To the Editor:

Michael Pine and associates have recently published an article in AANA Journal concluding that, for 8 surgical procedures, the type of anesthesia provider did not affect inpatient surgical mortality. The research behind the article looks impressive, but it is badly flawed. The fundamental defect is that all of the data were derived from administrative databases that were not designed to be used for clinical research. Dr. Pine’s selection of that experimental design is surprising because he previously issued a stern warning against using such methodology:

“...it was sensible to suggest that the use of purely administrative data to draw conclusions regarding comparative clinical performance should never be used for a definitive study of comparative clinical performance.”

The lead author of the recent AANA Journal article did not heed his own warning.

The most serious error resulting from the investigators’ use of administrative data to draw conclusions regarding clinical outcomes is that they analyzed “inpatient mortality” rather than “anesthesia-related mortality.” They did this because inpatient mortality was available in the Medicare administrative databases they used for the study, whereas anesthesia-related mortality was not. A comparison between inpatient mortality reported in the Pine article and anesthesia-related mortality reported by investigators who used clinical sources for their data illustrates the severity of this methodological flaw. Lagasse found 14 anesthesia-related deaths in a study of 184,472 anesthetics, giving an anesthesia-related mortality rate of 7.6 deaths per 100,000 cases. Biboulet et al., based on a study of 101,769 anesthetics, reported an anesthesia-related mortality rate of 5.9 per 100,000 anesthetics. In marked contrast, Pine et al., using Medicare billing data, found 1,551 inpatient deaths in 404,194 cases, which amounts to a mortality rate of 384 deaths per 100,000 cases. Pine’s inpatient mortality rate was more than 50 times greater than the anesthesia-related mortality rates reported by Lagasse and Biboulet. This indicates that anesthesia-related mortality makes up a very small proportion (less than 2%) of inpatient mortality. Consequently, anesthesia care had little or nothing to do with the vast majority (more than 98%) of the 1,551 inpatient deaths. Hence, the authors’ conclusion that “there were no significant differences in risk-adjusted mortality rates by type of anesthesia provider” is not at all surprising. The flawed design of the Pine study made it incapable of showing any scientifically valid relationship between anesthesia-related mortality and type of anesthesia provider. “Administrative data alone should never be used for a definitive study of comparative clinical performance.”

REFERENCES


Ronald A. Gabel, MD
Yarmouth Port, Massachusetts

Response:

Our use of purely administrative data to assess the relation between surgical mortality and type of anesthesia provider was largely a response to the publication by several highly respected researchers of some very alarming findings based on administrative data alone. Three years ago in an important anesthesia journal, these researchers reported 2.5 excess deaths per 1,000 surgical patients when anesthesiologists did not direct the administration of anesthesia. We believed such surprising results called for corroboration and clarification. To do this, we used the same source of data as Silber et al. Since our analyses did not confirm Silber et al’s results, we reached the “not surprising” conclusion that the type of anesthesia provider does not affect risk-adjusted surgical mortality rates.

Our present study never was intended to be “a definitive study of comparative clinical performance.” We certainly agree with Dr. Gable that inpatient mortality does not translate into anesthesia-related mortality, either in our paper or in Silber et al’s. In fact, we find it quite surprising that any researcher would attribute an excess mortality rate of 0.25% to differences in anesthesia care. Dr. Gable’s letter reinforces our contention that discerning clinicians should be very skeptical of the results and conclusions reported by Silber et al.

REFERENCE


Michael Pine, MD, MBA
Kathleen D. Holt, PhD
You-Bei Lou, PhD
Chicago, Illinois

LETTERS