bronchodilator-containing inhalers and placebo inhalers in a clinical trial setting. We are puzzled to be criticised for reference to older literature on electronic adherence devices[9] and sought only to point out that electronic monitoring of relevant dry powder inhalers had long been available to explore the hypothesis that timing of medication use was somehow important to the mechanism of action of SMART. Reddel and Yan have mentioned numerous 'errors' in our review but provided only examples of our emphasis on control and compliance assessments hitherto overlooked in SMART research; we look forward to correcting any errors of fact they detect and report to us.

Finally, we wish to clarify further our thoughts concerning the measurement of inflammatory indices in SMART-treated patients. We agree with Peters and Jenkins that control outcomes were neither superior nor inferior for SMART compared with fixed dose treatment in the study by Pavord and colleagues,[10] and would add that the study was neither adequately powered nor designed to examine this outcome. With respect to eosinophil counts being 'in the range of control', we are not sure that there is sufficient long-term literature using sputum eosinophil counts to declare with confidence that a particular level of airway eosinophilia is safe and acceptable in asthma. However, if one accepts that levels of <3% are tolerable, we note that it was only the mean sputum eosinophil count that was within this limit for SMART-treated patients and the rise in sputum eosinophils seen with SMART therapy probably increased the proportion of SMART-treated patients above the 'acceptable' limit. We find this increase as well as the doubling of biopsy eosinophil counts concerning.

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Editors' response

The review article by Chapman *et al*[1] has provoked a vigorous correspondence,[2–5] amongst other things calling on *Thorax* to 'respond appropriately' and even withdraw the manuscript. We inherited the manuscript from our predecessors and played no part in its commissioning or review. However, we are quite clear that the appropriate response is not to withdraw the manuscript, but rather to allow a vigorous debate in the correspondence columns. Withdrawal of the manuscript would only be the response if there was clear evidence of duplicate publication, data fabrication or some other piece of flagrant dishonesty, which is not the case. In this manuscript, the final conclusion is that we do not have enough evidence to determine whether a reactive asthma strategy such as SMART is preferable to a chronic suppressive study. This is undoubtedly true. Perhaps we will ultimately conclude that this question cannot be answered definitively and we should accept that there is more than one effective way to approach the goals of asthma control and risk reduction. Many would argue that this is a good thing as our patients have different expectations and concerns about chronic drug treatment for asthma.

We welcome debate about the article, and we will consider other relevant letters and articles if submitted, inviting the authors to respond. We are grateful to the reviewers, who do a fine job, but it is the authors who are responsible for the manuscript. Above all, we need to work together to design robust clinical trials with appropriate and relevant end points to answer the great questions about asthma treatment. Sound and fury, no matter what the source, is no substitute for primary data.

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