Reviewer's report

Title: Interaction of Social and Behavioural Factors in Non-Suspicious Unexpected Death in Infancy; Experience from Metropolitan Police Project Indigo investigation

Version: 2  Date: 10 July 2015

Reviewer: Peter Sidebotham

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This is an important population dataset on sudden unexpected death in infancy which provides some useful descriptive data. However, it is important to recognise that these are descriptive data only and not a full epidemiological study, therefore any conclusions about associations are very weak. This needs to be properly acknowledged. It seems to me that the authors have missed an opportunity to present more helpful data from the dataset, to give an overall profile of both explained and unexplained sudden infant deaths in a large metropolitan area over a significant time period, and to compare the profiles of both groups. There are a number of limitations in the interpretation of the data. There are also a small number of editorial amendments needed.

Major Compulsory Revisions

Methods
1. What statistical measures did you use to compare your overall data with national data?

Results
2. You provide gross figures for the medically explained, unexplained and ‘unascertained’ deaths. Presumably the ‘unexplained’ were classified as SIDS deaths. Were there no cases of accidental asphyxia? (other UK data suggest that ~4% of SUDI in E&W are classified as W75, asphyxia, with higher rates in other countries; this suggests that there would have been at least 18 asphyxial deaths in this cohort). It would be helpful to know how many suspicious deaths were excluded. It would also be helpful to know the different categories of medically explained deaths. As it stands, these data are too crude and lacking in detail to be of much value.

3. Page 6, line 104: it would be more appropriate to include the 14 ‘unascertained’ deaths in your category of ‘unexplained’: ‘and 270 remained unexplained, of which 14 (3%) were classified as “unascertained”; a term usually used…’

4. I wonder whether there would be value in presenting the demographic and environmental data as a table with a comparison between the medically explained and the unexplained deaths? This would likely show that many of the risk factors are present also in the explained deaths, but that, most likely, they
are more prevalent in the unexplained deaths.

5. For figure 1, I wonder if it would be helpful to discriminate between the medically explained and the unexplained deaths – was there a seasonal variation in both?

6. I’m not convinced the inclusion of figure 2 is worthwhile, since the vast majority of mothers were classed as unemployed. Occupational classification is of very limited use in maternal data.

7. The behavioural variables included are not particularly meaningful as descriptive data. It would be helpful to explore the interaction between co-sleeping and other risks. What proportion of co-sleeping deaths were with adult smokers, or with adults who had taken alcohol or drugs?

8. The linkage by postcode to IMD rank is helpful and shows a significant gradient (presume this is based on chi square for trend? It would be helpful to include the actual figure as well as the p-value).

9. Figure 4 (and the comments about mortality rates in London Boroughs), however, does not add anything over and above figure 3. It is difficult to distinguish the different shades, and the mapping will mean nothing to those outside London. Furthermore, the figure is labelled as showing infant mortality rates, and yet project indigo included deaths to age 2, so the correct denominator should surely be the borough population of 0-2 year olds for each year.

10. I find figure 5a puzzling – why do you need to include data on both ‘co-sleeping’ and ‘not co-sleeping’? Surely the two are mutually exclusive and should add up to 100% excluding not knowns? You state that there was a trend with greater proportion of co-sleeping in the more deprived areas. Was this trend significant? Either way you should state and provide statistics.

Discussion

11. The authors state that the numbers of SUDI deaths have remained relatively constant. This is correct, but of limited meaning without knowing rates. Are you able to calculate SUDI rates based on population estimates for 0-2 year olds for each year of the study?

12. Page 9, line 186: the statement ‘However, the additional findings from the present study allow assessment of how such social deprivation may affect risk behaviour’ does not follow. Please either provide some explanation of how they allow you to assess this or remove the sentence. I am not sure why the rest of this paragraph (line 188-201) is included and how it specifically links to your data. I would suggest these lines are removed, unless they can be shown to be relevant.

13. P10, l210: I’m not sure what the phrase ‘behaviour which is indicated with up to 40-fold increased risk of death’ means.

14. The statement (p10, l211) that ‘co-sleeping associated deaths were associated with accommodation problems in a relatively small number of cases’ is misleading as this was reported in 22 cases, which is not much less than the 28? reporting customary co-sleeping. I think it may be equally important to
highlight that customary co-sleeping was found in only 28/171 (16%) co-sleeping deaths reported customarily co-sleeping; the majority presumably therefore saw this as an exception to their usual practice. The sentence is also rather clumsy with two ‘associated’s in it. Can you not just refer to these as ‘co-sleeping deaths’ rather than ‘co-sleeping associated deaths’?

15. You comment that (p11, l222) ‘the data also demonstrate that several associations, such as social deprivation and smoking, remain valid for all infant deaths, regardless of cause.’ This would be more accurate as ‘remain valid for all unexpected infant deaths, regardless of cause.’ Also, it would help to demonstrate this if the results were presented in a table as a comparison of the explained and unexplained groups.

Conclusions

16. Your first statement (p13,l269) that the study ‘demonstrates continued association between infant death rate and social deprivation in a large urban population’ is true (though should more correctly state ‘between unexpected infant death rate and social deprivation’). The next statement (p13,l271) is not – you do not provide data on the relationship between social disadvantage and increased parental/carer risk behaviours such as use of cigarettes and alcohol, only on proportion of deaths which were associated with co-sleeping (and you have not indicated whether or not this was a significant trend).

17. P13,l274: I don’t think you can say, from the data you have presented, that ‘in most instances the reasons for co-sleeping are behavioural or cultural rather than practical’ That is simply your interpretation of the reasons given.

Minor essential revisions

18. P2, line 27: ‘We investigate such data to provide…’

19. P2, l37: ‘Routinely collected data provide…’ (data are plural) also, p5, l84: ‘data… were reviewed…’; ‘All data were collected…’; p8,l162: ‘The data are therefore…’; p12,l243: ‘since the data were collected…’; p12, l244: ‘since the data were fully anonymised…’

Level of interest: An article of limited interest

Quality of written English: Acceptable

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.