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# Cerebral and Renal Blood Flow Autoregulation

*To the Editor:*

I was delighted to see our work cited in the recent editorial, "Intraoperative hypotension and patient outcome: Does one size fit all?".[1,2] Unfortunately, the authors made unsupported claims about the differences between renal and cerebral blood flow autoregulation. Indeed, the editorial's conclusion that cerebral blood flow is determined by cerebral perfusion pressure and independent of changes in cardiac output is confirmed by our laboratory studies in nonhuman primates.[3–5] In our laboratory model, cardiopulmonary bypass flow was varied by adjusting pump output, whereas arterial blood pressure was independently manipulated by administration of intrathecal lidocaine. Furthermore, this finding that cerebral perfusion is dependent on mean arterial pressure and not dependent on cardiac output is strongly supported by the clinical data in cardiac surgery patients.[6]

In stark contrast, the editorial claim that, "the kidney can be hypoperfused at normal mean arterial pressure if cardiac output is compromised," is not supported by the cited reference.[7] In that study's piglet model, renal blood flow decreased during hemorrhagic hypotension. Although cardiac output was not measured at all, one would presume that the hemorrhage resulted in both decreased cardiac output and decreased arterial blood pressure. That decrease in renal blood flow may have been a consequence of hypotension, low cardiac output, or both. Whether or not changes in cardiac output, independent of arterial blood pressure, alter renal blood flow remains largely an open question.

## Competing Interests

The author declares no competing interests.

**Arthur E. Schwartz, M.D.,** The Mount Sinai School of Medicine, New York, New York. arthur.schwartz@mountsinai.org


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*In Reply:*

We thank Dr. Schwartz *et al.*[1–3] for his contribution to the understanding of cerebral blood flow during cardiopulmonary bypass. We are pleased that our citation is in agreement with his own published results. We disagree, however, on the interpretation of our previous study on renal blood flow.[4] Although it is true that cardiac output was not measured in the referenced article, our position that "the kidney can be hypoperfused at normal mean arterial pressure if cardiac output is compromised" is supported by the citation in question.[5]

In that study, piglets with a baseline mean arterial pressure between 50 and 60 mmHg underwent continuous slow hemorrhage over 3 to 4 h to demise. Cerebral blood flow was trended as a percentage of baseline using continuous laser-Doppler red cell flux monitoring with probes surgically placed against the cerebral cortex through a

dural incision. Renal blood flow was similarly trended using laser-Doppler probes placed surgically against the renal capsule. Red cell flux monitoring was plotted against cerebral and renal perfusion pressure respectively as a definitive standard pressure autoregulation curve. The aim of the study was to test the accuracy of separately measured metrics of dynamic vascular reactivity derived from near-infrared spectroscopy: the hemoglobin volume index describing cerebral vascular reactivity and the renovascular reactivity index to describe vascular reactivity in the kidney. Although not the primary aim of that study, it was observed in some animals that *renal blood flow fell during hemorrhagic shock before any change in arterial blood pressure occurred*. Although, cardiac output was not directly monitored, these findings of reduced renal blood flow despite no change in blood pressure can be explained only by a reduced cardiac output. This was demonstrated by the example data shown in the figures of the article. Thus, these findings support our contention that renal blood flow is dependent on both blood flow (cardiac output or cardiopulmonary bypass) and blood pressure as we state in our editorial.

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## Competing Interests

The authors declare no competing interests.

**Ken Brady, M.D., Christopher J. Rhee, M.D., Charles W. Hogue, M.D.** The Johns Hopkins University School of Medicine, Baltimore, Maryland (C.W.H.). chogue2@jhmi.edu

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# Not All Injections Are the Same

*To the Editor:*

In metaphorical terms, a problem arises when good apples are pooled with bad apples; they all get tarred with the same brush. This principle applies when all studies pertaining to spinal injections of steroids are pooled, as if they are all equal. Subsequently, the lay press publicizes sweeping conclusions such as "injecting any liquid, even plain saline solution, works just as well."[1] Such statements bring all injections into disrepute.

Admirably, Bicket *et al.*[2] used an ingenious statistical exercise to explore the conjecture that epidural injections of other agents are not fair controls as epidural injections of steroids. However, in their exploration, they pooled data on cervical and lumbar injections, on image-guided injections and blind injections, and on interlaminar, caudal, and transforaminal injections; they even included studies that did not involve steroids. Given that these various targets and techniques differ with respect to pathology, anatomy, technical accuracy, and evidence base, such pooling might not be legitimate, and at least clouds the true picture.

Prominent among the studies analyzed is that of Ghahreman *et al.*[3] which, indeed, the authors rank as rigorous. In the statistical analysis, this study stands out as an outlier; but it is also different in other respects. It is one of the few studies included in the review that used transforaminal injections, and it is the only study that actually addressed prospectively the very question being explored by the meta-analysis. In that regard, its results happen to contradict the conclusions of the review. It showed that the efficacy of transforaminal injection of steroid is significantly greater than that of transforaminal injection of nonsteroid. The authors of the review have referred to a conclusion that detecting a difference between treatment and control groups would not be practical but have not stated that this conclusion related specifically to long-term (12 months) outcomes.

It would have been more courteous, and more informative, had the authors stratified their analysis by region and by technique. Their conclusions might still apply to classical, blind epidural injections, but they would not apply to lumbar transforaminal injections. Lumbar transforaminal injection of steroids is significantly more often effective than transforaminal injection of either local anesthetic or saline, and intramuscular injection of either steroids or saline, and by the same magnitude in all cases.

It may be that these data could be overturned by future studies, but at present, they are the only direct data on this procedure. Those data defy the sweeping generalizations of the review, which are sensationalized by the lay press, and which serve the purpose of those who wish to deny reimbursement for epidural injections.

Meta-analysis of circumstantial evidence does not constitute proof; is not a substitute for well-designed controlled trials that address the issue. It serves only to raise an intriguing proposition worthy of studies that prospectively test it. In this