Adult sequelae of child sexual abuse

SIR: It was with great interest that we read the article about adult sequelae of child sexual abuse by Sheldrick (*Journal*, May 1991, **158**(suppl. 10), 55–62). We, too, have done research work on the prevalence and consequences of sexual abuse in childhood in psychiatric patients, and in a non-clinical sample. We feel that our findings (Kinzl & Biebl, 1991) confirm and supplement Sheldrick's results.

The traumagenic dynamics of powerlessness and betrayal (Finkelhor & Browne, 1985) were demonstrated in our patients. Together with a lack of emotional and social support on the part of the mother, the often-found ego weakness of the victim prevents an alloplastic mode of coping with the traumatic experience, which results in a labilisation, particularly in situations of separation and loss.

Compared with patients with mental disorders, the familial situation of the non-clinical sample with sexual abuse in childhood was significantly more frequently characterised by:

(a) more social support from extrafamilial confidant(e)s
(b) less frequent mental disorder on the part of the mother
(c) more positive experiences in partner relationships in later life
(d) less frequent and shorter-lasting incestuous sexual abuse by the father or stepfather.

FINKELHOR, D. & BROWNE, A. (1985) The traumatic impact of child sexual abuse: a conceptualization. *American Journal of Orthopsychiatry*, **55**, 530–541.

KINZL, J. & BIEBL, W. (1991) Sexual abuse of girls: aspects of the genesis of mental disorders and therapeutic implications. *Acta Psychiatrica Scandinavica*, **83**, 427–431.

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Prognosis of depression in the elderly

SIR: Meats *et al* (*Journal*, November 1991, **159**, 659–663) make the interesting observation that in four studies using in-patient samples, a relatively good prognosis has been observed for depression in old age, whereas in studies including both out-patients and in-patients, outcome was less favourable. The authors suggest two possible explanations: either different thresholds for rating symptoms, or inter-district differences in therapeutic effectiveness.

It is possible that the means of selecting the samples has been responsible for the different findings reported to date. In addition, other possible hypotheses can be suggested to explain the differences in outcome between in-patient-only and mixed samples. Firstly, there may be a bias towards the admission of patients who, it is anticipated, will respond to in-patient treatment. Secondly, those patients in out-patient follow up are often cases of resistant depression. Those who have responded favourably to treatment have perhaps tended to be discharged.

Finally, and perhaps more worryingly, perhaps psychiatrists are spending too much energy keeping patients out of hospital, and perhaps we should be admitting a higher proportion of cases for intensive treatment. In other words, in-patient treatment may be a more effective therapeutic measure, and this improvement in prognosis, in turn, might be due to more intensive treatment or to careful planning at the time of discharge.

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Decontextualisation and mythology

SIR: Dr Littlewood is entitled to express his viewpoint on *A History of Medicine in Papua New Guinea* (*Journal*, August 1991, **159**, 317) but his perjorative style is so littered with errors that the potential reader might well be more interested in the facts.

The book is described as "Whiggish" although there is nothing in the *Shorter Oxford English Dictionary* that would concur with such a deprecatory statement. There are no Whigs of any sort in Papua New Guinea.

He uses the American colloquialism 'medic', the word 'Melanesian' and the title 'Papua and New Guinea' all of which are grossly inappropriate. There are no 'medics' in Papua New Guinea; the word is unknown in the country. 'Melanesian' refers to a much wider geographical area than Papua New Guinea, and along with the use of 'Papua and New Guinea' shows an apparent lack of knowledge of the history, geography and name of the country.

He states that "a fuller account of how the different medical systems interact can be found in Frankel & Lewis' *A Continuing Trial of Treatment: Medical Pluralism in Papua New Guinea* (1989)". This is incorrect. With all due respect to the authors of that volume, which has virtues in other respects and which is written by 12 non-medical ethnographers

https://doi.org/10.1192/bjp.160.2.277c Published online by Cambridge University Press

and edited by two non-psychiatric physicians, no reference is made to neuropsychiatric disorders as such in its 334 pages, nor is there any mention appropriate to them in the long index of subjects. No mention is made of the ubiquitous untreated epileptic, the explosive amok-runner, the frequent bipolar affective disorders, the episodic dyscontrol individual, the *epigugu, deki, manga, buwa, bandovisi, dabanapaia, longlong, jirodusari* and many others defined as such by the people themselves and not by any decontextualised doctors. In short, the fuller account of interaction to which Dr Littlewood refers does not exist.

Dr Littlewood refers to what he calls 'dated attacks'. A historical record cannot be referred to as 'dated' nor are any attacks made on anyone.

He refers the reader to an alternative book on betel chewing without reference to the more comprehensive and broader account given in one of the author's *Arecaidinism* (*Canadian Journal of Psychiatry*, **24**, 481–488, 1979).

He states that the book is one "independent of any apparent knowledge of local social structure". This also is incorrect. He ignores the fact that three of the contributors are professional anthropologists, others have university qualifications in anthropology and still others are well versed in the social sciences. His use of the phrase 'social structure' implies a belief in a single such entity, a further apparent lack of knowledge of local circumstances. There is no such single entity; there are in fact 800 distinct and separate social structures, each one stemming from its own language.

I can appreciate and sympathise with Dr Littlewood in his attempt to write about a geographical area from such a limited database. There is a solution, however, if he wishes to continue in this fashion. He could, for example, abandon his London office and peripatetic yo-yo visits to various developing countries and get a permanent job of work in one of them. In this manner, his way would be easier, with at the same time an opportunity to make a worthwhile contribution to the traditional peoples themselves.

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AUTHORS' REPLY: Given Sir Burton's comments, it may be helpful for any reader to compare them with my review. I am concerned that I am taken perhaps as criticising his pioneering development of Western psychiatry in Papua New Guinea. I am not. The problem is his book.

"Yo-yo"? Brilliant trope, but I myself have never claimed any expertise in Melanesian studies, have never visited the area, kinetically or otherwise, nor published in the subject. Nonetheless, I have considerable reservations about *A History of Medicine in Papua New Guinea* which the space available for correspondence in the *Journal* allows me to consider at somewhat greater length than in its conventionally brief reviews.

I take Sir Burton's witty comments on national nomenclature (cf. Van der Veur, 1966), 'medic', 'structure' and 'Whiggish' as a pastiche of a position commonly advocated in the newer studies in cultural psychiatry: how much can we use a 'universalist' (etic but often Western) terminology to characterise the patterns of life in societies other than those of the observer? If 'psychosis' is a concept elaborated within Western psychiatry to describe and understand certain patterns which we understand as psychopathology, what does one lose – or gain – by applying them elsewhere? One of the famous instances of emic–etic confusions is the 'Seligman error': the anthropologist C. G. Seligman (1926) on this reading having denied the existence of severe mental illness in New Guinea for its patterns were apparently incorporated into existing social institutions. By contrast, Professor Burton-Bradley (1977), following F. M. Williams (1934), has articulated local religious movements in terms of psychopathology, an issue which developed into a notable debate in the *Transcultural Psychiatric Research Review*. My own position would be that we cannot reduce the etic to the emic, the naturalistic to the personalistic; nor the reverse. Both are equally valid and appropriate within their own limits of application.

The term "Whiggish" (and perhaps a larger dictionary would be helpful) refers to a type of writing, exemplified in 19th century "Whig historians" like Macaulay, which argued that historical development showed a clear, perhaps inevitable, progression to the modern and the liberal (Butterfield, 1931); the historical record is read through this virtually teleological assumption of the victory of contemporary rationalism. Objections to Whiggery are not only about its assumptions that what has happened was generally desirable, but that such history is not written by the 'losers'. In the issue under discussion, Burton-Bradley's book does not examine how the non-Europeans perceived and dealt with competing medical systems, how they made health-related choices, or indeed any serious consideration of what local medical practices were or are.

https://doi.org/10.1192/bjp.160.2.277c Published online by Cambridge University Press