



## LETTERS TO THE EDITOR

### Risk factors for infection in total knee arthroplasty, including a previously unreported intraoperative fracture and deep venous thrombosis<sup>☆</sup>



### Factores de riesgo para la infección en prótesis de rodilla, incluyendo la fractura intraoperatoria y la trombosis venosa profunda, no descritos previamente

Dear Editor,

We have to congratulate Dr. De Dios and Dr. Cordero-Ampuero for their work on “Risk factors for infection in total knee arthroplasty, including a previously unreported intraoperative fracture and deep venous thrombosis” published in the January edition of RECOT,<sup>1</sup> given that comprehension of these factors will be a great help, enabling traumatologists to try to reduce the rate of the terrible complication of arthroplasty infection.

However, reading the work raised a series of doubts in our minds that we believe were not sufficiently resolved by the final version of the text. Firstly, the authors report that the occurrence of a fracture during the operation is a statistically significant risk factor *per se* ( $P = .028$ ), in spite of its only have occurred once; 2 doubts arise about this point, given that another statistically significant risk factor is the duration of surgery. It would be necessary to know whether in the case of the fracture the duration of the operation increased (as would be logical), as this may be the risk factor in itself rather than the fracture (this being the specific cause why the surgery was prolonged). Also, when performing the statistical study of the risk factors for which the number of patients or controls for a variable is lower than

5 (as is the case for the intraoperative fracture) it would be impossible to interpret the significance of the contingency table, given that 50% of the boxes (of the 4 in the table) do not comply with the observed frequency of 5. In reality one of the 4 boxes will have a frequency lower than 5, as it would represent 25% of the boxes. To complicate matters even more, another of the factors analysed ( $BMI < 20$ ) has the same frequency as the intraoperative fracture (with a single case in the infections group and none in the control group). Nevertheless, the calculation of its level of significance (once again, with the doubt about the reliability of a contingency table under these conditions, which theoretically could not be interpreted) gives table 2 in this work a value of  $P = .076$ . However, unless there are other data which are not shown in the table, the value would have to be the same as the one for the fracture! We understand this to mean that at least one of these 2 values is incorrect. Secondly, and given that previous surgical operations (except for the arthroscopies and tibial osteotomies) were found by the authors to be a markedly significant risk factor for infection, we consider that the types of surgery in question should be specified. As far as possible it should be clarified whether the risk factor here is surgery in itself, or the resulting anatomical distortion (were the operations osteosynthesis due to fractures or realignments of the extensor apparatus, and was it necessary to remove material in the same operation, *etc.*?), the prolongation of the duration of surgery, the presence of previous infections, skin disorders or multiple incisions. . . given that this information may be of value for the readers of this paper in trying to reduce the incidence of infections.

### Level of evidence

Level of evidence V.

### Ethical disclosures

**Protection of human and animal subjects.** The authors declare that no experiments were performed on humans or animals for this investigation.

**Confidentiality of data.** The authors declare that they have followed the protocols of their work centre on the publication of patient data.

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**Right to privacy and informed consent.** The authors declare that no patient data appears in this article.

## Reference

1. De Dios M, Cordero-Ampuero J. Factores de riesgo para la infección en prótesis de rodilla, incluyendo la fractura intraoperatoria y la trombosis venosa profunda, no descritos previamente. *Rev Esp Cir Ortop Traumatol.* 2015;59:36–43.

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## Reply to: Risk factors for infection in total knee arthroplasty, including previously unreported intraoperative fracture and deep venous thrombosis<sup>☆</sup>



### Réplica a: Factores de riesgo para la infección en prótesis de rodilla, incluyendo la fractura intraoperatoria y la trombosis venosa profunda, no descritos previamente

Dear Editor,

We share the interest of Dr. Arriaza and Dr. Saavedra in understanding the risk of infection in knee arthroplasty, and we will try to answer the statistical questions they have asked about the paper.

The paper does indeed state that intraoperative fracture is a risk with statistical significance per se, even though only one case of this occurred. We did not forget at any time that this was a single case, and even in the original paper we warned and explained at all times that as it was a single case it had to be considered with the appropriate precaution, and that statistical significance is based on a unique event in a series of cases of infection. Once again in the discussion we stated that “we know of no previous description of an intraoperative fracture as a risk factor for knee arthroplasty, and in our comparison it was significantly more frequent among infected patients, although it has to be said that statistical significance is attained with a single infected case with no uninfected control”. I.e., we tried to make it absolutely clear that this is a new result which has to be approached with due caution, and that more research is required, although the results indicate that it may be relevant to take intraoperative fractures into account as a risk factor.

As to whether the prolonged duration of surgery associated with an intraoperative fracture could be the risk factor

in itself, and not the fracture, we would like to clarify that it no case did this study aim to investigate the interactions between variables (in this case, between the intraoperative fracture and the duration of the surgical operation). As we explained in the methodology, the sample is small, so that it would be too risky (and imprudent) to statistically analyse the interaction of risk factors. Moreover, as you yourselves state, there is a single case of intraoperative fracture, so that seeking a relationship between this fracture and the duration of the operation would be, at the very least, imprudent. This is a very new study which explores many factors simultaneously; the fundamental contribution of the study is based on analysing possible risk factors. Study of the interactions between them will be the object of analysis in subsequent studies that would be impossible to undertake without this previous research.

On the other hand your suggestion that the duration of the surgical operation may be the risk factor in itself, and not the fracture (the specific cause why the operation was prolonged) is erroneous from a methodological and statistical viewpoint. Both events (the fracture and the time) arose at the same time, so that it is impossible to establish a causal relationship with one of them (as they state that time would be the risk factor in itself) while negating the causal relationship with the other one (the fracture).<sup>1</sup> When 2 events occur at the same time it is not possible to establish any type of causal relationship whatsoever for one of them, given that the effect of one cannot be separated from the effect of the other. Both aspects (time and fracture) could perfectly well be independent risk factors for the development of an infection.

Regarding their remark on the validity of the Chi-squared analysis when the value is less than 5 (as is the case with the intraoperative fracture) it has to be pointed out that the criterion for carrying out this statistical analysis is not based on the frequency that is observed, but rather on the frequency that is expected.<sup>2,3</sup> It is true that 50% of the boxes do not fulfil the observed frequency of 5; nevertheless, authors such as Carrasco<sup>4</sup> state that a previous condition for the Chi-squared test is that the theoretical boxes (i.e., the expected frequency, and not the observed frequency, as you state in this reply) contain at least 5 individuals. Our contingency tables therefore fulfil the basic requisite to be able to be interpreted in terms of significance. Additionally, we wish to point out that the relationship of the variables and their clinical relevance has been quantified with the OR. Lastly, as we pointed out, due to the low frequency observed these results have to be analysed cautiously, as we do in the article.

Respecting the doubts that arose for you about the “low weight” factor (BMI < 20) and the similarity of results that

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