Depressive symptoms and cognitive decline

In their recent paper, Paterniti et al (2002) reported that depressive symptoms predict cognitive decline over a 4-year period. This is a well-designed and well-written study that replicates a previous finding from similarly well-designed studies. Negative findings on this question, however, are also common in the literature, including a report from the same French group a few years ago (Dufouil et al, 1996). I would like to point out some relevant issues overlooked by Paterniti et al.

First, I find it unfortunate that the paper cites few negative reports, with no mention in the discussion of the many longitudinal studies that have reported no association between depressive states and subsequent cognitive decline (Dufouil et al, 1996; Prince et al, 1996; Cervilla et al, 2000). It is particularly surprising that Paterniti et al quote the study by Chen et al (1999) as reporting that ‘depressive symptoms are predictive of cognitive decline’, when in fact they found that dementia predicted the onset of depressive symptoms but not the other way round.

Second, it is regrettable that Paterniti et al overlooked the only study to date addressing the very same question but for a considerably longer follow-up period (Cervilla et al, 2000). Longer follow-up periods could help to distinguish between psychopathology shared by depression and dementia (e.g. difficulties with memory and concentration, or apathy), as pointed out by a previous study by some co-authors of Paterniti’s paper (Dufouil et al, 1996). Indeed it could be argued that if depressive symptoms have a real capacity to predict cognitive decline, the latter should be expected to become more apparent as the study’s follow-up period lengthens. This, in fact, has not happened in our cohort (Prince et al, 1996; Cervilla et al, 2000) and I believe this adds potentially unique information to Paterniti et al’s discussion.

Finally, it is also regrettable that Paterniti’s group did not explore the repeatedly reported interaction with gender in considering whether depressive symptoms predict cognitive decline (Prince et al, 1996; Cervilla et al, 2000). Indeed, the latter studies have reported that if an association exists between depressive symptoms and cognitive decline, this seems to be the case in men only (Cervilla et al, 2000), or in men of above-median premorbid IQ (Prince et al, 1996).


J. Cervilla Sant Joan de Deu – 55M, Vilanova i la Geltrú, Barcelona, Spain

Preventing suicide

In his editorial, De Leo (2002) cites important papers of the past 8 years. He does not mention that over 100 years ago the great sociologist, Emile Durkheim (1897), stated that the suicide rate reflected patterns of social relationships within communities and that individual mental disorder had little bearing on this behaviour. His view has never been effectively refuted.

De Leo does, however, observe that ‘socio-economic events’ such as wars and economic fluctuations may ‘provoke effects’ that ‘would be incomparably bigger than any well-targeted anti-suicide initiative’. He recognised that in most Western countries, there is currently a ‘remarkable decline’ in youth suicide, which cannot be attributed to suicide prevention activities. Over the past 50 years, there have been synchronous, international trends in suicide (La Vecchia et al, 1994). All of these events are probably due to sociocultural influences rather than fluctuations in the prevalence of mental disorders, and substantiate Durkheim’s view.

De Leo states that suicidal behaviour attracts little interest among contemporary psychiatrists, as judged by the low number of contributions to suicidology journals. But this would seem to be the wrong yardstick. If Durkheim’s view is accepted, the most profitable approach to the prevention of suicide would be the creation of full employment and supportive environments, and the reduction of family breakdown.
and drug misuse. Such an approach would call for increased attention from sociologists, economists, clergy, educators and governments. In the defence of psychiatrists, in the psychiatric literature there is considerable interest in suicide prevention among people with mental illnesses.

De Leo sees promise for suicide prevention in antidepressants, functional neuroimaging and psychometric testing, but surely this would apply only in the clinical setting. It is important to reveal the alternative to identifying and intervening with people at high risk (which has been described as ineffective and even wasteful), that is, the public health approach, in which efforts are made to reduce the risk of suicide across the community (Rosenman, 1998).


**S. Pridmore** Department of Psychiatry, Royal Hobart Hospital, 48 Liverpool Street, Hobart, TAS, 7000, Australia

**Author’s reply:** Sociocultural factors are of great importance in suicide, and the deliberate manipulation of the sociocultural milieu (social engineering) would evoke a meaningful change in suicide mortality. However, this concept is theoretical and, like most approaches to suicide prevention among high-risk individuals, lacks rigorous scientific evidence. It is important to point out that while Emile Durkheim’s theories have never been effectively refuted, neither have they been supported by convincing empirical evidence.

My main contention is that the prevention of suicide, like other types of preventable death, requires a multifaceted approach that should incorporate interventions specific to high-risk individuals as well as public health approaches. As far as I am aware, this principle guides all existing national strategies, including the recently launched National Plan in England (September 2002). There is little doubt that strategies exclusively targeting high-risk subjects would produce only minimal reductions in mortality rates. Dr Pridmore maintains that counteracting unemployment and drug misuse, and improving community cohesiveness, would be profitable approaches to population-based suicide-prevention tactics. Once more, although shareable on the basis of common sense, convincing evidence for the effectiveness of these interventions is non-existent. For example, I recently reported in this journal on the impact of a telephone support service on suicide mortality among the elderly (De Leo et al., 2002). The supportive environment provided by that service had a significant impact only among female clients. Elderly men, who suffer from far higher rates of suicide than women, reported very little benefit. Similarly, full employment would surely positively affect suicide attempt rates, but maybe not suicide mortality.

The multi-disciplinary approach to suicide seems to me the conditio sine qua non under which prevention of this human tragedy can be effectively pursued. Given their professional exposure to suicidal individuals, psychiatrists are often in a privileged position to positively interfere with a suicidal process. To do it more consistently and on a larger scale, they should contribute more to suicide research, particularly within multi-disciplinary teams in collaboration with psychologists and sociologists, demographers and anthropologists. Complexity of causes requires complexity of remedies; there are no short cuts.


**D. De Leo** Griffith University, Australian Institute for Suicide Research and Prevention, Mt Gravatt Campus, 4111 Queensland, Australia

I read De Leo’s (2002) editorial on preventing suicide with interest. However, I would like to raise a few concerns. In spite of much development and understanding in both biological and psychological causes for suicide, the prevention of suicide remains an imperfect art. However, the comparison of suicide prevention with that of ischaemic heart disease seems inappropriate. The risk factors for ischaemic heart disease are well known, stable and quantifiable. Ideally, risk factors used for predictive purpose should be stable, whereas in suicide, clearly, most are not (Hawton, 1987). Therefore, when risk factors are not stable it will be difficult to apply the same analogy to suicide prevention.

The risk factors for suicide are different for community- and hospital-based populations. We have made progress in pharmacological interventions in hospital-based populations with lithium in bipolar disorders (Kallner et al., 2000) and clozapine in schizophrenia (Meltzer & Okayli, 1995), which have been shown to reduce suicide rates. However, the risk factors in community-based populations are different and a number of psychosocial risk factors have been reported to be significantly associated with the risk of suicide. We need to understand local perspectives and regional factors that influence suicide rates. There is a need for qualitative studies to examine these issues; the factors thus identified should then be explored in epidemiological studies.


**B. Ravi Shankar** Department of Psychiatry, Christian Medical College, Vellore 622002, India

**Author’s reply:** While the ability to prevent suicide is far less advanced than the prevention of heart disease, in my editorial the analogy highlighted the need for a multifaceted approach to anti-suicide strategies. I made the point that a single preventive measure would not be effective in reducing suicide mortality, as evidenced through the prevention of other types of death such as ischaemic heart disease. In the case of suicide, for example, the worldwide optimal treatment of depression would bring only a minimal reduction in suicide rates (further details available from the author upon request). None the less, fighting depression is generally perceived as the K constant of suicide prevention in existing national

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