Reviewer's report

Title: Coronary Collaterals and Risk for Restenosis After Percutaneous Coronary Interventions: A Meta-Analysis

Version: 1 Date: 1 September 2011

Reviewer: Hayley E Jones

Reviewer's report:

This is a well written, clear presentation of results from a systematic review and meta-analysis.

Major Compulsory Revisions

1) The authors conduct random effects meta-analyses and report a primary result of RR = 1.40 (1.09, 1.80). This is reported throughout the paper as if it were a pooled estimate of a single effect. However, in reality this is only an estimate of the average effect across studies, since random effects models have been used. Please refer to Higgins, Thompson and Spiegelhalter “A re-evaluation of random-effects meta-analysis”. Journal of the Royal Statistical Society Series A. (2009). 172 (1), pp137-159. for more about this distinction. The authors need to be explicit throughout the paper that all reported results are simply estimates of average effects. They should also consider reporting prediction intervals (as outlined in the reference above), which predict the effect that would be seen in a new study. Due to the heterogeneity between results across studies, this interval can be expected to be substantially wider than the confidence interval for the average effect. At the least, estimates of the between study heterogeneity tau and I^2 should be presented throughout the paper instead of in a separate subsection towards the end.

2) Section headed “Subset analyses” - Results from various meta-regressions are reported as showing “no significant effect”. However, meta-regressions are notoriously low powered. It would be informative to present the estimated ratios of risk ratios (with confidence intervals) from the meta-regressions, rather than (or in addition to) the p-values. Also, were the subgroup analyses and meta-regressions specified in the study protocol or were they post hoc? Please make clear in the text.

I suggest also reporting results from meta-regressions / subgroup analyses in a consistent manner. For the final comparison (patients with stable CAD vs patients with AMI) the RR for each subgroup is currently reported separately, and the authors appear quite confident about a difference existing between these two groups. However, since the two confidence intervals overlap, a meta-regression would not give a statistically significant result. I suggest reducing the emphasis
on this finding, or at least highlighting it as exploratory (particularly if it were a post hoc analysis).

3) Section headed “Limitations of this meta-analysis” - “further, the included studies did not adjust for potential confounding factors”. This seems like a big limitation! Do you think it’s plausible that confounding could have driven the primary result? Please discuss.

Minor Essential Revisions

4) Section “Search strategy” – Please briefly explain your decision not to include case-control studies.

5) Section headed “Data extraction and quality assessment”. Please make clear that the “analysis of studies with superior quality” was a sensitivity analysis, not the primary analysis.

6) The section headed “Subset analyses” really presents a mixture of results from subgroup analyses and meta-regressions. I suggest calling this “Investigation of heterogeneity” instead.

7) Section headed “Subset analyses” – POBA and BMS need defining.

8) Discussion – Please repeat the confidence intervals here when recapping on the results, as well as making clear once again that these are estimates of the average effect across studies (see 1).

Discretionary Revisions

9) In the Results paragraph of the Abstract, sentence starting “This risk ratio was consistent when...” it would be helpful to make explicit that this is describing the results of subgroup analyses. On first reading I understood these to instead be results for secondary outcomes on the full set of 7 studies.

10) Section “Endpoints” - It would be helpful to explain briefly why the primary endpoint was a dichotomised measure (rather than continuous) here. This is later discussed in the Limitations section.

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I have received consultancy fees from Novartis Pharma AG for statistical advice and computer programming in the areas of Bayesian subgroup analysis (2008) and network meta-analysis (2011). I do not believe this to be a competing interest in the context of this paper, and I declare that I have no other competing interests.