Reviewer's report

Title: Contribution of respiratory tract infections to child deaths: a data linkage study

Version: 3  Date: 13 September 2014

Reviewer: Mark Ashworth

Reviewer's report:

Thanks you for asking me to review this paper.

Major Compulsory Revisions

1) The assumption concerning winter excess of RTI related deaths might not be entirely valid. The assumption (line 125) states:

"We assumed that the winter excess of RTI-related deaths could potentially be prevented through prompt identification and treatment, universal vaccination strategies with effective vaccines or improved hygiene measures such as handwashing."

An equally valid assumption would be that indoor social activity in winter months contributed to excess RTI-related deaths. But noone would therefore suggest living outdoors in the winter months. In other words, a proportion of winter-excess deaths are NOT preventable.

Moreover, if the assumption was that universal vaccination strategies could be adopted to reduce excess RTI-related deaths, why was this not specifically tested. It is entirely possible, although not investigated in this study, that the excess deaths occured in those who had already been vaccinated.

These limitations do not diminish the importance of the findings. But they should be mentioned in the Discussion and the overall findings presented more cautiously. Any implication that ALL excess RTI-related deaths in winter are preventable should be avoided.

2) Similarly, although the thrust of the article quite rightly was on the potential for preventing RTI-related death, the majority of deaths occurred in children with Neuro conditions. And they have to die of something. So RTI probably represents an end-point of neurological (usually cerebral palsy etc) debility. If you successfully treat/prevent the RTI, it will surely only return after a few months of further progressive neurological decline. This issue of 'deferred death' rather than 'prevented death' should be discussed.

3) The issue of Flu, Pneumococcal vaccination and potential RSV vaccination in children is not clearly discussed. The authors merely conclude, 'However, evidence is needed on the effectiveness of these preventive programmes in practice'. The authors have identified and defined the categories of child death
which involved RTIs. They have implied preventability based on winter excess. But what about reasonable estimates of the preventability that could be attributable to RSV, Flu and Pneumococcal vaccination programmes (and in those with an without LTCs). Even a ball-park figure would give a context to the data - and there are several references which could be used to supply estimates (although I grant you that these are very rough estimates for this population).

Minor Essential Revisions

1) Abstract: I think it would help to clarify the statement 'These deaths declined by 2.3% per year in infants.....'. Firstly, the word 'these' is ambiguous and the sentence would be clearer if it began: 'In infants aged 28-364 days,.....'. Secondly, the rate of decline in deaths is described as 2.3% per year. Is this 2.3% per year, year on year (i.e. a reduction of well over 23% over the 10 years, based on the effect of cumulative reductions of 2.3%)? Or is it a total of 23% reduction over 10 years?

2) Introduction, para 1. The authors correctly cite the literature for deaths from pneumococcal infections prior to introduction of the pneumococcal vaccine in 2006. Why no reference to deaths since then (data are available)?

3) The Methods used by the authors in obtaining records are excellent. They did well to identify 14180 hospital records of the 22509 deaths. They should discuss in the Limitations section that records for the remaining one third of child deaths will be in primary care, may have contained data on RTIs, but were not included in this study. In practice, these data are not available and until the HSCIC issues with care.data are resolved, no researchers have national access to patient-level primary care data. Nevertheless, this should be stated as a Limitation.

4) The justification for the authors' Method comes in the Results. Had they confined their analysis to Death Certs alone, they would have merely identified 3339 child deaths attributable to RTIs. Using linked hospital records, they identified 5039 child deaths with codings for RTIs. The authors could make more of this finding of another ~1700 RTI-related deaths identified by case-note searches. It is mentioned in the Discussion, Pg6 Line 215, but not the fact that this yielded almost 50% additional cases.

5) A key finding is the change in RTIs over the 10-year study period. I could not see the actual figures. The authors display the data in Figure 1. See comment about Abstract above - it was unclear whether the key finding of 2.3% reduction each year (children aged 1 month to 1 year) referred to a cumulative reduction of 2.3% every year which over 10 years would add up to considerably more than 23%. To clarify, it would be more helpful if the authors summarised the TOTAL 10-year reduction in % in this age cohort.

6) Looking at the Supplementary Files, I thought Table S2 should be discussed further (in the Discussion). How does it happen that a sizeable proportion of children dying in hospital, have no record of an RTI in their case-notes and yet RTI appears on the Death Cert?
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

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