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Reply to "How to limit bias in quasiexperimental studies"☆

Respuesta a «Cómo limitar los sesgos en estudios cuasiexperimentales

Dear Editor,

First, we would like to thank the authors of the letter "How to limit bias in quasi-experimental studies"[1] for the interest shown in our publication and their comments on it.

The goal of our work[2] is to describe our experience after replacing the prophylaxis recommended in the clinical practice guidelines for cephalosporin arthroplasty surgery,[3] by a regimen of cefazolin + teicoplanin, after detecting that in our centre a high percentage of joint prosthesis infections were due to bacteria resistant to cephalosporins, particularly gram-positive, and to observe the favourable results obtained in other centres with the addition of a dose of teicoplanin to the usual dose of cephalosporin.[4]

We agree with the authors that inherent to such a study is a series of methodological limitations that must be taken into account when assessing their results and making decisions based on them. By using a historical comparison group, that of the patients immediately prior to the change in regimen, the study is subject to the possibility of bias. It is very true that, as a result of the lack of randomisation, there are some differences between the groups, such as a higher comorbidity index and a higher percentage of patients transfused in the comparison group. We do not believe that the difference in colonisation by methicillin-resistant *Staphylococcus aureus* is important because, although in relative value there were twice as many patients in the control group, the absolute number of patients is small (10 and 5 in each group, and it is not statistically significant) and all were decolonised prior to surgery (in fact, none of these patients suffered from an infection, so it does not affect the results). We did not mention the adherence to the rest of prophylactic measures because the only difference during the control period and the intervention period was the addition of teicoplanin, keeping the rest of the measures the same (hygiene, use of antiseptics, early removal of probes and drains, control of perioperative blood glucose, etc.).

We agree with the theoretical comments on the methodological or statistical aspects of the study, but most, if not all, of them are the result of having a sample size that, in our case, would take many years to achieve. Of course, a longer period of observation would be convenient to corroborate the results, limiting the bias mentioned by the authors, and making the result of the intervention more robust. As the authors, we undertake to publish them when we have them.

Obviously, randomised clinical trials provide a better level of evidence and make it possible to avoid this bias. However, conducting them offers difficulties known by all and also raises the ethical question of whether to use a control group maintaining cefazolin, knowing that in our centre an important part of the bacteria that cause joint prosthesis infection are resistant to it, and thus subject these patients to an excess risk of infection when favourable experiences have already been reported by adding a dose of glycopeptide, with a favourable adverse effects profile.[4,5]

In our opinion, beyond the statistical limitations, which we recognise, we believe that our work offers plausible results for two reasons. In the first place, the results are consistent, since adding an antibiotic that has activity specifically against methicillin-resistant gram-positive cocci exclusively reduces these infections and, on the other hand, gram-negative infections remain unchanged. On the other hand, it reproduces the results of other studies conducted with similar regimens.[4]

Whether or not this work should lead to a change in the usual clinical practice, each reader should evaluate it, in the event that it is useful to them if they have a problem similar to ours. Some authors already wonder if, given the epidemiological change of surgical infection in general, and prosthetic infection in particular, mainly referring to the increasing rate of resistance to antibiotics, current antibiotic prophylaxis guidelines (backed by clinical trials conducted in the last century) are still effective today.[6,7] The methodological limitations can make our conclusions more or less debatable, but, undoubtedly, they do not discard them, until a work with similar or better methodology is published that refutes them.

Conflicts of interest

José María Barbero declares that he has received fees for conferences from the pharmaceutical company Angelini. Jose Sanz Moreno declares that he has received fees due to teaching and consultancy collaborations from the pharmaceutical companies Gilead, Viid and Janssen. All other authors declare that they have no conflicts of interest.

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