

# RE: Relationship between sunbed use and melanoma risk in a large case-control study in the United Kingdom

Philippe Autier and Mathieu Boniol

International Prevention Research Institute, Lyon, France

Dear Editor,

In their case-control study, Elliott *et al.*<sup>1</sup> found no association between sunbed use and cutaneous melanoma (hereafter melanoma), including after stratification for age at first use and for duration of use. In a previous article, the same group reported a statistically significant reduced risk of melanoma associated with recreational sun exposure, in particular with week-end sun exposure.<sup>2</sup> Although these findings are not in agreement with a wealth of data showing increased risk of melanoma associated with recreational sun exposure and sunbed use, particularly when exposures start at younger age, some studies<sup>3,4</sup> found results similar to those found by Elliott *et al.*<sup>1</sup> The main issue we would like to raise is whether this study had an adequate design for investigating associations between melanoma and lifestyle risk factors.

The first part of the Elliott *et al.* study<sup>1</sup> consisted in a comparison of sunbed use between “population-ascertained cases” and “population-ascertained controls.” On this basis, in a recent work we classified this study as being population-based.<sup>3</sup> However, the previous article<sup>2</sup> provided more details on the methods, documenting that this case-control study was in fact not population based. Table 1 of the 2011 publication indicates that the main analysis was based on 855 cases and 483 controls. It is unusual and non-standard in case-control studies to have cases outnumbering controls by 44% ([875–483]/855). Controls were selected by general practitioners of cases among other patients attending the practice. The response rate of potential suitable controls was only 55% and there was strong imbalance in age distribution between cases and controls despite the fact that family doctors were instructed to select controls within the same 5-year age group as cases.<sup>2</sup> Subjects in the resulting control group were on average of higher socio-economic status than cases, when other epidemiological studies in the United Kingdom show that melanoma occurs more frequently in higher socio-economic groups.<sup>5,6</sup> Hence, the way controls were selected by family doctors may have led to a control group having lifestyle habits not at all reflecting habits prevailing in the general population, which may have resulted in biased estimations of risk associated with sunbed use and recreational exposure.

The second part of the Elliott *et al.* study<sup>1</sup> was based on a comparison with a group of sibling controls. This study part was small and did not have the statistical power to detect odds ratios less than 2.0. Furthermore, this study was restricted to 20% of all potential cases and sibling controls. This consider-

able attrition in response might have introduced selection bias. Also, siblings may share identical behaviors such as visiting indoor tanning parlours.

Biases related to the selection of subjects included in the control group are the Achille's tendon of case-control studies. If the statistical analysis can control for confounding, it cannot control for selection biases. We faced similar bias issues in a previous case-control study on melanoma and sunbed use in which controls were partly selected from general practitioner's patients.<sup>7</sup> We were aware of the methodological limitations of our study and duly published reasons why one should be cautious with interpreting our study results.<sup>8</sup> In this respect, while the case-control design of Elliott *et al.*<sup>1</sup> may have been appropriate for the study of genotypic and phenotypic traits, it was probably not appropriate for the study of lifestyle factors.

Yours sincerely,  
Philippe Autier  
Mathieu Boniol

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Correspondence to: Philippe Autier, iPRI, 95 Cours Lafayette, 69006, Lyon, France, E-mail: philippe.autier@i-pri.org