Reviewer’s report

Title: Social integration of patients in long-term maintenance treatment in northern Italy

Version: 1 Date: 16 December 2005

Reviewer: Antonia Domingo-Salvany

Reviewer’s report:

General

1. My main objection to the paper is related to the inferences made from a cross-sectional analysis. It is not possible to say (page 8, Discussion, second sentence) with the data available that “… their status and behaviour reflect outcomes of treatment rather than the period of drug use prior to treatment entry.”

2. Interpretation of results is difficult due to the lack of information on time “in treatment” of study subjects, they only provide length of drug use (prior to treatment?). The authors made sure that subjects had been at least 6 months in treatment, but the length of time in treatment can be very different among them. Information on this variable is important, and if available some analysis with other study variables should be done. If not possible, need to state in the discussion what influence it could have.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The sentence in the abstract’s conclusions “In Europe, around 70% .... healthcare programs.”, although interesting, doesn’t come from the study data.

The study population doesn’t constitute a representative cohort of drug treatment patients in the Veneto Region, as suggested in the objectives (page 3, aim).

It is not clear to me what time frame was taken into account when classifying subjects as unemployed, and neither whether all conditions had to be satisfied or only one. This is an important issue as it’s one of the main variables to which the authors assign a value for the main outcome “social integration”. There are also other issues linked to it: what are the criteria for unemployment used in other data bases utilized for comparison?. Is the unemployment rate of subjects treated in PHCDUs of the Veneto Region similar to that of Italy in general?.

Similarly, other variables changing over time need to be better specified in the methods section. “Questions regarding current behaviour referred to six months prior to the interview.” is not clear enough.

Although under ‘statistical analysis’ the authors say they analysed factors related to length of drug use in a multiple linear regression model, no results are given. I have doubts about the relevance of this analysis for the study objectives, however, this statement creates the expectation of finding the corresponding results.

Percentage of “living with parents”, was fairly similar to the general population but quite different
when stratifying by age. This would need more emphasis in the discussion.

Figure 4, much lower proportion of study subjects married. Not stated.

Page 8. The opinion of treatment centre personnel is very valuable but not to endorse the presence of an effect of maintenance treatment on patients’ employment status. Furthermore, lower employment rates in the past may be related to many different factors not controlled for in the study, including changes in the treatment provision.

Page 9, line 21: The sentence “The reduction in prevalence is due partly….” doesn’t give any support to the study objective. To my understanding the only relation would be the possible change in the proportion of intravenous users.

Page 9 and 10. The lower prevalence of HCV in patients under 25 may be related to their probably shorter length of drug use. Before being optimistic regarding a reduction of prevalence in the future it is necessary to control for this fact in the study data.

There is an aspect not analysed in the paper that may be relevant for the study objectives (aim ii)): the relation of serological status with integration.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The aim of the study is not the same in the abstract as in the main paper. The term “social integration” is a bit too ambitious for the information analysed in the study. This is also a consideration to take into account in the title of the manuscript.

Most references about effectiveness of maintenance treatment are quite old.

Page 6:
When calculating the percentages of final participants, those not eligible for the study, because they didn’t fulfil study criteria, shouldn’t be taken into account. The participation rate is then much higher (88.8 instead of 62.02 –not 65.6, as stated-).

What is it meant by “Primary route”? Is it the first route of substance administration or was it the principal route currently? Even though probably is not currently but before starting treatment. Need to state.

Page 9. misleading to say that 30% were married, as it was 30% of those with a stable relationship, but overall it was less than 15%.

Table 1: The way %ages are indicated is quite clumsy. There is a typing error with 551/552.

Table 2: serology instead of sierology.

Figure 2: it seems to me that the legend is not adequate (unemployment instead of seeking first job?).

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Discretionary Revisions (which the author can choose to ignore)

Page 4. “… of at least fifteen…”. It took me some time to understand that authors were referring to
Page 6. p values are given using different notations. It would be better to unify the way of specifying.

Page 7. Too many numbers when giving the percentages of missing data for analytical tests. Better if summarised.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

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

Statistical review: No

Declaration of competing interests: 
'I declare that I have no competing interests'