Reviewer's report

Title: Integration opportunities for HIV and family planning services in Addis Ababa, Ethiopia: an organizational network analysis

Version: 2

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Reviewer: Ravi Ram

Reviewer's report:

OVERALL COMMENTS

The authors have identified a very relevant topic with a somewhat unique analytic approach. The issue of integrating HIV/AIDS services and family planning remains relevant, though it has been dealt with by many publications, including several from ICF/Macro/Measure Evaluation. The network analysis distinguishes this draft from other publications on HIV-FP service integration. It is also refreshing that the authors have a very accessible writing style, in both their analysis and other areas of the paper. This paper will be a solid contribution to the literature, with only a few major revisions.

Major compulsory revisions

In the data for Table 3, values for in-degree and out-degree referrals are identical, but that appears to be a bad result that requires checking the analysis.

A much broader, rigorous literature review is required, with relevant field reports about HIV/AIDS and family planning models of integration, coordination or referral linkages from Africa or other low resource countries is necessary. The current review is limited and superficial, with a bias in field reports to the authors' own past publications.

Minor Essential Revisions

Notes on the Abstract (p. 2-3):

In the Background, it would be better to reemphasize the roles of civil society and national governments as determining public health resource allocations; foreign governments ("donors") are to follow the local agendas, and should not appear first. Also, the authors may change "private sector" to "commercial/for-profit sector", to distinguish from non-profit/missionary activities that are technically private but very different from commercial enterprises.

Under Methods, there are several problems regarding the claim, "We interviewed each of them ...". First, the selection criteria are omitted here (though described in the paper); second, the researchers interviewed representatives of the organizations (directors, etc.); it appears sloppy to say that they interviewed the organizations. Finally and most importantly, what the researchers appear to have actually done was to administer a structured questionnaire, in person with a senior manager from selected organizations, in order to assemble a dataset. The implication of "interview" in a methods section implies qualitative research, which
is clearly not the approach undertaken here.

The authors should be more specific about the type of network analysis undertaken and the methods and tools. The framework for the variables (referral links, densities, degrees, etc., can be referenced). For readers without background in network analyses, slightly more detail in the abstract, and a stronger theoretical elaboration in the paper, would be very helpful and further support the accessibility of the article.

The relevance of the sharing of the results with stakeholders is lost here – the authors can describe the role of the workshop in validating findings and assisting interpretation.

Under Results, there are key results described in the paper that are omitted in the abstract; on the other hand, details about 1-2 organizations should not appear in this high-level abstract and can be kept in the main paper.

Main Paper
Background
This draft can be developed more fully, to focus more precisely on HIV/AIDS-Family Planning service integration and referral evidence, and to review a broader range of published reports, including those that describe with data the effects of these vertical service delivery approaches. Examples of overgeneralized statements that detract from the precision of the paper are:

• the first paragraph of the Background (p 4): “HIV infection affects virtually every.........is intertwined with and affects the others.“ – conveys a lay tone rather than scientific rigor.

• third paragraph of Methods (p 6): “... is generally thought to be the sub-city most affected by HIV” – data are needed to make this statement stronger.

• Fifth paragraph of Methods (p 6): “Once the organizations were identified, we interviewed each of them ...” – it is not possible to interview the organizations. Instead, the team administered the survey questionnaire to senior representatives of the selected, participating service providers.

• Second paragraph of results (p10): “four out of five” – referring to 20 out of 25 facilities, but a bit distracting to use a reduced ratio in the flow of the paragraph, when “80%” would be preferable.

Only two citations of other studies are provided in the Background, and both were written by an author of the current draft paper. Later in the paper, 3 other studies are referenced; but none from Africa or other LDCs, which is a major flaw in the literature review. ICF/Macro/Measure Evaluation have published several studies of HIV/AIDS-FP integration from East Africa, and those would be essential publications to cite. A much broader literature review is required for a complete background.

Methods
The first paragraph of the Methods section (p. 5) properly belongs in the Background.
The census is well described, but the selection process (organizations that are named at least twice, except for commercial service providers) raises questions. Why was this selection criteria introduced? How many organizations were censused, and what is known about those that were excluded? There is a potential for introducing bias in this selection criteria, which should be addressed (for example, small CBOs that interact only with one larger facility would be systematically excluded by this criteria). Recommendations are to describe as much as possible about the excluded organizations (n, characteristics), and to consider what biases or limitations are introduced by their exclusion, for instance on the referral links or densities in each sub city.

Some characteristics of the respondent individuals would be helpful, to say more than they were managers or directors. Here also, the potential for normative bias or poorer data quality is possible, and that should be addressed by the authors. Senior staff may have a different perspective than actual clinical/field staff, and their incentive to present their organizations favourably is higher.

As noted earlier, the emphasis on “interview” rather than “survey questionnaire” is misleading in that it implies qualitative research, when that actual methods and results presented are quantitative. The fact that the questionnaire was personally administered is worth noting, but only in the context of a structured survey. The “few open-ended questions” (p 7) do not seem to add data to this report; if there is information from those questions relevant to this publication, that needs to be made clear and elaborated. The mentioning of “coded” and categorized responses to open-ended questions implies some qualitative analysis. No such analysis is mentioned, and no data are presented. That should be included, or else explained that the results are not linked to the findings of this paper.

Methods for data collection for results shown in Table 2 are not described, or mentioned. For methods related to Table 3 data (p 8), more explanation is required, such as for “links” (included in Table 3 but not defined in the methods. Consistent terminology is also required, such as for “total degree centrality” (in methods, p 8) or “mean degree” (Table 3), which may refer to the same constructs by different names. Similarly, the in/out referral degree can be explained better.

The appropriate ranges for these variables should also be explained. Density, centralization and reciprocity appear to be scaled between 0 and 1. Centralization is noted as linked to in/out/mean/total degree referrals, but those have values higher than 1.

For those without experience in network analysis, a bit more explanation about the applicability and relevance of the approach would be very helpful. The paper’s strength is in the data and findings, but that can be obscured without stronger methodological description of the analytic approach, in which the authors appear quite well-versed and should be able to convey simply.

On p 9, for the “results interpretation workshop”, it is nice to know that two documents were provided to facilitate future interaction between the organizations. Some further explanation of the relevance of the workshop to the findings of this paper would be helpful.
Results

The authors conflate “organization” with “facility”; it is important to distinguish between these, as some organizations undoubtedly operate multiple facilities (which should be described in the paper; as it is, there appear to be 25 and 26 facilities in the study but it is not clear).

The use of medians (p 10) implies a right-skew in the distribution, which should be described (perhaps the Kruskal-Wallis test?). The main outlier facility appears to be the government hospital; analysis with and without outliers should be conducted and described, and a justification for presenting data with or without the outliers should be provided.

In the data for Table 3, several questions appear that must be resolved:

• Values for in-degree and out-degree referrals are identical, but that appears to be a bad result that requires checking the analysis.
• Mean (or total?) degree is simply the sum of in- and out-degree referrals?
• How are in-, out- and mean-/total-degree referrals useful for researchers and the organizations? Further discussion on this is required.

Under referral patterns (p 11), the authors should check the use of the term “likelihood” as that has a specific statistical meaning; perhaps “probability” or “tendency” was meant?

Figure 1 is inadequately described. Does it merely consolidate raw data into graphic clusters by organization type? What do the positions of clusters and distances between them imply?

Too much text is spent describing specific instances (such as in the paragraphs at the top of page 12). Instead, it would be preferable to describe the distributions overall and within sub-cities, and classify outliers by certain characteristics.

Discussion

As in the background, the authors undermine the credibility of their report by claiming too few publications address HIV/AIDS-family planning service integration/coordination/referral. Also, the claim that only US-based networks are available for comparison suggests a superficial literature review (especially because those reports are limited to the current first author). A broader review is necessary, and findings from other reports must be weighed against the current findings, not simply presented as stand-alone data as on page 13.

Apart from broader review of other findings, the discussion can be more closely linked to the actual data and findings of this report.

The presentation of limitations should consider the methodological points and potential biases mentioned above.

The study focused on facilities, not patients. Under the conclusion, therefore, statements about women, or patients in general, must be defined more carefully. While such extrapolation follows expectations, there are no data in this paper, or its citations, to support the statements, thereby undermining the credibility of other, better supported findings. More can be said about findings at the
organizational and facility levels, however, based on the data.

References

As noted, the references are very weak for this paper. 8 of 21 references are for statistics and methods, including software packages. Only 5 concern field reports, from 4 sites in the US, UK and Australia. Two of these reports share authorship with the current researchers and another 2 are not even included in the discussion.

A much broader, relevant and rigorous literature review is required.

Figure 2: the organization of the various types of facilities may be reoriented to facilitate side-by-side comparison of the two sub-cities.

Discretionary revisions

Analysis can be done by category of service referred, either HIV/AIDS or family planning, to distinguish more interactions among the facilities that offer both services. Some sense about an optimal level of referrals, in some or all the key variables in Table 3, would be helpful as a reference point. The stratification by sub-city does not reveal much in a research perspective; pooled analysis of all 51 facilities should also be conducted. There is not statistical testing here; that should be explained in the methods section, along with the fuller background on network analysis.

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