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Reply to 'Letter to the editor: What makes fixation of femoral neck fractures fail? A systematic review and meta-analysis of risk factors'

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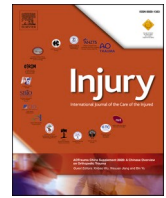
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Author's Reply

Reply to 'Letter to the editor: What makes fixation of femoral neck fractures fail? A systematic review and meta-analysis of risk factors'

Deventer, 10 march 2023

Dr. P. V. Giannoudis

Editor-in-Chief

Injury

Leeds, United Kingdom

Subject: Takanori Miura, Norio Yamamoto, Yosuke Tomita, Letter to the editor:

What makes fixation of femoral neck fractures fail? A systematic review and meta-analysis of risk factors, Injury (2023), doi: <https://doi.org/10.1016/j.injury.2023.01.006v>

Reference: JINJ 10,568

Dear editor,

We have read, with great interest, the letter to the editor from dr. Miura et al. and we thank them for their comments on our article [1]. They raised three concerns which we carefully reviewed and comment on below.

Firstly, they suggested that if at least one of the six domains in Supplementary Material 2 is rated as high, the overall Risk of Bias (RoB) in Table 2 cannot be low. However, this means that the Quality in Prognosis Studies (QUIPS) tool is correlated with the Grading of Recommendations Assessment, Development and Evaluation (GRADE) assessment in Table 2, which is not the case. We used the QUIPS tool to rate the risk of bias of the individual studies while the GRADE assessment was used to rate individual predictors of failure.

Secondly, the authors express concerns regarding an overestimation of the 'certainty of estimates' in Table 2. They state that if two of the five domains are rated moderate, the certainty of estimates cannot be high ('we are very confident the variation in risk associated with the prognostic factor lies close to that of the estimate'). We would like to clarify that we rated all the domains according to the legenda in Table 2. So a 'very low' rating in one of the domains indicates there is very little certainty in the estimate: the variation in risk associated with the prognostic factor is likely to be substantially different from the estimate (ergo a high RoB, inconsistency, imprecision etc.). Using this system we rated for every predictor each domain on a scale from 1 to 4, with 1 being very low and 4 being high. The average of the five domains is the 'certainty of estimates'. For example in 'Gender' the certainty of estimates = $(3 + 4 + 3 + 4 + 4)/5 = 3.6 = \text{high}$.

Thirdly, Dr. Miura also mentioned the domain 'Imprecision' could be overrated because of a small sample size. For predictors where 'Imprecision' is rated high (fracture reduction and age), the sample size was

more than 600 patients and >150 events. The predictors where 'Imprecision' is downrated to moderate (Gender, Smoking, Implants, Implant position and Displacement) the sample sizes ranged from 150 to 600 patients and always included more than 50 events.

In conclusion, we appreciate the interest of dr. Miura and his colleagues in our work and we thank them for their feedback, yet we do not share their concerns on the quality of our evidence assessment. We assessed the individual studies and predictors using objective criteria according to articles of Hayden et al. and Foroutan et al. [2,3].

Yours sincerely, on behalf of the co-authors,
Jorn Kalsbeek

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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