Letters to the Editor

A Safe Anesthetic Method Using Caudal Block and Ketamine for the Child with Congenital Myotonic Dystrophy

To the Editor:

A recent report on perioperative complications after general anesthesia in patients with myotonic dystrophy (DM) pointed out increased risk of perioperative pulmonary complications and severe muscular disability (1). We report the anesthetic management for a 2-yr-old boy with congenital DM using caudal block under ketamine sedation. The patient was scheduled for extension of the Achilles tendon. Ketamine (15 mg) was administrated IV. Then caudal block was performed and lidocaine (1%, 10 mL) was injected into the epidural space. After a 60-min operation, the patient recovered smoothly from anesthesia and the postoperative course was uneventful. We did not use inhaled anesthetics or neuromuscular relaxants, which may cause respiratory or circulatory depression. Tobias reported the epidural anesthesia for DM (2), and a recent report showed that a successful anesthetic technique using propofol and fentanyl in young DM patients (3). In general, it is difficult to perform caudal block for children without anesthesia, and safety of propofol is still unclear for children. However, in this case, sedation with ketamine was useful for the procedure of caudal block and during surgery. The anesthesia using caudal block under sedation by ketamine would be a safe method for the anesthesia of children with this disease.

Munehiro Shiraishi, MD
Kouichiro Minami, MD
Department of Anesthesiology
University of Occupational and Environmental Health, School of Medicine
Nippon Steel Yawata Memorial Hospital
Kitakyushu, Japan

Tatsuo Kadaya, MD
Department of Anesthesia
Nippon Steel Yawata Memorial Hospital
Kitakyushu, Japan

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Another Cause of Epidural Catheter Breakage?

To the Editor:

We read with great interest the article by Nishio et al. (1) and we would like to add some experiences we had in a similar case.

A 20-yr-old healthy parturient in active labor received an epidural catheter by loss-of-resistance-technique (Perifix® 300 Mini-Set G18 × 3 ¼). In a sitting position, a Tuohy needle was placed into the L3-4 epidural space, and the catheter was advanced 3 cm and secured by a suture at skin level (11 cm). No problems were reported. For catheter removal, the suture was cut with a surgical blade away from the catheter. Manual extraction was attempted but the catheter broke at 11 cm. Resonance imaging and computed tomography were performed, but both techniques failed to identify the catheter without doubt. As the patient remained without any sequelae, the catheter fragment was not removed (2). There was no obvious damage to the catheter, and the operators denied any damage to the catheter (e.g., withdrawal through the Tuohy needle, puncturing, cutting). However, we could not definitely exclude microlesions and it is possible that the suture itself may have caused the microlesions.

Each kind of trauma deteriorates the energy-absorbing capacity of catheters considerably (3). Therefore, fixation by suturing should only be performed if there is a good indication for it.

Wolfram Schummer, MD, DEAA, EDIC
Department of Neurology
University Hospital Hamburg-Eppendorf
Hamburg, Germany
Department of Anesthesia and Intensive Care Medicine
Friedrich-Schiller University
Jena, Germany

Claudia Schummer, MD
Department of Anesthesia and Intensive Care Medicine
Friedrich-Schiller University
Jena, Germany

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Is it Ethically Correct to Study the Quincke Spinal Needle in Obstetric Patients?

To the Editor:

We read with interest the study (1) comparing the incidence of postdural puncture headache (PDPH) and the need for epidural blood patch (EBP) between several different spinal needles. We question the need to include a spinal needle known to carry a high risk of PDPH in obstetric patients. Using the authors' own citations regarding the 25-gauge Quincke spinal needle, it has been established that a PDPH rate of 1 in 13 patients would be expected. Of these patients, approximately 67% will require an EBP—1 in 20 of patients subjected to the Quincke needle. That is 10 times the EBP rate known to occur after use of a pencil-point needle. The morbidity of these symptoms and their treatment should not be underestimated in the postpartum population.

Patients were told that they had <5% chance of a PDPH, without comment on the need for blood patching. We believe that patients should have been told that if randomized to the Quincke group, that they would be expected to have a PDPH risk at least three times that of current practice, and an EBP rate 10 times that which occurs in normal obstetric practice. In the absence of this information we believe the patients could not give adequate informed consent.

Two other issues also arise from this study. First, were patients required to pay for their EBP as a result of entering the study and being randomized to the Quincke group? Second, the fact that more staff anesthesiologists performed the spinal anesthetic with the Quincke needle than with the other needles negates any conclusion that can be drawn about the Quincke needle in relation to the others studied. Were the staff anesthesiologists themselves unhappy with the study design, as may be inferred by the authors comments?

We understand that it is satisfying to try and produce the "definitive" comparison of spinal needles, and that it is also of statistical