Reviewer's report

Title: Safety and efficacy of thrombectomy in patients undergoing primary percutaneous coronary intervention for Acute ST elevation MI: A Meta-Analysis of Randomized Controlled Trials

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Reviewer: Marek Brabec

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Safety and efficacy of thrombectomy in patients undergoing primary percutaneous coronary intervention for acute ST elevation MI: A meta-analysis of randomized controlled trials

Authors:

This is an interesting paper summarizing a lot of empirical work which has a substantial practical/clinical potential. Statistical methodology of the paper seems to be at a reasonable level. It uses modern and well-known meta-analytical approaches. The authors properly acknowledge limitations of the general methods used as well as of the particular setup employed in this paper.

Nevertheless, it might be useful to add a short discussion of implications related to the fact that the power of the presented study can be quite low for certain endpoints – even if it is meta-analytical. That is just a fact that can be hardly criticized per se – not much can be done about it until further accumulation of randomized clinical trials results. Nevertheless, it would be useful to emphasize that, in such a situation, it is not wise to interpret results that are statistically not significant as proofs of homogeneity, no differences among groups, etc. Insignificant results should be taken as preliminary outcomes, and they should be reviewed in future, when more evidence will become available (and/or when the analyses will be performed in more homogeneous groups).

Discussion of the fact that analyses did not show much difference between thrombectomy+PCI versus PCI alone but did show some differences within the first group, with respect to the detailed device classification should follow similar lines. More technically speaking, it might be interesting to consider also different amount of variability (e.g. in random effects) for different groups/subgroups (e.g. for different device classes).

There might be some points that would deserve clarification. For instance, on page 5, the text says: “Data was independently abstracted by two reviewers (UT, IH) and disagreements were resolved by consensus.” It is not clear, how often such the disagreement occurred. At least a rough figure would help to judge uncertainty underlying this phase of data processing.
Similarly, on page 7, the sentence “When the outcome did not occur in either group, we were unable to calculate effect sizes due to the empty cells and data were excluded from that particular trial.” does not provide any sense of how frequently the authors used this strategy. If it was not rare, than at least some sensitivity analysis should be done (e.g. replacing empty cells by some low number and comparing the results and comparing them to those obtained already). If the empty cells occurred really often, then the omission strategy might not be very good at all (those trials that have only one type of response in either arm might be very informative and their omission might be very much wasteful, if not biasing). An alternative would have to be quite a bit more sophisticated and technically demanding, however.

There are some places in the text, where an acronym use precedes its definition (several examples can be seen on page 2, right in the Abstract) – that is inconvenient and inconsistent – to say the least.

Typography might be improved – e.g. Background, Methods, Results paragraphs on page 2 should be emphasized (using bold, indentation, etc.).