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Competing interests: Stéphane Rinfret and Erick Schampaert receive funding from the medical device industry for their research.

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Our interest in the systematic review by Suzanne Ligthart and colleagues of published studies evaluating the cost-effectiveness of drug-eluting stents[1] was not dispassionate, as we are coauthors of 4 of the 19 published studies cited. We have several concerns about the analysis in this review.

The authors stipulated that each study included in the review had to be an "original cost-effectiveness analysis" and "from an unrestricted patient population." We do not believe that any of our included studies meet these criteria. References 17 and 19 are review articles that briefly describe the results of models that were presented at scientific symposia. Neither of these papers was intended to fully convey the underlying methods or assumptions of the models. In fact, these 2 papers describe virtually the same cost-effectiveness analysis. References 20 and 29 describe prospectively conducted empirical cost-effectiveness analyses that were performed alongside the SIRIUS and TAXUS-IV trials, respectively. As noted in the published articles, each of these studies' conclusions apply only to the highly selected types of patients in the trials. It is well-recognized that only approximately 40% of current recipients of drug-eluting stents (and a smaller proportion of all patients with stents) meet the inclusion criteria for the SIRIUS and TAXUS-IV trials, and thus we do not believe that our conclusions constitute a recommendation for widespread use of drug-eluting stents.

Second, we are concerned about potential errors in determining the funding sources for the cost-effectiveness studies. In the case of our own studies, Ligthart and colleagues categorized reference 17 as being unfunded (the journal in which the paper was published did not request information on conflicts of interest) and they categorized reference 19 as being funded by industry (because 1 of the authors reported having received grant support from several manufacturers of drug-eluting stents). Reference 19 was directly solicited by the journal's editors and the cost-effectiveness analysis it describes was entirely unfunded. Ligthart and colleagues state that "studies were considered to be sponsored if the original publications indicated that funding was provided directly by the manufacturer of a drug-eluting stent." Neither study meets this criterion. Had we been approached by the authors to clarify the funding sources for our studies, we would have been happy to provide the relevant details. Whether there were similar errors in categorizing other publications cited in the systemic review is unknown.

Third, we are concerned about the main outcome variable of the study: whether the conclusion of the study favoured widespread use of drug-eluting stents. The term "widespread" means different things to different people. Although Ligthart and colleagues were apparently able to reach consensus on this point, it is almost impossible to interpret the results of a study when the primary outcome measure is subjective and not well-defined. Given these 3 concerns and the small number of studies included in their sample, we suggest that the findings of Ligthart and colleagues may have several alternative interpretations beyond the ones they proposed.

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[One of the authors responds:]

We appreciate the interest in our recent article.[1] We echo the concern of Liana Falcone and Navdeep Tangri that the cost-effectiveness of drug-eluting stents should be scrutinized; our desire to understand the variability in research conclusions prompted our study.

We thank Stéphane Rinfret and Erick Schampaert for their observation that publicly funded studies were of higher quality, but we find it difficult to reconcile this statement with their specious suggestion of a bias pertaining to authors' undisclosed relationships with government agencies as this completely lacks face validity. We can only speculate how they were able to identify such funding. We identified their study in our literature search but excluded it as it involved only a subgroup of patients with drug-eluting stents whereas our outcome was the recommendation (or not) of widespread use. Their assertions that the authors of publicly funded studies are unlikely to encourage widespread adoption of an intervention unless it is expected to save costs and allow responsible policy statements to be produced reflect a misunderstanding of the role of these agencies. Very few medical advances save costs; the metric for this form of health services research is not cost savings but value for investment. Moreover, such research seeks to inform policy-making, not usurp its role in decision-making.

Rinfret and Schampaert also worry that our quality rating was biased by knowledge of the studies' conclusions and source of funding. Our quality rating was based on the clear, unambiguous and objective criteria found in the appendices of our article. The 4 evaluators of the conclusions and 1 of the 2 quality evaluators were blinded to the source of funding, and there were few discrepancies among the evaluators. We invite others, including Rinfret and Schampaert, to validate our findings.

In addition, they state that as a consequence of our publication "the independence and validity" of the work of researchers with industry support is compromised, "even in cases in which the support is unrestricted and the research is performed without any direct input from the funder." We had no way

of assessing unrestricted funding and consequently made no inferences about this issue. The only ones questioning the independence of these particular health researchers are Rinfret and Schampaert.

We also appreciate David Cohen and Ameet Bakhai's clarifications that some of their articles were not original cost-effectiveness studies, but this seems slightly disingenuous as these articles were reported in the electronic databases and have been referenced by others. A re-analysis of our data with their additional information would strengthen our overall conclusions. Their comment about not requiring a statement about the source of funding reinforces our general message of caveat lector or caveat emptor.

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Correction

The authors of a recent *CMAJ* article[1] have let us know that there was a minor error in their article. The odds ratios that were reported as measures for imbalance of patient characteristics in Table 1 were miscalculated. All counts, proportions and *p* values were correctly reported; however, some of the text describing Table 1 is also affected by this transcription error. Regarding the text, the authors have provided the following as a corrected substitute for the last 3 sentences that appear in the first paragraph of the Results section: "Patients who initiated conventional antipsychotic medications (n = 12 882) were slightly younger and *more* likely to be male than those who began using atypical antipsychotic medications (n = 24 359). The initiators of the conventional agents were slightly *more* likely than new users of the atypical agents to have cerebrovascular disease, diabetes, acute MI, other cardiovascular diseases, congestive heart failure and non-MI ischemic heart disease but *less* likely to have dementia, delirium, psychoses, mood disorders and other psychiatric disorders at baseline. Conventional antipsychotic medication users had lower rates of antidepressant use *but higher rates* of use of other psychotropic medications, total number of drugs, admissions to hospital and nursing home stays."

A corrected version of the table is available online (at www.cmaj.ca/cgi/content/full/176/11/1613/DC1).

The authors have assured us that none of the subsequent analyses were affected by this unfortunate oversight and that the interpretation of the study findings is not changed.

REFERENCE

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DOI:10.1503/cmaj.070582