

RISK FACTORS OF BREAST CANCER IN WOMEN IN KELANTAN, MALAYSIA

Dear Sir,

I read with great interest, the recently published article by Drs Norsa'adah et al⁽¹⁾ in the Singapore Medical Journal and I appreciated the authors' effort and work. However, I would like to comment briefly on some points in their article.

In Table V, the 95% CI did not align correctly with the adjusted odds-ratio (OR) as it is a row or a line higher. Moreover, the author should clearly state that this 95% CI belongs to the adjusted OR in the table footnote.

It would be better if the authors explained the definition of "(ir)regular menstrual cycle" variable in Table II. Is (ir)regularity in the year prior to diagnosis or life-long? It is not clear for the reader, especially when the sum of this variable was equal to the total number of participants' not the menstruating women. Also, the same could be applied for "ever-smoking" and "regular intake of alcohol" in Table IV.

The inclusion of weight and height in Table III is actually pointless, given the insignificance of the aforementioned variables. If the authors would like to add another anthropometric measurement to body mass index(BMI), they could use waist circumference or waist-hip ratio as indicators of central obesity. Moreover, weight is highly co-linear with BMI and should be carefully included with it in the same model.

The authors stated in the results section that "there were no significant differences in age....". I wonder if they were expecting any difference given that they matched their cases and controls according to age and ethnicity. I do not think that they should include age in Table I. On the other hand, the authors ignored using the potential interaction of age with other variables in their logistic regression models. For example, I would recommend using the "age interacted with parity" variable, especially if the authors cited that "the protective effect of parity was noted especially in..... and among women of 40 years or older"⁽²⁾. Also recommended is the use of the "age interacted with family history of breast cancer" variable, especially when they cited Pharoah et al's study where "the risk of breast cancer was higher in younger women, especially those under 40 years of age"⁽³⁾.

The discussion is long, with repetitions and redundancy. After the authors discussed the association of parity with the risk of breast cancer, they argued the latter's association with reproductive behaviours and education. Then, they returned again at the end of the column to discuss parity.

As shown in Table V, underweight patients were 3.9 times more likely to have breast cancer compared to normal weight patients. The authors mentioned that only briefly in their discussion but explained it ambiguously. Actually, I did not understand why they linked it with the small sample size of post-menopausal women and to the limitation of non-stratifying the participants according to the menopausal status, especially with the almost equal-sized sub-samples of post-menopause and also with using multiple logistic regression.

Finally, I could not find in the paper information on ethical issues such as informed consent and anonymity that the authors mentioned.

Yours sincerely,

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