Author’s response to reviews

Title: Symptomatic pes planus in children: a synthesis of allied health professional practices.

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Author’s response to reviews:

Response to reviewer reports

We appreciate the time it has taken to review our manuscript and thank the reviewers for their insightful and constructive comments. Please see below for a response to these. All additions to the text are highlighted in yellow. For clarity and ease of reviewing the manuscript, we have not highlighted/tracked the text removed from the manuscript.

Handling Editor

Please can you review each of the reviewers comments and revise the manuscript. Please can you add under limitations that the study was based in the UK only and cannot be generalise to other practitioners across the world, unless further work is conducted from an international perspective. Please avoid personal communication. Final comment is remove any speculative comments in the discussion.

Thank-you. We have undertaken a full review of the reviewer comments and revised the manuscript. In response to your points:
We have added the UK context and acknowledge that this limits the external validity of the work.

We have removed the personal communication.

We have refined the text throughout the discussion.

Reviewer #1:

After carefully reading this manuscript, I must say that, from my point of view, the authors have done research on an important topic: Assessment and management of symptomatic pes planus in children and synthesis of allied health professional practices. This could be interesting to industrial laboratories, government research centers, universities, private research organizations, and independent scientists, that frequently work in this area.

It could give them a wider concept about and helps advance recognition of the input of different health professionals into the management of this condition, and helps inform the need for further multi-professional work in this area.

This is an interesting aim with the evaluation of the planus foot scope. I have considered the quality of the manuscript redaction and presentation, the quality of the research methodology, the novelty and importance of the observations, and the appropriateness for the Journal's readers (according with the Journal's name) and I think that this manuscript joins adequate conditions to be accepted for publication in Journal Foot and Ankle Research.

I have no real problems with the text of this paper, only some suggestions that are mentioned below. It appears as if the authors have done the study well and have answered an interesting clinical question with their work.

Also, there are a minor concern with the manuscript that require attention prior to publication. These will be discussed below relative to the sections of the manuscript.

Thank-you for your feedback. Please see below for a full account of the changes made to the manuscript.
TITLE

The title of this manuscript are a little long. Perhaps a more concise version for clarity, interest and ease of read.

We have revised the title. This now reads:


INTRODUCTION

I suggest that background should be improved, with more details about the importance the quality of life in the planus foot. It is indeed important paper but it lacks several critical references, in which it was presented related with this condition, and it should be emphasized in the INTRODUCTION or Discussion of the authors' paper. More info on the impact of foot arch height on quality of life in 6-12 year olds https://www.ncbi.nlm.nih.gov/pubmed/?term=ana+requeijo+constenla and Foot Arch Height and Quality of Life in Adults: A Strobe Observational Study https://www.ncbi.nlm.nih.gov/pubmed/?term=gonzalo+barros+garcia

We appreciate the comment and understand why this has been proposed. However, the focus of the manuscript is to explore the assessment and management strategies adopted by health professionals. This hopefully explains why we haven’t cited all the work on this topic. There are review papers that we have cited which we feel will direct readers who want to read more on the topic. We have included the López López (2014) study but have not included the López López (2018) study which focuses on adults.

METHODS

If some statement has been taken into account, the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) criteria should be cited.
We have referenced the Checklist for Reporting Results of Internet E-Surveys (CHERRIES). We do not feel that the STROBE criteria is relevant for this study as our data collection did not involve any observation(s).

Likewise more detail about information calculate sample size and data should be provided. Also, please need include the data and record code and all information related with the ethics committee and explain aspects ethics and legal requirement about this research.

We have reviewed this in our manuscript. Given our response above, we have emphasised that this is a descriptive study. We have added text to explain this. We have also removed the significance tests from our results as our primary intention was not hypothesis testing. We have disclosed that ethical approval was granted prior to data collection and have added the institutional details.

RESULTS

The results is clear and concise with appropriate statistical analysis been performed appropriately and rigorously.

Thank-you. As outlined above, we have removed the significance tests. No further changes have been made.

DISCUSSION

The discussion is adequate and considers the wide available body of literature on either about this topic.

Thank-you. No further changes have been made.
CONCLUSIONS

Novel and interesting study.

Thank-you.

FIGURES AND TABLES:

Correct.

Reviewer #2: Thank you for your submission and the work and effort to do so are appreciated.

I am sorry that I have to disappoint the authors in this instance as this exploration, whilst an interesting extension of Kane's 2015 study, is based on a small and limited sample, such that meaningful conclusions beyond perhaps preliminary trends, are not reasonable.

Further, it should be noted that Kane's study, from which this survey was drawn, was directed at a small number of physical therapists who cared for children with delayed gross motor development and neurological conditions.

Overall issues, which have largely been conveyed, and which limit further, meaningful conclusions include:

- small sample n = 55, drawn from non-defined population size
- low response rate
- differing and disparate sub groups (n=6; 16; 33) with differing experience levels and case loads
- cross sectional survey data
- broad, varying descriptive for 'symptomatic' cases (ie no differentiation of foot pain, functional impairment, proximal joint problems, reduced quality of life)

In essence, it is likely 'drawing a long bow' to conclude practice inconsistencies (or any form of conclusion) given the small and limited sampling, wide definitions of symptomatic, disparate sub groups.
We acknowledge the comments raised by the reviewer and hope that the refinements made to the manuscript (in response to the handling editor, reviewer 1 and 3) will offer less of a “long bow”. We acknowledge the limitations with the survey but we feel that the data represents some broader issues which need exploration (e.g. paediatric workforce within the UK). We don’t agree that the sample is disparate and in fact, the collective experiences reported by the participants provides some novel data which explains current practices within the UK. We don’t see the cross-sectional survey design as a limitation although acknowledge the limitations with the design. Cross-sectional surveys are appropriate for a descriptive study and this is in keeping with many surveys published within the journal (Brenton-Rule et al., 2014; Paterson et al., 2014; Tehan and Chuter, 2015; Graham & Williams, 2018; Siddle et al., 2018; Chapman et al. 2019; Stevens et al., 2019 etc). The survey has allowed us to explore many aspects of the clinical interaction and we have been able to report data which explored indications for treatment, indications for withdrawal of treatment, common approaches to outcome measurement data. To our knowledge, this data is not reported in other studies.

Reviewer #3: Thank you for giving me the opportunity to review the following survey study Assessment and management of symptomatic pes planus in children: a synthesis of allied health professional practices. The premise of the study is of interest to this publication.

The methodology of the survey appears sound as the aims and objectives are clearly defined as is the population of interest, and a systematic approach was used to develop the questionnaire and domains within, with appropriate piloting of the survey tool used. The results generated and conclusions drawn could therefore be considered sound.

However; there are some points that if addressed I feel would improve this paper prior to publication.

Thank-you for your feedback. Please see below for a full account of the changes made to the manuscript.

Background

This is well written with supportive references. Some minor comments
Line 56 Is Flexible Pes Planus completely non osseous there may be skeletal dimorphism that may account for this e.g. Accessory bones, Articular facets (Kothari, Bhuva, Stebbins, Zavatsky, & Theologis, 2016)

This is an interesting comment. Kothari et al report that there may be differences in STJ morphology but this does not imply osseous involvement. From our experience, osseous involvement would imply some form adhesions between the bones affecting the joint articulation, and thus the range and quality of motion. This would apply in a rigid flatfoot (e.g. tarsal coalition and skewfoot). All survey respondents were asked to complete the survey based on experiences of symptomatic (and flexible i.e. non-osseous) pes planus. Accessory bones would present with typical symptoms which are different to that of pes planus. We hope that the survey respondents have appropriate knowledge and experience of the conditions to isolate these clinical presentations.

Line 57
"The flexible variant can be further characterised as asymptomatic (often referred to as physiological) or symptomatic (from either idiopathic or non idiopathic aetiology)."

I feel a citation here to support this statement is required.

Thank-you. We have added an additional citation into the manuscript.

Line 64
"however current opinion supports intervention for symptomatic presentations only [4, 6]."

I was surprised that the more modern Dars 2018 Delphi Survey was not also used here to back up this statement since you reference this earlier in the background.

We have cited this work in the manuscript and have added this here.
Methods

Was an a priori sample size calculated or theorised for the survey?

We did not undertake a priori sample size. There were difficulties with accessing data which offered a robust overview of the typical number of the different professions working in paediatrics (i.e. the target population). This is a descriptive study and we acknowledge the limitations with the sample size (and clarity around response rate) in the limitations. Further, we have refined any speculative comments in the discussion.

I would suggest adding the modified survey to enable the reader to assess the quality of it and ensure no ambiguous or biased terminology was used.

We have included the survey template as an appendix.

Line 133

Further elaboration is required as to how the Authors categorised the data what type of synthesis was used how was this validated how were disagreements resolved?

The free-text responses were not considered qualitative data due to their limited conceptual richness and as such, were coded into quantitative units to enable descriptive analysis. We have added further detail about this into the manuscript.

Line 136

I am uncertain what was the purpose and validity of analysing these two question in this way with respect to the other data generated in the survey, Please state for the reader which questions these were.

We agree with this and have removed the analysis from the manuscript.
Results

There appears to be a rather low completion rate for this study however you do clearly state this as a limiting factor and the affect this may have on external validity.

Line 161 please review sentence "and 3 had roles where 75 - 99% of their caseload paediatric foot. "

This has been removed.

I feel further graphical or table representation is required to showcase your results there are a number of results not presented in the tables given.

We have added a table of demographics, as well as some additional text within the results.

The Demographics section was somewhat confusing to see the overall data a table here would support your discussion.

We have simplified the text which describes the demographics and added a table. The table that we have added outlines: frequency of response from profession; typical practice; percentage caseload (paediatrics); years of experience in profession; years of experience (paediatrics).

Assessment

Further breakdown of the types of neurological assessment performed by the sample may be useful (Motor, Sensory)

We agree that this would be useful but don’t have this data.
Intervention

Line 206-213

In relation to professional use of prefabricated orthosis I could not find this data in your table, the odds ratio of this may have been interesting to read.

Our interest was in determining whether orthoses were a first line intervention. Our comparison was across the professions but we haven’t explored the specific orthotic choices between the professions.

Line 213-216 I assume this is categorical / qualitative data I think this is interesting and again I feel it would be better showcased in table form.

Respondents were asked to add a free text reply. The responses were categorised and presented as descriptives. Unfortunately, we don’t have any additional data to add.

Line 216 -221 Was there any discrepancy here by profession again a breakdown on the data in table form may help clarify this.

We have added to the text that there was one podiatrist who would intervene in the asymptomatic presentation.

Discussion

I was surprised not to see this recent paper in your work (Gijon-Nogueron, Martinez-Nova, Alfageme-Garcia, Montes-Alguacil, & Evans, 2019) in relation to discussion on possible criteria for assessment of pes planus.
We are aware of the work reporting normative values for the Foot Posture Index but don’t see where this applies in our discussion. It is beyond the scope of the discussion to cite all studies related to the clinical assessment of pes planus. We have cited a systematic review on this topic of clinical assessment of pes planus and this work includes reference to the FPI and other measures.

Banwell HA, Paris ME, Mackintosh S, Williams CM: Paediatric flexible flat foot: how are we measuring it and are we getting it right? A systematic review. J Foot Ankle Res