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Sir,


It was with a combination of astonishment and disbelief that I read the recently published paper by Hart et al. in the June edition of Respiratory Medicine.

The first thing that struck me was the unusually specific nature of the report. The authors had singled out a particular brand of equipment for very harsh criticism. The equipment in question (the Powerbreathe®) is only one of a number of such training devices now available. I was then astonished to find that as the inventor of this device, I was also singled out for negative comment. The first paragraph under the heading ‘Significance of Results’ (p530) reads like something from a tabloid newspaper expose, rather than a professional journal. This paragraph is extraordinary and I have never seen anything like it in a professional journal before; its tone and content go far beyond academic rivalry. Moreover, the authors substantiate their views with a negative finding that is statistically flawed (see attached critique). Failings in the author’s experimental design have led directly to their negative comment in your Journal with respect to the efficacy of the Powerbreathe®; this is unacceptable.

We believe that Hart et al’s study is fundamentally flawed as follows. The single most damning factor with respect to this paper’s credibility is one of statistical inadequacy. The authors base their conclusion that the Powerbreathe® does not improve inspiratory muscle function on one observation, viz., that twitch Pdi showed no statistically significant alteration following inspiratory muscle training (IMT). By the authors’ own admission, the reliability of twitch Pdi is extremely poor (p530), requiring unreasonably large subject numbers to identify statistically significant changes. A technique that requires 234 subjects to detect a 10% effect with 0·8 power at an α-level of 0·05 is too unreliable to be useful in any study with a repeated measures design. The authors must state what magnitude of effect size is required to produce an increase in twitch Pdi that is statistically significant using 12 subjects. In any case, it should have been obvious to the authors that it was statistically impossible to identify a significant change in twitch Pdi using six subjects per group. Accordingly, the efficacy of the Powerbreathe® was impossible to prove using their methodology.

It is extraordinary to conduct a power analysis only to dismiss its result when it suggests that an unreasonably large number of subjects are required to avoid a type 2 error. This defies logic and scientific reasoning. The result