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Title	**Novel educational program improves readiness to manage intimate partner violence within the fracture clinic: a pretest–posttest study**
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Reviewer 1	John Murnaghan
Institution	Holland Orthopaedic and Arthritic Centre, Surgery
General comments (author response in bold)	Topic of interest and importance for orthopaedic surgeons and health care practitioners working in emerg or fracture clinics. well described multicentre study. pretest-posttest design is acceptable in this setting and authors provided immediate and 3 months post completion scores. largest study of its kind to date. the measures provided indicate that the EDUCATE program had a significant effect on knowledge and several other subscores. authors reach reasonable conclusions about the value of this education program for health care providers in fracture clinics. **We would like to thank the reviewers for their helpful comments. We believe we have addressed each comment in the revised version of the manuscript.**
Reviewer 2	Josee Delisle RN, BScN, MSc (exp.surg.)
Institution	Hopital du Sacre Coeur de Montreal, Orthopaedics
General comments (author response in bold)	I read the article: ``Novel Educational Program Improves Readiness to Manage Intimate Partner Violence within the Fracture Clinic: A Pretest–Posttest Study`` The paper describes that Intimate Partner Violence (IPV) is prevalent among women presenting to fracture clinics and that health care providers (HCPs) may be unprepared to identify victims and provide appropriate support. An IPV educational program for HCPs who see patients in fracture clinics was developed. The purpose of this study was to measure the impact of this program, by comparing changes in scores of knowledge before and after the training. It was demonstrated that this educational program brought important improvements in participants' knowledge and ability to manage IPV cases. I think that the study is very important to the orthopaedic field. The background, methods and results are well described. The study was built upon previous work by increasing sample size with participants from different clinics which gives more power to the results. I think this article improves clinical practice and is transferable to other specialties. Minor comment: It might have been interesting to know the effect of previous IPV training effect alone on KABB (not in the regression model). Since half of the cohort had a previous IPV training, it would be interesting to see the impact of the program on their KABB compared to the participants that never had previous training. **We would like to thank the reviewers for their helpful comments. We believe we have addressed each comment in the revised version of the manuscript.**
Reviewer 3	Peter Wyer
Institution	Columbia University, Medicine
General comments (author response in bold)	This is an impressive study which largely accomplished what it set out to do. The important limitations pertain to things that clearly lay beyond the scope of your inquiry to address. In this regard, as you note in your own discussion of limitations, we do not know the extent to which a successful training program such as this actually improves the care of individuals at risk. This is not immediately answerable but should not be lost sight of. One thing you might consider in this regard is to, if possible, estimate the actual number per 1,000 patients seen in orthopedic clinics who are victims of IPV. This would provide a picture of whether the over-riding issue is one that can fruitfully be addressed in subsequent research. A second suggestion is to avoid the impression that your results reflect a practical significance that they in fact do not. You do explain the absence of a directly validated MCID in this area and your approach to a statistical derivation is clearly supported by the literature. However, your use of the term "clinically

	significant" throughout the manuscript can easily lull the reader into an inflated view. Perhaps you could use the phrase "statistically significant and important", which is less misleading in this regard. Otherwise, the issues detected by this reviewer are all relatively minor in nature. Perhaps the most prominent of them has to do with the propensities towards bias and other confounding inherent in a pre- test-retest design. This is important since knowledge gain served as your principal outcome and the issue even figures in the proposed title of your manuscript. The problems here have been well delineated by Cook et al (Adv in Health Sci Educ (2010) 15:455–464). This needs to be addressed in your limitations segment and you also need to verify that the same knowledge test was administered 3 times in the course of your study-prior to the training, immediately after the training and 3 months following the training. The 3 month mean knowledge scores were minimally lower than with the immediate post-test, consistent with a decay of retention. However, the effect of biases might well still be present and might impact on interpretation of the overall results. In this same regard, you need to report whether or not the results of the test-i.e. the correct answers- were supplied to the participants as part of the educational experience at any point during the protocol. Other issues are largely a matter of clarification and polishing of your reporting and presentation. SPECIFIC (Page numbers correspond to the pagination of the manuscript, not of the PDF) INTRODUCTION P. 1, L. 17-22: This is convincing attestation of the burden of suffering that relates to the clinical context you are addressing. If you could also report the statistics as total annual numbers and/or the numbers of patients seen per year in a typical fracture clinic who fulfill the criteria of violent of abuse, it would address the suggestion made earlier in this review and illuminate the feasibility of research that directly assessed the effectiveness of educational interventions such as your own in impacting clinical outcomes. This information could be placed in the discussion or limitations segments as you are inclined. P. 1, L. 32-33: Although the 3-month outcomes are your primary targets, I suggest you also include the fact that your assessments were performed at baseline and immediately after the intervention. Secondly, I find the abbreviation of "KABB" potentially confusing to readers. Acknowledging that the abbreviation is used in the published literature in this area, I suggest either spelling it out multiple times in the manuscript and/or including an appropriate box insert that makes it easy for the readers to remind themselves of what it stands for. Capitalizing and bolding the first letters of the components that precede first use of the abbreviation might also increase clarity and reader comfort. METHODS P. 1, L. 50-51: This is an important aspect of your overall design. Although clinical outcomes were clearly beyond the reach of your inquiry, tracking how many of your subjects ACTUALLY ENGAGED in training activity should have been a feasible assessment. Do you have data on this? P. 2, L. 3-4: This is a potentially important lapse in the reporting in the manuscript. The reader needs to know how many participants received only the telephone training, whether the telephone trainees received the other components of the intervention, and, if the number was not trivial, the outcomes associated with telephone only versus in-person access to the training. You might also explain why you elected to include these subjects instead of simply excluding them. If the numbers are substantial then you clearly need to include this category in your regression analysis and report sensitivity

	analyses with and without their inclusion. P. 2, L. 27-28: This suggests that the exact same form was used for all three assessments, including the "knowledge test" which you defined as your primary outcomes measure. As already noted, you need to clearly acknowledge that the identical test form was used for all three assessments and also divulge whether participants were supplied at any point with corrected answer sheets. The integrity of your assessment protocol hinges on this. P. 2, L. 51-53: The reviewer appreciates your attempts to be fully transparent on this issue, which relates to the fundamental structural limitation of your research inquiry, which in fact you have acknowledged later in the manuscript. I suggest that, in the limitations segment, you be very specific in acknowledging that the MCID, upon which your various assertions of "clinical significance" are based, is, ultimately arbitrary and that direct empirical validation will be required to determine the actual, practical, or "clinical", significance of the differences you have observed. See also the related suggestion in the general comments. P. 3, L. 31-34: See previous comment. RESULTS P. 3, L. 38-41 and Figure 1: Regarding the text, please include the tally of the subjects who were only trained by telephone and who did not receive the full educational intervention as it was designed. Regarding the FIGURE- the visual clarity would be improved substantially if you eliminated the split trajectory below the "140 participants enrolled" Box. This could be done easily provided that none of the 4 individuals who were lost to the immediate post-test follow-up re-emerged to take the 3-month follow-up assessments. Assuming this is the case, the whole thing can be consolidated into a single flow trajectory and be much more intuitively approachable by the reader. P. 3, L. 46-47 and Table 3: There is very little information included in the table since the right hand column is simple the middle column divided by 2. Perhaps this could be further simplified and folded into the text and/or into other tables. P. 3, L. 49-P. 4, L. 16 and Tables 4 and 5: There is unnecessary redundancy here in both text and tables. Turning to the tables, there is a great deal of redundancy in both of them. For example, the values in columns 4 and 5 are virtually identical to those in columns 6 and 7 save for those in the bottom 2 rows which themselves are also very close. The reader would like to be able to view the full progression from baseline through the immediate post course assessment through the 3 month follow up. Eliminating the redundant columns would allow the data to be presented in a single table which would in turn provide much more useful information to the readers with substantially decreased work on their part. Once this has been done the redundant paragraph in the text could also be easily eliminated. The results of the alternative analysis could be presented in a different integrated table, perhaps as an online appendix. INTERPRETATION P. 4, L. 23-26: This is an assertion that is in fact not directly supported by your findings since, with the exception of the knowledge text, the outcomes were all self-reported. Hence, this needs to be softened. P. 5, L. 14-16: This sentence is unintelligible. **We would like to thank the reviewers for their helpful comments. We believe we have addressed each comment in the revised version of the manuscript.**

Reviewer 4	Natalia Lewis
Institution	Bristol, UK
General comments (author response in bold)	Thank you for offering me the opportunity to review the above manuscript. It reports pre-post evaluation of a bespoke IPV training for health-care practitioners from fracture clinics (n=140). The manuscript is well structured and written in a way that is understandable to general medical readership. I would like to acknowledge authors' clarity in reporting and suggest revisions to: 1) put study in the context of the evidence-based health-care response to IPV, 2) improve reporting of the intervention, and 3) strengthen Method section. 1. Several systematic reviews showed that IPV training in isolation does not result in practitioners' behaviour change and does not create consistent sustainable change in health-care response to IPV. There is an international consensus that IPV training should be incorporated in an on-going support at individual, organisation, and system levels (see Zaher et al, 2014 Effect of domestic violence training: systematic review of randomized controlled trials; Turner et al, 2017 Interventions to improve the response of professionals to children exposed to domestic violence and abuse: a systematic review). This evidence should be acknowledged in Introduction. The results should be interpreted considering the current evidence and clinical guidelines (see WHO, 2013 Responding to intimate partner violence and sexual violence against women). 2. p.1, lines 26-33. How the EDUCATE programme was developed? Could you provide reference to the paper describing this process, theoretical underpinning, testing? It would be helpful to know how EDUCATE differs from other evidence-based IPV interventions for health-care practitioners (e.g., IRIS) and what programme components were "developed specifically for delivery in a fracture clinic setting". 3. I suggest the following revisions in Methods: p.1, line 42. How the six fracture clinics were selected and recruited? p.1, line 53. How the local IPV champions were identified? p.1, line 54. How many local champions were identified, trained in person and remotely? p.2, line 11. Description of the EDUCATE programme in text and Table 1 is not enough for replication or comparison with similar interventions. The TIDieR checklist (http://www.equator-network.org/reporting-guidelines/tidier/) is recommended by the Equator network for describing intervention. p.2, line 24 Sentence "Individuals who met all eligibility criteria … " can be deleted. It repeats what has been said on p.1, lines 44-47. p.2, lines 28-30. Format of data collection is not clear – paper/online/both?, self-administered/researcher-administered?, in-house/postal? p.2, lines 33-47. PREMIS is a standardised validated tool. What internal validity (Cronbach α) did it demonstrate in your study? This will allow to judge how trustworthy your findings are. p.2, lines 41-47. Justification of the choice of the primary and secondary outcomes. p.3, lines 14-15. Explain why were these independent variables chosen? Were they pre-specified before looking at the data and the analysis was carried out? p.3, lines 50-55 and further. When reporting differences in means in text, provide 95% CIs and p in brackets. p.5, lines 21-22. Add that you also did not evaluate the impact of the training on practitioners' behaviour or daily practice. I understood that you also did not evaluate the acceptability of the training to professionals and the feasibility of its delivery. p.5, lines 29-36. I think that your implementation recommendation is not supported by the results. WHO and national professional institutions recommend IPV training of front-line health-care professionals across all settings. You have demonstrated that the EDUCATE programme was associated with increased professionals' knowledge/attitudes/beliefs/self-reported behaviour at 3 months. Next logical step is to roll it out and test in a more robust study design (outcome evaluation, process

	evaluation, economic evaluation). I hope my suggestion will help to improve your manuscript. **We would like to thank the reviewers for their helpful comments. We believe we have addressed each comment in the revised version of the manuscript.**