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Correspondence

Active expiration or synchrony?

Sir,

I read with interest the paper of Field *et al*[1] and the ensuing correspondence regarding attempts to prevent so called 'active expiration against the ventilator'. Those engaged in this endeavour would, I think, do well to reflect on whether they have actually shown 'active' expiration in their infants.

Greenough *et al*[2] and subsequent investigators[1 3] have shown that in some infants making spontaneous respiratory efforts a period of expiratory flow may be observed during a portion of the inspiratory phase of mechanical ventilation. That this reflects an active expiratory effort is an assumption for which little support can be found in any of these authors' publications. I suggest that they have on the contrary observed no more than a form of synchrony between infant and ventilator.

During mechanical ventilation the force determining lung inflation is the transpulmonary pressure — that is, the difference between the positive pressure applied to the airway and the pressure in the pleural space. If during the inspiratory phase of mechanical ventilation the infant makes an inspiratory effort pleural pressure must become more negative, increasing transpulmonary pressure and therefore lung volume. Should the infant then relax while the ventilator inspiratory phase continues, transpulmonary pressure and lung volume must fall and expiratory flow must occur. No *active* expiratory effort is required from the infant.

Indeed, careful examination of Greenough's published recordings of 'active expiration'[2] reveals that the expiratory flow was not associated with any rise in oesophageal pressure above baseline, a finding incompatible with an active expiratory effort. On the two occasions when expiratory flow occurred during the inspiratory phase of the ventilator, however, this flow was immediately preceded by synchronous inspiratory effort.

The only prerequisite for this pattern of expiratory flow is that the inspiratory phase of the ventilator should last longer than the infant's own inspiratory effort. It comes as no surprise, therefore, that Field *et al* could make this pattern disappear by increasing rate or decreasing inspiratory:expiratory ratio, both strategies decreasing the inspiratory time of the ventilator. They claim to have converted active expiration into synchrony; I would suggest that they had synchrony throughout. (Unfortunately, these authors published no relevant recordings.)

Regardless of the mechanism of this expiratory flow, Greenough *et al* have convincingly shown it to be strongly associated with pneumothorax. If it is correct to assume that pneumothorax is the result of overdistension of some part of the lung it is intuitively hard to see why an active expiratory effort should put the infant at greatest risk. By decreasing transpulmonary pressure such a response should limit lung inflation. On the other hand, it is easy to imagine that intermittent synchronous breaths might be exposing the lung to occasional excessive transpulmonary pressures predispose to pneumothorax.

The search for ways of achieving 'synchrony' between infant and ventilator should proceed with caution; intermittent synchrony may be much the most dangerous pattern of interaction (as I believe Greenough has shown though misinterpreted). It is unforunate that at present the search seems to be directed at preventing what is no more than passive expiration. Greenough, South and Morley, and Field *et al* seem to be in agreement that further investigation of spontaneous respiratory activity is necessary; can I suggest that as a first step they examine the electromyogram signal of the abdominal wall before deciding what is active and what is not?

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Drs Field and Milner comment:

We were very interested in the comments of Dr Ramsden and agree that simultaneous abdominal wall electromyogram signals would be useful in resolving this controversy. We would like to make two points in reply.

Firstly, we have no doubt that neonates can and quite frequently do make active expiratory efforts while receiving mechanical ventilation. May we refer Dr Ramsden to the Figure in our paper.[1] In this he will see from the oesophageal pressure trace that the baby is making vigorous respiratory efforts, which are raising the positive end expiratory pressure within the endotracheal tube by over 5 cm of water and raising the peak inspiratory pressure by a similar amount when the active respiratory effort coincides with the inflation phase of the ventilator's cycle.

Secondly, we have consistently found that converting the baby from active expiration into synchrony leads to an increase in the tidal volume. We feel that this finding is not compatible with Dr Ramsden's hypothesis.

Dr Greenough comments:

We thank Dr Ramsden for his comments. He is certainly correct to point out the association of ventilator inflation occurring at end inspiration in our paper.[2] We would, however, prefer not to describe this association as 'synchrony' as we have previously used this term to describe a very particular interaction — that is, the commencement of ventilator inflation occurring with early inspiration.[4] Neither this nor Head's paradoxical reflex (provoked augmented inspiration), both inspiratory efforts occurring