Reviewer's report

**Title:** A two-stage Bayesian method to estimate the accuracy and disease prevalence for two dependent dichotomous screening tests when the status of individuals negative on both tests is unverified

**Version:** 2  
**Date:** 31 July 2014

**Reviewer:** Sheng T Luo

**Reviewer’s report:**

This manuscript developed a two-stage Bayesian method for two dependent dichotomous screening tests with unverified individuals who are negative on both tests. Provided below are some major compulsory revisions.

1. The authors claimed that they resolve the non-identifiability problem using two-stage Bayesian modeling and a two-step hierarchical uniform prior. However, I have found that this statement was not well-supported. For example, page 12, the authors use uniform priors to $S_{e1}$ and $S_{e2}$ with lower bounds 0.5 and 0.6, respectively. These priors were still very informative and strong. I would like to see the results when (1) the lower bounds were set to be 0, and (2) using Uniform(0, 1).

2. To follow point 1, I have found that the simulation study was not sufficient. I would like to see the results when weak priors are imposed on all parameters. If the results are as inaccurate as the case of strong priors for Sp alone (which I suspect that it will be the case as suggested by Table 5 under the column of strong priors for Sp alone), it indicates that it is the strong priors on Se that make the model identifiable. In that case, what is the use of the proposed methods in the absence of good priors for Se. Is it correct to say that the model discussed in the manuscript is not identifiable when weak priors are imposed on all parameters? Also, I have found that the simulation for incorrect priors for Se alone is redundant. It is well-known that incorrect priors lead to biased parameter estimation.

3. Page 15, lines 6-7, please rewrite this statement and explain how it was supported.

4. Page 15, lines 14-15, some insight is needed to explain why specificities were always estimated with greater precision than the sensitivities when the prevalence is low.

5. Page 15, lines 17-20, please rewrite this sentence and explain how it was supported by the results.

6. Page 15, lines 23-30 and Table 7, it was not clear what independent and dependent data mean and the difference between them. Same question for independent and dependent models. Some clarifications were necessary.

7. The English needs to be improved as many sentences were confusing. For example, Page 16, line 15-21, the authors repeatedly used the word “insight”. But
that was the “methods”, not insight. Page 17, line 2, “even in the absence of the priors” should be “in the absence of strong priors”. Page 18, line 4, “the modeling often only involves the priors for the sensitivities” is very confusing. Don’t you need to specify priors for other parameters?

8. Page 12, lines 31-32, the statement “adding approximately 0.13 to the sensitivity, at the expense of only an approximately 0.04 loss in specificity for the colorectal cancer data”. I did not see where these two numbers come from.

**Level of interest:** An article of limited interest

**Quality of written English:** Not suitable for publication unless extensively edited

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

**Declaration of competing interests:**

I declare that I have no competing interests.