

Letter to the editor

Author's reply on: 'House dust mite control measures for asthma'

The best way to study whether various interventions are effective in patients with mite-sensitive asthma is to perform a systematic review of the trials that have been performed. This is what we have done, and we included all trials of single or multiple interventions (1, 2). Our review is very large as it includes 54 trials and 3002 patients. It failed to find an effect, and our conclusion that 'Chemical and physical methods aimed at reducing exposure to house dust mite allergens cannot be recommended' is therefore valid.

When the overall effect is zero, as it was exactly for peak flow, or close to zero, as it was for the other outcomes, methodologists regard it as poor science (fishing expeditions trawling for significant results, or cherry picking), if researchers look for a possible effect in subgroups or in individual trials. Such an effect is highly likely to be spurious, and in this case, it would also be biologically implausible, as a positive effect in a subgroup would mean that the effect in the remaining patients is harmful.

Our review does not address multiple allergies and interventions that are also directed against smoking, cockroaches, etc. It is therefore off the point when de Blay et al. (3) are surprised that we did not include two such trials (3). We have already described that one of them, by Krieger et al., was a 'Multifactor intervention

trial ... Not clear how many were allergic to mites' (2). We have also described our reservations about the other trial we excluded, by Morgan et al. Interestingly, the allergen reduction was less than 50%, which is far too little to be expected to have any effect, but de Blay et al. (3) consider it a positive trial, whereas the authors of the other letter (4) argue that we should have excluded trials with low allergen reduction!

Kopp et al. (4) suggest that we should have divided the trials into those with allergen reduction and other trials. As we have noted above, subgroup analysis is not a good idea when there is no overall effect, but we were asked to do it anyway in the previous version of our review (5). We did not find any difference. We also noted that such analyses are difficult (1, 2), as the reporting and the methods are very inconsistent. For example, some authors recorded mite counts and some antigen levels, and not always from the same locations; some reports described a significant reduction from baseline but did not compare with the control group, and some only reported a significant *P*-value, which is not informative as *P* depends on the sample size. We therefore pragmatically used the authors' own views whether an allergen reduction was obtained when we did this superfluous analysis.

Kopp et al. (4) seem to have overlooked that we included trials in patients with asthma, not patients who were just sensitized to mites in a laboratory test. Thus, the trials we included were relevant.

It is not in accordance with the principles for evidence-based medicine and does not provide any useful guidance when Kopp et al. (4) recommend 'measures for mite allergen avoidance' (they do not specify which ones they mean) if 'a relevant house dust mite exposure exists' [what exactly is this; how much, and where and how should it be measured? And does such a boundary really exist, as quite low allergen concentrations can affect bronchial responsiveness? (6, 7)].

Whether or not multiple interventions directed simultaneously against mites, cockroaches, smoking, education, etc., in patients with multiple problems are effective is another matter. It is an interesting question that should be addressed in another review than ours. However, we can say with confidence that measures directed against house dust mites should not be included in such multiple intervention packages, as they do not work.

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