Author's response to reviews

Title: A description of physical therapists' knowledge in managing musculoskeletal conditions

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Author's response to reviews: see over
Dear Editor:

We are submitting a revised paper previously titled “A description of cognition in musculoskeletal medicine among physical therapists” (#1654031551520128) for publication consideration in BMC Musculoskeletal Disorders. Based on comments from the reviewers, the revised title of the paper is “A description of physical therapists’ knowledge in managing musculoskeletal conditions”.

We addressed each of the minor revisions and have provided a detailed point-by-point response to Dr. Woolf and Dr. Jette’s comments. Please contact me by phone at (210) 364-7410 or by email at childsjd@sbcglobal.net if you have any questions or require additional information. Thank you for your consideration of this manuscript.

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Response to Dr. Woolf’s Comments

1) The findings based on a knowledge test cannot be used to argue for changing ways of delivering care, as in the Conclusion - evidence of better patient outcomes is required.

   We agree that evidence of better outcomes (or similar outcomes at a lower cost) is necessary before recommending changes in care delivery processes. However, several clinical studies (referenced in the paper) have already been done, the results of which support a mode of healthcare delivery in which patients are able to seek care from a physical therapist without physician referral. As we state at the end of the introduction, these data may help to corroborate findings from the clinical studies demonstrating the benefits of direct access physical therapy.

2) The conclusion suggests direct comparison of results between therapists and physicians etc but it is the literature that has been compared. The opening sentence needs to be changed to state "... in musculoskeletal medicine than previously reported for medical students ...."

   We modified the text in the conclusion on p. 15 to reflect that we are not making a direct comparison but rather are comparing these data to previous studies using the same examination. We now say, “In comparison to previous studies among physicians....”

3) In the background the authors state that the WHO states that training programmes are inadequate. This in fact was the views of the authors in a paper published in the WHO Bulletin and is not the published view of the WHO.

   We removed direct reference to the WHO to avoid the perception that this sentiment represents the WHO’s official position on this issue.
Response to Dr. Jette’s Comments

1) The use of the term “cognition” in the title and text strikes me as odd. In my opinion, the correct term is “knowledge”. My dictionary gives a definition of cognition as “the mental faculty or process of acquiring knowledge”. The definition for knowledge is “awareness or possession of information, facts, ideas, truths.” I do not believe that this study provides information on a process.

The terminology used to describe the construct being assessed by this musculoskeletal examination has differed in the various publications. The construct was originally referred to by Freedman and Bernstein (Freedman and Bernstein, 1998) as “competency”, which is also not an accurate reflection of the underlying construct. Since competency conveys the idea of being able to effectively deliver care (in contrast to a cognitive understanding of the knowledge needed to deliver care), Matzkin et al referred to the construct as “cognition”. (Matzkin et al., 2005) Our rationale for using this word was to remain consistent with this terminology. Nevertheless, we agree that the term “knowledge” is a more accurate reflection of the construct being assessed. The word “knowledge” has been substituted for the word “cognition” throughout the paper.

2) I also don’t think that there is a need for the term “musculoskeletal medicine.” In fact, it is somewhat misleading. Physical therapists do not practice medicine, nor do they wish to or claim to. The items on the examination, as I read them, do not necessarily test knowledge of medicine. The examination seems to include items related to the anatomy and physiology of the musculoskeletal system.

We did not intend to convey the notion that physical therapists are practicing medicine. To avoid confusion, we have changed the title to read, “A description of physical therapists’ knowledge in managing musculoskeletal conditions”. We have also replaced the term “musculoskeletal medicine” with the phrase “managing musculoskeletal conditions” in instances throughout the paper referring to physical therapists’ knowledge. We left the term “musculoskeletal medicine” when it refers to physicians.

3) The question posed by the authors is an important one for the professions of physical therapy and medicine. In my opinion, the authors downplay the relevance of the question by referring to the issues of safety and effectiveness of care by physical therapists when they function as the first contact for a patient's musculoskeletal complaint late in the discussion. In my opinion, this is part of the need and justification for the study. Otherwise it is not really clear why the question is important to explore.

We agree with the reviewer’s comment and have bolstered the rationale for the study by shifting some of the text that now appears late in the discussion to the introduction on pp. 4-5.
4) A strength of the design is the use of a well validated outcomes instrument. In my opinion, there should be some detail about the content and scoring rather than simply referring to previous studies. As a reader, I find approach this frustrating. This issue becomes important, I think, when the results differ depending on whether one looks at actual scores or pass rates. How were the pass rates established?

   We have added specific details on how the examination was scored on p. 8. We believe this will clarify the differing implications for the overall score versus the passing rate.

5) In looking at the references that describe the examination, I note that the items are open-ended. For that reason answers would be open to some interpretation. Therefore, the expertise of the raters should be described.

   The answer key published by Freedman and Bernstein is actually fairly specific, minimizing the potential for excessive variability in how the responses are scored. The high reliability our raters achieved in the scoring process supports this contention. Nevertheless, there is still room for minor variation based on interpretation, thus we add text to describe the expertise of the raters on p. 8. We also now state that each rater was trained in the scoring procedures by one of the investigators.

6) It is unclear why the score for only one rater was used when two raters evaluated each question. I imagine that the details about ICCs on page 8 might be an attempt to justify this approach, but it is not clear to the reader what the statistics refer to. This section needs to be written more clearly.

   We only utilized the data from a single rater because the ICC was adequate (>90). Incorporating the average of two raters does not further reduce variability. Using data from a single rater is also consistent with the procedures utilized in the Freedman and Bernstein(Freedman and Bernstein, 1998) and Matzkin et al(Matzkin et al., 2005) studies. We made this clearer in the text on p. 9.

7) It is unclear why the two samples were used. The choice of two samples should be justified in light of the purpose. The purpose statement notes that knowledge among physical therapists will be described, but, in fact, there are both students and physical therapists sampled. In some places in the manuscript, the students are referred to as physical therapists. They are not; so the terms need to be carefully applied. On page 7, one sample is referred to as “patients.”

   The physical therapy students had completed all of the didactic material related to managing musculoskeletal conditions and, in most cases, had completed all of their clinical affiliations as well. In fact, most of the students who participated completed the study during the final 1-2 weeks before graduation. Since these individuals were in the terminal phase of training, we would argue that the results likely reflect a level of knowledge comparable to what might be observed among
a new graduate. Nevertheless, we have been careful to make a distinction between
the data that represents knowledge among physical therapy students versus
licensed physical therapists.

The examination was rigorously developed and validated among physician
interns, medical students and residents, and a variety of physician specialists.
Given the variety of experience levels represented by these subgroups, it was
important to assess knowledge among physical therapists with comparable levels
of experience. This rationale is stated on p. 5.

8) One sample is noted to include all physical therapists from the US military. The reader
should know how many people this is so that the response rate can be determined. The
response rate is unclear for this sample. Similarly, although the response rate from the
schools is noted, it is fairly clear from the table that not all students who were eligible
responded. The response rate is, therefore, not fully explored. Details should be provided
or this issue should be explored as a limitation.

We agree and have added the response rate in the results section on pp. 9-10. The
potential for selection bias was previously mentioned as a limitation and remains.

9) The PT programs are listed by name in Table 1. That makes me a bit uncomfortable. I
do not think that Table 1 is particularly helpful at any rate and could be deleted.

Table 1 was removed. The text now does not refer to any specific programs.

10) Because the authors chose to report analyses for both numerical score and proportion
passing, there are two different findings. In my opinion, there needs to be some
justification for examining the results in both ways and help to the reader in interpreting
the discrepancy in findings. What does it mean to have actual scores differ, and pass rates
not differ.

The potential interpretation for discrepant findings with respect to the overall
score and passing rate was added on pp. 12-13.

11) In the manuscript, the authors note that they cannot really compare their results with
those obtained from other studies because they were from unrelated studies. This
statement is confusing to me. It is my understanding that the studies used the same
instrument with the same scoring. One could, therefore, determine means and confidence
intervals. These calculations would allow some real comparisons. The figure is helpful,
but it only includes the actual scores. The narrative includes a statement that the pass rate
for PT students was 24%. How does this compare with the physician groups?

We appreciate the reviewer’s request that we be less descriptive and provide more
inferential statistics regarding the comparisons between physical therapy students,
licensed physical therapists, and physicians. We also recognize that the current
reviewer may not have been privy to the previous reviews of the paper. In fact, we
did report inferential statistics in the initial submission of the paper by calculating 95% confidence intervals around the means using the standard deviations, which have been consistently reported in the previous literature. However, the reviewer asked us to delete these comparisons in deference to more descriptive statistics since the data were collected from separate studies (despite having used an identical instrument and scoring procedures). This is the rationale for providing the figure instead. We added the passing rate for physician interns in the results section.

12) The discussion (page 11) includes a statement that the results support the safety and effectiveness of care by PTs in an autonomous setting. The following sentence, however, does not address this issue. The issue of cost is unrelated to the results and the purpose of this study. I am also not really sure that the results do support this contention given the average score and pass rates. Help the reader to understand what the scores mean for safety and effectiveness.

We agree that we cannot infer anything about the safety and/or cost-effectiveness of direct access physical therapy based on the results of this study. As pointed out by Dr. Woolf, evidence of better outcomes (or similar outcomes at a lower cost) is necessary before recommending changes in care delivery processes. In this case, many of the clinical studies (referenced in the paper) have already been done to support the safety and cost-effectiveness of direct access physical therapy. The data from this study simply help to corroborate findings from the clinical studies by demonstrating that experienced physical therapists have the requisite knowledge necessary for direct access physical therapy. We have eliminated the sentence about the safety and cost-effectiveness of direct access physical therapy care in its current location to avoid conveying the notion that these data have implications on the safety or effectiveness of direct access physical therapy.

13) One of the limitations that is not explored is the fact that there are no data about the students’ clinical education experiences. Because programs in PT do not have standardized curricula, the reader does not know how much clinical experience the students have or how much exposure to orthopedic curriculum (academic and clinical) they have. These potential differences could have an effect on the findings.

We now include the potential variability in orthopaedic clinical education experience as a potential confounder of performance on the examination on p. 13-14.

Reference List
