PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Intimate partner violence (IPV) and prescription of potentially addictive drugs: prospective cohort study of women in the Oslo Health Study
AUTHORS	Lise Eilin Stene, Grete Dyb, Aage Tverdal, Geir Wenberg Jacobsen, Berit Schei

VERSION 1 - REVIEW

REVIEWER	Dr. Janice Du Mont Scientist Women's College Research Institute Violence and Health Research Program 790 Bay Street, 7th Floor, Toronto, Ontario M5G 1N8 Statement of Competing Interests In 2001, i co-authored an article with the last author Berit Schei.
REVIEW RETURNED	05/12/2011

THE STUDY	Participants Patients may not be representative of actual patients the evidence might affect but this is discussed in part by the authors as a limitation of the study. Why were women limited to ages 30 to 60 years in original survey? What are implications for the study given that much intimate partner violence occurs in the young adult group less than 30 years of age, and certainly occurs after age 60? Standards of English Standard of English is good, but a thorough edit could smooth awkward sentences that occur here and there throughout the manuscript. Use of 'exposed', 'non exposed', 'unexposed', 'exposure category', etc. -- intimate partner violence is NOT a disease, it is a social issue with health implications. spellout CNS first time used Method Missing data. page 8 line 54 states "analyses were restricted to women with complete data...", do authors mean for multivariable

	models? Clarification: Throughout manuscript, it would help if authors consistently referred to violence in the same manner. also, once they have given acronym for intimate partner violence (IPV), they could then just use IPV consistently from there on
RESULTS & CONCLUSIONS	Results Figure 2 - numbers for sexual IPV do not seem to add up. states n = 193 or should it be 194? Discussion/Conclusions page 12 - line 45/46, what is "latter"? page 13 - lines 10-12 - need references to back up this assertation. Message could be clearer: Page 14 - lines 27 to 32 - 'comprehensive health service response'. What is being suggested? Please explain. Does this include screening? if so, might acknowledge that the evidence does not necessarily support this (Harriet MacMillan et al's work). also, this "assessment of IPV" arises in line 55 in Abstract under 'Conclusions', which seem a little weak given important findings of study.
REPORTING & ETHICS	i can not possibly know if the authors have undeclared conflicts of interest, etc., but given my knowledge of the area, i detect no obvious ethical concerns with this article.
GENERAL COMMENTS	I think that this a very important contribution to the field of intimate partner violence against women.

REVIEWER	Patrice M. Muchowski, Sc.D. Vice President, Clinical Services AdCare Hospital Worcester, MA. USA
REVIEW RETURNED	24/01/2012

THE STUDY	Item 3: One area that I think could be described more fully are the age cohorts that were selected, this only gives us information about women in certain age groups which may not necessarily be reflective of all women. These cohort groups may have been chosen as part of the general study some rationale might be useful. Itetm 6: Authors refer to "low participation rate" more discussion limitations associated with this would be useful.
RESULTS & CONCLUSIONS	Item 4: I am confused about the reference to many women not receving any medicine on pg. 8(assume this refers to prescriptions for potentially addictive medicien) and then interpreting Table 2 since it looks like many women did receive prescriptions
REPORTING & ETHICS	Item 2: It is clear that the research is part of a larger study, would be beneficial for the researchers to describe how they consented the patient to participate in the questionannaire, and give permission for the prescription data to be reviewed.
GENERAL COMMENTS	I think the authors chose a vulnerable population to examine. Analysis of the data according to the types of IVP is useful. Authors were also specific re the risks associated with prescribing addictive

	medication, and often the lack of thorough assessment of IPV by health care professionals. There is reference to the need of other types of interventions, would be helpful to have some suggestions included in the paper.

VERSION 1 – AUTHOR RESPONSE

Reviewers' comments and authors' response:

Reviewer: Dr. Janice Du Mont (reviewer 1)
Scientist
Women's College Research Institute
Violence and Health Research Program
790 Bay Street, 7th Floor,
Toronto, Ontario M5G 1N8

Statement of Competing Interests: In 2001, i co-authored an article with the last author Berit Schei.

Participants

Patients may not be representative of actual patients the evidence might affect but this is discussed in part by the authors as a limitation of the study.

Why were women limited to ages 30 to 60 years in original survey? What are implications for the study given that much intimate partner violence occurs in the young adult group less than 30 years of age, and certainly occurs after age 60?

Authors' response:

The selection of specific birth cohorts in the Oslo Health Study was performed in order to correspond with population based studies in other Norwegian counties. Some of the age cohorts had also been included in a previous study in Oslo, and were included in order to attain longitudinal data. The original survey included participants aged 75 and 76 years; however, questions about violence were included in supplementary questionnaires that were given to participants aged 30-60 years only. This age selection corresponded to an approaching national study of violence against women.1

We agree that we had not elaborated sufficiently on how the selection of age groups represented a limitation. Since our study was limited to women aged 30-60 years at baseline, the estimated association between IPV and prescription of potentially addictive drugs may not necessarily be valid for women in other age groups. In the revised manuscript we have described this as a study limitation in the discussion.

Reviewer 1:

Standards of English

Standard of English is good, but a thorough edit could smooth awkward sentences that occur here and there throughout the manuscript.

Use of 'exposed', 'non exposed', 'unexposed', 'exposure category', etc. -- intimate partner violence is NOT a disease, it is a social issue with health implications.

spellout CNS first time used

Authors' response:

We agree that intimate partner violence should not be considered a disease. In the revised manuscript we have substituted “exposed” and similar expressions with terms such as “experiences of IPV” or “reported IPV”. Furthermore, the first mention of CNS has been replaced by “central nervous system (CNS)”. We would consent to supplementary language editing if requested.

Reviewer 1:

Method

Missing data. page 8 line 54 states "analyses were restricted to women with complete data...", do authors mean for multivariable models?

Authors' response:

Both univariable χ^2 analyses and multivariable regression analyses were restricted to women with complete data on included variables. This has been specified in the revised manuscript. Furthermore, the number of women included in the respective analyses is reported in the tables.

Reviewer 1:

Clarification:
Throughout manuscript, it would help if authors consistently referred to violence in the same manner. also, once they have given acronym for intimate partner violence (IPV), they could then just use IPV consistently from there on

Authors' response:
We agree with your remark. In the revised manuscript we have endeavored to consistently refer to IPV.

Reviewer 1:

Results

Figure 2 - numbers for sexual IPV do not seem to add up. states n = 193 or should it be 194?

Authors' response:

We appreciate this correction. The number for sexual IPV should be n =193. However, the number “52” in figure 2 was wrong; it should have been “51”. This has been corrected in the revised manuscript.

Reviewer 1:

Discussion/Conclusions

page 12 - line 45/46, what is "latter"?
page 13 - lines 10-12 - need references to back up this assertation.

Authors' response:
In line 45/46, “latter” referred to intermediate variables. In order to clarify, we have replaced “latter” with “intermediate variables”. Furthermore, the assertion on page 13 – lines 10-12 has been backed up by appropriate references in the revised manuscript

Reviewer 1:

Message could be clearer:

Page 14 - lines 27 to 32 - 'comprehensive health service response'. What is being suggested? Please explain. Does this include screening? if so, might acknowledge that the evidence does not necessarily support this (Harriet MacMillan et al's work).

also, this "assessment of IPV" arises in line 55 in Abstract under 'Conclusions', which seem a little weak given important findings of study.

Authors' response:

We recognize that there is still limited evidence regarding effective health care interventions to prevent IPV and its associated adverse health impact. This has been emphasized in the revised manuscript, including a reference to Macmillan et al’s work. It is beyond the scope of our study to determine the optimal health service response to intimate partner violence, but we have added a reference to a recent study that found substantial benefit of a training programme in primary care settings. Furthermore, we have highlighted the need of future research to develop effective evidence-based health care interventions to women who have experienced IPV. We have also rewritten the conclusions in the abstract.

Reviewer 1:
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i can not possibly know if the authors have undeclared conflicts of interest, etc., but given my knowledge of the area, i detect no obvious ethical concerns with this article.

I think that this a very important contribution to the field of intimate partner violence against women.

Authors' response:
We thank the reviewer for the positive remark.

Reviewer: Patrice M. Muchowski, Sc.D. (reviewer 2)
Vice President, Clinical Services
AdCare Hospital
Worcester, MA. USA

Item 3: One area that I think could be described more fully are the age cohorts that were selected, this only gives us information about women in certain age groups which may not necessarily be reflective of all women. These cohort groups may have been chosen as part of the general study some rationale might be useful.

Authors' response:
Please see explanation above in the first response.

Reviewer 2:

Itetm 6: Authors refer to "low participation rate" more discussion limitations associated with this would be useful.

Authors' response:

We agree that it would be advantageous with a more thorough discussion on limitations associated with low participation rate. In the revised manuscript we have elaborated further about potential differential selection bias in the second section of the discussion.

Reviewer 2:

I am confused about the reference to many women not receving any medicine on pg. 8(assume this refers to prescriptions for potentially addictive medicien) and then interpreting Table 2 since it looks like many women did receive prescriptions

Authors' response:
We understand that this reference may have been confusing. It was included to describe the choice of statistical tests. For negative binomial models the choice of statistical test may depend upon the proportion of cases with zero counts. We agree that many women received prescriptions of potentially addictive drugs during follow-up. However, since nearly half of the women in our study did not receive any prescriptions there was statistically a large number of cases with zero counts, which sometimes favoured the use of a particular statistical test called the Vuong test. We have tried to clarify this in the revised manuscript.

Reviewer 2:

It is clear that the research is part of a larger study, would be beneficial for the researchers to describe how they consented the patient to participate in the questionannaire, and give permission for the prescription data to be reviewed.

Authors' response:
The consent procedure has been described in reference number 18. In the revised manuscript we have also denoted this in the methods. Unfortunately, we do not have the permission to share individually linked data from the Oslo health survey and the Norwegian Prescription Database. However, prescription data for the entire population in Norway are available in the web site denoted in reference number 20: http://www.norpd.no/Prevalens.aspx. In addition, we would willingly provide more detailed information about our analyses to those who are interested. Please contact the corresponding author for more information: lise.e.stene@ntnu.no.

Reviewer 2:

I think the authors chose a vulnerable population to examine. Analysis of the data according to the types of IVP is useful. Authors were also specific re the risks associated with prescribing addictive medication, and often the lack of thorough assessment of IPV by health care professionals. There is reference to the need of other types of interventions, would be helpful to have some suggestions included in the paper.

Authors' response:
We appreciate the positive remarks. Please see the second last response to reviewer 1 regarding health care interventions for women who have experienced IPV.

1 Neroien AI, Schei B. Partner violence and health: Results from the first national study on violence against women in Norway. Scandinavian Journal of Public Health 2008; 36: 161-168.