Finally, because we disagree with Gunter's arguments as detailed above, we disagree with his statement that our " . . . results only support a conclusion that the distribution of academic achievement scores in otherwise neurologically normal children with a single exposure to anesthesia in the first year of life for minor, peripheral surgery is completely consistent with that seen in the population at large." However, for numerous reasons detailed in the Discussion section of our article, we do not believe that our results established that exposure to anesthesia during infancy *was causally related* to the disproportionate number of children who had very low test scores. We made clear in the article that causation could not be determined from our study and that the findings should be considered tentative until further verification.

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Radial Artery Catherization

To the Editor:

I read with interest the report by Truong *et al.*[1] in which they describe the occurrence of a radial artery pseudoaneurysm after radial artery catheterization for monitoring. The authors state that the catheterization was accomplished on the first attempt. Because there was no apparent trauma and the wound culture grew out *Staphylococcus aureus*, this complication was apparently due to infection.

In their report, the authors do not describe the details of the placement, in particular the sterile prep and drape, and dressing used. That would have been important information to include. It is currently recommended that a sterile dressing with chlorhexidine be used. In our institution, we routinely use the Tegaderm CHG (3M Healthcare, St. Paul, MN). In addition to adhering tightly to the skin, it has a chlorhexidine-impregnated gel which contacts the insertion site. To the best of my knowledge, we have not had any infections related to arterial catheterizations with the use of such a dressing. Infection and subsequent radial artery pseudoaneurysm are rare and would suggest a possible departure from the standard sterile technique. Fortunately, the case described is a rare complication of radial artery catheterization.

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Reference

1. Truong AT, Thakar DR: Radial artery pseudoaneurysm: A rare complication with serious risk to life and limb. Anesthesiology 2013; 118:188

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In Reply:

We read with interest Dr. Neustein's comments. Our article[1] was published as "Images in Anesthesiology" and not as a Case Report. Because of the word limitation of this educational forum, many relevant details of the case were not included.

Under sterile conditions, skin preparation was done using Povidone-Iodine Prep Pad (PDI, Orangeburg, NY) and 70% isopropyl alcohol prep. Our institution also provides Prevantics Swab (PDI) containing 3.15% chlorhexidine gluconate and 70% isopropyl alcohol for skin preparation. Radial artery cannulation was performed with a 20-gauge 1¾-inch catheter (B. Braun Medical, Bethlehem, PA), and a Tegaderm Film dressing (3M Health Care, St. Paul, MN) was applied. After pneumonectomy for lung cancer, the patient was transferred to the intensive care unit for monitoring multiple comorbidities such as hypertension, coronary artery disease, and chronic obstructive pulmonary disease.

The Center for Disease Control 2011 Guidelines for prevention of catheter-related infections recommend preparing skin with a more than 0.5% chlorhexidine preparation with alcohol before peripheral arterial catheter insertion and during dressing changes. If there is a contraindication to chlorhexidine, tincture of iodine, an iodophor, or 70% alcohol can be used as alternatives. With regard to dressing, the Center for Disease Control recommends using a chlorhexidine-impregnated sponge dressing for temporary short-term catheters in cases of persistent central line-associated bloodstream infections. No recommendations are made for other types of chlorhexidine dressings due to insufficient evidence or lack of consensus regarding efficacy.

Catheter-related infections result from the convergence of many factors. These include patient-related factors, catheter-related factors, and institutional factors. To assign causation of the pseudoaneurysm to a departure from sterility alone overlooks the fact that this patient was immunocompromised with underlying comorbidities. Furthermore, infections related to arterial catheters are influenced not only by insertion techniques, but also by pressure transducer assemblies and number of entries into the monitoring system.

Although preventive measures for such a rare complication are indeed important, our main objective was to educate anesthesia providers, who may have never seen a radial artery pseudoaneurysm, to be familiar with its appearance and presentation to make an early diagnosis and timely management.

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1. Truong AT, Thakar DR: Radial artery pseudoaneurysm: A rare complication with serious risk to life and limb. Anesthesiology 2013; 118:188

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Clinical Relevance of Urodynamic Parameter Changes in Thoracic Epidural Analgesia

To the Editor:

We read with interest the investigation by Wuethrich *et al.*[1] on the effects of epidural analgesic mixture on urodynamic parameters after open renal surgery. However, we wish to raise several methodologic and interpretative concerns which may undermine the clinical validity of the authors' conclusions. First, the authors repeatedly use language when comparing the bupivacaine and bupivacaine plus fentanyl groups ("the addition of fentanyl enhances this effect" [Abstract, Conclusions], "was more pronounced in the bupivacaine/fentanyl group" [Results, first section], "is more pronounced if fentanyl is added" [Discussion, paragraph 3], "greater increase in PVRs" [Discussion, final paragraph]) that implies a significant difference among groups, when the intergroup differences to which these statements refer were not significant.

Second, change in postvoid residual (PVR) is not a clinically useful primary endpoint because the measurement of PVR is variable at different times and can reflect other factors such as rate of diuresis or psychological inhibition. Absolute PVR might be a relevant proxy for impending urinary retention or need for recatheterization, but the reference offered for an association between absolute PVR greater than 180 ml and bacteriuria is in uninstrumented, nonsurgical patients, and refers to a chronic increase in PVR, which has little relevance to the setting at hand. The authors state that a change in PVR of 230 ml is clinically relevant; however, this is highly dependent on factors such as bladder capacity and initial PVR. Furthermore, the same group has previously shown that even though a small difference in PVR may exist in patients with thoracic epidural analgesia (TEA), all patients were able to void, and none required recatheterization.[2]

The study design includes proxy outcomes for urinary retention, but by leaving urinary catheters in place until postoperative day 5 (POD5), this study does not allow for any direct estimation of the true rate of retention during TEA (which, in other studies, has been extremely low and not different in patients with TEA in whom the urinary catheter is removed earlier).[3,4] In addition, the finding of decreased compliance associated with TEA is confounded by continuous catheter drainage, which is known to decrease bladder compliance. We agree with the fact that appropriate monitoring for retention always should occur after the removal of urinary catheter, but we do not agree with the authors' conclusion that the urodynamic changes observed in this study should preclude any attempt to remove a urinary catheter until TEA is discontinued on POD5. Assuming adequate monitoring for retention, recatheterization is a more realistic adverse outcome than the "long-term debilitating morbidity, such as loss of bladder function after acute urinary retention" cited by the authors. The study was probably underpowered to examine urinary tract infection; none were observed in either group. Other studies have demonstrated a decreased rate of urinary tract infection with removal of urinary catheters on POD1 in patients receiving TEA with no difference in the incidence of retention.[3,4]

Ambiguity exists in how the authors report their primary endpoint, which relies on a measurement of postoperative PVR. Is this the PVR measured by urodynamics on POD2 or the PVR measured by noninvasive ultrasound after epidural removal on POD5? If the latter, the use of two different methods to calculate a Δ PVR fails to take into account the variability between methods. If the former, where are the results of the PVR measured on POD5? How many patients were in retention and required recatheterization after epidural removal?

Finally, the external validity of the study may be called into question by the observations that not a single patient required systemic or epidural opioids for breakthrough pain or experienced an episode of postoperative nausea and vomiting.

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