Peer Review Report

Review Report on Relapses in illicit drug use among probationers: Results in a risk group of Public Health Services in Bavaria
Original Article, Int J Public Health

Reviewer: Denis Yukhnenko
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EVALUATION

Q 1   Please summarize the main findings of the study.

The study analyzed urine samples from 380 individuals and found that relapse rates for drug use were high and primaarily occured within the first 3 years of probation supervision. The study suggests that more effective prevention strategies for substance abstinence should be developed for probationers at the beginning of the probation period.

Q 2   Please highlight the limitations and strengths.

The strength of the study is the employment of the urine screening for in a substantial cohort of probationers and a long follow-up period. The main limitation is the potentially innapropriate use of KM estimator for this type of competing risks data (clarified below).

Q 3   Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

The overall paper does provide some useful perspective on the management of drug use on probation. The study is unique and sufficiently powered. However, I think that the manuscript could benefit from addressing the following points.

Major points

1. In my opinion, the main analysis may not be entirely appropriate for the type of data and outcome used. If I understand correctly, the authors are addressing the competing risk problem, where one individual can be at risk of relapsing for several different drugs, and any first relapse means that they are out of the cohort. The authors used the KM estimator for this competing risk problem, which is not problematic per se. The KM estimator can be a simple and valid tool for competing risks: each event is analyzed separately while others are treated as censored (as done here, it seems). However, the proper use of the KM estimator requires that the competing risk events are independent of each other (for detailed analysis of this issue, please, refer to https://www.publichealth.columbia.edu/research/population-health-methods/competing-risk-analysis). Since drug use is highly comorbid, it is highly unlikely that the probabilities of using, say, cannabis and amphetamine, are independent of each other. Therefore, the probabilities obtained from the KM estimator can be misleading. To what extent is hard to say, likely not much, but still, I would suggest using a competing risk analysis method that does not rely on the assumption of independence between competing risk events. The Cumulative Incidence Function seems to be a good choice (as suggested in the article from Columbia referenced above). The CIF also allows for meaningful clinical interpretations. However, as I am not a statistician by training, I would advise double-checking with a statistical reviewer. Since this is the main analysis of the paper, its choice has to be clearly justified, and the method's main assumptions must be clearly stated.

2. My second point is related to the first one. Censoring was not clearly described in the methods. What events were used as censoring events besides the end of the available follow-up data? For example, death or other types of loss to follow-up. How many such incidents were there? On line 262, the authors mention that they use an open-study cohort. It is a bit confusing, as it was not mentioned in the methods. Did they actually use an open study cohort? Does it mean that they have left-censored data (individuals returning to the cohort)? If so, how was it dealt with, and what was its prevalence? Clarification is needed.

Minor points

1. As any identified drug use was considered a relapse, I would suggest providing a general survival plot indicating the overall survival of the whole cohort. It is directly relevant to the outcome of the probation programme (since it is linked to the termination of probation). Judging by the provided table alone, it is difficult for me to estimate how the probationers performed in general. I would suggest using this plot as the main figure.

2. The age variable is reported as mean age with CI. As age tends to be not normally distributed, I would suggest reporting median and interquartile range for age.

3. I believe that the methods section would benefit from providing additional information regarding the correctional context of urine sampling in Bavaria. Specifically, it would be helpful to explain which index offences are subjected to urine sampling and why the supervision period is so lengthy (i.e., 12+ years). This additional information would enhance the reader's understanding of the generalizability of the study's findings. If the word count allows, I suggest including this information.

PLEASE COMMENT

Q 4 **Is the title appropriate, concise, attractive?**

Yes. I would also suggest adding the region to the title (Bavaria, I assume), if it fits the Journal's style.

Q 5 **Are the keywords appropriate?**

Yes

Q 6 **Is the English language of sufficient quality?**

Yes

Q 7 **Is the quality of the figures and tables satisfactory?**

Yes.

Q 8 **Does the reference list cover the relevant literature adequately and in an unbiased manner?)**

Yes

QUALITY ASSESSMENT

Q 9 **Originality**

Q 10 **Rigor**

Q 11	Significance to the field	▮▮▮▮▯
Q 12	Interest to a general audience	▮▮▮▮▯
Q 13	Quality of the writing	▮▮▮▮▯
Q 14	Overall scientific quality of the study	▮▮▮▯▯

REVISION LEVEL

| Q 15 | Please make a recommendation based on your comments: |

Major revisions.