

CORRESPONDENCE

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In Reply:—We thank Dr. Mackenzie for his comments. The reason we submitted this article for publication was to share our experiences with this simple and effective means of aerosolizing lidocaine for topical airway anesthesia. Despite an extensive search of the literature, we were not able to find a technique similar to that described in our report, and it would be such a loss to keep this method to ourselves because “it really works!” We appreciate very much that Dr. Mackenzie called to our attention the similar device he and Dr. D. Tran had developed previously.

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Acetaminophen Dosage in Pediatric Practice

To the Editor:—The article by Korpela *et al.*¹ regarding the morphine-sparing effects of different doses of acetaminophen concerns us in several ways. Although the study design takes into account a “placebo” group, we think that this group is unnecessary and its use provokes significant ethical questions. It is unlikely that any anesthetic technique for this sort of surgery would be planned to involve no analgesia at all, as in the untreated group. Although the placebo effect is undoubtedly of some value, this group did not even have analgesia in the intraoperative and immediate postoperative periods until rescue anesthesia was given.

Two groups (groups I and II) received inadequate analgesia intraoperatively and in the immediate postoperative period. These groups also had consequent side effects from rescue intravenous morphine that was necessary. Surely, one of the aims of day-care anesthesia is to minimize side effects.

Second, the recent vogue of using ever increasing doses of acetaminophen—in this article 60 mg/kg acetaminophen is recommended—brings into question the principal of using simple and normally safe pediatric drugs in a way that could lead to difficult problems. The study design ensured that study patients received no further acetaminophen; however, acetaminophen is probably the most common drug used in the home. There are recent case reports of severe, reversible hepatic toxicity when acetaminophen is used in therapeutic amounts.² To use a single high dose of the drug simply to provide an adequate therapeutic level more quickly merely indicates the inadequacy of the

single-drug technique, and reinforces what we already know: Treating postoperative pain using perioperative analgesia, including local anesthesia, and simple analgesics (e.g., acetaminophen, nonsteroidal anti-inflammatory drugs) is far more beneficial. Encouraging ever increasing doses of this safe drug potentially could lead to serious morbidity. Surely it is better to use a “balanced” anesthetic technique.

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2. Morton NS, Arana A: Paracetamol induced fulminant hepatic failure in a child after 5 days of therapeutic doses. *Paediatr Anaesth* 1999; 9:463–5

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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.^{1,2} 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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What Anesthesiologists Should Know About What Neurologists Should Know About Declaring Brain Death

To the Editor:—Dr. Van Norman's review article concerning medical, legal, and ethical aspects of declaring brain death is at times perfunctory and unconvincing.¹ A general theme is that anesthesiologists should watch over other physicians's declarations of brain death, and one of the concrete demands is summarized as follows: "Anesthesiologists have an important responsibility in the process of assuring that some living patients are not sacrificed to benefit others."

Three cases that involve blatant misinterpretation and errors by "physicians" set the stage for a nimbly crafted report that discusses a variety of views surrounding the diagnosis of brain death. In addition to these three cases, which in their journalistic description unfortunately contain little detail about the neurologic assessment of these patients, Dr. Van Norman claims that in one study, two thirds of physicians were unable to correctly identify or apply the whole-brain criteria for the determination of brain death.² This study, which surveyed health professionals, indeed found major differences in the assessment of brain death and vegetative state among respondents

when two fictitious cases were presented. The study included intensive care nurses, medical residents, attending anesthesiologists, operating room nurses, and intensive care unit physicians; however, no neurologists and only 16 attending neurosurgeons or neurosurgeons in training were included in the survey.

It is encouraging to read that Dr. Van Norman believes that, in the determination of brain death, at least one of the attending physicians should be a neurologist or a neurosurgeon. The declaration of brain death necessitates academic precision, but who is qualified to determine brain death is a matter of discussion. One may argue that the determination should involve critical care neurologists and neurosurgeons, and perhaps one or two dedicated neurologists or neurosurgeons on call.

Although the article does not contain gross factual errors, there are some confusing recommendations when the confirmatory tests are discussed. The article lacks a comprehensive discussion of the validity of confirmatory tests. In addition, the report fails to address the inter-