

Authors' reply: Smith *et al* worry that we have underestimated the role of genetics in early-onset depression. They draw our attention to the published evidence for the importance of genetic factors in prepubertal depression, which itself is a marker for adult bipolar disorder, although not necessarily a marker for major depression (the topic of our editorial). From their own data, they report that three-quarters of 47 subjects who had a depressive episode by a mean age of 15 had a first-degree relative with an affective disorder. Strong evidence indeed of familiarity, but not necessarily of a genetic cause. Despite their certainty, many of us have problems with the precise nature of the evidence supporting genetic factors in major depression, in part because of the dimensional nature of depression, and in part because of the extensive comorbidity.

We opined that the heightened risk of depression in young people whose parents had depression was likely to be 'more psychological than genetic' and referred the reader to the review by Beardslee *et al* (1998). We provided evidence that intervention programmes for adolescents can reduce by half the probability of depression in the future. Smith *et al* argue that universal interventions are unlikely to be effective until we have complete understanding of how genetic and non-genetic factors interact to bring about the depressive phenotype, and that interventions targeted to high-risk groups should be deferred until we can reliably identify those individuals at high risk.

We strongly oppose this thinking. Most interventions in medicine are introduced before there is a complete understanding of the aetiology of the disorder, and usually before there is precise information as to which individuals will respond. Simply to know that an intervention can produce a reliable and significant benefit is sufficient to warrant implementation. We believe that this is the situation in regard to the prevention of major depression in young people.

Beardslee, W. R., Versage, E. M. & Gladstone, T. R. G. (1998) Children of affectively ill parents: a review of the past 10 years. *Journal of the American Academy of Child and Adolescent Psychiatry*, **37**, 1134–1141.

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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).

Cervilla, J., Prince, M., Joels, S., et al (2000) Does depression predict cognitive outcome 9 to 12 years later? Evidence from a prospective study of elderly hypertensives. *Psychological Medicine*, **30**, 1017–1023.

Chen, T., Ganguly, M., Mulsant, B., et al (1999) The temporal relationship between depressive symptoms and dementia. A community prospective study. *Archives of General Psychiatry*, **56**, 251–266.

Dufouil, C., Fuhrer, R., Dartigues, J. F., et al (1996) Longitudinal analysis of the association between depressive symptomatology and cognitive deterioration. *American Journal of Epidemiology*, **144**, 634–641.

Paterniti, S., Verdier-Taillefer, M. H., Dufouil, C., et al (2002) Depressive symptoms and cognitive decline in elderly people. Longitudinal study. *British Journal of Psychiatry*, **181**, 406–410.

Prince, M. J., Lewis, G., Bird, A., et al (1996) A longitudinal study of factors predicting change in cognitive test scores over time, in an older hypertensive population. *Psychological Medicine*, **26**, 555–568.

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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).

De Leo, D. (2002) Why are we not getting any closer to preventing suicide? *British Journal of Psychiatry*, **181**, 372–374.

Durkheim, E. (1897) *Suicide*. Republished (1952) as *Suicide: A Study in Sociology* (transl. J. A. Spaulding & E. Simpson; ed. G. Simpson). London: Routledge and Kegan Paul.

La Vecchia, C., Lucchini, F. & Levi, F. (1994) Worldwide trends in suicide mortality, 1955–1989. *Acta Psychiatrica Scandinavica*, **90**, 53–64.

Rosenman, S. (1998) Preventing suicide: what will work and what will not. *Medical Journal of Australia*, **169**, 100–102.

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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.

De Leo, D., Dello Buono, M. & Dwyer, J. (2002) Suicide among the elderly: the long-term impact of a telephone support and assessment intervention in northern Italy. *British Journal of Psychiatry*, **181**, 226–229.

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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.

De Leo, D. (2002) Why are we not getting any closer to preventing suicide? *British Journal of Psychiatry*, **181**, 372–374.

Hawton, K. (1987) Assessment of suicide risk. *British Journal of Psychiatry*, **150**, 145–153.

Kallner, G., Lindelius, R., Petterson, U., et al (2000) Mortality in 497 patients with affective disorders attending a lithium clinic or after having left it. *Pharmacopsychiatry*, **33**, 8–13

Meltzer, H. Y. & Okayli, G. O. (1995) Reduction of suicidality during clozapine treatment of neuroleptic-resistant schizophrenia: impact on risk–benefit assessment. *American Journal of Psychiatry*, **152**, 183–190.

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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