

## **Reviewer's report**

**Title:** Is it worth investing in mental health promotion and prevention of mental illness? A systematic review of the evidence from economic evaluations

**Version:** 1 **Date:** 23 June 2007

**Reviewer:** Filip Smit

### **Reviewer's report:**

#### Summary

This is a well written and engaging paper covering an important topic. After all, little is known about the cost-effectiveness of interventions aimed at mental health promotion and prevention of mental disorders. A systematic review was conducted to shed light on this issue. Fourteen high-quality studies were identified and reviewed. Outcomes were somewhat heterogeneous and there are difficulties in transferring results of cost-effectiveness studies across countries. Nevertheless, interventions directed at improving childhood development appear to be a promising area, but evidence for this proposition is best generated on a per-country basis.

#### Main points

I find the review well conducted (but see point 6 below), did not miss studies (I am only aware of one economic evaluation in this field that was missed by the authors – but this study, by dr. Denis Stant, has not yet been published), and I have no quarrels with the conclusions drawn from this review. This is a job well done.

#### Minor points

1. Perhaps avoid to use of a question in the title? – more appropriate for editorials.
2. Background, first sentence: perhaps better to cite the WHO study on the burden of disease? This would make for a strong case.
3. Background, second sentence: better not to make a mere assumption, but instead to provide direct evidence that preventing the onset of mental disorders is a viable option. I would suggest to refer the readers to the meta-analysis of Cuijpers P, Van Straten A, and Smit F. which appeared in the Journal of Nervous and Mental Disease 2005, 193: 119-125.
4. Background, third section: why introduce the methodological intricacies of conducting health economic evaluations in community settings here? It makes the introduction long, and is more suitable for the Discussion. Personally, I would remove this section from the MS altogether. It hardly serves any purpose.
5. Background, fourth section, first sentence “1 : 4 individuals can expect...”. Appropriate references for this statement can be the population-based

psychiatric surveys by Kessler et al (in the US) and Bijl et al. (in the NL).

6. Methods, searching the literature: why was the ERIC database not used? This seems to be an omission, especially given the authors' apparent interest in health promotion in young people.

7. Methods, the use of PPPs to both convert currencies and to take into account the buying power of the respective currencies is a good choice. Please, state the reference year to which the conversions were made. If the authors did not convert to US\$ for single and common reference year, then they may want to consider this as an option to improve the MS.

8. Figure 1 could be made more informative by stating the reasons on what grounds the 15 studies were excluded

9. The CEA by myself and colleagues (Brit J Psychiatry, 2006, 188: 330-336) indicated that there is a 70% probability of the minimal contact therapy to be more acceptable from a cost-effectiveness point of view than care-as-usual at WTP equal to zero (€0.00). When the WTP ceiling is raised to €20.000, then this likelihood increases to 80%. This is true when the societal perspective is taken and all costs are included in the CEA. When the focus is limited to the direct medical costs, then the acceptability of the intervention is 46% at WTP €0.00 and this becomes 70% under a WTP ceiling of €20.000. We have not been understood correctly by the authors, and our figures are misquoted.

10. The study by Lynch et al in the Archives Gen Psychiatry 2005, 26: 1241-8 tries to make a case for CBT in high-risk teenagers by stating that a WTP ceiling of US\$ 50,000 would be acceptable. This is unlikely to be the case for preventive interventions in depressive disorder, and a ceiling of US\$ 20,000 is probably more appropriate. Therefore, I find the Lynch article not very convincing, but that's a personal judgement.

11. Discussion, while it may be true that there is no single risk factor that explains more than 15% of the onset of mental disorder, it is well-known that the pathogenesis of mental disorders is not uni-causal, but multi-causal, and combinations of risk factors do a much better job in explaining onset. Therefore, preventive interventions should not be directed at a single factor, but at joint exposures to several risk factors, as was shown in several studies in identifying target groups for prevention (Smit et al. in the Archives Gen Psychiatry 2006, 63: 290-6; Schoevers et al in the Am J Psychiatry 2006; 163, 1611-21; Smit et al in the Brit J Psychiatry 2007, 190: 428-34). These 'enriched' samples of ultra-high risk populations can also be better used to demonstrate effectiveness of preventive interventions, because there is no longer a need to employ astronomically large sample sizes. Along these lines several successful randomised trials have been conducted in preventing schizophrenia – very much a disorder characterised by a low incidence.

12. Discussion, conclusions: the authors recommend that more economic evaluations be conducted retrospectively while using existing RCT data. They may refer to exactly that sort of work carried out by Eva Kaltenthaler and colleagues at the SchARR Technology Assessment Group of the University of Sheffield, UK; and the work of Gavin Andrews and colleagues at the University of

New South Wales, AUS.

13. I find the overly polite bow before the so called “holistic approach” unnecessary. It is well understood that economic evaluations do not inform public health planners about a plethora of other aspects relevant to their decision making, but then the first responsibility of health economics is to strengthen the evidence-base of health interventions with regard to their respective costs and outcomes. The review could perhaps better end on a note that much work remains to be done in this field, and I rather see this work be done with the tools we have, then making a perhaps premature move to methodologies that are even more complex to the point of becoming unmanageable.

#### Conclusion

The study by dr. Ingrid Zechmeister and her colleagues has intrinsic public health relevance, because it addresses an important issue: the formidable burden imposed on society by mental disorders, and hence the need that public health planners are informed by economic evaluations about the cost-effectiveness of interventions aimed at mental health promotion and prevention of mental disorders. The studies that are reviewed by dr. Zechmeister suggest that cost-effective prevention is possible, but the evidence for it is scant and cannot be easily transferred from one country to another. Therefore, we are in obvious need to strengthen the pertinent evidence-base, which is perhaps the most salient and important conclusion that can be drawn from this review. The publication of this manuscript can be recommended; it needs only a minor edit.

**What next?:** Accept after minor essential revisions

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

There are no conflicts of interest.