Reply

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We are grateful to Dr. Schallreuter and Dr. Wood for giving us the opportunity to return to this interesting issue and to discuss some controversial points.

In the literature there are very few reports indicating that keratinocytes may synthesize catecholamines: they are based on the fact that some enzymes involved in such a process [like tyrosine-hydroxylase (TH) and phenylethanolamine-N-methyltransferase (PNMT) have been found in keratinocytes [1, 2]. At present, however, there is no experimental data confirming that a significant part of plasma catecholamines or of their metabolites is derived from keratinocytes. With several limitations, the assumption inherent in the evaluation of plasma catecholamines is that norepinephrine (NE) in the peripheral blood is produced in nerve terminals and represents adrenergic neural activity, while epinephrine (E) originates from adrenal medulla and represents adrenal medullary activity, since the latter catechamine increases as a consequence of stress stimulation [3-5]. The

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The main purpose of our work was to evaluate sympathetic and adrenal-medullary activity in vitiligo, by dosing plasma catecholamines and their metabolites: as we did not detect significant variations, we tended to exclude the possibility of other sites producing these compounds. Nevertheless, our sentences 'Monoaminergic systems seem unlikely to be related to vitiligo ... However, variations cannot be excluded in genetically predisposed individuals during the onset or the active phases of disease' and, 'It is likely that the analysis of the sympathetic tone of a specific cutaneous area might give more information, especially in the progressive forms of vitiligo' [6] definitely do not exclude, unlike Dr. Schallreuter and Dr. Wood seem to understand, the involvement of the monoaminergic systems in the pathogenesis of vitiligo. Furthermore, if these authors found a higher than normal plasma concentration of NE in 27 out of 28 patients (i.e. 96%), it has to be concluded that this must be considered a predictive sign in the diagnosis of vitiligo.

In their opinion, the increase in NE plasma depends on reduced PNMT activity [7], the enzyme which transforms NE into E. According to their paper, PNMT activity is actually reduced in epidermal extracts from vitiliginous patients, but it is normal or even increased in the keratinocytes of the same patients. This contradicts what they reported in a previous paper [1]. Moreover an overall reduction in PNMT activity should also lead to the reduction of E plasma concentration. However the same authors found normal E levels in the plasma and urine [7], and did not explain how the NE excess is eliminated, since they reported normal urinary levels of unmodified amine in 69% of patients. We found otherwise normal plasma concentrations of NE metabolites in vitiligo patients [6].

We are certainly aware that many factors influence catecholamine evaluation. Even if the COMT and MAOA activities were high (in the plasma? or in the keratinocytes?) in vitiligo patients, they could not influence our data because the plasma samples were immediately stored at -80 °C.

Moreover if these enzymes were really acting, the products of this metabolism would turn out to be higher, i.e. meta-nephrine, MHPG, VMA and HVA; but this is not the case [6].

Finally, we would like to invite the authors to be more precise when quoting. We found the following mistakes: Elayan et al. [2] only showed that rat keratinocytes had an extraneural N-methyltransferase, which could transform exogenous NE into E: this is something different from an autocrine production of catecholamines. Since when is 28 (their cases) 'a significantly larger group' than 40 (our cases)? Reference 7 of their letter refers to a work where the catecholamine presence was investigated in the epidermis and not in the plasma. Quote 9 refers to a work in which the urinary levels of catecholamines and metabolites were never mentioned.

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Mutilating Lesion in Porokeratosis of Mibelli

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I read with interest the article by Handa et al. [1] wherein they have reported mutilating lesions in porokeratosis of Mibelli. This is of course rare, but has been documented earlier. In 1977 we

reported a 35-year-old male patient who had multiple and widespread lesions of porokeratosis of Mibelli of 20 years' duration [2]. There was no family history of similar lesions, no evidence of immunosuppression and the lesions extended to involve the palms and soles. The patient had resorption of the tips of the fingers and the toes along with flexion deformities at the proximal interphalangeal joints. The presence of flexion deformities of fingers and toes was observed to be unique though scarring over the skin could partly be responsible for it. In addition the patient had chronic indolent ulcers over the hands and feet which responded to systemic steroids but recurred on discontinuation.

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192

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Letters to Dermatology