Correspondence

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Use of the intraosseous route in resuscitation in a neonate

Dear Sir,

Intraosseous route is an alternative way of obtaining vascular access [1]. We report the case of a neonate who is still alive due to this technique.

A female aged 10 days was admitted to our institution with a 24-h history of decreased fluid intake, abnormal skin coloration and hypothermia. Her familiar and personal history was not relevant.

On admission she presented a poor peripheral perfussion, absence of palpable pulses, acidotic breathing pattern, pallor and marqued hypotonia. Cardiac murmur and fine crackels were heard at auscultation. Hepatomegaly (3 cm below costal margin) was noted. Laboratory data revealed leukocytosis with a preponderance of inmature forms, consumption coagulopathy and severe metabolic acidosis (pH = 6.98; bicarbonate = 5 mmol/l; base excess = −24.5 mmol/l). Attempts to cannulate peripheral or central veins (subclavian, femoral and jugular) failed, in spite of being an usual procedure performed at our service [2]; therefore an intraosseous needle was inserted into the right tibia. Fluids and drugs administration were initiated. During endotracheal intubation she suffered a cardiac arrest, recovering within a few minutes after the standard cardiopulmonary resuscitation techniques, including epinephrine administration through the intraosseous line, was initiated. Another intraosseous line was then placed into the left tibia for perfusion of dopamine. Both intraosseous needles were inserted into the medial surface of each tibia, 2 cm above the medial malleolus. The needles were fixed with a U-shaped splint device made by bending a tongue depressor into three parts, which were joined with strap.

Throughout the first 8 h of stay the only available route for fluid and drugs administration was the intraosseous one. A total of 240 ml fluids (sodium bicarbonate, human albumin, packed red cells, fresh frozen plasma and glucose solution), antibiotics (ampicilin, gentamicin), vitamin K and pancuronium were administered through these two lines.

After this period a venous central line cannulation became possible and both intraosseous lines were removed. There were no complications related to placement, use, or maintenance of the intraosseous route.

Patient's condition stabilized, and inotropic therapy could be discontinued within 48 h. Cultures were negatives. When shock remitted a differential pressure between brachial and femoral vessels was noted; persistent cardiomegaly was evident on chest X-ray. Aorta coarctation was diagnosed and the patient underwent cardiac surgery on day 15 after admission. At present she is at home in good condition.

It is well known the difficulty in obtaining intravascular access in critically ill pediatric patients [3]. We recommend the intraosseous route as a fast, safe and effective technique in lifethreating situations for infants and children, including the neonatal period.

Yours faithfully,

R. Martino Alba, M. J. Ruiz Lopez and J. Casado Flores

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Letter to the editor

Dear Sir,

Dr. K. L. Yang has recently confirmed that the ratio of breathing frequency to tidal volume (f/V_T) and that of inspiratory pressure per breath to maximal inspiratory pressure (P_I/P_Imax) are reliable indices of the ability to extubate, the final step of the "weaning" process [1]. I write asking Dr. Yang to clarify a few points and to make two comments on his very informative work. Figure 1 in the paper is a tracing of 16 breaths over 20 s which shows a progressive increase in subatmospheric (negative) pressure with the airway completely occluded. To what extent does the pressure obtained by this method represent true maximal voluntary inspiratory capability? Could that pressure not be the result of increased neuromuscular drive due to 16 consecutive unsatiated breaths? The method used to measure $P_I$max by Dr. Yang differs from that used in other recent reports and cited later in this letter. In the results section we are informed that the two ratios, separately *and* together are very reliable. How was the reported numeric value of the combination of the two ratios derived? I wonder if an odds ratio and on ROC analysis would add statistical strength to the overall analysis.

The paper is, in effect, the natural extension of his previous work with Dr. M. Tobin [2]. Indeed, the ratios of mean f to mean V_T in four other reports [3–6] also clearly distinguish between the failure and succes groups (see Table 1). The data from Yang's study and that from three other *prospective* studies [7–9] are given in the table. All four publications agree that the frequency: tidal volume ratio is a reasonably reliable index with a high degree of sensitivity and specificity. The same is true of the PI/$P_I$max ratio as shown in the table which gives the data from Yang's report and that from the studies by other investigators [4, 7, 8]. However, there are large differences in the group means between the studies such that it is impossible to identify an approximate universal threshold value (cut-off point) with any degree of certainty, depriving us of a normal or expected value for either ratio, particularly when one takes the reported scatter into account. Although it is incorrect to compare different patient samples, there is an apparent difference between medical and surgical patients, in the sense that the latter had lower values (see Table).

The second comment relates to the clinical application of the information provided. Given that (a) the work of breathing is a critical determinant of the ability to discontinue ventilatory support or to reduce the ventilator's contribution to total ventilation and that (b) work is proportional to the square of tidal volume $(V_T)^2$ but only twice frequency $(2\times f)$, it makes intuitive sense to use the more mathematically stringent ratio of f/V_T rather than the looser product of the two, minute volume. The clinician needs a simple, sensitive and non-invasive index of whether any changes made in the ventilator settings or other therapeutic steps were the correct ones and also needs to simply and quickly determine improvement or worsening in his/her patients' ventilatory status.

Table 1. Comparison of reports

Author	Parameter	Weaned	Not weaned	Stats.	Notes and comments
	f/V_T				
Yang and Tobin [2]					Cut-off at 105 b^{-min-1}
Tobin [3]		52.5	165.0		Mean f/mean V_T
Fernandez [4]		53.3	131.0		Mean f/mean V_T
Conti [5]		35.8	44.0		Mean f/mean V_T
Jabbour [6]		63.6	90.1		Mean f/mean V_T
Gandia [7]		71.0 (21)	103.0 (15)	Signif.	Mixed group
Capdevila [8]		50.0 (23)	69.0 (25)	Signif.	Surgical patients
Rivera [9]		40.2 (18.9)	75.4 (18)	Signif.	Surgical patients
Yang [1]		65.3 (7.1)	143.3 (15)	Signif.	Medical patients
	$P_I/P_I max$				
Fernandez [4]		5.5 (2.2)	20.1 (4.9)	Signif.	Cut-off at 15%
Gandia [7]		10.0 (3.0)	22.0 (5.0)	Signif.	Cut-off at 14%
Capdevila [8]		5.2 (1.8)	14.5 (4.2)	Signif.	Cut-off at 9%
Yang [1]		26.0 (3.0)	36.0 (4.0)	Signif.	Cut-off at 30%

I submit that the ratios may be the answer. They are certainly easier to obtain than say the equation of Jabbour et al. [6]. Perhaps Dr. Yang may wish to give us his thoughts on the use of these ratios in tracking clinical status.

Yours faithfully,

R. W. M. Wahba

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Author's reply

Dear Sir,

I appreciated the opportunity to responde to Dr. Mahba's letter. Maximal inspiratory pressure measurement is difficult in patients who are receiving mechanical ventilation. The method used in the current study was first described by Marini et al. [1, 2]. $P_I$max is affected by the lung volume, mainly due to the altering length-tension relationship and diaphragmatic configuration [3, 4]. Valid comparison on $P_I$max can be made only if the lung volume is held constant. As often is the case in critically ill patient, if maximal respiratory effort is not generated on command, P_{Imax} may be underestimated [2]. Thus, occlusion technique is necessary to induce maximal inspiratory effort (maximal inspiratory drive). Airway occlusion with a one-way valve for 20–30 s ensures that the $P_I$max is taken near the residual volume (RV) with maximal respiratory drive.

To determine the threshold value of the combination of two ratios, I used the same method as described in a previous paper [5]. For each variable, a threshold value was chosen that this threshold value provided the best discrimination between the weaning successful patients and weaning failure patients. Weaning success were those patients who met both criteria for weaning success ($f/V_T < 100$ *and* $P_I/P_I max < 0.3$) while the patients with all other combinations were classified as weaning failures. In the 3 studies references by Dr. Mahba [6–8], the ratio of $P_{0.1}/P_I max$ were examined. $P_{0.1}$ is the inspiratory force achieved in first 100 ms of inspiration and reflects inspiratory drive. This is different from P_I of this study. P_I is the inspiratory pressure during normal inspiration. $P_{0.1}/P_I max$ ratio is also a powerful predictor of weaning outcome. Recent results by Sasson et al. confirmed this [9].

In my own practice, I recorded the patient's daily spontaneous breathing pattern of a $f-V_T$ chart. This helps me to assess the patient's progress and identify patients who may be ready for weaning. Recently, pressure support ventilation has become a popular mode of mechanical

ventilation. However, there is no specific guideline to titrate the level of support. I have used f/V_T ratio as a guide to titrate pressure support. I noted that most patients tolerated the selected level of pressure support well as long as the f/V_T ratio is less than 80. The pressure support selected through this method rarely resulted in deterioration of clinical condition over time. Obviously, this is a personal observation. Future clinical research is needed to validate this method of titrating pressure support.

Yours faithfully,

K.L. Yang

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Severe cutaneous side effects of peripheral infusions with carbicarb half strength

Dear Sir,

Sepsis associated with multiple organ failure is a common cause of death in intensive care units. One of the main consequences of malperfusion is the development of metabolic acidosis. The use of sodium bicarbonate to increase pH is controversial and may actually worsen the cellular acidosis [1, 2]. The administration is generally not successful in improving either acidotic state or clinical status. Several other agents have been developed for the treatment of acidosis. The most promising of these agents are Tris-buffer (THAM, 2-amino-2-hydroxymethyl-1,3-propanediol), carbicarb (sodium bicarbonate 2.8% and disodium carbonate 3.6%, pH = 9.6) and dichloroacetate. The advantage of carbicarb should be avoiding an increase in PCO_2 [3, 4, 5].

In an attempt to examine the clinical relevance of this item in patients with septic shock and acidosis we set up a study, comparing the effects of carbicarb half strength with sodium bicarbonate infusions on hemodynamic and respiratory variables. Because of the resemblance with bicarbonate 4.2% and the lower osmolality we preferred to use carbicarb half strength. This report gives details of severe dermatologic side effects, obtained in a pilot experiment with 3 patients.

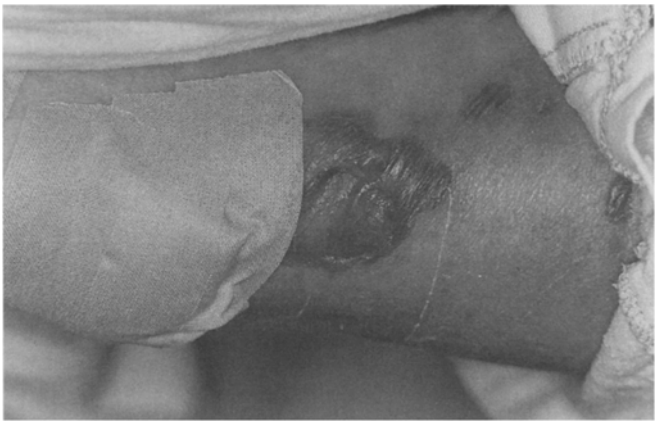

Fig. 1. Typical example of blisters caused by carbicarb half strength

These 3 patients (2 men, 1 woman, 63–74 years old) were all admitted to our intensive care unit for severe septic shock after major oncological or vascular surgery. All patients required mechanical ventilation, dialysis and circulatory support with fluids and vasoconstrictors e.g. dopamine (20–36 µg/kg/min) and norepinephrine (0.1–0.5 µg/kg/min).

Because of severe metabolic acidosis (mean pH 7.04, with normocapnia) we administered carbicarb half strength through a peripheral carrula (18 gauge, teflon) as a sole infusion. A few hours after the infusion blisters appeared around the infusion site, without pre-existing phlebitis (Fig. 1). All patients died ultimately from MOF; in one patient tissue necrotomy was performed.

The appearance of blisters and dermal necrosis in all our patients seemed to be associated with the peripheral administration of carbicarb half strength, drawn from relation in time and location of infusion. We can only speculate about the mechanism of these derma-toxic effects. It seems reasonable to assume that a substantial part of carbicarb, administered intravenously, diffuses more easily into the surrounding tissues because of (i) the increased permeability of the vessels, related to septic shock, (ii) the low flow state, also compatible with shock and (iii) augmented venoconstriction due to the use of vasoconstrictive agents. Thus an inappropiately high and toxic concentration of the alkaline agent may diffuse into the tissue. So, the chemical properties of carbicarb and the concomitant administration of vaso-active agents may explain the dermal necrosis with carbicarb half strength. This pilot study was terminated after these events. From our observation we conclude that in cases of administration of alkaline agents in patients treated with vasoconstrictors, a peripheral infusion should be avoided.

Yours faithfully,

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