In Reply.—We regret that Drs. Black and Mackersie clearly have misunderstood the most important result of our study. Our primary goal was to find the effective dose of rectal acetaminophen in children during a day-case surgical setting. When we designed the study groups, we seriously thought to include a small-dose group (10 mg/kg) instead of a placebo group; however, our clinical impression has been that this small dose has no effect on pain. Therefore, a pure placebo group was included in the study. Our anesthesia method with sevoflurane in nitrous oxide and oxygen provides excellent cardiovascular, endocrine, and ventilatory stability for superficial surgery. Pain was assessed and treated postoperatively as effectively as possible. Therefore, we did not see ethical compromises in our study design. The design enabled us to find an effective dose of acetaminophen for 50% of subjects.

Pain treatment of pediatric patients still is often guided by traditions or clinical impressions. Most likely, a balanced pain treatment approach provides better pain control than a single drug. However, to provide effective components for the balanced technique, we have to find the dose-response relation of these single components, and the possible synergism between the components. We recommended that a single dose of rectal acetaminophen should be at least 40 mg/kg and that a daily dose should be limited to that published previously. We do not recommend increasing the daily dose of acetaminophen, but suggest that a high single dose produces favorable clinical response beyond its expected pharmacokinetic profile. Our young patients would definitely benefit if similar study designs are carried out using other nonsteroidal antiinflammatory drugs and combinations of pain killers in children.

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pretation of clinical observations that may be compatible with brain death (but suggest otherwise) and critical evaluation of the apnea test procedure. These topics can be found in a practice parameter developed by the American Academy of Neurology and approved by its executive board. Many hospitals in and outside of the United States have adopted this parameter in its present form, or in a slightly modified form. These parameters are used by neurologists as the guidelines for declaration of brain death.

Can Brain Death Testing Be Perfect?

To the Editor:—I appreciated the excellent review of brain death by Van Norman. However, I must take issue with the implication that well-conducted testing will always correctly indicate whether a patient is dead or alive. As with all complex algorithms, any test sequence for the diagnosis of irreversible brain death may have hidden pitfalls, just as all software of any significant complexity will manifest occasional “bugs.” If we accept the notion that, as with all medical tests, testing for brain death has an associated sensitivity and specificity, we must also accept the notion that type I and type II testing errors will inevitably occur. This view is also supported by occasional reports of clinical conditions mimicking brain death. Finally, if one accepts the notion that still-living but impaired brain stem nuclei may sometimes recover to a degree, it is possible that some nuclei will wax and wane in function during the test period.

I am curious about what should be done with patients with zero prognosis for survival, but who still do not meet all brain death criteria because some small patch of neurons continues to survive. In most of these cases there is no hope of survival, life support is withdrawn, and somatic death follows promptly. I would suggest, however, that some persons—including myself—would be willing to allow organ retrieval to be performed in such a setting (i.e., a setting of “near-complete” brain death) rather than have the organs go to waste. But if I were to construct my living will to allow organ harvesting in this situation, would it be honored?

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