

patients, and of them only 3 received ESAs.¹¹ The study by Weltert *et al.* included only patients undergoing off-pump cardiac surgery and thus cannot be used to address safety and efficacy for on-pump surgery.¹² The study by Yoo *et al.* included only 74 patients (not much larger than our pilot trial) and seems to have not been properly blinded.¹³ Moreover, in that study transfusions were guided by the hemoglobin concentration, so it is hard to understand why the ESA group had a markedly lower intraoperative transfusion need than the control group (0.7 ± 0.7 vs. 1.2 ± 1.1 units/patient) when the two groups had very similar postinduction hemoglobin concentrations (11.6 ± 1.2 g/dl vs. 11.5 ± 1.4 g/dl) and reticulocyte counts (80 ± 24 vs. $75 \pm 27 \times 10^3/\mu\text{l}$). Thus, these studies do not provide strong support for the use of ESAs in cardiac surgery with CPB.

Finally, Dr. Faraoni *et al.* state that our results should be viewed with caution, with which we strongly agree because it was a *pilot* study. We specifically stated that “it would be inappropriate to modify clinical practice based on the results of this pilot study.” We did conclude that the intervention “reduces perioperative anemia and erythrocyte transfusions, and may reduce plasma iron levels,” and we stand by this conclusion because it is supported by the results. We also noted that “large multicenter trials adequately powered to determine if this intervention reduces postoperative acute kidney injury are warranted.” To that end, we have created a multidisciplinary research team at 20 institutions and have applied for peer-reviewed funding to conduct such a trial. Given that definitive safety and efficacy data are also lacking for alternative interventions aimed at reducing perioperative transfusions, such as but not limited to ESAs and acute normovolemic hemodilution, the only logical conclusion is that these interventions also should not be used outside of clinical trials that are properly designed to determine their overall risk-benefit profiles.

Keyvan Karkouti, M.D.,* Duminda N. Wijeyesundera, M.D., Terrence M. Yau, M.D., Stuart A. McCluskey, M.D., Christopher T. Chan, M.D., Mark A. Crowther, M.D., W. Scott Beattie, M.D. *Toronto General Hospital, University Health Network, University of Toronto, Toronto, Ontario, Canada. keyvan.karkouti@uhn.ca

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Table Your Contaminated Equipment during Induction

To the Editor:

Mecham’s and Hopf’s “A proposal to minimize work area contamination during induction” brought up some interesting points.¹ Preparing an area to isolate items contaminated during anesthesia induction is a good idea that deserves attention. However, we propose that the clean towel not be placed on the patient’s chest, as depicted. Rather, the towel should be placed “at a site easily reached,” as the authors also suggested.¹ A towel on the chest, covered with contaminated and bulky items (such as gloves, mask, laryngoscope), will need to be moved to confirm endotracheal tube position *via* auscultation of breath sounds and the epigastrium, crucial parts of the intubation process.^{2,3} The patient’s chest is not always a stable, flat surface, thus items may fall off of the towel and onto the floor. Using a Mayo stand or similar

mobile table would accomplish the authors' goal, with minimal interference in patient care. Such a practice could also be standardized, allowing for situations in which placing items on the patient's chest is not practical (*e.g.*, pediatrics).

Thomas M. Chalifoux, M.D.,* Matthew P. Feuer, M.D.

*University of Pittsburgh School of Medicine, Magee-Womens Hospital of University of Pittsburgh Medical Center, and Children's Hospital of Pittsburgh of University of Pittsburgh Medical Center, Pittsburgh, Pennsylvania. chaltx@upmc.edu

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In Reply:

We appreciate the comments of Chalifoux and Feuer regarding our recent *Images in Anesthesiology* article.¹ We agree that identifying a specific site to isolate contaminated items is the key point, and that anesthesiologists may choose different options in meeting the goal. In our experience, we have found the chest to be a convenient location that allows us to keep our patient under continuous direct vision. We have not found the towel containing the contaminated equipment to interfere with auscultation and confirmation of endotracheal tube placement, and the towel allows for easy and rapid removal of the equipment after placement is verified. Using the anesthesia machine is an alternative, but we find this requires turning away from the patient, although we have colleagues who prefer that configuration. We agree that the chest is not ideal for small pediatric patients, but in that case there is generally room on the operating table for the towel. A Mayo stand or similar mobile tray is an excellent alternative, but requires additional workspace and may not be convenient in all anesthetizing locations. The crux of our proposal is to have a convenient space clearly identified as dirty to reduce anesthesia workspace contamination after intubating a patient. Anesthesia providers should create a systematic approach that works for their unique set of circumstances.

Elise M. Mecham, M.D., Harriet W. Hopf, M.D.* *University of Utah, Salt Lake City, Utah. harriet.hopf@hsc.utah.edu

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Neurotoxicity: Rats *versus* Neonates

To the Editor:

We just finished reading, in the March 2012 issue, the excellent editorial by Dr. Davidson entitled "Neurotoxicity and the Need for Anesthesia in the Newborn."¹ We wish to make a few comments. It is quite true; neonates have no explicit memory and when receiving no anesthesia for a particular surgical procedure (*i.e.*, patent ductus arteriosus) will never remember what took place should they survive. These patients need to be immobile (muscle relaxant only) to suit the surgeon, but do they really need an analgesic to cloud their minds when the surgeon makes his incision? Is the central nervous system and brain at this moment really intact and mature to perceive pain sensation during an operation? Because neonates are very small, underweight, and not mature at this age, why make them totally unconscious or even semiconscious? The nervous system and brain are not developed to any great degree, so they won't feel anything. In the late 1960s and middle to late 1970s,²⁻⁴ neonates undergoing ligation of patent ductus arteriosus were semiconscious or totally conscious, had a muscle relaxant, had no narcotic for pain nor sedative and still survived with no neurotoxicity and no bad memories after growing up. These neonates showed no signs of distress during their procedure. One does not need a volatile anesthetic, potent narcotic, propofol, or other sedatives. If an anesthesia provider is worried about neurotoxicity of anesthetic drugs and agents, then the provider shouldn't administer the drugs. Performing research on animals such as rats and finding that certain medications and anesthetics cause neurotoxicity cannot or should not be extrapolated to humans. This research should be carried out in humans to confirm the hypothesis. It may be unpopular to say or suggest that neonates do not always need a hypnotic agent or such, but the fact remains many do not. Anesthesia providers (clinicians) must decide their technique based on factors such as patient height, weight, age, American Society of Anesthesiology class, surgical procedure, risk, and outcomes.

Again, data based on rat experiments⁵ should not be extrapolated automatically to humans. More research on humans is needed. The article³ was presented at the World Congress of Anesthesiology in 1976, Mexico City. The only question to arise was, "Did the neonate feel any pain?" The answer at the time was the same as now: "Does the neonate have a developed central nervous system and brain to perceive pain?" Is it developed? We do not know the exact answer to this very day. So the quandary still exists, and rat studies will not tell us emphatically, but if drugs and anesthetics are neurotoxic, then the clinician had better be careful in his decisions. The clinician must also be aware that administering an opioid to a very sick neonate could cause hypotension leading to a low pressure, which then leads to poor perfusion to vital organs, especially the heart and lungs, and poor perfusion to the central nervous system and brain. That is *neurotoxicity*. Also poor perfusion to the intestinal tract, which could