Reviewer's report

**Title:** Use of Common LRs: A Useful Tool for Taking Evidence Based Clinical History

**Version:** 1  **Date:** 11 February 2010

**Reviewer:** Jean Karl Soler

**Reviewer's report:**

Major necessary revisions

The article is a good piece, which makes a good point about studying combined likelihood ratios for categorising a group of diagnoses (say, infectious respiratory illness) as more or less likely based on the symptoms. This is a useful development on the research of LRs in clinical practice.

However, a major issue is that LRs for different diseases cannot simply be combined when the diseases have different prevalance (and therefore prior and posterior likelihoods) in the population. The statistical approach of averaging LRs seems to be weak, without weighting for different probabilities. Can you simply combine the LR for influenza and meningitis in GP? I have a major issue with the common likelihood ratio and its method of calculation.

Many important and common infectious diseases have been excluded from the example category (such as URTI and gastroenteritis, for example), and as such the category is incomplete. Thus the LR calculated does not decrease or increase the likelihood of the entire category of infectious diseases, but only those listed. This missing information would have a major influence on the common LRs for all infectious diseases. This is compounded by the fact that not all the combined LRs should have the same weight, as some diseases are far commoner than others.

LRs are not available for all symptoms and signs, and all diseases in medicine. Thus, the claim that this method should replace clinician diagnostic acumen is indefensible. The statements on diagnostic error are biased, and do not reflect the complexity of the literature on clinical decision making. The authors are not experts in diagnosis in GP and should not jump to conclusions on the quality of the diagnostic process in GP without objective evidence. Proposing a “better” system without evaluation is not scientifically acceptable.

From the abstract and text, it is not clear whether this is a review of the published literature, and in this case the search strategy should be described, or else new research, in which case the methodology of the development of this new approach should be described better and substantiated by contrasts with the existing literature.

The methodology section is missing, and we do not have a clear aim for this
The study is a review or new material?

The English of this article is not of sufficient standard, and requires a number of corrections.

The formulae should be subject to independent statistical review before publication.

Suggested corrections:

Page 4. Paragraph 1. If this is the introduction it should be labelled as such. The part on deficiencies in diagnosis by doctors is reductionist, and does not reflect the broad literature on the topic, including the controversy about whether Bayesian approaches and intuitive/pattern matching approaches to diagnosis are superior in all situations. In fact, from my perspective it seems that either may be better in different situations, with different doctors.

Page 4, last paragraph. “A Bayesian approach and objective refining of the probabilities...” Different categories of diseases – this term/concept should be defined and explained, and referenced to previous literature. There is a short explanation of the purpose of the article, which should be carried over to the abstract. There is no mention of prior probabilities, only likelihood ratios, and this is an important issue to be discussed. LRs cannot be used to predict a posterior probability without the prior probability.

Page 5. This article is missing an appropriate methodology section. It should not be published without it.

Page 5. Paragraph 1. This is an example of poor use of the article in English: “Studies on the diagnostic decision-making...” “The physicians’ lack...”

Page 5. Para 1. This is a sweeping statement based on one reference only, and should be removed, reworded, or else supported by more literature. The authors have not studied doctors’ decision making, and cannot conclude that it is deficient in general, on the basis of one reference.

Page 5. Paragraph 2. “In the field of clinical diagnosis, a central notion of EBM is the application of LRs in diagnostic decisions. LRs can be used to assign a probability ranking to a particular disease through refining its probability, i.e. disease assignment.”

Page 6. Paragraph 1. The condition of malaise has been studied by Kenter et al, in the European Journal of General Practice. You may consider including data from this study.


English correction: “When the physician encounters non-specific problems which lead to a wide set of differential diagnoses related to...”

Page 6. Section on expert-based decision making and its drawbacks. The argument on the potential failure of clinician expertise and in favour of analytical
approaches to diagnosis is far more complex than stated in this section. I have reviewed the literature on clinician decision making, and in summary, there are various theories of the diagnostic process and clinical reasoning, but there seems to be agreement that various types of reasoning and pattern recognition are at play, from the more analytical to the more intuitive. More experienced clinicians are able to use different types of mental diagnostic processes depending on the context of the problem. Common problems may be solved by pattern matching or intuition, requiring less information and less time, whilst more complex or infrequent problems may be solved by returning to basic skills of analytical processing of information gathered much more systematically. Thus experienced clinicians tend to indeed perform better than less experienced clinicians, and possess superior knowledge of many kinds, formal and informal. The process may fail due to a number of biases, and in some cases which are complex, the step-by-step analytical deductive process may produce better results. However, the Bayesian approach is not the best one in all circumstances. The authors need to re-write this section entirely to reflect current realities. The paragraph is too simplistic, and denigrates clinician experience.

Page 8. Section on use of common LRs to refine the probability of disease. This section is full of English grammatical errors. I also have a major issue with the exemplar category of infectious diseases, and the likelihood ration thus calculated. Many important and common infectious diseases have been excluded from the category (such as URTI and gastroenteritis, for example), and as such the category is incomplete. This missing information would have a major influence on the common LRs for all infectious diseases. Furthermore, the combination of these LRs assumes that these diseases contribute equally, but we know that this is not the case. Meningitis is far less common than influenza, and the LR for influenza is far more important than that of meningitis for a common LR for infectious disease. By ignoring the prior probabilities of these conditions, the theory presented by this paper is seriously weakened and rendered clinically inapplicable.

Page 8. Second paragraph. The statement that “this approach still lacks objectivity” and that GPs should use common LRs to refine category assignment more objectively, is a sweeping statement not supported by evidence. Firstly, the data used for this example are not all based on primary care data, and the LRs can be wrong for primary care. Secondly, there is no evidence that this approach to diagnosis, using category assignment, is more accurate than actual clinical practice for common infectious disease. The authors are not experts in diagnosis in GP and should not jump to conclusions on the quality of the diagnostic process in GP without objective evidence. Proposing a “better” system without evaluation is not scientifically acceptable. The method should be described without such value judgements.

Page 9. Paragraph 1. “However, there are major pitfalls in using these key clues and all physicians may not reach to the stage of using these clues. In addition there are variable diagnostic biases in using experience and heuristics.” This is again judgemental – there is no evidence that Bayesian methods provide
superior results to clinician diagnoses in all situations. In fact, this is known not to be true.

Page 9. Paragraph 1. “...the hallmark of the disease is only about 1.9 [28]”. Is this a percentage or a LR? In that case the statement should be made more clearly.

Conclusions. Please only include conclusions that are supported by the results of your study.

Table 1. Please define LR+, LR-, and category (in this table, maybe term it as category of infectious disease?)

Appendices 1 and 2 should be reviewed independently by a statistician.

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

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

'I declare that I have no competing interests'