The following is the abstract of the article discussed in the subsequent letter:

De Groote, Anne, Yves Verbandt, Manuel Paiva, and Pierre Mathys. Measurement of thoracoabdominal asynchrony: importance of sensor sensitivity to cross section deformations. J Appl Physiol 88: 1295–1302, 2000.—Discrepancies in the assessment of thoracoabdominal asynchrony are observed depending on the choice of respiratory movement sensors. We test the hypothesis that these discrepancies are due to a different dependence of the sensors on cross-sectional perimeter and area variations of the chest wall. First, we study the phase shift between perimeter and area ($\Phi_{PA}$) for an elliptical model, which is deformed by sinusoidal changes of its principle axes. We show that perimeter and area vary sinusoidally in the physiological range of deformations, and we discuss how $\Phi_{PA}$ depends on the ellipticity of the cross section, on the ratio of transverse and dorsoventral movement amplitudes, and on their phase difference. Second, we compute the relationship between perimeter, area, and the output of the inductive sensor, and we proceed by comparing inductive plethysmography with strain gauges for several cross section deformations. We demonstrate that both sensors can provide different phase information for identical cross section deformations and, hence, can estimate thoracoabdominal asynchrony differently. Furthermore, the complex dependence of the inductive sensor on perimeter and area warns against this sensor for the evaluation of thoracoabdominal asynchrony.

Piezoelectric sensor vs. respiratory inductive plethysmograph

To the Editor: We read the paper by De Groote et al. (2) and found several errors of omission and commission. Thus they wrote in the introduction, “In these studies, strain gauges, inductive plethysmographs, magnetometers, and linear differential transducers are used without any detailed discussion of their characteristics.” This statement contains errors of omission that should have been known to the authors or reviewers of the manuscript. Detailed discussion of validation of these sensors appeared in our review of noninvasive respiratory monitoring (4). Measurement of breath volumes among respiratory inductive plethysmograph, strain gauges, and impedance pneumograph, compared with the pneumotachograph, in babies revealed that the respiratory inductive plethysmograph was markedly superior to strain gauges and impedance pneumographs in estimating tidal volume in the prone posture. In this posture, waveforms were often absent from the strain gauges, because excursions were limited by compression of the baby’s body and the transducer between the mattress, thereby giving spurious central apneas and false estimations of rib cage and abdominal excursions (1). In a study prompted by the work of Martinot-Lagarde et al. (3), bench testing of the respiratory inductive plethysmograph revealed that the inductive plethysmograph measures cross-sectional area (5). Stretching a standard inductive plethysmographic transducer band around wooden dowels, placed in holes on a pegboard, to form 23 curved and 5 rectangular shapes produced an output voltage that was linear for both curved and rectangular shapes and produced changes in cross-sectional area within the physiological range. Because De Groote et al. (2) indicated that their study was based on the work of Martinot-Lagarde et al. (3), they should have reconciled their mathematical model, using the approximations mentioned in their paper, with actual data from bench testing. Finally, the last error of omission relates to the piezoelectric strain gauges used as a standard of comparison to the inductive plethysmograph. We did not find “detailed discussion of their characteristics” (vide supra) in their present paper, references in that paper, or through a search of the relevant literature. Moreover, no validation of the piezoelectric sensor to a volumetric standard or description of its linearity and frequency response is provided. De Groote et al. (2) state that strain sensitivity of the piezoelectric film, combined with the belt elasticity, determines the perimeter measurement. Taking this statement at face value, validation of this transducer would be dependent on the linearity and frequency response for perimeter measurement of both the transducer and the belt. Linearity in perimeter estimations or low-frequency response to measurement during breathing might distort the compartment waveforms and lead to errors in actual phase-angle measurements. Because “relatively stiff belts” surround the thorax and abdomen, how do the authors know that the piezoelectric sensor does not itself distort and restrict the compartments being tested for asynchrony? Finally, the authors did not consider, as a practical matter, the piezoelectric sensor estimation of phase angles during the shift from the supine to the prone posture.

Although De Groote et al. (2) did provide a schematic of the inductive plethysmographic transducer in their Fig. 7 as the basis for the mathematical model, their single example of actual data from a 4-mo-old baby used an unvalidated example of this technology, i.e., “The inductive belts were incorporated in a pajama...” We believe that such an arrangement has the potential to distort the sinusoidal layout of the inductive plethysmographic transducer, thereby altering output linearity because of mutual coupling of self-inductance caused by a more square than sinusoidal shape of the coils of the stand-alone transducer. Of course, this situation may not have occurred; however,
the authors should have been explicit on this point and have validated both the piezoelectric and inductive plethysmographic transducers applied to the body.

In conclusion, we do not believe that the theoretical evidence, unaccompanied by actual data, presented by De Groote et al. (2) justifies their warning against the use of the inductive plethysmographic sensor for the evaluation of thoracoabdominal asynchrony. Nor do we believe that the piezoelectric sensor was sufficiently tested as a reliable respiratory sensor to be utilized as a standard for comparison to inductive plethysmographic technology.

Conflict of interest disclosure: Marvin A. Sackner is Chief Executive Officer of Non-Invasive Monitoring Systems and owns 52% of its shares. No funding sources supported the writing of this letter.

REFERENCES


To the Editor: Sackner and Adams express criticisms that are the results of a misinterpretation of our paper (2). As indicated in the title, we studied the importance of sensor choice for the measurement of thoracoabdominal asynchrony, not for the evaluation of breath volume. Hence, the review of noninvasive respiratory monitoring, which is quoted by Sackner and Krieger (5), is irrelevant to our study. Our mathematical model has been validated by bench testing (1). In contrast to Watson et al. (7), we performed not only static, but also dynamic, tests in a physiological range. Because the analysis in our paper is based on a well-established theory (3, 4), we did not find it necessary to include its experimental validation. If Sackner and Adams had read our paper carefully, they would not have stated that we utilized piezoelectric strain gauges as a standard of comparison to inductive plethysmography. In the article, we consider strain gauges in general, as a class of sensors designed to be selectively sensitive to elongation. Specific piezoelectric strain gauges are used only for the introduction example, which induced the hypothesis of the study. Concerning the sensors used in the introductory example, their gain and phase characteristics, with respect to frequency as well as their sensitivity to perimeter changes, were obviously evaluated (1).

Finally, we are surprised by the reservations of Sackner and Adams concerning the use of inductive belts incorporated in a pajama, as Dr. Sackner is the inventor of the LifeShirt technology, in which inductive belts are incorporated in suits (http://www.lifeshirt.com). Because the inductive plethysmographic transducer is already made of a wire sewn into a piece of cloth, we do not think that our setup is inappropriate. Such a technique has been successfully used in adults on the ground and during the D-2 and Euromir 95 space missions (6) and, more recently, during the Neurolab flight (http://quest.arc.nasa.gov/neuron/photos/SS.html).

In conclusion, Sackner and Adams do not address any fundamental issues about the validity of our warning against inductive plethysmography for the measurement of thoracoabdominal asynchrony.

REFERENCES


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