Reviewer's report

Title: Measles vaccine coverage estimates in an outbreak three years after the nation-wide campaign in China: implications for measles elimination, 2013

Version: 1

Date: 4 December 2014

Reviewer: Felicity T Cutts

Reviewer's report:

This paper has greatly improved since the previous version and the authors have made a lot of effort to respond to previous comments. Remaining comments are:

Major Compulsory Revisions

Line 67 and lines 248-9. The recent national survey showed over 99% coverage for MCV1 and 93% for MCV2 – although the authors have provided a little more information about that survey, these findings do not support the conclusion that reported coverage is exaggerated. The journal that published the 2011 survey is not easily accessible. Some discussion of why the survey found such high coverage is needed – e.g. the authors could comment on the sampling method used (simply saying that children were randomly sampled is not enough – in reality it is rare that true random selection is feasible), and on data quality control in that survey compared to international guidelines, could be helpful. In lines 248-9: the authors reference surveys that support their own findings but ignore the 2011 survey which did not. It is here that some discussion of why the 2011 survey results may also have been inflated is needed.

It is good to see a section on limitations. The following additional limitations should be discussed:

- laboratory methods need to be discussed. How do the titers measured by the Virion EIA compare to other assays? Was the international reference serum used in the runs? How sensitive and specific is this assay compared to the gold standard plaque reduction neutralisation assay? There is presumably a chance of misclassification of serological status and this should be included in the limitations.

- Lines 169 and 175 show that there were more MCV2 doses administered than MCV1, and coverage was higher for MCV2 than MCV1 in some clinics. This is not possible as a child cannot have a 2nd dose of vaccine without having received a first dose. It implies that there is some migration between clinics, so that children who received a 1st dose elsewhere came for their 2nd dose, or alternatively that some doses were mis-recorded as second dose when in fact they were first dose.

Minor essential revisions

Lines 117 and 123 and 133 – in their response to previous comments, the authors clarified that coverage was measured among children aged 9 months or
older. Therefore should the methods also state children aged 9 months to 15 years old etc.?

Lines 149-150. “Sera samples for which the results were equivocal were retested using the same method and then categorized as negative or positive.” This is still not quite clear. What if the 2nd result was also equivocal?

Line 159 – thank you for including the incidence rates, but can you add the denominator (rates per 1000 or 100,000 or million population)?

Lines 279 – 290. These are important points but the authors should point out that if the administrative coverage estimates used birth cohorts as the denominator rather than children registered at clinics, the resulting coverage would be very similar to that found by all the other methods. Given that the data on birth cohorts are readily available, it would seem that the first step to take is to re-calculate coverage rates using the numerator data reported by clinics and the denominators from the statistics offices. If this does not help to explain why outbreaks are occurring then community-based surveys may be needed, and when they are done, they should use rigorous methods with probability sampling (and please provide some references for readers on coverage survey methods, e.g. recent papers available in open-access journals).

Level of interest: An article of importance in its field

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