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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 nervosa[3] 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

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groups. I was also wondering about the validity of making a personality disorder diagnosis in such young individuals. Overall, if the diagnosis of anorexia could become bulimia, EDNOS or no eating disorder, the authors failed to consider the reverse being true (with the relative exception of bulimia to anorexia) at the important initial stage of this study.

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Authors' reply: Dr Sekar has concerns that the individuals in our study had other eating disorders before the onset of anorexia in adolescence. The aim of the original study, that took place in 1985, was to investigate the prevalence of adolescent-onset anorexia (and to examine background factors in this sample), not the prevalence of bulimia nervosa or eating disorder not otherwise specified. The mothers of the individuals, who were diagnosed with anorexia at the time of the original study, were interviewed thoroughly regarding premorbid eating disturbances.[1] Furthermore, the individuals themselves were interviewed regarding the same topic. No individual in the anorexia group (or the comparison group) had another eating disorder before the onset of anorexia. The school nurses at the schools in Göteborg continued to follow all pupils born in 1970 regarding weight and height until leaving school, usually after age 18 years. In the process, individuals with a later adolescent-onset of anorexia were also found. We believe that we have missed no cases of anorexia born in 1970 with anorexia onset before age 18 years. Since the original study focused on adolescent-onset anorexia we have not continued the screening of individuals born in 1970 after leaving school. Mean age of anorexia onset in our sample was 14.3 years. Bulimia typically presents during or after late adolescence and it is rare for onset to occur before the age of 14 years.[2,3]

The study has a prospective and not a cross-sectional design, i.e. we have examined all individuals at four occasions, but we have interviewed them both regarding current eating disorders (and other psychiatric disorders) as well as eating disorders during the follow-up period.[4,5] Data regarding eating disorders during the last follow-up period, between Study III and Study IV, are available from the first author.

Dr Sekar is also worried about the two subgroups being too diverse; the birth cohort with individuals born in 1970 was pooled together with a group of individuals with adolescent-onset anorexia born in adjacent years (in most cases 1971–1973). In the original study, the two groups were compared using several hundred background parameters and found to be similar in virtually all key respects.[1]

The use of personality disorder diagnoses with teenagers is arguable, but we considered (and still consider) it justifiable in cases persistently (over a period of several years) showing the essential characteristics of a personality disorder described in the DSM–III–R (the diagnostic manual used at the time of the original study). This is explicitly suggested by DSM–III–R guidelines. In the original study, apart from the age criterion, all DSM–III–R criteria had to be fulfilled for a diagnosis of personality disorder to be made. All individuals receiving a diagnosis of personality disorder showed significant impairment in social functioning and/or subjective distress.[1]

To conclude, since the aim of the original study was to investigate prevalence of adolescent-onset anorexia, we did not screen for other eating disorders. Nevertheless, from the time of entering our study, all participants (anorexia group and comparison group) were examined in great detail regarding eating disorders (past, present, and longitudinally at several follow-up occasions). We believe that we can safely say that there were no individuals who had crossed-over from another eating disorder to anorexia before the onset of anorexia in adolescence.

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Fallacies in standardised mortality ratios in anorexia nervosa

The article by Papadopoulos *et al*[1] adds to the evidence of high mortality rates in anorexia nervosa. An impressively large cohort was obtained through the Swedish Cause-of-Death Register which includes all Swedish persons who died since 1952. The crude mortality rate for 6009 females with at least one hospital admission for anorexia nervosa was 4.41% over a mean follow-up of 13.4 years (averaging 0.33% per annum). This rate compares favourably with other studies (0.5–2.2% per annum),[2] yet the authors, after much manipulation of their data, conclude that the mortality rate in Swedish women was 'astonishingly' high.

We contest this finding based on misleading calculations of standardised mortality ratios (SMRs). Standardised mortality ratios are a means of comparing mortality in a specified patient population with a standard population. The SMR value will exceed 1 in proportion to the risk of death from the disease under study.

The authors have two different usages of SMR. The first is the customary one when the calculation is applied to a cohort of persons who have been given a specific diagnosis at the outset. In Table 3 this SMR is given as 6.2 for the 6009 patients with anorexia nervosa, among whom there occurred 265 deaths whereas the expected deaths were 42.6. So far, so good.

Their second approach was to count the number of deaths according to each specific cause of death, yielding a different kind