followed by semiautomated sequencing (using an ABI 377 sequencer; Applied Biosystems, Weiterstadt, Germany), uncovered a novel frameshift mutation in exon 2 of the DNA MMR gene hMLH1 (150insT; Fig 2). This mutation is predicted to lead to a premature stop codon that results in a truncated, nonfunctional DNA repair protein. In patients with MTS, only two different mutations in the hMLH1 gene have been reported so far, whereas 12 mutations have been identified in the hMSH2 gene (Kolodner et al, 1994; Liu et al, 1994; Kruse et al, 1996, 1998; Bapat et al, 1996).

In summary, a patient with an inherited DNA MMR defect (HNPCC) was identified for the first time on the basis of a single distinctive cutaneous tumor. Schwartz and Torre (1995) already pointed out that there may be a hiatus of many years before both elements—the sebaceous neoplasm and the internal cancer—are present in a patient to allow the diagnosis of MTS. Our results indicate that patients with isolated cystic sebaceous tumors are very likely to have an inherited DNA MMR defect. For these patients a lifelong surveillance for skin tumors and internal cancer of the HNPPC spectrum should be recommended.

We thank Walter H.C. Burgdorf, Department of Dermatology, Ludwig-Maximilians-University Munich, Germany, for valuable comments. This work was supported by the Deutsche Forschungsgemeinschaft and of the Medical Faculty of the University of Bonn.

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Superantigens in T Cell Mediated Skin Diseases – More than a Coincidence!

To the Editor

We read with interest the article by Jappe et al in the May 1998 issue of this journal, analyzing the frequency of superantigen-producing Staphylococcus aureus isolates in patients suffering from atopic dermatitis and nonatopic, healthy controls. The authors found that 45% of strains from atopic dermatitis patients (n = 22) were capable of producing superantigens that did not exceed the number of S. aureus strains isolated from healthy carriers capable of superantigen production (n = 8). Based on these findings, the authors question the hypothesis that skin colonization with superantigen producing S. aureus is an essential prerequisite in the pathogenesis of atopic dermatitis (Jappe et al, 1998).

We believe, however, that the design of the study is not suitable to address this question. The authors analyzed the effect of a defined environmental factor (staphylococcal superantigens) on two different groups of individuals: one with a disease–prone genetic background (i.e., atopy), the other one without this genetic background. The fact that an environmental factor is not sufficient to induce a disease in the absence of a disease–prone genetic background, does not exclude its importance in the disease process. Gliadin, for example, is tolerated by the large majority of individuals. Nevertheless, in the presence of a defined genetic background, i.e., specific HLA class II DQ and DR, alleles, gliadin causes celiac disease (Howell et al, 1986; Lundin et al, 1994). If Jappe et al applied the same criteria to draw conclusions in the study of the pathogenicity of gliadin, they would conclude that gliadin is not an essential prerequisite in the pathogenesis of celiac disease.

This clearly emphasizes that besides defined (as in celiac disease) or multifactorial (as in atopy) environmental triggers, the genetic background is of major importance. With respect to psoriasis and superantigens we recently demonstrated that superantigens do only induce a psoriasiform dermatitis if the genetic background of psoriasis is given, but not in healthy individuals (Boehncke et al, 1996). Additionally, in patients with the genetic background of atopy or psoriasis, we found that patients colonized with superantigen-producing S. aureus strains suffer from a more severe disease compared with patients that are colonized by S. aureus strains not capable of superantigen production (Zollner et al, manuscript submitted).1

With respect to the study by Jappe et al, we think the authors can only conclude that in the absence of the atopic background bacterial superantigens are not sufficient to induce (atopic) dermatitis. No conclusion should be drawn on the relevance of superantigens in atopic dermatitis.

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In Vitro Determination of Erythema and Immunologic Protection Afforded by Sunscreens do not Accord with In Vivo Assessments

To the Editor:

As members of the cosmetic industry involved in sunscreen research and development, and currently working on photoimmunologic protection afforded by sunscreens, we would like to add our comments to those from Gasparro (1998) and Wolf and Kripke (1998) on Davenport et al’s publication (1997), particularly after reading Chu et al’s (1998) responses to these comments.

The statement “sunscreens protect against immunosuppression beyond their designated sun protection factor (SPF)” is incorrect. Re-analysis of the data shows exactly the opposite. In addition, we want to state that SPF determined in vivo by the Diffey method often do not correlate with the in vivo SPF (Diffey and Farr, 1991). For example, based on our extensive experience, sunscreen A (2% octyl-methoxy cinnamate) would be expected to have an in vivo SPF of 2.5 (4.5 found in vitro), sunscreen B (2% o-PABA) a SPF of 2.5 (4.5 found in vitro), and sunscreen E (6% ZnO) a SPF of 5 (3.8 found in vitro). So, the conclusion that cream A, which provided the highest immune protection, is the cream that had the highest in vitro SPF is valid only for an in vitro situation and this model of evaluation.

We also disagree with one of the other conclusions of this work: “Protection by creams D and E, broad-spectrum sunscreens, is lower than protection afforded by pure UVB sunscreens (creams A and B).” Indeed, the published in vivo results issued from our laboratory and from other international teams have demonstrated just the opposite: sunscreens containing both UVA and UVB filters are more effective against photoimmunosuppression than pure UVB formulations (Bestak et al, 1997; Damian et al, 1999, Gueniche and Fourtanier, 1997; Moyal, 1998). We have also demonstrated that the higher the UVA protection level, the better the immune system is protected, and that sunscreen protection factor against immunosuppression are lower than their in vivo SPF.

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