

Reply

The authors are happy to respond to this letter to the editor (1). We agree on most of the comments by the author of this letter. However, we do not agree that the criteria for diagnosis of eczema in our study (2) were not precise enough. That has been shown in another study from the municipality of Odense comparing different diagnostic criteria for atopic eczema in infants (3) including both the diagnostic criteria used in our study and Hanifin & Rajka criteria. Although there was a slight overrepresentation of eczema in our study among those who have completed the follow-up, we do not suspect any significant bias because there were no differences regarding other background data and environmental factors between those seen at follow-ups and those with incomplete follow-up.

In the letter, it is mentioned that higher birth order and household size have been shown to be associated with lower cord blood IgE (CB IgE). This is interpreted by the author as a proof of the opinion that the effect of siblings on allergic outcome starts in utero. However, it has been shown in previous studies that several genetic and environmental factors may influence CB IgE level (4, 5). In a sample of our sample

size, it is not possible to adjust for several confounding factors in performing the regression analysis.

In our study, CB IgE was only predictive of eczema and food allergy that usually starts and is predominant in infancy and early childhood, and the predictive power of respiratory allergies was lost at later follow-up. The explanation for this may be that CB IgE particularly predicts IgE-mediated disease (reflecting the importance of genetic factors) and not non-IgE-mediated allergic diseases, where environmental factors may play a major role (6). Aside from genetic inheritance, family history (FH) may due to a common environment also reflect the influence of relevant environmental factors on the development of allergic disease.

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Sleep in children with atopic dermatitis

To the Editor,

Rolinck-Werninghaus et al. (1) evaluated single-nurse consultations for the assessment of the benefit, parents' self-confidence, and children's symptoms and sleep disturbance in 1628 children with atopic dermatitis (AD). Each evaluation was assessed by a questionnaire at the consultation and by telephone 14 days later. Mean age of children was 1.7 yr, and the authors recognized that their intervention was almost effective by increased self-confidence to handle the recommendations with perception of the consultation supportive. In addition, the frequency of symptoms of AD became lower with improvement of sleep disruption. The authors recommended their ways of intervention in combination with usual medical

care. Long-term follow-up or randomized controlled trials are warranted to substantiate their effect, as the authors mentioned it as the study limitation. I have some concerns on their study.

First, Ersrer et al. (2) reported that although rigorously designed trials provide limited evidence of the effectiveness of educational and psychological interventions to manage the condition of atopic eczema in children, there is a possibility of effective intervention models that may lead to improvements in disease severity and quality of life. They also mentioned that the examinations for the impact of intervention on eczema severity, quality of life, psychological distress, and cost-effectiveness are needed as further studies. Rolinck-Werning-

haus also recognized that they conducted a pilot study, and I appreciate their study with enough number of children with AD.

Second, Rolinck-Werninghaus et al. used a questionnaire survey from parents, and I recommend them for the use of physiological method to detect direct effect from the children. Chang et al. (3) measured sleep by actigraphy and polysomnography in patients with atopic dermatitis in combination with controls aged 1 to 18 yr. The patients with AD had significantly reduced sleep efficiency, longer sleep-onset latency, more sleep fragmentation, and less non-rapid eye movement sleep, and sleep parameters from actigraphy correlated well with those from polysomnography. From these physiological parameters, disease severity of AD was significantly associated with sleep disturbance. Gustafson et al. (4) also reported the feasibility of actigraphy monitoring in six children with mild-to-severe AD by wearing the wristbands for 7 days, and patients exhibited less total sleep time. In contrast, Wootton et al. (5) concluded that actigraphy did not correlate well with disease severity or quality of

life in children with eczema. Honaker and Meltzer (6) reviewed the assessment of bedtime problem and night wake in young children by clinical interview, sleep diaries, actigraphy, and subjective measures, and actigraphy has some problems of detecting sleep-onset evaluation. I previously described a case report of nap sleep in young adult patients with AD by actigraphy and sleep polysomnography (7). There were body movements by scratching skin in sleep stages 1 and 2, which was also observed by actigraphy. In contrast, he reported no sleep disturbance during nap sleep. I recommend a combination of subjective measures of sleep and physiological measures by actigraphy for the assessment of intervention on children with AD.

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Reply

To the Editor,

We read with interest the letter of Kawada concerning the assessment of sleep in children with atopic dermatitis (1) in reply to our manuscript (2) focusing on improved management of childhood atopic dermatitis after individually tailored nurse consultations. Our work focused on a better implementation of care recommendations within the families. We agree that sleep with respect to symptom control and quality of life of patients with atopic dermatitis and their families is an important topic. However, we disagree to Kawada's concern, that our measurement was inappropriate.

Our pilot study was designed to investigate a low-threshold education measure and its impact on disease severity and self-confidence of parents to handle different aspects of their child's disease. For this evaluation, symptoms were assessed in a parent-reported symptom score, among them also sleep comparable to the assessment in the SCORAD. We explained in the manuscript why we chose to keep the design of the pilot study simple and did not use further measurements. Our

instruments were sensitive enough to depict significant improvements of many items, including sleep, between the two time points of analysis.

Kawada cites a number of studies investigating sleep in atopic dermatitis by actigraphy and polysomnography, including his own work. However, a clear argument, why he criticizes that we did not use a direct measurement of children's sleep, is missing.

The patients included in our study are mainly very young children (mean age 1.7 yr). Parents with children of this age realize well the quality of their children's sleep, as nighttime waking of the child requires parental help with negative consequences for the families due to loss of sleep of child and parents (3). It has been shown that a strong correlation exists between the severity of the child's eczema and the sleep disturbances of the mother due to those of the child (4). We agree that there are better instruments to measure sleep quality. But, we do not agree that the instrument used was inappropriate for this study.