Reviewer’s report

Title: Gender-based distributional skewness of the Tanzania’s health workforce cadres: A cross-sectional health facility survey

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Reviewer: Neeru Gupta

Reviewer’s report:

I appreciate and applaud the authors’ efforts to highlight the issue of gender in the global health workforce research agenda, particularly in the context of an African country facing acute HRH shortage. However, I have important reservations concerning the way this manuscript is written. I recommend four areas where the manuscript should be revised/strengthened before it may be considered for publication consideration: (i) better appreciation of the Tanzanian context; (ii) choice of statistical methods; (iii) discussion of strengths/limitations of the data source; and (iv) clarity of value added to the global literature from the research results.

-- (I) BETTER APPRECIATION OF THE TANZANIAN CONTEXT. The Background and Discussion sections are rather generic, drawing largely on the global health literature, and offer little to help the reader better understand the Tanzanian context with regard to gender and health workforce. For example, what is the extent of geographical imbalance of the workforce in Tanzania? Is there evidence of gender imbalance in previous HRH studies in this context? How does gender imbalance act in concert with geographic imbalance? Is access to female providers a significant cultural issue among Tanzanian women? The paper’s introductory section should include clear statements of the specific objectives/hypotheses of the study and its innovative contributions to the evidence base; and the discussion needs to better focus on how the study objectives were met (or not), and relevance of the present results to decision-makers and health planners in the national context (beyond mere superficial mention that the workforce is gender-skewed) and other researchers (e.g. how might the research approach be improved/adapted in other contexts).

-- (II) CHOICE OF STATISTICAL METHODS. I found the use of the multivariate regression models confusing and the rationale for their use lacking. In particular, educational attainment is usually a pre-requisite for engagement as a health professional, so what is the added value of modeling its predictive value? More importantly, have the basic statistical assumptions of normally distributed variables and heteroscedasticity been met? How have the researchers addressed potential endogeneity bias?

-- (III) DISCUSSION OF STRENGTHS/LIMITATIONS OF THE DATA SOURCE. The authors note that the survey was meant to provide “nationally representative statistics of the country’s health workforce”, but then continue that the survey
only captured facilities providing EMOC services. Are these facilities truly representatives of all types of service delivery points in the country? Are facility-based workers truly representative all trained/qualified health professionals in the country, e.g. are they expected to share the same characteristics as those in management positions, who have emigrated, who are unemployed, or who are working outside the health sector (attrition)? What is the advantage of this particular facility-based source over other potential sources (e.g. administrative sources, or population-based census with occupation variables)? The authors themselves note there are many dimensions of healthcare practice; does the data source enable examination of variables such as “patient mix” and “patient selectivity” that would provide more insight into identified areas of interest in gender-oriented research?

-- (IV) CLARITY OF VALUE ADDED TO THE GLOBAL LITERATURE FROM THE RESEARCH RESULTS. The statement in the Conclusion, “it is not a surprise to find proportionately more female workers in the more laborious jobs at HF or managerial levels than the male staff cadre”, does not seem to be substantiated from the actual discussion/results of this study. What were the parameters used to define “laborious” versus “managerial” (I only see definitions of job titles in the analysis, not job tasks)? Does not the very use of a descriptor such as “male cadre” reinforce the underlying perceptions contributing to gender imbalance that the authors seem to otherwise indicate a hindrance to health system strengthening? Moreover, if indeed “it is not a surprise”, then what is the value added of this study to the global HRH community? How do the results for Tanzania compared with indicators of gender imbalance in other countries? What are the lessons learned from this study?

I BELIEVE THESE ISSUES AND QUESTIONS NEED TO BE ADDRESSED AS MAJOR COMPULSORY REVISIONS PRIOR TO DECISION ON PUBLICATION.

Other recommended edits:
-- The use of acronyms in the abstract should be avoided (e.g. “MCHA/MA” which has not yet been spelled out).
-- In the Discussion, the identified reference 17, seemingly a news release on a consultant’s voiced opinion, is clearly insufficient to substantiate the statement “It remains a fact that a strong health system requires a strong, adequate and balanced workforce to ensure effective and efficient delivery of health services.” Moreover, such statements should be focused on the issue at hand, i.e. gender imbalance (backed by sound evidence), rather than generic in nature.
-- On the last page of the main text, the identified reference 20 is old (dated from 1998); surely the authors can cite more recent global estimates.

Level of interest: An article of limited interest

Quality of written English: Acceptable
**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.