This case, like the prior ones,\textsuperscript{1,2} indicates that seizure activity may result from rather small doses of bupivacaine given intravenously. In all cases the rates of infusion were considerably less than that which produced seizures in subhuman primate studies.\textsuperscript{3} This would lead one to believe that perhaps the toxicity of bupivacaine systemically might be somewhat greater than has been suggested. When a high blood level of local anesthetic has been achieved in the mother, fetal levels are probably also high. In the absence of fetal distress, it would seem prudent to leave the fetus \textit{in utero} so that the local anesthetic may be cleared back across the placenta and metabolized by the mother rather than delivering an intoxicated fetus who is poorly able to metabolize local anesthetic. Finally, it would have been advisable to have made a second test injection after moving the patient. Catheters may perforate vessels during movement of the patient.

**References**

1. Yamashiro H: Bupivacaine induced seizure after accidental intravenous injection, a complication of epidural anesthesia. \textit{Anesthesiology} 47:472–473, 1977


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**Sterilization of Anesthetic Apparatus**

\textit{To the Editor:} — Mr. du Moulin and Dr. Saubermann have concluded that anesthetic techniques are not a potential source of postoperative nosocomial infection.\textsuperscript{1} Their contribution certainly indicates that healthy patients under \textit{non-contagion} conditions are not affected adversely by simple postanesthesia de-contamination efforts. I would hope, however, that their findings would not be interpreted to absolve all anesthesia equipment and techniques of potential for nosocomial infections. The abandonment of normal asepsis is not licensed by their study, to which they, by their own statements, agree. They say “Sanitary measures should not be relaxed at this time.” I concur!

The authors cite a case of the resection of an empyema without contamination of the anesthesia apparatus as one proof of their thesis that decontamination may not be needed. Yet in this case they do not show evidence that the tracheobronchial tract was contaminated by the Proteus infestation of the pleura. They present as evidence of colonization patients whose throat cultures were positive (by an entirely arbitrary figure), but present no evidence that such colonization was present in the trachea, which would have been the origin of any contamination of the apparatus. As corroboration of their findings they chose from the literature publications 21, 10, 45, 35, and 15 years old, rather than more current studies, which do implicate anesthesia equipment. There is considerable literature, including recent contributions from the authors’ own university by such an outstanding authority as Carl Walter, documenting that infections can stem from anesthesia equipment and techniques.\textsuperscript{2}

I do believe these studies are important and may be very significant. But they should be repeated under epidemic conditions and more careful definition of contamination. Until this is done, I fear that troublesome techniques of asepsis cannot be abandoned.

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**References**

1. du Moulin, GC, Saubermann AJ: The anesthesia machine and circle system are not likely to be sources of bacterial contamination. \textit{Anesthesiology} 47:353–357, 1977


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