Reviewer's report

Title: Assessing the effect of integrating a new method into family planning programs in India, Peru, and Rwanda

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Reviewer: John Stanback

Reviewer's report:

This well-written paper met all the criteria for inclusion in this journal. It describes a well-conceived, apparently well-implemented study in three countries: Peru, Rwanda, and India. The study made use of both service statistics and household surveys in control and intervention areas, which was helpful in triangulating findings and drawing conclusions.

Major Compulsory Revisions:

My main issue with the manuscript was the authors’ tendency to overstate some effects. For example, I take issue with the language used in describing the difference between client volumes in the intervention and control areas in Peru. Although the average did increase in the intervention areas, there is no basis to suggest “that SMD introduction stopped a declining trend.” It is an overstatement, since there is no data showing a declining trend in the intervention clinics. Even though the authors’ qualified the statement in the following sentences, they went on to imply causation in the Discussion section, saying “Analysis of new users…in Peru suggests that the total number of FP users did increase as a result of SDM integration.” What analysis?

Similarly, the language in the results section referring to the increase in prevalence in the Indian intervention area accurately stated that there was a significant increase in CPR following SDM integration (but did not imply causality). However, the finding seemed overstated in the Discussion, which wrongly claimed that the intervention had a “significant positive effect” on CPR.

This is a strong paper, but, given the universe of alternative explanations for these relatively small changes, I would like to see this causal attribution language moderated.

Minor Essential Revisions

I have to say I was also confused between the findings of the household survey, that did indeed imply “substantial” uptake of SDM in all three countries, compared to clinic service statistics that never exceeded, on average, 1.5 new SDM clients per month per clinic. How could such a method-specific prevalence be achieved with such low service statistics? I would like to see an explanation in the discussion that accounts for this seeming contradiction.

The heading for the ‘Discussion’ section is missing
Discretionary revisions:
Table 2 shows the results of a logistic regression. Most readers can judge the significance and direction of effect from the raw coefficients, but their magnitude might be easier to judge if the coefficients were exponentiated, which (if memory serves) provides easy to interpret odds ratios. However, I am not a statistician, so please don't take this as gospel.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I declare that I have no competing interests.