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From the authors:

We would like to thank A. McConnell and colleagues for their interest in our editorial; such pieces are intended to be thought provoking and it seems that we have achieved this goal. We should initially like to observe that one of us (M.I. Polkey), as associate editor, handled and supported the publication of P. Weiner′s paper. We do not think this is evidence of "vehement opposition" to inspiratory muscle training. Nevertheless, even if, as A. McConnell and colleagues argue, the therapy is of unequivocal benefit, this, in our view, makes understanding the basic mechanisms of even greater importance. In fact, some scepticism with regard to inspiratory muscle training is supported by a recent placebo-controlled trial of inspiratory muscle training, which concluded that "specific respiratory muscle training in highly fit competitive subjects may influence endurance exercise performance at most to a very limited extent" [1].

The relative contribution of diaphragm and ribcage muscle to exercise limitation remains a subject of great academic interest. We certainly agree that it is possible to achieve significant levels of exercise using the rib cage muscles alone, as we showed recently in patients with bilateral diaphragm paralysis [2]. This, of course, does not support the reverse: that it is possible to train the rib cage muscles in isolation. Therefore, we believe that a worthwhile training programme should increase the strength and endurance of the diaphragm, as well as that of the extradiaphragmatic inspiratory muscles. We do believe that mechanisms of action are relevant, and A. McConnell and colleagues may be interested in some recently analysed data from our study of the Powerbreathe™ [3], about which we had correspondence at the time. Both the controls and active groups in that study were also submitted to a trial of inspiratory muscle endurance, which we subsequently analysed using the protocol of HART *et al.* [4] described in the *European Respiratory Journal* in 2002. In the group allocated to active intervention, five of six subjects increased their endurance time, as did five of six controls studied (fig. 1). However, examination of the nomogram shows that this apparent improvement is achieved by alteration of breathing pattern, rather than any genuine increase in the endurance capacity of the muscle. Therefore, we remain of the view that understanding the mechanism is of utmost importance and this was reflected in our editorial.

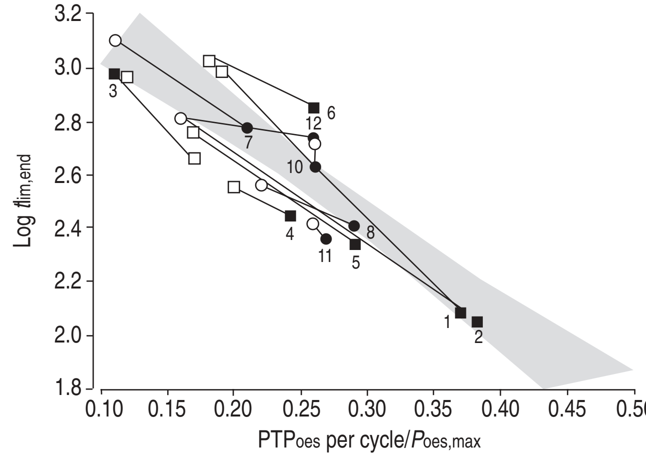

Fig. 1. – Testing respiratory muscle endurance. A plot of endurance time (Log $t_{lim,end}$) against the inspiratory muscle load to capacity ratio (PTPoes per cycle/$P_{oes,max}$) in six subjects who underwent inspiratory muscle training (■ and □) and nine subjects who underwent sham-training (● and ○) for a period of 6 weeks (numbers correspond to previously published data [3]; data are missing on subject 9, which was the subject in the control group that reduced endurance time post inspiratory muscle training). The shaded area represents the "normal values" (regression equation with 95% confidence bands) [4]. Closed symbols represent pre-training or sham-training, whereas open symbols represent post-training or sham-training, with a vertically upward movement representing an increase in endurance time. Although in most subjects there is an increase in endurance time, the data lie close to the shaded area indicating a change in breathing pattern, rather than a true increase in inspiratory muscle endurance; an increase in inspiratory muscle endurance would be observed as a significant movement away from the shaded area.

N. Hart*, J. Moxham#, M.I. Polkey*
*Royal Brompton Hospital and #King′s College Hospital, London, UK.

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