PostScript

LETTERS TO THE EDITOR

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The editors will decide as before whether to also publish it in a future paper issue.

Duplicate publication

We are writing to express our unease at what we believe is inappropriate censure imposed on our colleague Professor Corris concerning duplicate publications.1 Professor Corris was asked to write what was essentially a CME article for Clinical Medicine on a subject that he had recently reviewed in detail for Thorax. It was inevitable that there would be considerable duplication. The same papers and information were being discussed and there are limitations in the way complex arguments can be presented. It is universally accepted that a degree of duplication in review articles is completely different from trying to pass off as a new study previously published peer reviewed papers containing original data. It is 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 Warrall published in the same issue of Clinical Medicine and an earlier manuscript in the Lancet.1 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 confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused. Fraud in any shape or form in science is to be wholly deplored, but let us not be confused.

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

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References

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 microbial 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 Serum microimmunofluorescence (MIF) immunoglobulin G (IgG) titres for C pneumoniae were measured in 110 patients (FEV1 % 39.8 (16.3)) 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 the 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 % 39.8 (16.3)) were simultaneously sampled using nasopharyngeal aspirates by PCR and the 16 of 34 of patients consented to antibiotic therapy and 16 of 34 of patients consented to antibiotic therapy and 16 of 34 of patients consented to antibiotic therapy and 16 of 34 of patients consented to antibiotic therapy and 16 of 34 of patients consented to antibiotic therapy and 16 of 34 of patients consented to antibiotic therapy. 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).

Editors’ reply

We published the statement on duplicate publication, as did the other journal concerned, in response to a concern from the experts 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

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 (stable COPD) had a greater tendency towards frequent exacerbation. However, the difference in exacerbation frequency between the two groups was small (4% 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, 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.

**References**

2. Seemungal TAR, Donaldson GC, Paul EA, et al. Effect of _Chlamydia pneumoniae_ infection on respiratory samples positive for _C pneumoniae_ during acute infection; this would mean that PCR positive sputum samples. Sixty two patients were considered PCR negative; 24 had repeatedly negative PCR results on all 113 sputum samples and two patients had a single PCR positive sputum specimen (2/12 specimens).

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 indicates the possible role of _C pneumoniae_ chronic infection in disease progression in COPD patients. Further confirmation based on large scale trials is needed." However, even a slight increase in exacerbation frequency may have a role in disease progression.

**Authors’ reply**

We are grateful to Seemungal _et al_ for their comments regarding our recently published paper on _Chlamydia pneumoniae_ and chronic bronchitis. 

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. They also found no correlation between serological results and FEV₁, % predicted or exacerbation frequency. In fact, as in previous reports, they 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 the second part of their letter Seemungal _et al_ report the results of an analysis on a further group of 33 patients who were simultaneously sampled for nasal aspirates and induced sputum when stable and during exacerbation. They found no PCR positivity in stable patients, whereas in nine of 43 exacerbations _C pneumoniae_ was detected by PCR in respiratory specimens. The authors infer that DNA positivity in the sputum is a marker of _C pneumoniae_ acute infection; this would mean that around 30% of all acute exacerbations are sustained by _C pneumoniae_. However, the gold standard for acute infection is still considered serology on paired samples. Applying both PCR and serology on paired serum samples we found an acute infection in two of 34 exacerbations confirming our previous data of an overall incidence of 5–6%. Their definition of acute _C pneumoniae_ infection may explain, at least in part, why the authors could not detect any difference between exacerbations associated or not associated with _C pneumoniae_ in terms of inflammatory response.

The reported discrepancy in PCR positivity on respiratory samples between our study and that of Seemungal _et al_ may be related to different PCR techniques. In fact, we found 16/42 (38%) PCR positive patients with stable COPD, whereas they found 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 different PCR results may simply be related to PCR sensitivity, sputum 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, whereas they defined patients with a positive PCR result in one sputum sample positive for _C pneumoniae_.

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In the second part of their letter Seemungal _et al_ report the results of an analysis on a further group of 33 patients who were simultaneously sampled for nasal aspirates and induced sputum when stable and during exacerbation. They found no PCR positivity in stable patients, whereas in nine of 43 exacerbations _C pneumoniae_ was detected by PCR in respiratory specimens. The authors infer that DNA positivity in the sputum is a marker of _C pneumoniae_ acute infection; this would mean that around 30% of all acute exacerbations are sustained by _C pneumoniae_. However, the gold standard for acute infection is still considered serology on paired samples. Applying both PCR and serology on paired serum samples we found an acute infection in two of 34 exacerbations confirming our previous data of an overall incidence of 5–6%. Their definition of acute _C pneumoniae_ infection may explain, at least in part, why the authors could not detect any difference between exacerbations associated or not associated with _C pneumoniae_ in terms of inflammatory response.

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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 4.2 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 naive 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. 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


ICU outcomes in acute respiratory failure secondary to COPD

The data presented by Breen et al regarding the outcomes of patients with chronic obstructive pulmonary disease (COPD) are encouraging and lend support to the respiratory physician often faced with nihilistic attitudes towards ventilating these patients in acute respiratory failure. However, despite the proposition by the authors that certain patients with likely poor outcomes might have been excluded, the ICU stays for both groups (intubated and non-intubated) are strikingly short. This might suggest that the threshold for intubation as opposed to non-invasive ventilation (NIV) may have been lower before 1994 than in current practice. Although the authors explain the reason for the high levels of PaO2 on admission to the ICU, it could be that the severity of respiratory acidosis may have reflected excess oxygen therapy rather than the severity of the underlying mechanical respiratory failure, thus being more readily reversible and requiring a shorter period of ventilatory support. Although the decision to intubate is not solely based upon blood gases, with the increasing availability of NIV it might be that a subgroup of these patients would now be managed using controlled oxygen therapy, respiratory stimulants, and NIV.

As a result, I suspect that the physiological state of the patient that we offer to the ICU in our current practice may be worse than in this study with commensurate outcomes (longer stays and higher mortality). Despite this, many patients still do well and studies of this type need to continue to assess predictors of unfavourable end points.

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References


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 Campbell, Secretary to the Scadding-Morriston Davies Fellowship, Llandough Hospital, Penarth, Vale of Glamorgan CF64 2XX, UK by 31 January 2003.

BOOK REVIEW

Respiratory Medicine Specialist Handbook


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, yet reasonably 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 attainable and possibly of more practical relevance.

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Notice taken
Chlamydia pneumoniae and COPD exacerbation

T A R Seemungal, J A Wedzicha, P K MacCallum, S L Johnston and P A Lambert

Thorax 2002 57: 1087-1089
doi: 10.1136/thorax.57.12.1087-a

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