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Authors' reply: We welcome the interest in our study of suicide in patients with dementia in England and Wales. We found relatively lower risk of suicide during the first year of illness in dementia. Dr Haw writes that our findings are contrary to findings by Erlangsen *et al.*¹ However, such comparison is inaccurate. Erlangsen *et al* compared the risk of suicide in patients who were diagnosed with dementia during hospitalisation for physical or psychiatric illness with the risk of suicide in the general population. The authors point out that 'the findings cannot be generalised to persons with dementia who have not received the diagnosis while hospitalised'. The risk of suicide is known to be increased around the time of psychiatric hospitalisation.² Psychiatric in-patients would be expected to have more psychiatric disturbances. The study by Tsai *et al*,³ which Haw quotes to support the association with mild dementia, found that delusions were present in all seven of those who later died by suicide. Haw also seems to compare the literature on increased risk of attempted suicide in those with mild cognitive impairment with our study of completed suicide in patients with diagnosed dementia. One consideration during disclosure of the diagnosis of dementia is the potential for adverse reactions. Our findings suggest that unless the risk assessment, which should be done in any patient being given a diagnosis of a major physical or mental illness, identifies a specific suicide risk, the 'fear of suicide' should not be a major factor in the decision to not disclose the diagnosis of dementia.

We thank Salib who correctly points out that our Method omitted ICD-9 which was indeed the classification system in use by the Office of National Statistics in the earlier part of the study. The relevant ICD-9 codes were E950–E959 and E980–989 (excluding E988.8).

Our findings are based on National Confidential Inquiry data, so include individuals who died by suicide within 12 months of contact with specialist health services. When we examined general population deaths (suicide and undetermined verdicts) in older people during the period covered by this study, drowning was the third most common method of suicide overall after hanging and self-poisoning (National Confidential Inquiry into Suicide and Homicide, personal communication, 2009). This is consistent with Salib's findings. We agree that the method of suicide may be an important determinant of verdict and there are difficulties in establishing suicide as a cause in drowning. However, this does not affect our main findings, which are based on the conventional definitions of suicide used in previous research and national statistics.

Suicide prevention requires a variety of strategies.⁴ Although we agree that restricting access to drowning as a method of suicide may not be feasible, we do not agree that suicide prevention is futile in this group. Other strategies, for example the improved assessment and treatment of mental disorders, are likely to be worthwhile. We do not accept that younger individuals may be less amenable to prevention. However, different age groups may require a different preventive emphasis.⁵

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Adolescent-onset anorexia nervosa – missing half of the story?

As a psychiatrist working in an eating disorder service, I am always intrigued by the stability of the eating disorder diagnosis over time. As quoted by Treasure *et al*,¹ 'when long-term prognosis is considered, the overlap between anorexia nervosa and bulimia nervosa becomes more striking'. We all know that patients diagnosed with anorexia later switch over to other eating disorders and vice versa.² Hence, the paper about long-term outcome of anorexia nervosa³ attracted my attention. The methodology of recruiting is vital in a population-based study. Wentz *et al* have taken extreme steps to be as rigorous as possible. But I consider that they might have overlooked some of the aspects. The authors have described how the diagnosis of individual patients has changed over time from anorexia nervosa to bulimia nervosa to no eating disorder to eating disorder not otherwise specified (EDNOS) (their Fig.1). This highlights the diagnostic instability of these groups of illnesses. The authors have assessed individuals cross-sectionally and asserted them to have anorexia nervosa. The important information missing here is whether these individuals had symptoms of other eating disorders such as EDNOS before having symptoms of anorexia. Since the study is about long-term outcome of anorexia nervosa, Wentz *et al* should have taken adequate care to ascertain that the cohort they were following did in fact belong to the anorexia nervosa group. This drawback is further highlighted in the exclusion criteria of the study. Excluding patients at the initial stage (Study 1) of individuals with a history of eating disturbances could have excluded individuals who might have been suffering from a non-anorexic type of eating disorder. The authors assert that in the subsequent study they did not exclude patients who crossed over to other eating disorders (thereby promptly registering changes prospectively), but what they did by excluding certain people is to exclude these potential participants who could have shown crossing over from another type of eating disorder to anorexia nervosa.

There are other minor points that are worth mentioning. Comprehensive screening, that would have included patients of all severity, of individuals who were born in a particular year (1970), identified 24 cases of anorexia nervosa. Combining this with a less comprehensively assessed group of individuals (and thereby potentially picking up only very severe cases) could have resulted in heterogeneous populations being mixed. Instead of mixing these two cohorts with potential difference in their severity with a possible impact over their outcome including complications,⁴ the authors could have treated them as two