References
4 Worrell DA. To search and study out the secret of tropical diseases by way of experiment. Lancet 2001;1:358–84.

Editors' reply
We published the statement on duplicate publication, as did the other journal concerned, in response to a correspondent who pointed out the similarity between the two articles. When we looked through the article published in Clinical Medicine it was evident that large parts of the article in Thorax were reproduced verbatim.

We appreciate that review articles by the same author in different journals often contain overlapping information, but that was not the point on this occasion. The point was that much of the material was exactly the same, and Professor Corris had not explicitly acknowledged this or the contribution of other authors to it.

We have taken a firm line on duplicate publication and non-disclosure of related publications in the past and, although we accept that some degree of duplicate reporting is acceptable and common in review or opinion articles, having received a formal complaint about the article we did not feel able to dismiss it. This was particularly the case since Professor Corris was until very recently an Associate Editor of Thorax, and we were in danger of being open to accusations of special privilege for people who have been associated with the journal.

J Britton
Executive Editor

Chlamydia pneumoniae and COPD exacerbation
We read with interest the recent paper by Blasi et al which showed that Chlamydia pneumoniae infection is associated with higher rates of exacerbation and airway microvascular colonisation in patients with COPD.1 We have prospectively studied patients in the East London COPD study with daily monitoring using diary cards to detect COPD exacerbation defined using the same criteria.1,2 Serum microimmunofluorescence (MIF) immunoglobulin G (IgG) titres for C pneumoniae were measured in 110 patients (FEV1 % predicted, 41.7 (18.4)) with stable COPD during 1 year with simultaneous estimation of plasma fibrinogen and serum interleukin 6 (IL-6); 26% of the patients had IgG titres of ≥1 in 16 (fig 1). High C pneumoniae IgG titres were not related to FEV1 % predicted, exacerbation frequency, plasma fibrinogen, or serum IL-6 levels. In their paper Blasi et al did not report whether there was a relation between MIF titres and exacerbation frequency.

Blasi and colleagues found that 43% of patients when stable were positive for C pneumoniae by DNA polymerase chain reaction (PCR) using peripheral blood mononuclear cells (PBMCs). At exacerbation they have only shown data for these 34 (of 61) who consented to the antibiotic trial and all 34 were positive for C pneumoniae. In our study a further 33 patients (FEV1 % predicted, 39.8 (16.3)) were simultaneously sampled using nasopharyngeal respiratory infection for C pneumoniae when stable and during 43 COPD exacerbations. We found no C pneumoniae using a nested reverse transcriptase PCR adapted from Cunningham et al at stable baseline but nine patients (seven from induced sputum and another two in nasal aspirates)

It is a matter of some concern to us that you felt obliged to print a notice of duplicate publication for Professor Corris. While we all deplore dual publication of original scientific data, the purpose of review articles is to provide a form of CME for practicing physicians. It is therefore inevitable that, when an authority in a field is asked to give their current view on a subject, there will be considerable overlap with their previous thoughts on the subject. This does not make the article uninteresting to read, nor—as we are sure the Editors are aware—does it stop such articles being frequently referenced.

It is our belief that it is generally understood within the community that review articles by a given author are likely to contain significant overlap with previously published reviews by the same author and that, in this situation, it is rather “missing the point” to call this a duplicate publication.

To illustrate the point we enclose a list of review articles which all contain overlapping material concerning the assessment of respiratory muscle strength.1,2 With the exception of the article in Thorax (for which the invitation to write came following a prompt from us), the remaining articles were all written as a result of unsolicited requests by the editorial team of the journal concerned. Like Professor Corris’s articles, they serve a useful function because these journals reach widely differing audiences and in each case the text of the article has been aligned to fit the interests of the readership of the journal concerned.

Our belief is that reviews of this sort do serve a useful role in postgraduate medical education and, because writing them is not commonplace for people with authoritative opinions to write similar articles in more than one journal as shown by the similarities between the Harveian oration by Warrell published in the same issue of Clinical Medicine and an earlier manuscript in the Lancet.1,3 We believe such duplication is entirely appropriate, as surely it is our duty as educators to disseminate information to as wide an audience as possible. Fraud in any shape or form in science is to be wholly deplored, but let us not be so zealous in its pursuit that we smear the innocent to the detriment of us all.

At risk of another duplicate publication, we have also sent this letter to the editor of Clinical Medicine.

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References

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(28%) were positive for C. pneumoniae at exacerbation. The presence of C. pneumoniae was not associated with smoking history, FEV₁%, peak flow change at or peak flow recovery from COPD exacerbation, rate of peak flow recovery, IL-6 (fig 2) or IL-8 levels, or total and differential cell counts in induced sputum. The lack of relationship between C. pneumoniae detection and inflammatory markers at exacerbation suggests to us that C. pneumoniae exacerbations are no different from exacerbations not associated with C. pneumoniae.

We found no relationship between C. pneumoniae detection in the airway at exacerbation and exacerbation frequency (p=0.504), but Blasi et al found that C. pneumoniae positive patients (in stable COPD) had a greater tendency towards frequent exacerbation. However, the difference in exacerbation frequency between the two groups was small (6/16 exacerbations per year), and the authors need to be cautious about concluding that this difference could affect disease progression.

The main difference between the data of Blasi et al and ours is that in their study 16 of 42 patients (38%) enrolled in study 1 had sputum positive for C. pneumoniae by DNA PCR and a similar number (61/141, 43%) in study 2 in PBMCs, both during stable COPD. We sampled only once in stable COPD and found none, despite finding 28% at exacerbation. Blasi and colleagues sampled subjects repeatedly (at least four times), but it is not clear how many times they had to be positive to be defined as “respiratory samples positive for C. pneumoniae” by DNA PCR. The 16 positive patients provided 69 sputum samples; were all sputum samples positive on all occasions examined in these patients? Similarly, we were all 125 sputum samples from the 26 patients who were C. pneumoniae negative always negative? It would be helpful if the authors could give the data on the chronic nature of infection in their sputum samples.

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References

Authors’ reply
We are grateful to Seemungal et al for their comments regarding our recently published paper on Chlamydia pneumoniae and chronic bronchitis.1

Seemungal et al prospectively studied 110 patients with COPD for 1 year, evaluating serum microimmunofluorescence IgG titres, plasma fibrinogen, and IL-6 levels. They found no correlation between high IgG titres and FEV₁% predicted, exacerbation frequency, plasma fibrinogen, and serum IL-6 levels. We also found no correlation between serological results and FEV₁% predicted or exacerbation frequency. In fact, as in previous reports,2 we found a low degree of correlation between C. pneumoniae serology and peripheral blood mononuclear cell (PBMC) PCR. A greater degree of correlation was observed when IgG and IgA titres were combined but, unfortunately, no comparison is possible as Seemungal et al only performed IgG titre determinations. In any case, our findings are not truly comparable with 33 subjects in the study by Seemungal et al. The different number of subjects included in the two studies may explain some discrepancies. We do agree that caution is needed in interpreting the results of our study and stated that “our study indicate that PCR results may simply be related to PCR sensitivity, sputum quality/quantity, amount of DNA retrieved from the samples, and number of tested samples.” Seemungal et al tested a single induced sputum specimen for each stable patient whereas we analysed at least four spontaneous sputum samples for each stable patient. We defined any patient with at least two positive specimens as PCR positive. Further confirmation based on large scale trials is needed.

Our results on exacerbation frequency are based on the observation of 141 subjects for 2 years compared with 33 subjects in the study by Seemungal et al. The different number of subjects included in the two studies may explain some discrepancies. We do agree that caution is needed in interpreting the results of our study and stated that “our study indicate that PCR results may simply be related to PCR sensitivity, sputum quality/quantity, amount of DNA retrieved from the samples, and number of tested samples.” Seemungal et al tested a single induced sputum specimen for each stable patient whereas we analysed at least four spontaneous sputum samples for each stable patient. We defined any patient with at least two positive specimens as PCR positive. Further confirmation based on large scale trials is needed." We found no PCR positivity in stable COPD, whereas they found 0/33 and 9/33 (28%) in stable COPD and during an exacerbation, respectively. Considering that the rate of positivity in our stable patients is comparable to that of patients with exacerbation in Seemungal’s series, we think that “our study indicate that PCR results may simply be related to PCR sensitivity, sputum quality/quantity, amount of DNA retrieved from the samples, and number of tested samples.” Seemungal et al tested a single induced sputum specimen for each stable patient whereas we analysed at least four spontaneous sputum samples for each stable patient. We defined any patient with at least two positive specimens as PCR positive. Further confirmation based on large scale trials is needed.”

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References
Marginal benefits of adding formoterol

Price and colleagues’ conclusion that adding formoterol confers a therapeutic advantage to inhaled steroid in patients with mild to moderate asthma. During the 6 month follow up in part ii of the study the frequency of the secondary outcome of mild asthma exacerbations differed by 2.5 per patient per 6 months while the difference in poorly controlled asthma days was 1.22 per patient per 6 months. These differences, while statistically significant, are unlikely to be of real clinical relevance. Indeed, during the same period the difference in quality of life was neither significant nor clinically relevant. The main differences which were significant were in bronchodilator sensitive outcomes such as peak flow and reliever use, which are to be expected when patients are taking a 24/7 bronchodilator. These data are little different from those in steroid naïve patients in the OPTIMA trial over 12 months where the addition of formoterol to low dose budesonide improved lung function but not exacerbations, while in the same trial the addition of formoterol conferred only a small but significant reduction in exacerbations in patients previously treated with corticosteroids.2 Pointedly, neither of these studies evaluated any inflammatory surrogates. We would therefore suggest that these trials indicate that most patients with mild to moderate asthma can be adequately controlled on low to medium doses of inhaled budesonide alone, and that there is only a marginal advantage conferred by adding formoterol. Moreover, combination inhalers are considerably more expensive than inhaled steroid alone and their routine use is not warranted in primary care.

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Reference

BOOK REVIEW

Respiratory Medicine Specialist Handbooks


This is the first in a new series of specialist handbooks that aims to fill the niche between the comprehensive textbook and the pocket handbook. There are the obvious pitfalls of trying to squeeze in too much detail at the expense of accessibility or reducing the subject to little more than a series of disjointed notes. However, this book—for the most part—steers clear of both of these errors and has produced a very readable, rather than a detailed, summary of specialist respiratory medicine. The 31 chapters cover a wide variety of topics and the authors’ list is like a “Who’s Who?” of UK respiratory medicine.

It is possible to pick up this book, read a chapter in less than half an hour, and come away with an increased knowledge of the pathophysiology of the condition under study and, perhaps more usefully, the intricacies of practical management which is the focus of the book. It will therefore cater to the specialist registrar undergoing higher specialist training in providing a broad understanding in reasonable detail of most facets of respiratory medicine, but it could also be of use to the experienced physician in reaffirming, reminding, and refreshing of the basics, and perhaps updating knowledge with regard to more recent developments.

The book is attractively presented with short paragraphs of text interspersed with helpful tables and figures. For those who are stimulated to seek more information on any subject, each chapter has a selection of references for further reading. Whilst we all might aspire to read, study and inwardly digest a weighty, comprehensive tome of respiratory medicine, for most of us in busy clinical practice this proves difficult. This reviewer would therefore encourage reading and studying this excellent book as an alternative which is more affordable and possibly of more practical relevance.

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NOTICE

Scadding-Morriston Davies Joint Fellowship in Respiratory Medicine 2003

This fellowship is available to support visits to medical centres in the UK or abroad for the purpose of undertaking studies related to respiratory medicine. Applications are invited from medical graduates practising in the UK, including consultants and irrespective of the number of years in that grade. There is no application form but a curriculum vitae should be submitted together with a detailed account of the duration and nature of the work and the centres to be visited, confirming that these have agreed to provide the facilities required. Please state the sum of money needed for travel and subsistence. A sum of up to £20 000 can be awarded to the successful candidate, or the sum may be divided to support two or more applications. Applications should be sent to Dr I A Campell, Secretary to the Scadding-Morriston Davies Fellowship, Llandough Hospital, Penarth, Vale of Glamorgan CF64 2XX, UK by 31 January 2003.

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