LETTERS TO THE EDITOR

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References

Interrupter resistance
Sly and Lombardi in their recent editorial suggest that interrupter resistance (Rint) measurements are useful in the management of lung disease in young children. We believe this claim needs further consideration.

Rint measurements can be helpful when change following an intervention—such as the administration of a bronchodilator—is greater than its within-occasion repeatability but, for a measurement to be useful for following change with time in the individual, it must have acceptable between-occasion repeatability. In the same issue, Beelen et al reported between-occasion variability of 0.38 kPa/l.s (2 SD of the differences between measurements) in 25 healthy children. This figure is similar to that of Chan et al who reported 72 measurements in healthy children and 95 measurements in children with stable mild asthma. In the healthy children the between-occasion repeatability was 32% expected for age, but in the asthmatic children this rose to 52%. As a hallmark of asthma is bronchial lability, this is not unexpected. These figures need to be compared with the changes seen in a patient. Pao et al showed that, in an identical group of asthmatic children, a change in mean Rint of 16% occurred with treatment with inhaled corticosteroids. Although this change was demonstrated in a group of children, it would not be picked up easily in the individual because the between-occasion repeatability of Rint is much greater than the change expected.

Rint seems to be a good tool for research and, for that reason, measurements should be standardised. However, we believe its usefulness for the practising clinician is quite limited as measurements in the individual are not sufficiently reliable on a day to day basis. It is difficult to imagine that further refinement and standardisation of the method will improve this.

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References

Authors’ reply
We thank Drs Dundas and McKenzie for their comments. We agree with them that the interrupter resistance (Rint) is able to detect short term changes in airway caliber after bronchodilator inhalation. However, we must disagree with their comment that Rint has a poor long term repeatability and their consequent conclusion that Rint is not useful for routine clinical purposes. The long term repeatability (38 days apart) of Rint measurements (2 SD of the difference between two sets of measurements) reported by Beelen et al in healthy preschool children was actually 0.37 kPa/l.s in 25 children under field conditions and 0.28 kPa/l.s in 15 children under laboratory conditions. This value is very similar to the long term repeatability

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Atopic cough
The correspondence on atopic cough between McGarvey and Fujimura/Ogawa raises a number of very important issues regarding the diagnosis and treatment of chronic cough. These issues warrant careful consideration, not only because of the huge illness burden posed by the frequency of chronic cough but also because issues of terminology and recommended treatment continue to be unclear and irregularly applied.

The “3Rs” of chronic cough—rhinitis, reflux and reactive airways (asthma)—have a certain appeal. They are recognised, often repeated (primary research articles were outnumbered by reviews, letters and case reports on chronic cough in 2002–2003), and easily retained in the short term memory of busy clinicians. In clinical practice they are useful. But there are a number of crucial issues that remain to be addressed. The 3Rs frequently coexist in patients with chronic cough, which means there are more diseases than there are patients, and that can’t be a good thing. Also, what is the best way to tell if rhinitis/reflux/reactive airways present with chronic cough?

Furthermore, the 3Rs denote a single disease mechanism—namely, activation of the afferent limb of the cough reflex at the site of the disease process (nose, airway, esophagus, respectively) which is increasingly ignorant of other relevant mechanisms in chronic cough such as eosinophilic inflammation of the airway, extrathoracic airway hyperresponsiveness,3 eosophageal dysmotility,7 and airway prostaglandin mediator release, possibly a reflection of neurogenic inflammation.9

Problems also exist in relation to eosinophilic bronchitis: a descriptive term which indicates the pattern of airway inflammation present. When first described in chronic cough, eosinophilic bronchitis was reported as a disease mechanism and a marker of a good response to corticosteroid treatment.7 Recently, the term eosinophilic bronchitis has been used as a disease label in chronic cough—that is, a diagnosis in itself.8,10 In this way, eosinophilic bronchitis has been incorporated into the anatomic-diagnostic protocol as a cause of idiopathic cough to be considered when all other avenues have failed. This is problematic since eosinophilic bronchitis occurs in all three of the “Rs”11 and is also present in most patients labelled as having atopic cough. It also ignores the excellent and prompt response to corticosteroid treatment that occurs in eosinophilic bronchitis. It is less useful to consider eosinophilic bronchitis as a disease or a diagnosis of exclusion. After serious diseases have been ruled out, then perhaps the first approach to chronic cough should be a supervised trial of ‘Roids (steroids) and, if that fails, then go for the 3Rs.

As a hallmark of asthma is bronchial lability, this is not unexpected. These figures need to be compared with the changes seen in a patient. Pao et al showed that, in an identical group of asthmatic children, a change in mean Rint of 16% occurred with treatment with inhaled corticosteroids. Although this change was demonstrated in a group of children, it would not be picked up easily in the individual because the between-occasion repeatability of Rint is much greater than the change expected. Rint seems to be a good tool for research and, for that reason, measurements should be standardised. However, we believe its usefulness for the practising clinician is quite limited as measurements in the individual are not sufficiently reliable on a day to day basis. It is difficult to imagine that further refinement and standardisation of the method will improve this.

References

Authors’ reply
We thank Drs Dundas and McKenzie for their comments. We agree with them that the interrupter resistance (Rint) is able to detect short term changes in airway caliber after bronchodilator inhalation. However, we must disagree with their comment that Rint has a poor long term repeatability and their consequent conclusion that Rint is not useful for routine clinical purposes. The long term repeatability (38 days apart) of Rint measurements (2 SD of the difference between two sets of measurements) reported by Beelen et al in healthy preschool children was actually 0.37 kPa/l.s in 25 children under field conditions and 0.28 kPa/l.s in 15 children under laboratory conditions. This value is very similar to the long term repeatability
(3 weeks apart, 2 SD of the difference between two sets of measurements) reported by Chan et al. in 72 healthy preschool children (0.23 kPa/l.s) and the long term repeatability (2.5 months apart, 2 SD of the difference between two sets of measurements) that we found in children with a history of wheezing or cough (0.21 kPa/l.s). This shows exactly the opposite result—namely, a fall in pulmonary arterial flow caused by the indirect Fick method but was artefactually increased, despite a fall in pulmonary arterial blood flow, when measured by thermodilution. We agree with the authors that cardiac output measurements were performed using the thermodilution method before and after shunt creation and that the Fick method is the method of choice. We have therefore calculated the cardiac output using both methods and, interestingly, found a very good correlation in our patient population between both methods before and after shunt creation (r = 0.83 and r = 0.78, respectively), allowing us to present data measured with the thermodilution method. However, cardiac output measured by thermodilution was significantly lower than the calculation based on the Fick method. This has been described in tricuspid regurgitation.1,4 In our patient population the mean cardiac index increased by 31% after atrial septostomy measured with the thermodilution method and by 29% when measured with the Fick method.

The most important message of our paper is that creation of the small interatrial shunt improves cardiac output independently of the method used for its calculation. This is accompanied by a significant reduction in oxygen saturation (from 93.2% to 87.4%), but systemic oxygen transport is increased. This improves the patients’ symptoms and has the potential to influence prognosis in this selected population of patients with severe pulmonary arterial hypertension.

References

Treatment of severe acute childhood asthma
I am writing in response to Dr South’s recent editorial which highlighted how second line treatment for severe acute childhood asthma
is still the subject of debate.1 I conducted a survey of consultant paediatricians who were clinical leads in asthma at 582 NHS establishments across the UK (details from official published lists) in which they were asked to indicate their department’s preferred choice for second line treatment of acute severe asthma not responding adequately to first line treatment with high dose nebulised bronchodilators and corticosteroids. I also invited them to make any additional comments or remarks.

A total of 252 responses were received (43.3% response rate), of which 25 stated that their NHS establishment either did not treat children or did not treat acute asthma. The 227 remaining responses and feedback comments for each treatment choice are summarised in box 1.

The results highlight how clinical practice can sharply contrast with clinical guidelines. It is arguable how the results should be interpreted. Should we standardise with the majority of the UK and use IV aminophylline or should we follow guidelines and use IV salbutamol, despite the evidence being unclear and it only being used by a minority of departments across the UK? It would be interesting to repeat this exercise in 10 years’ time to see what direction departments across the UK decide to follow.

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Reference

Authors’ reply
Dr Vijayadeva’s survey of the choice of second line drug treatment for severe acute childhood asthma by UK consultant paediatricians has some very interesting findings. Most of the paediatricians (70.4%) gave IV aminophylline as their preferred choice despite the fact that national guidelines recommend IV salbutamol. Could this be practice change inertia, or is it the result of years of experience with the long established agent aminophylline and the lack of conviction that IV salbutamol is better?

That there is considerable practice variation is not a surprise and may be a situation, given that the evidence for superiority between IV salbutamol and IV aminophylline remains somewhat inconclusive. I was interested to note that some prescribers had switched to salbutamol in accordance with the guidelines but were now considering changing back to aminophylline as they felt it was more effective.

My conclusion from reading the literature, as outlined in my editorial in Thorax,1 is that the limited evidence suggests that aminophylline has advantages for efficacy in severe cases but at the cost of additional minor adverse effects. The higher rate of use of aminophylline by UK paediatricians sits comfortably with this. It may be the guideline, rather than the prescribing practice, which needs to be updated.

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Reference

Chronic cough in adults
Morice and Kastelik2 have produced an excellent review of the management of chronic cough in adults. As stated by the authors, cough may arise from anywhere in the distribution of the vagus. One of the less common causes of chronic cough is disease of the ear3 due to stimulation of Arnold’s nerve.3 This nerve is an auricular branch of the vagus nerve which supplies the posterior and inferior parts of the auditory canal. In the absence of auricular symptoms an otoscopy is not usually used in the investigation of patients with chronic cough. This diagnostic possibility may therefore be overlooked, resulting in many unnecessary examinations of the upper and lower respiratory tracts.4

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References

Authors’ reply
We thank the authors for their comments on our review of chronic cough in adults.3 The otoscopic examination of the external auditory meatus forms an important part of the assessment of patients with apparently idiopathic chronic cough. In 1832 Arnold described the ear-cough reflex, consisting of cough produced by a direct stimulation of the deep portion of the posterior wall of the external auditory meatus. In healthy subjects without chronic cough the reflex is present in 2–6% of the population.4,5 In chronic cough we hypothesised that vagal hypersensitivity might give rise to increased cough reflex sensitivity. However, infusion of capsaicin or citric acid into the external auditory meatus leads to cough only infrequently in our patients.

Cough arising from the ear is rare with only 15 cases having been reported.6 In some cases surgical treatment may be required. However, foreign bodies and ear wax have been the most commonly reported causes. The diagnostic clue is that other auricular symptoms are frequently reported. However, in apparently idiopathic cough otoscopy should be routinely performed.

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