The perils of conducting meta-analyses of observational data

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Dear Editors,

We read with interest Li and colleagues’ recent article in this journal [1], which conducted a systematic review and meta-analysis of the literature on associations between intimate partner violence (IPV) and HIV infection in women. The authors found a significant positive association between various types of IPV and HIV, concluding that “physical violence, sexual violence, a combination of physical and sexual violence, and any type of IPV were associated with HIV infection in women (p. 7)” [1].

The authors noted in their Table 1 that a past study of which we were co-authors reported no significant association between IPV and HIV in 10 Demographic and Health Surveys conducted between 2003 and 2007 [2]. We were therefore surprised to see that, in conducting their meta-analysis, the authors report that our data show significant positive relationships between HIV and several types of IPV (Figures 2 and 3 in [1]). With the exception of the figure for “any type of violence” which appears to be a typographical error, the figures quoted seem congruent with the unadjusted, pooled numbers of which appears to be a typographical error, the figures quoted [1]). With the exception of the figure for “any type of violence” which appears to be a typographical error, the figures quoted [1]).

The perils of conducting meta-analyses of observational data

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First, in integrating two multi-country studies [2,4], Li and colleagues have hidden any heterogeneity present within studies, by aggregating together data from all countries without allowing for country-level confounding. As can be seen in Table 3 of our paper [2], simply adding a fixed-effect indicator for each country into a bivariate logistic model with a binary exposure considerably attenuates the association between IPV and HIV. This change reflects confounding and effect-modification at the country level due to a positive relationship between country-level IPV rates and country-level HIV rates in our sample. Li and colleagues could have avoided this forced homogeneity by using country-specific numbers for each of the four exposure/outcome categories: when we analyzed each country individually, unadjusted associations were notably smaller than the pooled value used in the

The Cochrane Handbook for Systematic Reviews of Interventions notes that a systematic review need not include a meta-analysis and “*[If there is considerable variation in results, and particularly if there is inconsistency in the direction of effect, it may be misleading to quote an average value for the intervention effect.*]” (Section 9.5.3 of [8]). We believe that this paper highlights the potential hazards of using aggregate data to conduct meta-analysis of observational data, particularly when the data extracted from published papers are not adjusted for possible confounding variables. Even when literature is identified through an exhaustive systematic review, it may not be feasible or responsible to conduct a meta-analysis if the identified papers are not deemed comparable, or heterogeneity across trials and populations is detected [3]. Should the authors still wish to conduct an analysis, alternative approaches, such as using fully individual-level data [9], meta-regressions, or a combination of
aggregated and individualized data [10], may also prove fruitful in generating trustworthy effect measures.

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References