precipitated by non-specific stimuli, and crying or tearfulness observed in emotionalism may not be associated with alterations in mood (Poeck, 1969). No attempt was made to examine whether the symptoms assumed to be stress-related, in stroke patients with major depression, were correlated with the severity of functional impairment or with subjective perception of stressful situations.

It would be of interest to investigate the differences in prevalence of endogenous or melancholic symptoms between post-stroke major depression and major depressive disorder. Moreover, using the same classificatory procedure in sub-typing post-stroke depression and depressive disorders may cause limitations and constraints when phenomenological comparisons are made between these two disorders. To overcome this problem, classification of post-stroke depressive disorders could be approached from the bottom up, with the identification of depressive symptoms in stroke patients. By applying multivariate analysis, these symptoms can be grouped into clusters or syndromes, which can then be validated.

American Psychiatric Association (1987) *Diagnostic and Statistical Manual of Mental Disorders* (3rd edn, revised) (DSM–III–R). Washington, DC: APA.

Farmer, A. & McGuffin, P. (1989) The classification of the depressions. Contemporary confusion revisited. *British Journal of Psychiatry*, **155**, 437–443.

Gainotti, G., Azzoni, A. & Marra, C. (1999) Frequency, phenomenology and anatomical–clinical correlates of major post-stroke depression. *British Journal of Psychiatry*, **175**, 163–167.

Poeck, K. (1969) Pathophysiology of emotional disorders associated with brain damage. In *Handbook of Clinical Neurology* (Vol. 3) (eds P. J. Viken & G. W. Bruyn), pp. 343–366. New York: N. Holland.

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Author's reply: Dr Ramasubbu argues against our suggestion that psychological rather than neurological factors mostly account for post-stroke depression (PSD). His/her main argument is that our results could be due to a bias in selecting a control group of patients with endogenous depression. He/she remarks that it is not surprising that endogenous symptoms were more frequent in this control group, whereas affective symptoms related to brain damage prevailed in patients with major PSD. This argument could certainly be appropriate if the aim of our study consisted in matching the main symptoms of patients with major PSD and with major endogenous depression, but this was clearly not the scope of our paper. Patients with major endogenous depression were, in fact, the group against which we matched various subgroups of patients with major PSD observed at various time intervals from stroke. The scope of these comparisons consisted in assessing whether patients with endogenous depression are more similar to patients with major PSD observed immediately after the stroke than to those observed in more chronic post-stroke periods. A similar strategy had been used in previous studies of this field conducted by our research group, since the distinction between various forms of PSD plays a critical role in the model of PSD proposed by Robinson *et al* (see Starkstein & Robinson, 1989, for review). This model is, in fact, substantially based on the distinction between two forms of PSD: (a) the major form, due to a lesion encroaching upon the left frontal lobes and indistinguishable from the clinical and pathophysiological points of view from the major forms of endogenous depression; and (b) the minor form, considered as a form of reactive (dysthymic) depression and having no specific anatomical substrate. In a previous paper (Gainotti *et al*, 1997) aiming to test this original version of the Robinson *et al* model, the symptomatological profiles of patients affected by endogenous depression were matched with those in three groups of stroke patients, on the basis of DSM–III–R criteria, as having no depression, minor PSD, or major PSD. The following predictions were made: (a) if major PSD is indistinguishable from endogenous depression, whereas minor PSD is a reactive form of depression, then the symptomatological profile of patients with a major form of PSD should be more similar to that of patients with endogenous depression than to that of patients with a form of minor PSD; (b) if, on the contrary, no qualitative difference exists between major and minor forms of PSD, then patients with major PSD should be more similar to those with a minor form of PSD than to those with a form of endogenous depression. Our results clearly supported the second prediction showing that a continuum exists between major and minor forms of PSD.

To account for these and other data also inconsistent with the Robinson *et al* model, Herrmann & Wallesch (1993) proposed a restricted version of the model which assumes that only the forms of major PSD observed in the acute post-stroke periods are very similar to endogenous depression, whereas those observed in more chronic stages must be considered as reactive forms, mainly due to psychosocial factors.

To test this new version of the Robinson *et al* model with a strategy similar to that used in our previous paper, we matched the symptomatological profiles of patients with major PSD observed at various time intervals from stroke with those of patients with endogenous depression. Since the profiles of patients with major PSD observed at various time intervals from stroke were very similar, and were very different from those in patients with endogenous depression, we concluded that even the restricted version of the Robinson *et al* model is inconsistent with our data.

We therefore think that no methodological defect exists in this or in our previous study and that our data allow us to conclude that no qualitative difference exists either between minor and major forms of PSD or between forms of major PSD that arise at various time intervals from stroke.

Let us pass now to our suggestion that psychological factors, rather than neurological factors, mostly account for PSD. This suggestion was mostly due to the distinction that we have more analytically described in our previous study (Gainotti *et al*, 1997) between motivated and unmotivated affective patterns. The term 'motivated reactions' refers not only to the reactive symptoms of anxiety, emotionalism and catastrophic reactions, whose prevalence in patients with PSD could be due (as Dr Ramasubbu suggests) to a bias in the selection of the control group, but also to a qualitative analysis of the responses given by patients in sections of the Post-Stroke Depression Rating Scale (PSDRS) devised to evaluate 'depressed mood', 'guilt feelings' and 'thoughts of death and/or suicide'. In these sections, patients were requested to qualify their response by saying whether their bad mood, guilt feelings and thoughts of death were related to their actual condition or were independent from it. Patients with major or minor forms of PSD usually attributed depression to the consequences of stroke, felt guilty because they considered their previous lifestyle as partly responsible for their actual disease, and had stroke-related death worries, whereas patients with endogenous depression attributed guilt feelings to their moral

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worthlessness and had active suicidal tendencies, rather than death worries. Furthermore, in both of our studies, diurnal mood variations were motivated (i.e. linked to situations stressing handicaps and disabilities) in patients with major PSD, but unmotivated (with a prevalence of depression in the early morning) in patients with endogenous depression.

In conclusion, even if we share with Dr Ramasubbu some doubts about the validity of dichotomous endogenous/reactive classifications, we would stress two points: (a) our criticism was addressed to an influential model based on such dichotomy: and (b) our tentative hypothesis that psychological factors play an important role in PSD seems, at least in part, justified.

Gainotti, G., Azzoni, A., Razzano, C., *et al* (1997) The Post-Stroke Depression Rating Scale: a test specifically devised to investigate affective disorders of stroke patients. *Journal of Clinical and Experimental Neuropsychology*, **19**, 340–356.

Herrmann, M. & Wallesch, C. W. (1993) Depressive changes in stroke patients. *Disability Rehabilitation*, **15**, 55–66.

Starkstein, S. E. & Robinson, R. G. (1989) Affective disorders and cerebral vascular disease. *British Journal of Psychiatry*, **154**, 170–182.

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Psychiatry and civil unrest in Northern Ireland

In his editorial, Daly (1999) is critical of the research that has been done on the psychological impact of the civil unrest in Northern Ireland, both in terms of its quality and quantity. He states that opportunities for valuable research have probably been missed, and those studies that have been carried out, he weighs in the balance and finds wanting. I consider his article a potentially misleading reflection on psychiatry in Northern Ireland over the past 30 years.

Much of the research he reviews emerges as speculative and inconclusive. He is critical of Lyons' (1971) concept of "normal anxiety". The work of Cairns & Wilson (1984) is, Daly believes, of limited usefulness as the populations studied were rural, whereas violence is largely an urban phenomenon. Curran (1988) is exposed as mistaken in his view that individuals habituate to trauma. These authors published their findings 29, 16 and 12 years ago, respectively. It is all too easy to find fault today.

Daly concludes that lack of trust in the authorities and a fear of breaches of confidentiality have resulted in treatment avoidance and exacerbation of symptoms. He gives no evidence for these conclusions. Psychiatrists in Northern Ireland have striven to avoid opportunism and prejudice, and to maintain impartiality. It would be a matter of concern if this was not the public perception.

Daly widens the concept of victim to include "terrorists incarcerated for paramilitary crimes". If offenders are to be viewed thus, there is a risk of widening the concept of victim to the point where it becomes meaningless. The research to which he refers in his next sentence (Lyons & Harbinson, 1986) related to one crime only, that of murder. Political murderers were found to be a more stable group than non-political murderers. That paper had no comment to make on the victim status of prisoners or on political crimes in general, contrary to the impression conveyed by Daly. His subsequent reference to a report in a local newspaper (*Belfast Telegraph*, 26 September 1998), in the context of psychological problems consequent on imprisonment, is speculative.

Finally, Daly has overlooked a crucial consideration in his editorial. It is no exaggeration to say that the political situation in Northern Ireland has made it difficult, if not at times hazardous, to carry out research on offenders and victims. On occasions where research has been done, it has not been feasible to publish it. Psychiatrists practising in Northern Ireland over the past 30 years have laboured under difficulties not experienced by colleagues elsewhere in the UK. Daly should not victimise them.

Cairns, E. & Wilson, R. (1984) The impact of political violence on mild psychiatric morbidity in Northern Ireland. *British Journal of Psychiatry*, **145**, 331–635.

Curran, P. S. (1988) Psychiatric aspects of terrorist violence: Northern Ireland 1969–1987. *British Journal of Psychiatry*, **153**, 470–475.

Daly, O. E. (1999) Northern Ireland. The victims. *British Journal of Psychiatry*, **175**, 201–204.

Lyons, H. A. (1971) Psychiatric sequelae of the Belfast riots. *British Journal of Psychiatry*, **118**, 265–273.

___ & Harbinson, H. J. (1986) A comparison of political and non-political murderers in Northern Ireland, 1974–1984. *Medicine, Science and the Law*, **26**, 193–197.

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Author's reply: Dr Harbinson criticises my recent editorial in a rather defensive manner but does not provide any evidence to refute my opinion that opportunities for valuable research have been missed over the past 30 years or so.

He/she seems unhappy with my review of some of the research carried out, commenting that "it is all too easy to find fault today". In my editorial I commented that "at the time Lyons (1971) was carrying out his research the field of traumatology was in its infancy" and, in relation to Curran (1988), that "more recent research in the field suggests" a different view to that of Curran regarding habituation to trauma. The whole purpose of a literature review is to examine previous research critically in the light of further developments.

In the Social Services Inspectorate document referred to in my paper (Department of Health and Social Services, 1998), the issue of confidentiality was addressed; for example, "Another G.P. noted that 'the individuals that are most affected in our area are of a predominantly nationalist viewpoint. There is a fundamental distrust of Government agencies [and] distrust and fear of leakage of sensitive information' ". Information received from the project leader in the Social Services Inspectorate has confirmed a minority, but consistent, viewpoint, mainly from those of a nationalist background, that the authorities, including those working in health and social services, are not to be trusted (J. Park, personal communication, 1999). As Dr Harbinson writes, this indeed should be a matter of concern.

Dr Harbinson is critical of me for commenting that "some people would consider terrorists incarcerated for paramilitary crimes to be victims". It has been reported that a number of people who subsequently become involved in terrorist crime have themselves previously been victimised (Smyth, 1998). A study looking at 80 perpetrators of homicide found that 52% met criteria for current post-traumatic stress disorder (Pollock, 1999). It would seem unethical to exclude anyone from being considered a victim, and therefore a potential candidate for treatment, on the basis of having been involved in criminal activity. Dr Harbinson has commented that on occasions it has not been feasible to publish research carried out. I find it difficult to understand why properly structured and anonymised research could not have been published. In order to ensure that psychiatrists maintain impartiality, it is important that such research should be published whatever the results, provided the findings are clinically relevant.

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