

**AUDIT ON CARDIOVASCULAR DISEASE PREVENTIVE CARE IN GENERAL PRACTICE**

Dear Sir,

I read with great interest the recently published article by Chan et al in your prestigious journal and I appreciated the authors' effort and work.<sup>(1)</sup> However, I would like to make a few comments on it, because of the apparent gaps in its Results and Discussion, as well as the importance of the subject.

The authors compared the (non)achieved process indicators between patients presenting with acute vs. chronic conditions in Table IV. They gave examples of chronic conditions in their paper such as gout and chronic bronchial asthma, which actually could be linked with cardiovascular risk factors. However, other chronic conditions such as chronic dyspepsia, arthralgia, skin diseases and sinusitis, would be aggregated in the same group because they are commonly-encountered in primary care practice.

I wonder if it was better to compare the group of patients having diabetes mellitus and/or hypertension with others, especially as a large percentage of the patients had diabetes mellitus and/or hypertension (at least 41.5% of the sample). I also wonder if it would be better to display the control indicators of the aforementioned two groups. In Table III, the authors mentioned that 71.3% and 53% of the total sample were blood pressure measurement and blood sugar controlled, respectively. Adding normoglycaemic and/or normotensive subjects to the general pool of patients definitely diluted these percentages, and hence the reader would expect a lower level of control for the diabetics and hypertensive patients. Moreover, the general practitioners and researchers would then identify the percentage of the newly-diagnosed diabetics and hypertensives vs. the previously-diagnosed and/or registered patients.

The authors could also categorise their 1,345 patients into four groups, according to their being diabetic or not, smoker or not, and measurements of blood pressure, total cholesterol, LDL and HDL, taking age and gender into consideration. Grouping of patients would be based on the intensity of their cardiovascular risk ranging from the lowest risk to the highest. Comparison of the process and control variables between these groups would then be of more than one purpose; it would help to identify high-risk patients who deserve immediate attention, motivate patients to adhere to risk-reduction therapies, and help to modify the risk-reduction efforts based on the total risk estimate.<sup>(2-4)</sup>

Although the authors stated in the Discussion that "[their] data showed a significant difference between genders with regard to physician enquiry about their smoking status", their results did not comprise any data on that nor on any gender differences of the study indicators. Conversely, they contradicted their discussion by mentioning at the end of the Results that "there was no significant difference between patients' gender... and all the other criteria assessed". This also made the reader to enquire whether the authors were examining the chi-square for each single process indicator in the two groups per se (as they implicitly stated in the Discussion by providing the p-value for the smoking status) or for the whole set of indicators ending by one p-value, (and one chi-squared value not mentioned) in their Results. It also made the reader wonder why despite the short discussion on the "gender bias", the authors did not consider displaying their results gender-wise, given that gender is a determinant of cardiovascular risk below 50 years of age. Finally, the authors repeatedly mentioned the "lipid profile results" without explaining to the reader what it comprised. The authors also alluded, in their Discussion, to previous similar audits without comparison of those results with the current study, even though only gray publication was available on it.

Your sincerely,

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## REFERENCES

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