Author’s response to reviews

Title: "Principal Component Analysis of the Relationship Between Pelvic Inclination and Lumbar Lordosis"

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Author’s response to reviews:

Dear editor

This study began because the widely assumed biomechanical relationship between PI and LL did not appear to exist within surface topography scanning data in a clinical setting.

On page 2 of the Radiographic Measurement Manual produced by the Spinal Deformity Study Group it states that “understanding normal spinal anatomy is a prerequisite to identifying, treating and correcting spinal deformity”. We propose that understanding the biomechanical relationship between pelvic inclination and lumbar lordosis is no less critical when it comes to lumbopelvic deformity and dysfunction.

Rehabilitation professionals (e.g. physiotherapists, chiropractors, occupational therapists, etc…) provide corrective therapies in non-surgical and post-surgical treatment of lumbopelvic deformity and dysfunction. Without a quantification of the biomechanical relationship between pelvic inclination and lumbar lordosis there cannot be an accurate understanding of what constitutes effective treatment nor effective communication between caregivers when comparing best practice solutions.

Surface topography scanning has been established by other authors as an accurate and reliable form of measurement. We used this technology to record our data and principal component analysis to analyze the data. We used clinic assessment results from patients before and after a
course of treatment. Just to be clear, we are not making any claims regarding the therapeutic interventions used. The purpose was to provoke postural change and then measure how various spinal and pelvic parameters changed, literally and in relation to one another. Here we report some curious findings of interest to both the academic and clinical community.

This is a conversation that needs to be developed further. You and your reviewers have helped us begin this conversation more clearly. We hope you are satisfied with this rendition and look forward to seeing this work published.

We would like to thank the reviewers for their comments and suggestions and the editorial team for giving us more time to fully address them. We believe the insightful comments provided by the reviewers have strengthened our paper to where it now provides clear and useful information about the relationship between Pelvic Inclination/Tilt and Lumbar Lordosis. Moreover, it now constitutes a piece providing a much more solid foundation that can be built upon via future research in this field. We are most grateful.

All amendments in the revised manuscript have been highlighted in one file. New text is in yellow highlight. The items changed are indicated and discussed below. Our manuscript originally entitled “Pelvic Inclination Does Not Correlate with Lordotic Angle”, is now titled “A Biomechanical Study of the Relationship Between Pelvic Inclination and Lumbar Lordosis” as per suggestions from reviewers this title more accurately reflects out study. Following the suggestions of the reviewers we have made major revisions to the paper and believe we have successfully addressed their biggest concerns.

Comments from the reviewers and author responses:

Reviewer 1

The authors present a study in which they measured the geometrical parameters of the spine and pelvis via surface tomography, and then performed principal component analysis (PCA) on the obtained data to observe the dependency among the measured parameters as well as trends in the parameters that contribute to their variability. Their results show that there is no relationship between pelvic inclination and lumbar lordosis. Nevertheless, the authors need to elaborate more on their approach, especially from the point of view of data acquisition (accuracy, reliability and repeatability of surface topography measurements) as well as principal component analysis (from
the perspective of the stopping rule that defines the degree of the variance in the retained principal components). Detailed comments are provided below:

Comment #1: The title of the manuscript is in my opinion too general, as the conclusions to which the authors came need to be further validated, which was acknowledged also by the authors. I would propose that they somehow include in the title the fact that these findings are "based on surface topography measurements" and "principal component analysis".

Response: We appreciate the insights and questions from reviewer one which have helped us to realize that many of the concerns expressed by all reviewers were in some way linked to the title of the manuscript. Our manuscript originally entitled “Pelvic Inclination Does Not Correlate with Lordotic Angle”, is now titled “A Biomechanical Study of the Relationship Between Pelvic Inclination and Lumbar Lordosis”. We feel the current title more accurately communicates that this study was undertaken to explore how bony alignment relationships change in relationship to one another. We did not set out to prove the efficacy of a given therapeutic system to bring about an intended therapeutic outcome, but to objectively monitor change of spinal and pelvic alignment measures and determine if those changes were in any way related to one another.

Reviewer One’s comments also helped us to realize that we needed to elaborate more on the accuracy, repeatability and reliability of surface topography (ST) scanning and the appropriateness of principal component analysis (PCA) for the purposes of a study of this kind.

Surface topography scanning has been established by other authors as an accurate and reliable form of measurement.

Comment #2: Abstract appears to be too long, especially the Background and Methods sections. The Background part is somehow a speculation, except for the last sentence, which describes the aim of the study. The abbreviation PI is commonly used for pelvic incidence, and not for pelvic inclination. Expressions like "Golden Calf" are not adequate for scientific publications. In the Methods part, the expression "to measure the spine and pelvis" is ambiguous - please be more precise, as the spine and pelvis are not being measured, but specific morphological parameters are obtained through surface topography measurements. The exact model and manufacturer of the scanner is usually not reported in the abstract. Please state your hypothesis clearly in the Background part and refer to it clearly in the Conclusion part. Please use abbreviations consistently once defined (e.g. LL was defined, but "lumbar lordosis" still appears in the Results and Conclusions part).
Response: We thank Reviewer One for these insights. The Abstract has been edited for length, precision and clarity. We have also done our best to use our abbreviations consistently once defined.

Comment #3: In the first paragraph of section Introduction, the authors state that the morphological parameter named "pelvic inclination" was defined by Legaye et al. (1998, 2011). However, no such term can be found in the cited publications. The authors should be more precise and follow the established terms: do they mean pelvic tilt (or tilting) - PT, pelvic incidence - PI, or a different one?

Response: The reference to Legaye et al. was an error and the one concern expressed by all three reviewers. Unfortunately, the terminology used by some authors in North America differs from some authors in Europe. To eliminate further confusion, we used The Spinal Deformity Study Group’s Radiographic Measurement Manual as a common terminology reference and is now cited. Often the term “pelvic tilt” is used in place of “pelvic inclination”. We are not able to use the abbreviation (PT) to refer to “pelvic tilt” as PT is already used to refer to “pelvic torsion” in our original data. When you see PI please remember that refers to “pelvic inclination” and NOT pelvic incidence.

Comment #4: The whole data analysis that leads to the obtained conclusions is based on the accuracy of the surface topography scanner in performing the described measurements of pelvic and spinal parameters. Could the authors perform experiments to assess the reproducibility, reliability and accuracy? This needs to be presented, as for example the variability in reproducibility may show or clear the correlation among some of the observed parameters, which was not or was, respectively, observed by the authors. On the other hand, from the cited work of Knott et al. (2016), it can be observed that while there is a strong correlation between lumbar lordosis measured by surface topography and X-rays (Pearson correlation of 0.867), the average difference in measurements was ±9.7 degrees, which is quite large. On the other hand, weak correlation was observed for pelvic obliquity (0.223) with a relatively low average difference of ±2.3 degrees. How do these results influence the perspective and conclusions of the present study?

Response: Many x-ray studies show that there is variance between measurements that are repeated by the same examiner, and when repeated by different examiners. The variance in the
ST measurements is similar to those. Also, the repeatability of ST is very good. Previous authors have come to the conclusion that ST is a reliable instrument for measurement.


Comment #5: The resolution of all figures is too low to observe the depicted elements. I do not know if this the authors are responsible for this or the submission system, but it appears that the resolution of figures is 72 dpi, which is ridiculous. For a proper assessment, they should be of 600 dpi (or at least of 300 dpi). Moreover, in Figure 1 the morphological parameters have to be marked in the figure as well, and not only reported in its caption.

Response: The image files included in the manuscript have been enlarged and labeled as requested.

Comment #6: The subfigures in Figure 2 should be presented in a more proper way. If the data are binned, the abscissa should show the values that occurred for each measured parameter, and not the label "Population" (this label actually wrongly refers to the actual population of observed subjects).

Response: The subfigures have been labelled as requested.

Comment #7: The authors mention that they conducted the Grubb's test for outliers, however, no results are reported. How many outliers were there, why did the outliers occur, etc.? Or it
reported in section Results - Distribution of the data, where they report that "no statistically significant outliers"? If so, then they have to report in more in detail by referring to the above-mentioned test.

Response: “Grubbs’ test at a significance level of 0.05 did not detect any statistically significant outliers within any of the parameters.” Is now included in text in the results section.

Comment #8: In the caption of Table 1, the authors report "107 normal subjects". It was however understood that the subjects underwent treatment because of pelvic misalignment. Is it still correct to label such subjects as "normal"? Moreover, the values in Table 1 are reported without units - please correct.

Response: We have removed the term “normal” and included the units of measurement in Table 1.

Comment #9: The reference to the Supplemental Figure 1 occurs so many times that it is weird that the authors did not incorporate it into a regular figure. For example, each subfigure of Figure 3 could be divided into top and bottom section, one representing the current Figure 3 and the other Supplemental Figure 1.

Response: Supplementary Figure 1 is now contained within Figure 3 as requested.

Comment #10: The authors have to provide the characteristics of the studied population in terms of gender and age. Also, the distribution according to gender and analysis of statistical differences between genders should be provided, as they may bias the resulting findings.

Response: The authors have chosen not to include demographic data so as not to bias the presented results focusing on a single demographic parameter. While we do agree there will be gender, age and potentially lifestyle covariates, the authors believe a larger data set will do better to dissect out such confounders and mediators in the data in comparison to the smaller data set presented here.
Comment #11: Why was the D'Agostino-Pearson test used for testing the normal distribution of data, and not some other test, for example, the Shapiro-Wilk test, which is more established in such studies? Please elaborate.

Response: The authors recognize that the Shapiro-Wilk test generally comes out toward the top against a wide variety of non-normal distributions in power comparisons with other possible choices. While we wouldn't claim it's the best possible omnibus test of normality, it's a very solid choice. However, the D'Agostino-Pearson test was chosen for several reasons. It first computes the skewness and kurtosis to quantify how far the distribution is from Gaussian in terms of asymmetry and shape. It then calculates how far each of these values differs from the value expected with a Gaussian distribution and computes a single P value from the sum of these discrepancies. It is a versatile and powerful normality test and is recommended highly by several biostatisticians at the University of Calgary. This is because the D'Agostino-Pearson is an excellent test for differences in skewness or kurtosis. This being said, we do recognize that not every non-normal distribution differs substantively in skewness or kurtosis. Indeed, it's a trivial matter to find distinctly non-normal distributions with the same skewness and kurtosis as the normal. Thus, for the statistical issues relevant to the data presented here, the authors believe that the D'AP and the Shapiro-Wilk tests are tied given the figures included herein.

Comment #12: In section Results - Principal component analysis, the authors mention the "null hypothesis", which should be defined already in the Introduction.

Response: The hypothesis is generally implied in the introduction in that we expect a relationship, but do not yet understand what relationships truly exist. Thus, an explicit hypothesis cannot be stated, however, it is the nature of PCA analysis that the outcome provides the hypothesis. Regardless we have stated a general hypothesis in the results section linked to the PCA.

Comment #13: In the same section as above, they describe the isolation of principal components PC1 and PC2 as the two components that contribute the most to the variability of the data. However, when analyzing data with PCA, each principal component can be described by how it affects the data. The authors should provide such descriptions as well, meaning how PC1 and PC2 affect the data, since both account for almost 50% of the variability.
Response: To properly interpret each component, one must first compute the correlations between the original data and each principal component using the correlation procedure. Interpretation of the principal components is based on finding which variables are most strongly correlated with each component (i.e., which of these numbers are large in magnitude, the farthest from zero in either direction). Importantly the authors believe that which numbers to consider to be large or small is a subjective decision; one must determine at what level the correlation is of importance. Owing to this subjectivity, and because such interpretations are clearly beyond the scope of journal and likely the common readers, such analysis and descriptions have been excluded in favour of more simply understandable interpretations. Furthermore, scatter plots of principal component scores are typically required within interpretation to identify the locations of each point in the plot to see if places with high levels of a given component tend to be clustered in a particular region or parameters; again, we believe such an analysis is beyond the scope of the objective of the manuscript.

Comment #14: In relation to the previous comment, the authors chose to retain just the first two principal components (PC), which represent 43.5% and 47.2% of the total variability in the data before and after treatment. However, the retained variability is relatively low, as it does not even reach 50%. This is basically a problem of the "stopping rule" that questions how many PCs should be retained so that they are still representative for the underlying data. While a rule of thumb is at least 80% or even 90%, the others retain those with corresponding eigenvalues larger than 1. The authors should also study "Stopping rules in principal components analysis: a comparison of heuristical and statistical approaches" by D.A. Jackson (Ecology, 74:2204-2214, 1993) and "How many principal components? stopping rules for determining the number of non-trivial axes revisited" by Peres-Neto et al. (Computational Statistics & Data Analysis, 49:974-997, 2005) for a deeper insight of this problem. Nevertheless, retaining below 50% of variance does not seem convincing, and the authors should at least prove that the conclusions and findings they report remain the same even when they include more PCs (and therefore retain a larger variance) into the analysis.

Stopping rules in PCA of ~80% is an excellent ‘rule of thumb’ for low complexity data typical of the data sets of the 1990’s when the first two papers were published. Certainly, over the years many studies, tutorials and blogs in packages such as R-Studio have discussed this issue. Since the mid 2000’s data sets have become far more complex in fields of metabolomics, proteomics, and genomics where I (Turner) am more familiar with. It is now well accepted in these fields that as the number of variables increase one must go down to lower stopping percentages to tease out hidden trends. In fact, it is now common that the most interesting discoveries are in the lower variances representing as little as 10% of the variance. Thus, we feel it is not of value within our study to explore further heuristically and statistical analysis for the hypothesis and
conclusions made. Certainly as we move the work forward with larger patient numbers to explore the contributions of sex, age, BMI, etc…such further PCs, heuristic heat map correlation analysis, OPLS models and other similar approaches to extract information and trends from complex data will be exploited.

Comment #15: In relation to the previous comment, it is assumed that the authors used linear PCA, therefore inferring a linear relationship among the studied variables. Is this assumption correct? Please elaborate.

Response: The reviewer is correct. PCA performed here was performed on a symmetric correlation or covariance matrix. Thus, the principal components are normalized linear combinations of the original predictors in the data set. Therefore, the first PC is a linear combination of the original predictor variables which captures the maximum variable in the data set. The second principal component is also a linear combination of original predictors which captures the remaining variance in the data set and is uncorrelated with the first PC. As the two PC are uncorrelated, their directions are orthogonal. All succeeding PCs follow a similar concept (i.e., they capture the remaining variation without being correlated with the previous component.

Comment #16: While PCA analysis is an interesting approach, the authors should also report the Pearson correlation among the measured variables, as such statistics is established in similar studies and would also allow to compare their findings to other studies.

Response: Owing to the fact that ours is the first study to use surface topography, we caution comparison between studies with simple linear correlation statistics such as the Pearson correlation coefficient (or likewise). For example, a correlation coefficient of -0.062 means that the straight-line model through the data explains 0.38% of the variance of the data and the slope of the line is negative. There is no more meaning of this value from a statistical point of view, and what it may mean practically needs to be interpreted by experts in the field. Usually, there simply is no very sensible interpretation. To prevent concern from readers on if this presented correlation value (for example, -0.062) is significant, the authors have chosen PCA analysis. Simply put, the authors did not want concern for statistic significance to change the interpretation or meaning of the value (i.e., if it is “relevant” or not and what we may deduce from this knowledge). Moreover, the authors have chosen PCA because the principal components are the eigen vectors of the correlation matrix, and therefore, each principal
component multiplies by the correlation matrix gives the same correlation matrix times the corresponding eigen value.

Hard to do this directly as this study is the first to use the measurement approach. Of course, each technology to measure skeletal parameters have systematic errors and we caution the comparison between techniques at this time.

Comment #17: As noted in a previous comment, the quality (resolution) of Figure 3 has to be improved.

Response: All image files included in the manuscript have been enhanced as requested. Loss of resolution occurred through manuscript submission.

Comment #18: The authors correctly conclude that this topic needs more discussion and investigation, and more properly devised studies in the future. As a result, I would suggest that the authors address the community with a milder tone, especially in the first paragraph of section Discussion and in the Background part of the Abstract.

Response: We appreciate the insights and observations shared by Reviewer One which have helped us to realize that the tone of our dialogue may have been interpreted by some readers as harsh. We feel the changes we have made to the manuscript have resulted in a final product that has a milder tone, is more precise, and is easier to read.

Comment #19: On several occasions, the authors use expressions that are too colloquial and not appropriate for a scientific publication. Also, please use abbreviations consistently after they are defined, e.g. PCA, ST, etc. On the other hand, some abbreviations are not defined, e.g. AIS. Some parts of the manuscript should be revised (e.g. the first sentence in section Results - Population).

Response: We have edited the manuscript for clarity and brevity. We have also addressed inconsistent use of abbreviations.
Reviewer 2

Comment #1 (Line 17): Explain how scoliosis enters this discussion.

Response: Scoliosis is one of the more severe causes of trunk deformity and imbalance. Scoliosis experts must develop skills in identifying the early signs of scoliosis by assessing small changes in trunk balance and alignment. Your readers should find this study interesting as it furthers knowledge about how small changes in the spine and pelvis are related, and how they relate to overall sagittal and coronal balance of the trunk.

Comment #2 (Line 47): This is why the label of idiopathic was applied. The category of idiopathic has decreased with genetic analysis and identification of syndrome associated scoliosis.

Response: We heartily applaud these advancements and hope that improvements in our understanding of the mechanisms underlying common postural imbalance will play a role in diminishing the size of the idiopathic category even more. We firmly believe that this conversation concerning biomechanical relationships and interdependencies will have tremendous value as it evolves further.

Comment #3 (Line 59): Is ST a validated surrogate for pelvic inclination? State that pelvic inclination is different from pelvic incidence, which is fixed.

Response: As indicated above for reviewer 1; Previous authors have found ST to be an accurate, reliable and repeatable surrogate for PI.

Comment #4 (Line 63): Was this a consecutive series?

Response: No, patient set is random, and although consecutive data sets exists for some patients only pre and post treatment data were included.

Comment #5 (Line 64): What was the diagnosis for which patients were being treated?

Response: The patients were being treated for a wide variety of musculoskeletal issues believed to be at least in part related to postural imbalances involving the pelvis and spine. In the future we imagine a study could be performed with asymptomatic subjects to explore this subject more specifically.

Comment #6 (Line 68): Define "malalignment" and correlate that with symptoms requiring treatment.

Response: “Malalignment” in this context refers to a lack of bilateral postural balance or symmetry. This series of questions from Reviewer 2 has helped us to realize that we were not very clear in the initial manuscript. This study was never intended to investigate malalignment as it relates to symptoms, but rather the relationship between different alignment parameters as they change. This has been stated in the first paragraph of the Discussion.

Comment #7 (Line 89): In view of no identifiers, can the authors exclude age as a significant variable in lumbar lordosis?
Response: Most studies on the effect of age have suggested that lumbar lordosis decreases slightly or remains the same throughout a person’s lifetime. Our primary focus was not on the absolute value of LL in any one individual, but when the amount of LL changed (whether a little or a lot) how did it change in relation to the change measured in PI?


Comment #8 (Line 92): Were the authors able to ascertain compliance with the daily exercise routine?

Response: Although in a clinical setting we hope that patients comply with their corrective exercise prescriptions, the fact that we were measuring changes in postural alignment parameters in an attempt to identify relational patterns in how those parameters changed, exercise prescription compliance was not a primary concern. If our sample size was sufficient we believed we would find a very comprehensive range of postural changes to measure and analyze.

Comment #9 (Line 102): Does the body mass index affect the appearance of dimples of Venus?

Response: Yes, the body mass index is relevant to this discussion. However, at this point in its evolution ST scanning is able to detect the dimples of Venus very accurately in most subjects. In some clinically obese individuals, accurate measurement is not possible without the use of reflective markers. One study comparing the accuracy of the measurements in patients with different BMI found that the accuracy was consistent for BMI up to 30, and when greater than 30

Overall, although this is a good question, since our patent set did not fall into this problem, we have not made any changes in the manuscript.

Comment #10 (Line 104): What is the abbreviation DL?

Response: “DL” refers to “Dimple Left” (the dimple of Venus on the left of center).

Comment #11 (Line 190): Neurological elements of the spine that innervate paraspinous muscles and provide positional information are important.

Response: We agree that the paraspinal muscles and the neurological elements of the spine that innervate them are important in helping the patient maintain a balanced trunk. But their influence cannot completely account for trunk balance. The position of the pelvis, lower extremities, head, etc. all influence the overall balance of the trunk.

Comment #12 (Line 191): Can this "poor alignment" be parametrically defined?

Response: Yes, we believe that trunk alignment does fall in a parametric distribution within a population of adults.

Comment #13 (Line 225): Include the Bettany-Saltikov J et al., 2017 paper in the bibliography.

Response: The Bettany-Saltikov paper has been removed from both the paper and the bibliography. Unlike this paper, we are not investigating the use of conservative treatment of
AID or of any other kind of scoliosis. Simply put, we are studying the relationship between different alignment parameters as they change rather than selling or studying a certain kind of therapeutic intervention. To avoid reader confusions, the paper has been removed. We thank the reviewer for allowing us this opportunity to critically assess the references in our study and the underlying message of our manuscript.

Comment #14 (Line 228): The conflation of sagittal deformity (scoliosis) with other diagnoses is confusing. Are the authors stating that the study validates their treatment when applied to AIS, especially without identifying age or skeletal maturity?

Response: We regret purposefully replacing “postural imbalance” with “scoliosis” in many places in the previous version of the manuscript. Intended to help your readers find this study to be relevant to their interests, it has clearly been more confusing than helpful. The purpose of this study was never to promote the therapeutic system employed by the clinic this study was undertaken at. The therapy system they use has proven to bring about changes in posture, so was an appropriate venue for measuring the relationships between different spinal and pelvic alignment parameters. We are certainly NOT stating that this study validates the treatment that was used at the clinic the study was conducted at when applied to AIS.

Comment #15 (Line 248): Cite reference.


Comment #16 (Line 250): Is the relationship between LL and PIA fixed or subject to dynamic changes? Is the lack of relationship between lumbar lordosis and pelvic inclination unimportant in all age groups and diagnoses?
Response: Since LL and PI both move independently during gait, their relationship is probably not fixed. This is a fascinating question. Our study involved the measurement of these parameters with the subjects in a static, standing position, at two different points in time. Our results suggest that there is no correlation between these two characteristics. However, the Pelvic Tilt floor exercise involves increasing and decreasing LL by increasing and decreasing PI while the subject is in a supine position. In the abstract for The mechanical relationship between the rearfoot, pelvis and low-back, in Gait Posture. 2010 Oct;32(4):637-40. doi: 10.1016/j.gaitpost.2010.09.007. Duval K, et al state that “the effects of pelvic tilt on the lumbar spine were only noticeable when pelvic tilt was exaggerated”. This topic clearly requires further investigation but is beyond the direct context of our study. We have included these comments in the last paragraph of the Discussion.

Reviewer 3

Comment #1: My greatest concern is the definition of “pelvic inclination”. On the line 52, the authors referred to Legoye's article that described "Pelvic Incidence". On the line 199, the authors described Pelvic inclination (i.e. Pelvic Tilt). Both Pelvic Incidence and Pelvic Tilt may be difficult to measure by surface topography. Please clarify the definition of the pelvic inclination and how to measure it on the surface topography image.

Response: The reference to Legaye et al. was an error and the one concern expressed by all three reviewers. Unfortunately, the terminology used by some authors in North America differs from some authors in Europe. To eliminate further confusion, we note at the beginning of the Methods that we chose to use the terminology of The Spinal Deformity Study Group’s Radiographic Measurement Manual as a common terminology reference. Often the term “pelvic tilt” is used in place of “pelvic inclination”. We used PI to refer to “pelvic inclination” because “pelvic torsion” was a parameter being measured and was already referred to as “PT”. We repeat here that throughout this study PI refers to pelvic inclination and NOT pelvic incidence. We do not manually measure pelvic tilt on the surface topography images. The scanner software generates a 3D representation of the skeleton and the alignment measurements. Our study is limited to the data we directly obtained from this approach. We recognize that pelvic incidence is an entirely different measurement altogether, and we have not attempted to measure it using ST. Pelvic inclination/tilt (PI) however, is measurable using ST.
As stated above, x-ray studies show that there is variance between measurements that are repeated by the same examiner, and when repeated by different examiners. The variance in the ST measurements is similar to those. Also, the repeatability of ST is very good. Previous authors have come to the conclusion that ST is a reliable instrument for measurement.


Comment #2: On the line 83, 119, and 86, Please clarify the patients' demographic data, such as age, gender, diagnosis and the treatment they underwent. Please describe how to do the manual therapy and corrective exercise in details.

Response: The subjects consisted of male and female participants being treated for a wide range of musculoskeletal issues believed to be at least in part related to postural imbalances involving the pelvis and spine. In the future this study should be performed with asymptomatic subjects to explore this subject of sub demographics more specifically. This study was not conducted to prove or promote any specific manual therapy and/or corrective exercise system. The therapy system used at the clinic in question has proven to bring about changes in posture, so was an appropriate venue for measuring the relationships between different spinal and pelvic alignment parameters as they change. Please see our response to reviewer 1: the authors have chosen not to include demographic data so as not to bias the presented results focusing on a single demographic parameter. While we do agree there will be gender, age and potentially lifestyle covariates, the authors believe a larger data set will do better to dissect out such confounders and mediators in the data in comparison to the smaller data set presented here.
3 Why did the treatment finish at 12 weeks?

Response: The purpose of this study was never to promote the therapeutic system employed by the clinic this study was undertaken at. The therapy system they use has proven to bring about changes in posture, so was an appropriate venue for measuring the relationships between different spinal and pelvic alignment parameters as those values changed. The twelve-week course of treatment happens to be a part of the therapy protocol employed at that clinic but its only relevance to this discussion is that many of the subjects did not require more care beyond that time and therefore had no further ST scan results to analyze.