

A similar case can be made for increasingly aggressive vaginal birth after cesareans and prolonging the first and second stages of labor, not due to a lack of training, but because studies have shown that these procedures are inherently more dangerous than CD.⁵ Besides the medical-legal problems that the obstetrician is almost surely to face when complications arise, there is the psychological toll that follows from being involved in a case that resulted in a patient's injury whether or not there was any negligence on the obstetrician's part. And therein lies the rub. The eternal conflict exists between the people sitting behind desks, dictating what practicing physicians should do without regard to the effect it will have on their patients, their practices, their lives, and their livelihoods, and the obstetrician who is responsible for the well being of two patients in every clinical encounter. Obstetricians must resist the pressure to depart from accepted safe procedures for the minefield of unproven practices, at the behest of administrators who have no evidence of their safety, for their patients' sake as well as their own. ■

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REPLY



Many thanks for your comments. We agree with every word. Let us dare to go a step further.

We are beholden to societal trends that affect our entire culture. The distortions we see in obstetrics and gynecology, this turning away from rational thought toward ideology, is universal. You refer to “people sitting behind desks”—well, the people causing such distortions are everywhere. They are in our midst because some of us have bought into this ideology and in fact have built a career on it. This is deplorable. We should never forget that we are working for the well-being of our patients, not to serve political correctness or statistical “norms.”

One would assume that the litigious environment in the United States would act to protect practitioners there from the most extreme consequences of this trend and to a degree this is the case. We are encouraged that after previous iterations were rejected, this article was published in the *American Journal of Obstetrics and Gynecology*.¹ It is a positive sign that the editorship of this journal is opening up our subject to scrutiny and debate. This gives us hope for the future of obstetrics and gynecology. ■

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Current base deficit is not a relevant marker of neonatal metabolic acidosis



TO THE EDITORS: The article by Clark et al¹ is a relevant attempt to assess the limits of electronic fetal heart rate monitoring to prevent neonatal metabolic acidosis (NMA), which is an intermediate biological marker of asphyxia and risk of neonatal encephalopathy. The challenge is to identify clinical information, biomarkers, and electrophysiological indicators that would best support clinical

decision and better identify newborns who will benefit from therapeutic hypothermia to prevent postasphyxia cerebral damage. Although results from animal studies were promising, clinical research is still inconclusive when identifying biomarkers of asphyxia in human newborns,² most probably due to the lack of specificity of these biomarkers. At the present time a reliable biological marker

of asphyxia harmful to the brain and organs is still to be found.

Clark et al¹ used the base deficit without specifying if they used in vitro or in vivo base deficit, for which results diverge if acidemia is present. Moreover, when using base deficit in vivo, correction made to reach eucapnia when hypercapnia is found is based on adult pH and PCO₂ values, which differ from newborn normal references. We have just completed a systematic critique of this topic³ that has been positively received by several leading authors in this field. The current approach in clinical biochemical laboratories to calculate base deficit overestimates (by a factor of 2-4) the frequency of NMA (2.2% in Clark et al¹ groups vs 0.6% in ours).⁴ Aiming at identifying a reliable and predictive biomarker for newborn asphyxia, we proposed a new innovative approach—birth-related neonatal eucapnic pH—that is only representative of the metabolic component of the blood acidity.

We hypothesize that the predictability of NMA is superior when electronic fetal heart rate monitoring is coupled with neonatal eucapnic pH than with base deficit. We would like to invite these authors to conduct a complementary study using neonatal eucapnic pH to verify our hypothesis. The neonatal eucapnic pH parameter is simple and easy to calculate using a tool available at the digital distribution platform Apple Store (App Store), and we are readily available to collaborate with any perinatologists interested in this new approach should they require additional information. ■

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Dr Ouellet has a copyright on the Application Neonatal Eucapnic pH (Apps store).

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REPLY



We appreciate the authors' letter, which nicely articulates an important but often underappreciated fact: at very high levels, partial pressure of carbon dioxide (pCO₂) has a significant impact on calculated base excess. Thus, in the presence of markedly elevated pCO₂ values, the traditional distinction between short-term, clinically benign fetal respiratory acidemia and longer term, potentially important fetal metabolic acidemia cannot be made using base excess and pH alone. The elevations of pCO₂ commonly seen in adult medicine are generally such that this effect is clinically insignificant. However, when using blood gas analysis in an attempt to determine the presence, severity, and duration of putative in utero oxygen deprivation, this effect often becomes important. Our use of standard base excess calculations reflects current clinical capabilities in most laboratories; our results simply confirm in clinical terms the authors' mathematical observations.^{1,2}

Various methods have been proposed to correct for this pCO₂ effect. In addition to the authors' previously published use of eucapnic pH, other investigators have distinguished the commonly performed blood base excess from extracellular fluid base excess and proposed the latter as a more accurate approach. All use mathematical models based on modifications of the Henderson-Hasselbalch and Van Slyke equations.²⁻⁶ In our opinion, all of them are valid. Thus, we do not disagree with the premise of these authors and will certainly consider their generous proposal for collaboration.

However, such collaboration would seem to us to be a very low priority for the following reasons. Even if one could perfectly correct calculation of base deficit to achieve a Platonian ideal, we are unsure just how useful this would be. While neonatal encephalopathy due to intrapartum events is rare with a pH >7.0 and a BD <-16, these are not absolute threshold values; current newborn cooling guidelines rely heavily on clinical indicia of hypoxia and recognize the need for cooling under certain circumstances with a pH as high as 7.15 and a BD as low as -10.⁷ Thus, tweaking base excess values is unlikely to change either obstetric or neonatal management, or enhance our understanding of prenatal events leading to encephalopathy.

As outlined in our study, we are of the opinion that the limits of utility of electronic fetal heart rate monitoring and cord blood gas measurements have been reached and clearly identified.¹ We have wrung just about all the useful information to be had out of these 2 valuable techniques and