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TW has served as consultant for Pfizer and Abbott; JK has participated in unrestricted educational activities supported by Abbott for which speakers fees were received from 2002 to 2005 and was co-investigator on a clinical trial endorsed by Abbott which was concluded in 2005; CvdE received research grants and speakers fees from GlaxoSmithKline. None of the other authors has any conflict of interest or financial relationships with a commercial entity that has an interest in the subject of this manuscript.

REFERENCES


Association between sibship size and allergic diseases in the Glasgow Alumni Study

We read the interesting study by Kinra et al which gives us important information on the relationship between sibship size, birth order, and allergic disease in British students born in the first half of the 20th century. There are, however, a few points which we would like to raise:

1. The authors observed a stronger association between sibship size and allergy in the oldest cohort and interpreted this finding as supporting the hygiene hypothesis because of a postulated larger difference in hygiene between larger and smaller families in this cohort compared with younger cohorts. However, another possible explanation—not related to the hygiene hypothesis—is the change of determinants of family size. With modernisation and emancipation of women and the change of determinants of family size and the discovery of the biochemical rhythm in the female reproductive cycle and the increasing popularity of condoms, all taking place in the first half of the 20th century, the determinants of family size may have shifted considerably during this period with probable consequences for the association between family size and allergy.

2. A similar interaction between socioeconomic status (SES) and birth order was interpreted as—if not a chance finding—supporting the hygiene hypothesis. However, other explanations cannot be excluded if we assume a prenatal birth order effect: a stronger relationship between birth order and allergy in lower SES categories might be due to a possibly higher rate of spontaneous abortions in these groups,

3. In the comparison of the results with those of other studies, the authors point out that two “negative” studies were due to lack of power. Firstly, it should be noted that these studies were not negative. In the study by Jarvis et al a significant negative association (adjusted for birth order and relevant determinants) between allergy and sibship size was found, while in our study the corresponding association with birth order was highly significant. Secondly, in our study the adjusted association with sibship size was indeed not significant (p value for trend 0.34), but the adjusted odds ratio (OR) for one extra sibling (allergy/no allergy) was positive (1.07) while its 95% confidence interval (95% CI) of 0.85 to 1.34 excluded any important negative trends (OR and 95% CI for trend not shown in paper).

4. The contents of table 2 are not in agreement with the title: the results for asthma and combined allergic diseases are not shown.

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Dr Kinra was asked to comment, but no reply was received by the time this issue of Thorax went to press.

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In the paper entitled “COPD exacerbations: 4 – Preventing them” by S Scott, P Walker, and P A Calverley which appeared in the May issue of Thorax (2006; 61: 440–7), the dose of tiotropium used in the studies by Casaburi and Brusasco referred to in table 1 on page 444 which currently reads “18 μg twice daily” should read “18 μg once daily”.

The publishers apologise for this error.

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