

## 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

<b>TITLE (PROVISIONAL)</b>	Internalised Homonegativity predicts HIV-Associated Risk Behavior in European Men who have Sex with Men in a 38-country cross-sectional study: Some Public Health Implications of Homophobia
<b>AUTHORS</b>	Ross, Michael; Berg, Rigmor; Schmidt, Axel; Hospers, Harm; Breviglieri, Michele; Furegato, Martina; Waetherburn, Peter

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Dr Lisa McDaid Programme Leader (Track) – Sexual Health MRC/CSO Social and Public Health Sciences Unit Medical Research Council, UK
<b>REVIEW RETURNED</b>	04-Oct-2012

<b>RESULTS &amp; CONCLUSIONS</b>	<p>The paper presents an interesting premise and it is great to see findings from EMIS being submitted for publication. However, I have a number of concerns and the paper is often difficult to follow and not well presented. I suggest the following revisions would improve the manuscript. I have four major concerns:</p> <ul style="list-style-type: none"><li>• The paper in general, and the results section in particular, are difficult to follow. You frequently jump between different concepts: IH, HIV testing and risk behaviours; and I would suggest clearer sequencing throughout would improve the manuscript. The results section is very difficult to follow and the tables and figures are not adequately described. I suggest you make major revisions to this section and ensure that you fully describe each finding and the content of each table and figure.</li><li>• Although EMIS is an excellent resource with a tremendous sample size, I worry about the combination of such a varied range of countries. First, the paper would benefit from greater description of the countries involved, the samples achieved, and, most essentially, how comparable they are. The current description in the text of table 1 – “Basic demographic data are presented in Table 1 from participants with valid IH data.” – is wholly inadequate. Second, the authors themselves note that the countries can be grouped into clusters (and note that here the full country names should be provided), but it was not clear to me how the clustering is reflected or accounted for in the analysis. This is partly because the description of the results is inadequate, but given the massive sample size, would it be better to present the results separately for each cluster? I would think this would also allow for a more</li></ul>
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	<p>considered discussion of the different HIV prevention needs of different areas.</p> <ul style="list-style-type: none"> <li>• Would recent HIV testing (i.e. in the previous 12 months) be a better outcome to consider? That is, if you had an HIV test 10 years ago, but have had significant sexual risk behaviour since then, having ever had an HIV test is not really a good indicator of health behaviour.</li> <li>• Public health implications – the name of the paper suggests this will be the focus of the manuscript, but the issues appear to be given only a sentence in the Discussion. I would suggest that this is given much greater attention, or that the title of the paper is changed.</li> </ul> <p>Minor revisions</p> <ul style="list-style-type: none"> <li>• A number of items in the Abstract are poorly defined (e.g. GINI coefficient, size of place settlement) and should be described in more detail.</li> <li>• There is considerable repetition in the Methods section that could be reduced.</li> <li>• Table 2 is introduced in the text before Table 1.</li> <li>• As noted above, the full country names should be given in the Results.</li> <li>• Limitations of the study, and of the Internet sample, should be discussed in more detail.</li> </ul>
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<b>REVIEWER</b>	Kristian Daneback, PhD Associate Professor University of Gothenburg Sweden
<b>REVIEW RETURNED</b>	11-Oct-2012

<b>THE STUDY</b>	<p>This is an interesting and relevant study of how (perceived) attitudes and policy toward non-heterosexual identity may be related to sexual health risks among gay men (and vary) across Europe. I am particularly impressed with the large sample in this study; it clearly shows the benefits of using the internet for data collection, not the least in hard to find populations where no sample frames are available (MSMs and Internet users). Analyses of large samples/subsamples always produce statistically significant results and the authors rightly refrain from reporting p-values. Probability values are very important when analyzing small samples where the risk of having obtained a skewed sample is likely and, thus, the risk taken when generalizing must be estimated by calculating the p-value. However, by using very large samples as in this study (up to infinite samples) the probability of having included all the variation possible in the sample increases. While this is of particular relevance for a prevalence study, it is of less importance in an association/epidemiological study where relationships between variables are the main interest and effect sizes of greater importance. This makes (multivariate) regression analyses a sound</p>
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	<p>choice as it allows for controlling for independent variables, but also cluster and SEM analyses.</p> <p>I had only few comments that hopefully can help improving the manuscript further. For example, I wonder if the aim and research questions could be made more explicit in the Introduction? It is somewhat clearer in the Abstract, but could be specified. I also wonder if it would be worthwhile giving the 95% C.I.s for the linear regression analysis presented in table 3. Especially since many of the coefficients seem to be close to 0 (zero).</p> <p>Minor comments: Is there a reason for using MSM and Gay Men in the title and not one or the other? I think that "liberal" should be italicized in the Results section (1st para, 5th line). Hatzenbuehler et al. (254) should probably be Hatzenbuehler et al. (25).</p>
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<b>REVIEWER</b>	<p>Frank Y. Wong, Ph.D.          Associate Professor          Dept of Behavioral Sciences and Health Education          Hubert Department of Global Health (Joint Appointment)          Center for AIDS Research          Rollins School of Public Health          Emory University</p>
<b>REVIEW RETURNED</b>	20-Oct-2012

<b>GENERAL COMMENTS</b>	<p>This study makes important contributions regarding the relationship of macro-social determinants such as legal environments pertaining homosexuality and/or same-sex behaviors and practices and meso- and micro-level indicators such as internalized homosexuality and their impact on HIV-related risks behaviors. A major assumption of the study is that such social macro-social determinants might shape internalized homophobia, an individual level attribute. To better align with this argument, research findings should be presented in the following order:</p> <ol style="list-style-type: none"> <li>1. Demographic (keep Table 1)</li> <li>2. Figure 2 becomes Figure 1 – here the argument is that macro-social determinants such as GINI Coefficient are related to internalized homophobia</li> <li>3. Table 2 (keep Table 2)</li> <li>4. Table 3 – re-run analysis by first entering meso- and micro-variables such as internalized homophobia followed by macro-social determinants (e.g., GINI Coefficients) on HIV-related risk behaviors. That is, macro-social determinants should still be accounting for significant variance.</li> <li>5. Figure 3 (keep)</li> </ol> <p>Is there a more appropriate citation than "Wikipedia?"</p>
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## VERSION 1 – AUTHOR RESPONSE

### Responses to Reviewers

The responses of the reviewers have been very helpful and resulted in a significant improvement in clarity in the paper. We were initially constrained by a 3,000 word limit which caused unnecessary brevity, and thank the Editor for raising that limit.

#### Reviewer 1:

We have heavily edited the paper, and the results section, and added in headings as well as moving around sections (reviewer 3 also commented on this). We have added additional explanation to the results section as you suggested, plus an additional analysis suggested by Reviewer 3.

The issue of the range of countries is indeed both a blessing and a curse. The number enables us to treat country as a variable and use data reduction methods (cluster analysis) as an approach. On the other hand, there is variability, but we are able to describe this variability on several simple continua (LGB discrimination, Gini coefficient, IH). Because IH and cluster are pretty co-linear (I have added in that graph to Figure 2) we are treating cluster as a measure of criterion validity for IH here (since IH is the variable of focus in the paper). Thank you for providing the stimulus to clarify our thinking about what the clusters are actually demonstrating.

We calculated the relationships between the binary HIV test result and the time-associated one (how long ago was the last HIV test). The correlation was .88 and the non-linear nature of the categories of time since the last HIV test made it more difficult to use that as a variable (especially with one time category as “never”). We believe that this probably warrants another paper with a much more focused look at HIV testing within several categorical time frames, rather than as one variable predicted by IH, which is the focus of the present paper. We appreciate the suggestion and will follow it through in a more in-depth analysis specifically of HIV testing.

You are correct about the title of the paper, and we have added another paragraph about possible interventions.

We added definitions in the abstract but these took us over the 300 word limit so we took them out again..

We have performed some reduction in the Methods section.

We have moved Table 1 up in the text.

Full country names are given in results.

Limitations of the Internet sample are discussed in more detail.

Reviewer 2: We agree with all the reviewer’s suggestions. We have changed the title as suggested, and added the 95% confidence intervals of B for all regressions (although as he notes, these have very small CIs given the sample size). We have also made the corrections that he has noted in the text.

Reviewer 3: We have added an additional Table. Now, Table 3 simply reports on the regressions used to compute path coefficients for Figure 3 (Paths associated with IH) which by necessity required simultaneous entry to function as path coefficients. Table 4 adds a stepwise regression as suggested to predict the associations of the variable Perceived Control over Sexual Risk Behavior, since that was unexpectedly an important emerging variable in Figure 2 for condom behavior. We took this reviewer’s suggestion about using stepwise analyses but decided to use stepwise entry of a combination of macro, meso and micro variables so as not to unduly constrain the entry using blocks, and to provide a better precision on the combination of multi-level variables. The results are exciting and we appreciate this reviewer’s suggestion! In the event, the Macro variables did emerge

early and as a block. We have also re-ordered the variables in the format that the Reviewer has suggested and believe that they are clearer now.

We have re-organized the paper in accordance with Reviewer 3's suggestions, and re-numbered the Tables and Figures as suggested, and we believe that it now flows better. We wish the legal data had a clearer reference than "Wikipedia" but have been unable to find one for all Europe! The Wikipedia reference contains the Table of Countries and is thus a data source, and as a data source with a Table of Laws by each country in the world (not just an opinion), and the only source with each country listed, we feel obliged to reference it.

We also thank the late Reviewer 4 for his useful critique on effect sizes, which we have responded to in the Discussion.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Dr Lisa McDaid Programme Leader - Sexual Health MRC/CSO Social & Public Health Sciences Unit Glasgow, UK  No competing interests.
<b>REVIEW RETURNED</b>	06-Dec-2012

<b>GENERAL COMMENTS</b>	The authors have clearly addressed all of the reviewers comments and the paper has been significantly improved. I suggest three additional changes. -The key messages should be rewritten to be more general and self-explanatory, avoiding the use of acronyms and technical terms. -The full title of the EMIS study should be given when it first appears in the text of the manuscript. -The start of the Discussion refers to a Swedish comparison of Internet and national and venue based samples. There are UK data that do the same and the paper would be strengthened by including these (for example, see Evans et al 2007).
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### VERSION 2 – AUTHOR RESPONSE

Thank you. Quite simply, I concur with all the suggestions of the reviewer and have made all these changes. I have introduced the full EMIS acronym the first time it appears in the text. I have re-written the Key Points getting rid of acronyms. I have added the Evans paper to the text and references.