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An impact of hormones on nevi and melanoma

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To the Editor: Driscoll and Grant-Kels¹ published a review article on hormones, nevi, and melanoma in the December 2007 issue of the *Journal*. We wish to raise and discuss several methodologic issues that we believe are of relevance to the readership.

Driscoll and Grant-Kels incorrectly defined the study conducted on the data from the Swedish Cancer Registries² as a case-control study. It is clear that the Swedish study was a cohort study conducted on 5533 women diagnosed with primary cutaneous melanoma during their childbearing years (185 diagnosed with melanoma during pregnancy; 5348 diagnosed while nonpregnant). It is absolutely crucial for the readership to distinguish a case-control study from a cohort study, which is the archetype for all epidemiologic studies. In a cohort study, groups are defined on the basis of presence of exposure to a suspected risk factor, while in the case-control studies, subjects are selected on the basis whether they do (cases) or do not have (controls) a disease under study. As a result, cohort studies provide estimates of exposure-specific rates and risks, whereas case-control studies provide only estimates of ratio measures of effect. Furthermore, Driscoll and Grant-Kels throughout the manuscript wrongly used the phrase “controlled trials” (trials are intervention studies) to define observational studies (case-control and cohort studies) and descriptive studies (case reports/series).

Driscoll and Grant-Kels criticized the value of the Swedish study because of incomplete tumor thickness data. However, although data on Breslow thickness were available for 74.1% of women in the pregnant group and 39.7% of women in the nonpregnant group, a regression analysis—including different prognostic variables in the multivariate model, which can control for several confounders simultaneously—was performed on the data from 2262 women (137 in the pregnant group and 2125 in the nonpregnant group), which still represents a sample larger than a number of women enrolled in any of six case-control studies included in their review. Also, sensitivity analysis of overall survival comparing women with missing Breslow data with women with Breslow data failed to find the difference between groups thus suggesting that missing data cannot be considered as a source of a bias.

Driscoll and Grant-Kels did not include updated evidence on the effect of pregnancy subsequent to diagnosis of melanoma on survival (for example, data from the Swedish study are not included).

Appropriate use of methods of evidence-based medicine and critical appraisal is crucial when writing reviews.³ We appreciate the opportunity to remind readers of basic concepts in clinical epidemiology in narrative reviews.

References

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