**Reviewer's report**

**Title:** Concurrent sexual partnerships and associated factors: a cross-sectional population-based survey in a rural community in Africa with a generalised HIV epidemic

**Version:** 1  **Date:** 6 May 2011

**Reviewer:** Helen Epstein

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Helen Epstein

Measuring concurrency and its association with HIV is complex. Unfortunately, the authors make a number of common mistakes, including measuring the wrong indicator and neglecting to consider the possibility of under-reporting, especially by women. Ideally, the authors would have measured current (day of interview) concurrency at two points in time and HIV incidence in the partner of the person practicing concurrency (controlling for his/her behavior) which is the closest thing we have to a “gold standard” for measuring the relationship. The authors’ approach, measuring any concurrency in past year vs prevalent infection in the practitioner, has many pitfalls, which are discussed in detail in Epstein, H and Morris M “Sexual Relationship Concurrency and HIV: An Inconvenient Truth.” Journal of the International AIDS Society, March 2011. I strongly urge the authors to consider the arguments in that paper, which is available online for free.

A few detailed comments:

**Abstract:**

Background: There is actually little controversy over the definition of concurrency, but there is considerable confusion about how to measure it. UNAIDS recently came up with a set of proposed measures (Lancet 2009) but as far as I am aware, they have not been approved by the agency’s Indicator Review Panel. Particularly controversial is their measure of “point prevalence” which uses a very complicated “6 month retrospective” indicator. The problems with this, in terms of bias and difficulty of analysis are discussed in Morris, Wawer and Epstein PlosOne 2010. In addition, the UNAIDS indicator contains no measure of coital frequency, a potentially serious problem. (See Epstein, et al Lancet 2010)

Results: The authors found concurrency point prevalence to be 17% in men and 0.5% in women (using the problematic 6 mos retrospective indicator). The figure for women is very low and probably an underestimate. Measuring sexual
behavior in women—especially multiple partnerships-- is extremely difficult, and face to face questionnaire interviews may not be the best way to go.

See for example:


Langhaug LF, Sherr L, Cowan FM: How to improve the validity of sexual behaviour reporting: systematic review of questionnaire delivery modes in developing countries. Tropical Medicine & International Health 2010, 15:362-381.


….and many other references on this topic. The fact that this issue isn’t discussed in the article at all seems a serious omission.

Conclusions: Given the above concerns, it seems premature to say that these findings “provide a baseline” for measuring concurrency and HIV incidence in future studies. Rather, they provide a first attempt to measure this complex behavior and relate it to other characteristics in people at risk of infection.

Background

This section does not seem to have been informed by a balanced reading of the literature. The authors refer to Lurie and Rosenthal’s critique of the concurrency hypothesis in AIDS and Behavior, but don’t cite the three separate responses from Mah and Halperin, Epstein and Morris that were printed in the same journal. The authors cite Sawers and Stillwaggon’s critique of the concurrency hypothesis in the Journal of the International AIDS Society, but don’t discuss the rebuttals by Epstein and Morris and by Goodreau. From these articles, it should become clear that the evidence for the importance of concurrency does not just “derive from mathematical models” but from empirical research as well.

The authors also refer to a paper in AIDS suggesting polygyny was protective against HIV by Reniers and Watkins, but don’t cite the critiques of it by Epstein and Stanton in AIDS and by Epstein and Morris in JIAS. Most problematic was that the authors controlled for extramarital sex in their analysis, which would be
the obvious route through which HIV would enter polygynous relationships. Also, with the exception of the study under review, all other studies have found that, all other things being equal, polygyny is associated with HIV, STIs and extramarital sex on the part of the man or woman.

Results
The language here is quite vague, and it is difficult to figure out what the authors are saying. They write, “HIV prevalence was 8% in men and 37% in women in concurrent partnerships…” does this mean among the people who HAD concurrent partners? Or does this include those who were in a relationship in which at least one partner had a concurrent partner? And are the authors referring to concurrency in the past year? Or concurrency 6 months before the interview?

Also, given that HIV prevalence was 4.5% among all men and 7.4% among all women (from the second para under “characteristics of cohort participants”), I am surprised that the authors found no relationship between HIV status and concurrency, even by this imperfect measure, especially in the unadjusted models….but perhaps I am missing something.

Discussion
The concurrency modules in the DHS surveys were highly flawed, although the newest questionnaires are improved, as discussed in Epstein and Morris JIAS 2010. This should be mentioned.

“So far as we are aware it is the first population-based survey to measure the prevalence of concurrency in sub-Saharan Africa using the new standards….”

Not really, see the 2009 Lesotho DHS, and Morris, Epstein and Wawer PLOS 2010 on Ugandan, Thai and US adults. That study used the “day of interview” measure of point prevalence, which is sounder, and also measured coital frequency. In addition, although not peer-reviewed, PSI has carried out a concurrency survey in Botswana and perhaps other countries using the “point prevalence” measure. These reports are not peer reviewed, but they are available and should be referenced. Also see Morris et al. AJPH 2009, on US adolescents.

The authors write, “Our findings emphasise the importance of framing these HIV prevention efforts in the context of the deep-rooted social, economic and cultural determinants of gender dynamics.” Can they elaborate on what such a program might look like?

The authors conclude, “Our findings provide a baseline from which to measure concurrency and HIV incidence in future surveys, and a benchmark against which the findings of other studies also using the standard definition and methodological approaches can be compared.”

As stated above, this reviewer is skeptical about this claim.
Suggested major compulsory revisions:

1) Revise the discussion of the evidence base for concurrency, as above.

2) Recast the discussion of the findings related to the relationship between concurrency and HIV, noting that the present attempt was limited by the methods of data collection used.

3) Make recommendations for a future study which would use improved methods to ask similar questions.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests