

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Psychometric properties of the WHO Violence Against Women instrument in a male population-based sample in Sweden
AUTHORS	Nybergh, Lotta; Taft, Charles; Krantz, Gunilla

VERSION 1 - REVIEW

REVIEWER	Dr Holly Johnson Associate Professor Department of Criminology University of Ottawa Ottawa, Canada K1N 6N5 I declare that I have no competing interests.
REVIEW RETURNED	30-Sep-2012

GENERAL COMMENTS	<p>My comments are very minor. Overall, this is a well-written article about an important methodological question in intimate partner violence research: can instruments designed to measure women's experiences of partner violence be applied with equal validity to male populations.</p> <p>The Methods section should specify that it was a postal survey (this is in the abstract only).</p> <p>One suggested reference for review of literature regarding gender differences in IPV on page 4: Ansara & Hindin (2010) Exploring gender differences in the patterns of intimate partner violence in Canada: a latent class approach. <i>J Epidemiol Community Health</i>. 64:849-854.</p> <p>It would be good to know how representative the sample is according to the socio-demographic factors presented in Table 1 according to census or some other data source. Also, the response categories for Educational Level do not appear to be exhaustive, ie., what about other types of training and certification that is not acquired in university?</p> <p>Specify how the WHO guidelines for ethical conduct were addressed on a postal survey, eg, confidentiality, safety for respondents, minimizing and responding to emotional trauma.</p> <p>Additional limitations could be mentioned: (1) the sub-sample of respondents who answered both the VAWI and the NorAQ is relatively small; (2) violence experienced earlier in the lifetime may have been undercounted because of the structure of the questionnaire (this is mentioned on page 16 but merits mention as a limitation); and (3) question wording on the NorAQ may threaten the reliability of estimates of psychological violence, ie., "systematically"</p>
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	<p>and "for a longer period" may not consistently have the same meaning for all study participants, unless definitions were provided that are not shown in the article. Or, is it possible this is a reflection of the translation from Swedish to English?</p> <p>On page 19, there is a typo in the 3rd line of physical violence: "trashed" I think should be "thrashed".</p> <p>With respect to the companion article, "Psychometric properties of the WHO Violence Against Women instrument in a female population-based sample in Sweden" I also recommend it for publication. The suggestions above also apply to this article in addition to a few others:</p> <p>Is it a requirement for sample selection that respondents are currently in a relationship or have had a relationship in the past? This is not specified in the Methods section; however, on page 9 it states "the rest of the sample was single, widowed or divorced, but had previously been in a relationship". This should be clarified.</p> <p>Can you explain why the lifetime prevalence of IPV among the sub-sample in Table 5 is so different than the prevalence shown in Table 4? Is it an artefact of sampling?</p> <p>On page 6, 5th line of 2nd paragraph, "higher among men" should be "higher among women".</p>
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REVIEWER	<p>Dr. Edward Ko-ling Chan Associate Professor Department of Social Work and Social Administration The University of Hong Kong Pokfulam Road, Hong Kong</p> <p>I have no conflict of interest to the study.</p>
REVIEW RETURNED	01-Oct-2012

GENERAL COMMENTS	<p>The purpose of this manuscript is to explore the psychometric properties of the WHO's Violence Against Women instrument (VAWI) in a population sample of Swedish men. With regard to the limited number of research on the validation of violence assessment instruments in a sample of men, the manuscript has the potential to become an important piece of reference for those who would like to know more about it. The manuscript is generally well written. However, there are several problems and suggestions are summarized below that may strengthen the study conclusions.</p> <p>Introduction: Fine.</p> <p>Methods</p> <ol style="list-style-type: none"> 1. P.6, line 18: Citation of the companion paper is needed. 2. A brief description of the sampling method is needed. What was the sampling frame? Were they households? What sampling strategy was employed? Were there any inclusion and exclusion criteria? 3. Similarly, more information about the second data collection is needed. Were the respondents having similar demographic characteristics? 4. A brief comparison of the non-respondents and the final sample
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	<p>was conducted, as well as the excluded sample and the final sample. One may have a hypothesis that these excluded men may be unwilling to report violence. Comparison of the excluded sample and the final sample on the completed items may be considered to show preliminary evidence if there is a gap in reporting IPV. Possible impact on the study should be addressed.</p> <p>5. Was the VAWI translated into Swedish or other languages? If yes, was translation and back translation conducted?</p> <p>Results</p> <p>6. What are the Cronbach's alpha coefficients for the subscales in the three-component solution?</p> <p>7. The function of Table 4 in the validation was not clearly addressed. Why to place it in findings? How is it used for the test of external validity?</p> <p>8. The authors had addressed the need to validate the VAWI in a sample of men which may show different patterns. However, they are using NorAQ which was previously validated in both genders. The authors have to argue how this could be used as a golden standard to validate the VAWI for male sample?</p> <p>Discussion</p> <p>9. The authors reported higher prevalence rates reported in this study but did not give interpretation if it could be attributed to the sample selected or instrument used in this study.</p> <p>10. The authors have to justify how the discrepancies in prevalence reported in the two surveys could be justified to confirm external validity.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

I declare that I have no competing interests.

My comments are very minor. Overall, this is a well-written article about an important methodological question in intimate partner violence research: can instruments designed to measure women's experiences of partner violence be applied with equal validity to male populations.

The Methods section should specify that it was a postal survey (this is in the abstract only).

Authors' response: Thank you for pointing this out. The following has been added to the methods section, under "Procedure, study population and response rate": "A postal survey was administered through Statistics Sweden." This section was also further modified according to comments from reviewer 2.

One suggested reference for review of literature regarding gender differences in IPV on page 4: Ansara & Hindin (2010) Exploring gender differences in the patterns of intimate partner violence in Canada: a latent class approach. *J Epidemiol Community Health*. 64:849-854.

Authors' response: We have used this reference in a recently submitted prevalence article comparing women's and men's exposure to IPV in Sweden, and which discusses in depth the gender differences of IPV. In our opinion, this reference does not provide additional context for this article, but we are happy to add it if the referee deems it essential.

It would be good to know how representative the sample is according to the socio-demographic factors presented in Table 1 according to census or some other data source. Also, the response categories for Educational Level do not appear to be exhaustive, ie., what about other types of training and certification that is not acquired in university?

Authors' response: Due to financial and time constraints we are unfortunately not able to conduct an exact comparison between the socio-demographic factors in Table 1 and the Swedish population of men in 2009. However, we were able to retrieve information and compare our final sample with the Swedish population of men in 2009 with respect to 1) the five age groups and 2) civil status (married/unmarried/divorced or widowed). This comparison showed that unmarried men and those of younger age (18-29 years) were underrepresented in our final sample. We also compared 3) country of birth (Sweden/outside Sweden), 4) income and 5) educational level, although these comparisons were somewhat hampered by differing age groups and/or categorizations between data available from the national population register maintained by Statistics Sweden and the data used in our study. However, the results showed that those born outside Sweden were underrepresented in our final sample, whereas our sample was representative for income and education. Data for and analysis of this comparison may be requested from the corresponding author. Finally, we assume that the referee is referring to vocational training and perhaps associate degrees. Vocational training programs are incorporated into Swedish high schools and curriculums are similar to college prep curriculums in that vocational students meet eligibility requirements for admission to university. In other words, there are no clear differences between the two and we therefore included both in the "high school" category. Also, there is no counterpart to community colleges in Sweden. Hence, we feel that the educational categories used in our study are meaningful within a Swedish context.

Specify how the WHO guidelines for ethical conduct were addressed on a postal survey, eg, confidentiality, safety for respondents, minimizing and responding to emotional trauma.

Authors' response: The following has been added under "ethical considerations": "The Regional Ethics Review Board located in Gothenburg gave approval for this study (Dnr: 527-08) and the WHO ethical and safety recommendations for research on domestic violence against women as applicable to a postal survey were followed.(29) For example, a letter informing about the upcoming survey was sent to prospective respondents in advance so they could decline the survey before receiving it. Also, only one survey per household was sent out for safety reasons. Additionally, full anonymity and confidentiality were guaranteed and contact information to a general practitioner (third author on this study), a psychologist and a contact person at Statistics Sweden was provided for additional information and/or referral."

Additional limitations could be mentioned: (1) the sub-sample of respondents who answered both the VAWI and the NorAQ is relatively small; (2) violence experienced earlier in the lifetime may have been undercounted because of the structure of the questionnaire (this is mentioned on page 16 but merits mention as a limitation); and (3) question wording on the NorAQ may threaten the reliability of estimates of psychological violence, ie., "systematically" and "for a longer period" may not consistently have the same meaning for all study participants, unless definitions were provided that are not shown in the article. Or, is it possible this is a reflection of the translation from Swedish to English?

Authors' response: We agree with points 1-2 and have added the following to the limitations section: "Furthermore, the sub-sample of respondents who answered both the VAWI and the NorAQ is small, which limits our ability to draw conclusions or generalize to the target population." and "Also, the earlier-in-life estimates may have been underestimated due to a minor detail on the questionnaire layout." Point 3 is interesting; however, NorAQ has been shown to be valid and reliable in previous studies by other authors. As it was not the aim of the current study to investigate the validity of

NorAQ, we consider further exploration of this point to be outside the scope of this study.

On page 19, there is a typo in the 3rd line of physical violence: "trashed" I think should be "thrashed".

Authors' response: Thank you for noticing this; we have corrected the spelling.

Reviewer: 2

I have no conflict of interest to the study.

The purpose of this manuscript is to explore the psychometric properties of the WHO's Violence Against Women instrument (VAWI) in a population sample of Swedish men. With regard to the limited number of research on the validation of violence assessment instruments in a sample of men, the manuscript has the potential to become an important piece of reference for those who would like to know more about it. The manuscript is generally well written. However, there are several problems and suggestions are summarized below that may strengthen the study conclusions.

Introduction: Fine.

Methods

1. P.6, line 18: Citation of the companion paper is needed.

Authors' response: The following citation of the companion article has been added at the end of the sentence on page 6, line 18: "Data collection procedures, questionnaires and statistical analyses were the same as those used in the companion paper and are described in greater detail there (see companion paper entitled "Psychometric properties of the WHO Violence Against Women instrument in a female population-based sample in Sweden")."

2. A brief description of the sampling method is needed. What was the sampling frame? Were they households? What sampling strategy was employed? Were there any inclusion and exclusion criteria?

Authors' response: We have added and/or clarified the following in the methods section: "A postal survey was administered through Statistics Sweden. The sampling frame was based on the national population register and consisted of all registered individuals in Sweden aged 18-65. Out of these, Statistics Sweden employed a simple random sample of 1009 men. Although the sampling frame was based on registered individuals, only one survey per household was sent for ethical and safety reasons." and "Inclusion criteria for this study were: age 18-65 years, registered in Sweden, able to answer a survey written in Swedish and having been or currently in an intimate relationship."

3. Similarly, more information about the second data collection is needed. Were the respondents having similar demographic characteristics?

Authors' response: The random sample taken for the second data collection was drawn from the final sample of the first data collection. In this regard the idea was that they would have similar socio-demographic characteristics. However, since the final sample of the second data collection was small (N=50), we do not claim that the socio-demographic characteristics are generalizable to a population-based level. The following has been added to the limitations section: "Furthermore, the sub-sample of respondents who answered both the VAWI and the NorAQ is small, which limits our ability to draw conclusions or generalize to the target population ."

4. A brief comparison of the non-respondents and the final sample was conducted, as well as the excluded sample and the final sample. One may have a hypothesis that these excluded men may be

unwilling to report violence. Comparison of the excluded sample and the final sample on the completed items may be considered to show preliminary evidence if there is a gap in reporting IPV. Possible impact on the study should be addressed.

Authors' response: The following phrase and accompanying two references have been added to the limitations section: "Given that previous studies have found some of these groups to be associated with higher levels of IPV exposure among men, our study may have underreported IPV.(32, 33)"

5. Was the VAWI translated into Swedish or other languages? If yes, was translation and back translation conducted?

Authors' response: We have added the following sentence to the methods section, "assessment instruments": "The VAWI items were translated and adapted to a Swedish context by a senior researcher (third author) with extensive knowledge about intimate partner violence." A back-translation was not conducted.

Results

6. What are the Cronbach's alpha coefficients for the subscales in the three-component solution?

Authors' response: The alpha coefficient for the first component is 0.93, for the second 0.85 and for the third 0.71. As this was an exploratory study, we decided not to make any final recommendations as to the suitability of the VAWI for use in assessing intimate violence among men and hence decided to present the alpha coefficients of the hypothesized VAWI structure instead.

7. The function of Table 4 in the validation was not clearly addressed. Why to place it in findings? How is it used for the test of external validity?

Authors' response: The function of table 4 was to present the prevalence rates obtained with the VAWI in order to compare them with those reported in other similar studies (in this case a population-based study conducted in Finland). The aim was to see if the prevalence rates obtained with the VAWI would fall within a reasonable range of those reported by similar, previous studies. This is not a "head-to-head" test of the external validity of the questionnaire, but it does give an indication of its validity in relation to other instruments whose aim is to assess prevalence of IPV exposure. However, this question raises a very important and challenging point about the difficulties in defining a gold standard within IPV research, since there is no objective diagnostic test of IPV in the same way as for diabetes, for example. Instead, different answers and prevalence rates will be obtained depending on the questions asked, the framing of the survey and so on – what, then, should be considered the "true" gold standard? These difficulties also illuminate the need for standardized instruments for comparisons of IPV exposure between and within countries, which we address in greater detail in the companion article conducted among women.

8. The authors had addressed the need to validate the VAWI in a sample of men which may show different patterns. However, they are using NorAQ which was previously validated in both genders. The authors have to argue how this could be used as a golden standard to validate the VAWI for male sample?

Authors' response: NorAQ was chosen since it is the only available, validated instrument assessing violence in Sweden among a male population-based sample. Moreover, there is evidence for its good validity and reliability. Hence, under the results section, VAWI and NorAQ, we write: "NorAQ was chosen as it is the only questionnaire measuring violence that has been validated in Sweden in both a male and female (see companion article) population-based sample." To our knowledge, there are no studies investigating the factorial structure of NorAQ among men, which may indeed be an interesting

research question to pursue – however, it was not the instrument under investigation in the current study.

Discussion

9. The authors reported higher prevalence rates reported in this study but did not give interpretation if it could be attributed to the sample selected or instrument used in this study.

Authors' response: We agree with this point and have added the following: "Moreover, the discrepancies may be due to differences in the definitions of violent acts. For example, the high estimates of sexual IPV found in the current study are most likely attributable to the first and relatively less severe violence item "Demanded to have sex with me even though I did not want to (but did not use physical force)". However, if this item were excluded and only the following two and relatively more severe VAWI items were counted, then our rate would be more similar to that in the Finnish study. Finally, the observed differences may reflect actual differences between the two countries."

10. The authors have to justify how the discrepancies in prevalence reported in the two surveys could be justified to confirm external validity.

Authors' response: Although the prevalence rates of physical and sexual IPV are higher in the VAWI, only the difference in the psychological violence scale is statistically significant. Under the results section we look further into this difference: "However, only the difference in psychological IPV was statistically significant (30.6% vs. 10.2%; $p < 0.05$). This difference owed principally to the VAWI items "Insulted me in a way that made me feel bad about myself" (24%) and "Belittled and humiliated me in front of other people" (16%). Prevalence rates for the other items on this scale were similar to corresponding items in the NorAQ (see Appendix 1)." However, the small sample size of this comparison prohibits any strong conclusions and we are careful throughout the manuscript not to use the results from this comparison as confirmation of external validity per se. Hence, we mention this limitation both in the discussion and have now also added a sentence about this in the limitations section of this paper (see under comment 3). Finally, the reviewer's comment could also be understood as an extension to the comment number 7, which raises interesting questions with regards to the difficulties of confirming external validity of IPV assessment instruments. What should we validate against when different definitions of IPV and differing ways of asking about IPV (survey vs. interview for example) etc. are likely to give different answers? These issues are not easy to resolve and bring us back to the difficulties of confirming external validity of violence assessment instruments.

Additionally, a period was added after the sentence in the second bullet point of the article focus (1), a spelling mistake was corrected in the contributorship statement where "amd" was corrected to "and" (2) and an additional spelling mistake was corrected in table 5, where "VAW" was corrected to "VAWI" (3).

VERSION 2 – REVIEW

REVIEWER	Edward Chan, Associate Professor Department of Social Work and Social Administration The University of Hong Kong, Hong Kong I have no conflict of interest to this study.
REVIEW RETURNED	26-Oct-2012
GENERAL COMMENTS	The authors have responded to the queries. I'm satisfied with the responses and suggest to accept it as it is.