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Discussion

DR. ROBERT M. FILLER (Toronto, Ontario): I would like to congratulate Dr. Bartlett and his associates for their very fine presentation and the beautiful demonstration that indicates that this very sophisticated and complex method of life support can be applied to the very small infant with a minimum of complications. Dr. Bartlett's work over the past 10 years has been extraordinary and, I think, very well recognized around the world.

However, there is an issue that remains to be solved, and that deals with the conclusion that ECMO is the procedure of choice when standard ventilatory support fails. This has not been completely accepted in many neonatal centers. The main issue appears to be whether infants treated by ECMO could have been salvaged by other means.

All agree that Dr. Bartlett and his associates have selected only desperately ill neonates for treatment, but reports from other centers that do not use ECMO and employ more conventional means of respiratory support indicate equivalent survival data in what appear to be equally ill infants.

For example, I was supplied data by our neonatal intensive care head at the Hospital for Sick Children, in which we use high frequency oscillation, a ventilation method originally designed by Dr. Bryan at our institution. This has been used for infants with severe respiratory failure. For those of you who are unfamiliar with it, it is basically a technique in which a piston pump is attached to an endotracheal tube and oscillates gases at a frequency of 900 cycles per minute.

Since 1983 there were 31 neonates with severe respiratory failure from a variety of causes, similar to those treated by Dr. Bartlett and his group. This excludes infants with diaphragmatic hernia, however. The 31 patients have been treated in the past 3 years. The indications have been similar to Dr. Bartlett's, in that arterial alveolar oxygen gradients have been greater than 600 mm, with high mean airway pressures or hypercarbia with PCO_2s greater than 50, with peak airway pressures greater than 30 cm H_2O. Twenty-five of the 31 infants treated have survived, and three of the five deaths were unrelated to acute respiratory failure, results very similar to what we see with ECMO.

I am very interested in hearing Dr. Bartlett's comments on this controversial area. This remains the one part of the system that I think we need to know a little more about.

DR. LAZAR J. GREENFIELD (Richmond, Virginia): I want to express sincere appreciation and indebtedness to Dr. Bartlett not only for his excellent presentation and the opportunity to review his manuscript, but primarily for the leadership he has shown in this important area and his willingness to share his expertise.

Six years ago, one of our best residents, Tom Krummel, had the opportunity to spend time with Dr. Bartlett and was then able to establish the program for us at the Medical College of Virginia. He is now a member of our faculty, and under his guidance we have reviewed our experience with the ECMO program since 1980.

(Slide) All of the infants were desperately ill and met objective criteria for mortality rate in excess of 95% in our own institution. The technique was venoarterial and was identical to that indicated by Dr. Bartlett. As you can see, our experience is less than Dr. Bartlett's, but we have also enjoyed a number of survivors under adverse circumstances.

(Slide) We have been particularly interested in the follow-up of these patients, specifically their growth and development, and have followed their cardiopulmonary, neuropsychological and renal maturation.

(Slide) In these survivors the only unfavorable result occurred in one child who sustained three cardiac arrests and also had air embolism during the process of decannulation. This particular child now has moderate cerebral palsy but is intellectually bright.

(Slide) The first two survivors have continued to grow and develop normally. We see them now at age 5 years.

(Slide) I think these are the most rewarding aspects of this particular application.

(Slide) Our conclusion is that ECMO is a safe procedure, normal development is possible in these children, but further follow-up and widespread verification are needed.

I have some questions for Dr. Bartlett. We are interested particularly in the modifications that he is making in the technique that will allow it to be used without heparin and would like to hear the status of that application. We are wondering about the indications that he might be using at the present time for other types of pediatric support particularly in the case of congenital heart disease. Finally, some comments about the ventilator pathophysiology and whether there are means of identifying those more dangerous ventilator settings that would indicate a need for ECMO support at an earlier time.

DR. RICHARD E. CLARK (Bethesda, Maryland): I too rise to congratulate Dr. Bartlett, who had a dream 20 years ago and now has established a technique that has been taken up by others who have been able to document his initial success. I wish to ask Dr. Bartlett two questions. First, Dr. Ted Kolobow continues to work in our laboratory with young sheep and induces pulmonary barotrauma that is ameliorated with long-term ECMO support and removal of positive pressure ventilation. He is now using a single cannula technique in which blood is extracted from the circulation, allowed to resonate in a membrane oxygenator for a short period of time, and then is reinfused. This saves one cutdown and cannulation. My question is: have you tested this system in the laboratory or clinically in your neonatal group, and, if so, what are the results of this simplified system?

Secondly, I, too, like Dr. Greenfield, think that the sick child you are treating provides an excellent opportunity to study the problem of barotrauma. What clinicopathologic protocols are now in progress to study the influence of continued ventilation, various modes of ventilation, or no ventilation at all for various periods of time in relation to the regression of the severe pulmonary disease while on the ECMO system?

DR. ROBERT H. BARTLETT (Closing discussion): I thank the discussants for their comments. Dr. Filler raised the question: Is it appropriate to be using this radical invasive technique with inherent risks in babies who might survive otherwise? We obviously have paid a lot of attention to that question in this series and waited until we had seen 50 patients before we entered our controlled prospective randomized study. In that study, 100% of the patients treated with ECMO who were full-term babies survived, and all the control patients died. The statistical method is interesting in that study. I will not go into it, but I will tell you that it was published in the Journal *Pediatrics* in October 1985, and that is where you can find that documentation.

My neonatal friends around the country usually have the response that Dr. Filler articulated, that is, that babies die in other neonatal units but not in mine, and I cannot understand why we need this extravagant therapy. If that is true in that particular unit, quite obviously they do not need ECMO. I do not mean to be facetious. Improvements will occur in neonatal ventilator management that will make this technique obsolete, and they will occur fairly quickly. There are centers currently (Columbia and Toronto, for example) where the methods of ventilator management are considerably different from what they are elsewhere, and results appear to be considerably better.

Dr. Greenfield, thank you for your comments. The experience at MCV and at Pittsburgh was early corroboration of this work in other institutions, and I am very grateful that you have supported it well over these last 6 years. The work with the heparin-coated system, allowing the use of extracorporeal circulation without anticoagulation, is proceeding well but slowly. We have had sheep on bypass for 4 days with no systemic heparinization, no bleeding, and no problems. We are still months or years away from clinical application, primarily awaiting help from the FDA.

We have used the technique for support of other patient groups, as you asked. It is an excellent technique of cardiac support for the patient with biventricular failure in whom an aortic balloon or LVAD or RVAD is not satisfactory. Specifically, we treated two patients with failing hearts following cardiac transplantation (treated by Dr. Behrendt) with success, and we think the ultimate application of this technique will probably be in cardiac failure patients. We will also return to adults with respiratory disease.

Peak airway pressure is the factor that leads to the persistence of pulmonary vasospasm and respiratory failure, I believe, as Ted Kolibow has nicely pointed out over the years. We have studied that in some detail in the patients on bypass. It does provide an interesting model. I won't take the time to go into the details.

Finally, I would like to acknowledge four members of this Association who have supported this work over the years. I have mentioned Drs. Robert Gross and Francis Moore. I would personally like to thank Dr. John Connolly and Dr. Jeremiah Turcotte, my chairman in the departments where this work has been carried out. They have been incredibly supportive.