

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Change in prevalence of psychological and economic abuse, and controlling behaviors against women by an intimate partner in two cross-sectional studies in New Zealand, 2003-2019
AUTHORS	Fanslow, Janet; Malihi, Zarintaj; Hashemi, Ladan; Gulliver, Pauline; McIntosh, Tracey

VERSION 1 – REVIEW

REVIEWER	Yukiko Washio RTI International USA
REVIEW RETURNED	28-Oct-2020

GENERAL COMMENTS	<p>Change in prevalence rates of women's physical and sexual IPV in New Zealand</p> <p>Nation wide household survey was conducted in 2003 and 2019, and data on IPV were analyzed specific to women populations. The project is significant and indicative of future research directions. Authors might want to reconsider the analyses by comparing women who responded to both years so there will be one on one matching in how the prevalence has changed over the years.</p> <p>Introduction – Association between IPV and women's belief need to be stated which direction (positive or inverse)</p> <p>Analytic procedure – moderate/severe physical IPV and any IPV but what about sexual IPV? Used both categories?</p> <p>Changes in physical IPV prevalence rates – 30% in both years?</p> <p>Lifetime physical IPV for multivariate analyses, as lifetime sexual IPV multivariate analyses?</p>
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REVIEWER	Andrew Gibbs South African Medical Research Council South Africa
REVIEW RETURNED	11-Dec-2020

GENERAL COMMENTS	<p>This paper looks at the differences between the past year and lifetime prevalence rates for emotional, economic IPV and controlling behaviours in New Zealand, and also looks at risk factors for them, and also looks at whether there is an interaction between education and time and relationship status and time. Overall, it is a clear paper that is well written. I mainly am concerned with the discussion, which I find not to properly engage with the findings – and I think that many of the interesting ideas are in the conclusion, so there needs to be a rearranging of these.</p> <p>I think they key themes that need addressing are:</p> <ol style="list-style-type: none">1. Why does past year emotional IPV decrease, but lifetime economic IPV and controlling behaviours increase? This is touched
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	<p>on in the conclusion, but it needs to be in the discussion first. Alongside the argument that tactics may be changing, it may be that these are behaviours with different drivers and function differently to physical and emotional IPV – alongside the argument they put forward</p> <p>2. I don't think we need comparison particularly with other studies about trends – it may be things change differently in different countries, and the question is really about what's happening in NZ</p> <p>3. I think the argument about the impact of national interventions is interesting and this needs to be discussed in a separate paragraph, about the limits of interventions and that they may have differential effects on different groups, or on different forms of violence</p> <p>4. The increase in economic IPV and controlling behaviours – for those over 30 is likely driven by the fact they are older therefore have more chance of having experienced it, alongside the other reasons put forward</p> <p>5. The paragraph on social support needs expanding – why may this be the case and what are the implications – it's not that whole family intervention needed – but that social support survivors needs to be in place</p> <p>6. There is no discussion of the implications of the interaction models for education and relationship status and these need to be discussed</p> <p>Minor issues In the methods section, greater clarity about the sampling framework used and whether it was a self-weighting sample or not is needed (and any notes on weighting in the analysis). We need to understand what the sample was representative of – New Zealand or smaller space.</p> <p>Was the same sampling framework used in the two rounds?</p> <p>Page 6: mentioned “we seek to understand the impact of population-based strategies” this is not the aim of the paper, as you are not looking at that at all and don't measure etc. So needs editing</p> <p>In tables and text, for adjusted multivariable models – should it not be aOR?</p> <p>Limitations: need to expand to include no past year economic and controlling behaviour measures, samples are not the same, and economic IPV is very small sample in 2019.</p>
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REVIEWER	Kathryn Yount Hubert Department of Global Health and Department of Sociology, Emory University
REVIEW RETURNED	12-Dec-2020

GENERAL COMMENTS	<p>This paper assesses the change in prevalence of intimate partner violence, by type, comparing estimates from two regional studies in New Zealand conducted by the authors in 2003 and subsequently in 2019. The authors use the WHO module to measure experiences of psychological, economic IPV and controlling behaviors experienced by ever partnered women 18-64 years. Efforts to assess trends in IPV are important, given that SDG5 calls on national governments to monitor changes in intimate partner violence. Hopefully the feedback below will strengthen the contributions of this manuscript.</p> <p>1. The abstract needs to describe the sample more clearly by</p>
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	<p>outcome (e.g., ever-partnered women 18-64 years?)</p> <p>2. Background Literature. The background would benefit from more recent studies of the global prevalence of IPV (Devries et al. 2013) and international studies of the forms of IPV that are the focus of the paper (e.g., Yount and Krause (2016). Economic Coercion and Partner Violence Against Wives in Vietnam: A Unified Framework? Journal of Interpersonal Violence).</p> <p>3. Samples. The surveys seem to be conducted in different regions in New Zealand, and indeed, the two samples differ on several demographic characteristics (age, education, independent income, 'deprivation'). These differences in the sample could be one explanation for differences in prevalences of different forms of violence, and this limitation needs to be more explicit in the discussion.</p> <p>4. Rationale for IPV variable construction. A stronger rationale for the ways in which the IPV variables are constructed would be useful.</p> <p>5. Socio-demographic variables. The socio-demographic variables warrant better justification and clarification. For example, why are there no measures of relative gender equality in the partnership to get at women's dependence or status inconsistency in the partnership? Why is 'marital status' included if the sample is ever-partnered women (e.g., do you mean married, divorced, widowed, separated?). More detail on this in the Independent variables section of the methods is warranted.</p> <p>6. Measurement invariance of the IPV variables. A key, and problematic assumption, of the authors is that the measures of IPV assessed over time are 'psychometrically invariant' across groups and time. Given differences in the samples and the 16 year time lag, assessing first the cross-group and cross-time measurement invariance of the IPV questions is warranted. The evidence on this is lacking in the literature, and so I would simply recommend to the authors that they mention measurement non-invariance as a potential limitation of the analysis in the Discussion.</p> <p>7. Another problem with the IPV prevalence measures is that they are based on a very limited item set---4 acts of psychological violence; 2 acts of economic violence; 3 acts of controlling behavior. The original revised CTS included 39 acts of violence, and we know that prevalence estimates are sensitive to the type of act included as well as the number of different acts included. A more honest discussion of the limitations of the prevalence estimates, given the relatively few questions on each type included is needed.</p> <p>8. Finally, it is not entirely clear whether the questions on IPV are worded exactly the same way, what other modules were included in the questionnaire, how interviewers were trained in each survey. Differences across waves in question wording, survey design, and interviewer training can have a substantial impact on prevalence estimates, and more discussion of all of these potential differences are needed in the methods section, and possibly the discussion/limitations section.</p> <p>9. For all of these reasons, I would interpret the results regarding trends in prevalence with more caution and make study-design recommendations to other researchers to improve our ability to</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Yukiko Washio

Institution and Country: RTI International USA Please state any competing interests or state 'None declared': None declared

Change in prevalence rates of women's physical and sexual IPV in New Zealand Nation wide household survey was conducted in 2003 and 2019, and data on IPV were analyzed specific to women populations.

We think this review was intended for the companion manuscript that we submitted to BMJ Open, Manuscript Number bmjopen-2020-044907. Nevertheless, we address the points raised by the reviewer below.

The project is significant and indicative of future research directions.
Thank you, we are pleased that you think the project is significant.

Authors might want to reconsider the analyses by comparing women who responded to both years so there will be one on one matching in how the prevalence has changed over the years.

Thank you for this suggestion. Unfortunately, one to one matching is not possible, as we did not interview the same women at two different time points. Data collection was carried out with two independent samples.

Introduction – Association between IPV and women's belief need to be stated which direction (positive or inverse) Analytic procedure – moderate/severe physical IPV and any IPV but what about sexual IPV? Used both categories?

Changes in physical IPV prevalence rates – 30% in both years?

Lifetime physical IPV for multivariate analyses, as lifetime sexual IPV multivariate analyses?

These suggestions relate to the companion manuscript, bmjopen-2020-044907, so we have not responded to these points for the present article.

Reviewer: 2

Reviewer Name: Andrew Gibbs

Institution and Country: South African Medical Research Council, South Africa Please state any competing interests or state 'None declared': None declared

This paper looks at the differences between the past year and lifetime prevalence rates for emotional, economic IPV and controlling behaviours in New Zealand, and also looks at risk factors for them, and also looks at whether there is an interaction between education and time and relationship status and time. Overall, it is a clear paper that is well written. I mainly am concerned with the discussion, which I find not to properly engage with the findings – and I think that many of the interesting ideas are in the conclusion, so there needs to be a rearranging of these.

Thank you. We are pleased that the reviewer considers the manuscript to be clear, well written, and that it contains interesting ideas. We note the need to expand on the discussion and engage more fully with the findings and have revised the manuscript accordingly (see new paragraphs, page 24). We think this has strengthened the manuscript considerably, and we are grateful to the reviewer for bringing this to our attention. Please see our responses to the individual points below.

I think they key themes that need addressing are:

1. Why does past year emotional IPV decrease, but lifetime economic IPV and controlling behaviours increase? This is touched on in the conclusion, but it needs to be in the discussion first. Alongside the argument that tactics may be changing, it may be that these are behaviours with

different drivers and function differently to physical and emotional IPV – alongside the argument they put forward

Agreed. This has now been addressed directly in the discussion, and the potential for different drivers noted (see page 24, end of first paragraph).

2. I don't think we need comparison particularly with other studies about trends – it may be things change differently in different countries, and the question is really about what's happening in NZ

Agreed. We have now minimised the discussion points related to findings from other countries, and have included more detail about what has occurred in New Zealand (page 24, 2nd paragraph).

3. I think the argument about the impact of national interventions is interesting and this needs to be discussed in a separate paragraph, about the limits of interventions and that they may have differential effects on different groups, or on different forms of violence

Agreed. We have increased discussion of these points, within the confines of the available word limit. (page 24, 2nd paragraph)

4. The increase in economic IPV and controlling behaviours – for those over 30 is likely driven by the fact they are older therefore have more chance of having experienced it, alongside the other reasons put forward

Noted and agreed. This additional reason has been added to the paragraph (page 23, bottom of page).

5. The paragraph on social support needs expanding – why may this be the case and what are the implications – it's not that whole family intervention needed – but that social support survivors needs to be in place

We have noted the point about the wider need for social support (page 24, last paragraph), but have not expanded this paragraph greatly, due to word count limitations.

6. There is no discussion of the implications of the interaction models for education and relationship status and these need to be discussed

After re-doing the analyses to take into account the sample weighting, the interactions no longer reach significance, so we have not included a discussion about these.

Minor issues

In the methods section, greater clarity about the sampling framework used and whether it was a self-weighting sample or not is needed (and any notes on weighting in the analysis). We need to understand what the sample was representative of – New Zealand or smaller space.

Response: We have now included further information on the sampling framework used, and the procedures undertaken to account for the sampling design. This includes use of 'STRATA' to account for the location where participants were recruited from, cluster sampling, and weighting based on number of total eligible women per household. This explanation is provided in the methods section: page 9).

Was the same sampling framework used in the two rounds?

Response: Yes. We added a sentence to clarify this based on your question. (methods, bottom of page 10).

Page 6: mentioned "we seek to understand the impact of population-based strategies" this is not the aim of the paper, as you are not looking at that at all and don't measure etc. So needs editing

Thank you. We have edited this sentence, which now reads: "*In the current investigation, we sought to explore if there have been changes in the prevalence of women's experience of psychological abuse, controlling behaviours and economic abuse by intimate partners.*" (bottom of page 6)

In tables and text, for adjusted multivariable models – should it not be aOR?

Response: thank you for our comment. We have amended this in all tables with multivariate analyses, and in the text.

Limitations: need to expand to include no past year economic and controlling behaviour measures, samples are not the same, and economic IPV is very small sample in 2019.

Noted and agreed. These limitations have now been included in the discussion (page 26).

Reviewer: 3

Reviewer Name: Kathryn Yount

Institution and Country: Hubert Department of Global Health and Department of Sociology, Emory University, USA Please state any competing interests or state 'None declared': None declared

This paper assesses the change in prevalence of intimate partner violence, by type, comparing estimates from two regional studies in New Zealand conducted by the authors in 2003 and subsequently in 2019. The authors use the WHO module to measure experiences of psychological, economic IPV and controlling behaviors experienced by ever-partnered women 18-64 years. Efforts to assess trends in IPV are important, given that SDG5 calls on national governments to monitor changes in intimate partner violence. Hopefully the feedback below will strengthen the contributions of this manuscript.

Thank you. We are glad the reviewer considers that effort to assess trends in IPV are important, and we are grateful for the helpful feedback, which we agree strengthens the contributions of this manuscript.

1. The abstract needs to describe the sample more clearly by outcome (e.g., ever-partnered women 18-64 years?)

This change has been made, thank you for the suggestion (page 2).

2. Background Literature. The background would benefit from more recent studies of the global prevalence of IPV (Devries et al. 2013) and international studies of the forms of IPV that are the focus of the paper (e.g., Yount and Krause (2016). Economic Coercion and Partner Violence Against Wives in Vietnam: A Unified Framework? *Journal of Interpersonal Violence*).

Thank for these helpful suggestions. We have now included the suggested article by Yount, Krause and VanderEnde (2016). We have not included the reference to Devries, et al, as this relates to the prevalence of physical and sexual IPV.

3. Samples. The surveys seem to be conducted in different regions in New Zealand, and indeed, the two samples differ on several demographic characteristics (age, education, independent income, 'deprivation'). These differences in the sample could be one explanation for differences in prevalences of different forms of violence, and this limitation needs to be more explicit in the discussion.

Adjusted Odds Ratios were used to control for socio-demographic factors, and the differences in prevalence remained significant after this adjustment, suggesting that the differences in the two samples cannot fully explain the changes in prevalence rate over time.

4. Rationale for IPV variable construction. A stronger rationale for the ways in which the IPV variables are constructed would be useful.

We have included further detail on this in the paragraph describing outcome variables (top of page 9).

5. Socio-demographic variables. The socio-demographic variables warrant better justification and clarification. For example, why are there no measures of relative gender equality in the partnership to get at women's dependence or status inconsistency in the partnership? Why is 'marital status' included if the sample is ever-partnered women (e.g., do you mean married, divorced, widowed, separated?). More detail on this in the Independent variables section of the methods is warranted.

Response:

The name of the "Marital status" variable has been changed to "Relationship Status" to better convey what is being assessed by this variable. As the reviewer has surmised, the aim was to assess if there were variations in IPV experience associated with partnership status (current, divorced or widowed).

6. Measurement invariance of the IPV variables. A key, and problematic assumption, of the authors is that the measures of IPV assessed over time are 'psychometrically invariant' across groups and time. Given differences in the samples and the 16 year time lag, assessing first the cross-group and cross-time measurement invariance of the IPV questions is warranted. The evidence on this is lacking in the literature, and so I would simply recommend to the authors that they mention measurement non-invariance as a potential limitation of the analysis in the Discussion.

Thank you. We agree and have now included this limitation in the discussion (page 26).

7. Another problem with the IPV prevalence measures is that they are based on a very limited item set---4 acts of psychological violence; 2 acts of economic violence; 3 acts of controlling behavior. The original revised CTS included 39 acts of violence, and we know that prevalence estimates are sensitive to the type of act included as well as the number of different acts included. A more honest discussion of the limitations of the prevalence estimates, given the relatively few questions on each type included is needed.

While we agree that there were relatively few questions used to measure these types of violence, the number and wording of the questions used were identical in the two surveys. As such, the prevalence estimates obtained should be comparable over time. Future research could indeed ask about more acts of these types of violence, and we have noted this in the discussion (page 26).

8. Finally, it is not entirely clear whether the questions on IPV are worded exactly the same way, what other modules were included in the questionnaire, how interviewers were trained in each survey. Differences across waves in question wording, survey design, and interviewer training can have a substantial impact on prevalence estimates, and more discussion of all of these potential differences are needed in the methods section, and possibly the discussion/limitations section.

Thank you, these are important points. We have now clarified that question wording was identical (page 9), and interviewer training was comparable across surveys (page 7).

9. For all of these reasons, I would interpret the results regarding trends in prevalence with more caution and make study-design recommendations to other researchers to improve our ability to assess trends over time.

Thank you for this helpful suggestion. We have now included further study design recommendations in the section on Strengths and future study directions (page 26).

VERSION 2 – REVIEW

REVIEWER	Yukiko Washio RTI, USA
REVIEW RETURNED	17-Jan-2021

GENERAL COMMENTS	Comments adequately addressed.
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REVIEWER	Andrew Gibbs SAMRC, South Africa
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REVIEW RETURNED	01-Feb-2021
GENERAL COMMENTS	I think the authors have done a good job in revising this paper based on the feedback given by the reviewers, and I think it's suitable for publication.