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SIR,---Yuksel and colleagues describe a cheap and easy solution to a common and difficult problem.1 It cannot help but be impressed by the size of the improvement seen during active treatment.

There are, however, certain aspects of the study which concern me. Firstly, the study was not randomised and all the patients were
given active treatment after placebo. Although this is disputed by the authors, in this group of children where symptoms are likely to have been exaggerated by viral infections, one would have expected some improvement with time, irrespective of treatment. This possibility is borne out to some extent, as on active treatment there was a 27% reduction in the score for ‘runny nose/unwell’; these are symp-
toms which are suggestive of a viral infection. These symptoms could not have been expected to respond to a bronchodilator and therefore should not have been added to cough and wheeze (their table 1). Also adding
together scores for cough and wheeze (their table 1) could double the score for a single event. Admittedly the percentage fall in each symptom category, with active treatment, is remarkable.

Secondly, the authors state ‘Administration of the bronchodilator resulted in an increase in functional residual capacity both immediately before and during the two week active period’. This is patently not borne out by the results in their table 3, where there is no significant difference in functional residual capacity before and after terbutaline (mean 275 (75) and 269 (58) in the placebo period and 322 (83) and 325 (95) in the active period). Indeed two and
three subjects, respectively, showed a ‘clini-
cally significant’ decrease in functional residual capacity after terbutaline compared with four and two with a ‘clinically significant’ increase. If we are to believe, as the authors suggest, that an increase in functional residual capacity reflects an improvement in lung function, then the long term improvement in functional residual capacity on active treatment occurred without any evidence of an immedi-
ate response. The authors do not address this discrepancy between the long and the short term benefit but misleadingly suggest that an increase in functional residual capacity had occurred.

One is left with a sneaking suspicion that at least part of the improvement on active treat-
ment, in symptoms and lung function, could be related to changes with time. Before accept-
ing the alternative conclusion, that in this group of children twice daily terbutaline has a dramatic effect on symptoms, which is largely independent of its immediate bronchodilator properties, it would be wise to confirm the results with a better designed study.

Dr Greenough and Yuksel comment: We thank Dr Wilson for her interest in our paper. In that preliminary study, as no previous data was available, we set out to assess if bronchodilator given by a simple spacer had any effect on symptomatic preterm infants in the first year of life. We found over the two week period in which bronchodilator was administered a significant improvement in lung function and reduction in symptom score.

We are surprised Dr Wilson felt it necessary to reanalyse the acute lung function data as we did not claim that the change in functional residual capacity 10 minutes after bronchodilator administration via a coffee cup, resulted in a significant improvement in functional residual capacity and a reduction in symptom score. Unfortunately many infants are symptomatic in the first year of life, the coffee cup technique is cheap and easy; we therefore, like Dr Wilson, are impressed and we are also pleased with the size of improvement seen during active treatment.1

Effective bronchodilator treatment by a simple spacer device for wheezy premature infants

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Gender and the progression of Escherichia coli 0157:H7 enteritis to haemolytic uraemic syndrome

SIR,—The epidemiologic studies of Milford et al continue to advance our understanding of Verocytotoxin producing Escherichia coli and the associated haemolytic uraemic syndrome.1 Notable among their findings is the excess of females in the probable prototypic haemolytic uraemic syndrome subgroup and the observation that such excess has been observed previously in other studies. Our own studies have also previously suggested an excess of females among patients with E coli 0157:H7 associated haemolytic uraemic syndrome.2 Although in a univariate analysis this excess was not signifi-
cantly different from a control group of patients who suffered from E coli 0157:H7 enteritis only, a multivariate analysis disclosed that female gender was possibly an important predictor variable for progression to haemoly-
tic uraemic syndrome.

In order to determine whether the latter association was coincidental, we continued our study to include 37 patients with haemolytic uraemic syndrome and 95 patients with only enteritis. The sex ratios (MF) in the two groups were 17/20 and 33/42 respectively. In univariate analysis, gender was not associated