BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	What is the effect of prolonged sitting and physical activity on thoracic spinal mobility? An observational study of young adults in a UK university setting
AUTHORS	Heneghan, Nicol; Baker, Gemma; Thomas, Kimberley; Falla, Deborah; Rushton, Alison

VERSION 1 – REVIEW

REVIEWER	DR.G.SUDHIR Consultant Spine Surgeon Sri Ramachandra Medical University Chennai, Tamil Nadu, India
REVIEW RETURNED	09-Oct-2017

GENERAL COMMENTS	The authors have aimed to analyse the correlation between physical activity and mobility of thoracic spine which is well written and well presented except for a few clarifications and suggestions: 1.) The authors have quoted that thoracic movement dysfunction has been linked to the pathologies in the neck, shoulder and elbow based on other studies - Did they find any correlation between thoracic spine mobility and mobility of cervical and lumbar spine spine in this study? 2.) The authors ahve classified the groups based on NICE guidelines and Dunstan et al for sitting duration. Did they consider the duration of continuous (or prolonged) sitting versus intermittent sitting for sitters (as those who sit intermittently with breaks in between should not be considered as prolonged sitters and the musculoskeletal strength may vary between these groups) 3.) Is physical activity determined only by time i.e., 150 minutes or by the type of activity also? Kindly clarify 4.) Statistical analysis needs to be elaborate and it would be better if a concise statistical table is presented 5.) There is a significant statistical difference between males and females in the three groups with majority of subjects in low activity and sitters being females. Did they compare the thoracic mobility between males and females within the groups? (If there was a gender difference within the groups, it could mean that females have a reduced thoracic mobility compared to males) 6.) Kindly provide the correlational statistics 7.) Discussion section could be elaborated based on the results with some references.

REVIEWER	Kenji Endo M.D., Ph.D. Assistant Professor Department of Orthopedic surgery,

	Tokyo Medical University
REVIEW RETURNED	09-Oct-2017

GENERAL COMMENTS	Very interesting and well discussed from the results

REVIEWER	William J. Montelpare Applied Human Sciences, Faculty of Science/Faculty of Nursing, University of Prince Edward Island Charlottetown, PEI, Canada C1A 4P3
REVIEW RETURNED	20-Nov-2017

GENERAL COMMENTS	This paper presents a novel approach to understanding the influence of sedentary behavior on spinal mobility and the accompanying muscle stiffness that results from the lack of physical activity. The authors provided a clear, concise approach to the problem and an appropriate application of the STROBE methodological checklist for case control studies. The results provide the impetus for further research in this area, as the findings support the notion that sedentary behaviors such as the combination of sitting with low overall physical activity can have a direct influence on musculoskeletal health and related physical health degradation.

REVIEWER	Igor Burstyn Drexel University, USA
REVIEW RETURNED	21-Dec-2017

GENERAL COMMENTS	Study design seems to be incomplete. There are 2 factors: physical activity (2 levels) and sitting (3 levels: <4, 4-7, >7 hrs). This, the appropriate factorial design has 2*3 factors, and yet the authors have only 3 by design. I have no idea how one can control for each factor in analysis to draw statistical inferences. This seems to me a major flaw that greatly detracts from usefulness of the study; it cannot be remedied. Use of statistical tools like ANCOVA and mixed effects linear models (described below) cannot overcome deficiency in design, though I suspect that ANCOVA is more vulnerable to bias than the alternatives. Random effects models to account for correlation and learning within 3 attempts seem to be worth considering: I would use random intercept and slope (fitted to attempts nested within participant). ANCOVA is an OK tool to use to account for repeats and gender imbalance but it is inferior to mixed effects models that can be implemented in SPSS these days. Specifically, mixed effects models with REML algorithm can help with imbalance in factors and correctly (more flexibly) model repeats, while controlling for uneven distribution of gender. They also allow to control for age and BMI which is now not considered in analysis beyond hypothesis test in crude comparison, which is not appropriate for this analysis. When person correlation was used, did they authors check assumption of normality? Spearman rank correlation is more robust and does not require such check. Subjective classification of "strength of association" should be removed from the paper. Reliance on p-values in interpretation is misplaced for the highly imbalanced design and is against guidelines for statistical analysis of observational data (as per Am Stat Association's recent statement):

	please focus on effect estimates and clinical significance instead.
	Please describe procedures used to reduce bias in detail. The one-sentence general statement to this effect is not helpful in understanding quality of work.

| **REVIEWER** | Conor Gissane
St Marys University
Twickenham
Middlesex
TW1 4SX
United Kingdom |
| **REVIEW RETURNED** | 21-Dec-2017 |

GENERAL COMMENTS	bmjopen-2017-019371
	The influence of sedentary behaviour and physical activity on thoracic spinal mobility in young adults: an observational study
	Abstract
	Page 3 lines 32 to 37
	Thoracic spine mobility (mean [SD], 95% CI) for the S, PA and LA group were 64.75[1.20] 62.37, 67.14°), 74.96[1.18] 72.61, 77.31°), 68.44[1.22] 66.02, 70.86°) respectively. Significant differences in thoracic mobility were detected between S and LA, S and PA ($p<0.001$). Correlations between thoracic rotation and exercise duration ($r=0.67$, $p<0.001$), sitting duration ($r=-0.29$, $p<0.001$) and days exercised ($r=0.45$, $p<0.001$) were observed.
	The volume of data presented here is excessive, you report mean, sd and 95% CI. Is the objective to describe the individual groups or make inferences?
	Statistical methods
	Page 9 lines 22 to 44
	Here you state that you have used oneway ANOVA and an ANCOVA for further analysis.
	When I get to the results (Page 10 line 22) I find reference to a oneway ANCOVA and no reference to the ANOVA. The groups are very much "self-selected", they brought characteristics they allowed the researchers to categorise them. The ANOVA is for use in a designed experiment where the researcher allocates participants to the groups and is inappropriate in this situation.
	Despite its name, the ANCOVA is a regression technique and is more suited to this situation. However, what you present looks like an ANOVA output. Reporting this as a regression would allow you to report how much a factor (group, gender, etc) influenced thoracic spine mobility.
	Using Options > Parameter estimates would allow you to state how

	many degrees each factor changed the score.
	Page 10 Table 1
	The vertical lines are not needed.
	Page 11 line 15 to 16
	As per the abstract, I question the need and the motivation for mean, sd and 95% CI.
	Page 11 line 46 to 51
	The correlations seem to be correct and acceptable. But, these factors could have been included in a linear regression along with the other factors examined.

VERSION 1 – AUTHOR RESPONSE

Editorial Requirements:
- Please revise your title to state the research question, study design, and location. This is the preferred format for the journal.

Revised to 'What is the effect of prolonged sitting and physical activity on thoracic spinal mobility? An observational study of young adults in a UK university setting'

Please revise the Strengths and Limitations section (after the abstract) to focus on the methodological strengths and limitations of your study rather than summarizing the results.

Revised to include
•   The inclusion of accelerometry would have been useful to verify self-reported behaviours
•   Whilst the study sample size used an a priori power calculation of the primary outcome (a validated measure of thoracic mobility), individual group sample size was insufficient to support further post hoc analysis

Reviewer: 1
Reviewer Name: DR.G.SUDHIR
Institution and Country: Consultant Spine Surgeon, Sri Ramachandra Medical University, Chennai, Tamil Nadu, India
Please state any competing interests: None Declared

Please leave your comments for the authors below

The authors have aimed to analyse the correlation between physical activity and mobility of thoracic spine which is well written and well presented except for a few clarifications and suggestions:

1.) The authors have quoted that thoracic movement dysfunction has been linked to the pathologies in the neck, shoulder and elbow based on other studies - Did they find any correlation between thoracic spine mobility and mobility of cervical and lumbar spine spine in this study?

Thank you for your very positive comments on our manuscript.
Whilst of interest this was outside the scope or aims of the project and measurement of the cervical and lumbar spine was not included.

2.) The authors ahve classified the groups based on NICE guidelines and Dunstan et al for sitting duration. Did they consider the duration of continuous (or prolonged) sitting versus intermittent sitting for sitters (as those who sit intermittently with breaks in between should not be considered as prolonged sitters and the musculoskeletal strength may vary between these groups)

We agree that whilst our study relied on self-report of behaviours that patterns of sitting could affect findings. We have however acknowledged this in the limitations section and propose using accelerometers in future projects to have more detail on behaviour including patterns of prolonged sitting.

3.) Is physical activity determined only by time i.e., 150 minutes or by the type of activity also? Kindly clarify

As indicated in the abstract, rationale and methods, physical activity needed to be of 'moderate' intensity to meet the published guidelines. We have added this to the discussion section for consistency.

4.) Statistical analysis needs to be elaborate and it would be better if a concise statistical table is presented

The statistical analysis section has been elaborated for clarity around the objectives of the project as suggested.

5.) There is a significant statistical difference between males and females in the three groups with majority of subjects in low activity and sitters being females. Did they compare the thoracic mobility between males and females within the groups? (If there was a gender difference within the groups, it could mean that females have a reduced thoracic mobility compared to males)

To address the issue of group gender imbalance we used an ANCOVA as part of the analysis, which allowed us to control for gender and enables confidence in our findings.

6.) Kindly provide the correlational statistics

Correlational statistics are provided on page 11/12, under the title 'Other analyses: Correlational analysis'

7.) Discussion section could be elaborated based on the results with some references.

Additional references have been added to the discussion although elaborating on the discussion is difficult given the paucity of research in this field.

Reviewer: 2
Reviewer Name: Kenji Endo M.D., Ph.D.
Institution and Country: Assistant Professor, Department of Orthopedic surgery, Tokyo Medical University
Please state any competing interests: None declared

Please leave your comments for the authors below
Very interesting and well discussed from the results

Thank you for your positive comments on this manuscript

Reviewer: 3
Reviewer Name: William J. Montelpare
Institution and Country: Applied Human Sciences, Faculty of Science/Faculty of Nursing, University of Prince Edward Island Charlottetown, PEI, Canada, C1A 4P3
Please state any competing interests: None declared

Please leave your comments for the authors below

This paper presents a novel approach to understanding the influence of sedentary behavior on spinal mobility and the accompanying muscle stiffness that results from the lack of physical activity. The authors provided a clear, concise approach to the problem and an appropriate application of the STROBE methodological checklist for case control studies. The results provide the impetus for further research in this area, as the findings support the notion that sedentary behaviors such as the combination of sitting with low overall physical activity can have a direct influence on musculoskeletal health and related physical health degradation.

Thank you for your very positive comments on our manuscript

Reviewer: 4
Reviewer Name: Igor Burstyn
Institution and Country: Drexel University, USA
Please state any competing interests: None declared

Please leave your comments for the authors below

Study design seems to be incomplete. There are 2 factors: physical activity (2 levels) and sitting (3 levels: <4, 4-7, >7 hrs). This, the appropriate factorial design has 2*3 factors, and yet the authors have only 3 by design. I have no idea how one can control for each factor in analysis to draw statistical inferences. This seems to me a major flaw that greatly detracts from usefulness of the study; it cannot be remedied. Use of statistical tools like ANCOVA and mixed effects linear models (described below) cannot overcome deficiency in design, though I suspect that ANCOVA is more vulnerable to bias than the alternatives.
Random effects models to account for correlation and learning within 3 attempts seem to be worth considering: I would use random intercept and slope (fitted to attempts nested within participant). ANCOVA is an OK tool to use to account for repeats and gender imbalance but it is inferior to mixed effects models that can be implemented in SPSS these days. Specifically, mixed effects models with REML algorithm can help with imbalance in factors and correctly (more flexibly) model repeats, while controlling for uneven distribution of gender. They also allow to control for age and BMI which is now not considered in analysis beyond hypothesis test in crude comparison, which is not appropriate for this analysis.

We have now re-worked some of the manuscript to clarify any misunderstanding of the aims and design of the reported study.
As an observational study we are seeking to investigate differences in thoracic mobility (dependant variable) across the three groups (independent variable) based on self-reported behaviours.

We believe that there was a misunderstanding given the original way that we described our groups and therefore we have changed the terminology to ensure greater clarity. There were 3 groups defined as follows:

Group 1, sitters: Individuals who participate in <150 minutes of physical activity per week and sit >7 hours per day

Group 2, physically active: Individuals who participate in >150 minutes of physical activity per week and sit <4 hours per day

Group 3, low activity: Individuals who spend between 4-7 hours sitting daily and <150 minutes of physical activity per week

Thus the only factor is group. Hence the selection of a one-way ANOVA.

We apologise for the lack of clarity in the original version of the manuscript and trust that with the clearer definition of the 3 groups, it is evident that the only factor for analysis is group.

The ANCOVA was then used to control for the observed gender imbalance which was evident following completion of the data collection and analysis of the group characteristics.

Participant characteristics including BMI and age were used solely to describe the sample.

When person correlation was used, did they authors check assumption of normality? Spearman rank correlation is more robust and does not require such check. Subjective classification of "strength of association" should be removed from the paper.

Thank you. We have revised our analysis to report Spearman rank correlation as we agreed this to be a preferable analysis.

Although it was considered a useful inclusion for the reader, classification of strength of association has been removed as suggested.

Reliance on p-values in interpretation is misplaced for the highly imbalanced design and is against guidelines for statistical analysis of observational data (as per Am Stat Association's recent statement): please focus on effect estimates and clinical significance instead.

As an observational study the authors support the use of p values in this instance. However we have also now reported the effect size as suggested.

Please see response above about the appropriateness of the one-way ANOVA for our study design.

Please describe procedures used to reduce bias in detail. The one-sentence general statement to this effect is not helpful in understanding quality of work.

Additional information provided: A number of measures were put in place to minimise the influence of bias, including use of a validated measurement approach [29], standardisation of procedures through training of assessor, assessor blinding, controlling for environmental variables, avoidance of physical activity prior to testing, partial blinding of participants in that they were not made aware of a priori planned comparison between groups and piloting of all procedures in advance of the main study.

Reviewer: 5
Reviewer Name: Conor Gissane
Institution and Country: St Marys University, Twickenham, Middlesex, TW1 4SX, United Kingdom
Please state any competing interests: None

Please leave your comments for the authors below

bmjopen-2017-019371

The influence of sedentary behaviour and physical activity on thoracic spinal mobility in young adults: an observational study
Abstract
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Thoracic spine mobility (mean [SD], 95% CI) for the S, PA and LA group were 64.75[1.20] 62.37, 67.14°), 74.96[1.18] 72.61, 77.31°), 68.44[1.22] 66.02, 70.86°) respectively. Significant differences in thoracic mobility were detected between S and LA, S and PA (p<0.001). Correlations between thoracic rotation and exercise duration (r=0.67, p<0.001), sitting duration (r=-0.29, p<0.001) and days exercised (r=0.45, p<0.001) were observed. The volume of data presented here is excessive, you report mean, sd and 95% CI. Is the objective to describe the individual groups or make inferences?

Thank you, we recognise there is a considerable amount of information and have now removed CI data so results are clearer and focused to describing individual group differences, our primary aim.

Statistical methods
Page 9 lines 22 to 44

Here you state that you have used oneway ANOVA and an ANCOVA for further analysis.

When I get to the results (Page 10 line 22) I find reference to a oneway ANCOVA and no reference to the ANOVA. The groups are very much "self-selected", they brought characteristics they allowed the researchers to categorise them. The ANOVA is for use in a designed experiment where the researcher allocates participants to the groups and is inappropriate in this situation.
Despite its name, the ANCOVA is a regression technique and is more suited to this situation. However, what you present looks like an ANOVA output. Reporting this as a regression would allow you to report how much a factor (group, gender, etc) influenced thoracic spine mobility. Using Options > Parameter estimates would allow you to state how many degrees each factor changed the score.

Thank you. We recognise that this could be clearer and made revisions accordingly. As an observational study we are seeking to investigate differences in thoracic mobility (dependant variable) across the three groups (independent variable) based on self-report behaviours. As such between groups differences for which an ANOVA is appropriate using group as the factor.

We believe that there was a misunderstanding given the original way that we described our groups and therefore we have changed the terminology to ensure greater clarity. There were 3 groups defined as follows:
Group 1, sitters: Individuals who participate in <150 minutes of physical activity per week and sit >7 hours per day
Group 2, physically active: Individuals who participate in >150 minutes of physical activity per week and sit <4 hours per day
Group 3, low activity: Individuals who spend between 4-7 hours sitting daily and <150 minutes of physical activity per week

Thus the only factor is group. Hence the selection of a one-way ANOVA.

We apologise for the lack of clarity in the original version of the manuscript and trust that with the clearer definition of the 3 groups, it is evident that the only factor for analysis is group.

The ANCOVA was used to control for the observed gender imbalance which was only evident following completion of the data collection and analysis of the group characteristics. Recognising the gender imbalance we subsequently used an ANCOVA

Page 10 Table 1 The vertical lines are not needed

Vertical lines now removed

Page 11 line 15 to 16
As per the abstract, I question the need and the motivation for mean, sd and 95% CI.

The CI data has been removed

Page 11 line 46 to 51
The correlations seem to be correct and acceptable. But, these factors could have been included in a linear regression along with the other factors examined.

Thank you. We appreciate that this would be an option, but based on the design, sample size and objectives of the study feel that the correlations are sufficient.

VERSION 2 – REVIEW

REVIEWER	Igor Burstyn Drexel University, USA
REVIEW RETURNED	02-Feb-2018

GENERAL COMMENTS	The authors are advised to seek help from a statistician. I am uncertain that they designed an informative study.

REVIEWER	Conor Gissane St Mary's University Twickenham Middlesex TW1 4SX United Kingdom
REVIEW RETURNED	15-Feb-2018

GENERAL COMMENTS	bmjopen-2017-019371.R1 The influence of sedentary behaviour and physical activity on thoracic spinal mobility in young adults: an observational study Abstract Page 3 Results section This does read better that the last submission. The inclusion of the effect size is good. Page 9 lines 22 to 44 The statistics section now includes mention of the ANOVA and the

	ANCOVA.
	It is good to see that the authors stated which effect size they used,
	Table 1 looks better without the extra lines

VERSION 2 – AUTHOR RESPONSE

Dear Dr Bedi

Thank you for your email and outline of minor revisions required to the aforementioned manuscript.In view of the comments regrading the images we have now removed these from the manuscript as per your proposal thus avoiding an concern regarding identification.

We have added a statement regarding Patient and Public Involvement in the methods section.

Finally in response to reviewer 4, we have over the course of this research drawn on the advice of a statistician so are confident that the design and subsequent analysis is appropriate to research aims.

We hope this addresses any outstanding queries and thank you for considering this work for publication in BMJ Open.

Kind regards
Nicola