

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Condom Use during Commercial Sex among Clients of Hijra Sex Workers in Karachi, Pakistan
AUTHORS	Siddiqui, Anees; Qian, Han; Altaf, Arshad; Cassell, Holly; Shah, Sharaf; Vermund, Sten

VERSION 1 - REVIEW

REVIEWER	<p>Peter Weatherburn Senior Lecturer, Sexual Health and HIV London School of Hygiene and Tropical Medicine 15-17 Tavistock Place London. WC1H 9SH United Kingdom T: +44-20-7927 2797 F: +44-20-7927 2701</p> <p>I have no competing interests.</p>
REVIEW RETURNED	27-May-2011

THE STUDY	The Abstract states the Objective as "To describe the prevalence and predictors of condom use and sexual risk in male clients of Hijra sex workers (HSWs) in Karachi, Pakistan". However, the Key Message section emphasises that it is feasible to access hard-to-reach populations and "intervention program should also focus (on HSW) clients". The two hypotheses (page 7) return to the determinants of condom use. The Discussion addresses all these issues with no obvious prioritisation. This variation in the research question and the key outcome measures leads to a lack of clarity on the purpose of the paper - is it a feasibility study (proof of method) with some interesting (though not surprising findings)?
RESULTS & CONCLUSIONS	<p>See comment about about the key purpose of the paper - what is it?</p> <p>There are also mixed messages about HIV knowledge - key results appear in the discussion and in table 2, where no statistical effect is observed. Yet the key message concerning HSW clients suggests educational interventions should also focus on clients.</p> <p>Also no interpretation (no referencing) occurs in relation to the significant impact of education on condom use. This merits some discussion.</p>
REPORTING & ETHICS	While verbal consent was sought, there is no mention of Ethics Committee approval that I can see.
GENERAL COMMENTS	In any amendment please standardise the description "the last commercial sex act" and remove reference to "proximate sex act".

REVIEWER	Sarah Hawkes Reader
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	Institute of Global Health University College London
	No competing interests
REVIEW RETURNED	01-Jun-2011

THE STUDY	<p>The study included a reasonably large incentive for participation (Rs 500); given this (which is perhaps unavoidable in this setting), the authors need to acknowledge the potential for bias (and even possible collusion) that this may raise.</p> <p>There is a noticeable lack of description of key explanatory variables. What is a "regular" or "casual" client? If a man buys sex twice from the same HSW is he now regular or casual? What about five times? The lack of explicit definition makes interpretation difficult.</p> <p>The references are not complete (e.g. 1 and 2) nor a proper reference (e.g. 15). Major paper from Lancet (Rajabani et al) is missing from references - this would have been a valuable guide to context.</p> <p>Numbers and estimates of prevalence among HSW seem to have been selectively gathered from the existing literature. Suggest that the authors look again at the published epidemiology (peer reviewed and sentinel) from Pakistan.</p>
RESULTS & CONCLUSIONS	Clarity of results (and validity of conclusions) would be enhanced by more specific definitions of variables (as noted above) plus a more nuanced discussion of possible sources of bias.
REPORTING & ETHICS	No mention of approval by ethics board (IRB) seems to be included. Oral consent for participation is mentioned by no details given of how this was recorded for each participant (even oral consent leaves a paper trail).

VERSION 1 – AUTHOR RESPONSE

Comments by Peter Weatherburn

- The Abstract states the Objective as "To describe the prevalence and predictors of condom use and sexual risk in male clients of Hirja sex workers (HSWs) in Karachi, Pakistan". However, the Key Message section emphasises that it is feasible to access hard-to-reach populations and "intervention program should also focus (on HSW) clients". The two hypotheses (page 7) return to the determinants of condom use. The Discussion addresses all these issues with no obvious prioritisation. This variation in the research question and the key outcome measures leads to a lack of clarity on the purpose of the paper - is it a feasibility study (proof of method) with some interesting (though not surprising findings)?

Changes have been made in light of above observation, in abstract and in discussion part.

- There are also mixed messages about HIV knowledge - key results appear in the discussion and in table 2, where no statistical effect is observed. Yet the key message concerning HSW clients suggests educational interventions should also focus on clients.

Changes have been made in light of above observation in the abstract.

- Also no interpretation (no referencing) occurs in relation to the significant impact of education on condom use. This merits some discussion.

- While verbal consent was sought, there is no mention of Ethics Committee approval that I can see. The issue has been addressed by incorporating statement about IRB in methods section.

- In any amendment please standardise the description "the last commercial sex act" and remove reference to "proximate sex act".

As per suggestion this term has been standardised.

Comments of Sarah Hawkes

- The study included a reasonably large incentive for participation (Rs 500); given this (which is perhaps unavoidable in this setting), the authors need to acknowledge the potential for bias (and even possible collusion) that this may raise.

The issue of compensation has been highlighted in the paragraph addressing.

There is a noticeable lack of description of key explanatory variables. What is a "regular" or "casual" client? If a man buys sex twice from the same HSW is he now regular or casual? What about five times? The lack of explicit definition makes interpretation difficult.

Case definitions of "regular" and "casual" have been added in the methods section.

- The references are not complete (e.g. 1 and 2) nor a proper reference (e.g. 15). Major paper from Lancet (Rajabani et al) is missing from references - this would have been a valuable guide to context. The references have been completed as per reviewer's suggestion including Rajabali et al.
- Numbers and estimates of prevalence among HSW seem to have been selectively gathered from the existing literature. Suggest that the authors look again at the published epidemiology (peer reviewed and sentinel) from Pakistan.

We have referenced at least two published articles from peer reviewed journals and external reports (Second Generation Surveillance, National AIDS Control Program). We realize there is paucity of data/literature on this particular issue however if the reviewer can elaborate a bit more we will certainly make that revision in the manuscript.

- Clarity of results (and validity of conclusions) would be enhanced by more specific definitions of variables (as noted above) plus a more nuanced discussion of possible sources of bias. To make variable clear case definitions added in the methods section.
- No mention of approval by ethics board (IRB) seems to be included. Oral consent for participation is mentioned by no details given of how this was recorded for each participant (even oral consent leaves a paper trail). Statement about IRB incorporated in method section.

Anees Siddiqui, MBBS, MPH

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VERSION 2 - REVIEW

REVIEWER	<i>Sarah Hawkes</i>
REVIEW RETURNED	22-Jul-2011

GENERAL COMMENTS	Reviewer completed checklist only. No further comments were made
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REVIEWER	<i>Peter Weatherburn</i>
REVIEW RETURNED	16-Aug-2011

THE STUDY	The standard of written English is adequate but some amendment will be necessary at the editorial stage.
GENERAL COMMENTS	I have four suggestions for amendments, all of which will strengthen the manuscript prior to publication.

	<p>On page 9 the statement "73% were migrants" requires clarification. I assume that these are men not born in Karachi, rather than men not born in Pakistan. Please clarify that on this definition migrancy is within Pakistan, rather from another country of birth.</p> <p>The opening sentence on page 10, starting "The last HSW client was a regular client for 44% of participants..." This sentence reads as if the participants in the survey were the HSW and not their clients. It is confusing to the reader and requires revision.</p> <p>I assume that the authors did not ask whether anal intercourse occurred in the last HSW encounter for each client surveyed, but assumed that it always did. It seems reasonable to assume that some of the clients will not have used a condom (or lubricant) in their last HSW encounter because they did not have anal intercourse. This is a major limitation and must be addressed.</p> <p>Similarly in Table 1, the "Type of lubricant used" variable adds to 186 participants and not 203. I assume 17 men reported using no lubricant in their last encounter. This should be reported and all percentages recalculated to a baseline of 203.</p>
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VERSION 2 – AUTHOR RESPONSE

Dear Mr. Sands,

Thanks for your voluble comments, I have gone through the manuscript and tables as per your and reviewers suggestions and following are the responses for each comments.

Comments by Peter Weatherburn

- On page 9 the statement "73% were migrants" requires clarification. I assume that these are men not born in Karachi, rather than men not born in Pakistan. Please clarify that on this definition migrancy is within Pakistan, rather from another country of birth.

As per suggestion changes have been made.

- The opening sentence on page 10, starting "The last HSW client was a regular client for 44% of participants..." This sentence reads as if the participants in the survey were the HSW and not their clients. It is confusing to the reader and requires revision.

The sentence has been re phrased to make it more clear and understanding.

- I assume that the authors did not ask whether anal intercourse occurred in the last HSW encounter for each client surveyed, but assumed that it always did. It seems reasonable to assume that some of the clients will not have used a condom (or lubricant) in their last HSW encounter because they did not have anal intercourse. This is a major limitation and must be addressed.

This assumption is not correct, in fact every study subject was specifically asked for type of intercourse (sex), use of condom and lubricant.

- Similarly in Table 1, the "Type of lubricant used" variable adds to 186 participants and not 203. I assume 17 men reported using no lubricant in their last encounter. This should be reported and all percentages recalculated to a baseline of 203.

The changes have been incorporated in Table 1.

From BMJ Open:

- Please amend the contributorship statement. All authors should meet the ICMJE criteria and it should be explained how they do so. At present it reads like there is one author 'Prof. Sten H, Vermund and Dr. Han-Zhu helped me in designing the study'. See http://www.icmje.org/ethical_1author.html.

Contributorship statement revised to meet the BMJ Open requirements.

- Methods: please state that 'The study was given ethics approval by ...' rather than 'The study was ethically approved ...'.

Change made in method section to address this observation.

I will be happy to answer any further queries related to this manuscript.