Author's response to reviews

Title: Clinical and radiological outcomes after management of traumatic knee dislocation by open single stage complete reconstruction

Authors:

Michael T Hirschmann (Michael.Hirschmann@ksbh.ch)
Nadia Zimmermann (nadia.zimmermann@stud.unibas.ch)
Thomas Rychen (thomas.rychen@ksbh.ch)
Christian Candrian (christian.candrian@eoc.ch)
Damir Hudetz (damir.hudetz@zg.t-com.hr)
Lukas G Lorez (lukas.lorez@hin.ch)
Felix Amsler (felix.amsler@amslerconsulting.ch)
Werner Müller (w.u.mueller@datacomm.ch)
Niklaus F Friederich (niklaus-f.friederich@unibas.ch)

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Author's response to reviews: see over
Dear Dr. Jiang,

Thank you very much for reviewing our manuscript and giving us the opportunity to submit a revised version of it. In response to the reviewer comments we have made the following suggested changes and highlighted them in our manuscript, which you find enclosed.

The final manuscript has been seen and approved by all authors and they have taken due care to ensure the integrity of the work. We declare that all authors have contributed to the paper and are familiar with the contents of the final draft. We hereby affirm that the submitted manuscript is our original work, has not been published or is being considered for publication elsewhere. There is no conflict of interest.

Best regards from Basel, Yours sincerely,

Michael T. Hirschmann, MD

Reviewer: peter kloen

Abstract:

As this was retrospective how were pre-op scores (e.g. Tegner) determined? The patients were asked to rate their preinjury Tegner score at follow-up.

Background:

What do they mean with “radiological investigation regularly underestimates the injury”? Was MRI performed?

This is a general statement well supported by the evidence of the literature. According to Twaddle et al. (JBJS Br. 1996) MRI shows a low sensitivity particularly for PCL, LCL and posterolateral corner injuries.
Was this a single surgeon series? If not, how many surgeons?
This was a series of a team of surgeons around Prof. Werner Müller consisting of 7 surgeons, who developed the treatment algorithm.

Was the technique "consistent" during the 26 year period? Really?
Indeed the treatment philosophy (open complete single stage reconstruction of ACL, PCL, collaterals and popliteal tendon), which has been introduced by Prof. Werner Müller, did not change during this 26 years period. The fixation devices (post screws) and the rehabilitation program are also still unchanged.

Methods:
48 patients were treated within 2 weeks, 20 later than 2 weeks. How much later than 2 weeks for that group? What was the reason for delay?
The patients treated after 2 weeks from injury, presented that late to our clinic, which was the main reason for the delay of this group. Furthermore, many patients lived in countries across Europe, what is another explanation for the delay.
Within 2 weeks n=48, within 2-4 weeks n=7, within 4-8 weeks n=5, >8 weeks n=8

Informed consent was obtained already in 1980? Consent for what?
Informed consent was obtained at follow-up for study participation, particularly for obtaining conventional and stress radiographs.

Surgical technique:
Page 6: Specify “peripheral structures”
The phrase was deleted as it was redundant.

“Touch weight bearing” must be “toe touch weightbearing”?
Changed to partial weight bearing.

Follow up: "Independent senior orthopedic resident". It seems one can delete independent. He "had not been involved in the index surgery" is enough information.
Deleted as suggested.

The "Cooper test" was not listed in the abstract/introduction.
Newly included into the abstract.

Results:
Page 9: Do the authors think the extension deficit (in 19%) is due to harvesting the grafts, impingement, arthrofibrosis or other reasons?
In our experience the extension deficit is related to the conservative rehabilitation program including 6 weeks of 3/4 scotch cast splint with 10° extension deficit.

Complications and reoperations:
page 10: Is "insufficient result" a good term? More often it is something like excellent/good/fair/poor.
Changed as suggested.

Is TKR at 17 resp. 23 yr after injury to be considered as a reoperation?
We agree and changed the term to „secondary surgeries“. 
Discussion:
The discussion is too long.
We tried to shorten the discussion substantially.

page 17: The negative effect of smoking should not be explained as a probably related to microvascularity. No data are presented in this study on microvascularity.
The paragraph was deleted as suggested.

The authors advocate to operate "within 40 days". That is not based on their data. There were not two groups (<40 and >40 days).
For statistical univariate analysis we separated the patients in two groups (<40 and >40 days). Patients treated >40 days from injury showed a higher need for change of their profession than patients treated earlier.

It is difficult to the scientific merits of this manuscript. It is likely a personal series of a very experienced surgeon. Their results might be hard to repeat for less experienced surgeons. This is important and it might be good to mention it in the paper. This type of surgery is not to be taken too lightly.
We entirely agree, hence we emphasized on this issue in the discussion part of the manuscript.

Are there differences in the literature comparing open vs. arthroscopic repair?
A variety of surgical procedures both open [8, 20, 26, 28, 30, 43] and arthroscopy-assisted [18, 20] have been reported. There is no literature comparing open versus arthroscopic techniques.

Are there outcome studies available on non-operative treatment of these injuries using similar outcome scores? If so, can they compare with these data?
Most authors agree that non-surgical treatment with cast immobilization produces inferior results compared to surgical treatment regimens [5, 16, 28, 42]. These studies have been performed long time ago and showed high rates of extension and flexion deficits.

Lastly, this group obviously preferred the open technique. I would think that with the arthroscopic reconstruction (or combined arthroscopic for ACL/PCL and open for LCL/MCL/peripheral) the results could even improve. Please provide a clear recommendation for treatment at the end of the conclusion, i.e. do they think it should only be done "open"?
Although arthroscopic techniques might promise improved outcome the evidence on this issue is controversial. To date no study using arthroscopic techniques (such as Fanelli et al., Ibrahim et al.) could show superior results. In our experience open reconstruction still has its place, as there are inherent parts of the injury (collateral ligament injuries and popliteal injury) which have to be treated openly. Also arthroscopic techniques are associated with the risk of a compartment syndrome as the joint capsule is frequently ruptured leading to fluid outflow into the calf. We tried to emphasize in the discussion part.

Reviewer: Rainer Ludtke
Study design
The authors compared patients from 1980 to those from 2006. I doubt that these patients indeed form a homogeneous group (which is the premise to apply
statistical methods). Diagnoses, operation techniques and concomitant medications/interventions might have changed.
In fact, the treatment philosophy (open complete single stage reconstruction of ACL, PCL, collaterals and popliteal tendon), which has been introduced by Prof. Werner Müller, did not change during this 26 years period. The fixation devices and the rehabilitation program are also still unchanged. Hence, this clearly is with regards to this point as homogenous as it could be.

Moreover, it uncertain whether the outcomes are comparable across patients. Most outcomes change over time (it is e.g. well-known that quality of life decreases with age), as do predictors like smoking status, weight, or additional injuries.
Your critique is theoretically correct – but we have correlated age at injury, age at control and time since injury with all other variables and we found significant correlations for Tegner score (before and after injury) which is very sensitive to age because it measures different competitive sports. It also correlates with secondary operation, but not for all other quality of life scores. The same results are true for multivariate analysis.

A total number of 74 patients is rather small to reliably assess which predictors affect outcome.
Although a total number of 74 patients is one of the biggest studies published dealing with long-term outcome after surgical treatment of knee dislocations, we do agree to your statement and emphasized on this in the limitations part of the manuscript.

Statistics
The statistical model (repeated measurement ANCOVA) is insufficiently described. What were the outcome parameters, which factor determined repeated measurements (presumably time), how many time points were considered?
We did not use this model – we deleted it from the text.

Moreover, it is unclear, how the stepwise variable selection was done. Was is a forward or backward selection? What were the criteria to drop or leave a variable?
We now describe the detailed method in the method section and add a table for the results of the different outcome scores.

No details are giving whether missing values occurred and how they were dealt with (which is especially important for the stepwise variable selection).
There were very few missing values – if they occurred, they were replaced by the mean in regression analysis. This information is added in the methods section.

No efforts were made to adjust the results for multiple statistical errors. In consequence, most of the reported predictors are probably false positive results.
We tested 225 correlations and found about 50 of them to be significant. With a p of .05 we expect 11-12 false positive results. We therefore are very careful interpreting results with only one of the outcome scores. When computing multivariate analysis we find our results to be quite consistent. Nevertheless we added this point to the discussion section (limitations).

Results
From my point of view, presentation of results is not acceptable. Predictors are not adequately defined (what e.g. does “higher degree of secondary education”, or “greater age at injury” exactly mean?). Moreover, for each predictor and outcome an effect estimate, supplemented by an 95% confidence interval, should be reported, on how much this predictor affects the outcome. Simple “+” and “-” notation does give sufficient information.

We changed the presentation of the results in tables 4 and 5 completely. To give an overview of the univariate results we plotted Pearson correlation r with a wide range of outcome scores. In this presentation which allows the reader easily to handle the huge number of correlations, we put up with the fact that Chi square or t-test statistics would be more adequate for some of the data. We controlled the significant results of those correlations with the adequate statistical model.

We argue that adding 95% confidence intervals to these results would exceed the capacity of the reader who is interested to get clinical results.

We added to the method section, how we defined sociodemographic variables.