We don't dispute the potential detrimental effect of low blood pressure, but if the lowest blood pressure was indeed the baseline value, *i.e.*, before anesthesia, then the conclusion needs to focus not just on intraprocedural blood pressure but on the contributions of hypotension from the onset of the stroke, emergency medical services care, and management in the emergency department. These may be longer periods of hypotension than the actual procedure.

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In Reply:

We thank Drs. Williams, Gelb, and Talke for their interest and comments on our paper.[1]

Dr. Williams has highlighted a potential contributor to secondary brain injury that we were unable to control for in our retrospective study. Both hypocapnia and hyercapnia have plausible mechanisms for worsening blood flow to the critically ischemic penumbra. Hypocapnia may result in further cerebral vasoconstriction and, as Dr. Williams has pointed out, may be associated with poor outcome after head trauma. We are aware that studies have shown that hypercapnia may have a neuroprotective effect after ischemia in immature animals, but we are not aware of any clinical evidence in humans to support this finding. The proposed mechanism is that of improved collateral flow due to vasodilation; however, a consequence of vasodilation may ultimately be brain edema and increases to intracranial pressure. Normocapnia is probably a safe goal at this time. Unfortunately, we did not have periprocedural blood-gas tensions available to us but we acknowledge the importance of this information.

We agree with Drs. Gelb and Talke that blood pressure management throughout the precanalization period is likely to be a critical issue. Our interventional team is currently trying to develop institutional guidelines for management of blood pressure in this setting, because current national guidelines are not particularly helpful for this group of patients.

We must apologize for the title of table 1; the use of 'Baseline' is misleading—it does not apply to the blood pressure measurements. In this article we did not report any 'baseline' (preintervention) blood pressure values. The values in table 1 were those obtained during the procedure—the same values that were reported in the 'Results' section (page 400). We did not attempt to define a 'baseline blood pressure value,' for the reasons that are outlined in the discussion (page 403, top).

This confusion generated by the misleading title does not detract from the justified concern of Drs. Gelb and Talke that blood pressure management may be important in all phases of acute stroke treatment.

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In Reply:

We would like to thank Dr. Williams for responding to our editorial,[1] which appeared in the February 2012 issue of ANESTHESIOLOGY.

Dr. Williams makes the point that patients sedated for endovascular treatment of acute ischemia may have higher arterial carbon dioxide partial pressure levels than patients treated for the same problem but receiving a general anesthetic. There are two points to this argument. The first is that patients receiving general anesthesia have a lower arterial carbon dioxide partial pressure than patients sedated without a general anesthetic. The second is that vasodilation from retention of carbon dioxide in the sedated patients will dilate the cerebral vasculature and protect penumbral areas by that mechanism.

First, with a general anesthetic the partial pressure of carbon dioxide can be regulated to whatever level is required. It is incorrect to assume that the patient will be hyperventilated and thereby have a lower carbon dioxide partial pressure than will be achieved without intubation, although that may be the case if the anesthesiologist hyperventilates the patient. Dr. Williams is also correct that sedation may cause the patient to hypoventilate and retain carbon dioxide.

Second, it is assumed that ischemic cerebral regions dilate anyway. Cerebral blood flow is probably pressure dependent in the penumbra. The issue of where to keep the partial pressure of carbon dioxide has been discussed extensively in management of patients receiving carotid endarterectomy under general anesthesia.[2–4] What was found was that it was difficult to predict the effect of dilating or constricting the surrounding healthy tissue on the ischemic cerebral areas. If you increase the partial pressure of carbon dioxide, dilate the noninvolved cerebral areas, you may shunt blood to normal brain tissues

away from ischemic areas, referred to as "countersteal."[2–4] On the other hand, if you decrease the partial pressure of carbon dioxide, constricting the noninvolved cerebral areas, you may not increase blood flow to the ischemic areas because they are limited already by the thrombus, which occludes the lumen, and you may cause the noninvolved areas to become relatively ischemic.[3,5]

We still think that the overwhelming evidence from stroke management and from this paper is for maintenance of systolic blood pressures more than 140 mmHg and less than 200 mmHg as the best strategy to provide cerebral perfusion to ischemic brain through whatever collaterals may be available. Davis *et al.* did not provide any data related to end-tidal partial pressure of carbon dioxide.[6]

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Lipid Emulsion Recommendations

To the Editor:

We read with interest the work of Ruan *et al.*[1] in the February 2012 issue of ANESTHESIOLOGY; the article explored the effect of both triglyceride chain length and pH modulation on lipid sequestration of cardiotoxic local anesthetics in human serum *in vitro*. The authors are to be lauded for their efforts to expound the physicochemical interaction known widely as the "sink," purported to be primarily responsible for the beneficial effects demonstrated in animal models and human subjects suffering local anesthetic systemic toxicity. Their results in many respects epitomize the difficulties associated with forwarding a therapy such as lipid rescue – itself a product of a chance laboratory observation, when definitive knowledge of mechanistic action for lipid remains to be fully elucidated.

Ruan *et al.* demonstrated the superiority of mixed medium and long-chain triglyceride preparations in sequestration of lipophilioc local anesthetics, seemingly independent of pH, when compared with long-chain triglyceride in *in vitro* human serum. These results, nevertheless, conflict with the findings of Li *et al.*[2] (from the December 2011 issue of ANESTHESIOLOGY), who demonstrated advantages with long-chain triglycerides in an intact animal model. The observed disparity in outcomes between such bench-top and whole animal experiments was discussed expertly in an accompanying editorial.[3]

Although the advancement of any therapy is necessarily paved with conjecture and discourse, such as evidenced in microcosm with these two papers, conclusions drawn from such work must be tempered against the findings of prior investigators' and the associated relevant (and, in the case of lipid therapy, substantial) bodies of work. It is therefore concerning that in their concluding remarks Ruan *et al.* "call into question the current advanced cardiac life support guidelines specifying use of a long-chain triglyceride emulsion" in local anesthetic systemic toxicity on the basis of their findings alone, before the existence of a body of work supporting alternative lipid emulsion preparations as clearly superior. The work of Ruan *et al.* clearly represents one step of many in the evolution of lipid emulsion therapy. Their results, however, are insufficient to alter current recommendations[4] for lipid infusion in local anesthetic systemic toxicity.

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