criteria for scientific overviews (Roberts et al, reference 11).[1]

Step by step neurocritical research has been able to improve the control of raised intracranial pressure, but the conclusion that this improves mortality and morbidity after head injury is scientifically unproved and may prove false.

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Competing interests: None declared.

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Studies of efficacy of medical and surgical interventions are urgently needed

Editor—The editorial by Wasserberg highlights the lack of evidence on the effectiveness of currently used protocols in managing clinically significant head injury.[1] The current large-scale study of corticosteroid treatment will be a welcome exception.[2]

The management of raised intracranial pressure also deserves proper trials. A systematic review in adults and children found that no class I data clarify the role of monitoring intracranial pressure in acute coma, traumatic or non-traumatic.[3]

In 1998 we conducted a retrospective audit of the management of paediatric head injury in the north of England. We audited the cases of 54 children with head trauma and Glasgow coma scores of 8 or less who were admitted to eight paediatric intensive care units for ventilatory management in 1994. Three of the units routinely monitored intracranial pressure, three rarely did so, and two did so selectively. In the 19 children who were monitored the use of interventions to lower intracranial pressure or increase cerebral perfusion pressure significantly increased, as did the duration of ventilation (median 7 days *v* 2 days, $P < 0.001$). No obvious difference was seen in outcome (19 monitored: four died, six had a good outcome; 35 not monitored: nine died, 18 had a good outcome), but the numbers were far too small to detect any benefit or disadvantage below a level of 20%.

At the time of the audit, decompression was rarely considered and treatment of raised intracranial pressure relied on medical interventions. Surgical decompression is generally considered much earlier for compartment syndromes in the calf or abdomen and, although neurosurgical decompression seems logical, it is worrying that it is coming into use without any formal study of its effectiveness. As the management of raised intracranial pressure has implications for resources, formal studies of the efficacy of medical and surgical interventions are urgently needed.

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Neurostimulants may be piece in brain injury rehabilitation puzzle

Editor—I share Wasserberg's frustration with the lack of benefit of neuroprotective drugs in treating head injury despite the initial high hopes from laboratory results and in animal studies.[1] A more promising pharmacological intervention seldom used in the United Kingdom is the neurostimulant group of drugs, which proved useful in managing some of the long term cognitive impairments secondary to traumatic brain injury.

Speed of information processing could be improved with methylphenidate, and short term memory problems showed some improvement with donepezil.[2 3] Most of the evidence comes from small trials or single subject design studies, but that did not stop the use of neurostimulants from becoming standard practice in the United States for selected patients with such cognitive problems.[2]

Researchers in neurological rehabilitation have always found it difficult to organise large randomised trials or analyse results of smaller trials for various reasons, such as the difficulty of randomising patients because of their heterogeneity and the use of different outcome measures. The main difficulty, however, comes from the fact that rehabilitation outcome depends on complex interactions between medical, therapeutic, and psychosocial factors.

These difficulties in research methods are unlikely to be resolved in the near future. If we as doctors continue to wait for clear unequivocal proof of the effectiveness of a particular intervention in rehabilitating patients with brain injury, patients might miss out on a chance of a real difference in their quality of life.

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Psychiatric aspects of head injury need to be taken into consideration

Editor—Wasserberg's editorial on treating head injuries provides a constructively critical view on managing this common major health problem, particularly in young adults,[1] but awareness of the psychiatric sequelae of both major and minor head injuries needs to be increased urgently to complete the picture.

The effects of head injury on mental functions have mostly been studied in patients with severe trauma, which results in several neurological and psychological sequelae, including cognitive impairment, personality change, psychoses, affective disorders, and suicide.[2] Studies of the consequences of minor head injury are much rarer, although such patients often complain of psychiatric difficulties.[3] The wide range of symptoms commonly reported after minor trauma include headache, dizziness, hypersensitivity to noise, fatigue, impaired concentration, memory difficulty, irritability, anxiety, and depression.[2]

The psychiatric presentation often comes to light a few weeks or months after the event. The trauma may first present as comparatively subtle affective or behavioural changes. The patient, while providing the history, may not associate the complaints with the traumatic event.

Dinan and I found that the prevalence of depression (*Diagnostic and Statistical Manual of Mental Disorders*, third edition (DSM-III)) in patients with minor head injury was 15.7% two to 12 months after the trauma. The problems of all these patients were undetected and untreated.[4] The prolactin responses to challenges with buspirone and fenfluramine were significantly blunted, which implies serotonin dysfunction after trauma. This dysfunction returned to normal with clinical recovery after treatment with amitriptyline, although we found that depression after head injury was resistant to standard antidepressant treatment.[5]

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Robust evidence is needed in treating acne

EDITOR—Although the review and commentary by Webster et al on the management of acne contain good sense, the opinions presented do not fully reflect the evidence base or the risks associated with antibiotics.[1]

Webster promotes the use of antibiotics in the routine management of even mild acne when there is considerable global effort to limit antimicrobial prescribing to reduce resistance in key pathogens (www.cdc.gov/drugresistance/actionplan/html).[2] The extended antibiotic courses used in treating acne exert immense selective pressure on commensal flora,[3] and, as Webster says, resistance in *Propionibacterium acnes* is compromising efficacy. Antibiotics should therefore be stopped as soon as no further improvement is evident and not routinely used for maintenance; alternatives exist that have similar efficacy and have preventive action—for example, topical retinoids.[4]

Webster says that acne resistant to conventional regimens can be managed by increasing the frequency of topical and the dose of oral treatment. With antibiotics, such strategies have not been proved to increase efficacy,[4] but they will increase selective pressure. In addition, higher strength benzyl peroxide is in general no more effective than lower strength preparations but is associated with more irritation.[4]

No evidence from randomised controlled trials supports the use of second generation tetracyclines (doxycycline and minocycline) over (oxy)tetracycline for acne.[4] We disagree that ciprofloxacin and co-trimoxazole should be used. Ciprofloxacin given orally shows rapid selectivity, which promotes resistance,[5] and quinolones are not recommended in adolescents because of the associated risk of arthropathy. The sulphamethoxazole component of co-trimoxazole is associated with significant side effects, and the combination has not been proved to be more effective than trimethoprim alone.

Poyner and Cunliffe's suggestion to tell patients to expect little improvement in the first month and 20% improvement a month thereafter is incompatible with Webster's recommendation that failure to respond in four to eight weeks should prompt a substantial change of treatment. Given the large variability in clinical response between patients, they may simply be advised that improvement might not be immediately apparent and to return after six to eight weeks for review.

These issues illustrate the urgent need for better research into the treatment of acne and a re-evaluation of fact from myth. A recent systematic review of acne trials published by the US Agency for Healthcare Research and Quality found reliable evidence of efficacy in only 14 of 250 comparisons (www.ahrq.gov/clinic/evrptfiles.htm#acne). Robust evidence that answers the key questions necessary to prescribers is imperative given the overall burden of the disease both for individual patients and for healthcare resources.

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Galantamine may be effective in treating autistic disorder

EDITOR—Wilcock et al report galantamine to be an effective and well tolerated drug in Alzheimer's disease.[1] The mechanisms of autistic disorders are not completely understood. At least one kind of autism (Heller's dementia) is clinically quite similar to Alzheimer's disease.

No specific drugs seem to improve autism significantly. Desipramine, dextroamphetamine, clonidine, neuroleptics, and methylphenidate are reported to be only slightly effective but also to have possible severe adverse effects.[2–4] We conducted a placebo controlled, double blind crossover randomised controlled trial investigating the efficacy of galantamine in autistic disorders.

The participants were 20 boys attending an outpatient clinic (mean age 7.4 (SD 3.2) years; mean intelligence quotient (IQ) 68 (11) on the Leiter international performance scale of the revised Wechsler intelligence scale for children). They were without medical or neurological illnesses, had autistic disorder diagnosed by ICD-10 criteria, had been unsuccessfully treated with methylphenidate, clonidine, desipramine, and neuroleptics for more than six weeks, and had not received drug treatment for at least two weeks. Written informed consent was obtained. Participants were included in the study if their irritability, motor activity, eye contact, and expressive language (maximum 10 word vocabulary) were inadequate for their developmental level.

When parent and teacher scores were combined, mean scores were slightly lower during treatment with galantamine than during treatment with placebo for irritability classified by ratings of the aberrant behaviour checklist[5] (galantamine 11.5 (7.6) *v* placebo 15.1 (5.4), P=0.039), hyperactivity (17.2 (12.8) *v* 21.7 (15.4), P=0.038), inadequate eye contact (placebo 7.6 (3.2) *v* 8.4 (5.2), P=0.049), and inappropriate speech (4.7 (3.1) *v* 6.2 (2.4), P=0.045).

Clinicians' scores of videotaped sessions using the modified children's psychiatric rating scale for autism were not significantly different between galantamine and placebo.

None of the subjects seemed to have headaches or stomach aches, although the reporting of such side effects was limited by participants' expressive language and social skills.

Galantamine seems to be not only effective in treating Alzheimer's disease but may also be also moderately effective in the short term treatment of irritability in children with autistic disorder.

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Broad statements do not tell whole story of prison medicine

EDITOR—I work in custodial medicine and would find abhorrent the idea of referring to my patients, albeit incarcerated, as anything other than my patients. However, the news article by Gulland describing how the NHS is to take over responsibility for prison health services does not tell quite the whole story.[1]

Obviously differences exist across the service. In my experience a prison doctor sees urgent cases during the same day and around three days routinely. A consultant psychiatrist can be accessed the same week or within two weeks. Nurses are available immediately. Well man clinics, bloodborne virus clinics, etc, are provided. Locally, general practitioner waiting lists are around 14 days, and the time to see a consultant psychiatrist is five months. It seems to me that prisoners don't fare badly—in a medical sense.

The average prisoner (particularly drug misusers) can expect to see a police surgeon when arrested, a nurse on reaching the prison, and another nurse with a doctor the next day. Consultations in prison can be fairly spurious—often reflecting drug seeking behaviour—not seeking help—as I believe Gulland's article implies.

Legislation, not prisons, generates inappropriate medical tasks,[2 3] a remnant of the days when prison regimes truly were draconian. Custodial medicine is a complex skill, sorting out the genuine disease (of which

there is a great deal) in a population that may try to manipulate medical services for other than a desire to get well.

If anything, problems with access to health care seem to exist after prisoners are freed. Often a prisoner has lost his or her general practitioner and cannot quickly access addiction services, or support services if he or she is drug free.

Often housing has been lost and families split up. Having no fixed abode can affect the ability to be followed up by a mental health team. There can be little doubt that, for some health professionals, prisoners are a group with which they wish little contact. I have witnessed the unconscious prejudice of colleagues when confronted by a handcuffed person.

Many prison healthcare staff are highly trained individuals working in difficult circumstances. I question whether most mainstream NHS staff have the skills to deal with prisoners or could operate well in such a restrictive environment. Staff turnover in new prison health centres is well above the average. Not all prisoners are a danger, as some suggest, but clearly all are patients—the two are not necessarily mutually exclusive.

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Watchfulness may not be as good as surgery for prostate cancer

Editor—Gottlieb's news story that watchful waiting is as good as surgery in prostate cancer raises three issues.[1]

Firstly, the conclusion would be possible only if the study had been specifically designed as a non-inferiority or equivalence trial and the resulting confidence limits lay within a prespecified and widely accepted interval.

Secondly, the primary end point is given as death from prostate cancer. The result of the study was roughly a halving of the risk of death from prostate cancer during follow up, the 95% confidence interval for the hazard ratio being 0.27 to 0.91. Taking the results at face value, this is direct evidence that radical prostatectomy is actually superior to watchful waiting—contrary to the headline.

Finally, because all cause mortality was specified as a secondary end point, the fact that the overall mortality was similar cannot be taken as evidence that the treatments are equivalent—especially as all cause mortality is also known to be an insensitive measure of efficacy when the target disease is responsible for only a small fraction of all deaths in the study population. This is why screening trials use cause specific mortality rather than all cause mortality as an end point.

Clearly other factors (quality of life and costs) need to be considered before radical prostatectomy can be routinely offered instead of watchful waiting, but these were not the primary aim of the study. I wonder whether such a misleadingly negative statement would have been acceptable if a treatment effect of the same magnitude had been observed in a breast cancer trial, for example?

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People with partial hearing need to seek hearing potential

Editor—The two deaf mental healthcare professionals who successfully procreated a deaf child by using a deaf sperm donor are reported to have said that a hearing child would be a blessing and a deaf child would be a special blessing.[1] Apparently, Mother Nature (or God, if you will) saw fit to provide them with a son with partial or residual hearing. This outcome was predictable (if not poetic) and adds a further dimension to the ethical dilemma.

I believe that the subsequent refusal of the parents to allow treatment of the partial hearing defect constitutes unethical behaviour. Would you deny this child glasses, so that he could not see an oncoming car? Then why would you deny him a cochlear implant or hearing aid to hear the same oncoming car? Having accepted that this child's life would not be that bad so as to make it not worth while, surely the onus is now on his parents to maximise his potential in all spheres of life, including both hearing and deaf.

The parents' reported stance on the matter (that their son may have a hearing aid later in life, if he so wished) ignores the fact that most speech and language development occurs in the first few years of life. Their decision effectively disables their child further.

My parents were informed that their partially hearing son would not succeed in mainstream schooling, and a deaf school was recommended. Nevertheless, I can look back on a reasonably happy mainstream schooling, despite missing the punch lines to most of the jokes and never being a party to all the whisperings in class.

I therefore cannot accept that this child will necessarily experience the isolation that his two deaf parents refer to. I believe that this deaf couple fears losing their only child to the hearing world. Ironically, in the future, he may not forgive them for denying him due care, and then they may truly lose his love and respect.

A world of difference lies between the challenges of profound deafness and partial deafness. Although the deaf culture is praiseworthy for its integration of people who are profoundly deaf, those with partial hearing need to explore their hearing potential and, whenever possible, maximise their ability to communicate in the logocentric world. My understanding of ethics says that this child deserves to have his hearing potential investigated with an open mind as to treatment possibilities.

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On communication—editors and reporters should not be blamed

Editor—It's a tale so sad it's difficult to suppress a smile. In only two of eight interviews was Fallowfield able to communicate what she wanted to say about communication skills.[1] I too am depressed by the standard of some—but certainly not all—of the medical reporting on news programmes but it's a bit of a copout to blame editors and reporters, who work to a different agenda from people who seek publicity.

Doctors accused of having poor communication skills are not allowed to blame their patients. The fault, their teachers tell them, lies not in their audience but in themselves. Surely the same stricture applies to people who teach or promote communication skills.

The media in the 21st century are difficult and dangerous channels to navigate for those who seek publicity for their work or for themselves. Yet people can win themselves a chance to say what they want if they acquire the necessary skills. Many of these are the skills that doctors need with patients: seeking to understand the world in which the patient, as opposed to the doctor, lives and works; considering the patient's rather than the doctor's reason for having a consultation, learning what went wrong by analysing bitter experiences; and so on.

There are, however, many other skills, best learnt from those who do this sort of thing for a living. One of the most useful is to follow Larkin's commendable advice on bmj.com to turn down the offer of an interview that is unlikely to be fruitful.[2]

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