

suicidal behaviour is raised by 'pro-suicide' internet sites, as we lack all but the most preliminary studies in this area. Those studies that have been completed, in line with earlier research on 'pro-anorexia' sites, reported that 'pro-self-injury' boards relay mixed messages – clearly providing social support, coping methods and understanding, but also tending to minimise the significance of self-harming behaviour.³ On the basis of current evidence, we might hypothesise that the use of such websites could equally be a protective factor or a risk factor.

The authors also mention internet addiction but seem unaware that the existing research is based on inconsistent criteria, is subject to widespread sample bias, relies almost entirely on correlative studies,⁴ and that the concept itself lacks conceptual validity.⁵ I challenge the authors to find any empirical studies to support their claim that in Asia 'cardiopulmonary-related deaths and even game-related murders in internet cafes are now regarded as serious public health issues'.

I wholeheartedly support the authors' contention that clinicians should consider the role of the internet in the lives of patients, but I would stress that this needs to be done with an understanding of the relevant research literature and a working knowledge of both the technology and culture of the medium.

We ask no less in other areas of clinical work and this is particularly important in a time when fears about the internet are amplified by the media with little regard to the evidence base.

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- 5 Bell V. Online information, extreme communities and internet therapy: Is the internet good for our mental health? *J Ment Health* 2007; **16**: 445–57.

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Authors' reply: We welcome Dr Bell's interest in our letter and would be happy to debate the issue – but find ourselves entirely in agreement with him. He makes some crucial points which we too would emphasise. In particular, we all share the 'contention that clinicians should consider the role of the internet in the lives of patients'. We too 'would stress that this needs to be done with an understanding of the relevant research literature and a working knowledge of both the technology and culture of the medium.' Sadly, there is too little sound evidence to inform our attitudes.

Bell argues rightly that internet use could 'equally be a protective factor' and indeed one of us (J.M.) has participated in research exploiting the potential for delivering therapy via the web.

Bell is right in suggesting that until we have a better understanding of the complex and subtle influences which may be disseminated by the medium of the internet – and indeed by other communication media too – we and our colleagues are likely to fall into the trap of caricaturing both risks and benefits of internet use.

We are certainly aware that the term 'internet addiction' is itself a caricature of a diagnosis rather than a well-explored entity,

but in the absence of empirical studies we are obliged to rely on anecdotal evidence. It has been as a result of some distressing clinical experiences, as well as concerns raised sensationally rather than scientifically in the media, that we have been moved to highlight the issue and to embark on our own preliminary studies.

Our letter does not aim to re-ignite a debate on whether the internet is helpful or harmful. As Dr Bell has observed, such a reductionist approach belies the complexity and variety of internet-based activities, any of which may have an influence in either direction.¹ We instead reflect that without empirical data to inform us, and where there is the possibility of either risk or benefit, careful and sensitive questioning of patients with high internet use may be a valuable component of a full psychiatric assessment.

The internet has taken a central place in modern culture particularly among younger people. Although we may not fully understand the complex interactions of the web and mental health, and while we await research to enlighten us, we are left with the choice to either ignore or engage with this phenomenon. Legislators, mental health advocates,² concerned parents and media journalists have all focused their efforts. It is time for scientists and clinicians to follow suit. In our view, this begins with the careful taking of an internet history.

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- 2 Papyrus: Prevention of Young Suicide. 2008 (<http://www.papyrus-uk.org/news.html>).

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Dementia: suicide by drowning

Purandare *et al's* article on suicide in dementia is a valuable contribution to suicide research in the elderly, particularly in those with dementia.¹ The authors have already dealt with a number of methodological limitations quite succinctly. One important limitation in particular is the choice of controls. As the authors rightly stated, a control group of patients with dementia who had not died by suicide would have been more appropriate.

In the Method section, the authors referred to ICD–10 only and not ICD–9. As far as I am aware from my own experience dealing with the Office for National Statistics (ONS), ICD–10 has been used by ONS only since 2001. Prior to this date and for the first 5 years of Purandare *et al's* study period (1996–2000), the ONS used ICD–9. If the authors applied the same criteria in their selection of suicide and open verdicts in cases reported between April 1996 and December 2000, then I assume they would have selected: ICD–9 E950–E959 for suicide and E980–989 excluding E988.8 for open verdicts respectively in a similar manner as they did with ICD–10 (p. 175). However, this very relevant fact does not appear to have been mentioned or explained by the authors, and was quite possibly omitted from the manuscript in error. However, this omission, which covers 5 years of a 9-year study, ought to be acknowledged and duly corrected.

I am grateful that the paper provides the opportunity to make one or two comments on some issues relating to drowning as a method of suicide in the elderly. Suicide by drowning accounted for 13.5% of total elderly suicide, being the third commonest cause of death in elderly suicide in England and Wales during 1979–2001 (16% for women as the second commonest cause of

death after overdose in elderly female suicide and 12% for men) and the fourth commonest cause of death by suicide in elderly males.² Interestingly, Purandare *et al*'s findings clearly show that drowning is the second commonest method of suicide by elderly psychiatric patients, with or without a dementia diagnosis.

The diagnosis of drowning itself may be difficult as there are no specific features at autopsy.³ During the investigation of a body found in water, a wide range of possibilities other than drowning have to be considered: accident, suicide, misadventure or homicide. Even when the diagnosis of drowning is confirmed (or excluded) the manner of death may remain undetermined.³ It is also difficult to determine whether underlying natural diseases play a role in the death, wrongly regarded as self-harm.⁴ There are some examples of this; 'sudden death in bathroom' has been reported,⁵ occurring mostly in winter and 80% of cases were elderly persons who died while bathing. Cardiac arrest and subsequent drowning in bathtubs were attributed to the sudden reduction in blood pressure and cardiac arrhythmia, and not due to fatal self-harm.⁵ Drowning has also been attributed to sudden unexpected death in epilepsy either subsequent to a seizure or occurring suddenly without explanation.⁶ It may be relevant that in a local study in Cheshire, UK, bathroom drowning accounted for 7% of all elderly drowning.⁷ Niacin deficiency, similar to pellagra, has been reported in high-income countries in people with alcohol dependency with poor eating habits and self-neglect, malabsorption, malignancy and nutritional deficiencies.⁸ It may be reasonable to assume that some elderly people with dementia, especially those who live alone, develop mental and physical changes due to an easily overlooked nicotinamide deficiency which could result in accidental drowning in an attempt to alleviate skin irritation as is the case in some pellagra sufferers.² It is interesting to note that Lunetta *et al*³ reported that 2.6% of bodies found in water were found to have died of natural causes after an initially suspected suicide. A review of trends in elderly suicide by drowning in England and Wales 1979–2001 revealed that suicide by drowning in the elderly attracted only 38% verdicts of suicide but 62% open verdict.² The high rate of open verdicts in death by drowning compared with any other method of fatal self-harm in England and Wales simply confirms the difficulties in reaching a firm conclusion in drowning death. Combining suicide and all undetermined deaths in suicide by drowning as a matter of course, especially in nationally collected statistics and consequently in research, may result in grossly exaggerated rates and misleading trends. Suicide by drowning is probably not amenable to prevention and although the elderly are often thought to benefit from suicide prevention more than younger adults, this is not likely to be the case regarding drowning, perhaps sadly more so in those with dementia.

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Purandare *et al*¹ used National Confidential Inquiry data to compare the characteristics of dementia patients who died by suicide with those of age-matched suicides with other diagnoses. They say empirical data on suicide in dementia are scarce and largely based upon case reports. Although the literature in this area contains a number of interesting, albeit highly atypical, case reports of patients with less common subtypes of dementia who died by suicide, there are also a substantial number of reports on suicide in older adults with various psychiatric diagnoses. The real problem with this literature is the quality of the studies; the majority are methodologically flawed, for example not employing a sensitive method for detecting mild cognitive impairment or absence of a control group, also use of coroner's records or death certificates to determine psychiatric diagnoses, sources known to underreport cases of dementia. The overall finding from this literature is that suicide appears to be uncommon in dementia, although the risk in Huntington's disease is in the region of threefold compared with the general population.² However, in a recent cohort study based on Danish case registers, it was found that for younger patients (50–69 years) diagnosed with dementia during psychiatric hospitalisation the risk of suicide was over eight times that of the age-matched general population, and the risk was threefold for patients aged over 70.³

Purandare *et al*¹ report suicide in dementia to be uncommon in the first year following diagnosis and highlight 1–5 years after first contact with services as the high-risk period. The case–control design of their study used a convenient but not very informative control group, namely age- and gender-matched suicides with other psychiatric disorders, and consequently the findings do not shed much new light on the association between suicide and dementia. No information is provided about severity of dementia or subtype (dementia subtype is of interest since fronto-temporal dementia would be expected to be associated with impulsive acts of suicide and self-harm, as frontal lobe impairment is associated with impulsiveness). The finding that suicide is less common soon after diagnosis is counter-intuitive, and contrary to the findings of Erlangsen *et al*³ where suicide was most common in the first 6 months after diagnosis. There is also evidence that when attempted suicide occurs in dementia it is more common in early, mild disease and when accompanied by depression.^{4,5}

- 1 Purandare N, Oude Voshaar RC, Rodway C, Bickley H, Burns A, Kapur N. Suicide in dementia: 9-year national clinical survey in England and Wales. *Br J Psychiatry* 2009; **194**: 175–80.
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