Reviewer’s report

Title: Child and adolescent psychiatric patients and later criminality. A prospective study

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Reviewer: Ellen Kjelsberg

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General

There is some gold here but it is well hidden. A substantial amount of work is needed, in order to make the manuscript publishable.

Major Compulsory Revisions

1. In the Abstract the authors state that “This study investigated whether or not risk changed over the past 50 years for former CAP patients to be registered as criminals as adults”. I am not convinced that the study design chosen can answer this research question in a satisfactory manner. I find it hard to accept that the study population from the Child Guidance Clinics in Stockholm recruited during the years in 1953-1955 can be directly compared with the 1975-1990 Jämtland cohort. Firstly, the Stockholm cohort is not properly described. What was the age at CAP treatment in that population? From what I can understand (p. 9) there must have been substantial differences in age at admission to CAP and the psychiatric disorders present in the two cohorts. Systematic information on these issues must be provided. In addition, the Stockholm cohort consisted of out-patients only, while the Jämtland cohort included both in- and out-patients. One population was urban, the other rural. And what about admission rules for entry into care? I imagine the treatment regimes were substantially different. What about length of therapy/hospital admissions?

2. If the authors want to address the issue of changes in criminal activity of CAP patients over time, I suggest that they focus on exploring possible secular trends in their own Jämtland population over the years they have investigated: 1975-1990. They might find it useful to read my research report: Kjelsberg E. “Conduct disordered adolescents hospitalised 1963-1990. Secular trends in criminal activity” published in European Child & Adolescent Psychiatry recently (2005; 14: 191-199). In this article I have explored a very similar material as to changes over time in criminal activity. Their own material spans 15 years and a comparison between cohorts may be warranted, due to changes in the use of illegal drug and increasing adolescent delinquency over the years in question.

3. A comparison with the Stockholm study should be confined to the Discussion section, with a short discussion of possible explanations for the differences found, taking into account reasons why the two study populations might not be directly comparable. The Stockholm cohort should also be cited in Table 7.

4. One serious shortcoming of the paper is that it does not discuss the vast changes in the illegal drugs scene that has taken place over the last several decades. The authors focus extensively on alcohol consumption and alcohol legislation but do not mention illegal drugs. Illegal drug use is more than alcohol use linked to criminal activity: it is illegal to possess, trade, and use, and drug users do often support their habit through illegal activities.

5. There seems to be some confusion regarding the classification and categorisation of crimes. There are inconsistencies, between the text and the tables, and between tables. Particularly, the categories used in Table 2 seem odd. Some of the five categories used (Pilfering, Severe property damage, Vandalism, Violence and Narcotics) seem to be overlapping (Serious property damage and Vandalism) while others seem to be missing (Crimes against property). (I can not believe that Crimes against property is meant to be covered by the category Severe property damage?). Usually, Crimes against the Penal Code lists burglary, larceny, aggravated larceny and motor vehicle theft among Crimes against property” while Misdemeanours against the Criminal Code, on the other hand, usually include Petty larceny (what the authors call pilfering?). Other misdemeanours include Inflicting damage to property and Misdemeanour against public order or peace. These terms and definitions may vary between countries, due to differences in legislation, and it is important that the authors clarify if they are used differently in Sweden than in other Western countries.
6. As already mentioned, I do not believe that the result from the Värmland should be directly compared to the Stockholm study. But even if such a comparison was valid, I do not believe that the authors can conclude that the differences observed were due to the four reasons listed in the Conclusion of the Abstract. The study design can, at best, demonstrate that changes in criminal behaviour have taken place. I can not conclude as to causal factors. The authors may (and should) discuss these issues but they must be careful not to conclude. And again, illegal drug use has not been listed as a possible contributing factor.

7. The diagnostic procedures have to be described in detail. If a plain translation into the DSM-10 system of the clinical diagnoses given at the time of CAP treatment was all they did, this have to be clearly stated, and the inherent weaknesses discussed. Gender specific prevalences of the different disorders should be provided, preferably in a table. The authors state in their research hypothesis (p. 4) that they wanted to investigate CAP patients who “were identified by CAP units as having behavioural and school problems and dysfunctional families” at the two points in time. Does this imply that they selected a sub-population for these analyses? If so, how was this carried through?

8. To summarize: My main advice is to revisit the data and concentrate on a proper description of all the interesting findings in the Jämtland data. These data deserve publication, in their own right. Descriptive data should be followed by multivariate analyses eliciting gender-specific predictors for criminal development, as they have already started to do. Comparison with the Stockholm study results should be left to the Discussion.

Minor Essential Revisions

1. Firstly, the paper is in dire need of tightening up. The Background and the Discussion need to be much more focused, and references to international research literature should be incorporated whenever appropriate. Moreover, the discussion should limit itself to discuss issues that are addressed by the current research!

2. I find the title of the article somewhat misleading. The authors have not conducted a proper prospective investigation. They have, at the time of the follow-up, re-visited old hospital records. Hence, the study could, at best, be called quasi-prospective.

3. The Abstract needs to be re-written, in order to reflect the contents of the article. Routine statistical procedures need not appear in the Methods section of the Abstract.

4. The Background starts with mentioning a number of Swedish research findings, with the references (1-14) lumped together at the end of the paragraph. The different research results cited need to be linked up to the articles where they originated.

5. The authors do often make statements without giving proper references, for example on p. 8: “… a comparison between CAP inpatients in Jämtland and Stockholm in 1981 …”. Proper references to the article/report/book must be provided.

6. I am not able to follow the argument put forth in the middle paragraph on p. 9.

7. The Table headings need improving.

8. The authors need to substantiate their claim that the rise in crime rates has been higher in CAP patients than in the general population. In the manuscript they only cite point prevalences.

Discretionary Revisions

1. Table 6: Why was not significance testing carried out on the demographical data?

2. Figure 1 states that during the years 1910-1950 there was “Alcoholism in men only”. This statement can not be taken seriously. Furthermore, the Figure seems to be out of context in this article.

3. The authors frequently cite p-values= 0.000. P-values are never zero. The term p< .001 etc. should be used. When the computer comes up with p= .000 it really means p< .0005.
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory 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:
I declare that I have no competing interests.