patients, Jogi was careful to vent the pulmonary artery and keep the tricuspid valve incompetent until the right atrium could be closed without admitting air; however, the increase in VD/VT still occurred.

The suggestion that the changes might have been caused by a reduction in pulmonary artery wall tension was presumably made because most of Yates' patients had pulmonary hypertension. However, we saw the increase even in our youngest patients with ASD, and these probably had normal pulmonary artery pressures. The magnitude of the increase was not age-related and, therefore, probably not pressure-related. Quite possibly, the reduction in pulmonary flow (by a factor of 2-3 in most patients) was important for the observed transient failure of pulmonary capillary homeostasis. The flat single breath test phase III, typical also of hypoperfusion, supports this [4]. The improvement in gas exchange after sternal suture suggests that the lung quickly adapts to the reduced flow; ventilation is not a problem after operation.

Yates and colleagues [1] also found that carbon dioxide elimination during undisturbed anaesthesia was (insignificantly) lower in the cyanotic children than in those with LR shunting. Notwithstanding the lack of significance, the authors suggest that the difference was the result of less efficient ventilation in cyanotic children. This explanation implies that steady state had not been achieved at the time of measurement, and that carbon dioxide was being retained in these children, but not in the others. However, at the second measurement, PaCO2 was further reduced in the cyanotic children, and had increased in the others. The non-steady state explanation is therefore wrong. If there is a true biological difference, it may result from lower tissue carbon dioxide production. Is it not possible that cyanotic children consume less oxygen and, therefore, produce less carbon dioxide? My own preliminary results suggest that this may indeed be the case.

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REFERENCES


HEAT AND MOISTURE EXCHANGERS

Sir,—We had been waiting with some interest for the publication of "Heat and moisture exchangers in artificial ventilation" by Tilling and Hayes [1] as this is the key reference to the test method used in Health Equipment Information 166 [2].

The authors have designed a unique test apparatus in the belief that it is possible for patients to exhale air which is not fully saturated with water vapour. On basic physical principles this seems most unlikely: gas from a ventilated perfused alveolus must be fully saturated with water vapour; as this gas is exhaled it will cool by a few degrees, but will remain fully saturated.

In their discussion the authors admit that their unconventional hypothesis, that the exhaled air from patients can be significantly and variably desaturated, will be contested, but they say that only measurements on patients will settle the question. It is unfortunate that they did not consult the published literature before arriving at their hypothesis. Koch and colleagues [3] measured the temperature and humidity in a patient with a tracheostomy and with a heat and moisture exchanger (HME) fitted, and demonstrated that the expired air was fully saturated. Further confirmation is found in studies on patients in whom the trachea was intubated [4], who did not have the benefit of an HME: again, 100% saturation was found during expiration. Our initial studies with a modified commercial dewpoint hygrometer (Series 3000 Dew Point Meter, Mitchell Instruments Ltd, Unit 9, Nuffield Close, Nuffield Road, Cambridge CB4 1SS), not surprisingly, confirm that the expired air is effectively fully saturated when it leaves the tracheal tube in patients connected to an HME.

In the introduction the authors refer to two publications [5, 6] relating to "possible and actual drawbacks" of HMEs. Neither of these papers mentions such devices, let alone comments on their use. The first deals with the evaluation of heated humidifiers and the second is an investigation of the use of in-line breathing filters when using a hot water humidifier.

It is difficult to see the logic or practical value in that part of the investigation where the authors used a tidal volume of 54 ml when testing an HME with a deadspace of 97 ml [2]. It is not surprising that they describe the results of this part of the test as "interesting" and invoke either uptake of energy from the surroundings or an exothermic reaction to try to explain an apparent production of energy by the HME.

If there is interest in the function of a paediatric HME and the effect of leaks on its performance, the study by Gedeon and co-workers [7] used a model which gives a more accurate analogue to the clinical situation.

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Sir,—Thank you for the opportunity of replying to Drs Bethune and Latimer. First, we must take issue with their statement that our paper is the “key reference to the test method used in HEI 166”. This is not the case. As we pointed out in HEI, the method used to produce the results in that publication was closely modelled on that of Weeks and Ramsey [1], although we chose to use a humidity sensor which we had described elsewhere [2]. We constructed a new, somewhat simpler test rig to confirm a suspicion we had formed during our DHSS work that leaks between heat and moisture exchangers (HME) and our ISO test lung might be influencing the humidity delivered at the patient connection port of the system.

In our account of the method, we have stated clearly that water was introduced into our relatively crude lung simulator, the Lung Ventilator Function Analyser, to produce saturation of its contained gas by water vapour and that saturation was confirmed before and after each test. We think the sentence at the opening of our discussion may have been misinterpreted as a description of our method. What we wished to imply was that the generation of saturation at the point of production of humidified gas in such test rigs does not guarantee saturation under all conditions in adjacent parts of a dynamic system.

We did consult the literature, including the classic work of Dery and co-workers [3], as this has long been a natural starting point for any serious study of respiratory humidity, together with other sources confirming Dery’s original observations. We agree that alveolar gas is almost certainly saturated with water vapour at the prevailing temperature, but that is not to say that, when gas is introduced into the large airways from the exterior and mixes with gas leaving the alveoli, this condition still obtains. The graph included in a communication from Drs Bethune and Shelly [4] confirms this, in that they showed the “humidity range in the upper trachea during inspiration with nasal breathing” to be in the range 56–78% R.H. at 37 °C, attributing this to Dery. We suspect that the hygrometer to which Drs Bethune and Latimer refer is insufficiently rapid in response to reflect the changing conditions and must be used outside the respiratory tract, albeit at a site in communication with it. Of course, any significant decrease in temperature will ensure saturation down-stream and we do not dispute that the gases reaching an HME are likely to be saturated, as we believe would have been the case in our study. Nevertheless, the water content at the lower temperature would be lower than that of gas saturated with water vapour at body temperature.

The reference [6] (DHSS, 1986) in connection with the “possible and actual drawbacks” of HME is correct. These appear on page 36 of HEI No. 166. We could also have added the DHSS’s “Hazard” Health Notice HN(HAZARD) (857), referring to a problem with the proprietary device which Dr Bethune favours, but, instead, mentioned this also on page 36 of HEI No. 166. We think the title of Buckley’s paper indicates its content and the reason for it having been cited. Moreover, he refers to the dangers of linking HMEs with heated humidifiers, a practice which has been indulged in by those believing that the output of HME might need to be supplemented.

The tidal volumes were representative values in the clinical range produced by the ventilator settings and have no individual significance. Although we knew that the ICOR HME for neonates was to be released, it was not on sale at the time of the study. Therefore, we used an adult HME which we knew to be one of the most efficient. It may be that the compressible volume which Drs Bethune and Latimer describe as “deadspace” could have had a detrimental effect on the results, especially in the case of the smallest delivered volume. Conversely, it might be argued that failure of some gas to pass out of the HME and re-enter it might have had the opposite effect. All we set out to show was that the presence of a leak detracted from the efficiency of an HME and that this might be in some way related to the magnitude of that leak. We have not purported to have discovered the reasons for the deterioration, other than the obvious one of gas lost from the system, but we tried to stimulate further interest by outlining the possibilities from a thermodynamic viewpoint. Further, all models thus far, including our own, are the crudest imitations of the lung itself and we believe we are right to say that work in human subjects is the only way to confirm the relevance of the findings for patients. In the meantime, we hoped that our work might alert practitioners humidifying patients with leaks around tracheal tubes to the possibility that values of humidity lower than those normally returned by their HME might apply. These circumstances are common in neonatal and small paediatric practice and the leak becomes more severe when compliance decreases or airways resistance increases.

The study by Gedeon, Mebius and Palmer [5] was not available to us when we submitted our paper for consideration, but we did refer to the new HME from ICOR, the subject of Gedeon’s study, believing that it might show an improvement on its adult counterparts when specially designed for the application. We were, of course, delighted to learn that these workers had noted the same deterioration in performance when leaks were present and had investigated it independently, with conclusions similar to our own, but that the device had performed well under stringent test conditions.

We noted that HME are being used extensively and increasingly and that clinical experience in neonates in a centre of known excellence had been satisfactory [6]. We acknowledge the value of HME in many circumstances, but we do not accept that they are the only or necessarily the best answer to all humidification problems.

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