The authors reply

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Patient-Ventilator Asynchrony During Assisted Ventilation in Children: The time to Rethink Our knowledge

To the Editor:

The study from Blokpoel et al (1) about patient-ventilator asynchrony (PVA) in children is quite interesting and raises an important issue for all PICU, where mechanical ventilation (MV) is one of the most practiced interventions. Up to 64% of all admitted children are mechanically ventilated for at least 24 hours (2). We would like to make some suggestions and remarks and contribute to this debate.

Only 45 patients were enrolled in 1 year and 6 months of study. We should ask if there were other patients in condition to be enrolled in the study and were excluded, or if this PICU really has a low rate of patients under MV. This limits somewhat the strength of this study.

An important issue is: when the researcher decided to record the patient's waves? What was the criterion? Does a 30-minute record reflect the whole situation of the patient in terms of PVA? We also would like to know if there is a statistical superiority of one mode of MV over others (is pressure regulated volume control superior to synchronized intermittent mandatory ventilation in terms of PVA?). It would be interesting if neurally adjusted ventilation assist (NAVA) was also tested to determine if it allows improved patient-ventilator synchronization (3). A study by Yonis et al (4) in adults showed that the total number of asynchronies in NAVA is lower than that in pressure support ventilation. Another interesting question is: is there a superiority of one type of MV device over others (since only one type of ventilator was used, we would not know)?

The most important finding in the study in our opinion is that a reduction in the duration of MV was observed with higher levels of asynchrony, which is completely counterintuitive, and although the authors tried to find some explanations, all of them are speculative.

This study has several limitations, but the issue is quite interesting and deserves a large well-designed multi-center study with different kinds of ventilation strategies.

The authors have disclosed that they do not have any potential conflicts of interest.

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The authors reply:

We would like to thank Colleti Junior and Carvalho(1) for the interest in our article about patient-ventilator asynchrony (PVA) during assisted ventilation in children (2). They raise a few important questions which we will address accordingly. First, the authors discuss the sample size. To put this into perspective, our unit admits approximately 800 patients per year of whom two third are mechanically ventilated. As discussed in the article, patients were randomly chosen when they were on conventional mechanical ventilation. Given the lack of knowledge on PVA in invasively ventilated children, we feel confident that a sample size of 45 patients is large enough to provide the necessary information. To compare, the study in adults by De Wit et al (3) had a sample size of 60 patients. The reason behind this is the fact that all pressure, flow, and volume tracings recorded for 30 minutes were analyzed manually. This was a very time consuming procedure. Increasing the sample size would not change the findings of our study.

Second, patients were randomly selected if they were able to trigger the ventilator and did not suffer from neurologic diseases as discussed in the article. We acknowledge that by using this technique we may have underestimated the level of asynchrony, especially when studying the prevalence of ineffective triggering. This requires further study. Blanch et al (4) performed an observational study in which they assessed the prevalence and time course of asynchronies. In this particular study, a significantly higher prevalence of asynchrony was observed to occur during the daytime.

Third, mode of ventilation did not affect the level of asynchrony ($p = 0.45$). However, it is important to keep in mind our study was not designed to study the effect of ventilatory mode on PVA. We agree with the authors that it is very interesting to study if measuring the electrical activity of respiratory muscles including the diaphragm (for instance using neurally

adjusted ventilation or transcutaneous electromyography) provides more information on PVA in children.

We fully agree with the authors that actually very little is known about PVA in mechanically ventilated children. The mechanisms underlying PVA in children are unclear, hence, we can only speculate on our finding of increased PVA with decreased ventilatory support. Acknowledging the limitations of our study, we wholeheartedly support the call for further research.

The authors have disclosed that they do not have any potential conflicts of interest.

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Strain Echocardiography in Pediatric Sepsis: Direct Guide to Hemodynamic Therapy in the Future?

To the Editor:

The article published in a recent issue of *Pediatric Critical Care Medicine* by Haileselassie et al (1) is very current and innovative, especially in relation to sepsis in pediatric patients. However, there are some points issues in the study that we would like to better understand. In the first place, the normal reference values of strain were taken from a study performed with different software than that used in the study of sepsis (2). Different reading software offers different reference values. In addition, we do not know what software was used in the "internal controls." A recent meta-analysis published by Levy et al (3) tried to establish a consensus, including 46 studies that were carried out with all the equipment available on the market.

Second, there is a wide variation between the date of admission to the PICU due to sepsis and the echocardiogram: 1–15 days. The average is 2.1 days; however, in this case, the median would better reflect the reality. During 15 days of treatment of sepsis, a lot can happen with the patient's myocardium. Furthermore, few patients were enrolled ($n = 23$) with a great range of ages.

Another issue is that no significant difference was found between the strain rate of septic patients and normal controls ($p = 0.27$). However, the authors claim that there is a significant correlation between strain rate and lactate in septic patients. What is the relevance of this correlation? Have the authors considered other biomarkers such as troponin T and amino-terminal pro-brain natriuretic peptide (NT-proBNP)? After all, myocardial dysfunction in sepsis is not only the result of hypoperfusion but also the action of inflammatory mediators. A recent study by De Geer et al (4) reported a correlation of strain echocardiography in septic shock and NT-proBNP, besides left ventricular ejection fraction.

The great contribution of this study is to make us think if, in septic patients, the replacement of the conventional assessment of cardiac function by strain echocardiography is the future.

The authors have disclosed that they do not have any potential conflicts of interest.

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The authors reply:

We appreciate the thoughtful comments by Leal et al (1) regarding our investigation (2). While some of the questions highlighted in the Letter to the Editor are addressed in this response, other comments emphasize the need for prospective projects that evaluate the utility of strain echocardiography (SE) as a metric of cardiac function in septic shock.

A concern noted in the letter addresses the variability in the postprocessing software between our sepsis cohort and the published normative values. The controls described by Marcus et al (3) utilized a vendor-specific postprocessing software (EchoPAC version 6.1.0; GE Vingmed Ultrasound AS, Horten, Norway) for offline speckle-tracking SE analysis, while the data presented in this study utilized a vendor-nonspecific software (EchoInsight; Epsilon Imaging, Ann Arbor, MI) for endocardial border tracking SE analysis (2).

Although the concept of 2D SE is fundamentally the same regardless of the software used, measurement variability between