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Letters to the Editor

To the Editor:

We read with great interest the article by Rost et al.[1] describing the effects of neck position on endotracheal tube (ETT) location in low birth weight infants. Their finding that neck flexion results in a caudad movement of the endotracheal tube, whereas neck extension results in a cephalad movement, confirms previous reports[2–5] and is consistent with our clinical experience. However, we disagree with their conclusion that neck flexion should not be a principal consideration in the management of ETT location.

Due to postmortem loss of tissue elasticity, measurements performed on cadavers may not reflect the true situation in a live infant. We postulate that this loss of tissue elasticity could account for the differential effects of flexion and extension of the neck on the magnitude of tube displacement. Moreover, their report does not state whether the infants studied were appropriate-for-gestational-age or small-for-dates. The relatively large caput of the small-for-gestational-age infant might influence the position of the airway, while attempting to maintain a neutral head position and thus influence concurrent measurements. Thus, we believe the data presented should be used with caution when applied in the clinical situation. We agree with the authors that decisions regarding ETT management should be made on a case-by-case basis. Optimal ETT positioning has always been an important aspect of respiratory care of the ventilator-dependent infant. It has gained even more importance with the introduction of exogeneous surfactant treatment of infants with respiratory distress syndrome or other respiratory disorders with surfactant inactivation, e.g., streptococcal pneumonia and meconium aspiration syndrome. Misinterpretation of ETT position due to variations in neck position prior to the administration of surfactant might result in its unilateral deposition, causing complications such as unilateral pulmonary emphysema or pneumothorax. This problem can be avoided by standardization of the head position during the X-ray procedure to control the position of the tube after its insertion. For this purpose, we constructed a plastic form that was modified from the one described by Etches and Finer[6] (see Fig. 1) that holds the head of the infant in a neutral position during X-ray procedures and during the administration of surfactant. This form will ensure that the tube position during surfactant administration is equal to the photographed position (see Fig. 1). Since the size of neonates may vary widely, we constructed forms of three different sizes. Applying this device was accepted well by both nurses and radiology technicians. We believe that implementation of this device has reduced the number of refixations of endotracheal tubes that appeared to be in a suboptimal position. Whether this device will help to reduce the incidence of air leak syndrome remains to be determined and is a subject of our ongoing investigations.

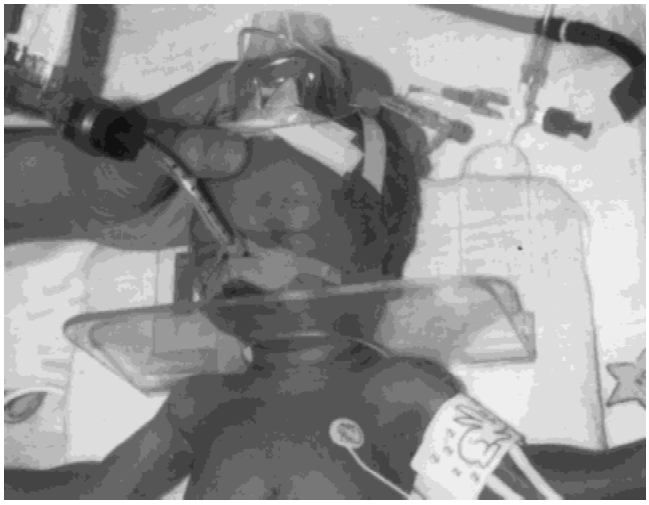

Figure 1.

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The Authors Respond:

We appreciate the comments by Drs. Harry Molendijk and Sidarto Bambang Oetomo.[1] We would like to respond to three specific points they presented.

The first point was that our results on neck flexion and extension in low birth weight (LBW) infants confirmed previous reports.[2–5] Only two of these investigations[2,3] dealt with radiographic evaluation of infants identified as LBW, and limitations of both of these investigations were individually addressed in our report. Two other investigations dealt with dissections of the trachea,[4,5] not radiographic evaluation. While conclusions were similar, the degree of flexion and extension was not quantified by any of these authors. In one investigation,[5] a group of living neonates was evaluated radiographically, but the degrees of flexion and extension and birth weights were not noted. We want to emphasize that our investigation was a systematic radiographic investigation in LBW infants with fixed, quantified degrees of flexion and extension.

The second point deals with the question of postmortem loss of tissue elasticity. Realizing that this could be an issue, we performed flexion and extension maneuvers as near to the time of death as possible. In no case did radiographic examination take place more than 30 min after death. We believe that this period is well before substantial changes in elasticity occur, although we have no proof of this.

The third point questioned whether the LBW infants we studied were appropriate for gestational age or small for dates. All but one infant (patient 7, Table 1) was preterm and appropriate for gestational age.[6] The one small-for-gestational-age infant was 37 weeks old. All of the infants fit the criteria for LBW infants. Even excluding the single infant who was small-for-gestational-age, the data on direction and magnitude of changes in endotracheal tube position did not significantly change. In addition, neutral position was defined for each child irrespective of occiput shape, cranial size, etc. We measured the differences in endotracheal tube position between flexion and extension; each subject served as his/her own control. We took the "neutral" position irrespective of individual head morphology, as is done clinically. The assumption is made that the child is in neutral position when the head is positioned as illustrated in Figure 1 of Drs. Molendijk and Oetomo.[1] While it is an interesting speculation that head sizes and shapes may effect neck position and tracheal anatomy, this would be difficult to test, as previously addressed.[6]

Use of a device for positioning the head in intubated infants is a method which the authors of the letter have found useful.[1] We have not found it necessary, providing that appropriate attention is paid to head position. For example, surfactant misadministration is exceptionally rare and even less often of clinical consequence. We would also argue that this device does not obviate attention to tension on the tubing, which we reiterate is a factor often neglected in endotracheal tube position.[6]

In summary, Drs. Molendijk and Oetomo acknowledge that assessment of endotracheal tube placement relative to neck flexion and extension must be made in individual cases. However, we have provided data in a systematic fashion that enables the intensive care nursery team to make a better informed decision about factors affecting endotracheal tube position in LBW infants.

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Role of Progestational Agents in the Treatment of Undernourished Patients With Cystic Fibrosis

To the Editor:

I have read with interest the preliminary report by Nasr et al.,[1] entitled "Treatment of Anorexia and Weight Loss With Megestrol Acetate in Patients With Cystic Fibrosis." Progestational agents have been known to improve appetite and cause weight gain. Megestrol acetate oral suspension (Megace®, Bristol-Myers Squibb Oncology, Princeton, NJ) has done so in cachectic patients with cancer and AIDS. Medroxy progesterone acetate (Depo Provera® Contraceptive Injection, Upjohn, Kalamazoo, MI) also causes weight gain. The following summarizes our experience with these medications in underweight adult cystic fibrosis patients.

We performed a nonrandomized pre- and postintervention study to assess the feasibility of using these agents as aids to weight gain in adults with cystic fibrosis (CF). All 10 patients were adults (ages 18–27) with moderate to severe pulmonary impairment (FEV_1 18–67% of predicted values, *B. cepacia* infection in 8). Treatment was prompted by a request for birth control in the 2 women given Depo Provera (150 mg intramuscularly at 3-month intervals); in 7 men whose weight was <80% ideal body weight (IBW); in 1 man who wished to decrease nocturnal gastrostomy tube feedings. Weights were recorded for 6 months prior to treatment and from 10 days after initiation of therapy for the duration of follow-up (1–16 months). Men were instructed to ingest 800 mg megestrol acetate suspension per day until weight gain was adequate, and then to reduce the dose to 400 mg per day as needed. At the end of the study period they were asked to record their perceptions of treatment related benefit and adverse effects.

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Both women, whose pretest weights were above IBW, had mean weight gains of 10 and 13 pounds. All but 2 of the men gained weight. One (FEV_1 18% of predicted values) never took the medication consistently because of an unpleasant "full" feeling. The second patient, whose pretest weight was 86% IBW, was able to maintain this weight after reducing his tube feedings. For the total group of men, mean weight increased from 76% IBW to 84% IBW ($P < 0.025$). All study patients reported a marked increase in appetite. Seven of the men experienced a weight gain that made them feel and look better. Two men experienced initial worsening of shortness of breath that resolved within weeks. Four men developed a cushingoid appearance which was a cause of distress to one. None developed diabetes, and there was no impairment of diabetic control in one woman treated with insulin.

We concluded that progestational agents are promising aids to weight gain and maintenance in selected CF patients. Prospective controlled studies are essential, however, to evaluate long-term efficacy and safety. Based on these preliminary data, we have initiated a prospective double-blind, placebo-controlled randomized trial evaluating the effects of megestrol acetate on weight, body composition, bone density, pulmonary function, exercise tolerance, resting energy expenditure, caloric intake, and quality of life. We are monitoring adrenal function and glucose tolerance. Lastly, the effects of acute and long-term withdrawal of the medication are being investigated. We are actively recruiting underweight subjects with CF (aged >17 years) for this study. The primary sponsor is the National Cystic Fibrosis Foundation, with some financial support by Bristol-Myers Squibb Oncology. Our hope is that this medication can be scientifically studied for use in cystic fibrosis *before* it becomes widely used on the basis of anecdotal data.

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To the Editor: Ibuprofen Therapy in Cystic Fibrosis

In the August 1999 issue of Pediatric Pulmonology (Volume 28:2), the Selected Abstracts Section[1] reviews an

article by Scott et al. on ibuprofen pharmacokinetics.[2] The editorial comment recommends that pharmacokinetic levels be extended to subdivisions of the first hour. The abstract states that performing hourly levels for pharmacokinetics will result in missed peaks in 90% of cases. However, the actual paper compared 3 formulations: non-chewable tablets, liquid, and chewable tablets. The authors state clearly that, "this study supports the recommended protocol for tablets for the initiation of high dose ibuprofen, that is, 1, 2, and 3 hours after administration." The liquid formulation and chewable tablets have faster times to peak and require a different schedule for measuring levels.

I am principal investigator on the Canadian Cystic Fibrosis Foundation-Medical Research Council funded trial of high-dose ibuprofen in children with cystic fibrosis (CF). We are using the non-chewable tablets and perform pharmacokinetics as recommended, i.e., hourly for 3 hours, as supported by this paper.

The pioneering work by Konstan et al[3] demonstrated the potential of anti-inflammatory therapy to significantly retard the progression of lung disease in CF. There is still much work to be done in this area. However, it is important that pharmacokinetic studies be applied in a manner that is appropriate to the formulation being used.

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Response by Author:

Dr. Lands is correct that the paper by Scott et al.[1] supports the present blood-sampling protocol for ibuprofen tablets. Certainly, additional blood sampling at 30 and 45 min is required following ibuprofen suspension to detect the peak plasma concentration. There were insufficient numbers of subjects who received the chewable tablet to make firm recommendations about pharmacokinetics. Concern about toxicity with high-dose ibuprofen is one factor limiting widespread use of this anti-inflammatory therapy in cystic fibrosis.[2] Dr. Lands and his colleagues are to be commended for their continued evaluation of safety and efficacy of ibuprofen in cystic fibrosis.

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