LETTERS TO THE EDITOR

Mechanical ventilation of the newborn

EDITOR—Dr Baumer, Beresford, Shaw, Manning, and their co-investigators in the two recent studies on mechanical ventilation of the newborn are to be congratulated for their hard work and persistence in performing these difficult clinical trials. In both, the authors conclude that patient triggered ventilation (PTV) provided no added benefit, in Baumer’s trial, there was a trend towards more pneumothorax in infants below 28 weeks gestation. Both studies attempted to assess important clinical outcomes, such as the duration of mechanical ventilation, the incidence of chronic lung disease, and potential neurological injury. Their inability to tease out any of the potential benefits of PTV suggested by smaller studies is disappointing. Still, I would suggest that we do not conclude from these works that no benefits are possible, nor that these techniques should be abandoned. An inescapable limitation of a large randomised trial is characterised by the Heisenberg uncertainty principle. Like the electron, optimal clinical treatment is a moving target. By its very nature, a study protocol will no longer reflect the clinical arena as well at the study’s completion as it did at the outset. In these studies, the authors evaluate PTV as a new and unproven clinical mode. Unfortunately, in neither study were they able to incorporate intermittent or continuous tidal volume measurement during ventilation. Like PTV, volume targeting is a new and relatively untested technique, although there is evidence that it may improve outcomes. Current neonatal ventilators commonly include tidal volume monitoring as a standard feature; some have the ability to provide volume controlled ventilation during a variety of patient triggered modes. One may speculate that the trend toward increased pneumothorax in the Baumer trial and the similarity in duration of ventilation and intracranial haemorrhage in both trials between the control and PTV groups may in part be due to unmeasured fluctuations in tidal volume during pressure preset mechanical ventilation.

Another major problem faced by clinicians today is the difficulty in performing studies large enough to detect differences in meaningful outcomes. The authors of both current studies comment on their inability to realistically recruit the thousands of patients needed to delineate further differences between these treatments. Thus, at a time when the technology is finally available to measure tiny changes in volume and pressure at the bedside, and to reasonably estimate the impact of these changes on individual pulmonary functions, our clinical outcomes are good enough to make assessment of these new techniques exceedingly difficult. The authors of these studies have done an admirable job. I hope we can avoid the temptation to conclude that the old ways are the best ways, and that the new techniques available for neonatal mechanical ventilation are just “bells and whistles.”

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PTV: should it be patient triggered and patient terminated ventilation?

EDITOR—We were surprised at the results of the two studies published in your journal by Baumer,1 and Beresford et al.2 Our experience with triggered ventilation over ten years is shown in table 1.

In comparison with the outcome figures in the articles, our incidence of complications of pneumothoraces (PTX), intraventricular haemorrhage (IVH) and retinopathy of prematurity (ROP) were significantly less. As explained in our original article3 and subsequently shown by others, pressure and flow triggered systems perform suboptimally in infants less than 1500 g. While bench testing may suggest an adequate response time, clinical practice indicates that these systems are compromised by the following: (1) chest wall and lung compliance, (2) airway resistance, (3) leak around the endotracheal (ET) tube, (4) ET tube resistance, and (5) systems compliance. The trigger delay may be aggravated by each of these factors, especially in the very low birth weight infants.

We believe that the inability of the patient to terminate the insufflation of gases at the onset of exhalation leads to increased intrathoracic pressure, even intracranial pressure. Thus, if there is trigger delay as postulated above, the ventilator continues to force gases into the infant during the expiratory phase causing active exhalation and with consequent deleterious effects.

The system used in our unit is triggered by modified impedance technology. Peak detectors within the system detect onset of inspiration and exhalation with sensitivity and rapidity. Furthermore, since the sensitivity depends on the rate of change of impedance, it is more sensitive when applied to very low birth weight infants or with increased rate of respiration. This may explain the marked difference in outcome in our clinical study, compared with the pressure triggered system, as shown by the application of the system in 1701 infants weighing less than 1500 g over ten years. There were 1270 infants in the same group weighing less than 250 g. The only problem we have encountered is that of some cardiorespiratory monitors are incompatible with the triggering device. The signal processing through the monitors is crucial to the optimal performance of the respiratory animal input signal to the trigger/terminator. Prototypes of the system were used initially but since 1993, the commercially available system (Sechrist SAVI, Sechrist Industries, Anaheim, CA, USA) was used exclusively.

In large multicentre studies, derivation of consensus and consistent application of a standardised “conventional ventilation” protocol is very difficult. This may skew some of the outcome data. Perhaps the limitations of flow and pressure triggered systems need to be considered prior to abandoning triggered systems in the respiratory support of newborns. Active exhalation predisposes some of these infants to the complications cited. The incidence of ROP in our experience is less than that reported in the literature. Possibly the same mechanism described above also predisposes the infants to ROP.

Given all of the above, further studies and analysis may be prudent. Such studies of patient triggered ventilation should also incorporate the capability of patient terminated ventilation.

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Table 1 Complications of prematurity 1991–1999

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<tr>
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To trigger or not to trigger?

Editor,—The international randomised controlled trial by Dr Baumer1 concluded that there was no benefit of patient triggered ventilation (PTV), but an added risk of increased pneumothorax in those less than 28 weeks gestation. In the same issue, Beresford et al2 concluded in a similar trial (with slightly more mature newborns 29 weeks ± 2) that PTV was feasible with no significant differences noted in medium term outcomes.

Chronic lung disease (CLD) is multifactorial in origin and in Baumer’s trial significant factors, like ventilation pressures, intratracheal growth retardation, use of postnatal steroids, and nasal continuous positive airway pressure (nasal CPAP) use, have been not compared between the groups.

Intratracheal growth retardation has been shown to be an important risk factor for CLD.3 In a trial where primary outcome is CLD, omission of the data regarding use of postnatal steroid use is not mentioned in their study. Beresford et al did not use nasal CPAP in any of their infants, but used synchronised intermittent mandatory ventilation (SIMV) in the weaning phase.

To the clinician, who has to make the choice of using these commonly available tools to reduce death and morbidity in this vulnerable group of infants, is the choice clear or more unclear? I leave the readers to decide.

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Dr Baumer responds on behalf of the trigger trial collaborators:

Visveshwara emphasises that the results of the trigger trial should not be interpreted as demonstrating lack of benefit for patient triggered ventilation with any of the sensors they chose to use. I would concur with this statement, which was emphasised in the paper. However, Visveshwara should not be surprised to find different outcome rates in the patients whose results he presents, as they are a different group of infants from those reported in either study. The evidence for benefit from the impedance technique is unconvincing, based as it is on a controlled study of 40 randomised cases against 110 uncontrolled cases from one centre. A multicentre randomised controlled trial of sufficient power is needed to demonstrate benefit from the impedance and patient terminated ventilation techniques he describes. To date, such a study has not been performed.

Yadav suggests that important risk factors have not been compared in the study. He describes two different types of risk factor, namely inherent factors in the infant (intratracheal growth retardation) and treatments applied to the infants (ventilation pressures, use of postnatal steroids and of continuous positive airway pressure (CPAP)).

In a large randomised controlled study, individual patients will have varying degrees of risk for the outcomes being measured. The purpose of the study design is to allocate patients in such a way that the overall risk for each arm is the same. The larger the study, the less likely that there will be an unequal balance of risk, assuming that the randomisation process is performed correctly. We reported very similar birthweights and gestations in the two groups. The proportion of growth retarded infants was therefore allocated equally, and will not have biased the results.

A study comparing two modes of ventilation cannot be conducted with the attendant clinicians blind to treatment allocation. The study protocol required all other treatments to be applied equally to infants in both arms of the study. There were written treatment protocols for each intervention. However, it is still possible that other treatments could have been applied unequally, with the possibility of bias resulting.

Interpretation of ventilator pressures is difficult, as in the trigger ventilation technique weaning was undertaken at lower peak inspiratory pressures. In Plymouth, trigger ventilated infants entered in the trial had slightly lower peak inspiratory pressures in the first 72 hours, consistent with the different weaning policy. However, as the duration of ventilation did not differ between the groups, it is reasonable to conclude that there was no systematic bias in the application of ventilation.

Information on the postnatal use of steroidaloids was collected in the trial. There was no difference in the proportion of infants receiving postnatal steroids (25.4% vs 26.0%), nor in the postnatal age at which they were first administered (median 15 v 17 days). There is therefore no evidence of bias resulting from their use.

The use of CPAP for weaning from ventilation has not been demonstrated to reduce chronic lung disease in randomised controlled trials. The paper Yadav cites discusses the possibility of testing a policy of early use of nasal CPAP. This is not relevant to the trigger trial, as infants recruited were by definition already being ventilated. I would also like to qualify Yadav’s statement that we found an increased risk of pneumothorax in infants less than 28 weeks gestation. The difference was not statistically significant, suggesting that the observed difference may have occurred as a result of chance.

The trial therefore shows no evidence of bias, and the finding that patient triggered ventilation has no additional benefit over intermittent mandatory ventilation using the ventilators and techniques studied remains valid. The trial cannot assist clinicians in their choice of other modalities of support such as early use of CPAP or postnatal use of steroids and is applicable both to growth retarded as well as appropriately grown preterm infants.

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chronic lung disease could be confidently detected; no such effect was demonstrated. Although we applaud Baumer's effort in organising a trial of this magnitude, we must caution that the conclusions should be confined to the specifics of this trial and may not be representative of PTV in this population.

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Dr Baumer comments on behalf of the trial collaborators:

It was heartening to see the interest shown in the trigger ventilation trial, and I share the correspondents' caution that this trial can only show the effectiveness of the trigger ventilation devices, to find better methods for detecting asynchrony, and to investigate the use of different approaches to PTV such as the ones suggested by the correspondents.

I would like to use this opportunity to pay tribute to the two trial coordinators (Sue Ellis and Tom Mill), to the trial statistician (David Wright), and to the data monitoring committee (David Field and Diana Elbourne), whose details were inadvertently omitted from the final paper, and without whom, together with the trial collaborators, the study would not have been possible.

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