Reviewer's report

**Title:** Understanding drug-related mortality in released prisoners: A national review of coronial records

**Version:** 1 **Date:** 14 September 2011

**Reviewer:** Amy Jo Harzke

**Reviewer's report:**

There are few specific revisions that this reviewer would consider both “major” and “compulsory.” However, taken together, the several minor essential revisions listed below, suggest the need for a major revision of the paper. In general, the clarity, specificity, and coherence of the paper could be improved. More specifically, the Introduction should clearly explain the rationale for the study and set up the specific aims; the stated aims should be clearly reflected in the description of the analytic approach and, in turn, the description of the results; and the final discussion should reiterate the major findings in the context of the relevant literature.

**Major Compulsory Revisions**

Given previously published studies of mortality among ex-prisoners, it seems essential that the authors include something about the time period between release and death. As the manuscript currently reads, it seems possible that death could have occurred many years after release from prison. This raises some questions about the relevance of this study’s findings to previously published studies, many of which specify the time of death via release from prison (e.g., within 6-months, within 1 year after release). If the authors are unable with the present data to quantify the time period between release and death, this limitation and its implications should be addressed. If the authors are able to quantify the time period from release to death, this should be quantified and included in the paper in a meaningful way.

The number of ex-prisoner deaths seems rather small given the length of the study period (2000-2007). It seems possible that the nature of the data or the data search methods could have resulted in an underestimation the number of ex-prisoner deaths. These possibilities and their implications should be addressed, both in the Methods and as potential limitations of the study.

**Minor Essential Revisions**

In the Abstract and in the Introduction, the authors state that “relatively few studies have investigated why these individuals are at increased risk” (italics mine). The use of “why” is too strong; that is, as part of the rationale for this study, it seems to imply that the reported study will address reasons for or make causal inferences about mortality risk. It does not and cannot. The following revision, which mirrors language used in the rest of the paper, is suggested: “relatively few studies have investigated the circumstances surrounding the
deaths of released prisoners.”

The Results portion of the Abstract does not reflect the major analyses or findings of the study. Specifically, the authors report the proportion of ex-prisoners who died of accidental drug-related causes that were employed, homeless, or had a pre-existing physical or mental health condition. For all of these variables, ex-prisoners who died of drug-related causes were compared to those who died of other causes, and the comparisons showed no statistical or substantive difference between the two groups. Therefore, reporting these proportions in the Abstract is potentially misleading. Minimally, if the authors wish to report these proportions in the Abstract as main findings, the authors should mention that there was no statistical difference between the two groups with respect to these variables. Preferably, however, the authors should report the main findings from their study here in the Abstract, which were largely about the differences between ex-prisoners who died of accidental drug-related causes vs. those who died of other causes.

The Introduction needs some revision in order to provide a more explicit and specific rationale for the study. Are the authors suggesting that a main, potential contribution of the study is that previous studies have assessed the relationship between post-release mortality and in-prison characteristics or events (e.g., in-prison victimization, in-prison psychiatric hospital admission), while the present study examines the relationship between post-release mortality and post-release circumstances? Or, are the authors suggesting that the specific type of data source they are using allows for a complete description of the circumstances of death post-release, because previous studies have relied on the linkage of quantitative datasets rather than on data abstracted from text records? If the former, this point should be explicitly made. If the latter, then this point should be explicitly made, and the nature of the source data should be briefly described (e.g., “free-text” or narratives requiring abstraction and coding). Also, the authors state that previous studies have been limited to “basic demographic” and “criminological” but the literature review suggests that previous studies have considered factors well beyond the scope of demographic or criminological domains (e.g., taking medications that affect CNS, having a small primary support network). Additionally, the authors indicate that their study will assess “psychosocial” characteristics of ex-prisoner decedents, but this is a misnomer. The characteristics they refer to as “psychosocial” (e.g., employment status, marital status) would more appropriately come under the heading of “socioeconomic” characteristics. This should be changed throughout the narrative of the paper and in the heading of Table 1.

The stated aims of the study (pg. 4) do not reflect a major component of the analysis. That is, there is not an aim that reflects the comparison of ex-prisoners who died due to accidental drug-related causes vs. those who died due to other causes. This is not a trivial omission, as it makes it difficult for the reader to follow the different emphases in the Results section.

Although it is mentioned as a limitation in the Discussion, it seems it should be explicitly mentioned somewhere in the Methods section that the data that were extracted from NCIS profiles were not recorded in a uniform or standard manner.
The authors do not adequately describe the data abstraction form(s) or processes. The authors should describe the form more completely or consider adding the actual form as an appendix. In terms of processes, by whom were the abstracting and coding done? Was inter-rater agreement assessed? How were discrepancies between raters resolved?

Related to the above, in the Data Collection section, it seems that more information could be provided about the coding of some characteristics. What constituted a “mental health condition”? What constituted a “physical health condition”? Similarly, some characteristics or variables to be abstracted could be more named more specifically in the Data Collection section (e.g. “social circumstances” could be marital status, employment status, etc.).

In the Analysis section, the description of the analysis is limited to the comparison of ex-prisoners who died due to accidental drug-related causes vs. those who died due to other causes. The descriptive aims are not mentioned.

Also in the Analysis section, do the authors mean to say that t-tests were used for “normally distributed data” (pg.5) or for “continuous” data (as opposed to categorical data). Obviously, not all continuous data are normally distributed, but unless the authors checked the distribution of continuous variables before running t-tests, it seems more appropriate to say “continuous” data.

The level of significance for statistical comparisons should be stated in the Analysis section.

The title of Table 2 should be revised to include the drug use data that are reported therein.

In several places, the authors use “criminogenic” (e.g., top of pg. 5) when it appears they mean “criminologic” or “criminological.”

As for the Discussion, in general, it is brief and could be expanded and further contextualized in the relevant literature. An example of an area that could be expanded is the reference to the use of drugs in unusual settings. The authors cite two studies regarding accidental heroin overdose in “unusual settings” but do not cite any studies regarding the use of drugs in such settings. The authors could mention here that some evidences suggests the place of drug use (and potentially of overdose) may vary by drug type, gender, and other characteristics.

Also in the discussion, the authors state (pg. 7) that drug related deaths in their study were associated with unemployment. This was NOT a finding in their study. Ex-prisoners who died from accidental drug-related causes showed similar levels of employment to ex-prisoners who died due to other causes.

It would be helpful in all sections of the paper, and especially the Discussion section, to distinguish carefully between challenges that were faced by all ex-prisoner decedents vs. challenges that were faced more often or differentially by those who died due to accidental drug-related causes.

Discretionary Revisions

It seems that the subtitle of the paper should be “a review of national coronial records” rather than “a national review of coronial records;” the records
themselves are “national,” but the review is not.
The term “coronial” may seem unusual to some readers (perhaps only those who are not Australian), although it is less unwieldy than “cause-of-death” which is mostly commonly used in the United States and other contexts.
It is unclear (pg.7) why those in a home environment would be “beyond the reach of emergency services.” What types of emergency services could not be accessed from a residential setting?

**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, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.