Reviewer's report

Title: Psychosis among "healthy" siblings of schizophrenia patients

Version: 2 Date: 14 September 2005

Reviewer: Matti Isohanni

Reviewer's report:

The basic question and aims of this paper are scientifically partly new, important and relevant. The study sample and methods are valid.

Some major compulsory revisions are needed:

1. The research process and, especially sampling and formation of the complex study population are poorly described. This is especially important in cases lost to follow-up; interpretation of the results depends strongly on their influence. The flow chart in Figure 1 is unclear (eg. different time points or cases lost to follow-up are not clearly mentioned; families were selectively contacted) and should be reconsidered and rewritten. This uncertainty makes the assessment of the results and their quality difficult.
2. What were 14 siblings “healthy” (or not being) in registers but psychotic (or psychotic symptoms) before 1991; this interesting group should be described in more detail. 11 were non-functional psychosis? Why were they not in registers? Was their sociodemography or clinical profile different (mild or comorbid psychosis?) than 16 who developed psychosis later?
3. The research environment (Finland with its unique registers) is not similar as the environment even in Nordic and Western countries. The limitations of the generalisation of the results should be discussed.
4. The authors should reread the references 13-14, 16 and also Moilanen et al 2003 Soc Psych Psych Epid 38:305-310; their conclusion is that register diagnoses in Finland are not very reliable; they may be specific but not very sensitive.

Minor essential revisions

5. The use of the term “schizophrenia spectrum” is inconsistent and not operationalised eg in ICD 10 or DSM. However, it is usually not applied to psychoses NOS as is done in the results, but to cluster A personality disorders and psychotic symptoms and disorders sharing clinical features with schizophrenia but not meeting the criteria of full diagnosis.

Discretionary revisions:

6. The reliability of the evaluators was not assessed. This may explain the surprising finding that schizo-affective disorders were more prevalent than schizophrenia. Or is the reason relatively high age of siblings? This is worth of a short comment.
7. Reference to textbook (ref 1) is questionable; probably some new and exhaustive review or original paper is a better alternative.
8. In families with schizophrenia relatives have an increased risk, in addition to schizophrenia, aso to other mental disorders, as was also in this study. I wonder that only ref 24 is given here, not also some recent article from another Finnish study, managed by Prof. Tienari.
9. If some psychotic cases are lost due to insufficient diagnostics and powerless registers, how does this bias the results? The results are less conservative? This aspect is worth of a comment.
This is an article of importance in its field, especially to those with closely related research interests. I am not able to assess the linguistic quality. No statistic re-evaluations are needed. Due to the limited quality of reporting, evaluation of this paper is not easy. My guess is that it should be accepted after minor revisions (content) but it needs major stylistic changes and clearer presentation. I am unable to decide acceptance until the authors have performed revisions.

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

**Declaration of competing interests:**

I have some scientific collaboration (necessary in a small country like Finland) with Drs Suvisaari and Lönnqvist (using the same laboratories to do genetic analyses, writing textbooks etc.). In my mind this does not prevent reviewing their drafts. I have no economic competing interests.