

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.

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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.

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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

strategies. This happens despite growing evidence substantiating a much reduced life-span risk for suicide in depression than that reported in earlier investigations (Bostwick & Pankratz, 2000). Given the complexity of its pathways, the prevention of suicide, like the prevention of many types of death, requires a combination of approaches, such as public and medical education, promoting community connectedness, controlling access to means, early identification and intervention, etc.

It is certainly true that risk factors for suicide are unstable and may change over time (De Leo, 2002), but probably more important is the (mostly unexplored) interaction between risk and protective factors. This is the really crucial issue in suicide prevention (by the way, protective conditions of course counteract also the risk of ischaemic heart disease: the Mediterranean diet and omega-3-fatty acids have already convincingly underlined the role of local differences in mortality rates). And this recalls another important point raised by Dr Ravi Shankar, which refers to the local (cultural/traditional) specificity of suicidal behaviour. In countries such as China, risk factors for suicide are not dissimilar from those of Western countries – what varies is their ranking in terms of importance and expressivity (Phillips *et al.*, 2002). Furthermore, it is well-known that within the same country there may be contiguous areas with largely differing suicide rates and that the same risk factors may operate differently in different social contexts.

To identify the exact components of a multifaceted prevention programme, tailored to local characteristics, greater knowledge of risk and protective factors is needed for both the psychiatric and general populations. Prevention of suicide is currently based on scant evidence. Therefore, I fully agree with Dr Ravi Shankar's view that more sound research is required. Prevention must be grounded in evidence if it is likely to have an effect on suicide mortality.

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I would like to comment on the editorial by De Leo (2002) which came to the conclusion that little is new in suicide prevention. Since nothing was mentioned about pharmacotherapeutic advances in suicide, I would like to take the opportunity to discuss recent information concerning the role of novel antipsychotics in the reduction of suicidality.

Suicide rates in schizophrenia are about 13 times greater than in the general population, and make a substantial contribution to the overall suicide statistics in the UK. Suicide rates in schizophrenia were unaffected by the advent of conventional neuroleptics. This was not because these drugs are ineffective, rather that they also come with adverse events that put patients at risk for suicide – most particularly akathisia and depression. However, there is now evidence that atypical antipsychotics – most particularly clozapine – may have antisuicidal potential. This was first hinted at by a mirror-image study by Meltzer & Okayli (1995), which suggested an 86% reduction in suicidality. Subsequently, a large epidemiological study (Walker *et al.*, 1997) including data on completed suicides showed that deaths from suicide in clozapine users occurred at a rate of 39 per 100 000 patient-years compared with 222 per 100 000 patient-years in former users of clozapine. Our own UK clozapine study (Munro *et al.*, 1999) confirmed this result. There are also suggestions from pivotal studies of olanzapine that suicidality is also reduced in users of this drug (Tran *et al.*, 1997).

All these observations have their limitations, which led Novartis, in collaboration with the US Food and Drug Administration (FDA), to embark on a randomised controlled trial of clozapine *v.* olanzapine in the reduction of suicidality in schizophrenia (the InterSePT study), the results of which have recently been reported (Meltzer *et al.*, 2003). Overall there was a 25% reduction in all key measures for suicidality in favour of clozapine. This has recently led the Psychopharmacology Advisory Committee to the FDA to recommend that this body approves suicidality in schizophrenia (not restricted to treatment resistance) as a new indication for clozapine. It is disappointing that the National Suicide Prevention Strategy for England and Wales has little to say about the role of new treatments in suicide prevention. However, in a recent modelling study of ours (Warner *et al.*, 2003), which also took into account drop-out rates

and treatment failure rates, we calculated that one-quarter of the target for suicide reduction in all patients in contact with mental health services could be achieved by the broader use of clozapine in treatment resistance. If clozapine were to be approved for suicidality, 50% of all patients with schizophrenia would be technically eligible. Again, calculating in drop-outs and failures an even more substantial proportion of the national target could be met. Much is made of the rates of thromboembolism and agranulocytosis with this drug. However, in comparison with overall reduction in all-cause mortality as well as the reduction in suicidality with treatment with clozapine, such caution is not supported by the epidemiological evidence for the overall advantage of this drug (Walker *et al.*, 1997).

Declaration of interest

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Fluoxetine in relapse prevention of PTSD

Martenyi *et al.* (2002) suggest that fluoxetine is effective and well-tolerated in the