A Letter Regarding Article, " Part 15: Neonatal Resuscitation: 2010 American Heart Association Guidelines for Cardiopulmonary Resuscitation and Emergency Cardiovascular Care".

To the Editor:

We have read "Part 15: Neonatal Resuscitation: 2010 American Heart Association Guidelines for Cardiopulmonary Resuscitation and Emergency Cardiovascular Care" by Kattwinkel and colleagues [1] with great interest, and admire the authors for their endeavors to provide an extremely comprehensive and very balanced overview of recent studies and evidence-based data on neonatal resuscitation. However, there are two aspects that in our opinion need to be clarified. As the authors show "table in Figure", which includes saturations measured from both preductal and postductal sites, following both operative and vaginal deliveries, and those occurring at sea level and at altitude". To verify data in this table, we attended 35 deliveries to measure their preductal saturation. Their gestational ages ranged from 37 1/7 to 40 3/7 weeks and birth body weight ranged from 2440 to 3960 g. Their preductal saturations were listed in the table 1. Basically, our data were not to tally consistent with Kattwinkel's, especially at 4 min. There were only two cases with preductal saturation below 80% at 4 min in our study. Even so, our data and Kattwinkel's both pointed out a fact that all values were becoming better even without oxygen supplement or tracheal suction to these neonates as time went by. We therefore agree that that the need to suction the

trachea for secretions and/or meconium may not be essential in a vigorous infant.

Although the tracheal suctioning is a common routine technique, suctioning of the nasopharynx can create bradycardia and that suctioning of the trachea in intubated babies can be associated with deterioration of pulmonary compliance and oxygenation. It is puzzling that Kattwinkel and colleagues [1] in this paper page S911 mentioned that tracheal suction can be associated with the "**reduction**" in cerebral blood flow velocity when performed routinely, but the same group published a similar (or the same) content in Circulation 2010 Oct 19;122(16 Suppl 2) page S518, showed that suction may result in "**increase**" in cerebral blood flow and intracranial pressure[2]. Ironically, these two papers, showing different conclusions, sited the same reference[3], which did not show any data related to the change of cerebral blood flow in suctioning. In fact, Perlman JM, one of main authors in these two similar and contradictory papers, had had determined the relationship of suctioning to changes in the cerebral circulation in 1983[4] and clearly showed a prominent increase in cerebral blood flow velocity with suctioning. Hence, the routine suctioning may have potentially deleterious effects and may endanger newborns in conditions such as cerebral hemorrhage.

Medicine has finally recognized that neonatal anatomy and pathophysiology is totally different than found in older children and adults[5]. With the advanced guidelines for the technique of resuscitation, we can provide corrective and non-harmful assistance to achieve cardiorespiratory stability in newborn infants when they are in need.

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Time	**range**	**range**	**mean±SD**

1 min	60-65%	45-85%	65.34±11.26
2 min	65-70%	53-90%	73.51±10.12
3 min	70-75%	60-98%	80.69±9.43
4 min	75-80%	75-98%	87.17±6.06
5 min	80-85%	78-99%	93.74±4.86
10 min	85-95%	85-100%	97.83±2.46

Reference List

(1) Kattwinkel J, Perlman JM, Aziz K et al. Part 15: neonatal resuscitation: 2010 American Heart Association Guidelines for Cardiopulmonary Resuscitation and Emergency Cardiovascular Care. *Circulation* 2010 November 2;122(18 Suppl 3):S909-S919.

(2) Perlman JM, Wyllie J, Kattwinkel J et al. Part 11: Neonatal resuscitation: 2010 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science With Treatment Recommendations. *Circulation* 2010 October 19;122(16 Suppl 2):S516-S538.

(3) Simbruner G, Coradello H, Fodor M, Havelec L, Lubec G, Pollak A. Effect of tracheal suction on oxygenation, circulation, and lung mechanics in newborn infants. *Arch Dis Child* 1981 May;56(5):326-30.

(4) Perlman JM, Volpe JJ. Suctioning in the preterm infant: effects on cerebral blood flow velocity, intracranial pressure, and arterial blood pressure. *Pediatrics* 1983 September;72(3):329-34.

(5) Perlman J, Kattwinkel J, Wyllie J, Guinsburg R, Velaphi S. Neonatal resuscitation: In pursuit of evidence gaps in knowledge. *Resuscitation* 2012 January 13.

February 25, 2012

Dear Dr. Fan:

We have been asked to respond to your letter of February 17, 2012, as we had participated in coordinating the data collection and drafting the NRP Guidelines that you referred to.

We will address your specific questions, but first thought it would be helpful to explain how the two documents were developed – The Consensus on Science document (COSTR), developed by the International Committee on Resuscitation (ILCOR), and the Guidelines for Neonatal Resuscitation document, developed by the American Heart Association and American Academy of Pediatrics for use in the United States. Both documents were developed after an intensive 4-year literature review, resulting in creation of item-specific worksheets that may be accessed at the end of the original COSTAR document available at DOI: 10.1161/CIRCULATIONAHA.110.971127 2010;*122;S516*-S538 We strongly urge you to read the worksheets for more detailed information, and in particular the references. The two documents do not always completely agree as to their treatment recommendations, as COSTR focuses on an international consensus about the science, while Guidelines is aimed at care practices felt to be appropriate to the U.S. Other countries, such as Great Britain, have developed their own unique guidelines. A perfect example of these differences is the COSTAR algorithm which lacks an oxygen saturation table. At an international level it was felt that the evidence did not support presentation of a specific minute by minute saturation range. However, the U.S. Guidelines writing group felt that there were sufficient data from which to guide the US provider.

As we understand your concerns, you were requesting clarification regarding two specific areas:

1. Where did the data come from in the table of oxygen saturations following birth? You had presented oximetry data you collected from 35 newborns delivered at term and concluded that data in the published table were not consistent with your data. You noted that the means values collected from your cohort were slightly higher than the published target ranges and only 2 of your 35 subjects had saturations below the target range at 4 minutes of age.
2. Why did the Guidelines refer to suctioning as resulting in a "reduction" in cerebral blood flow velocity, when the companion COSTR document, the cited reference, and work published by one of us (JMP) described an increase in cerebral blood flow and intracranial pressure?

With regard to your first concern, the data for the table were taken from a series of studies. The pertinent worksheets that cite the original references can be found at the following links: (http://circ.ahajournals.org/site/C2010/NRP-014A.pdf and http://circ.ahajournals.org/site/C2010/NRP-014B.pdf). We then compiled data from the multiple studies involving many hundreds of subjects, and adjusted the range slightly to what we felt were more easily remembered targets, rounding to the nearest 5%. One of the largest studies published after the COSTR and Guidelines documents had been finalized actually showed an even wider range of the interquartile limits (see Dawson, et al, *Pediatrics*, 2010;Jun;125:e1340-1347), so it is not surprising that the range of values that you presented from your relatively small number of subjects did not completely agree with the interquartile ranges that we presented as a guideline.

Your second concern reflects an unfortunate choice of words on our part. The COSTAR document correctly points out the increase rather than a reduction in CBFV with suctioning. While we agree that most of the published studies, including the one cited, do indeed report an increase rather than decrease in cerebral blood flow and intracranial pressure, there is no doubt that if sufficient bradycardia and hypoxemia were to occur, the resulting decrease in cardiac output might well cause a decrease in both variables. Actually, there is at least one study showing a decrease in CBF with suctioning (J Perinat Med. 2005;33(5):435-41). As you are well aware cerebral blood flow is dependent on mean systemic blood pressure particularly in a pressure passive state. Indeed the increase in CBFV with suctioning is thought to reflect the associated increase in mean arterial blood pressure (Perlman et al 1983, Kaiser et al 2005). Perhaps a more appropriate phrasing would have been "marked fluctuations in cerebral blood flow and pressures." The point is that unwarranted suctioning should be avoided for a variety of reasons, with abrupt alterations in cerebral blood flow (whether it is increased or decreased) being only one. (see link to specific worksheet at http://circ.ahajournals.org/site/C2010/NRP-011A.pdf)

With regard to which reference was chosen, we should point out that, in trying to distill literally thousands of hours of debate and a review of many hundreds of articles into a very space-limited manuscript intended to accurately convey the consensus of the COSTR and Guidelines committees, we often had to choose 1 or 2 references at the most for the sometimes dozens of references relevant to each paragraph. In the case you cited, it appears that we may not have chosen the one most representative of the points being made in the previous sentences. Again, we encourage you to consult the specific worksheets for a more references focused at the issue of interest.

We thank you for your efforts in trying to keep the evolving resuscitation guidelines as evidence-based as possible, including accurate citations of the most appropriate current literature.

Sincerely,

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