Correspondence

EDITED BY MATTHEW HOTOPF

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SSRIs and deliberate self-harm

Markowitz (2001), commenting on a study by Donovan et al (2000), which indicated that selective serotonin reuptake inhibitors (SSRIs) were more associated with presentations of deliberate self-harm to accident and emergency departments than were other antidepressants, suggested that it was ‘astounding’ that Donovan et al had not taken into account the fact that their results might stem simply from a preferential prescribing of SSRIs to patients with borderline personality disorder – a patient group particularly prone to self-harm. Dr Markowitz’s points about the Donovan et al study come down essentially to two – that this study was not randomised and that there was no placebo control. There are, however, data in the public domain bearing on these points to which he may not have had access.

Khan et al (2000) published a meta-analysis of randomised controlled trials submitted to the US Food and Drug Administration (FDA) showing suicides and suicide attempts on a number of recently licensed antidepressants. Requests to the FDA under freedom of information provisions (further details available from the author upon request) indicate that three of five suicide attempts characterised as placebo suicide attempts in the sertraline trial programme reported by Khan et al occurred during washout rather than while on placebo. Similarly, in the paroxetine trial programme reported by Khan et al, both suicides and three of six suicide attempts characterised as placebo suicides and attempts appear to have been washout rather than placebo suicides or attempts. Taking this information into account and analysing the drug v. ‘true placebo’ data for absolute numbers of patients reveals a statistically significant general increase in the risk of suicide acts on novel antidepressants compared with placebo and a specific increase for paroxetine.

In the light of these randomised, placebo-controlled findings it would seem that Dr Donovan et al were cautious and understated in their discussion of their results.

Declaration of interest

I have had consultations with, been a principal investigator or clinical trialist for, been a chairman or speaker at international symposia for or been in receipt of support to attend foreign meetings from a number of pharmaceutical companies with interests in the manufacture of antidepressants, including SmithKline Beecham and Pfizer. I have been an expert witness for the plaintiffs (US) and claimants (UK) in legal actions involving SSRIs.


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Response from Pfizer: The possible association between SSRIs and suicidal behaviour has been the subject of intense discussion throughout the 1990s, following the publication of case reports of suicidal behaviour suspected to be associated with fluoxetine.

Healy states that three of five suicide attempts characterised as placebo suicide attempts in the sertraline trial programme reported by Khan et al occurred during the washout period rather than while on placebo. All of the five suicide attempts to which he refers occurred while patients were on placebo, three of which occurred during the washout period. Healy similarly states that three of six suicide attempts and two completed suicides also occurred during washout rather than while on placebo in the paroxetine trial programme. Pfizer does not have access to data regarding other companies’ products and cannot therefore comment whether this is accurate or not. Healy concludes that taking this information into account and reanalysing the data, there is a statistically significant general increase in the risk of suicidal acts in patients taking novel antidepressants when compared with placebo. However, since this is based on inaccurate information, at least as regards sertraline, it is not a justifiable conclusion.

Pfizer has submitted information specific to when deaths occurred to the Medicines Control Agency (MCA) as well as other regulatory bodies, in compliance with worldwide regulatory requirements.

As with all medicines, the safety of the SSRIs is continually monitored by the MCA and the independent expert advisory body, the Committee on Safety of Medicines (CSM). Since 1992 a number of epidemiological studies and analyses of clinical trial data have failed to establish a causal association between the SSRIs and suicidal behaviour, and the CSM has reviewed this issue on a number of occasions. The most recent review, conducted in 2001 and discussed at the CSM on 12 December 2001, concluded that ‘the current evidence is insufficient to confirm a causal association between SSRIs and suicidal behaviour’ (Commons Hansard Written Answers, 2002) and advised that the issue should be kept under review.

The product information and the British National Formulary warn that patients should be closely monitored for suicidal impulses, and an article emphasising this advice was also published in the MCA/CSM drug safety bulletin in September 2000.

Commons Hansard Written Answers (2002) 4 February, Vol. 379, part no. 94, column 780W.


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Response from GlaxoSmithKline: Dr Healy responds to concerns raised by Markowitz (2001) about the potential for misinterpretation of the Donovan et al (2000) study. Markowitz points out that the patient populations receiving SSRIs and tricyclic antidepressants were not similar, and accordingly that comparisons of the effects of the two classes of antidepressants on suicide risk are not meaningful. Dr Healy suggests that there are data in the public domain bearing on this issue, citing the meta-analysis performed by Khan et al (2000) and data obtained from the US Food and Drug Administration. Khan et al found no difference in suicide or suicide attempts with the use of antidepressants compared with placebo. Dr Healy claims that suicides and suicide attempts during ‘washout rather than while on placebo’ invalidate the results of Dr Khan et al’s analysis.

With respect to paroxetine, Dr Healy misstates the scope of the Khan et al meta-analysis, and the conclusions he draws lack substantive support. Dr Healy fails to recognise that the exposure time of patients on paroxetine in the clinical studies was substantially different and far greater than that on placebo – under these circumstances an analysis of absolute numbers of patients with no consideration of time of exposure is not meaningful. Furthermore, contrary to Dr Healy’s implication, the Khan et al report was not limited to randomised, placebo-controlled studies. In the case of paroxetine, the studies covered included open label extensions, studies without placebo arms, and studies that were not randomised. When one considers only the randomised, controlled portions of the placebo-controlled trials (excluding events occurring during placebo run-in) included in the Khan et al analysis, there are no statistically significant differences in suicides or suicide attempts between paroxetine and placebo, either in absolute numbers of patients or when adjusted for time of exposure.

Donovan et al caution about the conclusions that should be drawn from the study. They point out that physicians are following guidance to prescribe antidepressants that are purportedly ‘safer in overdose’ to patients who are perceived to be at greater risk of deliberate self-harm. Consistent with Dr Markowitz’s comments, this prejudices against SSRIs when associations are made between their use and deliberate self-harm. Donovan et al also note that it is problematic attributing the cause of deliberate self-harm to antidepressant treatment when such behaviour occurs as a symptom of depressive illness itself and that establishment of cause and effect is ‘almost impossible’.

The ‘drug v. ‘true placebo’’ analysis Dr Healy describes is not only scientifically invalid, but also misleading. Major depressive disorder is a potentially very serious illness associated with substantial morbidity, mortality, suicidal ideation, suicide attempts and completed suicide. Unwarranted conclusions about the use and risk of antidepressants, including paroxetine, do a disservice to patients and physicians.


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Is it ethical to use a placebo?

Michelson et al (2001) evaluated the efficacy of fluoxetine in panic disorder and reported that fluoxetine was associated with a significantly greater proportion of panic-free patients compared with placebo. We read this double-blind randomised study with interest and wish to raise some concerns about the use of a placebo arm.

The use of placebo arms in randomised controlled trials remains a controversial issue and has been criticised on ethical grounds. In this context, the Declaration of Helsinki demands that individual patients in a study ‘be assured of the best proven diagnostic and therapeutical method’ even in the control group (Rothman & Michels, 1994). This statement clearly discards the use of a placebo as control when a ‘proven’ treatment exists. The declaration also directs that a study that violates its precepts should not be accepted for publication.

In addition to this, a trial that aims to establish whether a treatment is better than placebo may be trying to answer the wrong question. After all, even if a new treatment is worse than an existing one, it may still be ‘effective’ in that it is better than no treatment (placebo). In this regard, Hill (1963) pointed out that the essential medical question at issue is how the new treatment compares with the old one, not whether the new treatment is better than nothing.

As there are many drugs with proven efficacy in panic disorder (i.e. benzodiazepines, tricyclic antidepressants, selective serotonin reuptake inhibitors, monoamine oxidase inhibitors, reversible inhibitors of monoamine oxidase A and buspirone), we are keen to know why the authors did not try to compare the efficacy of fluoxetine with existing drugs. It appears that they are keen to reflect a drug-specific effect rather than demonstrating the relative efficacy. In this context Cochrane (1989) stated that no new treatments should be introduced into medicine unless they have been shown, in randomised controlled trials, to be superior to existing treatments or equivalent to existing treatments but cheaper or safer.


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Author’s reply: Drs Campbell and Jainer touch on an area of controversy in the design and conduct of psychiatric drug efficacy studies, arguing that the Declaration of Helsinki ‘clearly discards the use of placebo . . . when a “proven” treatment exists’. We disagree with their interpretation of the Declaration on several grounds, and note the broad support for careful use of placebo in psychiatric trials (Temple & Ellenberg, 2000) (including the support of the multiple independent ethical review boards that approved the protocol for our study). There are abundant data that non-specific interventions can have marked beneficial effects, albeit on average less than active drugs. Non-drug therapies are
often offered to patients as an alternative to drug therapies, and the absence of risk related to adverse drug effects can offset the potential for lesser efficacy. In our trial, both treatment groups had marked improvement from baseline. In this regard, placebo is not ‘no treatment’.

Drs Campbell and Jainer suggest drawing conclusions about drug efficacy based solely on comparisons of active agents. Unfortunately, in many trials a drug previously shown to be active is not superior to placebo despite adequate powering and the use of standard trial designs. Such trials are often referred to as ‘failed’ and in anxiety and depression are extremely common. A comparison of a new agent against a drug previously shown to be active without a placebo comparator is uninterpretable unless one agent is superior to the other. Concluding that a drug is efficacious without a placebo comparison can lead to an incorrect assumption of drug-specific effects if neither the investigational drug nor the active drug was, in that trial, any better than placebo would have been if included. Introducing a drug into therapeutic use on the basis of such a trial would expose patients to a compound with no greater benefit than placebo but all the risks of a pharmacological intervention (Temple & Ellenberg, 2000). Placebo is also critical in the assessment of safety, as it provides a base rate for determining which adverse events are truly related to the investigational drug. For these reasons, placebo-controlled trials are almost universally demanded by regulatory bodies to demonstrate efficacy for new pharmacological interventions.

Drs Campbell and Jainer also assert ‘no new treatments should be introduced into medicine unless they have been shown . . . to be superior to existing treatments . . . [or] cheaper and safer’. This absolute statement reflects several misconceptions and confounds the investigation of a drug with its introduction into general use. There is no general agreement about how to define or demonstrate equivalent or relative efficacy – precisely the reasons why most regulatory bodies will not consider relative efficacy claims in labelling. Furthermore, clinical trials provide information about group responses. Individual patients may not respond to or tolerate a particular drug, yet benefit from a different drug that is not, on average, more efficacious or safer than the first agent – it is in patients’ interest to have several choices. For example, using Campbell and Jainer’s procedure, the selective serotonin reuptake inhibitors, now proven to be safer and better tolerated than tricyclic antidepressants, might well have not been introduced into practice.

Finally, price is not an issue of trial design or science, but determined by the value that patients and purchasers put on a drug, based on evidence about the drug and experience with it (effectiveness as well as efficacy). Whether new drugs for panic or other psychiatric disorders should be ‘introduced into medicine’ and how they should be priced are decisions made on the basis of assessments of data about safety, efficacy and potential place in the therapeutic armamentarium – decisions that cannot be made before the data are collected. Campbell and Jainer may feel that the results of this trial do not warrant further investigation of the use of fluoxetine for panic disorder, although we would disagree. It is, however, wrong to suggest that the trial as designed should not have been performed or published.

Declaration of interest
D.M. is an employee of Eli Lilly and Co.


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User-led research and evidence in psychiatry

The editorial by Faulkner & Thomas (2002) raises serious issues, as did another recent paper (Bracken & Thomas, 2001: see van Beinum, 2001). They present a false dichotomy between (morally good) ‘users’ and (morally irresponsible) researchers, from which flows an unwarranted assumption that somehow psychiatric research rarely has the interests of patients at heart. Their editorial, with its unsubstantiated statements, poor definitions, political bannering and lack of understanding of both science and the research process, is the antithesis of considered and evidence-based argument.

There is, however, no doubt that patients and their families should have a substantial voice in helping to set the questions that research attempts to answer, and in establishing mechanisms for ensuring the importance of this process. This does not mean, however, that being a ‘user’ somehow qualifies a person as a top-notch research scientist. Thus, for example, the user-led research quoted by the authors (Faulkner, 2000) was deeply flawed, in that it did not address the issue of researcher bias and some of the conclusions bore no relation to the evidence presented. User groups have their own political agendas and are not representative of the body of patients as a whole.

There is a difference between asking socially relevant questions and conducting sound research. Good research is difficult to do and is best done by teams of well-trained research scientists. Arguing, as Faulkner and Thomas do, that psychiatrists and funding bodies should give equal weight to research conducted by groups of users and by professional researchers is a travesty. We do patients (and ourselves, for many of us have been, or will become, users) no favours by contounding good research with political correctness, for there is nothing more unethical or wasteful than poor research on vulnerable patients.


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Authors’ reply: We are grateful to Dr van Beinum for drawing our attention to the weaknesses of our editorial. In particular, it is good that he has highlighted the issues of researcher bias and the reprehensible wastefulness of ‘poor research on vulnerable patients’. Presumably, he assumes that professional research, undertaken by ‘teams of well-trained research scientists’, is of high quality and free of bias. Is this so? Let us consider by way of example the drug treatment of schizophrenia. Thornley & Adams (1998) examined the quality of 2000 controlled trials for treatment for schizophrenia from the Cochrane Schizophrenia Group’s register. They concluded
that half a century of studies of limited quality, duration and clinical utility left much scope for well-planned, conducted and reported trials. The consistently poor quality of reporting is likely to have resulted in an overoptimistic estimation of the effects of drug treatments for the condition. So much for good-quality research in the professional evidence base. What about bias?

The editors of our leading medical journals are clearly concerned about bias in research, particularly that which originates in conflicts of financial interest. Stelfox et al (1998) studied papers published in the New England Journal of Medicine on the use of calcium-channel antagonists in the treatment of cardiovascular disorders. They found that 96% of the authors of positive studies have received financial support from drug companies, compared with 37% of authors of negative studies. In a recent editorial in the New England Journal of Medicine, Marcia Angell (2000: p. 1516) has described the intertwining of academic medicine and the pharmaceutical industry in America, which extends far beyond grant support for research to include:

‘…a host of other financial arrangements. Researchers serve as consultants to companies whose products they are studying, join advisory boards and speakers’ bureaus, enter into patent and royalty arrangements, agree to be the listed authors of articles ghostwritten by interested companies, promote drugs and devices at company-sponsored symposiums, and allow themselves to be plied with expensive gifts and trips to luxurious settings. Many also have equity interests in the companies.’

She argues that these links are less to do with the transfer of technology across from academia to industry, than they are to do with marketing and profit. This influence also extends to guidelines on clinical practice. In a recent survey (Choudhry et al, 2002), 87% of 200 authors of clinical guidelines had financial links with at least one drug company, including companies whose products they endorsed. Over half of the authors had been paid to conduct research.

Of course user-led research is biased, but so is most research. Some psychiatric research is of high quality and undertaken out of the highest ideals. Equally, much of it has a murky, less idealistic pedigree, driven by commercial interest. User groups certainly have their own ‘political agendas’, but to pretend that psychiatry does not is either extremely naïve or dishonest. It is time for us to reflect on the need for a little honesty and humility, and for us to acknowledge that there are serious doubts about the independence and integrity of much of what we, as psychiatrists, consider to be ‘evidence’. To begin with, we need a debate about the influence that the drug companies have on our academic institutions, at our conferences, in our journals and in our consulting rooms.


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Perceived failure of community care

In his editorial on care in the community, Julian Leff (2001) describes processes comparable with those in The Netherlands, resulting in a call for ‘increasing restrictive mental health legislation enacted by governments pandering to public misperceptions’. This may be an indication that this process is more universal and not restricted to the situation in the UK. A few points may lead to more ‘perceived failure’ if not addressed.

Dr Leff states that ‘there is substantial evidence of considerable success ... of the 130 psychiatric hospitals ... in 1975, only 14 remain open, with fewer than 200 patients in each’. Does this imply that it would have been a failure if it were 25 hospitals with 300 patients each? Closing hospitals should not be a goal as such, but a means to provide better services to patients. That a new generation of psychiatrists ‘not only have never worked in a psychiatric hospital but have never seen one’ may not be such a desired development. In the coming decades in-patient facilities will still be needed and the number of them may fluctuate because of new treatment modalities and the capacity of society to harbour patients. An increase or decrease should not be an indicator of success or failure at all.

The ‘invisibility of a community service’ as grounds for ‘perceived failure’ is interesting in relation to the statement that ‘the architectural presence of the asylum has been replaced by an apparent absence’. Were many asylum not tucked away at the outskirts of the city, if not further away? Mental health care should make itself, and its diversity of services, more visible. Could it be that professionals, patients and relatives have a somewhat defensive stance regarding the public and the media? In The Hague after the merger in 1999 of all psychiatric hospitals, community mental health organisations and addiction organisations, posters were put on trams and bus stops leading to a high visibility, which was well perceived.

Would a comprehensive community psychiatric service catering to all the needs of the catchment area population enhance the perception of success? In The Netherlands in recent years this development has started in some areas owing to large-scale mergers of mental health organisations. This has led to a disappearance of administrative and financial boundaries between in-, out- and day-care patient services. In The Hague there are indications that the needs of patients, family, general practitioners and police are better identified and addressed, leading to a visible profile and higher perceived success.

If we want to know what our targets are in ‘a public relations job of this kind’, we are at the brink of a more fundamental shift in defining and positioning the concept of (community) mental health. Who can identify him or herself with a psychiatric patient? Are there not fundamental differences between a patient with schizophrenia, agoraphobia or bipolar disorder? In The Netherlands generalisation and stereotyping lead to the situation that the acts of one person with an addiction and personality disorder may damage the positive image of mental health in general for a certain period.

Community-oriented care is a success for a subgroup of patients with psychiatric disorders. Perceived failure in one area should not lead to a situation that the whole of mental health services, including care in the community, is perceived as a failure.

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Author’s reply: Regrettably, Dr Hoencamp has misinterpreted a number of phrases in my editorial. Rather than calling for increasingly restrictive legislation, I was warning the reader against this alarming consequence of public and governmental misperceptions of care in the community. On another point, I certainly did not mean to imply that closing psychiatric hospitals is itself an indication of a successful policy. The evidence of success to which I was referring consists of the growing body of research showing that the quality of life of discharged long-stay patients is improved by relocation in community homes (e.g. Leff & Trieman, 2000). Dr Hoencamp is of course right that in-patient facilities will continue to be needed, but there is no reason for them to be located in the outdated structures of the psychiatric hospitals. There are undoubtedly problems with admission wards in district general hospitals, but these can be remedied by improved architectural design and the provision of alternatives such as acute day hospitals (Creed et al, 1990).

Although many asylums were deliberately built outside of towns, urban expansion brought them within the ambit of residential areas. Even those that remained remote, engendered in the public mind the image of life-long incarceration. I agree with Dr Hoencamp that more should be done to publicise community mental health services. We should be proud of what has been achieved and promote a high visibility. He raises the issue of the diversity of psychiatric disorders and the difficulty the public and the media have in distinguishing them. This dilemma faces any organisation attempting to change public attitudes towards people with mental illness and the services they need. The Royal College of Psychiatrists’ campaign ‘Changing Minds: Every Family in the Land’ addresses a wide range of psychiatric disorders, while the World Psychiatric Association’s ‘Global Campaign against the Stigma of Schizophrenia’ focuses on that one condition. Hopefully the results of these programmes will indicate which is the more effective strategy. However, early results from the World Psychiatric Association campaign indicate that education aimed at teenagers in schools produces the most positive change in attitudes. A good strategy would seem to be the inclusion in the school curriculum of information about the diversity of disorders and treatment modalities in psychiatry.


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Use of outcomes measures by psychiatrists

Gilbody et al (2002) highlight the poor adherence of psychiatrists to using instruments to measure clinical outcomes. Assessment tools and outcome measures have been in use among practitioners working with people with learning disability for many years. There are many validated tools and reliable measures available for use in clinical practice that are routinely used. The take-up of assessment tools and outcome measurements has perhaps been influenced by the proportion of this patient group who have poor verbal skills, making access to their mental state and internal world a challenge to the clinician.

Observation of behaviour is an important element of assessment of mental health problems in people with learning disabilities. The Mini Psychiatric Assessment Schedule for Adults with Developmental Disability (PAS-ADD) is commonly used to detect psychopathology in people presenting with challenging behaviour that may be due to mental illness. It has been shown to have good reliability and validity (Prosser et al, 1998). The Health of the Nation Outcome Scales for People with Learning Disabilities (HoNOS-LD) is a useful tool to measure change over time to a therapeutic intervention (Roy et al, 2002). Clinical observations can be carried out by any clinician in the multi-disciplinary team trained in the application of these clinical tools.

It could be argued that the use of these instruments is reductionist and does not communicate the breadth of human experience and suffering of patients. Where language fails to express the impact of mental illness and social distress, I would hold that the use of rating scales can objectively indicate the nature of the suffering and the effectiveness of interventions made in patient care.

I believe the key to the use of tools in the future will depend on educating trainees to use these instruments and allowing them to be freely available in clinical practice. Of course they would gain greater prominence in practice were they to form part of assessment in the MRCPsych examinations!


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Dissent as a symptom: why China has questions to answer

In his letter taking issue with our claim that psychiatry is abused by the Chinese authorities for political control purposes, Dr Sing Lee (2001) cites his own experience in examining patients there suffering from ‘qigong-related mental disorder’. He concludes that this culture-bound syndrome both exists and can be a serious condition, and that the psychiatric detention of Falun Gong practitioners in China today is therefore not a sign of the political abuse of psychiatry. Without challenging the validity of Chinese psychiatrists’ diagnoses of qigong-related mental disorder in particular cases, it should be stressed that the Chinese authorities themselves hardly ever mention this diagnosis when justifying the psychiatric detention of Falun Gong practitioners. Indeed, recent articles in the Chinese psychiatric literature have stated
that this diagnosis is not generally applicable to Falun Gong practitioners, who are instead alleged to be suffering from a separate and more serious condition (albeit one not recognised in the official Chinese Classification of Mental Disorders) for which the term ‘evil cult-related mental disorder’ has conveniently been coined (Shen & Gong, 2000).

More worryingly, Dr Lee makes no reference in his letter to the reality that the Falun Gong practitioners concerned were first arrested by the police, in most cases to prevent them from staging peaceful demonstrations against the Chinese Government’s continuing suppression of their spiritual movement nationwide. To our knowledge, the articles from the Chinese psychiatric literature concerning qigong-related mental disorder that Dr Lee refers to in rebuttal of the claims of political psychiatric abuse in China do not address the cases of patients detained by the police – they were all apparently voluntary patients. On both the above key counts, therefore, the Falun Gong cases (all of whom were reportedly arrested and then forcibly committed) evidently fall into a different category from that with which Dr Lee is personally familiar.

The recently compiled evidence of the state abuse of psychiatry against political dissidents over the past decades (Munro, 2000, 2002), most of which comes from the official Chinese psychiatric literature, is overwhelming in quantity and specificity. Given this past track record, the burden of proof now clearly falls upon the Chinese authorities to convince their own citizens and the outside world that the several hundred reported cases of arrested Falun Gong practitioners sent to involuntary psychiatric treatment (see http://hrreports.faluninfo.net/book4/CategoryIndex.htm) are, as Dr Lee seems to believe, valid and suitable cases for treatment. The simplest way to do this would be for the Chinese authorities to allow suitably qualified outside observers free access to the Falun Gong psychiatric detainees so that their mental conditions can be independently evaluated. Thus far they have shown no such willingness, and several Chinese nationals who tried to document such cases have been jailed.

We acknowledge that Chinese psychiatry as a whole is not generally complicit in these politically motivated distortions of ethical psychiatry and that they are largely (though by no means entirely) confined to the domain of forensic psychiatry. This was also true, however, in the case of the former Soviet Union and certain Eastern European countries, where political dissidents, religious nonconformists and others formed but a small minority of the overall psychiatric inmate population. Then as now, the key issue is that the numbers of those affected is none the less substantial, and that any psychiatric diagnosis based on politics – whatever the scale – poses a potentially wider ethical threat to the profession. This is why the World Psychiatric Association, through its Madrid Declaration on Ethical Standards (see http://www.wpanet.org/home.html), has specifically banned member societies from engaging in politically based diagnosis of any kind.

Finally, the most pressing point to note is that, whereas the incidence of such practices in China had apparently been steadily declining since the late 1980s, the Government’s crackdown on Falun Gong since July 1999 has resulted in a sharp renewal of politically abusive psychiatry. Failure by the international psychiatric community to speak out clearly against this disturbing trend now could well give the green light to a further expansion of these measures by the Chinese authorities in their ongoing fight against domestic dissent of all kinds.

Declaration of interest


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Cognitive impairment v. dementia
The February 2002 issue of the Journal contained a number of useful reviews of the major disorders that lead to dementia. In his paper on vascular dementia, Stewart (2002) suggested that we need to be ‘identifying cognitive decline at a much earlier stage than dementia’. It occurred to me some time ago that the term ‘dementia’ has outlived its usefulness. It derives from a time when mental health workers were few and only gross changes in mental state were noted and dealt with. It still carries with it the therapeutic nihilism of those times and even the anticholinesterase inhibitors do little to dispel this, as they work for only a minority and for a short period of time. The term suggests a black-and-white distinction (‘demented’ or ‘not demented’). In fact there are infinite variety of shades of grey. I have had a number of experiences of patients being referred as ‘demented’ largely for ‘disposal’ and have found that when we have taken them off toxic medication, treated their chronic chest infections, improved their diabetes and hypertension care, reduced the severity of their heart failure, got rid of their anaemia and managed their depression, anxiety or psychosis etc. we have been able to discharge them home or to relatively inexpensive long-term care. I see the person as cognitively impaired, and work to reduce the severity of that impairment – not simply by prescribing anti-dementia drugs. The widespread use of standardised ratings, such as the Mini-Mental State Examination (Folstein et al, 1975) and more advanced variations such as Cambridge Examination for Mental Disorder of the Elderly (CAMDEX; now revised, Roth et al, 1999) have greatly improved doctors’ ability to screen cognitive function, and while National Institute for Clinical Excellence guidelines encourage us to think of a specific score that delineates those with dementia from those without dementia, we are all aware that this is driven by accountancy rather than by medicine. At a practical level it is possible to get through the working week without using the term dementia and more accurately convey the person’s mental state by speaking of cognitive impairment and elaborating on which areas are intact and which are dysfunctional.

Having written the above I feel some reluctance to post it as I think it is likely to raise more hackles than nods of agreement. It is as if I have suggested putting down a
much loved but ancient family dog just because it is no longer able to deter burglars and chase cats. I hope that in my lifetime we will have developed drugs that will prevent the onset of disorders that lead to dementia. When an effective cocktail of cleaving agents, anti-oxidants, free-radical scavengers and neurochemical enhancers is available we will all be having our cognitive function tested by primary care on an annual basis much as we do now with our blood pressure. The ageing pooch will then die quietly in its bed.


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One hundred years ago

Insanity and the death penalty

Sirs: – I have read with interest Sir W. T. Gairdner’s letter in your issue of July 26th, p. 242, but I cannot agree with him that any idea of vengeance is present in the punishment meted out to the criminal by the State. Vengeance is entirely a personal emotion but the State knows no such passions, being impersonal, and in a strictly impartial manner enforces by suitable punishments the laws it has enacted. If the punishments do not have the desired deterrent effect they may be useless, but I submit that they cannot be regarded as State vengeance on the criminal. It is true that the poet speaks of “the wild justice of revenge”, because the act of the vengeful man and that of the State may equally deter from crime, but the motives of the act in the two cases are altogether different. With regard to verdicts, “Guilty, but insane”, they would be contradictory under the present law. If a criminal is insane in the legal sense he cannot be guilty in the eyes of the law. That the legal definition of insanity in McNaughton’s case is by no means in accordance with our present knowledge of mental disease may be readily admitted, but none the less I am one of those who do not think that the public safety will gain by modern views of mental pathology being permitted further to diminish criminal responsibility.

I am, Sirs, yours faithfully,

Hackney-road, N.E., July 26th, 1902.

M. Greenwood

REFERENCE

Lancet, 2 August 1902, p. 313.

Researchers: by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey

Corrigendum

SSRIs and deliberate self-harm

D. Healy

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References

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