Reviewer's report

Title: A cost effectiveness study of caesarean-section deliveries by clinical officers, general practitioners and obstetricians in Burkina Faso

Version: 1 Date: 15 January 2009

Reviewer: Staffan Bergström

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There are both major compulsory and minor essential revisions:

1. Is the question posed by the authors new and well defined?

There is no explicit hypothesis stated but, implicitly, the conclusion is a response to an underlying well defined question.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

The big problem here is the choice of a retrospective approach with all uncertainties that imply. The authors should justify this choice, not merely due to its simplicity or, even worse, "because of time and resource constraints" (p.4).

3. Are the data sound and well controlled?

From the retrospective approach follow several questions:

a) The authors state (p.4) that "Casemixes by providers were assessed by exploring the association relationship of the key effectiveness measure with providers adjusting for reported diagnosis and referral status (proxy for severity of cases)". Given the retrospective approach how can this be convincing or even indicative? Much more clarity is required here.

b) District hospitals (DHs) are said to be "standardized" (p.8) in BF. What does that imply in actual practice? Obviously there is a risk that ruralmost DHs deviate substantially in function from urbanmost DHs. This has of course important implications for newborn CFR, particularly since the timing aspect was ignored by the authors [p.8: " Also, the reliability of our main effectiveness measure (newborn case fatality rates) suffered from lack of precision on timing of deaths (retrospective study) which may be useful in associating deaths with surgical teams or monitoring of the labour. "].

c) The authors state that "there was a 20% shorter duration for the operation and a 30% shorter duration of post-operative hospital stay with obstetricians" (p.5). Who made the decision re discharge of patients? In the same paragraph it is said that the postop (maternal) complications in the three groups did not differ. So, what do both these differences in duration imply?
d) Cost effectiveness (p.7). I think these precise calculations are not possible to make given the facts, firstly (admitted by the authors above) that timing of a newborn’s deaths was ignored and, secondly, that the district hospitals cannot be assumed to comparable, and, hence comparisons must be assumed to be skewed. On p.7 the authors state that "The lower part of Table 4 presents the Incremental Cost Effectiveness Ratio (ICER) of adverse outcomes for newborns after a caesarean delivery by type of providers. These figures can be interpreted as the additional cost of saving a newborn’s life, moving from one provider to another". This interpretation is not convincing giving the design of the study.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Questionable for reasons detailed above.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

There are several assumptions that must be better justified. See above.

6. Do the title and abstract accurately convey what has been found?

It is thought-provoking to read (p.9) the authors' caveat that the study, in fact, may be invalid: "Unfortunately, retrospective records are deficient, and a prospective data collection was beyond the scope of this study. It is therefore difficult to reliably attribute adverse outcomes to configurations of providers. Any inference should thus be made with caution". This is a too authors-friendly excuse for choosing a substandard design.

Level of interest: An article of importance in its field

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.