Reviewer's report

Title: The Probability of Cost-Effectiveness

Authors:

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Reviewer: Dr Andrew R. Willan

Level of interest: A paper whose findings are important to those with closely related research interests

Advice on publication: Unable to decide on acceptance or rejection until the authors have responded to the compulsory revisions

General Comments
The authors take issue with a definition for the probability of cost-effectiveness proposed by Willan1, and propose one of their own. The main difference reflects the fact that the authors take a Bayesian approach for which probability statements about population parameters are permitted, while the definition they criticize is based on a frequentist approach for which probability statement are confined to events.

Specific Comments (compulsory revisions required for all)
1. The authors claim, with no substantiation, that knowing the probability that a patient given treatment 2 has a higher net benefit than a patient given treatment 1 is of "little on no relevance to a policy maker". The health care providers I deal with are most definitely interested in knowing how likely the next patient will benefit from potential interventions. Furthermore, it is not just the probability for the next patient, it also the probability for all future patients, providing the aggregate prospective as well. Most population parameters estimated in clinical trials apply to individual patients (e.g. mean difference in cost, mean difference in effectiveness), but they are still very much of interest to the consumers of the results of clinical trial since they provide estimates of the clinical significance of the difference between treatments 1 and 2.

2. The authors state, "Willan's dislike of the fact that Q(K) depends on the data seems to be based on failing to appreciate that it is an inference, not a parameter." The authors are mistaken. Willan makes it abundantly clear that his concern about Q(K) being labeled "the probability of cost-effectiveness" is that it is an inference. Quoting their quote from Willan's paper, "Attaching the label 'the probability that the intervention is cost-effective' to this quantity could mislead policy makers into thinking that treatment is highly beneficial compared to standard. What, in fact, is high is our confidence that the INB, however small, is not zero." And quoting from Willan's abstract, "These definitions are, in fact, expressions of the certainty with which the current evidence would lead us to believe that the treatment under consideration is cost-effective."

4. The statement that, "This seems to indicate a low opinion of policy makers" is not substantiated.
Recognizing the fact that people can be confused by confusing labels does not confer a low opinion.
5. I agree with the authors when they state, "Our certainty, based on available evidence, that treatment 2 is more cost-effective that treatment 1 is properly expressed by a probability" whether it be a p-value or a Bayesian posterior probability. That does not mean that consumers of the results of clinical trials will not be confused by the label "the probability of cost-effective."
6. The authors make good point when the take issue with the estimator proposed by Willan. Because of the likely positive correlation between a patient's observed net benefit on treatment 1 and his or her observed net benefit on treatment 2, the proposed estimator is biased toward 0.5. Therefore, if in truth net benefit on treatment 2 is larger than on treatment 1, Willan's estimator provides a lower bound for the estimate. It does not mean, according to the authors that, "it is completely impossible to learn about p(K)." Nor does it mean, "his proposed estimate of this probability is logically flawed." Using the data from a clinical trial, one could easily provide a sensitivity analysis over a range of correlations.

Reference

Competing interests:
None declared.