Weyland and colleagues⁴ described a laboratory validation in a pediatric setting comparing the Deltatrac system with mass spectrometry and wet gas spirometry. They found a mean bias of -3.2% (SD 11.5%), including the following conditions: F_{IO_2} (inspired oxygen fraction) 0.21 to 0.8, F_{EO_2} (expired oxygen fraction) 0.01 to 0.05, V_E (expired volume) 300 to 6000 mL/min, V_T (tidal volume) 8 to 300 mL, Paw (airway opening pressure) 10 to 60 mbar, and relative humidity 10% to 60%.⁴ These wide ranges of respiratory variables might explain the wider ranges of the standard deviation.

Bauer and colleagues⁵ performed an in vitro validation and clinical testing for ventilated preterm infants. In vitro validation was done by methanol burning during intermittent positive-pressure ventilation (IPPV) with two commonly used ventilators: Sechrist IV 100B (Sechrist Industries, Anaheim, Calif) and the Infant star software version 83 (Nellcor Puritan Bennett Inc, Carlsbad, Calif). They found a mean difference between VO_2 measurements using the Deltatrac II system and VO_2 determined by the methanol burning method from -2% to 2% (max SD $\pm 5\%$) depending on the ventilator and peak inspiratory pressure they used.

For clinical testing, they developed a breath collector to avoid gas leakage around uncuffed endotracheal tubes or during nasal continuous positive airway pressure (CPAP). This test arrangement simultaneously sampled expired air expelled at the ventilator and escaping via the tube leak from the infant's mouth and nose and allowed VO_2 measurements that are not affected by tube leaks.

They studied 15 preterm infants during IPPV and 10 preterm infants during nasal CPAP and determined a mean difference of VO_2 of 6.5 (SD 1.5) mL $\cdot$ kg⁻¹ $\cdot$ min⁻¹ during IPPV and 6.6 (SD 2.2) mL $\cdot$ kg⁻¹ $\cdot$ min⁻¹ during nasal CPAP.

In conclusion, these studies prove that the Deltatrac II system is a feasible and reliable device to measure VO_2 in ventilated infants if air leaks can be excluded.

We determined air leak by measuring inspiratory and expiratory tidal volume using the features of the ventilator (Siemens Servo Ventilator 900 D; Siemens, Erlangen, Germany). As described in the Methods section, we had to exclude 12 patients because of air leakage of more than 5%.

Finally, Berger and Bergstra correctly pointed out that a shunt ratio can be calculated using oxygen saturation measurements. A determination of pulmonary or systemic blood flow is necessary to calculate absolute shunt volumes.

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Triiodothyronine in neonatal heart surgery: Looking for an answer To the Editor:

We congratulate Mackie and colleagues¹ on their randomized study on triiodothyronine (T3) treatment in neonatal heart surgery. In their study group the authors correctly enrolled a homogenous group of infants at highest risk of postoperative low cardiac output syndrome and marked thyroid hormone suppression.² As already shown by several other authors, the thyroid hormone levels are suppressed after cardiopulmonary bypass (CPB), decreasing to a nadir at around 48 hours after CPB and recovering over approximately 1 week. Logically, treatment with T3 after CPB raises the T3 plasma concentrations to normal levels, and no negative effects have been demonstrated so far. Previous studies, as well as

the study presented, had relatively small subject numbers, thereby limiting the ability to determine a clinical effect. This led to the initiation of the Triiodothyronine for Infants and Children Undergoing Cardiopulmonary Bypass (TRICC) study, a randomized, multicenter clinical trial designed to determine safety and efficacy of T3 supplementation in children less than 2 years of age undergoing surgical procedures for congenital heart disease.³

We would, however, counsel some caution in the interpretation of the main results of this study mainly for two reasons:

First, to our knowledge many centers worldwide would use in these patients peritoneal dialysis (PD) in the early postoperative phase to minimize the effects of systemic inflammatory response and achieve adequate fluid balance.⁴ Usually peritoneal catheters are placed at the end of the operation to drain excess fluid as a routine institutional procedure, and PD is started after admission to the intensive care unit either on a prophylactic basis or after oliguria, anuria, fluid overload, hyperkalemia, or acidosis. PD is able to achieve adequate negative fluid balance in nearly all instances and treat other underlying causes simultaneously. Thus the positive achievement of negative fluid balance at day 2 versus day 2.5 may be clinically negligible under most given circumstances.

Second, the authors quote the effect of the T3 infusion on the cardiac output, systolic blood pressure and, herewith, indirectly on the requirements of catecholamines. Unfortunately, no information is provided on the absolute use of vasodilators (such as phenoxymethylamine, sodium nitroprusside, and milrinone), nor is there differentiation in the catecholamine use with regard to dopamine. It is widely known that dopamine has a direct negative effect on thyroid function and thyroid hormone levels in infants and children.⁵ Other factors such as the use of amiodarone should be discussed too. Finally, the overall dropout rate of cardiac output measurements in both groups (14/42) seems relatively high and unbalanced between the two small study groups. This additional information seems crucial to enable a more objective assessment of the study.

Despite these limitations, we believe that the study by Mackie and associates adds valuable information to the controversial dispute on the efficiency of T3 as

an inotropic agent after CPB. However, we hope some of the questions may be answered by the results of the TRICC study.

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Reply to the Editor:

We appreciate the interest of Haas and Camphausen in our recently published trial of triiodothyronine (T3) in neonatal heart surgery.¹ The points they raised are addressed below.

Our center, like many other centers performing infant heart surgery in the United States, routinely uses high-dose intravenous loop and thiazide diuretics rather than peritoneal dialysis to manage postoperative fluid overload. We agree that the results of our study cannot be generalized to patients who are treated with peritoneal dialysis.

The inotrope score used in our study, adapted from Wernovsky and associates,² was determined by the use of dopamine, dobutamine, milrinone, epinephrine, and norepinephrine. The cumulative dose of dopamine in particular did not differ between treatment groups during the first 5 days after the operation. Vasodilator use

was also similar between treatment groups: milrinone was used routinely in our patients, with no difference in cumulative milrinone dose between treatment arms at 5 days. A single patient (randomized to the T3 group) received nitroprusside; no patients received phenoxymethamine. Amiodarone was not used in any subject.

We planned our trial so that cardiac output was one of two primary outcome measures. The technique used (direct measurement of oxygen consumption by real-time gas exchange) provides an objective determination of cardiac output in children who are sedated, ventilated, and stable.³ During the study period, infants were weaned from mechanical ventilation at increasingly shorter times after surgery, rendering this measurement infeasible at 48 hours after surgery in several subjects. However, the proportion of patients in each treatment group in whom this end point could not be measured (9/22 in the T3 group and 5/20 in the placebo group) was not statistically significant ($P = .34$).

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A new modeling for each population To the Editor:

I read with great interest the recently published study by Fakler and associates¹ in the August 2005 issue of the *Journal*. They found, in their investigation, a poor agreement between the measured oxygen consumption (VO_2) and the VO_2 estimated using the formulas of Krovetz and Goldbloom² and LaFarge and Miettinen.³ According to the Krovetz-Goldbloom formula, the mean difference was -53 mL/min/m^2 and the limits of agreement were -95.5 and -11.1 mL/min/m^2 for the lower and upper limits, respectively. Use of the Krovetz-Goldbloom formula led to a systematic and significant overestimation in VO_2 values ($P = .0001$). However, a significant correlation was shown between measured and assumed VO_2 values with this formula ($R = .61$; $P = .0001$). According to the LaFarge-Miettinen formula, the mean difference was -15.6 mL/min/m^2 and the limits of agreement were -120.0 and 88.8 mL/min/m^2 for the lower and upper limits, respectively. A systematic and significant overestimation was also reported using the LaFarge-Miettinen formula. A significant correlation was, however, indicated between measured and assumed VO_2 with this model ($R = 0.38$; $P = .0037$). I speculated that the reason that the measured VO_2 values were significantly different from the assumed VO_2 using both Krovetz-Goldbloom and LaFarge-Miettinen formulas was because of a methodologic weakness in statistics and modeling.

As discussed by the authors, these differences between measured VO_2 and VO_2 estimated by the formulas might be due to a difference of population. I agree with this statement, but more credible is the argument that these differences are explained by the coefficients of these formulas (Krovetz-Goldbloom formula: $\text{VO}_2 = a \cdot \text{height} + b \cdot \text{weight} - c$; LaFarge-Miettinen formula: $\text{VO}_2 = a' - b' \cdot \ln(\text{age}) + c' \cdot \text{heart rate}$ for male subjects and $\text{VO}_2 = a' - b'' \cdot \ln(\text{age}) + c' \cdot \text{heart rate}$ for female subjects). These coefficients (a , b , c and a' , b' , b'' and c') depend on the characteristics of the population included in the study. That is why the use of published formulas without any coefficient correction cannot be applied in other populations, as shown in another study.⁴ To overcome this problem, new formulas with new coefficients should be calculated from the population