Anesthesiology 2009; 111:450

Administrative Databases: Are They Useful for Clinical Analyses?

To the Editor:—We are pleased that Rosero *et al.*[1] pursued a study of the epidemiology of malignant hyperthermia (MH). We agree with Rosero *et al.* that reporting to the North American Malignant Hyperthermia Registry may underestimate MH mortality. From the Malignant Hyperthermia Association of the United States Hotline and the American Society of Anesthesiologists Closed Claims Project database we are aware of deaths clearly due to MH that were not reported to the Registry. Reports to the North American Malignant Hyperthermia Registry are voluntary, but provide key details that administrative databases cannot.[2]† We encourage readers to report suspected MH episodes to the Registry, using forms that are available online at www.mhreg.org.

Rosero *et al.* define MH cases by the hospital discharge diagnosis code of malignant hyperthermia as a result of anesthetics, after excluding other conditions associated with hyperthermia. Did the authors attempt to confirm that the 2,312 MH cases not admitted from another health facility had been exposed to an anesthetic, for example by linking them to surgical or procedural International Classification of Disease, ninth revision (ICD-9) codes? The diagnosis of MH would be more certain if there was some evidence of anesthetic exposure, not just a code.

Like any database, the "output" depends on the accuracy of the data entered. The Nationwide Inpatient Sample depends on accurate coding by medical records departments, which in turn are dependent on clinical documentation. The diagnosis of MH requires no supporting evidence to be coded as such. Other studies have shown that incorrect coding and diagnostic inaccuracy can undermine calculations derived from administrative databases.[3,4]

Rosero *et al.* conclude that the incidence of MH increased from 2000 to 2005. An equally plausible explanation is reporting bias: As coders became aware of the new ICD-9 code for MH, they used it more often. This code (995.86) was approved in 1997, thanks to the efforts of the Malignant Hyperthermia Association of the United States and American Society of Anesthesiologists, and coders may have been unaware of its existence, given the rarity of MH.

Finally, we point out an error in Rosero *et al.*'s paper. The 2007 review by Rosenberg *et al.*[5] did not consider our 2008 report of an MH-associated mortality rate of 1.4% to be "controversial."[2] Their review was published almost a year before our paper, making it impossible to cite.

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(Accepted for publication April 23, 2009.)

Anesthesiology 2009; 111:450–1

In Reply:—We would like to thank Dr. Memtsoudis and Larach *et al.* for their comments regarding our study. We welcome the opportunity to address the criticisms and concerns expressed by these authors.

First, Dr. Memtsoudis speculates that our study may have overestimated the number of malignant hyperthermia (MH) episodes, because some patients with the diagnosis may have been transferred between hospitals. However, this possibility is unlikely. The National Inpatient Sample is a stratified probability sample of hospitals in the United States, and the sampling strata are based on five hospital characteristics (geographic region, urban or rural location, teaching status, bed size, and hospital control). Within each stratum, a particular hospital has a 0.2 probability of being selected in the database, which decreases the chances of an MH case being captured multiple times in the database. We feel that this low probability decreases the chances of an MH case being captured multiple times in the database and, therefore, disagree that interhospital transfers may have impacted our estimates in any substantial way. Furthermore, information about vital status of patients at discharge (whether or not they died during hospitalization) is quite accurate in the National Inpatient Sample, and is not affected by transfers between facilities. Thus, if the incidence of MH was lower than that reported in our study, then the mortality rate from MH would be even higher than we found, leading to the same conclusion that current mortality from MH in the United States is higher than that previously reported.

We attempted to address the concern raised by Dr. Memtsoudis regarding the use of weighted or unweighted data in our manuscript, where we stated that our results were based on weighted analyses of the database, taking into account the sampling design of the National Inpatient Sample and using the statistical tools available for the analysis of stratified samples. Finally, we agree with the limitations raised by Dr. Memtsoudis related to the administrative nature of the NIS database, and discussed them in our manuscript. Nevertheless, the benefits of using such databases are well recognized and should not be overlooked.

The main concern of Larach *et al.* is that our study may have included a significant number of cases erroneously coded as MH episodes. We acknowledge that case ascertainment is a major source of bias in studies using administrative databases. Accordingly, we excluded patients with diagnosis codes of other conditions associated with hyperthermia. While we may have missed some cases, we eliminated many that were most likely miscoded. However, given that information to confirm the diagnosis of MH and the type of anesthesia are not available in the database, our study has still a potential for overreporting. Although the median age of our sample was only 39 yr and more than 90% of the patients had a low comorbidity index, we

found an overall mortality rate of 11.7% associated with a diagnosis of MH, which exceeds the in-hospital mortality reported for serious conditions affecting even older and sicker populations.[1–3] This high mortality in a low-risk population supports the idea that our estimates about the incidence and fatality rate of MH are not distant from the true values in the general population. Furthermore, although the cases were selected based just on a diagnosis code, coding by medical records departments depends on information provided by clinicians. Therefore, as we stated in our discussion, our results underscore the magnitude of the clinical problem, given that patients with a diagnosis of suspected MH should be treated as MH-susceptible until proven otherwise.

Although there are limitations to our study, we disagree with the letter authors in that we believe these data support an increase in the incidence of MH. Although one of the causes of increasing incidence could be increased awareness of the MH code, we do not believe that this issue had an important impact on the trend of our observations. MH is so rare that during the five-year study period, each coding department of the more than 5,000 hospitals of the NIS universe was exposed on average to only 0.5 MH cases. Accordingly, the hypothesis that the coders became aware of the MH code seems to be baseless. Furthermore, to minimize this bias, we excluded from the study data on the first 3 yr (1997 to 1999) in which the diagnosis of MH was available in the *International Classification of Diseases, 9th Revision, Clinical Modification.*

We agree with Larach *et al.* that the reports to North American Malignant Hyperthermia Registry provide excellent information, and any cases of MH must be reported to the Registry. In addition, the efforts of American Society of Anesthesiologists and Malignant Hyperthermia Association of the United States in obtaining the approval for MH coding are well recognized and commendable. In fact, our manuscript does not suggest that readers stop reporting to the Malignant Hyperthermia Association of the United States registry. Despite their limitations, administrative databases provide valuable information, and it is our belief that information from the administrative databases and registries complements each other and neither should be excluded as we try to better understand MH. We acknowledge the error in the reference on the introduction of our paper, which should make reference to the 5% mortality rate cited by European reports, and not to the rate reported by the North American Malignant Hyperthermia Registry study. Nevertheless, the MH-associated mortality rate remains controversial.

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(Accepted for publication April 23, 2009.)

Anesthesiology 2009; 111:451–2

Detecting the Etiologies of Acute Airway Obstruction Associated with the *Laryngeal Mask Airway Supreme*™

To the Editor:—We read the recent case report by Kleine-Brueggeney *et al.* with interest.[1] This report raises several questions. Details as to the patient's head position, height and weight, depth of device insertion, cuff inflation volume, and use of any of the known maneuvers to detect device malposition are critical for problem-solving in supraglottic airway management.

The *Laryngeal Mask Airway ProSeal*™ (*LMA-P*™) and *Laryngeal Mask Airway Supreme*™ (*LMA-S*™) were compared in two recent studies.[2,3] In a series of 93 anesthetized, paralyzed, adult female patients, Eschertzhuber *et al.*[2] concluded that ease of insertion, gastric tube placement, and fiberoptic position are similar for the *LMA-P*™ and *LMA-S*™, but oropharyngeal leak pressure and intracuff pressure are slightly higher for the *LMA-P*™. A prospective, randomized crossover study comparing the *LMA-P*™ and *LMA-S*™ in 36 fasted female patients by Verghese found similar results.[3] These studies suggest that many of the previously published findings regarding the performance of the *LMA-P*™ may apply to the *LMA-S*™.

Kleine-Brueggeney *et al.* chose a size 5 *LMA-S*™ for their patient. Airway obstruction developed immediately after cuff inflation. This clinical finding suggests several possible etiologies.

A recent study by Xue *et al.* found that head flexion impaired the passage of an orogastric tube *via* the drain tube of the *LMA-P*™.[4] Patient head position was not specified by the authors.

The authors do not specify the patient's height and weight, only the body mass index of 30.2 kg/m^2. The reader must assume that the authors chose to insert a size 5 *LMA-S*™ based on the manufacturer's recommended weight-based guidelines (size 5 *LMA-S*™ for patients weighing 70–100 kg).

Goldman *et al.* recently presented a study in which correct *LMA-S*™ size was chosen by correlating the patient's Guedel oral airway size. Guedel oral airway size was judged by aligning its tip with the angle of the jaw and its proximal end with the corner of the patient's mouth. This maneuver was done next to the patient's head just before anesthetic induction. In a series of 100 patients, 77% of women required a size 3 *LMA-S*™ using an 80-mm, size 3 oral airway, while 77% percent of men required a size 4 *LMA-S*™ using a 90-mm, size 4 oral airway as a size guide. The remaining patients required the next-largest size *LMA-S*™. Appropriate size of the *LMA-S*™ was accurate using this method, regardless of the patient's body weight.

Other clinical findings that confirm appropriate *LMA-S*™ size include insertion of more than 50% of the bite block at the level of the teeth/gums.[5] The issue of acute airway obstruction may have resolved entirely if the authors had chosen to downsize to a size 4 *LMA-S*™, rather than to reinsert the size 5 *LMA-S*™.

The authors do not specify the amount of air used to inflate the cuff or its resulting pressure. Manufacturer's guidelines indicate that the cuff inflation volume should not exceed 45 ml for a size 5 *LMA-S*™. Clinically, overinflation of the cuff could lead to narrowing of the glottic inlet as a result of extrinsic compression. The combination of inappropriate size and cuff overinflation can cause the events described.

Finally, five types of *LMA-P*™ malposition have been described after insertion.[6] The incidence of *LMA-P*™ malposition is approximately

Drs. Osborn and Behringer have served on the honorarium speakers bureau for LMA North America, Inc., San Diego, California. Dr. Verghese receives an annual honorarium from The Laryngeal Mask Company Limited, Jersey, Channel Islands.