Correspondence

EDITED BY KIRIAKOS XENITIDIS and COLIN CAMPBELL

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Omega-3 fatty acid for recurrent self-harm: unanswered questions

The study by Hallahan et al (2007) has clinically important implications but before accepting the findings as valid we wish to raise a few points regarding some of the methodological and analytical aspects.

Of the 392 patients initially assessed for eligibility, only 39 (10%) completed the study, a large number (343) having been excluded for various reasons. Although this rigorous selection procedure might have enhanced the internal validity of the findings, we are concerned that the generalisability of the findings in the real-world clinical situation (i.e. external validity) might have been compromised.

Certain sample characteristics merit attention. Apart from mentioning that participants had had at least one lifetime self-harm episode in addition to the index episode, the report does not provide any data on the number, frequency, severity and recency of self-harm episodes. These data are important to characterise the sample and to ensure that they did not differ between the two groups. For example, the risk profile of a 60-year-old patient with two self-harm episodes 10 years apart would be very different from that of a 20-year-old with the previous episode only 10 days prior to the index episode. Furthermore, in patients with borderline and other personality disorders, suicidality and impulsivity can vary drastically over time, even in a single day. Instruments rated every 4 or 6 weeks might not capture the ‘real’ picture. Finally, significantly more participants in the placebo group were single or divorced compared with the active drug group. In view of this significant difference, marital status should have been included in the logistic regression and other analyses.

For analysis of suicidality scores the two groups were compared after categorical classification of values (no suicidal ideation v. presence of any suicidal ideation) to obtain a statistically significant difference. For all other variables of interest mean scores were compared. When the mean suicidality scores were compared the difference was not statistically significant. Indeed, it is interesting to note that the proportion of self-harm episodes was actually higher during the study period in the patients on active drug (7/22, 38.2%) compared with those in the placebo group (7/27, 25.9%), although the difference was not statistically significant.

Finally, it is not clear what the findings really mean in terms of decrease in ‘surrogate markers of suicidal behaviour’. Hallahan et al discuss the findings in terms of improved mood and well-being, but the logistic regression analysis showed that depression and other psychological measures did not have any effect on the suicidality score. Other surrogate markers such as impulsivity and aggression scores were not significantly different between the two groups.


D. Basu Postgraduate Institute of Medical Education and Research, Chandigarh, India. Email: db_sm2002@yahoo.com

P. K. Barnwal Postgraduate Institute of Medical Education and Research, Chandigarh, India doi: 10.1192/bjp.191.3.264

Authors’ reply: We thank Basu & Barnwal for their comments. As regards exclusion of so many patients, we stress that easily the biggest reason for exclusion was that the episode of self-harm was the patient’s first. We make it clear why we chose recurrent self-harm rather than all patients with self-harm. The other exclusion criteria seem reasonable (regular fish consumption, etc.) and we see no reason why the findings are not applicable to ‘real-world’ patients. We knew that with such a small population subgroup analysis would be of dubious validity, therefore further defining the groups (e.g. according to recency of other self-harm episodes) was redundant. We certainly should have excluded those patients whose other episode(s) of self-harm were remote from the current one, but we chose not to.

We agree that more measuring points would have been desirable, especially in this capricious sample. This was a resource issue rather than a methodological one. We note the point regarding marital status being different between the two groups but re-analysis of the data controlling for this did not materially affect the results. It was agreed at study outset that in the absence of sufficient power to analyse actual differences in recurrent self-harm we would use the suicidal ideation sub-scale of the OAS–M. One either has suicidal ideation or not (whereas one can have ‘some’ depressed mood) and it seems appropriate to use a categorical measure here.

We suggest using ‘potential marker’ for ‘surrogate marker’ and confess we used the latter word loosely. There was quite good correlation (r=0.5) between measures of depression and the OAS–M suicidality sub-scale score. None the less logistic regression suggested that changes in suicidality were independent of depression scores, which indicates that factors additional to affect drive suicidal ideation. We agree that these findings could be clinically important. However, our findings can be regarded as no more than pilot data, owing to the small sample size. As fish oils are not patentable products, a larger study (with enough power to investigate actual reductions in self-harm) is unlikely to come from industry. Therefore we are continuing to seek funding for such a study.

M. Garland St Ita’s Hospital, Portran, County Dublin, Ireland. Email: mgarland@ireland.com

B. Hallahan Department of Psychiatry, Beaumont Hospital and the Royal College of Surgeons in Ireland

J. R. Hibbeln Laboratory of Membrane Biochemistry and Biophysics, National Institute on Alcohol Abuse and Alcoholism, Rockville, Maryland, USA

J. M. Davis Institute of Psychiatry, University of Illinois at Chicago, Illinois, USA doi: 10.1192/bjp.191.3.264a
Callous–unemotional traits and autistic psychopathy

Viding et al (2007) made no reference to autistic psychopathy (Asperger, 1944) nor did any of the other papers in Supplement 49 on assessment risk and outcome in severe personality disorder. The severe unempathic conduct and aggression problems were well recognised by Asperger (1944) and overlap with what Viding et al (2007) describe as ‘more severe, aggressive, and stable pattern of antisocial behaviour and a specific neurocognitive profile indicative of defects in affect processing’. This is precisely what children (and adults) with autistic psychopathy and antisocial behaviour demonstrate (Fitzgerald, 2001, 2003).

Diagnostic stability: clinical v. research

Baca-Garcia et al (2007) highlight some of the important issues related to current nosological systems but other issues need consideration. They voice their concern that with such a high degree of diagnostic instability, the validity of epidemiological, clinical and pharmacological research is questionable. However, in most studies appropriate diagnostic schedules and interviews are used for assessment of patients and a high degree of diagnostic stability has been shown for patients assessed in this manner (Tsuang et al, 1981; Schimmelmann et al, 2005).

Baca-Garcia et al (2007) did not discuss factors such as the level of qualification and number of years of experience in psychiatry of the evaluators, whether the patients were evaluated by the same or different assessors at each visit, the place (i.e. in-patient, outpatient, emergency setting) of first contact, the mean duration of contact, etc., which can influence diagnostic stability. It is also not clear whether at each follow-up proper diagnostic evaluations of patients were performed before diagnosis was recorded.

Furthermore, diagnosis was recorded using ICD–9 codes, but clinicians were using the ICD–10 classification system and this might have lead to errors in conversions and reconversions. Although Baca-Garcia et al reported that clinicians entered one or two diagnoses at the time of evaluation, they have not presented any data regarding comorbidity. Furthermore, when we compare the ‘diagnosis received in at least 76% of evaluations’ the diagnostic stability in the emergency setting was more than in the out-patient setting for all disorders except eating disorders. This perhaps reflects the likelihood of the evaluators recording the previous diagnosis rather than doing a complete diagnostic evaluation in the emergency setting.

Baca-Garcia et al raise issues which are common in day-to-day practice and highlight the fact that the proper evaluation of the patient requires use of appropriate diagnostic schedules and obtaining information from all possible sources. It is inappropriate to conclude from the study that our diagnostic systems and all research based on this nosological system are flawed.

Authors’ reply: Asperger’s use of the term psychopathy refers to personality disorder/psychopathology rather than to psychopathy as defined by current criteria. Recent research carried out with colleagues indicates that although there are individuals who have the neurocognitive profile associated with both autistic-spectrum disorders and psychopathy, most individuals with autistic-spectrum disorders (even those with antisocial behaviour) do not show neurocognitive deficits characteristic of psychopathy (Rogers et al, 2006). More importantly, a case review of 177 cases originally diagnosed by Asperger found no raised incidence of criminal offences compared with rates in the general population (Hippler & Klicpera, 2003). It is clear that there are individuals with Asperger’s syndrome/autistic-spectrum disorder who commit crimes (Baron-Cohen, 1988; Scragg & Shah, 1994). However, Asperger’s psychopathy does not equal psychopathy as defined by current practice.


E. M. Viding Department of Psychology, University College London, London, UK. Email: e.viding@ucl.ac.uk doi: 10.1192/bjp.191.3.265a

Authors’ reply: Our article reports on diagnoses of real patients in the real world and hence variability ranges and the diagnostic process may be affected by factors such as psychiatrist or practice characteristics.

Regarding the question of whether full assessments were performed at each visit, we believe that practitioners tend not to update diagnoses if there is no salient clinical change. We hypothesised that clinicians would be less likely to change diagnoses, biasing the data against our reported finding.

Perhaps the most compelling point is that not all diagnoses were unstable. Thus, it is more likely that our findings reflect inconsistencies in our nosological
system rather than clinician or practice characteristics, or setting effects. For example, some disorders may not always begin with the features required for diagnosis (e.g. mania in bipolar disorder) and therefore diagnostic instability may reflect the time required to consolidate the diagnosis (Baca-Garcia et al., 2007).

Our nosological system is in constant evolution, with major revisions each 15 years. Unfortunately, administrative procedures change more slowly than psychiatrists. Recording from one ICD system to another may affect the validity of diagnoses but not stability, since any error in the conversion of diagnostic codes would likely be constant, given the use of computerised algorithms.

Diagnoses in pharmacological and clinical studies have good internal validity (appropriate diagnostic schedules and interviews). In general, follow-up periods are short and selection bias is likely since participants are selected from specific programmes or units, often based on meeting specific entry criteria. Of note, Perala et al. (2007) recently reported that the National Hospital Discharge Register was the most reliable means of screening for psychotic and bipolar disorder and was much better than the Composite International Diagnostic Interview (CIDI). They concluded that multiple information sources are key to accurate diagnoses. Studies such as ours, where patients are followed over long periods and across several settings, are closer to this approach than clinical trials based on diagnostic schedules and interviews performed in a research unit over a short period or large cross-sectional epidemiological studies based on a single assessment.


E. Baca-Garcia Department of Psychiatry, Fundacion Jimenez Diaz University Hospital, Autonomous University of Madrid, Spain. Email: ebacgar2@yahoo.es

M. M. Perez-Rodriguez Department of Psychiatry, Ramon y Cajal University Hospital, Madrid, Spain

M. A. Oquendo Department of Neuroscience at the New York State Psychiatric Institute and Columbia University, New York, USA. doi: 10.1192/bjp.191.3.265c

Limitations of cognitive–behavioural therapy for sleep disorders in older adults

When the possible side-effects of hypnotics are considered, there is an argument for alternative treatments of sleep disorders in older adults. Svivertsen & Nordhus (2007) emphasised the role of cognitive–behavioural therapy (CBT) in the management of sleep disorders in this population. However, there are also limitations to this approach.

Mental health practitioners or physicians with formal sleep medicine training currently deliver CBT, but they are few in number and could not cater for all that need therapy (Wetzler & Winslow, 2006). This could be the main reason for the prescribing of hypnotics for older adults despite knowledge of their side-effect profile and potential for misuse. Therefore, more workshops are needed for training of mental health professionals in CBT so that they can incorporate these techniques in their routine care of older adults.

There are no clear guidelines about the optimum number and duration of treatment sessions for sleep disorders, particularly for the elderly. It is also unclear how long CBT continues to be effective. Moreover, CBT refers to a number of non-pharmacological treatments for insomnia, but which are the most effective needs more research. There is insufficient evidence to recommend sleep hygiene education, imagery training and cognitive therapy as single therapies or as additions to other specific approaches (Morgenthaler et al., 2006).

Research groups are also working on other effective non-pharmacological interventions for older adults such as acupuncture (Chen et al., 1999). Exercise (Montgomery & Dennis, 2004), although not appropriate for all in this population, may also help in inducing sleep. Nevertheless, Sivertsen & Nordhus gave a new insight into this neglected area and provided an impetus for more studies in the elderly.


O. Prakash Geriatric Clinic and Services, Department of Psychiatry, National Institute of Mental Health and Neurosciences, Bangalore 560029, India. Email: op@nimhans.kar.nic.in doi: 10.1192/bjp.191.3.266c

Authors’ reply: Dr Prakash calls for more training workshops to improve implementation of cognitive–behavioural therapy (CBT) for older adults with sleep disorder. Although we agree that there are too few sleep specialists, we believe that the key to more effective implementation is to provide the same training for other health professionals, including primary care nurses. Although there is no consensus on which component should be included in CBT for insomnia, our experience is that sleep restriction and stimulus control are both crucial for improving sleep in this age group. These components can easily be adapted for use by most health professionals.

In Norway, the Norwegian Medical Association has started to offer training workshops on CBT for insomnia for its members and the Norwegian Psychological Association will soon follow this important initiative.

However, we share Dr Prakash’s concern that there is still insufficient research on how to optimise the treatment and there is clearly a need for studies to determine which component works best and for whom.

B. Sivertsen Department of Clinical Psychology, University of Bergen, Bergen, Norway. Email: Borge.Sivertsen@psyk.uib.no

I. H. Nordhus Department of Clinical Psychology, University of Bergen, Norway doi: 10.1192/bjp.191.3.266a
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