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

TITLE (PROVISIONAL)	The value of screening and intervening for intimate partner violence in South African primary care: Project evaluation
AUTHORS	Joyner K. & Mash B.

VERSION 1 - REVIEW

REVIEWER	Richard P Barth, PhD, MSW University of Maryland Baltimore, MD 21210
REVIEW RETURNED	03/08/2011

THE STUDY	The authors indicate in the abstract, and again later in the text, that the article reports on benefits and harms. THis appears incorrect. Although the authors studied the possibility of benefits and harms there is no report of any harms. Althougyh there is mention of a woman being frightened this was related to divorce not directly related to the groups (which actually offer alternatives to divorce via counseling). PAge 7 indicates that no respondents reported the intervention as harmful. Mention of harms should be deleted. The term "Canadian activists" is an odd term that I have never seen in a professional journal. Were they specialists in IPV? Did they have a professional identification? Calling them "activists" doesn't tell us very much. p. 7. The authors should test to see whether those who showed up for the follow-up interview differed significantly from those who left the study--especially with regard to the seriousness of the risk they faced from their partners, but on other factors, as well. This is critical to meeting the expectations of the CONSORT standards.
RESULTS & CONCLUSIONS	THe abstract indicates that the "study did not measure the effect of the intervention on the abuse" but it did. This could be more clearly written. The study did not measure the effect because it did not have a causal design. It did, however, measure the level of abuse it just did not directly measure it (i.e., by observation). nonetheless, the researchers did get the measure of victim self-report, a quite good measure, in this case. p. 7. Here the authors begin to discuss the findings by quoting women. How were the quotes selected? Did the authors use any safeguards to ensure that the quotes are representative of the women's views? Is it possible that only extrement statements were selected? I would find it clarifying to know how many statements there were in all covering each of the sub-topics. So, did a lot of people indicate that there were partner changes or just a very few.

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	Anything that the authors can do to provide more context about these quotes would improve the methods for the qualitative analysis and give the reader more confidence that the responses are "typical".
REPORTING & ETHICS	As mentioned above, more information should be provided about who did not return for the foll0w-up because they might have been very different from the others and might have had a very different experience...
GENERAL COMMENTS	None

REVIEWER	Angela Taft Associate Professor/Principal Research Fellow Mother and Child Health Research La Trobe University Melbourne, Australia
REVIEW RETURNED	31/07/2011

THE STUDY	Design: The authors argue that they have evaluated both the screening and management of an IPV intervention in agreed South African primary care facilities. Without access to the Protocol (which could be a supplementary attachment) or methods for ascertaining how many and what proportion of women were screened, how many (%) disclosed, how many referred took-up the offer and subsequently how many were managed, the article only really reports the assessment of those who agreed to be managed in the intervention. The participation rates should have been tabulated. It is not sufficient to refer readers to an unpublished dissertation. I am particularly concerned at the absence of an ethics statement in a field in which safety of both subjects and staff is very important. I have no doubt the authors did make plans for this, but the extent to which they did so should be reported. Description and representativeness of participants: The age, location, and socio-economic status and marital status and numbers of children of participants and the general female clinic populations should have been described/tabulated so readers can understand how typical they were of those attending the clinics. As their abuse levels were severe, the authors should comment on whether these were thought to be typical of the patient population or that their intervention mainly identified the most severe.This intervention may be useful for women experiencing high or severe abuse - what about the other women at earlier stages? Methods: There is no reference or rationale for your study design. You could provide a supplementary document with the primary care professional curriculum in an online journal. How long did it last, what did it cover? Did it include safety for staff or documentation guide. How was consent obtained from staff or patient participants? What were your interview questions? This is important, as the majority of your quotations are not from women but staff. It is very important to let us know the status of quotations with pseudonyms so readers can judge whether the quotes are the same or different respondents. I am of the view that you should report % or n/n to allow readers to assess statements such as 'participants explained that (p7, l.24)... or 'cited many reasons P8,l.37...multiple participants p10, l.14) especially if the quotes are from staff. I commend the authors for reporting subjects feelings of harm, however your reporting of the comments on harm 'negative

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	reactions (p9, l.30). contradict the statement (p7, L.15) that no participants reported the intervention as harmful) Findings: The article does not report sufficient findings in a form in which readers can yet judge its integrity. As well as the screening participation uptake overall referred to above, it would have been useful to know the measure and degrees of overlap of abuse (I am not aware of infidelity, while upsetting, being considered abuse) and by what criteria high or severe scales were measured. Some strategies seem to be SA specific and warrant explanation. Why was referral to a psychiatric nurse considered risk-free\beneficial? In some countries it would be grounds for a partner to claim child custody as she is mentally unfit. It would help to understand the helpfulness ratings in your tables if you described the effectiveness of the current system in supporting primary care providers and women in your introduction. There are clearly limitations.
RESULTS & CONCLUSIONS	I have outlined above why I do not consider that the conclusion that screening is beneficial has been answered in the form in which they are presented in this article. It is a shame as with an honest presentation of how many women were screened, disclosed, accepted referral and were then managed in a resource poor and 'masculinist' environment could help locate where important improvements to support primary care staff in a health system intervention need to be made. You do need to consider previous recommendations and findings of screening studies to support your conclusions.
REPORTING & ETHICS	Please see above re ethics
GENERAL COMMENTS	None

REVIEWER	Tamara Shefer Professor Women's and Gender Studies University of the Western Cape Cape Town South Africa
REVIEW RETURNED	25/07/2011

THE STUDY	Overall this is a valuable paper that has relevance for knowledge production, policy and practice in the national and international context of gender-based violence. It is well written and reads well. It shows a strong understanding of the key challenges of IPV at the primary health care level in South Africa. My key concerns and areas that I feel need better or different elaboration in order to meet the requirements for publication and strengthen the paper include the following: 1) the literature review is comprehensive and up to date with respect to the clinical and epidemiological aspects of IPV. However I feel the author/s could better locate the study in feminist work and critical men's studies work on gender-based violence and male violence in South AFrica. THe author/s for example reference an edited general and relatively old text on masculinity in SA to support the argument that violence is a cornerstone of male power - there are considerably more and more recent works on this that should be acknowledged rather than an edited text (see for example the work of Kopano Ratele on masculinity and violence and a recent special edition of the Journal of Psychology in Africa on masculinity that highlights this argument and the way in which dominant forms of masculinity are bound up with violence (against women and other men)- there is a

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	growing body of SA and international work on masculinity and violence that would be better acknowledged than referencing an edited text (rather cite relevant empirical and theoretical work directly). Also the work of feminist work on GBV as documented in local journals like FeministAfrica and Agenda are not well documented by the author/s as key locating work. The author/s could also acknowledge the larger debates about the gap between policy and practice with respect to gender transformation that have been raised by gender researchers in SA - see for example edition by Gouws on the problematics of women's citizenship. 2) Linked to the above concern with contextualising of the study in larger feminist-informed work on gender-based violence, is a lack of a clear definition of the term IPV - there is a far larger history of work on violence against women and a clear delineation of what the term IPV includes, at least for the purposes of this study, and how it relates to other definitional and conceptual frameworks previously employed (violence against women - GBV - IPV) would greatly strengthen the paper. 3) More clarification is needed on the research design - in particular the quantitative measures - for example, how is abuse defined in the questionnaire used in the initial consultation from which data is extracted?
RESULTS & CONCLUSIONS	My key concern with respect to this paper and the presentation of the results in particular is the way in which the qualitative results are presented. These results are presented in two voices - one being the participants' own voice/s and the other apparently the notes of the interviewer/staff on the project. This is not only confusing to the reader but also is problematic since while presented as the participants' perceptions, these are at times actually representing the perceptions of the note-taker/staff/student nurse of the perceptions of the participant! I would suggest that the author/s either elaborate why they have not used participants' voices consistently if reporting on participants' perceptions and if they indeed do need to rely on the notes of the staff/interviewer/student nurse then this should be explained and clearly differentiated from the voice of participants. Methodologically it is inappropriate to conflate these two pieces of different data and two different voices. I would also suggest the author/s look at the overlap between the two sections of 1) value of the intervention and 2) perceptions of the impact of the intervention on the women and their situations - there is considerable overlap and it could be better streamlined for logical flow and readability and to avoid unnecessary overlap and repetition. Finally, there are a few key conclusions that I do not feel are clearly supported by the findings - for example the conclusion that the style of interaction with the nurse facilitates the positive outcomes - yet there are few quotes from the participants that highlight aspects of the style of communication in itself - either the data needs to be better drawn on to support the conclusion or the conclusion needs to be rephrased. Also some attention needs to be paid to providing a more nuanced reflection of the positive outcomes - at times for example it is evident from the findings that participants were engaged in other interventions like support groups that clearly contributed to the changes that are being reported on here. The author/s need to be more careful about inadvertently attributing all of the successful outcomes reported on by participants to the intervention when not solely derived from it.
REPORTING & ETHICS	A clearer statement of ethical considerations and procedures should be included in the overview of the methodology of the study.
GENERAL COMMENTS	None

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VERSION 1 – AUTHOR RESPONSE

Reviewers' recommendations followed by Joyner & Mash responses and revisions made:
1. Mention of harms should be deleted in abstract
Response: Done

2. Canadian activists -
Response: Replaced with Canadian specialists in IPV

3. Test to see whether those who showed up for follow-up differed significantly from those who left the study – especially with regard to seriousness of risk faced from partners and other factors too. NB for meeting CONSORT standards
Response: COREQ is more appropriate to this study design, and in line with BMJ requirements. However, Table I has been expanded to speak to comparative follow-up data.

4. More information should be provided about who did not return for follow-up because might have been very different from the others and might have had a very different experience.
Response: See Tables I & III, and where relevant, this aspect has been highlighted in the text.

5. Adjust abstract: study DID measure effect of intervention on abuse – Response: Done. Researchers got the measure of participants' self-report .

6. How were quotes selected? Safeguards to ensure quotes are representative of womens' views? Possible that only extrement statements were selected? How many statements were they in all covering each of the sub-topics? How many indicated partner changes? Anything authors can do to provide more context about these quotes would improve the methods for the qualitative analysis and give reader more confidence that the responses are "typical"
Quotes that best captured predominant themes were selected.
In complying with the 4th request it became evident that 45% of overall sample (56/168) had experienced benefit within their personal relationship. Given how negatively/pessimistically results relating to domestic violence work are usually represented, we reiterate that these findings are significant and of benefit to the international community.
Clearer detail relating to 'no real change' and 'ended relationship' has been provided.

Improved r/ships with partner: 56 women
No real change in relationship: 39 women
Ended relationship: 11 women
Improved mental state: 39
Value of safety assessment: 85
Value of safety planning: 77

Context has been more clearly delineated in various parts of article for resubmission.

7. Author/s could better locate the study in feminist work and critical men's studies work on gender-based violence and male violence in South Africa. there are considerably more recent works on this that should be acknowledged rather than an edited text (see for example the work of Kopano Ratele on masculinity and violence and a recent special edition of the Journal of Psychology in Africa on masculinity that highlights this argument and the way in which dominant forms of masculinity are bound up with violence (against women and other men)- there is a growing body of SA and international work on masculinity and violence that would be better acknowledged than referencing an edited text (rather cite relevant empirical and theoretical work directly). Also the work of feminist work

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on GBV as documented in local journals like Feminist Africa and Agenda are not well documented by the author/s as key locating work.

Response: We have provided a different citation to do better justice to the literature. Since masculinity is not a focus of this article we left it there.
However, we have also picked up on the issue in the recommendations paragraph – sentence alluding that this type of intervention focused on women but future research should include a perspective about men.

8. The author/s could also acknowledge the larger debates about the gap between policy and practice with respect to gender transformation that have been raised by gender researchers in SA - see for example edition by Gouws on the problematics of women's citizenship.
Response: We have referred to Gouws.

9. Linked to the above concern with contextualising of the study in larger feminist-informed work on gender-based violence, is a lack of a clear definition of the term IPV - there is a far larger history of work on violence against women and a clear delineation of what the term IPV includes, at least for the purposes of this study, and how it relates to other definitional and conceptual frameworks previously employed (violence against women - GBV - IPV) would greatly strengthen the paper.
Response: See 2nd paragraph of introduction.

10. More clarification is needed on the research design - in particular the quantitative measures - for example, how is abuse defined in the questionnaire used in the initial consultation from which data is extracted?
Response: Both quantitative and qualitative items were defined by the protocol tested within the project or our modification thereof. The instruments have been supplied for appendices to publication.
For example, the items assessing IPV were clearly specified forms of emotional, financial, sexual and physical.

11. Qualitative results are presented in two voices - one being the participants' own voice/s and the other apparently the notes of the interviewer/staff on the project. This is not only confusing to the reader but also is problematic since while presented as the participants' perceptions, these are at times actually representing the perceptions of the note-taker/staff/student nurse of the perceptions of the participant! I would suggest that the author/s either elaborate why they have not used participants' voices consistently if reporting on participants' perceptions and if they indeed do need to rely on the notes of the staff/interviewer/student nurse then this should be explained and clearly differentiated from the voice of participants. Methodologically it is inappropriate to conflate these two pieces of different data and two different voices.
Response:Now explained in the methods section: all data was collected in same way. Interviewers asked semi-structured questions (see instrument as appendix) and wrote down what patient said verbatim in the 1st person or paraphrased in 3rd person.

12. I would also suggest the author/s look at the overlap between the two sections of 1) value of the intervention and 2) perceptions of the impact of the intervention on the women and their situations - there is considerable overlap and it could be better streamlined for logical flow and readability and to avoid unnecessary overlap and repetition.
Response: Since both aspects overlap markedly I have reframed the section to cover both, as the text itself does justice to representing the variety of those issues.

13. Finally, there are a few key conclusions that I do not feel are clearly supported by the findings - for example the conclusion that the style of interaction with the nurse facilitates the positive outcomes -

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yet there are few quotes from the participants that highlight aspects of the style of communication in itself - either the data needs to be better drawn on to support the conclusion or the conclusion needs to be rephrased. Also some attention needs to be paid to providing a more nuanced reflection of the positive outcomes - at times for example it is evident from the findings that participants were engaged in other interventions like support groups that clearly contributed to the changes that are being reported on here. The author/s need to be more careful about inadvertently attributing all of the successful outcomes reported on by participants to the intervention when not solely derived from it.
Response: We disagree and feel that Therapeutic effect of empathic listening and Value of clinical care as first two themes presented under findings, speak powerfully to the contrast of the nurse researcher's approach with usual primary care. Consequently we contest that the style of interaction seems to enhance positive outcomes.

The support group the participant refers to is one she attended as a result of the intervention!

14. I would suggest that authors refrain from using the term 'harm' in talking about the impact of the programme they report on in the abstract and body of the paper. I would suggest rather using challenges and/or negative impact as a better representation of what is being reported on.
Response:Done

15. While this may be in keeping with the style of the journal and the larger disciplinary and professional context of this study, I would recommend that the author/s avoid the use of regulatory, de-personalising and objectifying language like the terms 'manage', 'managed' and 'management' to refer to the participation of research participants/clients in this programme as well as disease discourse like 'diagnosed with IPV' since in my view this reproduces problematic disempowering and pathologising constructions of both research participants and clients of public services which seems contrary to the goals of the researchers who locate themselves in gender and human rights discourses.
Response:Wherever possible, we have replaced language used with variations of "care". However, while we appreciate the sentiment of this reviewer, for the health systems context this terminology is appropriate. It doesn't imply managing the person but rather a system of dealing with the challenges of IPV for the health care sector. As such it is vital that this hugely and historically neglected area of health care is framed in terminology that health providers can relate to.

16. I am particularly concerned at the absence of an ethics statement in a field in which safety of both subjects and staff is very important. I have no doubt the authors did make plans for this, but the extent to which they did so should be reported.
Response:Done.

17. Design: The authors argue that they have evaluated both the screening and management of an IPV intervention in agreed South African primary care facilities. Without access to the Protocol (which could be a supplementary attachment) or methods for ascertaining how many and what proportion of women were screened, how many (%) disclosed, how many referred took-up the offer and subsequently how many were managed, the article only really reports the assessment of those who agreed to be managed in the intervention. The participation rates should have been tabulated. It is not sufficient to refer readers to an unpublished dissertation.
Response: We have edited screening out (except when referring to the published protocol we implemented) as, on reflection, we realised that it is a peripheral aspect of this article. We have written another article on case finding and recognition which is submitted elsewhere. Here the focus on the benefits of the intervention.

The doctoral thesis is published on Stellenbosch University's sunscholar website – http://hdl.handle.net/10019.1/2515

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Since we currently have two articles submitted, but have not yet heard, can only cite original dissertation at this stage.

18. Description and representativeness of participants: The age, location, socio-economic status and marital status and numbers of children of participants and the general female clinic populations should have been described/tabulated so readers can understand how typical they were of those attending the clinics. As their abuse levels were severe, the authors should comment on whether these were thought to be typical of the patient population or that their intervention mainly identified the most severe. This intervention may be useful for women experiencing high or severe abuse - what about the other women at earlier stages?
Response: We do not have socio-economic data on the general female clinic population and limited socio-economic data on the study population. In SA public sector primary care specifically provides a service for those with no insurance, and minimal if any income. Additional information on age, marital status and children has been added.

All women attending the primary care clinic were screened and there was no attempt to target those with more severe abuse or with specific presenting symptoms. It is possible that women experiencing more severe abuse might have disclosed more readily and be more open to an offer of help.

19. Methods: There is no reference or rationale for your study design.
Response: The rationale for describing this as project evaluation is given and the way in which the methods responsible for the findings presented here fit into the larger study design have been described. We wanted to avoid a lengthy explanation of the larger study design as it is not necessary in terms of understanding how the findings presented here were obtained.

20. You could provide a supplementary document with the primary care professional curriculum in an online journal. How long did it last, what did it cover? Did it include safety for staff or documentation guide.
Response: The training for health providers was given as a lunchtime seminar at each facility. Each provider was provided with a laminated menu of possible questions to select from when asking females 18 and over, in order to identify and refer potential participants to the study nurse.
Comprehensive training by specialists in each field was provided to the research team as follows:
Urban training course
A three-day training course was conducted immediately prior to the urban data collection phase.
Topics for the first day included:
♣ the purpose of the study
♣ a brief overview of IPV literature and the study design
♣ an introduction to action research
♣ the adapted protocol (first interview tool, Appendix X)
♣ forensic evidence collection and
♣ how to complete the J88, which is the official medico-legal document for police purposes
The second day focused on:
♣ coding medical records according to the International Classification for Primary Care
♣ the Domestic Violence Act of 1998
♣ role plays in order to learn how to use the mental health assessment guides
Day three focused on:
♣ interviewing techniques
♣ participant observation and
♣ follow-up interview tool.
Rural training course
A refresher training course was held the day before the rural data collection began. It was co-ordinated by Joyner (principal investigator), and attended by the rural researcher who had attended

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the first course, five months before, and three potential follow-up researchers. After introducing the study, all research instruments for both interviews were reviewed. Training on forensic documentation and how to complete the J88 effectively was repeated.

21. How was consent obtained from staff or patient participants?
Response:Written permission was obtained from relevant regional directors of the Department of Health and specific Facility Managers agreed that there CHC would participate in the study. Details relating to ethical issues such as informed, written consent have been added into the methods section.

22. What were your interview questions? This is important, as the majority of your quotations are not from women but staff. It is very important to let us know the status of quotations with pseudonyms so readers can judge whether the quotes are the same or different respondents. I am of the view that you should report % or n/n to /allow readers to assess statements such as 'participants explained that (p7, l.24)... or 'cited many reasons P8,l.37...multiple participants p10, l.14) especially if the quotes are from staff. Response:The interview questions for both interviews have been provided as appendices.

Participants' codes have been included to identify quotes instead of pseudonyms, as this is how we decided to provide anonymity.

Quantification of the qualitative data has been added for key findings as per comment 6 above.

23. I commend the authors for reporting subjects feelings of harm, however your reporting of the comments on harm 'negative reactions (p9, l.30). contradict the statement (p7, L.15) that no participants reported the intervention as harmful)
Response: Contradicts 1. No harm.
In r/ship to components of Table 4, no body reported them as harmful. However, occasional issues were raised on interview.

24. Findings: The article does not report sufficient findings in a form in which readers can yet judge its integrity. As well as the screening participation uptake overall referred to above, it would have been useful to know the measure and degrees of overlap of abuse (I am not aware of infidelity, while upsetting, being considered abuse) and by what criteria high or severe scales were measured.
Response: Infidelity was defined in the protocol as a form of sexual abuse, although it could perhaps be more accurately understood as emotional abuse. Since we were testing a specific protocol, we were rather confined by its parameters. Nevertheless this is an interesting issue since polygamy is cultural normative across much of South Africa. However, we will take that up in another article. Severity was defined by the protocol's safety assessment and is critiqued as a limitation.

25. Some strategies seem to be SA specific and warrant explanation. Why was referral to a psychiatric nurse considered risk-free\beneficial? In some countries it would be grounds for a partner to claim child custody as she is mentally unfit.
Response: Interesting point - have added at end of first paragraph in discussion. The SA context is so fraught with trauma and violence, that care from a psychiatric nurse would not be used ipso facto to claim custody on grounds of mental unfitness. On the contrary, in SA we have a national crisis of paternal abdication.

26. It would help to understand the helpfulness ratings in your tables if you described the effectiveness of the current system in supporting primary care providers and women in your introduction. There are clearly limitations.
Response: The description requested is actually provided in paragraph 5 of introduction.

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27. I have outlined above why I do not consider that the conclusion that screening is beneficial has been answered in the form in which they are presented in this article. It is a shame as with an honest presentation of how many women were screened, disclosed, accepted referral and were then managed in a resource poor and 'masculinist' environment could help locate where important improvements to support primary care staff in a health system intervention need to be made. You do need to consider previous recommendations and findings of screening studies to support your conclusions.
Response: There seems to be misunderstanding here. At no stage did we intend dishonesty when presenting data in this manuscript. Thank you for alerting us to the fact that we need to confine our discussion to the key points at hand, and leave our engagement with the screening debate to another article which provides an approach and rationale for IPV case finding in SA primary care context (in review).

VERSION 2 - REVIEW

REVIEWER	Richard P Barth, PhD, MSW University of Maryland Baltimore, MD 21210
REVIEW RETURNED	05/09/2011

The author filled in the checklist and made no further comments.

REVIEWER	Angela Taft MPH PhD Associate Professor Mother and Child Health Research La Trobe University Melbourne, Australia
REVIEW RETURNED	12/09/2011

THE STUDY	Study question: Is the overall study question - is screening effective or acceptable to women who participate (all or some women) or is screening more effective than case finding in this context? This is unclear. It is problematic as for all intent and purposes, this article should follow the major screening outcomes paper. Design: If this is impact evaluation, the methods would answer the question, was this strategy acceptable to women and what did they perceive as its benefits. However, this paper does not state this as the aim and needs to address this. Representativeness of the study population to the actual population. Again, the suthors did not answer this criticism adequately. They have acknowledged that providers were uncooperative and did not screen many women, but we dont know what proportion of women in the target group attend the clinics and whether 168 women is 10%, 40% or 60% of the potential for those who are reached. There is no acknowledgment that as more severaly abused women participated that this might represent a particular group. There is nothing wrong with this, but it needs an honest account.

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	Limitations are not fully acknowledged. Table 3: Column three requires clarification and amendment. Table 5: Would be improved with Helpful and very helpful collapsed and the unhelpful and harmful columns also with DK in the middle. Then the figures should be presented in ascending order
RESULTS & CONCLUSIONS	Results and findings (see the above) Data well-presented: As data on differences between those who stayed and those who did not reurn are presnted in tables, they do not need to be emphasised in the text. It is the differences between those who particiopated and those who didnt that is important. As you have not described how the WHO adapted mental health measure is scored and how data were abstracted, it is hard to make sense of your presentation of mean scores and why women were 'suspected' of differing diagnoses. Discussion: There is no data to support the claim' how - referral to a psychiatric nurse' is valued etc at all. The tone is problematic, does not nuance the findings - ie that while a large minority experienced beneficial changes, one in three experienced no change and one in ten left the relationship. This should be described at the opening to the discussion and then what was valued should follow. The statement 'contrary to popular belief (what belief?) most survivors...is this a SA belief? It is unfortunate that Appendices A+B were not attached so this reviewer could not assess these. The limitations should include the differences between those who did and did not participate andf what this might mean in the SA primary care contect and what further testing might be required. Some comment on the reasons for health care professional involvement (given the readership of this journal) would be helpful. Limitation of the safety assessment should not be introduced here but better described in the methods section describing the intervention.
REPORTING & ETHICS	Nevertheless the ethics clearances should mention the given number (Human Ethics Committee 2010# 234)
GENERAL COMMENTS	None

REVIEWER	Tamara Shefer Professor Women's and Gender Studies University of the Western Cape Cape Town South Africa
REVIEW RETURNED	09/09/2011

THE STUDY	
RESULTS & CONCLUSIONS	
REPORTING & ETHICS	
GENERAL COMMENTS	The author/s have attended well to my concerns raised in my initial review. In particular: 1) included far more detail on the participants, research design and the methodology. In particular explained use of first and third person

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	in the analysis 2) included a clear statement on ethics 3)literature review more comprehensive and reflective of current debates 4) has dealt with the structural concerns of overlapping sections - reframed, reads logically and succinctly now 5) has dealt with language that I found problematic (e.g harm) and replaced with more careful phrasing approriate to the discipline.

VERSION 2 – AUTHOR RESPONSE

Reviewer: Tamara Shefer
Professor
Women's and Gender Studies
University of the Western Cape
South Africa

The author/s have attended well to my concerns raised in my initial review. In particular:
1) included far more detail on the participants, research design and the methodology. In particular explained use of first and third person in the analysis
2) included a clear statement on ethics
3)literature review more comprehensive and reflective of current debates
4) has dealt with the structural concerns of overlapping sections - reframed, reads logically and succinctly now
5) has dealt with language that I found problematic (e.g. harm) and replaced with more careful phrasing appropriate to the discipline.
Thank you

Reviewer: Richard P. Barth, PhD
Dean and Professor
School of Social Work
University of Maryland
Baltimore, MD 21201

The revisions fully meet the expectations of this reviewer
Thank you

Reviewer: Angela Taft MPH PhD

Associate Professor
Mother and Child Health Research
La Trobe University
Melbourne, Australia

Study question:
Is the overall study question - is screening effective or acceptable to women who participate (all or some women) or is screening more effective than case finding in this context? This is unclear. It is problematic as for all intent and purposes, this article should follow the major screening outcomes paper.
The study question addressed in this paper is not about the effectiveness of screening or case finding, but about the intervention that followed. Specifically “the benefits of this intervention from the participants’ perspective”. A separate article from this study has addressed the issue of screening. The study question is given at the end of the introduction. We have tried to make the distinction

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clearer as to how this article's focus fits into the larger study agenda.

Design: If this is impact evaluation, the methods would answer the question, was this strategy acceptable to women and what did they perceive as its benefits. However, this paper does not state this as the aim and needs to address this.
We have altered the statement on the aim of the study at the end of the introduction to reflect this more specific formulation.
Representativeness of the study population to the actual population.
Again, the suthors did not answer this criticism adequately. They have acknowledged that providers were uncooperative and did not screen many women, but we dont know what proportion of women in the target group attend the clinics and whether 168 women is 10%, 40% or 60% of the potential for those who are reached. There is no acknowledgment that as more severaly abused women participated that this might represent a particular group. There is nothing wrong with this, but it needs an honest account.
It is not possible to know what % of women suffering from IPV in these communities attended the clinics and what % of them were identified. It is also not possible to compare the group that were identified with the group that were not. These limitations are elaborated on further in the discussion.
Table 3: Column three requires clarification and amendment.
Clarification has been added in the methods and a more direct reference given to the screening tool used.

Table 5: Would be improved with Helpful and very helpful collapsed and the unhelpful and harmful columns also with DK in the middle. Then the figures should be presented in ascending order
Done
Data well-presented: As data on differences between those who stayed and those who did not reurn are presnted in tables, they do not need to be emphasised in the text. It is the differences between those who particiopated and those who didnt that is important.
The data is not reiterated in the text and only a comment is made for the reader to highlight the fact that there were no significant differences. We feel that this is useful to point out to the reader.
As you have not described how the WHO adapted mental health measure is scored and how data were abstracted, it is hard to make sense of your presentation of mean scores and why women were 'suspected' of differing diagnoses.
Additional explanation has been added in the methods and a reference given to the tool used. Readers that require more detail can access the reference which is open access.
Discussion: There is no data to support the claim' how - referral to a psychiatric nurse' is valued etc at all. The tone is problematic, does not nuance the findings - ie that while a large minority experienced beneficial changes, one in three experienced no change and one in ten left the relationship. This should be described at the opening to the discussion and then what was valued should follow.
Table IV and V show that half of the women referred to the psych nurse attended and of those that attended 93% found it very helpful/helpful.

The introduction to the discussion has highlighted the key findings more clearly.

The statement 'contrary to popular belief (what belief?) most survivors...is this a SA belief?
Reworded.
It is unfortunate that Appendices A+B were not attached so this reviewer could not assess these.
Will request for these appendices to be made available to reviewers.
The limitations should include the differences between those who did and did not participate andf what this might mean in the SA primary care contect and what further testing might be required.

Some comment on the reasons for health care professional involvement (given the readership of this

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journal) would be helpful.
Some additional comments are added regarding the evaluation of the model in future to address some of these issues.

The introduction does argue for the relevance of IPV to health care services and professionals.
Limitation of the safety assessment should not be introduced here but better described in the methods section describing the intervention.
Moved to methods
Nevertheless the ethics clearances should mention the given number (Human Ethics Committee 2010# 234)
Done
Would you be willing to share your data? Cast your vote in our Online Poll

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Correction

Joyner K, Mash RJ. The value of intervening for intimate partner violence in South African primary care: project evaluation. *BMJ Open* 2011;1:e000254.

In the first key message of the article summary box "listening" was inadvertently omitted. The sentence should read: "Women diagnosed with IPV in primary care perceive benefit from an intervention characterised by both empathic, non-judgemental listening and a comprehensive approach to the clinical, mental, social and legal aspects." The journal apologises for this error.

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