prospective and retrospective studies that would allow for the control of comorbidity and variations in anesthetic management, the examination of effects according to surgical procedure, the determination of effect by LD type, and more comprehensive measures of academic achievement, cognitive/memory functions, and quality of life. This study represents an initial attempt at unraveling this complex and difficult issue. Other studies planned and currently under way will, no doubt, add to the slowly accumulating body of clinical data that we hope will help to resolve this important and difficult issue.

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To the Editor—We were disappointed that Anesthesiology chose to publish the articles by Kalkman et al.1 and Wilder et al.2 without an accompanying cautionary editorial. Kalkman et al.1 state, “children undergoing urologic surgery at age less than 24 months showed more behavioral disturbances . . . although the results were not statistically significant.” We disagree with this statement; namely, because statistical significance was not achieved, more behavioral disturbances were not observed. Furthermore, they go on to perform a sample size calculation to determine the number of patients that would be required to detect a statistically significant effect of the effect size they found. Their estimate for such a potential association between anesthesia and behavioral problems could be explained by chance alone, and using such an estimate to guide future studies is misleading. Wilder et al.2 were unable to separate out the effects of multiple anesthetics from the effects of the underlying clinical problems requiring multiple procedures. By publishing these two studies as part of a larger series including several animal models, Anesthesiology seems to send the message that two independent teams reported similar findings in humans. At a minimum, a cautionary editorial putting these studies into context was warranted. Studies such as these, reported on by the lay media, may cause an already wary public much alarm and put pediatric anesthesiologists in an impossible position. Parental concerns regarding the possible deleterious effects of anesthesia will not be assuaged by statistical explanations. Anesthesiology has an obligation beyond merely reporting interesting studies. We are sure that, like us, other readers are looking for perspective.

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In Reply.—We thank Dr. Raghunathan et al. for their letter regarding their disappointment that we did not publish a cautionary editorial regarding the reports by Wilder et al.1 and Kalkman et al.2 in the April issue of Anesthesiology. These clinical articles, which were published with laboratory work presented at the Anesthesiology/Foundation for Anesthesia Education and Research session at the 2008 Annual Meeting of the American Society of Anesthesiologists, were accompanied by an editorial by Drs. Patel and Sun,3 thought leaders in research regarding the mechanisms and clinical relevance of neurodevelopment after exposure to anesthetics. Regarding the clinical article, they concluded in their editorial, “Although two retrospective studies herein suggest that a correlation between anesthetic exposure early in life is associated with learning and behavioral abnormalities later in life, the data cannot be considered to be evidence of the existence of anesthetic neurotoxicity in humans. The absence of rigorously conducted prospective randomized trials precludes recommendations on clinical
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“Innocent Prattle” and the Quality of Scientific Discourse

To the Editor—We read with interest the editorial titled “Innocent Prattle” by Dr. Lagasse1 that accompanied our article on anesthesia mortality.2 As we described, the recent 10th revision of the International Classification of Diseases (ICD-10) codes now includes extensive data on anesthesia complications. Its adoption by the United States to classify death certificate data offers both the opportunity and the obligation for researchers to engage in thoughtful analyses of these data. Our study was the first to accept that challenge. As stated in our article,2 our objectives were to “develop a comprehensive set of anesthesia safety indicators based on the latest version of the ICD and to apply these indicators to a national data system for understanding the epidemiology of anesthesia-related mortality.” By any measure, we have achieved these objectives despite Dr. Lagasse’s critique. It is well recognized and extensively discussed in our article that administrative data, such as those from ICD-coded, multiple-cause-of-death files, may underestimate the true incidence of adverse outcomes of medical care. It has been estimated, for example, that adverse drug effects reported to the US Food and Drug Administration account for substantially less (<20%) than the true incidence.3 However, such data can and have been crucial in detecting trends, identifying safety problems, and defining strategies to improve drug safety. In addition, thoughtful analyses will allow further granularity to be either detected from the current data or built into future ICD editions. Dr. Lagasse seems to disagree with our view that the opportunity should not be lost to analyze the ICD-10-coded mortality data as presented in our article and seems to view such analyses as “innocent prattle.”

Although vigorous argument, discussion, and even disagreement are essential and useful parts of the scientific process, derogatory comments about colleagues’ work are not. It would be a pity if learned publications fall into the trap of adopting the headline style of some popular tabloid newspapers. A deeper reading of the message of Hans Christian Andersen might be that substance and reality (read: scientific data) trump posturing and belief regardless of one’s perceived status. We will look forward to the application and validation by the scientific community of the techniques described in our article to monitor anesthesia safety and improve patient outcomes in the future.

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