illness, because there may be varying periods of duration of untreated psychosis and this can have its own treatment implications. Despite these shortcomings, findings of the study suggest that even with a national healthcare system in place and the wider dissemination of treatment guidelines, there is still only a modest impact of these on real clinical practice. The possible effect of treatment guidelines is reflected by the fact that today patients receive fewer trials of other antipsychotics (2.8 *v.* 4 trials) before being started on clozapine compared with earlier studies.[2]

1 Howes OD, Vergunst F, Gee S, McGuire P, Kapur S, Taylor D. Adherence to treatment guidelines in clinical practice: study of antipsychotic treatment prior to clozapine initiation. *Br J Psychiatry* 2012; **201**: 481–5.

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doi: 10.1192/bjp.202.2.154b

Authors' reply: The first point raised is that the delay to clozapine initiation may not be a true reflection of the actual delay because patients may have been offered clozapine but refused it. This, of course, depends on what delay you are interested in. In our study we used the delay from the point at which treatment guidelines recommend a patient should start clozapine.[1] In our view this is the key, clinically relevant, delay. However, Sharma & Grover are right in suggesting that this delay does not necessarily mean that clinicians have delayed offering clozapine, although if this were the case it implies that it has taken on average 4 years for patients to agree to start clozapine. In practice it seems likely that there are a number of patient, clinician and service factors that may underlie the delay we observed in our study. Understanding these will be important if delays are to be reduced in the future. The availability of biomarkers for treatment resistance, as indicated by a recent study,[2] could also contribute to identifying treatment-resistant patients earlier. Sharma & Grover also rightly raise the issue that duration of untreated psychosis was not assessed in our study. Consequently, we cannot exclude the possibility that the duration of illness was in fact longer in our sample and thus that the delay to effective treatment was in fact longer.

Declaration of interest

O.D.H. has been on the speaker bureaux and/or received investigator-initiated charitable research funding from AstraZeneca, Bristol-Myers Squibb, Eli Lilly, and Jansenn-Cilag. D.T. has received consultancy fees, lecturing honoraria and/or research funding from AstraZeneca, Janssen-Cilag, Servier, Sanofi-aventis, Lundbeck, Bristol-Myers Squibb, Novartis, Eli Lilly and Wyeth.

1 Howes OD, Vergunst F, Gee S, McGuire P, Kapur S, Taylor D. Adherence to treatment guidelines in clinical practice: study of antipsychotic treatment prior to clozapine initiation. *Br J Psychiatry* 2012; **201**: 481–5.

2 Demjaha A, Murray RM, McGuire PK, Kapur S, Howes OD. Dopamine synthesis capacity in patients with treatment-resistant schizophrenia. *Am J Psychiatry* 2012; **169**: 1203–10.

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doi: 10.1192/bjp.202.2.155

Attention-deficit hyperactivity disorder across the lifespan

Michielsen *et al* conclude that the personality traits they call attention-deficit hyperactivity disorder (ADHD) 'do not fade or disappear in adulthood'.[1] Yet such a gradual extinction throughout life is precisely what their study proves.

The authors quote prevalences from previous studies as high as 7% in children and 4.4% in working-age adults. Their own study shows a prevalence in old age of 2.8%, with higher rates in the 60- to 70-year age group (4.0%) than in those over 70 (1.1%). In other words, there is a steady decline in the prevalence of ADHD caseness throughout life, way over and above that which could plausibly be caused by higher mortality among impulsive individuals.

These data show conclusively that, in common with many problematic personality styles, poor attention, impulsivity and hyperactivity tend to gradually lessen in intensity with age. Thus the study is further evidence that ADHD merely represents a cluster of personality traits which, given their high prevalence, cannot even be considered abnormal, rather than a disease entity.

Declaration of interest

The views expressed are those of the author and are not necessarily shared by his employer.

1 Michielsen M, Semeijn E, Comijs HC, van de Ven P, Beekman ATF, Deeg DJH, et al. Prevalence of attention-deficit hyperactivity disorder in older adults in The Netherlands. *Br J Psychiatry* 2012; **201**: 298–305.

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doi: 10.1192/bjp.202.2.155a

Michielsen *et al*,[1] while describing the background and aim of this study, mention that ADHD could lead to significant impairment in older age without providing evidence of such impairment. Certainly from clinical experience and previous studies we know that there are other mental disorders such as depressive illness, anxiety disorder and dementia which are relatively common in older age and likely to cause either similar or more severe impairment. The authors discuss this in some detail in their description of the limitations of this study but fail to consider this when drawing a conclusion about prevalence.

It is essential, according to DSM-IV criteria, for a diagnosis of ADHD to rule out any possibility of the symptoms being better accounted for by another mental disorder.[2] Unfortunately, the authors do not rule this out while studying the prevalence despite using a diagnostic instrument strongly based on the DSM-IV criteria.

Before we start diagnosing ADHD in older age groups it is important to exclude more prevalent and widely recognised mental health problems such as mild cognitive impairment and dementia. Looking at the diagnostic instrument DIVA 2.0, we can easily identify many symptoms which can be more readily explained by other more prevalent functional and organic illnesses.[3] This explains why the DIVA 2.0 (as the authors in this study rightly mention) has no evidence for its use in old age. Is retrospective data collected from an older person's recall of being inattentive or hyperactive as a child in different situations valid? More so when DSM-IV clearly advises caution for diagnosing this even in adults without any corroborating information, which was missing in this study.

We would thus suggest extreme caution before we start even suggesting the concept of ADHD in older adults and taking this