Author's response to reviews

Title: Trends in knee arthroscopy and subsequent arthroplasty in an Australian population: a retrospective cohort study

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The Editorial Team:
BMC Musculoskeletal Disorders


Dear Editor,

Thank you for providing the reviewers comments regarding the above manuscript. We have addressed each comment separately below, with the comment *italicised*, the response in plain font, and any extracts from the manuscript indented. We have also attached a copy of the updated manuscript with Track Changes.

Reviewer 1

*The main methodological concern I have is that, in the absence of data on laterality, the authors have assumed that the TKA and arthroscopy occurred ipsilaterally. The authors have tried to address this issue by undertaking an audit of 42 patient medical records from two institutions. I think the authors need to explain further how this sample of records were selected and demonstrate if they are representative of the overall cohort of patients. Specifically, did the 42 records cover all 8 years represented in the data?*

The 42 records were retrieved through hospital databases over the periods available from the two hospitals in which the senior author worked, and was therefore able to access. These periods did not extend to all 8 years represented in the data.

*Within this audited sample, 5% of records had contralateral procedures (i.e. the arthroscopy and TKA occurred on opposite sides). Can the authors please comment on how a potential 5% error rate is likely to impact their findings?*

The 5% error in laterality is likely to overestimate the observed conversion rate to TKR. The effect of this error rate has been further acknowledged in the discussion as follows:

“Therefore our rate of conversion to TKR is likely to be an overestimation of the true rate.”

*Is there reason to think the proportion of contralateral procedures could be higher in certain sub-groups of patients, particularly older patients who are more likely to undergo TKA?*

The laterality of the second procedure only concerns the subgroup that underwent TKR after arthroscopy, which is mainly confined to older patients. We cannot determine if age predicted the likelihood of laterality.

1. Discussion- paragraph 4-5
*The authors note that the rate of TKA within 2 years of an arthroscopy was higher for patients aged 65 and older than what has been identified previously. Could the authors please comment as to whether this finding may is the result of a higher chance of having contralateral procedures in older people?*
The rates reported in our study are higher than rates from other countries. It is possible the high rate of conversion in the patients over 65 is in part due to the acknowledged error in laterality. We have added the following to the Discussion:

“Therefore our rate of conversion to TKR is likely to be an overestimation of the true rate.”

2. Methods- Paragraph 4
The authors used a negative binomial regression analysis to study changes in rates of procedures and conversion to arthroplasty over years. Could the authors please clarify if negative binomial regression was used due to the dispersion of the data? Was an offset variable used in the model? If so, why was this offset chosen?

Yes. The following paragraph has been added to the methods to clarify our statistical methods.

“Negative binomial regression models were applied to determine the percentage change in rate and to accommodate for over-dispersion in the data, with the log of NSW population used as an offset to control for the population changes over time.”

Minor Essential Revisions
1. Methods- Paragraph 1
While the use of administrative data has been questioned in the past, I think these methods are becoming more acceptable internationally. I happen to know that CHeReL undertakes routine quality checks of the APDC data and their data linkage processes. I think mention of this in the methods could help to allay any concerns readers may have about the use of administrative data.

Authors agree and have incorporated this information in the methods, as follows:

“A routine quality check by CHeReL using a 1000-person sample from our data found a false link error rate of 4/1,000 records (0.4%).”

2. Also, could the authors please clarify whether the APDC collects data from all public and private hospitals in NSW? (i.e. is it likely to have 100% coverage of arthroscopies and arthroplasties performed in the state?)

The APDC is a census of all admitted patient services provided by NSW Public and Private Hospitals. Reporting to the Admitted Patient Data Collection (APDC) is a requirement under the Health Administration Act 1982 for public hospitals, and the Private Hospitals and Day Procedures Centres Act 1988 and section 44(1) Health Insurance Act 1973 for private hospitals. The APDC covers all admitted patient activity provided by Public Hospitals, Public Psychiatric Hospitals, Multi Purpose Services, Private Hospitals and Private Day Procedure Centres in NSW.

The following sentence has been added to the methods:
“The APDC contains mandatory data from all hospitals (public and private) and day-procedure centres in NSW.”

3. Methods- Paragraph 3
The authors also note that data on indication for surgery were poorly reported. This leads to questions about the quality of reporting for other variables used in the study. Can the authors please quantify the rates of missing data? Were any methods used to address high rates of missing-ness?

There were no missing data for the primary variables (age, gender and procedure code) in the data provided. The rates of missing data pertaining to procedure codes not entered cannot be quantified, as the procedure code was used as an inclusion criteria.

4. Results-Paragraph 1
According to the relevant standards for this paper (the STROBE guidelines for cohort studies), results should provide characteristics of study participants. I think the results section might benefit from a Table briefly describing the overall characteristics (e.g. age/gender distribution, number of institutions (if available) treatment in public and private settings, etc).

This table has been added to the manuscript as Table 2, with the following sentence added to the methods:

“Descriptive statistics for all patients undergoing knee arthroscopy are provided in Table 2.”

5. Discussion
In the second paragraph of the Discussion and in the Conclusion, the authors refer to a 9-year study period. It seemed to be that the study was undertaken from July 2000 to December 2008, which is only 7.5 years. Can the authors please correct this?

This has been corrected.

6. Discussion- paragraph 3
The authors state in this paragraph that “increasing rates of arthroscopies have been reported elsewhere, attributed to the increasing rates of OA.” And in the next sentence state that “data from Canada and England have shown that the utilisation of knee arthroscopy for the treatment of osteoarthritis decreased between 1993 and 2004.” Both sentences reference the same study by Hawker et al. Could you please clarify if the rates of arthroscopy increased or decreased in this study?

We apologise for the confusion and have altered the relevant section of the manuscript as follows:

“We found a lack of significant change in rates of knee arthroscopy by age group. Data from Canada and England have shown that the utilisation of knee arthroscopy for the treatment of osteoarthritis decreased between 1993 and 2004, but the overall rates of arthroscopy increased in England, and remained steady in Ontario.”
7. There have been several other studies on rates of arthroscopies both in the United States and Australia that deserve mention in the discussion when contrasting these findings with others. While the authors do mention the study by Hawker, there have been at least three other recent studies (please see Kim et al Journal of Bone & Joint (American), Potts et al American Journal of Sports Med and Bohensky et al MJA).

These studies have been incorporated into the discussion.

8. Discussion- paragraph 6
Limitations
As the APDC only captures data on patients treated in New South Wales, is it possible that interstate migration, especially among younger patients, could have impacted the findings in relation to conversion to TKA?

The rate of emigration would be difficult to determine for this population of patients. However, we consider that any effect would lead to an underestimate of the revision rate to TKR. Given a lack of data and the likelihood of underestimation, we have not acknowledged this in the manuscript but would be happy to do so if required.

9. Figure 2- Procedures per 100,000 population by age group I found this graph a little confusing due to the 7 groups/lines. Is it possible to simplify it by aggregating some of the age groups together- for example 24-44 and 45-64?

Figure 2 has been altered as requested.

Reviewer 2

In the literature, arthroscopy is described as a valuable tool for treating mechanical symptoms related to early stages of knee arthritis with satisfactory postoperative clinical results, as well as the arthroscopy procedures can improve clinical symptom and delay TKA intervention.

However, the indication of knee arthroscopy is a point that should be considered, because it is related to the effectiveness of this intervention.

So, I would like to suggest to the authors to include in this analysis the data of: clinical evaluation: etiological diagnosis, knee range of motion, level of pain, and maybe, some clinical scores that was applied on defining the arthroscopy approach. Radiological evaluation: stage of knee arthritis, knee alignment, patella height...

So, these aspects are relevant because they offer us more information about the patients, improving the analysis and the understanding of the results.

We agree that the underlying diagnosis (and thus the indication for arthroscopy) would help clarify trends here (rates in arthroscopy over time in various age groups). The administrative databases used in this study do not reliably provide the underlying diagnosis, nor any of the other variables suggested and were therefore not provided in the study. The deficiencies of data linkage of health databases have been acknowledged in the manuscript in the discussion.

Another point that could be considered is the TKA outcomes after prior knee surgery. So, does minor knee surgical procedure have some influences in the TKA outcomes?
We are not aware of any data that demonstrates whether TKA preceded by arthroscopy influences outcome after TKA.

*It should be noted that retrospective studies in contrast to prospective randomized studies are subject to selection bias among groups. In this study, the authors emphasized the limitations related to administrative datasets, which could comprise incomplete patient data and coding errors, as well as the affected side and the indication of knee arthroscopic procedure. So, the limitations of the study were clearly stated, but it should be outstand that it could leave to some difficulties on the analysis of the data and results.*

The limitations regarding laterality have been expanded and further data has been added regarding the error rate in the dataset in the discussion regarding laterality:

> “Therefore our rate of conversion to TKR is likely to be an overestimation of the true rate.”

And in the methods regarding data linkage accuracy:

> “A routine quality check by CHeReL using a 1000-person sample from our data found a false link error rate of 4/1,000 records (0.4%).”

Thank you for providing the opportunity to revise the manuscript; we look forward to your response.

Regards,

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