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Wirshing, 1989); perhaps particularly unless emphasis is laid on training and monitoring AIMS examiners. Pseudoparkinsonism and akathisia would be expected to be more common on haloperidol.

Further studies comparing the TD risk of atypical and conventional antipsychotic medications are needed.

Beasley, C. M., Deliva, M. A., Tamura, R. N., *et al* (1999) Randomised double-blind comparison of the incidence of tardive dyskinesia in patients with schizophrenia during long-term treatment with olanzapine or haloperidol. *British Journal of Psychiatry*, **174**, 23–30.

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Author's reply: We appreciate the opportunity to respond to several interesting points raised by Dr Woods in his letter regarding our recent article (Beasley *et al*, 1999).

Dr Woods suggests the possibility that the reported difference in rates of development of tardive dyskinesia (TD) may have been due to the insensitivity of the assay procedure (periodic administration of the Abnormal Involuntary Movement Scale (AIMS)) to "true TD". The letter further suggests that this may have been due to the lack of systematic rater training and/or lack of interrater reliability. It is true that raters were not trained systematically nor was interrater reliability evaluated. However, for these hypotheses to be tenable, the insensitivity would have been selective for the olanzapine treatment group. Low interrater reliability is generally believed to increase variability in a random fashion and thereby create a situation where differences are more difficult to detect owing to this source random error. Further, with the haloperidol treatment group, there was clearly substantial sensitivity with our assay procedure as discussed and addressed at length in our article.

As noted in the letter, the comparison of baseline prevalence of TD detected in our study and the estimates of point prevalence for TD in the population of patients suffering from schizophrenia and with demographic characteristics similar to those of our research sample affords a method to judge the validity of our assay procedure for TD. The percentage of patients identified as having TD at baseline in our study population (defined as meeting Schooler–Kane criteria or having a history of TD recorded on their charts) was 28.8%. Kane *et al* (1988) found a point of prevalence of 23.4% across three sites. Additional studies and reviews suggest a prevalence of about 20% among patients with schizophrenia (Kane *et al*, 1980; Kane & Smith, 1982). Our baseline prevalence and the point prevalence of TD cited in the literature are comparable, suggesting at least good reliability between investigators conducting our study and the raters generating the point prevalence data in the literature described above. If the point prevalence data cited in the literature are accepted as accurately describing the point prevalence of "true TD", the concordance also suggests the validity of the ratings of our investigators.

We have acknowledged that cases identified during the initial six-week examination period may have been identified because of confounding of withdrawal phenomena. The possibility also exists that AIMS ratings were confounded by akathisia and/or parkinsonism. Dr Woods suggests that an analysis excluding that initial study period would be useful. Although not displayed graphically, such an analysis was included in Tables 1 and 3 and indicates a clear difference between olanzapine treatment and haloperidol treatment. Also, although the data were not included in the article, Barnes Akathisia Scale (Barnes, 1989) and Simpson–Angus Scale (Simpson & Angus, 1970) scores correlated poorly with the AIMS scores in our study.

We believe that we cautioned the readers of our article not to accept the absolute rate of development of TD found in our study as necessarily valid. However, the relative differences between rates of development of TD during olanzapine and haloperidol treatments in these large samples of patients would appear well established and valid.

We agree that further studies of the incidence and rate of development of TD in patients treated with typical compared with atypical antipsychotics is warranted. It would be useful to standardise frequency of observation and optimise validity and relativity of assay methods.

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Atypical antipsychotics and neuroleptic malignant syndrome

Sir: We were interested to read the article by Barnes & McPhillips (1999), particularly the paragraph about neuroleptic malignant syndrome (NMS) in which it was suggested that the newer atypicals (sertindole, olanzapine and quetiapine) have not been associated with NMS.

We are sorry to note that this is not the case. Presumably since the above article went to press, there have been at least two case reports of NMS in association with olanzapine (Filice *et al*, 1998; Burkhard *et al*, 1999) in which olanzapine was the only neuroleptic used. With regard to quetiapine we are informed (Zeneca, personal communication, 1998) that their internal safety database has established possible and definite cases of NMS associated with quetiapine usage.

Although the atypicals have been rightly celebrated for their efficacy and side-effect profiles, any drug whose mechanism of action involves the blockade and down-regulation of central dopamine receptors should be regarded as a potential cause of NMS. Clinicians should maintain as great a vigilance with the atypical neuroleptics as with the older, traditional ones.