Reviewer’s report

Title: Are Coronary Event Rates Declining Slower in Women Than in Men? - Evidence From Two Population-Based Myocardial Infarction Registers in Finland

Version: 1 Date: 27 July 2007

Reviewer: Yariv Gerber

Reviewer’s report:

General:
This is a nicely written manuscript on an important topic. The authors make use of the excellent data sources of the FINAMI register and the Finnish Cardiovascular Disease Register (CVDR) to examine a clearly defined question

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached):

None

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct):

1. Methods: To what extent could the transition from ICD-9 to ICD-10 affect the results? Also, could the improved validity of CHD diagnosis and mortality data (Pajunen et al., Eur J Cardiovasc Prev Rehabil 2005) play a role in the present temporal trend analysis?

2. Statistical Methods: The assumptions of using Poisson regression are restrictive. Did the authors test for overdispersion and autocorrelation?

3. Statistical Methods: “…were the gender by time period-interaction was the main outcome of interest”. Isn’t the time by gender the main exposure and CHD counts the outcome?

4. Results: Confidence intervals of two rates can overlap and still be highly significant at “standard” levels (e.g., <.01). I bet that most of the overlapping 95% CIs reported for women in the FINAMI between the 2 periods (e.g., from 726 [688-764] to 656 [625-688] in the age-standardized incidence, or from 282 [266-298] to 256 [241-270] in mortality) are in fact highly significant. Anyway, I would deemphasize the significant/non-significant issue and focus more on the absolute and relative differences in decline between men and women.


6. Discussion: p. 12: “Our current finding, abolishing the gender difference in CHD incidence trends after adjusting for the effect of troponins, is in line with our previous report…”. Is this surprising given that the authors modeled the Poisson
regression in a way that “takes into account the effect of troponins using the correction coefficients derived in our previous work”?

Discretionary Revisions (which the author can choose to ignore):
1. Is it possible that the gender by year interaction is in fact a reflection of the age by year interaction? Did the authors try to adjust for the latter effect?

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:
I declare that I have no competing interests.