

of different clinical management strategies, they are suboptimal for evaluating the optimal efficacy of a specific strategy under ideal circumstances.²

It is not feasible to use a large observational dataset to define the impact of neuromuscular blockade on mask ventilation because of several issues: First, asking providers to document additional elements and attempt mask ventilation before neuromuscular blockade solely for research purposes may require institutional review board evaluation and patient consent. Second, the acuity of the induction period demands a parsimonious approach to documentation in general. Third, it would be difficult to control confounding clinical factors such as depth of anesthesia, dosage of neuromuscular blockade, experience of providers, mask ventilation technique, and timing of mask ventilation attempts.

Goodwin performed a prospective, controlled trial evaluating the impact of neuromuscular blockade in 30 patients with normal airways.³ Contrary to our clinical experience and that espoused by Dr. Myatt, they found that neuromuscular blockade did not alter the efficacy of mask ventilation, measured by tidal volume. Because the studied population was limited to patients with normal airways, everyday clinicians are left to make decisions without data. Patients exhibiting risk factors for difficult mask ventilation such as obesity, limited jaw protrusion, bearded facial hair, advanced age, oropharyngeal dis-

proportion, and a history of snoring⁴ may be a population worthy of a controlled, prospective study. Such a study would prove to be time-consuming, expensive, difficult, and impractical, given the low incidence. Until then, our observational data describing the use of neuromuscular blockade in patients with impossible mask ventilation may have to suffice.

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(Accepted for publication July 29, 2009.)

Anesthesiology 2009; 111:1165

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Anesthesia-related Mortality

To the Editor:—We have read with great interest both the editorial of Dr. Lagasse¹ and the article of Li *et al.*,² which were related to the epidemiology of anesthesia-related mortality in the United States. The study by Li *et al.*² used the data from the National Vital Statistics System and the International Classification of Diseases codes (10th revision) to assess mortality related to anesthesia in the United States between 1999 and 2005. A similar study has been performed in France in 1999 and was correctly referenced by the authors.³ The major advantage of such a methodology is the completeness of mortality data retrieved from the National Vital Statistics System. However, when trying to identify deaths related to anesthesia, and to describe the precise degree of imputation, some problems arise.

First, the coding system is not detailed enough to capture the precise mechanism(s) that led to death and to ascertain a causal relationship. To assess more precisely the pathophysiological mechanism(s) that led to death, and subsequently to clarify the relation to anesthesia in the sequence of events, an expert analysis remains necessary, which may even be better replaced by a peer discussion with the anesthesiologist in charge of the case and who reported the death.

Second, as Dr. Lagasse noticed in his Editorial, the 10th revision of International Classification of Diseases is curiously quite poor regarding anesthesia. Items are more numerous for anesthesia for pregnancy and labor than for anesthesia in general. They explore mainly the surgical time and are mostly limited to anesthetic medication side effects or overdose. What about aspiration occurring during an emergency procedure, for example? Also, what about hemorrhage and/or delayed blood transfusion? This could be one of the limitations of this method, as the authors have noticed themselves in the discussion section of the article. Maybe the use of specific keywords related to anesthesia practice, in addition of the selection of International Clas-

sification of Diseases codes as described, could enhance the sensibility of the filter. However, even adding these suggestions might not be powerful enough to capture all cases. In the experience of the Mortality Research Group of the French Society of Anesthesia and Intensive Care,³ we have experienced that in some cases death certificates did not mention any specific International Classification of Diseases code or any previously determined specific keyword. The patients' files could be included in the survey only because researchers had chosen to select also death certificates in which a surgical (or invasive) procedure was mentioned.

Third, although one could manipulate in many ways the method to select death certificates to detect all cases that have a relation to anesthesia, is it the real problem? The specificity of the filter will never reach 100 percent. We only assess the visible part of the iceberg. Maybe it would be more efficient to monitor the same indicator along time as the trend is likely a valuable marker, even if absolute data are very approximate. It could thus be very interesting to choose an indicator both strongly related to anesthesia and reproducible to assess over time the trend of anesthesia-related mortality through a national mortality database, rather than simply obtain punctual data through a great nationwide survey.

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(Accepted for publication July 29, 2009.)

The above letter was sent to the author of the referenced editorial. The author declined to reply.—James C. Eisenach, M.D., Editor-in-Chief.

Anesthesiology, V 111, No 5, Nov 2009