interviewed in this study did so. Could this unusual feature be in some way related to their being the relatives of depressed probands rather than random community sample cases?

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SIR: We are grateful to Dr Craig and Professor Murphy for their close reading of our paper and the opportunity to resolve an apparent ambiguity. Their second paragraph is quite correct. In Tables III and IV of our paper, the life events referred to were in the three months before onset for those relatives with an ID level of 5 or greater, and three months before interview for the rest. We hope that this partially relieves their puzzlement referred to in the paragraph, and that we can fully relieve their perplexity by explaining that the same definition of life event was used for both proband and relative in Table III. They should note, however, that we are not making a case that life events have greater impact on probands than their relatives (and we agree that it would be difficult to sustain such an argument on the basis of the data described in our set of papers). The main point is that life events are strongly associated with the onset of depression in a community sample, but not in a sample of first-degree relatives.

The third question relates to the frequency of life events and the comparison we make between the community sample and the first-degree relatives. In our Tables IV and V we use the same definition of life events in both groups. Although we do not provide the results in the paper we did in fact find that threatening life events of any definition were more frequent in relatives than in the community. The apparent discrepancy between the event rate in the community group and that given by Bebbington *et al* (1981) is due to our use of figures which are weighted back to Bebbington's orginal sample in order to achieve an accurate estimate of the population frequency of recent events. This is necessary because of the two-stage sampling strategy employed by Bebbington *et al* (1981).

The final point raised by Dr Craig and Professor Murphy is an interesting one, and the question of why there should be a comparatively small proportion of chronic cases among first-degree relatives has also occurred to us. We are inclined toward a more mundane explanation than the one they offer. It seems likely to us that the dating of onset of very broadly defined depression is an inexact procedure and one where we may have adopted a different definition of time of onset from previous workers who have focused on community samples. We used a Past History Schedule (PHS) in conjunction with the Present State Examination (PSE) (McGuffin *et al*, 1986). The PHS/PSE interviews identify past episodes and define the most severe occurrence if multiple episodes are evident. It may be that this approach more clearly delineates the episodic nature of depression than does a less structured enquiry about past PSE-type symptoms over an extended period.

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Obsessive-compulsive rituals

SIR: Regarding Katz *et al's* letter from CIBA-GEIGY (*Journal*, December 1988, **153**, 845) about our clomipramine study (*Journal*, April 1988, **152**, 522–534), we can understand that our findings may be unwelcome to them. Their letter contains some confusion and mistakes that need clarification and correction.

In our literature review we wrote "There is no evidence that clomipramine is significantly better than other tricyclic drugs in OCD despite a widely held belief to the contrary." CIBA-GEIGY claim that