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

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Reviewer: Mark Colvin

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1.0 The research question is clearly stated: to determine the prevalence of concurrency, to investigate its association with sociodemographic and behavioural factors and with HIV prevalence in a particular community.

1.1 The setting and methods are clearly and comprehensively described. The authors have used the definitions and methods recently recommended by UNAIDS which is an important factor because previous research into concurrency has been plagued by authors using a myriad of definitions and methods making comparison between studies difficult. Although not all parties may believe that the UNAIDS definitions are ideal, it does allow for standardisation using this agreed approach.

1.2 Although the response rate was disappointingly low and may bias the prevalence estimates of concurrency, this alone probably does not exert significant bias on the association between concurrency and associated factors.

1.3 With the provisos raised under “Major revisions” below, the data appear sound and the description and methods of analysis are appropriate.

1.4 The finding that there was no significant association between concurrency and HIV infection in men is important because it is contrary to the popular theory that concurrency is “driving” HIV in Africa. The data also fails to show an increase in HIV prevalence among the spouses of men involved in concurrent relationships which goes against the theory that it is primarily the partners that are at risk rather than the person involved in concurrency.

1.5 The limitations are discussed adequately apart from the issue of under-reporting among women.

1.6 The discussion and conclusions, with the exception of the “major revisions” below are appropriate and based on the study results.

1.7 The main strength of this paper is that is one of the first to use the new, internationally recommended and standardised definitions and methods to research sexual concurrency. However, as the authors point out, the issue of the importance of concurrency in driving HIV transmission will be resolved by conducting large scale, longitudinal studies which allow HIV incidence to be
measured. Cross sectional studies such as this one will not resolve this major (not to mention vitriolic) international debate.

Major Revisions

2.0 Low rates of concurrency among women. The very low rate of self-reported, concurrent relationships among women (0.9%) vs 15% reported by men, cannot reflect reality. Either men are grossly over-reporting (unlikely) or women are grossly under-reporting (more likely). There is no other plausible explanation for such discrepancies. It is possible that fewer women are having more concurrent partners than men have on average but the data does not reflect this plus it is unlikely to account for such big differences. It cannot be that almost all the women who are engaged in concurrent sexual relationships somehow are not accessed through such community-based studies time after time.

2.1 Certainly, other studies show gender differences in self-reported concurrency/multiple partnerships but usually not to this degree. Multiple partnerships in the last year reported by women varied between 22% and 9% (men: 61% to 46%) in Swaziland in community-based surveys between 2002 and 2008 (Cockcroft, 2010). Ho-Foster (2010) in a national survey in Botswana report a 19% concurrency rate in the last year among men and 6% among women. (They also report that concurrency was more common among higher earning women which is different from what these authors report and is commonly believed about poor, young women being more likely to be in concurrent partnerships).

2.3 Many other authors also seem to gloss over this problem of gender discrepancies and merrily report how their findings are compatible with other reports but such extensive under-reporting not only creates small numbers (17 in this case!) to work with but, more importantly, is likely to also create bias and hence not be representative of women.

2.4 The authors conclude, from 17 cases, that women in concurrent relationships are more likely to be young, unmarried and of lower SES. This makes sense, fits with our preconceptions about the “sugar-daddy” syndrome and may be true but could easily be due to under-reporting by older, married women, i.e. bias. See appendix 1 to this review which discusses the issue of underreporting by women.

2.5 I would recommend removing women altogether from this analysis because of small numbers and likely bias. However, the decision in this regard should be taken by the editors.

3.0 Including intra-marital, polygynous relationships as concurrent relationships. Another concern that I have is that polygynous, concurrent relationships are lumped in together with extra-marital concurrent relationships (with the exception of one definition of concurrency which excludes polygynous relationships). As the authors acknowledge, polygyny may “deserve special consideration” and cite Reniers (2011) who makes it clear that polygyny has a protective or “benign effect on the spread of HIV”. If this is true, then including polygynous relationships as concurrent relationships an any analysis will “dilute” certain
associations between concurrency and HIV.

3.1 Because of this, I would recommend that intra-marital, polygynous concurrency be analysed completely separately because in the narrative it is not always clear which measure of concurrency is being used. Also, it would mean that the authors do not have to report the obvious “finding” that polygyny is associated with concurrency among men. Duh – by definition it is!

4.0 Reviewer unable to follow the logic: I fail to follow the logic in paragraph 2 of the discussion. Because there is no association between HIV prevalence and concurrency in men, why is the risk of HIV infection in men automatically because of “the type of partner involved in concurrency and other characteristics of the partner.”? Why could it not be condom use, sexual frequency or a range of other factors? The next sentence in the paragraph also does not follow logically because there is no evidence presented that the persons partners are at higher risk of HIV. Indeed, the authors themselves, in paragraph 5 of the discussion, confirm that there is no higher prevalence among spouses (see last paragraph in results section). Maybe I have completely missed something here but clarity is required.

References


Appendix 1. Under-reporting by women

In many studies using self-reporting on sexual partnerships, it is important to factor in underreporting of secondary partners and in particular extramarital affairs. Gregson et al. (2002b), in a study in Manicaland (Zimbabwe), compared two interview methods, a standard face-to-face interview in which the respondent’s answers were known by the interviewer and an Informed Confidential Voting Interview (ICVI) in which the respondent submitted responses through a ballot box and the responses were not known by the interviewer. When interviewed through ICVI, men were more likely to report multiple sexual partners currently (OR = 1.33, p = .028), in the past month (OR = 1.71, p = .002), and in the past year (OR = 1.35, p = .002). The discrepancies in women’s reporting of multiple partners were even more striking, with women more than 5 times as likely to report having multiple partners currently (OR = 5.24, p = .001), nearly 3 times as likely to report multiple partners in past month (OR = 2.92, p = .001), and nearly twice as likely to report having multiple partners in past year (OR = 1.97, p = .003) when interviewed using the ICVI method versus face-to-face interviews. Gregson et al. also report that this effect was strongest among
married women, that women cohabiting with their husbands were least likely to report extramarital partners (OR = 0.40), and women in rural areas were less likely than women in estates and business centers to report multiple sexual partners (OR = 0.36). The only reporting of extramarital partners among married women (9 of 881 women) and rural women (13 or 1175) was through the ICVI method.


Level of interest: An article of importance in its field

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