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depressive disorder might be associated with free-radical damage and subsequent lipid system abnormalities.

Maes *et al* (1996) found a significant decrease in the ω3 fractions in cholesteryl esters in patients with major depression compared with minor depression or healthy controls. They also reported a significantly increased ratio of arachidonic acid to eicosapentaenoic acid in both cholesteryl esters and phospholipids. Peet *et al* (1998) found depleted total ω3 PUFA and docosahexaenoic acid in the red blood cell membrane of patients with depression. They have argued that the changes in serotonin receptor number and function caused by changes in PUFA provide the theoretical rationale connecting fatty acids with current neurotransmitter and receptor theories of depression. The possibility of dietary supplementation of fatty acids has not received enough attention in clinical trials of depression, although there are anecdotal reports of their efficacy in stress-induced aggressive behaviour (Hamazaki *et al*, 1996).

Hence, advances in lipid neurochemistry may prove useful not only in understanding schizophrenia, but also in understanding and potentially treating other psychiatric diseases including depressive disorders.

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S. Swain Department of Neurochemistry, Institute of Human Behaviour & Allied Sciences (IHBAS), Shahdara, Delhi 110095, India

C. Pradhan Department of Psychiatry, Institute of Human Behaviour & Allied Sciences (IHBAS), Shahdara, Delhi 110095, India

Family functioning and adolescent anorexia nervosa

Sir: We read with interest the article on difficulties in family functioning and adolescent anorexia nervosa by Gowers & North (1999). This is an important area of research and we applaud the authors for a data-based publication in the *Journal*. We would, however, like to offer an alternative interpretation of their findings.

There were no significant associations between global ratings of family functioning by clinician, parent and patient and severity of anorexia nervosa; however, the authors conclude that there are some associations with separate sub-scales of family functioning. It is important first to highlight that these correlations are very small. The patients' opinion that family difficulties, such as resolving problems, increase with their increasing weight or sexual activity, is not an unexpected finding. Most patients with anorexia experience a re-emergence of feelings, particularly those of a conflictual nature, as their weight increases. This is particularly evident around sexual feelings, with which a significant proportion of adolescents with anorexia struggle. The experience of a decrease in difficult feelings with weight loss reinforces the usefulness of anorexia as a defence. Their heightened awareness of family difficulties with weight increase and clinical recovery is then to be expected.

The authors report no correlation between change in family functioning and clinical change as defined by the Morgan–Russell scale (1988). They have not given the range of change scores on their measure of family functioning, or the standard deviation. These would have facilitated the reader's assessment of the findings. The authors previously reported family functioning as predicting outcome of the anorexic illness at one year. That observation and their current finding of minimal change in family functioning during treatment cannot be offered as evidence for or against family dynamics as contributory factors in the aetiology of anorexia. We suggest that it would be more beneficial to contrast engagement or ability to effectively use therapy with family functioning. Anorexia is not caused by disturbed family dynamics; severity and aetiology of the illness is multi-factorial. However, the capacity of a family to use a treatment may be severely diminished by their own internal and difficult dynamics. The Family Assessment Device may merely be a measure of engagement which, in turn, will impact on outcome of an anorexic illness.

Gowers, S. & North, C. (1999) Difficulties in family functioning and adolescent anorexia nervosa. *British Journal of Psychiatry*, **174**, 63–66.

Morgan, H. G. & Hayward, A. E. (1988) Clinical assessment of anorexia nervosa. The Morgan–Russell outcome assessment schedule. *British Journal of Psychiatry*, **152**, 367–371.

C. Dare Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF

A. Key St George's Hospital Medical School, Cranmer Terrace, Tooting, London SW17 0RE

Authors' reply: We are grateful to Drs Dare and Key for their interest in and comments on our paper. They are right to draw attention to the limitations of our findings and to put the role of family difficulties in perspective alongside other variables in the multi-factorial aetiology of anorexia nervosa.

However, we wish to take issue with two of their points. First, although we agree with Dare & Key that anorexia nervosa can be interpreted as a defensive strategy against difficult feelings, this view is not universally held and, where it is, it is usually construed as an individual defence, rather than serving to reduce difficulties within the family. We contend that this is a new finding.

Second, and more importantly, the assertion that family dynamics do not cause anorexia nervosa is surely unhelpful. Although in our series, our clinical experience led us to believe that the families were without exception caring, conscientious and determined to be good families, surely family life has to be considered one of the major influences on adolescent development. If family factors can contribute to positive aspects of development like resilience, confidence, educational success and the ability to form satisfactory interpersonal relationships, then surely they can play a part in the development of negative ones. We suggest that the following features, impacting on family functioning, may, among others, be very likely to contribute to the development of eating disorders: parental disharmony and separation; parental eating disorders; death and severe illness in first-degree relatives; emotional, physical and sexual abuse; and delinquency and unwanted pregnancies in siblings and their perceived affects on family life.

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Of course, the aetiology of anorexia nervosa is multi-factorial, but to suggest that difficulties in family functioning do not cause the condition, is surely as inaccurate as to say that the condition is not caused by genetic factors, life events or societal pressures to be thin. All these factors are likely to play a part, to a greater or lesser extent, in an individual case.

S. G. Gowers Academic Unit, Young People's Centre, Pine Lodge, 79 Liverpool Road, Chester CH2 1AW

C. North North Essex Child & Family Consultation Service, Clacton-on-Sea, Essex

Suicide attempts *v.* deliberate self-harm

Isometsä & Lönnqvist (1998), who focus on a clinical variable of deliberate self-harm for predicting future repetition or eventual suicide, only touched upon the issue of the ambiguity in their definition of suicide attempts/deliberate self-harm. The authors contend that this ambiguity in definition does not introduce "any bias in the reporting of previous attempts".

Unfortunately, any ambiguity in a definition always has the potential to introduce bias in epidemiological studies and this is why operating definitions of caseness are so important. Stating their operational definition could have removed the ambiguity and the potential for bias.

It is important to recognise the two syndromes of self-harm, namely suicide attempt (failed suicide) and non-fatal deliberate self-harm when death was not intended, sometimes pejoratively referred to as parasuicide, but which term remains useful. In the former syndrome, the act is designed to be fatal and any survival is accidental. In the latter, the act is designed not to be fatal and so any death is accidental. Deliberate self-harm is more common in females, although differences in gender-specific rates are narrowing (Hawton *et al*, 1997). It often leads to frustration among health care workers and deterioration in their relationship with the patient, with the potential for developing "malignant alienation" (Watts & Morgan, 1994). Any approach to secondary prevention of suicide must recognise this distinction, although the attitude towards the latter syndrome must be serious, sensitive and sympathetic. Unless the definition of caseness of 'suicide attempt' in this study was clearly stated to exclude the second group of subjects, there could be a serious bias to the study. This could account for the higher proportion of female suicides who were not first-time attempters. The bias in the study would be that the finding that most male completed suicides die in their first suicide attempt may be a spurious one resulting from the lack of operational definition of suicide attempt and the inclusion of 'parasuicide' in the study.

Also, the authors have not indicated whether pregnant women and postnatal women have been excluded from the study. Pregnancy and motherhood have been postulated to provide some protection against suicide (Appleby, 1991).

The implication is that the major conclusion of the study – that the approach to suicide prevention should be different for men and women – may be erroneous.

Appleby, L. (1991) Suicide during pregnancy and in the first postnatal year. *British Medical Journal*, **302**, 137–140.

Hawton, K., Fagg, J., Simkin, S., *et al* (1997) Trends in deliberate self-harm in Oxford, 1985–1995. Implications for clinical services and prevention of suicide. *British Journal of Psychiatry*, **171**, 556–560.

Isometsä, E. T. & Lönnqvist, J. K. (1998) Suicide attempts preceding completed suicide. *British Journal of Psychiatry*, **173**, 531–535.

Watts, D. & Morgan, G. (1994) Malignant alienation: Dangers for patients who are hard to like. *British Journal of Psychiatry*, **164**, 11–15.

L. O. Ogundipe Edward Myers Addiction Unit, City General Hospital, Stoke-on-Trent ST4 6QG

Authors' reply: Dr Ogundipe has given us the opportunity to clarify several points. His main argument was that our definition of a suicide attempt was ambiguous and did not exclude parasuicides (as opposed to true suicide attempts), which resulted in a bias in our comparison between genders, as more females conduct parasuicidal acts than males; we disagree.

The definition for a suicide attempt in the research phase of the National Suicide Prevention Project in Finland was a clinical one, based on the field researcher's (a mental health professional) evaluation of the clinical significance of the act. Some degree of suicide intention needed to be present in order to rate an act as a suicide attempt, and the act needed to be fully undertaken, not only planned or aborted. We do not share Dr Ogundipe's conviction that suicidal acts can be reliably classified into 'suicide attempts' and 'parasuicides'. Studies of suicide intent among suicide attempters/parasuicides do not show a bimodal distribution of suicide intent (Suominen *et al*, 1997). In the pan-European WHO/Euro Multicentre Study on Parasuicide, the concepts of 'suicide attempt' and 'parasuicide' were used interchangeably (Ostamo & Lönnqvist, 1994).

Contrary to what Dr Ogundipe expects, the higher rate of parasuicide among females is not true in Finland. In fact, Finland is the only country in Europe where males seem to have a slightly higher incidence of parasuicides than females (Ostamo & Lönnqvist, 1994).

Our overall prevalence of suicides with previous attempts (44%) in Finland was generally in line with the five earlier, smaller studies we referred to (21–32%). As the mean age of suicides in Finland was somewhat lower, and preceding attempts are more prevalent in the younger age groups, the minor difference would likely totally disappear if the data were age-adjusted before comparisons. Finally, we would like to stress that we did not claim that suicide prevention needs to be totally different for males or females. On the grounds of our nationwide data, we pointed out the importance of early intervention, particularly (but not exclusively) among males.

Ostamo, A. & Lönnqvist, J. (1994) The epidemiology of attempted suicide in Helsinki, Finland 1989–1993. In *Attempted Suicide in Europe. Findings from the Multicentre Study on Parasuicide by the WHO Regional Office for Europe* (eds A. J. F. M. Kerkhof, A. Schmidtke, U. Bille-Brahe, *et al*), pp. 137–158. Leiden: DSWO Press.

Suominen, K., Isometsä, E., Henriksson, M., *et al* (1997) Differences in hopelessness, suicide intent and impulsivity among suicide attempters with major depression, alcohol dependence or both. *Acta Psychiatrica Scandinavica*, **96**, 142–149.

E. Isometsä, J. Lönnqvist Department of Mental Health and Alcohol Research, National Public Health Institute, Helsinki, Finland

Lithium and suicide prevention

Sir: Isacsson *et al* (1999) recommended that highly suicidal patients should be referred to psychiatric hospitals for supervision and often electroconvulsive therapy (ECT). We would like to draw attention to observations that might contribute to the prevention of suicides in highly suicidal patients.