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In Reply:—Lam and Mayberg express concern that our results[1] contradict some prior studies and offer an alternative mechanism by which our results may have been observed.

First, the studies cited by Lam and Mayberg are not reasonable benchmarks because both studies measured actual tissue blood flow with ^{133}Xe, which we believe cannot be used interchangeably with conduit vessel velocity. A more comparable paper would be that of Werner *et al.*[2] However, their use of a different species, significant other anesthetics including isoflurane and nitrous oxide, and the occurrence of large changes in blood pressure probably renders a comparison here meaningless as well.

Second, an intraarterial sampling catheter would have been useful for *post hoc* validation of ventilatory stability but would not have been useful for real-time control of normocapnia, and for the ethical and consent reasons outlined in our discussion, we chose to use the noninvasive technique, once validated. We know of no evidence that clinically inapparent chest wall rigidity can significantly alter the A-a gradient of carbon dioxide or that these opioids can cause pulmonary hypotension to a degree that can alter ventilatory dead space in the absence of significant changes in systemic blood pressure.

Finally, Lam and Mayberg's final comment is also on shaky ground, statistically speaking. Our data demonstrated variances that were somewhat larger than the differences between the means of end-tidal *versus* arterial carbon dioxide. The presence of variance about a mean implies that some subjects had a larger value than the mean, and an equal number had a lower number. This observation provides no evidence for a bias or offset of the mean of 7–8 mmHg as posited by Lam and Mayberg.

It is worth reemphasizing that transcranial Doppler ultrasonography provides useful information in its own right that does not always mirror blood flow. We believe that the transcranial Doppler technique, by providing information on velocity, will blossom into a useful noninvasive tool for pharmacologic research.

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Prior Vasectomy and Anaphylaxis Following Protamine: No Cause and Effect

To the Editor:—By choice of title and concluding sentence, the authors of a recent paper[1] imply that their patient suffered two anaphylactic reactions to protamine, that IgG antibodies to protamine were responsible for these reactions, and that IgG antibodies to protamine were a response to prior vasectomy. The case report, as presented, does not support these implications.

The first contention is that both reactions to protamine were anaphylactic. However, the first reaction, transient pulmonary hypertension and systemic hypotension responding "promptly" to boluses of inotropic agents and vasopressors, is more consistent with the pulmonary vasoconstriction syndrome described by others.[2,3] Heparin-protamine complexes, not immunoglobulins, are hypothesized to be responsible for this idiosyncratic reaction to protamine.[4]

The later reaction to protamine, systemic hypotension, and bradycardia responding "promptly" to fluids, antihistamines, and steroids, again suggests a nonanaphylactic origin. The transient nature of the reaction and return of stable hemodynamics without intense resuscitation is typical of histamine release, which occurs commonly with protamine administration.[5]

Intraoperative anaphylaxis usually presents as peripheral and pulmonary edema, bronchospasm with hypoxemia, and systemic hypotension without pulmonary hypertension, all potentially contributing to cardiovascular collapse.[6] Therapy usually requires high infusion rates of epinephrine, many liters of fluid, and prolonged mechanical ventilation.[7] This patient's reactions to protamine differed in quality and severity from the classic anaphylactic reaction.