Correspondence

EDITED BY TOM FAHY

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Long-term mortality after first psychiatric admission

Sir: Naik *et al* (1997) connect to the debate about the *Health of the Nation* targets, stating that their study gives a more correct view of the mortality risk of first-admission psychiatric patients than the prevailing one, namely that they constitute a high-risk group, regarding both all-cause mortality and suicide. However, their results are very difficult to generalise, since they have studied a very small sample over a long time, instead of studying a large sample in the established high-risk period, which is the first year after discharge.

In our own study of 1089 first-admission patients (Hansen *et al*, 1997), the all-cause mortality in the first year after discharge compared with the general population was 5.1 (95% CI 3.7–7.2) in men, and 3.1 (95% CI 1.9–5.0) in women. For suicide, the standardised mortality ratios were at the level of 30 times those in the general population. Naik *et al* studied 86 people for 16 years, finding 12 deaths and no suicides. This is certainly a low rate of death, but the problem is that a low number of deaths will have a very broad confidence interval, meaning that one cannot be sure that this death rate is statistically different from, for instance, the threefold greater rate that we found. After all, rather than studying the absolute death rate of the patients of a specific hospital, we wish to use population samples of discharged patients to tell us something about the probable death rate of psychiatric patients in general. To do this, a sample that is large enough to attain sufficient statistical power must be studied. If the sample is large (we observed 1998 persons for 11 462 person-years), the follow-up period does not have to be long, especially since the high-risk period is the first year. On these grounds, I think their criticism of other studies falls wide of the mark, and does not warrant the conclusion that first-admission psychiatric patients should not be considered a high-risk group.

Naik, P. C., Davies, S., Buckley, A. M., *et al* (1997) Long-term mortality after first psychiatric admission. *British Journal of Psychiatry*, **170**, 43–46.

Hansen, V., Arnesen, E. & Jacobsen, B. K. (1997) Total mortality in people admitted to a psychiatric hospital. *British Journal of Psychiatry*, **170**, 186–190.

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Sir: Naik *et al* (1997) report on 87 individuals followed up for 18 years after first admission for in-patient psychiatric care. They conclude that the lack of suicides in their series suggests that first-admission patients are not a high-risk group for suicide and thus should not be targeted. We do not agree that this conclusion can be drawn from their work. Such a conclusion, if quoted outwith the context of the paper, has significant potential implications for both service commissioners and service providers.

The authors list among the limitations of their study that their sample size is small but do not discuss this in further detail. We estimate that the statistical power of this study to demonstrate a difference in the suicide rate from the remainder of the population is around 15%. This is substantially lower than the 80% power which would usually be required in such work. The expected mortality from suicide and the confidence intervals for the observed: expected ratio for suicide are not given, but it seems likely that one death in their cohort could have changed the conclusions drawn from the work. The qualitative information in this paper is interesting. There was, however, very little chance that the authors would have been able to reject their null hypothesis.

Naik, P. C., Davies, S., Buckley, A. M., *et al* (1997) Long-term mortality after first psychiatric admission. *British Journal of Psychiatry*, **170**, 43–46.

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Authors' reply: Criticism of a small sample size can be overstated. Smaller samples allow for more complete, intensive and reliable follow-up than record-linkage studies do. Our experience with a UK register of admissions is that over 30% of linkage information can be inaccurate. The absence of suicide in our series would have been much less striking and informative if the follow-up was dependent on record-linkage only. There is often a trade-off between accuracy and sample size, but Dr Hansen *et al*'s (1997) large Norwegian database does appear to be of high quality.

Survival analyses are surprisingly powerful, making use of all longitudinal information. Although we followed only 87 patients, this covered 1262 person-years. Our 95% confidence interval for the standardised mortality ratio was 0.7–2.4, narrower than those cited by Dr Hansen for the much larger study.

Dr Hansen, and Dr Stark and colleagues, argue against the new hypothesis that first-admission cohorts are not at especially high risk for suicide in the long term. Our aim was to challenge the orthodox view as not proven. If other data confirmed our finding, then there would be "significant potential implications", and this is why we published, despite the tentative conclusions. The argument has two strands. The first relates to lack of evidence. Surprisingly, even in Hansen *et al*'s (1997) paper there are few data about suicide in a first-admission cohort. The analysis includes previously admitted patients, and also deaths before and immediately after discharge, which must bias the sample. It also appears that the excess deaths among first-admission patients were confined to those with a diagnosis of organic psychosis. We invite Dr Hansen to present data restricted to suicides in a first-admission series, using survival techniques to examine whether the acknowledged high risk in the period immediately after discharge does continue into the longer term.

The second argument is one of relative risk, comparing observed rates in first-admission patients with expected rates in other possible priority groups. As an example, Dr Hansen *et al*'s entire sample of in-patients has, in the short term, a 30-fold increased risk of suicide over the general population. This is of the same order as Gunnell & Frankel (1994) calculated for all deliberate self-harm patients, many of whom are not subject to routine follow-up in UK psychiatry. To put our results in further context, in the Borough of Broxtowe

https://doi.org/10.1192/S000712500025959X Published online by Cambridge University Press

the average suicide rate during the follow-up was around 8 per 100 000 per year, but the yearly rate varied between 2 and 16 per 100 000. In 16 years 140 people committed suicide but none of these was from our series. In a four-year follow-up of all 42 patients in Nottingham with three or more admissions in the year 1990, there were two cases of suicide, one homicide, and a further patient committed suicide shortly after the follow-up period ended (Davies & Payne, 1996). For a clinician, such a population of frequently admitted patients might be a better choice for the focusing of scarce resources. The case for blanket coverage of all first-admission patients remains without any evidence base.

Davies, S. & Payne, S. (1996) Patients repeatedly admitted to psychiatric wards: a four-year follow-up. *Psychiatric Bulletin*, **20**, 342–344.

Gunnel, D. & Frankel, S. (1994) Prevention of suicide: aspirations and evidence. *British Medical Journal*, **308**, 1227–1233.

Hansen, V., Arnesen, E. & Jacobsen, B. K. (1997) Total mortality in people admitted to a psychiatric hospital. *British Journal of Psychiatry*, **170**, 186–190.

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Transcending barriers between religion and psychiatry

Sir: George Carey (1997) suggests there is common ground to be shared by us, and it is my belief that here is an opportunity to collate knowledge and experience, and combine our resources. The hope would be to make progress in those 'Cinderella' disorders which at present tend to receive such curt treatment in our clinics.

There are pools of suffering individuals with 'broken' personalities, in places that are accessible to both disciplines. As a Christian psychiatrist, I have met with many people who have given their lives to Jesus as a result of such agonising situations that have resulted from their self-driven wills. Personalities that have quite simply given up the fight. Some of them have done so during times of reflection in prison. Others are addicts who have reached their turning-point at the 'rock-bottom' of their lives.

Alcoholics Anonymous and other 12-step fellowships have been uniquely successful in giving back the lives to millions of sufferers worldwide whose options would otherwise have been either more oblivion or suicide. Those in recovery have made a decision, through recognition of their powerlessness and folly of their self-will, that they needed to hand over their lives to God and begin to walk a different path.

We know that many personality-disordered patients are victims of abuse in childhood, with no experience of a loving father in their tender years, and consequently no adequate sense of moral values. This is where I have seen the healing ministry in its element, taking the souls of the lost and weary and giving hope with a new set of values and a new family – the Church – with all the essentials of unconditional love, forgiveness and nurturing that these vulnerable people so badly need.

Carey, G. (1997) Towards wholeness: transcending the barriers between religion and psychiatry. *British Journal of Psychiatry*, **170**, 396–397.

White, J. (1991) *Changing on the Inside*. London: Eagle.

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Gender and age at onset of schizophrenia

Sir: Jablensky & Cole (1997) have confused cause and consequence in suggesting that the gender differences in age at onset of schizophrenia are a result of marital status. The age at which symptoms begin and gender difference in onset are important clues to the underlying biological nature of the disorder (DeLisi, 1992). Jablensky & Cole suggest that marriage somehow delays onset of the illness, supporting this concept with data showing that married males and females do not differ in age at onset of schizophrenia. However, they fail to explain why this effect is more relevant for males than females, and why unmarried males still have an earlier age at onset than unmarried females.

Females marry at an earlier age than males, due not only to their level of brain development, and also to different reasons for mate choice. Females favour males who are more established, independent and can care for them, whereas mate choice in males has more to do with physical attraction than abilities. Thus, males, unlike females, must achieve a certain level of independence with academic and social success prior to marriage, whereas females may be dependent on parental care-taking until they marry and transfer this role to a spouse. A measure of ability to live independently is an indication of premorbid functioning prior to the onset of psychotic symptoms. One would then expect, given what is known about the development of schizophrenia, that males who have a vulnerability for schizophrenia would be less likely to marry than females, and if they did marry, they would belong to a select group of 'late-onset' males, whereas females are more likely to marry regardless of premorbid level of functioning.

Thus, the 'married' group of schizophrenics will contain both early- and late-onset females and only late-onset males, skewing the data toward no difference in age at onset between males and females. In Jablensky & Cole's analysis, single males have an earlier age at onset than single females – consistent with the notion that neither may have gained a degree of independence, but other factors affect age at onset. The authors fail to take the above into consideration and thus have not convincingly dispelled the notion that marital status is a consequence of age at onset of schizophrenia and that gender differences in age at onset suggest an underlying biological process, perhaps indicating a role for the sex chromosomes (Crow, 1995).

Crow, T. J. (1995) Aetiology of schizophrenia: an evolutionary view. *International Clinical Psychopharmacology*, **10** (suppl. 3), 49–56.

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Jablensky, A. & Cole, S. W. (1997) Is the earlier age at onset of schizophrenia in males a confounded finding? Results from a cross-cultural investigation. *British Journal of Psychiatry*, **170**, 234–240.

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War pensions

Sir: Significant mental health problems as a result of the Second World War are commonplace for European clinicians

https://doi.org/10.1192/S000712500025959X Published online by Cambridge University Press