Correspondence

EDITED BY MATTHEW HOTOPF

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Declarations of interest

In a recent editorial, Thompson (2001) argues strongly against the findings of a systematic review in the same issue of the Journal (Barbui & Hotopf, 2001). The Journal requires authors of original papers, but not of editorials, to declare financial interests. In this case your editorial writer did not record his consultancies (past or present) to companies that manufacture selective serotonin reuptake inhibitors. Neither did he indicate that the recent study he cited to support his case (Thompson et al, 2000) was, in fact, carried out by a pharmaceutical company. The company (Eli Lilly) manufactures one of the antidepressants, the benefits of which are questioned by Barbui & Hotopf’s systematic review.

The Journal began to publish Declarations of Interest in 1999, but only for original papers not editorials. We urge extension of the Declarations to include editorials. We would support more stringent criteria for editorials and similarly for reviews than for original papers – in line with the New England Journal of Medicine which for 10 years has “had a policy that prohibits editorialists and authors of review articles from having any financial connection with a company that benefits from a drug or device discussed in the editorial or review article” (Angell & Kassirer, 1996).


The Journal of Psychiatry in China? ■ Lest we forget, again ■ An alternative to interruption of treatment in recurrent clozapine-induced severe neutropenia

Author’s reply: I am pleased to be able to respond to the letter from Drs Owens & House with which I am in partial agreement. I am happy to confirm that the cited study (Thompson et al, 2000) was designed by me, carried out by Eli Lilly in a UK primary care context, and was analysed and written up by my colleagues and I. These facts are acknowledged in the primary research publication in the American Journal of Psychiatry.

I am also happy to confirm that, along with occasional paid lectures, I currently hold a consultancy with Organon UK and have recently held a similar consultancy with Janssen and with Philips, but not with Eli Lilly. Readers of the Journal might also wish to know that these interests have been declared to the Royal College of Psychiatrists and are known to my employing University.

I completely agree that conflicts of interest should be declared. However, I wonder whether the editors of the New England Journal of Medicine would include in their censorship policy the authors of editorials in which the objective is solely to comment on the methodological adequacy of an original article. If so, that appears to me to be more a prohibition on freedom of speech than anything related to evidence-based medicine, and would have precluded my criticisms of the Barbui & Hotopf article. If that is the objective of Drs Owens & House, then I cannot agree with them on that point.

Finally, I have not been able to find any scientific points in Owens & House’s letter to which I can respond and I therefore assume that they are in full agreement with my criticisms. Otherwise, I am sure that they would have presented rational arguments against my analysis instead of taking their argument ad hominem.

C. Thompson Department of Mental Health, 1st Floor Department of Psychiatry, University of Southampton, Royal South Hants Hospital, Brittons Terrace, Southampton SO14 0YH, UK

Editor’s response: In response to concerns brought to my attention in recent months, including those presented by Drs Owens and House, the Editorial Board now requires that authors of editorials and items of correspondence submit a Declaration of Interest, as authors of papers have been required to do for some time now. Similarly, assessors participating in peer review will in future be instructed not to assess material in which they have an interest. This change in policy is reflected in the Instructions for Authors, published in the July issue of the Journal, available on-line at http://bja.rcpsych.org/misc/fora.shtml and available upon request from the publishers. Declarations of Interest for papers and editorials will be published as a matter of course. In the interests of space, Declarations of Interest for items of correspondence will be published at the Correspondence Editor’s discretion.

I have acted as proxy Correspondence Editor in the acceptance of the above letter and its response, in view of Dr Hotopf’s interest in the matter.

G. Wilkinson Editor, British Journal of Psychiatry, 17 Belgrave Square, London SW1X 8PG, UK

Comparative cannabis use data

MacCoun & Reuter (2001) examine alternative legal regimes for controlling cannabis availability and use. They claim that the Dutch experience (the coffee shop system with decriminalisation of purchase, followed by “commercial promotion”) significantly increases cannabis use prevalence. They conclude, however, that primary harm comes more from criminalisation than from decriminalisation. They base their conclusions on the comparison of cannabis use data from The Netherlands and from other countries. Rightly, they warn that “meaningful cross-sectional comparisons of drug use should be matched for survey year, measure of prevalence . . . and age groups covered in the estimate”. They forget that

D. Owens, A. House Academic Unit of Psychiatry & Behavioural Sciences, University of Leeds School of Medicine, 15 Hyde Terrace, Leeds LS2 9LT, UK

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the comparisons should also be matched for type of geographical area. Comparing Flatbush to New York City would have limited relevance, even if correctly matched for age group etc. To compare cannabis use in New York City with somewhere else, one would have to look for a similar area, both in address density and in variation of population and lifestyle. Amsterdam could be compared to San Francisco, because these cities are very similar in size and cultural characteristics, but not to New York City, a metropolis over 10 times as large, or to the USA as a whole. Such comparisons are wrong and without meaning.

We agree with MacCoun & Reuter that decriminalising cannabis merits serious consideration. But we disagree with their observations on “commercialisation”. In this letter we will turn most of our attention to the epidemiological material the authors base their conclusions on.

MacCoun & Reuter focus entirely on cannabis prevalence (assuming that a lower prevalence is better than a higher one) without considering whether this is the most relevant issue; the social and legal consequences of the use of cannabis could be considered at least as important. But given that a comparison of prevalence figures is a useful first step towards informed comparisons, we propose that the conclusion of MacCoun & Reuter that the commercial type of Dutch coffee shop system increases cannabis prevalence is based on statistically ill-founded comparisons of Dutch prevalence figures with those in other Western nations.

MacCoun & Reuter compare cannabis prevalence figures of a Dutch city or nationwide with prevalence figures from the USA or other Western nations. Differences are summed and averaged, resulting in (among others) a mean Dutch-US difference and a mean Dutch-European difference. This is statistically erroneous for reasons we supply below.

First, in 16 cases a Dutch city is compared with a nation (UK, USA, Sweden, etc.). By doing this, MacCoun & Reuter presuppose that prevalence rates are the same all over The Netherlands. This is incorrect: in our 1997 national survey we found large geographical differences between locations with different address densities, a measure of urbanisation. For example, lifetime prevalence of cannabis use in Amsterdam (address density > 3000/km²) was 36.7%, the average national prevalence was 15.6% and average prevalence in rural areas (address density < 500/km²) 10.5%. Correct international comparisons can be made, but have to be between comparable geographical or urban areas. Despite the sensitivity MacCoun & Reuter demand for correct comparisons, nationwide US figures (260 million inhabitants, including major metropolitan areas) are compared with the small Dutch city of Tilburg (165 000 inhabitants).

Second, comparisons are arbitrarily selected. For example, replacing prevalence figures for Amsterdam (the city most often chosen in MacCoun & Reuter’s comparisons) with figures for Rotterdam changes the outcomes of the average difference in cannabis prevalence between the Dutch and other systems.

Third, MacCoun & Reuter state that the lifetime prevalence of cannabis in The Netherlands has increased consistently and sharply in the age group 18–20, stating: “the increases . . . provide the strongest evidence that the Dutch regime might have increased cannabis use among the young”. This finding is based on school survey data (lifetime cannabis use in 1984: 15%, in 1996: 44%). Again, the choice of figures that are compared is crucial. Moreover, the Dutch school survey data of the age group 18–20 is an extremely biased selection of this age cohort. The school survey takes place in some primary schools, but mostly in secondary educational institutions, that are designed for 12- to 18-year-olds. However, some persons remain much longer in this system for a variety of reasons but they are atypical for the age group in general. They bias the school survey estimate for this age group.

More suitable figures are given by Statistics Netherlands (CBS) and by the Centre for Drug Research (CEDRO), and reflect a much more moderate increase or no increase at all. Statistics Netherlands measures cannabis use prevalence in a national representative sample. For the age group 18–20 lifetime cannabis use remains at the same level over time (17% in 1989, 19% in 1990, 18% in 1991, 20% in 1992 and 14% in 1993; data from D. J. B.). Using CEDRO data, we are able to produce trend data for the city of Amsterdam for the same age group 18–20: in 1987 lifetime cannabis use was 34%, rising to 44% in 1997. This is a rather modest increase in cannabis use, very similar to the slowly rising consumption levels of other European and US measurements. The 18- to 20-year-olds in the samples from Amsterdam are randomly selected from the citizen registry, and represent the age group much better than 18- to 20-year-olds still attending school. The ‘dramatic’ increase that MacCoun & Reuter hypothesise in Dutch cannabis use in the period 1984–1996 (as reflected in the same age group) does not exist.

Finally, the most serious flaw develops by creating a series of ‘absolute’ differences between Dutch and other data, and averaging them. MacCoun & Reuter create the suggestion that too large or too small differences will be averaged and thereby, in the form of an ‘average’ difference, become more reliable. The opposite is true. If pears can not be compared to apples, their ‘differences’ can not be used for normal mathematical computations.

**Declaration of interest**

None. The views expressed by D. J. B. are the author’s own and do not necessarily reflect the policies of CBS.


**M. D. Abraham, P. D. A. Cohen** Centre for Drug Research, University of Amsterdam, Postbus 94208, 1090 GE Amsterdam, The Netherlands

**D. J. Beukenhorst** Centraal Bureau voor de Statistiek (CBS), Heerlen, The Netherlands

**Authors’ reply:** We thank Abraham et al for their comments, but they have misrepresented our paper, and we find their arguments either misleading or unconvincing.

Abraham et al complain that 16 of our 28 statistical comparisons contrast a Dutch city with a national estimate from the USA or another nation, suggesting that we “pre-suppose that prevalence rates are the same all over The Netherlands”. We made no such presupposition. As we clearly stated in our article: “American surveys indicate little difference, on average, between large metropolitan samples and the USA as a whole... but the estimates in Table 1 suggest that Amsterdam has a higher fraction of...”
marijuana users than smaller Dutch communities. US rates are basically identical to those in Amsterdam and Utrecht, and higher than those in Tilburg”. We then note that “unfortunately, many of the available contrasts between The Netherlands and her European neighbours suffer from the same weakness, comparing rates for an entire nation as a whole to those in the largest city of another nation”. And we state that the contrasts where the Dutch rates are higher are mostly “attributable to comparisons limited to Amsterdam”. We conclude that “Dutch rates are somewhat lower than those of the USA but somewhat higher than those of some, but not all, of its neighbours. Amsterdam’s level of marijuana use is comparable to that of the USA”.

Abraham et al further complain that our comparisons were “arbitrarily selected”. In fact, our 1997 Science article included every Dutch cannabis prevalence rate for which we could find a reasonable international contrast matched by year, age range and type of prevalence. Our recent update in the British Journal of Psychiatry added another 13 comparisons. We welcome further comparisons but a fair reading of both papers makes it clear that we attempted to be exhaustive, given the limited availability of Dutch drug prevalence data in English-language sources. (Indeed, where possible we had Dutch-language sources translated.) In any case, we emphasise that we drew no policy conclusions from these static cross-sectional comparisons. That portion of our article was an attempt to correct grossly misleading comparisons of Dutch and US rates in the American media.

We are taken to task for using the Dutch school survey data from the Trimbos Institute, rather than data from Statistics Netherlands or the CEDRO Amsterdam survey. As noted below, we did in fact report CEDRO estimates. But the 1990s Amsterdam trends mentioned by Abraham et al are not relevant to our commercialisation thesis; as we explained in our article, the dramatic growth in cannabis commercialisation in Amsterdam occurred between 1980 and 1988 and almost every Western nation saw increases in cannabis use after 1992 for reasons apparently unrelated to drug policy.

We are delighted to learn of the national Statistics Netherlands estimates, which as far as we can tell have not been cited previously in the English-language literature — although the search engine on their website produces no statistics for “drugs”, “drug”, “cannabis” or “marijuana”. But now we are puzzled as to why a 1997 paper by Marieke Langemeyer announcing CEDRO’s own national survey stated that “The implementation of the national survey means that finally, The Netherlands will have a decent source of data that serves multiple purposes among which the basic information for health care, prevention, education and drug policy. Hopefully, it is the beginning of a high quality drug research tradition”. Similarly, a CEDRO press release of 14 April 1998 stated that “figures for the entire country will soon no longer have to be based on local surveys since a national study on drug use in The Netherlands is currently being carried out by CEDRO”. Moreover, neither the CEDRO nor the Trimbos researchers mention these data in their English-language monographs on Dutch drug use trends.

Our Fig. 1 showed that during the 1984–1992 period the Trimbos lifetime estimates rose even more steeply for the age 16–17 group than for the age 18–20 group. This clearly undermines the concern raised by Abraham et al about a selection bias involving older students, but at any rate, that criticism misses the point. Sampling biases of the Trimbos school survey do not preclude its use for studying trends over time. Moreover, our trend analysis compared it to age 18–20 trends from the US Monitoring the Future school survey. The Trimbos researchers state that their survey was designed to permit comparisons to that US survey (see Pomp et al, 1991: 11).

Abraham et al complain that we averaged non-comparable estimates, but fail to mention that we grouped our estimates so that ‘city v. nation’ averages and ‘nation v. nation’ averages were presented separately. We think our averaging was well within contemporary standards of meta-analysis, but no matter — we presented the raw data so readers could decide for themselves. At any rate, no conclusions of our work hinged on these averages — indeed, we did not even include them in our presentation of these data in our forthcoming book, Drug War Heresies (MacCoun & Reuter, 2001b).

Abraham et al suggest that our alleged inattention to the geographical issue undermined our inferences about the effects of commercialisation. On the contrary, the fact that cannabis prevalence is higher in Amsterdam is quite consistent with our hypothesis. During the 1980s, when we contend the commercialisation effect occurred, various estimates suggest that over a quarter of all Dutch cannabis coffee shops were in Amsterdam, yet Amsterdam accounted for only about 5% of the total Dutch population. As late as 1997, Abraham et al (1999) reported that last-year users from the highest-density Dutch addresses were more likely to cite coffee shops as their cannabis source than were users from low-density Dutch addresses.

As we stated in the article, the evidence for our commercialisation hypothesis was indirect and at best purely correlational, though we noted that it is consistent with evidence on gambling, tobacco and alcohol marketing. Moreover, the quasi-legal status of the Dutch system, which tends to keep prices high, almost surely underestimates the likely commercialisation effects of full legalisation. Given weak data, our inferences may well be wrong, but we think the comments of Abraham et al shed little light on that question.


R. MacCoun Richard & Rhoda Goldman School of Public Policy, University of California, 2607 Hearst Avenue, Berkeley, CA 94720-7320, USA

P. Reuter School of Public Affairs and Department of Criminology, University of Maryland, USA

Prognosis of depression and generalised anxiety in primary care

Van den Brink et al (2001) studied general practitioners’ (GPs’) prognostic predictions for depression and general anxiety. They found the prognosis was in general more pessimistic than the observed course and failed to attain maximal performance in
comparison with a statistical model based on baseline variables. I would like to express three concerns about the technical details of this article.

First, the kappas they report are Cohen’s kappas whereby the disagreement between “full recovery within 6 months” and “partial recovery” is penalised equally to disagreement between “full recovery within 6 months” and “no recovery”. Clinically, however, the former is apparently less grave than the latter. More appropriate statistics would be weighted kappas, which are 0.31 (95% CI 0.15–0.46) for GP prognosis for depression, 0.35 (95% CI 0.16–0.54) for GP prognosis for anxiety, 0.36 (95% CI 0.43–0.70) for model prognosis for depression and 0.51 (95% CI 0.33–0.69) for model prognosis for anxiety. These figures are appreciably larger than those originally reported.

Moreover, regardless of whether we use Cohen’s kappas or weighted kappas, the authors did not examine whether the GP prediction is indeed statistically significantly worse than the model’s. The reported 95% confidence intervals overlap, and we do not know whether the clinicians are actually performing worse than the maximally attainable model.

Third, as the authors rightly note in the Discussion, their way of using the total sample to construct a predictive model may have ‘overfitted’ the model to the data and produced artificially inflated agreement. A more ideal way may have been the ‘leaving-one-out method’ (Lachenbruch, 1975), in which analysts would repeatedly build a model based on a sample minus one observation and examine whether each model could predict the one excluded observation.

In this connection it may be worthwhile to point out that the comparison between human performance and that of a statistical model is a theme repeatedly found in clinical psychology (Meehl, 1954; Goldberg, 1970). These studies conclude that, because of the inevitable random error in human judgement, the latter almost always outperforms the former. It will, therefore, be most interesting to see how, in the authors’ next round of proposed investigation, clinicians can improve their performance if they are given feedback on prognostic factors.


T. A. Furukawa Department of Psychiatry, Nagoya City University Medical School, Muzuh-cho, Muzuh-ku, Nagoya 467-8601, Japan

The stigma of suicide

The Royal College of Psychiatrists is leading a campaign to reduce the stigma attached to mental illness. Stigmatisation of suicide has very deep roots in our collective thinking and judgement. Suicide was tolerated by the Greeks and Romans (Alvarez, 1990), but Aristotle argued that suicide weakens the economy and upsets the gods, and in so-doing he initiated stigmatisation of the act. Hinduism and Buddhism, among other Eastern religions, have not had a traditionally negative view of suicide. In the Judeo-Christian tradition, stigma against suicide is not evident until the fourth century; the Bible does not condemn suicide (Barracough, 1992), but St Augustine considered suicide as unacceptable within Christian values (Pritchard, 1996). Gradually, the stigma against suicide intensified in Europe and became a great sin, shame and eventually a crime. A number of philosophers and writers including William Shakespeare sought to encourage a more understanding and compassionate view but this movement had little impact before Durkheim’s studies made clear the social rather than moral origins of suicide (Rettelstol, 1993).

Although suicide and attempted suicide were decriminalised in 1961 (Levine & Pyke, 1999), we have practised since within a culture of ambivalence wherein stigma is neither high nor totally eliminated. Indeed, the multicultural/multifaith dimension within society and its thinking has complicated matters considerably.

The stigma surrounding suicide remains just high enough to discourage people – especially the elderly – from talking about their suicidal thoughts. Some people feel that they might be labelled as weak, lacking faith, coming from bad families or indeed ‘mad’ if they were to declare their suicidal thoughts. This does not help when we are trying to detect early signs of suicide or reaching out to help victims of despair.

Any approach to prevent suicide should include the removal of blame and stigmatisation of that individual and his or her family. One would hope that all teachers and professionals from the different faiths will take into account this insight into the condition. Scientific approaches and spiritual approaches can work together in order to eliminate this kind of stigma and to make people more comfortable in trying to seek help in their moments of despair.


G. Tadros, D. Jolley Wolverhampton Health Care NHS Trust, Penn Hospital, Penn Road, Wolverhampton WV4 5HA, UK

Who is politicising psychiatry in China?

Having researched on qi gong-related mental health problems in China, I am up set to read the statement of Lyons (2001), based indirectly on estimates from Amnesty International and a letter to the Lancet, that “Soviet-style psychiatry is alive and well in the People’s Republic”.

In China, resurgence of interest in qi gong (‘exercise of vital energy’) started as early as 1980, when Chinese people were recovering from the social chaos brought about by the Cultural Revolution (1966–1976.). It is worth noting that qi gong-induced mental disorder was reported by Chinese psychiatrists long before recent accusations that psychiatry in China is used to imprison people who practise a specific kind of qi gong known as fazang. There have been a sizeable number of controlled phenomenological, treatment and outcome studies published in the past two decades that testify that qi gong-related mental disorders do not fall into a specific disease category recognised in the modern classifications (see Lee, 1996, for a brief review). In my own field studies, I interviewed people who suffered from acute psychosis induced by the inappropriate practice of qi gong in several regions of China as well as in Hong Kong. The condition is intriguing but real, and is deserving of


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more research from both medical and social science perspectives (Lee & Yu, 1995).

It has been estimated that not less than 5% of people in China practise qigong. The proportion of such people who develop psychiatric complications remains unknown but is likely to be very small. None the less, China has a population of 1.3 billion. The estimate that 600 people received psychiatric treatment for qigong-induced mental disorder cannot automatically be assumed to represent an abuse of psychiatry. China is a huge, heterogeneous and rapidly transforming country in which the standard of psychiatry varies widely from region to region. So the possibility of unethical psychiatric practice certainly exists. Additionally, in the current period of market reforms, the Chinese Government has withdrawn central funding for health care, and hospitals are forced to generate an increasingly large part of their own incomes. As a result, China is in the paradoxical position of having an inadequate number of psychiatric beds, yet at the same time a large number of beds that are unoccupied because families cannot afford to send patients to hospitals on a fee-for-service basis (Lee & Kleinman, 2000).

As a Chinese researcher who can be critical of recent social and moral changes in the country, I feel obliged to point out that even if unethical psychiatric practice existed in China (as it does in the USA, UK, or elsewhere), it would simply not be on the scale seen in the former Soviet Union. I must conclude that the views of Lyons and others who reach similar conclusions (e.g. “Contortions of Psychiatry in China”, New York Times, 25 March 2001) are premature and even dangerous, and beg the question of who really is politicosing psychiatry in China. As an international leader in psychiatry, the World Psychiatric Association must undertake its review of the accusations against Chinese psychiatrists prudently.

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**Lest we forget, again**

May I comment upon and add to the recent paper by Jones & Wessely (2001).

The deployment of psychiatrists in both World Wars was a constant battle waged against ignorance and prejudice (Shephard, 2000), even when as well prepared and aware as the Americans prior to the First World War (Salmon, 1917). It appears that each generation is doomed to relearn the lessons of combat psychiatry.

Although the authors describe its effects, they make no mention of combat psychiatry’s touchstone, ‘evacuation syndromes’. Described by the Russians in 1904–1905 they revealed what happened when a soldier’s social role is replaced by that of a patient, i.e. the ‘fixation’ of symptoms (Awtorkatrow, 1907). A similar type of problem may be seen in civil practice (Hadarl, 1996).

Combat-related military diagnostic practice has been, and remains, problematic (especially in research) as the aim is to minimise stigma, normalise the experience where possible and positively emphasise recovery (McCarroll et al, 1993). During combat, military medical officers have the moral and ethical dilemma that their ‘patient’ is the organisation rather than the individual and is affected by whether they relate predominantly to the majority (civilian) or minority (military) culture. While doctors may feel compassion towards those who break down, evacuation may mean the lives of those who remain behind are made more uncomfortable and dangerous – hardly surprising therefore that peers or commanders may not view breakdown sympathetically.

Acute or post-combat psychological reactions are multi-factorial in aetiology. Their genesis is the product of an interaction between the individual, the event, the environment (before, during and after) and the culture from which individuals hail and to which they return. Hence, rates may range from 0 to 100% in the same theatre of operations (Noy et al, 1987). Although there is a direct relationship between physical and psychological casualty rates, this relationship may be stated more bluntly: winners get fewer psychological casualties.

The word ‘fatigue’ is used loosely by Jones & Wessely. During the discussion of the Normandy offensive they state that “high percentages were also a function of widespread battle fatigue in soldiers who had already fought in North Africa . . .”; this should read war-weariness. In 1939, unlike 1914, there was no euphoria about the impending war and throughout the Second World War there was a feeling that “I’ve done my bit, now it’s time for someone else to do their’s” – this certainly seemed true in experienced veterans recalled to duty in Normandy and Korea.

Although often forgotten, the lessons of military psychiatry are as true today as in 1904–1905. Military psychiatrists cannot escape the social consequences of their labelling behaviours – perhaps this is the current combat psychiatry lesson to be forgotten!


Hadarl, N. M. (1996) If you have to prove you are ill, you can’t get well: the object lesson of fibromyalgia. Spine, 21, 2377–2400.


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**Authors’ reply:** Constructive criticism from a collaborator is always welcome. Palmer is right to point out the importance of evacuation syndromes, although sadly no psychiatric casualty statistics are readily available from the Russo-Japanese war. He also properly points out the broader cultural environment in which psychological casualties are framed. However, the purpose of our paper was to suggest that these factors shape the expression of these disorders rather than their incidence, which is largely determined by battle intensity irrespective of place or period. This explains why rates may vary considerably in the same theatre of operations as we demonstrated (Jones & Wessely, 2001, Table 4). Although it is generally true that “winners get fewer

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S. Lee Department of Psychiatry, I/Y/F, Prince of Wales Hospital, Shatin, Hong Kong

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I. P. Palmer Department of Psychiatry, Royal Defence Medical College, Firt Blockhouse, Gosport, Hampshire PO12 2AB, UK
psychological casualties”, this does not apply to Pyrrhic victories. Ultimately, the French defeated the Germans at Verdun in December 1916 but suffered greater casualties, many of which were treated in their newly established ‘neurological’ centres set up close to the front line (Roudelbush, 1995).

We cannot accept that the term ‘fatigue’ was misused. In fact, the War Office report (1951) from which we quoted used both “exhaustion” and “fatigue” to describe servicemen suffering from acute combat stress (War Office, 1951: 7). It is not true to say that all of these men were simply ‘war-weary’ as Palmer claims. A detailed analysis of 133 cases admitted to 30 corps’s Exhaustion Centre in the week ending 18 June 1944 showed that 47 (30.7%) were recently enlisted replacements (Wishart, 1944). It is likely that these men had not been given adequate time to become fully assimilated in their units and, without the protection of group cohesion, rapidly broke down. Equally, UK reservists recalled to fight in Korea, who might be presumed to have been war weary, often recorded lower rates of cold injury (an index of morale) than their younger and less experienced counterparts (Watts, 1952).


An alternative to interruption of treatment in recurrent clozapine-induced severe neutropenia

The use of clozapine is limited by the potential for haematological adverse effects (Young et al, 1998). Facing the occurrence of neutropenia the generally accepted attitude is to interrupt the treatment, and rechallenge with clozapine is usually avoided. We report the case of a woman with schizophrenia who was re-challenged with clozapine 10 years after she had developed severe neutropenia under clozapine, and who has been kept on this medication despite the occurrence of three episodes of severe neutropenia by using granulocyte and macrophage colony stimulating factor (GM-CSF) repeatedly.

Miss M. was first admitted in 1988 for an acute psychotic episode. After failing to respond to two standard neuroleptics she was started on clozapine. Her clinical situation improved markedly. The treatment was interrupted after 6 weeks when she developed severe neutropenia. Despite various treatments she continued to hallucinate and be delusional over the next 10 years. In 1998 she was admitted because of the aggravation of her clinical state. During her 8-month hospital stay, olanzapine and serindole, alone and combined with benzodiazepines, antidepressants and mood stabilisers, were tried without improvement. Clozapine was reintroduced. The clinical situation improved markedly and the patient left the hospital 3 weeks later. She eventually went through three episodes of severe neutropenia at weeks 10, 35 and 48, that were all successfully treated with one subcutaneous injection of GM-CSF. The clozapine dose had been gradually increased up to 450 mg/day by week 40.

The use of colony stimulating factors has been reported as a means to continue treatment despite the occurrence of severe neutropenia. However, in the case described the cytokines had to be administered only once and the dosage of clozapine was relatively low (Sperner-Unterreger et al, 1998). In the present situation, the treatment was continued despite three successive episodes of severe neutropenia and the dosage of clozapine being increased up to 450 mg/day. Even if this strategy should remain exceptional, it offers an alternative to the interruption of treatment with clozapine in some of the most severe cases.

Declaration of interest

None with respect to Novartis (manufacturers of clozapine). P.B. is on the advisory boards of Pfizer and Eli Lilly, and has received grants from Pfizer, Lundbeck, AstraZeneca and Aventis, who have interests in the manufacture of antipsychotics.


P. Conus, N. Nander, P. Baumann

Département universitaire de psychiatrie adulte, CH-I081 Prilly-Lausanne, Switzerland

One hundred years ago

The Medico-Psychological Association of Great Britain and Ireland

The Medico-Psychological Association of Great Britain and Ireland held a general meeting on Nov. 21st at 11, Chandos-street, W., which was presided over by Dr. Fletcher Beach and was numerously attended. The meeting began at four and lasted for nearly three hours, three papers, with interesting discussions on each, being read in that time. The first paper was on Mental Disorders dependent on Toxemias, by Sir Dyce Duckworth, and will be found printed in full at p. 1475 of this issue of The Lancet. Our report of the discussions and of the other papers will
Declarations of interest
D. Owens and A. House
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