

Letter to the editor

Review about sublingual immunotherapy in asthma: reply letter

I would like to clarify the points raised by Larenas-Linnemann and Cox, in relation to the systematic review by Calamita et al. (1) on sublingual immunotherapy (SLIT) in asthma cases.

All the studies included in our review involved the problem of asthma and, despite the existence of studies also encompassing rhinitis/conjunctivitis, our analyses on such studies solely related to asthma [see Figs 2, 4 and 8 of Calamita et al. (1)]. Although the study by Novembre et al. (2) only included patients with rhinitis/conjunctivitis, these authors analyzed cases that progressed to asthma and thus we used this in investigating outcomes from asthma development (disease worsening). We considered that the change in the risk of progression from rhinitis/conjunctivitis to an asthma condition was a fundamental matter for analysis, and this was performed through overall verification of the worsening of asthma-related parameters, which is seen in Fig. 2 of the meta-analysis (1). Regarding the fact that the number of patients utilized in the safety analysis on SLIT in asthma exceeded the number utilized in the efficacy study (also including patients with rhinitis and/or conjunctivitis symptoms, either together with asthma or not), this does not change the main conclusion, given that there are no serious reactions during SLIT use. This serves to further reinforce the credibility of the

safety of SLIT.

The study by Marogna et al. (3) analyzed reductions in bronchial hyperreactivity, and these authors presented an improvement ratio relating to the total number of patients to whom the bronchial challenge test was applied, i.e. 271 patients (see p. 1208 of the study cited). However, their estimate for the effect of the treatment was made using an intention-to-treat analysis (the best way of analyzing categorical variables), in which the number of patients initially randomized (319) was utilized. It is also important to note that, because relative risk was analyzed (in which the effect of the treatment is measured in relation to the effect in the placebo group), the fact that the same criterion was utilized in the control group (total number of patients) would logically prevent any kind of over-estimation of SLIT efficacy.

With regard to the papers that were not included, although Clavel et al. (4) included patients with asthma, they did not present asthma-related outcomes that could be utilized in the review; Tari et al. (5) presented their results as graphs, which meant they could be used because exact values were needed and it was impossible to obtain them by simple visual inspection; and Ippoliti et al. (6) did not present standard deviations.

Finally, regarding the data from Niu et al. (7), their analysis was carried out by

intention-to-treat, and therefore we took into consideration the 110 patients initially randomized, rather than 97 (data supplied by the authors). Regarding Rolinck-Werninghaus et al. (8) and Purello-D'Ambrósio et al. (9), the results from the asthma symptom scores were presented as mean values and standard deviations in relation to the total number of allergic patients, which were obtained in Rolinck-Werninghaus et al. (8) from Table 2 of the article, and in Purello-D'Ambrósio et al. (9) from data supplied directly by the author. Consequently, it was not possible for us to alter the value of *n*, as this could have caused distortion of the results presented by these authors.

Concluding, it is clear that this review followed the rigid norms recommended by the Cochrane Collaboration, which give it methodological credibility. Nonetheless, as we said previously, despite the safety and possible benefits found in favor of SLIT for treating asthma, unequivocal proof of the evidence observed still depends on complementary studies.

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