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

Clark et al[1] 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 topic[3] 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 al[1] 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 Platonic 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

additional attempts at fine-tuning of these processes are not likely to yield information of significant clinical utility. Rather, we believe a refocus of research efforts on alternative or adjunctive techniques of assessment not based on heart rate and blood gas analysis will be necessary to make further significant impacts upon the prevention of perinatal neurologic impairment. ■

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Intrauterine hemostatic balloon placement: is <12 hours really better?

TO THE EDITORS: We commend Einerson et al[1] for answering a very important unsolved question: whether the Bakri balloon can be removed <12 hours after placement. Their retrospective observational study showed that Bakri balloon removal before or after 12 hours does not affect outcomes, and thus concluded "if ongoing hemorrhage has abated, it is reasonable to consider removal of an intrauterine balloon by 12 hours after its initial placement." We have a concern and an addition.

Our concern regards selection bias. Who decided early (<12 hours) vs late (>12 hours) removal? They stated, "the duration of balloon was at the discretion of the clinical provider and was not dictated by protocol." They also stated, "the study was performed at a single academic institution with a high obstetric volume, a postpartum hemorrhage protocol, and experience with balloon." It is therefore likely that experienced obstetricians ("experts") decided. Although the background characteristics did not differ between the early and late groups (their Table 1), this does not eliminate selection bias. For example, uterine contraction usually affects decision-making by an expert. If uterine contraction becomes better after balloon placement, there is no need for prolonged placement, but if the uterus contracts but occasionally becomes floppy (repeatedly floppy contraction), it may be better to place it longer. There may be no difference demonstrable in Table 1 to differentiate these 2 situations. In addition to uterine contraction, experienced obstetricians evaluate early vs late removal by intuition, which may involve, for example, bleeding pattern, placental location, and placental separation pattern. The basis of this intuition should be analyzed and determined, so that less experienced obstetricians can utilize this experience. The data of Einerson et al[1] should be interpreted as, "it is reasonable to consider removal of an intrauterine balloon by 12 hours based on the judgment of an experienced obstetrician to the extent that they can judge the merits of early vs late removal in this patient."

Our addition regards balloon prolapse. While the balloon remained intrauterine in 274 patients (study population), it was prolapsed in 33. Balloon prolapse should be prevented, and is preventable. We devised "holding the cervix" (closing the cervical ostium with forceps, preventing balloon prolapse),[2] "abdominal traction stitch" (balloon shaft being pulled cephalad through the abdominal wall),[3] and their combination[4] to achieve this goal. Depending on the situation, we use either procedure, preventing balloon prolapse. The outcome of these 33 patients with balloon prolapse is of interest. ■

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