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CORRESPONDENCE

Chlamydophila pneumoniae in asthma

To the Editors:

BISCIONE *et al*. [1] reported a significant association of upper airway *Chlamydophila pneumoniae* RT-PCR positivity in atopic asthmatics (cumulative rate 22%) compared with nonatopic nonasthmatic spouses (9%). In the discussion, BISCIONE *et al*. [1] argued in favour of acute, rather than chronic, infection as the explanation for their observation that detection was mostly intermittent rather than persistent. I would like to address three issues raised by this interesting study.

First, in the introduction, BISCIONE *et al*. [1] stated that *C. pneumoniae* serology could not differentiate acute from other infections. It is correct to state that serology cannot diagnose chronic infection, but there are established serological criteria for both acute primary and acute secondary infection using the microimmunofluorescence (MIF) test [2]. They also implied that *C. pneumoniae* serology was nonspecific, *i.e.* cross-reactive with other Chlamydia species [1], but specificity can be achieved by parallel measurement of other Chlamydia species. For example, the first study they cited as exhibiting these deficiencies was that of HAHN *et al*. [3], who performed a prospective microbiological and serological study that included acute and convalescent *C. pneumoniae* MIF serology and was, therefore, able to distinguish serological acute *C. pneumoniae* infection from other serological patterns. In addition, HAHN *et al*. [3] included species-specific testing for *C. trachomatis* antibody as a control, and reported that subjects without evidence for an acute *C. pneumoniae* infection had a strong, statistically significant and specific ''dose-response'' association of *C. pneumoniae* antibody with wheezing and acute asthmatic bronchitis. We interpreted these serological associations as consistent with either reinfection or chronic infection. Serial MIF testing using acknowledged criteria [2] would have established whether the positive detections reported by BISCIONE *et al*. [1] were related to acute infection or not.

Secondly, BISCIONE *et al*. [1] stated (correctly in my opinion) that their data could not distinguish acute infection, reactivation, colonisation or chronic infection. It is unlikely that a 22% cumulative incidence rate over 3 months was caused by acute exogenous infections because the annual nonepidemic *C. pneumoniae* acute infection rate in the adult population is $<2\%$ [4]. Acute *C. pneumoniae* infections can be asymptomatic or associated with only minor respiratory complaints, but a significant minority will cause lower respiratory tract illness [5]. It would be informative to know whether the BISCIONE *et al*. [1] study was conducted during an epidemic of *C. pneumoniae* infection in the community, and whether any of the positive detections were associated with an acute respiratory illness.

Thirdly, interpretation of the results is confounded by the mismatch in atopic status introduced by comparing atopic cases with nonatopic controls, *i.e.* one could argue that atopes are more susceptible to infection than nonatopes, independent of disease status. Future research should include a combination of sensitive nucleic acid detection, serial serological testing, clinical data and appropriate control groups to address the issue of exactly what type of *Chlamydophila pneumoniae* infection is associated with asthma. Uncertainty about the exact type of infection, however, should not delay performance of clinical trials to establish whether asthma is treatable with antichlamydial antimicrobials.

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REFERENCES

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From the authors:

We thank D.L. Hahn for his interest in our study [1] and for commenting upon the interesting issues he raised relating to the study and its interpretation, particularly regarding the presence of acute infection, reactivation, colonisation or chronic infection. These are important issues and informed debate about them is to be welcomed.

D.L. Hahn comments that, in the introduction, we stated that *Chlamydophila pneumoniae* serology could not differentiate acute from other infections. In fact, we stated that serology cannot reliably differentiate between past and present infection, or acute and chronic infection. We acknowledge that there are published proposed serological criteria for acute primary infection using the microimmunofluorescence (MIF) test [2]. However, these authors stated that ''standardized definitions