physiologic event. This would be a patient safety concern. Disentanglement of these two distinct scenarios is essential, so proper focus can be given to our utmost concern: patient safety.

It was inappropriate to include anecdotal views regarding supervision in this article, especially when they are included without comment and qualification. Specifically, we cite the statement that "several of our colleagues offered feedback that they do not think that it is necessary for the supervising anesthesiologist to be physically present for induction or emergence in straightforward cases with experienced certified registered nurse anesthetists, as long as they are immediately available." Except in the rarest of cases, hearsay should not be a part of a scientific article, and in this case it clearly does not reflect the standard of care by anesthesiologists in the United States. Such statements have a great potential to be misunderstood and misused by readers.

ASA remains supportive of care administered personally by an anesthesiologist as well as by an anesthesia care team. We particularly support keeping the practice of anesthesia aligned with the highest standard of patient safety, hence the necessity that the anesthesiologist "personally participates in the most demanding procedures in the anesthesia plan, including induction and emergence" and is "available for immediate diagnosis and treatment of emergencies."*

Anesthesiologists provide proven value to the quality and safety of perioperative care.[2] Active leadership by anesthesiologists ensures that we are present for critical portions of each case, to both avoid complications and to provide rescue from adverse events when they might occur.

"Medical direction" and "medical supervision" are terms defined in Medicare regulation.* The authors seem to erroneously interchange the terms "supervision" and "medical direction." Although on the surface they may seem the same, there are significant differences, both clinically and by federal regulation. The interchangeable use of these terms has the potential to create confusion. Some individuals and groups have already come to the erroneous conclusion that the study demonstrates that anesthesiologists are not fulfilling their medical direction responsibilities.† This conclusion is not supported by the current study.

It should be pointed out that this study employed a mathematical model to evaluate what would happen without staggered starts; however, it did not collect data on what really occurred. Every day in this country, anesthesiologists prioritize which cases to start first, when they may safely leave, and what aspects of care require their presence. Although in some systems staggered starts may not be structurally embedded in the formal operating room "schedule," they are a reality in practice as anesthesiologists focus on patient safety.

* CMS Manual System, Pub 100-04 Medicare Claims Processing Transmittal 1324. Available at: https://www.cms.gov/transmittals/downloads/R1324CP.PDF. Accessed April 20, 2012.

† Malina DP: AANA President. Nurse Anesthesia. Available at: http://www.nurse-anesthesia.org/content.php/388-Journal-Anesthesiology-C. Accessed April 20, 2012.

ASA supports the highest standard in quality of care and patient safety. One model to achieve this standard is an anesthesia care team comprised of members who work together for a common goal, having diverse roles that synergize to provide exceptional patient-centered medical care. We hope the issues brought forth in the article by Epstein and Dexter will engender vigorous discussion, and that our letter will help highlight limitations in the study methodology and make more transparent some of the opaque aspects of the regulatory environment intrinsic to the authors' investigation.

Jerry A. Cohen, M.D., Norman A. Cohen, M.D., James D. Grant, M.D., Daniel J. Cole, M.D.‡ ‡American Society of Anesthesiologists, Park Ridge, Illinois. cole.daniel@mayo.edu

References

1. Epstein RH, Dexter F: Influence of supervision ratios by anesthesiologists on first-case starts and critical portions of anesthetics. Anesthesiology 2012; 116:683–91
2. Silber JH, Kennedy SK, Even-Shoshan O, Chen W, Koziol LF, Showan AM, Longnecker DE: Anesthesiologist direction and patient outcomes. Anesthesiology 2000; 93:152–63

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In Reply:

We appreciate the interest in our work from the officers and committee chairs of the American Society of Anesthesiologists. To lessen the possibility that we have misrepresented or misinterpreted the comments of these authors, in our reply, we quote from their respective letters and identify the specific authors. We provide explanations as to why we think the results and conclusions of our original article are reliable and valid.

1. Cohen *et al.* write, "Every day in this country, anesthesiologists prioritize which cases to start first, when they may safely leave, and what aspects of care require their presence. Although in some systems staggered starts may not be structurally embedded in the formal operating room 'schedule,' they are a reality in practice as anesthesiologists focus on patient safety."

Our research was motivated by the previous report from Paoletti and Marty of France, who performed a simulation study to calculate the percentage of days in which there would be waiting for an anesthesiologist in at least one operating room (OR).[1] Their results were published in the *British Journal of Anaesthesia* in 2007. Cohen *et al.* state that the percentage should be high; Paoletti and Marty's simulation study found it was high,[1] and so did our data analysis.[2] Thus, the scientifically useful results of our research were principally the time of the day when the percentage risk of waiting was the largest (our second hypothesis) and the parameters most highly affecting those percentage waits.[2]

2. Cohen *et al.* comment that "this study was published . . . based on a methodologically-suspect mathematical model."

Paoletti and Marty's mathematical model[1] was, in retrospect, excellent, based on their results being nearly identical to those we obtained by summing events from anesthesia information management systems data.[2] Given the concordance, we can rely on the[1] mathematical model's finding that the principal factors affecting the incidence of waiting in six ORs supervised by two anesthesiologists are: use of staggered starts, mean durations of the critical periods of inductions and emergence, and the shortest durations of cases. These findings and the calculated percentages assume conservatively that any anesthesiologist not occupied in a nonpreemptive task can cross-cover, thus creating a massive team.[1,2]

3. Elsewhere, Abouleish and Stead state that "in the United States, 'medical supervision' of anesthesia care by an anesthesiologist differs from 'medical direction' of anesthesia care." Similarly, Cohen *et al.* comment that "this study . . . included terminology that was confusing and acted to obfuscate a conclusion relevant to the study hypotheses. . . . The authors seem to erroneously interchange the terms 'supervision' and 'medical direction.'"

The following is the first sentence of the Abstract from Paoletti and Marty's *British Journal of Anaesthesia* paper, with the italic emphasis being our addition[1]:

"Numerous hospitals implement a ratio of one anesthetist *supervising* nonmedically qualified anesthetist practitioners in two or more operating theaters."

The same language is used by the Brazilian authors, de Oliveira Filho *et al.*, in the title of their 2008 article in *Anesthesia and Analgesia*, reporting on how to evaluate the quality of anesthesiologists' supervisory skills[3]:

"An instrument designed for faculty *supervision* evaluation by anesthesia residents and its psychometric properties."

Aware of the possible confusion resulting from the use of differing terminologies by leading anesthesia journals and international authors, by design we did not use the phrases "medical supervision" or "medical direction" in our paper.[2] Furthermore, the word "direction" was not used in any form anywhere in our paper.[2] We conducted (and repeated before authoring our response) a PubMed search of the phrases "medical supervision" and "medical direction" in the abstracts of articles in the following journals: *Anesthesiology, Anesthesia & Analgesia, Acta Anaesthesiologica Scandinavica, Anaesthesia and Intensive Care, Anaesthesia, British Journal of Anaesthesia, Canadian Journal of Anaesthesia, European Journal of Anaesthesiology,* the *Japanese Journal of Anesthesiology,* the *Middle East Journal of Anesthesiology*, and the *Journal of Clinical Anesthesia.* The phrase "medical supervision" was never used in these articles' abstracts. "Medical direction" was used in one *Anesthesiology* article abstract, one *Anesthesia & Analgesia* article abstract, and not once in any of the other journals' abstracts. Since *Anesthesiology* is read globally, and most articles are from countries other than the United States, our use of the generic noun "supervision" seemed more appropriate to us than use of a U.S. billing term.

4. The two letters commenting on our original research article again hit on similar themes when Abouleish and Stead state that "the word 'lapses' is misleading since really what the authors found were 'overlaps' based on their self-defined critical portions. They did not demonstrate any lapses in care by the anesthesiologist or the team. They did not study what actually happened; rather they used their broad definitions to determine if potential overlaps would occur." To this, Cohen *et al.* add, "an alleged 'supervision lapse' could occur when the induction of an anesthetic is delayed for a few minutes while waiting for the medically-directing anesthesiologist . . . . At most, this delay would result in a possible inconvenience to the surgeon and a decrement in efficiency of perioperative resources."

Yes, both sets of authors are correct. Our results show that[2]:

"Administrators who want to reduce their anesthesia group's costs by encouraging them to decrease their anesthesiologist supervision ratios need to consider the effect of our findings on the timeliness of first-case starts, which is often a major institutional focus.[4] At a ratio of one anesthesiologist to three anesthesia providers, it will not be possible to start all ORs simultaneously and have sufficient anesthesiologists to supervise all critical portions of cases on most days. Either the administrators will need to accept the fact that the additional OR often will be delayed from its scheduled start time, or agree to rearrange the OR schedule so that first cases supervised simultaneously by each anesthesiologist will have staggered start times."

Our endpoint was appropriate because although staggered starts are easy to implement, neither formally adjusting start times nor recommending to anesthesiologists the night before which ORs to start first can be done accurately without statistical calculations of historical workload.[4–6] Anesthesiologists who make the decisions without those calculations make decisions that are worse than random chance.[4,6,7]

5. Cohen *et al.* also state, "in a first scenario, an alleged 'supervision lapse' could occur when the induction of an anesthetic is delayed for a few minutes while waiting for the medically-directing anesthesiologist. … In a second scenario … during a potentially deleterious physiologic event. This would be a patient safety concern. Disentanglement of these two distinct scenarios is essential."

Yes, that is why we separately analyzed these scenarios. We calculated that[2]:

"Fewer than 20% of the minutes of critical portions … were accounted for by minutes with … physiologic events ["considered as critical portions of cases"] ($P < 0.0001$, mean 14.7%, SE 0.5%). Excluding physiologic events occurring during critical portions reduced the percentage to 13.8% (SE 0.4%)."

6. According to Abouleish and Stead, "the authors chose to define [induction of general anesthesia] … as when the patient enters the OR to intubation (or the equivalent) +

3 min. Therefore, they include within their definition of the induction the following events: transportation into the OR, movement of the patient from the stretcher to the bed, placement of the IV (if not done in holding), placement of standard monitors, and waiting for the surgeon to arrive. This overly broad definition creates artificial 'conflicts,' where none in fact occur."

We observed that[2]:

"The average peak activity (total providers needed) during cases occurred at the start of the workday for most days ($P < 0.0001$)."

Thus, what matters are the behaviors, described by Abouleish and Stead, among the first cases of the day. 'Conflicts' were underestimated, because few such cases were so brief that emergence would start while induction was ongoing in other first cases[1,2]:

"The fact that we studied a tertiary hospital with many long cases rather than an outpatient surgery center with short cases is not a limitation because, from the simulation study,[1] our results would be even stronger for short cases."

At the studied hospital, the placement of the IV is done in the holding area for nonpediatric cases. The surgeon or a surgical resident is available because otherwise the patient is not brought to the OR. The patient is logged into the anesthesia information management system as entering the OR after the stretcher is brought into the OR and positioned next to the OR table. Thus, the definitions we used to analyze the data from the study hospital were appropriate for that hospital. Because conditions may differ among facilities, we evaluated whether our results were typical[1]:

"From . . . the French simulation study[1] with 24 ORs, a staffing ratio of 1:2, and one additional floater anesthesiologist (*i.e.*, effective supervision ratio of 1:1.8), the expected incidence of supervision lapses is 12%. We observed a 12% incidence with a supervision ratio of 1:1.7."

We also evaluated whether our time periods were comparable with those of another U.S. hospital with detailed observational data:[8]

"The mean number of minutes of critical portions of first-case starts was 22.2 min (95% CI 21.8 to 22.8 min). This observation matched [the] observational finding [of 22 min] reported previously from Yale-New Haven Hospital[8] ($P = 0.29$). Thus, the third hypothesis that the mean number of critical minutes for first-case starts would match the anesthesia release time measured by observers[8] was confirmed."

7. Cohen *et al.* also comment that "of paramount concern to us . . . a requirement that the anesthesiologist cannot leave the first patient for which he or she induces general anesthesia under medical direction until the patient is 'turned over to the surgical team' (mean anesthesia release time was 22.2 min in the study population) . . . the methodology . . . create[s] false and overstated 'supervision lapses.' Of note, the authors acknowledge this concern as 'the principal limitations of our study…' in the Discussion."

Please refer to our previous response to see that we did not make assumption of 22 min. The value of 22 min was used to check that Thomas Jefferson University Hospital data were comparable with that of the hospital (Yale) that had studied induction times by observers.[8] We did so because indeed the "tasks considered as critical portions of the anesthetic" were the principal limitation of our study. We used the actual time of intubation to determine when the anesthesiologist could likely have left. The 3 min we allowed after intubation accounted for supervisory tasks such as confirming proper placement of the endotracheal tube, ensuring adequate taping of the endotracheal tube, and verifying that the postinduction/intubation blood pressure was in a satisfactory range.

8. Abouleish and Stead comment that, "instead of looking at the first-case starts, the authors chose to also look at other portions of anesthesia care as well."

We could not assume *a priori* that the first cases of the day were the most important from the perspective of supervision. That needed to be tested:

"As predicted [by the French simulation study] . . . the average peak activity (total providers needed) during cases occurred at the start of the workday for most days ($P < 0.0001$) . . . . The start of the OR day is the period of time when the anesthesiologist supervision requirement is greatest. Even with lunch breaks included, this result is so robust that changes in the anesthesiologist supervision ratio can be described to administrators simply in terms of the effect on first-case starts. This finding is useful because the psychology of first-case starts is already understood (*e.g.*, how they are interpreted economically).[4] As the economics of first-case starts are also fully developed, the decision to stagger first-case starts appropriately *versus* having more anesthesiologists can be modeled for each facility."[4–6,9]

Anesthesiologists have led the development of the science of OR management. Hopefully they will also play a large role in its application at their facilities. We stand by the appropriateness of the methodology and the conclusions of our paper.[2]

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References

1. Paoletti X, Marty J: Consequences of running more operating theatres than anaesthetists to staff them: A stochastic simulation study. Br J Anaesth 2007; 98:462–9
2. Epstein RH, Dexter F: Influence of supervision ratios by anesthesiologists on first-case starts and critical portions of anesthetics. ANESTHESIOLOGY 2012; 116:683–91
3. de Oliveira Filho GR, Dal Mago AJ, Garcia JH, Goldschmidt R: An instrument designed for faculty supervision evaluation by anesthesia residents and its psychometric properties. Anesth Analg 2008; 107:1316–22
4. Dexter EU, Dexter F, Masursky D, Garver MP, Nussmeier NA: Both bias and lack of knowledge influence organizational focus on first case of the day starts. Anesth Analg 2009; 108:1257–61

5. McIntosh C, Dexter F, Epstein RH: The impact of service-specific staffing, case scheduling, turnovers, and first-case starts on anesthesia group and operating room productivity: A tutorial using data from an Australian hospital. Anesth Analg 2006; 103:1499–516
6. Dexter F, Willemsen-Dunlap A, Lee JD: Operating room managerial decision-making on the day of surgery with and without computer recommendations and status displays. Anesth Analg 2007; 105:419–29
7. Dexter F, Lee JD, Dow AJ, Lubarsky DA: A psychological basis for anesthesiologists' operating room managerial decision-making on the day of surgery. Anesth Analg 2007; 105:430–4
8. Escobar A, Davis EA, Ehrenwerth J, Watrous GA, Fisch GS, Kain ZN, Barash PG: Task analysis of the preincision surgical period: An independent observer-based study of 1558 cases. Anesth Analg 2006; 103:922–7
9. Dexter F, Epstein RH: Typical savings from each minute reduction in tardy first case of the day starts. Anesth Analg 2009; 108:1262–7

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In Reply:

Because the letters by Drs. Abouleish and Cole touch on peer review and the mission of ANESTHESIOLOGY, I will also respond. Dr. Abouleish is familiar with the rigor of peer review at this journal, because he recently served on our Associate Editorial Board. Manuscripts are reviewed by content experts, often including one Editor or Associate Editor, and each review is rated for quality by the Handling Editor. Detailed instructions for reviewers are available on our Web-based system for peer review, and reviewers are asked to specifically rate the following five factors of quality and importance in the manuscript: clinical impact, scientific impact, interest to the specialty, novelty, and definitive interpretation. As Editor-in-Chief, I am responsible for all decisions, whether I handle the manuscript personally or review the recommended decision from the Handling Editor.

The mission of this journal is, "Promoting scientific discovery and knowledge in perioperative, critical care, and pain medicine to advance patient care." Our goal is to provide the highest quality research, rated according to the criteria above, in order to better understand the foundations of our specialty and to affirm or revise practice. I believe that the correspondence concerning the original article[1] has helped clarify its contribution in this regard, and I thank all the authors of the manuscript and these letters for this discussion.

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Reference

1. Epstein RH, Dexter F: Influence of supervision ratios by anesthesiologists on first-case starts and critical portions of anesthetics. ANESTHESIOLOGY 2012; 116:683–91

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Understanding the Mechanics of Laryngospasm Is Crucial for Proper Treatment

To the Editor:

In their case scenario, "Perianesthetic management of laryngospasm in children," Orlianguet *et al.*[1] presented a 10-month-old boy who developed an inspiratory stridor after sevoflurane induction, which was initially managed by a jaw thrust and positive pressure ventilation. When the stridor recurred, manual ventilation became difficult with increased resistance to insufflation. Despite a jaw thrust, positive pressure ventilation with $F_{IO_2} = 1$, and propofol, the obstruction was not relieved and severe hypoxemia (oxygen saturation measured by pulse oximetry, or Sp_{O_2}, = 52%) ensued, requiring the administration of succinylcholine and tracheal intubation.

A basic understanding of the mechanics of laryngospasm is crucial for proper treatment.[2] In his classic article, Fink described three types of laryngospasm: expiratory stridor, inspiratory stridor, and ball-valve obstruction.[3] The stridors are controlled by the intrinsic laryngeal muscles, whereas the ball-valve closure is controlled by both the intrinsic and extrinsic laryngeal muscles. The expiratory stridor occurs as a result of active adduction of the vocal cords. The inspiratory stridor is produced passively as a result of the loss of tone of the abductor muscles. Because the velocity is greater where the passage is most narrow, airway pressure at the subglottic area becomes less than atmospheric during inspiratory efforts, and the passage of gases through the glottis generates a force that approximates the vocal cords together resulting in inspiratory stridor. Positive airway pressure can stent the airway and correct both expiratory and inspiratory stridors.[3]

In ball-valve obstruction, laryngeal closure occurs at three levels: the true vocal cords, the false cords, and the redundant supraglottic tissue.[3,4] The approximation of the vocal cords (and false cords) is swiftly followed by contraction of the extrinsic laryngeal muscles, shortening of the thyrohyoid distance resulting in complete closure, and cessation of airflow.[3,4] In this situation, applying positive pressure can worsen the obstruction,[3] as evidenced in the current case. By distending both pyriform fossae, the aryepiglottic folds are pressed more firmly against each other, which reinforces the closure.[3] In contrast, the jaw thrust (also referred to as maximum mandibular advancement)[5] can be effective in correcting ball-valve closure. The forward mandibular movement is transmitted through the geniohyoid muscles to the hyoid bone and the hyoepiglottic ligament. Consequently, the epiglottis and the redundant supraglottic tissue are pulled away from the false cords, and the laryngeal passage is reopened.[3] How-

This letter was sent to the author of the original article, who felt that a reply was not necessary.—James C. Eisenach, M.D., Editor-in-Chief.