In Reply:

We thank Harvey and Cave for their comments and appreciate their concerns. As they note, the mechanism(s) by which lipid can reverse local anesthetic systemic toxicity has yet to be fully defined, but it is generally accepted that the predominant effect results from drawing anesthetic from the plasma phase, reducing its effective concentration at the site of action. Consequently, one of the objectives of our studies was to determine the impact of triglyceride chain length on lipid sequestration of anesthetics from human serum *in vitro*.[1] Although data from similar experiments had been published,[2] these prior experiments examined extraction from buffer, rather than serum. Surprisingly, our results sharply conflicted with these data, as we found greater extraction with Lipofundin® (B. Braun Melsungen AG, Melsungen, Germany), a mixed medium-chain triglyceride and long-chain triglyceride (LCT) formulation, when compared with Intralipid® (Fresenius Kabi, Uppsala Sweden), an emulsion containing exclusively LCTs.

Despite any uncertainty regarding mechanism, lipid resuscitation has become well established as a clinical practice, and promulgated by guidelines published by several authoritative organizations, including the Association of Anaesthetists of Great Britain and Ireland,† the American Society of Regional Anesthesia and Pain Medicine,[3] and the American Heart Association.[4] A unique aspect of the American Heart Association's guidelines is the association's explicit recommendation to use an emulsion containing exclusively LCTs. This suggestion was apparently based on the previously published *in vitro* studies,[2] which our data challenged.[1] However, while we noted the greater extraction by a mixed lipid emulsion, we cautioned, "*in vivo* studies that confirm [our findings] are obviously required before drawing any confident conclusions." Despite this limitation, our data had immediate clinical relevance – many (if not most) facilities do not carry more than one lipid formulation, and it was our strong belief that clinicians should not hesitate to use either formulation given both had shown experimental efficacy, and both had been used with apparent success clinically to treat local anesthetic systemic toxicity.

Harvey and Cave take objection to our questioning the exclusive use of a LCT emulsion, citing an *in vivo* study[5] that has been published after the American Heart Association guidelines, and after acceptance of our manuscript. Nevertheless, we agree with their assertion that greater confidence should be placed on data derived in "whole animal" as opposed to "bench-top" experiments, at least as a general principal. However, although the cited study demonstrated superiority of the LCT formulation, there was no significant difference in return of spontaneous circulation, only a higher rate of recurrent asystole with the mixed lipid emulsion. And as the authors note, this difference may reflect the shorter half-life of medium-chain triglycerides. Moreover, return of spontaneous circulation was actually faster with the mixed lipid emulsion, though this difference did not reach statistical significance.

Harvey and Cave comment that our results "are insufficient to alter current recommendations for lipid infusion" in local anesthetic systemic toxicity, referencing the American Society of Regional Anesthesia and Pain Medicine guidelines.[3] One of the authors of our paper (KD) was a coauthor of these guidelines, which were deliberately crafted to avoid stipulating a specific lipid, using the generic term "lipid emulsion" throughout.

Based on the available literature, we would agree that the scales have tipped in favor of LCT formulations, but in the absence of more definite evidence, clinicians should not hesitate to use a mixed lipid emulsion to treat local anesthetic systemic toxicity. And regardless of formulation, the recent *in vivo* study by Li *et al.*,[5] as well as clinical experience, emphasize the importance of an adequate continuous lipid infusion following successful response to bolus administration.

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What Is the Incidence of Inadvertent Dural Puncture during Epidural Anesthesia in Obstetrics?

To the Editor:

We appreciate the editorial views of Drs. Flood and Li in their article, "A Terrible Headache in Obstetric Anesthesia,"

† http://www.aagbi.org/sites/default/files/la_toxicity_2010_0.pdf. Accessed May 14, 2012.

This letter was sent to the author of the referenced Editorial View, who felt that a reply was not necessary.—James C. Eisenach, M.D., Editor-in-Chief.

as they emphasized the need for careful evaluation of any headache after epidural anesthesia to rule out rare but possible subarachnoid hemorrhage.[1] The authors make the statement that "the incidence of postdural puncture headache is approximately 1%, regardless of whether a spinal or epidural technique was performed." We believe the current incidence of inadvertent dural puncture after epidural anesthesia in labor and delivery is much less than the 1% they quote.[2] Katircioglu *et al.* reported an incidence of 0.13% in their large obstetric anesthesia practice.[3] Gleeson reported an incidence of 0.19% from multiple centers that performed more than 1,000 epidurals annually.[4] We also found and still maintain an incidence of approximately 0.16% in our own large obstetric anesthesia practice.[5] We therefore do not agree that the 1% incidence of inadvertent dural puncture quoted in this editorial[1,2] accurately represents the results of current large obstetrical anesthesia practices. Instead, we believe an occurrence rate of less than 0.2% would be more representative of today's anesthesia practice.

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