Pseudo-AIDS, AIDS Panic or AIDS Phobia?

SIR: Miller *et al* (*Journal*, May 1985, **146**, 550–551) reported on two men with excessive concern about AIDS: one had a diagnosis of anxiety neurosis, and the other had a diagnosis of depressive illness. The authors argued that this was a new psychiatric condition arising from the AIDS epidemic and coined the term 'pseudo-AIDS'.

Since the publication of this article, similar conditions have been described and called in turn 'AIDS-induced psychogenic state' (O'Brien & Hassanyeh, *Journal*, July 1985, **146**, 91); 'AIDS phobia' (Jacob *et al*, *Journal*, March 1987, **150**, 412); or 'AIDS panic' (Windgassen & Soni, *Journal*, July 1987, **151**, 126–127).

All of these definitions seem to place AIDS in a causative role in creating such conditions. We join O'Brien (*Journal*, July 1987, **151**, 127) in stating that what is important in patients presenting with excessive concern about AIDS, but without the disease, is not AIDS itself, but the underlying psychiatric state.

The common feature of the psychiatric illness, with AIDS concern as a symptom, is hypochondriasis. In hypochondriasis, the illness of which the sufferer has fearful apprehension is usually inherently frightening and often stigmatised by society, classical examples being syphilis and cancer. In addition, the sufferer is often someone who has already been brought into psychological contact with the disease, e.g. by a relative, friend, or even health workers. The fear of AIDS meets all of these criteria (Thompson & Riccio, 1987). Indeed, Miller (1987) has stated that such patients are similar to those with worries about other venereal diseases.

Fear of AIDS may also be a part of the content of any psychopathology, such as obsessional disorders (Goldmeier, 1987), affective disorders (Lippert, 1986), schizophrenia (Rapport, 1985), or monosymptomatic hypochondriacal delusional states (Lippert, 1986). In such cases of severe psychiatric disorders, the fact that AIDS is the content or object of the psychopathology is of little significance in the diagnosis or treatment of the disorder, which should be carried out in the usual way (Thompson & Riccio, 1987).

Finally, it can be argued that the term 'AIDS phobia' is misleading. The modern definition of phobia requires the presence not only of irrational fear of a harmless stimulus, but also avoidance of that stimulus. Thus, to be genuinely AIDS phobic, the patient would be required to irrationally avoid AIDS – almost a paradox, except when there is avoidance of AIDS even as a thought or a topic of conversation. The term 'AIDS phobia' is therefore a misnomer, since the patients to whom the term is most applied, i.e. the 'worried well', show little or no tendency to irrationally avoid AIDS.

M. RICCIO
C. THOMPSON

Charing Cross Hospital
Fulham Palace Road
London W6 8RF

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Neuroleptic Malignant Syndrome (NMS): A Misnomer?

SIR: Kellam's review (*Journal*, June 1987, **150**, 752–759) is apt. Perhaps it is time for the nomenclature of so-called NMS to be revised. Besides its possible occurrence before the advent of neuroleptics, it is now well reported that non-neuroleptics are also implicated, e.g. tetrabenazine (Burke *et al*, 1981), levo-dopa (Henderson & Wooten, 1981), and dothiepine (Grant, 1984). One case of NMS was attributed to metoclopramide combined with cimetidine (Destee *et al*, 1981), another was seen in an overdose of benzodiazepines, phenelzine, dothiepin, and alcohol (Ritchie, 1983), and yet another was attributed to amphetamines (Chayasirisobhan *et al*, 1983). The current restricted nomenclature, using the word neuroleptic, is not an accurate reflection of current facts and predisposes those not familiar with the literature to a sense of false security.

As most cases appear to occur in the context of treatment procedures, "Iatrogenic malignant syndrome" might be more appropriate, but we would guess that this would prove unpopular, particularly in the context of explaining the condition to patients, their relatives, or possibly their legal advisers.

ASHOK N. SINGH
JAMES MAGUIRE

St Brigid's Hospital
Ardee
Co. Louth, Ireland

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SIR: Kellam (*Journal*, June 1987, **150**, 752–759) has included Stauder's lethal catatonia on inadequate grounds, quoting Mann *et al* (1986), who consider NMS to be a neuroleptic-induced iatrogenic form of lethal catatonia. A similar opinion has also been expressed by Lindesay (1986), who perceived NMS as a hybrid of iatrogenic disorder and mis-diagnosed lethal catatonia. Lindesay suggested that lethal catatonia represents an idiopathic form of the disorder, whereas NMS may represent an iatrogenic form.

While not rejecting these opinions outright, it has to be kept in mind that the so-called lethal catatonia has only face validity as a nosological entity. There have been no well-planned prospective studies on its descriptive validity, construct validity, or predictive validity. Whatever information is available at the moment on lethal catatonia is a compilation of anecdotal reports such as the reviews by Mann *et al* (1986) and Kellam. Moreover, lethal catatonia is not mentioned in the current diagnostic and classification systems (DSM-III, ICD–9), and most psychiatrists no longer diagnose Stauder's lethal catatonia. Indeed, the term 'catatonia' is obsolete and confusing and should be eliminted from psychiatric terminology (*Lancet*, 1986), despite recent attempts to rehabilitate it (Barnes *et al*, 1986; Mann *et al*, 1986).

A superficial clinical resemblance between an iatrogenic syndrome and an entity of historical importance should not be grounds to contest the nosological status of NMS.

ADITYAN JEE

Department of Psychological Medicine
University of Malaya,
59100 Kuala Lumpur
Malaysia

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SIR: I agree with Singh & Maguire. Possibly if a new name for the syndrome is required it should be simply descriptive, as the aetiology remains obscure. I would suggest 'pyrexial catatonia'.

I am grateful to Adityan Jee for directing me to the invaluable article by Barnes *et al*. I would agree that 'catatonia', and especially its 'acute lethal' sub-type, only have face validity as clinical entities. I am not sure that any of the ICD–9 or DSM–III categories have much more. If we were to restrict ourselves to those categories whose validity had been demonstrated by well-planned prospective studies on their descriptive validity, construct validity, and predictive validity we should have few diagnostic entities left to use, either clinically or for further study.

My impression remains that a syndrome marked by rigidity and abnormalities of movement (catatonia) has been observed by several generations of psychiatrists, probably since they started to record their observations systematically. It was most often associated with other symptoms of what we now call schizophrenia, but has become rare since the advent of neuroleptic drugs. Occasionally it was associated with a fulminating course and death in hyperpyrexia. If this syndrome were to occur now it would usually do so in a patient already on neuroleptics, which would account for the Parkinsonian features now commonly seen and thus be indistinguishable from the neuroleptic malignant syndrome. I am therefore prepared to question whether neuroleptics or the other well-documented changes in dopaminergic drive, such as stopping L-dopa, are always to blame for the syndrome. I am currently enquiring of Barnes *et al* whether their idiopathic cases were pyrexial, in the hope that they may have recorded neuroleptic malignant syndrome in a drug-free patient.

The syndrome is probably stuck with its current name. My thesis is that we need to remind ourselves that we are not certain of either the implied aetiology or outcome. Vigorous treatment is to be encouraged in view of the reduction in mortality which can apparently be expected.

Recent work by Addonizio *et al* (1986) suggested the presence of a partial NMS in eight patients, as well as the full syndrome in two patients, out of a series of 82. All showed pyrexia and extra-pyramidal rigidity or tremor, and in the partial cases the symptoms remitted without the neuroleptics being