Author's response to reviews

Title: Increasing safer sexual behavior among male-to-female transgenders in Laos through an integrated social marketing approach

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Author's response to reviews:

To the editors:

Please accept this revised submission of our manuscript. Below please find a list of the changes that were made in response to the reviewers’ comments (in italics).

Many apologies for the delays from our side; several of the authors on the project have moved on to other companies and we had trouble getting ahold of them to answer all the reviewers’ queries.

Thank you for your consideration.

Best,

Megan Kays

Reviewer #1:

Major revisions:
The data are from 2004 and 2006. Why is this paper send to BMCph in 2011?
Unfortunately, the process of publication is slow at PSI. However, we still felt that these data, though dated, are important about an underserved population.

Design: this is a national campaign, including health communications and availability of condoms; which is fine. But, it is almost impossible to evaluate national campaigns because there is no comparison group. The only serious alternative is a longitudinal study with many measurement points over time and changes in data related to changes in campaign activities. Two measurement points is not enough. There is no way to tell if the differences are related to the campaign. Correlations with exposure are not convincing because exposure may or may not be the cause or the effect of the campaign. This is not devaluating the authors’ work; it summarizes just the facts about the difficulties in evaluating
national campaigns. I don’t think the authors had a better alternative. We recognize that this is one of the shortcomings of the absence of a control/comparison group, which was not possible. This shortcoming is acknowledged in the text, though we feel that the evidence suggests impact.

One thing that is extremely important is that the data gathering at T2 is identical to earlier data gathering at T1. The authors are not very outspoken about this. Do they have convincing arguments for both measurements being identical as far as possible?

We have included some additional information about the process of data collection; though there were small differences in approach, by and large the methodology was the same and the populations similar.

There are huge differences between the two data sets, some positive, some negative. And the negative changes are surprisingly large for some items. Moreover, there are some completely illogical combinations, for example using water-based lubricants more while at the same time the knowledge about water-based lubricants has almost diminished. I’m sorry to say that the results seem to be almost random. My impression is that the authors paint a more positive picture of the effects of their campaign than is warranted. We have noted where the findings seem contradictory, though we don’t feel that they are random.

We need to know more about transgender and HIV prevention. The authors have interesting data. I would suggest to present those data more as two surveys with a lot of useful information and not so much as an evaluation study. We have maintained the approach as evaluation, but feel that this does not diminish the ability of the findings to tell a story about transgender behavior, as well, as these data are included.

Reviewer #2:
1. Is the question posed by the authors well defined?
   The question is unclear, sometimes simplistic explanations work best to answer a question and would be clearer in the Introduction section, then answered in the discussion with additional references to literature and findings. We sought to improve the clarity of the research question, which hopefully is elucidated better in both the introduction and discussion.

2. Are the methods appropriate and well described?
   I am unclear with the methodology. Quantitative surveys were ‘said’ to be administered but it appears from the description, these surveys were not
anonymous, nor independently completed by the participant. The method used was a more structured interview, conducted by a ‘peer’ and in some cases in the presence of others (11.3%). There appears no means of assuring that a subject was not approached twice at different venues by different field workers. The survey was pre-tested by 24 respondents; however the outcome was not reported. Why were pre test sites not included?? I do not see the rationale behind this?
We included additional information on the methodology.
There was no explanation for an increase in sites in 2006? An explanation is now included.

3. Are the data sound?
Was the research study approved by a structured Ethics committee? The method in which the data was collected needs to be considered, especially entry criteria.
I am concerned that nearly half of both cohorts were aged 15-20 years. I would be interested to know how many subjects actually fell into the 15 year old age group and the ages of their male sexual partners as this touches on ephebophilia issues. The sexual preference of older men for pubescent and adolescent boys who look like girls could indeed effect sexual behavior?
The age of consent in Laos is 15 y/o, however does this warrant inclusion in research studies? In most Western countries the age most Ethics committees agree upon is 18 y/o, an age whereby there is some understanding of the research process being undertaken as well as informed consent. As such this study could not be compared to studies with an older age entry criteria.
It would be interesting to see if exclusion of 15y/o would yield different results as these ‘youngsters’ could have been ‘influenced’ and even coerced by an authoritative figure, even though seen as a ‘peer’ as mentioned in the limitations.
Youth were included in this study because they fall under the age of consent and because prior qualitative work suggested that they were already engaging in risky behaviors at that age. Thus, we did not want to exclude them, as they present an important group at risk. This study was approved by agencies in Laos with the understanding that youth would be included in the sample. If other authors are interested in comparing our dataset with those older than 18, we can provide the raw data and they can limit the sample to exclude those under 18.

There was not one report of sex work in the occupation data which is unexpected
Sex work was not a major focus of this study as they did engage in some transactional sex but formalized sex work was not a major theme that emerged from the qualitative work. Future studies should examine this, but this study focused much more on regular and casual partners rather than paid partners.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

The analysis section is not clear to me, however I am not adequately qualified to assess the statistics. I will make general comments.

The section “Measures and Data Collection” 3rd paragraph makes reference to both surveys and questionnaires, this should be standardized throughout the paper.

The word questionnaires is used to mean the actual question guide only. Survey is used more generally to refer to the quantitative study.

The tables used for results were confusing. In Table 2, rather than use asterisks to show significance it would have been more appropriate to show a p value in the Sig. column for all variables. Were the Chi-square (#2), degrees of freedom (df) performed? Authors should consider reporting these and p-values in all tables for consistency.

P-values were included. This analysis did not focus on chi-squares.

In the exposure category, how was n.s identified when there was no comparative data in 2004? This was changed to n/a.

A simplified analysis may have been more appropriate, the data should speak for itself in the text with reference to the tables and not be a major effort to read and understand. The results section is confusing. A focus on only significant variables may be warranted.

We reduced the length of the results to focus on the significant variables.

Table 3 was too complicated with too many comparisons which when reading the footnotes did not make sense.

We sought to clarify the footnotes so that the table would be easier to read.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

The discussion would benefit with additional references to literature and actual
research findings. As is, there appears to be omissions of supporting data for some of the general statements made.

We have sought to include more evidence to support our assertions, when possible, given the limited evidence base on transgender HIV prevention in Asia.

6. Are limitations of the work clearly stated?
Limitations are mentioned but should be highlighted by a subheading as it is an important issue.
We have subheaded the limitations.

Age of subjects should be highlighted especially if many are 15 year olds.
We have highlighted this.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Literature review could have included a broader search to include Western countries where transgender issues have been widely researched.
There appeared to be limited peer reviewed citations.
We specifically chose not to include literature from non-Asian countries, as we feel that gender constructs and transgender identity and behaviors vary widely from region to region.

8. Do the title and abstract accurately convey what has been found?
Not sure if findings accurately reflected the title and abstract, The conclusion could be more cohesive with reference to past literature and results from the research.
We made changes to the conclusion to attempt to make it more clear and cohesive, changing wording and referencing literature and the research.

9. Is the writing acceptable?
The writing is complex in parts for a simple study and not an easy read, there appears an attempt to over report to make justifications.
We attempted to simplify the results and conclusions and not overstate the findings.

Reviewer #3:
The success of an integrated approach at increasing safer sexual behavior among transgendered men in Laos: findings from a multi-round survey

General comments:
This paper presents findings from the evaluation of an HIV prevention intervention with transgender persons in Laos. The paper addresses an
understudied area within HIV prevention work and presents interesting and relevant findings for the field. The paper has three main limitations. First, there is a lack of clarity and consistency in the definition of the study population. Second, the paper lacks a clear definition of the argument the authors are making to guide the presentation of results. What is the story they are trying to tell with these data? This is not coming out sufficiently in the current form. Third, the paper would be strengthened by expanding the consideration of both empirical and theoretical insights to interpret the data. Please see comments below by section that will hopefully be useful to the authors in their revisions.

We have attempted to improve the clarity of the study population, and to make the definition of the paper clearer. We have also included more empirical evidence to support our conclusions, though we were limited by the availability of literature.

Major Compulsory Revisions

1. Definition of the study population:
The authors use the term “transgendered men”, which is not clearly defined and creates confusion in the interpretation of data. First, the term “transgender” generally appears to be the preferred term over “transgendered”. According to GLAAD, transgender should be used as an adjective, not a noun (i.e. transgender person, transgender individual). This is a debated topic but the term that appears to be considered the most respectable is “transgender” as an adjective used with “individual”, “person” or “people”.

The second point of confusion is the use of “men”. If the population is transgender, the authors should reconsider referring to it as “men”. Does this refer to male-to-female transgender persons? This should be clarified and used consistently.

It is also not clear what the authors mean on page 3 when they state: ‘On the basis of their biological behavior”. What does biological behavior mean?

Finally, on page 11 the authors use the term “Transgender MW” and “Transgendered MM”. The introduction of new terms adds further complexity to the understanding of the study population. With regard to Transgender MM, defined as biological males who identify as female, but in appearance are male, who determines that their appearance is male? This requires further explanation if it is going to be used as a meaningful category of analysis.

For this, we have changed all the terminology to be consistent and accurately reflect the study population through the use of the term “male-to-female
transgender” meaning a person who was born as a biological male but self-identifies as female, regardless of physical appearance.

2. Purpose of the paper
The purpose of the paper needs to be more clearly and succinctly stated in the introduction and needs to recognize the vulnerability and human rights of transgender persons in Laos, not just their potential as “bridges” in HIV transmission. The current introduction emphasizes the bridging role without discussing why we should care about transgender persons as human beings. We have acknowledged the importance of this population in its own right, and not solely as bridging population.

Recognition of the stigma and discrimination experienced by this population is made on page 24 in the Conclusions section; the paper would be strengthened by incorporating this recognition into the introduction and using it to frame the significance of this work.

We have earlier acknowledged the importance of stigma in framing transgender issues.

The Discussion section starts with 2 clearly stated objectives. These should be stated in the introduction as well and the methods should be revised to explain to the reader how the data presented responds to those two objectives.

We sought to revise the paper to include these objectives.

The abstract should also be reviewed to clearly state the objectives and purpose of the paper (and perhaps to also be shortened, depending on journal specifications).

We have revised the abstract to reflect this change.

3. Background Section
Consider labeling this section “Introduction”.

Done.

Overall this section is long and requires revision to improve clarity and set-up the presentation of their data.

The authors may want to consider using sub-headings to organize the sections of the introduction to make it easier for the reader to follow.

Although we did not subhead it, we significantly reduced the length and sought to improve the organization and flow.

As mentioned earlier, the social vulnerability of transgender persons needs to be
recognized.
Done.

The description of previous studies with waria in Indonesia and transgender persons in Thailand is interesting but could be stated more succinctly. Page 4 includes substantial repetition.
We clarified this section to reduce repetition.

The authors go back and forth between using the terms “transgender” and “MSM”. In general, it would seem appropriate to frame points made in the paper around “transgender persons” and identify data that used the broader MSM term only when needed. For example, on page 4 the paragraph “Inconsistent condom use and other risk sexual behavior among MSM have been associated with poor HIV knowledge…”. This sentence should focus on transgender persons.
It is somewhat confusing to include data from the current study in the background section. Consider dropping.
We have modified the language to be clearer about referencing MSM or transgenders.

On page 5 the authors identify as a “major programmatic challenge” that many “Asian MSM” do not self-identify as gay or bisexual… Again, it would improve the clarity if the narrative focused on transgender persons. It is also not clear what the challenge is? The authors need to explain what specific challenges are created by the way men self-identify. How this impacts programming is not explicitly stated.
This was revised to more clearly explain that traditional programs targeting MSM may not resonate with transgenders, as they may not self-identify as MSM.

Page 6, consider dropping paragraph “Taken together…” to make the intro more succinct. It is overly descriptive and not clear what it contributes to the aim of the paper.
This was reduced.

Page 6, the authors indicate that their literature review indicated only 1 intervention with a transgender target audience. This paper should be cited. The authors may also want to review Bockting et al 2005 and De Santis et al 2010 (and possibly others) for additional examples of evaluations of transgender interventions that could provide relevant insights for the current study. The paper would be strengthened by a more comprehensive review of the transgender HIV
prevention intervention literature.

We made a specific choice not to include these types of papers as they focus on Western transgender populations and have very different programmatic approaches. Though there is limited Asian data to draw from, we did not feel comfortable extrapolating the experiences of transgenders in Miami to transgenders in Laos.

4. Methods

Theoretical Framework: while the authors indicate that the PSI framework is described in more detail elsewhere, it would aid in understanding their measures if they included a conceptual model/logic model for the current study, defining how they conceptualized and measured each construct (opportunity, ability, motivation).

We have included a longer, more detailed explanation of our theoretical framework.

Consider moving the description of the intervention to the Introduction section. It would also help to know how the intervention was designed. For example, what made them decide to use drop in centers? The authors should explain more how their formative work and other factors informed the intervention.

More information was included on the formative work and use of drop-in centers earlier in the article.

See earlier comments regarding the paragraph on page 11 classifying transgender participants at MM and MW.

Revised.

Why do the authors think that their multi-item scales were not reliable? This should be discussed (or alternatively, drop discussion of multi-item scales).

This discussion was dropped.

Did the authors ask about participant involvement in sex work or transactional sex? This would be important to mention as many transgender populations have high levels of involvement in sex work and/or transactional sex, which is one of the main drivers of HIV vulnerability can heighten their vulnerability to HIV.

Sex work was not a main focus of this study, nor was transactional sex. This is largely because the formative work revealed that this population did not consider itself as engaging in these behaviors on the whole, though money and gifts were exchanged. However, it is a limitation of this study, and should certainly be explored by future studies.

5. Results
Consider merging T1 and T2 together as they both present trend data pre-post. We did not merge these as they are separate rounds, but sought to present them more coherently.

Consider categorizing/summarizing socio-demographic variables for a more succinct presentation of data. Some of these were categorized.

Consider only including narrative on significant differences (and just keeping non-sig changes pre-post in the tables). We have reduced the results to focus on significant variables.

The Evaluation section is extremely long and would benefit from revision and cutting to read more like an article and less like a summary report. Referring back to the objective(s) to which this section responds may help to focus the presentation of data. One approach would be to simply list all changes that cannot be attributed to exposure to PSI and only elaborate on those that can be attributed. The authors have important and compelling data to present on how exposure was associated with HIV protective behaviors but the impact is lost in the length of the presentation. There appears to be enough rich findings to justify focusing on those that are most robust and significant. We have sought to do so.

6. Discussion
The discussion section requires more interpretation and contextualization of the data. In its current form, the Discussion includes extensive repetition of study findings that were already presented in the results narrative and tables without sufficient interpretation of those findings. The authors do not cite literature (empirical or theoretical) to interpret their data, which is a major limitation of the paper.

We included more literature; however, there was limited literature available on Asian TG to contextualize the paper.

The inclusion of data not shown on page 20 should be reviewed – these seem like potentially interesting findings that could be included in the paper? For the sake of length and a focused paper, we chose to exclude this.

7. Conclusion
Would be stronger as a 1 paragraph summary of most relevant findings and implications.
We have revised accordingly.