Submission to Senate Inquiry into Suicide in Australia

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I am pleased to have the opportunity to make a submission to the Senate Inquiry into Suicide in Australia, and, in particular, to be able to comment on the adequacy of the current program of research into suicide and suicide prevention.

Documents associated with the National Suicide Prevention Strategy and state/territory suicide prevention strategies have consistently recognized that good quality research is necessary in order to build an evidence base in the area of suicide prevention. The question remains, however, as to whether Australia has fostered the right kind of research.

Various international commentators have argued recently that insufficient effort is being put into intervention research, and that this is limiting our knowledge of which suicide prevention strategies might be the most effective.[1 2] A widely-quoted systematic review by Mann and colleagues[3] could only point to two strategies for which the authors felt there was unequivocal evidence of effectiveness – restricting access to lethal means of suicide and educating physicians to detect, diagnose and manage depression. The authors did not reject all other strategies as ineffective, but could not find sufficient numbers of high calibre studies examining other approaches to make a judgement one way or the other.

My colleagues and I have conducted several pieces of work which suggest that Australia is not giving sufficient emphasis to intervention-based suicide prevention research. In 2005, we undertook a critical review of the 156 projects that were funded under the original National Suicide Prevention Strategy, using the individual project reports as our primary data source.[4] These projects involved a wide range of universal, selective and indicated interventions, and were aimed at various at-risk groups. The Strategy recognized that this created an ideal opportunity to examine the effectiveness of different approaches to suicide prevention, and organizations that received funding for these projects were contractually obligated to evaluate them. This was a laudable aim in theory, but in practice the evaluations were methodologically too weak to contribute much to the evidence base regarding what works and what doesn't work in suicide prevention. Most involved retrospective opinion-gathering exercises regarding the extent to which participants were satisfied with the given activity; very few involved any pre- and post- data collection on suicide-related outcomes, and still fewer involved any sort of comparison group. The small minority of evaluations that were conducted well were generally undertaken by external evaluators.

More recently, my colleagues and I conducted a similar exercise as part of our evaluation of the Queensland Government Suicide Prevention Strategy.[5] Again, we reviewed the projects

that were funded over the duration of the Strategy. This time, however, individual project reports were not available to us, and we were reliant on annual reports which contained summary information about the projects. This made it more difficult to determine the exact number and nature of projects, but it was clear that, like their national counterparts, these state-based projects covered the full spectrum of interventions and targeted a range of at-risk groups. Once again, the opportunity to contribute to the evidence base regarding the relative effectiveness of these different interventions was missed because of the paucity of evaluative information.

In addition to examining the strength of the evaluations that have been conducted alongside funded projects, my colleagues and I have also reviewed the scope of academic studies undertaken in the suicide prevention field in Australia.[6] Specifically, we reviewed 263 articles published in scientific journals between 1999 and 2006, and scrutinized 36 grants funded by the National Health and Medical Research Council and the Australian Research Council during the same period. We compared the profile of research identified in this way with stakeholders' views on where future priorities might lie, via a questionnaire administered to 11 groups comprising 231 individuals with an interest in suicide prevention. The journal articles most commonly reported on studies of descriptive epidemiology, despite the grants tending to fund intervention studies. Stakeholders indicated that emphasis should be given to intervention studies.

This recurring theme of a lack of emphasis on good quality evaluations of suicide prevention activities is not unique to Australia. We recently searched the books of abstracts from two of the largest regular international conferences held in consecutive years from 2003 to 2008 – the International Association for Suicide Prevention's Congresses and the European Symposia on Suicide and Suicidal Behaviour. We identified 1,209 abstracts in total and found that only 12% of these abstracts described intervention studies, compared with 48% that pertained to epidemiological studies. Even when we expanded the criteria to include evaluations of broader suicide prevention programs, only another 6% of abstracts were accounted for.[7]

It is worth considering why intervention research in the suicide prevention field in Australia (and overseas) is not stronger and more definitive. The problem of study design has beset many suicide prevention researchers who have tried to evaluate interventions. In research circles, the randomised controlled trial is generally regarded as the 'gold standard' for assessing the effectiveness of an intervention. Many potentially useful suicide prevention initiatives are not amenable to evaluation in this way, however. Universal interventions are particularly difficult to test via this design. For example, an intervention involving the erection of barriers to prevent jumping from a bridge is, by definition, introduced to the whole community. It is not possible to randomise half the community to receive the bridge intervention and half to act as a control group. At best, it might be possible to mount an evaluation that employs a quasi-experimental design where a community with a similar socio-demographic profile, a similar suicide rate and a similar bridge is used as a control. Typically, though, if such an intervention is evaluated at all, the evaluation employs a simple before-and-after design which considers suicide rates in the given community pre- and post-the erection of the bridge barrier, with no point of comparison.

This is compounded by the fact that suicide is an emotive area, and, as a consequence, intervention research in the suicide prevention field faces particular ethical challenges. For example, trials of both pharmacological and non-pharmacological therapies for depressed individuals often explicitly exclude suicidal individuals for ethical reasons, which means that their utility as selective interventions for suicide prevention remains untested. Conversely, there are situations where it is regarded as ethically improper to withhold potentially useful

interventions from suicidal individuals, which means that it is difficult to compare outcomes for those who are exposed to the intervention against outcomes for any sort of comparison group even when it might be practically possible.

In addition practical and ethical problems associated with designing rigorous evaluations of suicide prevention activities, the funding sources available for this kind of work have presented some issues. As alluded to above, funding for this kind of work comes from two sources: federal and state/territory health departments, which provide resources for internal or external evaluations of particular suicide prevention activities that they have funded; and academic granting bodies like the National Health and Medical Research Council and the Australian Research Council. Our work suggests that each has advantages and disadvantages. Contracts awarded by health departments provide for evaluations of a range of often large and complex initiatives, but the evaluations tend to be constrained (e.g., the intervention is often well under way by the time the evaluation is commissioned, making it difficult to gather baseline information). Grants received from academic granting bodies are investigator-driven and peer-reviewed, so they are typically very strong methodologically, but the funding is usually limited so the interventions they test tend to be fairly small in scale.

As Professor Bob Goldney and others have said in the past,[8] we need to get smarter about the way in which we evaluate suicide prevention interventions. We need to recognise that some interventions, by their very nature, will not be amenable to randomised controlled trials but that we must apply the most rigorous designs that we can. The program evaluation field has addressed this in other areas by developing methods for evaluating complex interventions. For example, they typically explicate the 'program logic' of given interventions which involves clarifying the causal pathway by which the program would be expected to work in order to test whether in fact it does work in that way. Similarly, they often use 'triangulation' or an approach where they use multiple methods and data sources to explore the same evaluation question, on the rationale that if these different pieces of the jigsaw start to form a coherent picture then conclusions can be drawn with greater certainty.

We also need to help funding bodies understand the particular problems faced in evaluating interventions designed to prevent suicide. In an ideal world, for example, key academic granting bodies and health departments might form a partnership whereby the former funded evaluations of programs rolled out by the latter. That way, large-scale, complex suicide prevention activities could be subject to the same rigorous evaluation as smaller, more discrete interventions.

The body of knowledge about what works and what doesn't work in suicide prevention is a work in progress. To date, there has been a strong emphasis on epidemiological studies. These have been crucial in helping us determine where to focus our efforts in suicide prevention, because they have highlighted particular characteristics that make people vulnerable to suicide. The time has come, however, to develop and test interventions that may reduce a particular risk factor (or strengthen a particular protective factor) identified in earlier epidemiological work. This next phase in the field of suicidology will be crucial if we are to combat the problem of suicide.

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