Author’s response to reviews

Title: Are family planning vouchers effective in increasing use, improving equity and reaching the underserved? An evaluation of a voucher program in Pakistan

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Attached in the main submission as a separate file in a table format.

Copying here as well.

Response to round 2 of reviewer comments for submitted manuscript titled:

Are family planning vouchers effective in increasing use and reaching the underserved? An evaluation of a voucher program in Pakistan

Mellissa Withers (Reviewer 1): I feel that the authors' have done an adequate job of responding in detail to the reviewers' comments. I feel that this paper is now worthy of publication and would be of interest to the journal's audience.

Author comments

Thank you.

Ilene S Speizer (Reviewer 2)

Query: While the authors are resistant to showing the data at endline separated out by voucher and non-voucher sample in the intervention area, this descriptive information would strengthen the presentation at times.
Because, as indicated by the authors, 99% of the voucher clients were users and IUD was the most common method adopted by voucher clients, the intervention area data will, by default, have higher modern method use; this makes it hard to compare across the two areas and over time.

Further, for the discontinuation analyses it would be good to at least show the results by time period to make the point that there are improvements over time in continuation in the intervention area. The sample size might be too small to do this by voucher and non-voucher group but the voucher group should be large enough to merit its own column in the discontinuation table (at endline). The switching information would be particularly salient at endline for the voucher group since the intervention offered the opportunity to switch. This would strengthen the presentation of how the program was useful.

Response:

a) The authors are not resistant in this regards, however the authors feels that this is not related to the design of the study and does not fulfil any of the stated objectives. It is not useful in our view to compare results between the general population in the intervention arm and the control arm as it will not yield information of significance to study objectives. The results of the intervention arm are being compared with the control arm as a whole.

b) That is the essence of the intervention area. In any intervention study it is expected that the intervention group/arm would yield some effect. That is the main thrust of the hypothesis is that vouchers are an effective mechanism to enhance uptake of contraceptives. The results supports our hypothesis.

c) This is a good suggestion. The authors have already pointed out in the response to the first round of comments that the authors have only recently completed a follow-up study exclusively for the voucher clients. The protocol for the study has recently been published and analysis/report are being prepared Reference: Ali M, Azmat SK and Hamza HB. Assessment of modern contraceptives continuation, switching and discontinuation among clients in Pakistan: study protocol of 24-months post family planning voucher intervention follow up. BMC Health Services Research. 2018 May 11; 18 (1):359. doi: 10.1186/s12913-018-3156-0.

The results of above-mentioned study will be more meaningful as it provides baseline information for voucher clients for comparison and observing improvements, if any, over time.
Query: The authors still do not address the problem of overlapping sampling frames. The explanation provided by the authors that the frames do not overlap is not a complete explanation since the removal of the households where voucher clients resided means removing potential non-users as well. The discussion of weights that is added provides the adjustment to their sample based on the stratification (and can be dropped). It does not address the sampling challenge that comes from the sampling frames overlapping; at a minimum the bias introduced with overlapping sampling frames should be addressed as a limitation in the paper if the authors are unable to adjust the sampling weights accordingly.

Response:

The authors respectfully disagree with this comment. This is not an overlapping sampling frame situation.

This refers to the selection of a respondent from within a household. Removal of potential non-users from households where voucher clients resided would be based on the assumption that there were more than one eligible respondents in households where a voucher client resided.

Even in those instances where this were to be the case, since only one respondent was randomly selected from a sampled household the chance of selecting a non-user or a user would be the same.

Thus in the sampling frame of the households without voucher clients there would be potential users as well as non-users. Having a separate sampling frame for households with voucher clients does not affect the selection of users/ non-users from the household without voucher clients.

Query: Details of the intervention - while there are four bullets on the intervention and the reader is referred to another article about what it included, for this paper it is important to know who was targeted - all households or poor households; and whether ALL households in the intervention area were reached/eligible. This is relevant for this paper trying to evaluate the program.

Response:

Thank you for this advice. Unfortunately, due to the word count of the journal, we are unable to include the complete details.
However, we have now expanded/revised the intervention details in this new version of revised manuscript in order to provide the reviewer more insights (Pg.5 Lines 7-26).

The reason to publish a separate protocol paper fulfils the desire of the evaluators, future researcher and epidemiologists to read the entire design and intervention in detail so that the word count can be curtailed for the findings reporting manuscript. I hope the esteemed reviewer will also agree with the above justification as to add more content is beyond our scope and it is editor’s request to keep the word count to journal’s criteria. Thank you.

Comments on the results section

Query: The first paragraph only describes the endline sample. This is characteristic of the entire results section that is very minimalist with few details on what is in the tables.

Response:

Thank you for pointing this out. The authors also have to keep into consideration the length of the paper.

There is a lengthy discussion section which in the authors’ view is more useful in this context. We have expanded on the sampling details on the page 6 (line 13-19) that was done in the previous round of comments.

Query:

Demographic characteristics - drop marital status from first sub-heading since there is only one sentence and it is about age - all women appear to be in union - perhaps describe more details of the sample, including differences between the intervention and control areas from table 2 in this paragraph (ideally you could mention in the text p-values if things are significantly different between the groups).

Response:

- As suggested, authors have dropped marital status from first sub-heading

- It is a survey of married women of reproductive age (MWRA).
Query:

Table 3 - I still think it would be helpful to see descriptively (since it can't be done in MV analysis) what the prevalence of CPR is for the endline/non-voucher and endline/voucher groups to see how the endline/non-voucher group compares to baseline intervention group. Perhaps provide the values in the text for the reader that questions the bias of the sampling strategy like this reviewer. The large increase in IUD use was contributed by the voucher clients which is good and would explain the lower discontinuation as well. The details on the voucher clients use and methods of choice comes up later on page 10 (targeting voucher clients) and would be useful to present earlier to inform this presentation.

Response:

Thank you for the suggestion. The authors have addressed this issue previously in the first round of comments and believes that in line with the objectives of the study, it will be best to present the results as described in the manuscript to ensure a flow in the manuscript so that the readers can follow it up as per the stated objectives of the manuscript.

Query: The discussion of Table 4 on awareness is biased as it only points out the significant increase in knowledge of pills, injections, and IUDs in intervention areas whereas there were significant increases in the control area as well. Further, at endline, the control area had higher awareness than the intervention area.

Response:

Thank you for highlighting the point. A statement has been added at line 15 page 10

Please do note, however, that we had already written … “At end line, awareness of methods was significantly higher in both intervention and in control areas” (lines 11-12, page 10).

Query: "Targeting voucher clients" - the first sentence "as mentioned in the methods section…” This is not actually mentioned in the methods section and again the reader is referred to another paper which leaves the reader hanging. This would be the place to indicate that 99% of voucher clients were using a method followed by the method mix that is given (IUD and condom). Ideally, this information would come earlier in the results section since it is related to all the results that follow.
Response:

Thank you for highlighting the error. The statement has been edited (line 15, page 11)

Statement added (line 17-18, page 11)

Query: The statement (right before reference to Table 6): "The data also indicates that compared to their affluent counterpart clients the odds for better contraceptive knowledge for any method, ever use of any method and modern method use was higher among the lowest two quintiles." This is confusing since it states "clients" but I think this is based on the DID analyses that uses the full baseline and endline samples. This statement should also say that this is from multivariate analyses. Again, this is an example of a minimalist approach to writing. Could time (baseline vs. endline) be put into this analysis?

Response:

Thank you for highlighting the error. It was not clear and has been edited. This is basically from the multivariate analysis and has been edited accordingly to reflect information from table 6. (lines 21-29, page 11)

Query: Equity analysis - perhaps give a little more detail in the text. For example, not only is the overall intervention group higher than the control group, but this is true for the poorest quintile. That said, the statement is not necessarily true for the richest quintile in comparing intervention and control groups.

At the bottom of Table 7, it says "antenatal care from skill birth attendants"; this needs to be fixed. Is there any reason why the concentration index analyses are not done separately by baseline and endline?

Response:

Thank you for highlighting the point. Detailed has been added in text. Please see (from lines 13-23, page 12)

In reference to the second part of your question, we wanted to adjust baseline and endline to ascertain the true effect of the intervention.
Query: Tables: Tables 5, 6 and 7 - the title should clarify which data are being used in these tables/analyses. I had to do the math to figure out that Table 5 was baseline and endline jointly.

Response: Information added to titles as suggested.