

0.7 units (4.7 ± 0.4 vs 4.0 ± 0.4 , respectively). Furthermore, a potential weakness of this protocol was its small sample size ($n = 12$ per group). The rating scale used is, at best, a surrogate marker. The study did not find any differences in patient outcome. In fact, to the best of our knowledge, no study has identified that maintaining deep neuromuscular blockade improves surgical outcome or reduces complication rates. We are not convinced that it is reasonable to generalize from a single study of limited sample size showing a weak difference in surrogate markers in lieu of clinically important differences in patient outcomes or incidence of adverse events. Indeed, the utter lack of important differences in clinical outcomes or incidence of adverse events in any of the cited studies is more consistent with the evidence showing no benefit.

Regrettably, investigators keep asking the wrong questions. For example, we think it is rather pointless to compare clinical conditions for laparoscopy during deep neuromuscular block versus no block at all.^{8,9} Such protocols do not reflect the reality of routine anesthetic practice. To summarize our position, with the exception of the article by Martini et al.,⁴ we have not been able to identify any studies that compare operating conditions for laparoscopy performed under deep neuromuscular block versus moderate block maintained until the end of surgery. Thus, we stand by our statement that the relative benefits of a sustained deep neuromuscular block over a sustained moderate block for laparoscopy are as yet unproven. The available data suggest that there are no important clinical benefits.

In our practice, where sugammadex is not available, if the surgeon says conditions are less than satisfactory, we then take action. We administer additional relaxant, opioid, hypnotic, change the ventilatory pattern, or some combination of these. Problem solved. None of the cited studies consider the possibility that the surgeon and the anesthesiologist might actually communicate during surgery to maintain optimal surgical conditions without overdosing the patient.

Finally, de Boer et al. suggest that further studies regarding this question are required. We believe that a prerequisite for any additional research is identifying a clinical problem that needs to be addressed. We cannot countenance intentionally administering an overdose of rocuronium to research subjects undergoing laparoscopic surgery in hopes of solving a nonexistent problem.

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Laparoscopic Surgery and Muscle Relaxants: Is Deep Block Really Not Helpful?

To the Editor:

I have read the review by Kopman and Naguib¹ with great interest! However, although comprehensively written by 2 of the most distinguished authors in their field, I dare to disagree with the format in which some of the studies are represented and with the review's conclusions. I will confine my detailed criticism to the 3 most relevant headings from the original review.

NONLAPAROSCOPIC SURGERY: ARE RELAXANTS ALWAYS NECESSARY?

The authors make the point that “lessons learned from general surgery have applicability to laparoscopic surgery as well.”¹ The first reference discussed in detail is a study by Tammisto and Olkkola,² which concludes that “as anesthesia deepened, less intense block was required.” In the context of volatile-based anesthesia, this is not surprising. However, I would like to draw attention to a quote from the same article²: “We conclude that there is a linear relationship between the end-tidal concentrations of enflurane and the degree of neuromuscular block ... However, due to huge interindividual variation, certain ‘overdosing’ of neuromuscular blocking drugs is necessary to guarantee adequate muscle relaxation of abdominal muscles during all stages of upper abdominal surgery.” Depth of anesthesia therefore appears to be not

sufficiently predictive to avoid unsatisfactory operating conditions. The discussion of the study also states that tightness of abdominal muscles might have gone unnoticed because surgeons only complained when surgical conditions were grossly unacceptable (i.e., coughing). Therefore, I agree with the review's authors that lessons can be learned from this study; however, my conclusion would be that depth of anesthesia is an unreliable predictor of surgical working conditions. It may also be mentioned that in the light of ongoing intense (and admittedly controversial) research into the possible side effects of "too deep" anesthesia, mentioning deep anesthesia as an easy way to improve surgical conditions may be seen as somewhat counterintuitive.

In the review article, the authors also quote an investigation by King et al.³ as stating that 70% of patients did not require a neuromuscular-blocking agent to achieve good or excellent operating conditions. In response, I will highlight another statement by King et al.³: "Nonetheless, vecuronium significantly increased the proportion of patients in whom at least adequate (# grade 3) surgical field ratings were maintained throughout the procedure, from 72% (placebo group) to 98%." In my view, at least this significantly changes the representation of the study, because it implies the question of whether nonadequate surgical conditions can and should really be tolerated in 30% of patients.

DOES DEEP NEUROMUSCULAR BLOCK PROVIDE BETTER LAPAROSCOPIC SURGICAL CONDITIONS THAN MODERATE LEVELS OF BLOCK?

Kopman and Naguib¹ commence this section by quoting the study by Chassard et al.⁴ as finding no differences in surgical conditions in patients receiving either a neuromuscular-blocking agent to a twitch depression of 10% of control or no blocking drugs. According to a previous article by Kopman et al.⁵ as well as the authors' definition of block levels in their review, this constitutes at best a "moderate versus no block study" and may hence be misplaced in the section of the review. Another larger study by Chen et al.⁶ is quoted in the same context. Although the review acknowledges many of the shortcomings of this article (i.e., no depth of block monitored), it quotes that "Satisfactory conditions for ventilation and operation were consistently achieved with and without muscle relaxants..."¹ The readers should know that this study makes no mention of a standardized assessment of surgical working conditions. Are "satisfactory" conditions optimal or even "good"? The latter remains speculative. Two further articles^{7,8} referenced in the same context investigated paralyzed versus nonparalyzed patients. In both studies, nonparalyzed patients were allowed to breathe spontaneously throughout the procedure. However, in the trial by Williams et al.⁸ pneumoperitoneum was rated as inadequate for trocar insertion in 5 of 18 (28%) patients (versus adequate in 100% of the paralyzed patients). Although the latter is acknowledged in the review, the review speculates whether this may have been the result of the fact that the nonparalyzed patients were breathing spontaneously and conclude that this may be a suboptimal technique for laparoscopic surgery.¹ Would the lack of neuromuscular block not

provide an alternative and perhaps even more likely explanation for unsatisfactory surgical conditions?

A study by Paek et al.⁹ is quoted as "more universally applicable" in the argument against a significant benefit of deep block. In this study, patients received either no additional rocuronium (allowed to recover from the induction dose of 0.6 mg/kg) or incremental bolus doses to maintain a train-of-four (TOF) of 2 twitches. The authors state that "there were no complaints from any of the participating surgeons."⁹ Besides the fact that the latter may not qualify as a standardized assessment of operating conditions, it should be highlighted that the "paralyzed" group in this study received a mere average of an additional 24 mg of rocuronium during procedures lasting approximately 103 minutes. The nonparalyzed group also only experienced a recovery of 4/4 twitches after 72 minutes! Because of the large dose of rocuronium administered to patients in both groups on induction, this study may have ultimately compared 2 very similarly blocked groups for the larger part of the operation. Hence, the results could in fact be seen as universally applicable to demonstrate the benefits of (albeit moderate) muscle relaxation.

I completely agree with the authors that well-designed trials comparing deep with moderate block are rare if not nonexistent. However, the review quotes 3 studies,¹⁰⁻¹² which at least attempt to provide such a comparison. First mentioned in the review is a trial by Martini et al.¹⁰ comparing surgical working conditions on a well-defined 5-point scale under conditions of either moderate (TOF 1-2) or deep (post tetanic count [PTC] 1-2) neuromuscular block. The review mentions that the mean difference of 0.7 points (4 vs 4.7 points) on the 1 to 5 scale had been, although statistically significant, relatively small. However, it is worth noting that this 18% difference was rated as "important and clinically significant" by the participating surgeons.¹⁰ Although this merely constitutes subjective opinion and not patient outcome, it should probably not be completely ignored. Furthermore, I would like to bring to attention that in this study, deep block resulted in 67% excellent surgical ratings, with moderate block, and this was found in only 34%. Despite this, conditions were rated "good" ("...a wide laparoscopic working field with sporadic muscle contractions, movements, or both")¹⁰ in >80% of patients in the moderate-block group. However, although sporadic muscle contractions or movements may not prevent successful and safe surgery in many instances, it may be permitted to ask the question whether we can or should accept suboptimal surgical conditions in 2 of 3 patients when possibly dealing with higher risk procedures (i.e., robotic surgery). The same question applies when reading about the number of treatment failures (unacceptable surgical conditions; $n = 14$) reported by Dubois et al.¹¹ in patients in their shallow block group. It is of course correct that Kopman and Naguib point out that the depth of block in this group might have been outside the definition of a moderate block. However, it is still interesting that at least half of these events were recorded either before the recovery of 4 twitches ($n = 3$) or at least at relatively low TOF ratios (<40%; $n = 4$). Ultimately, this may be close to what the intention to maintain moderate block might produce in clinical reality. Although I also

agree with the review that some deep block patients were found paralyzed at more shallow levels (hence possibly better defined as moderate blocks), no treatment failure was reported at a TOF ≤ 1 . Even when accepting that deep block in this study may have better been replaced with a moderate but a continuously monitored and maintained block, one can still conclude that allowing gradual recovery from deep to moderate neuromuscular block during laparoscopic surgery may result in undesirable operating conditions.

CONCLUSIONS

In their own conclusion, the authors of this review mention a study by De Jong¹³ as a noteworthy argument for a profound block to be unnecessary to achieve excellent surgical conditions. Although De Jong indeed found excellent conditions at the level of moderate block in most of his patients, his methodology for muscle relaxation (i.e., continuous succinylcholine administration) may have been appropriate in the 1960s but may fit the description of being awkward in today's practice. Furthermore, it is left unclear whether and how the surgeons in this trial were blinded. As a matter of fact, the author never investigated whether a more profound block than the moderate block used in the study could have improved surgical conditions even more. In view of the findings by De Jong of increasing numbers of excellent ratings with increasing depth of neuromuscular block, one could at least hypothesize that this might have been the case.

Finally, the review concludes that "Current practice in the United States where sugammadex is not available and deep block is not routinely practiced for laparoscopic surgery suggests that there is no pressing need to change current clinical routines."¹ In my view, this circulus in probando is difficult to accept as the final word of this review. Current practice in the United States alone can certainly not explain why there is currently no need to change clinical practice. Only data from well-conducted trials could, but this is ultimately still missing. Therefore, I fully agree that we have not yet seen sufficiently well-designed studies to recommend an optimum level of neuromuscular block. However, in the meantime, and based on reviewing Kopman and Naguib's references in a slightly different light and as outlined earlier, I would personally adopt a slightly different conclusion. I may also refer to a recently published very balanced review about muscle relaxation in abdominal and gynecological surgery in my support.¹⁴

There is strong evidence that muscle relaxation per se improves laparoscopic surgical working conditions. Although a deep (versus moderate) block may theoretically be beneficial, well-designed trials have yet to establish a correlation with improved patient outcome. Thus, currently, there is no hard evidence to generally recommend a specifically deep neuromuscular block. However, maintenance of deep to moderate neuromuscular block (versus a one-off dose of a muscle relaxant on induction of anesthesia) throughout surgery seems to avoid undesirable surgical conditions. On first sight, this seems more feasible than it may actually be. With neostigmine-based reversal having just recently been (re-)confirmed to be

problematic,^{15,16} anesthesiologists without unhindered access to sugammadex may find even the maintenance of moderate block during laparoscopic surgery challenging. In this context, careful—ideally quantitative—neuromuscular monitoring is a crucial component of relaxant anesthesia. Because sugammadex is not yet available in the United States, I fully agree with Kopman and Naguib that at this point in time, a change of practice cannot, therefore, be recommended.

However, where neuromuscular monitoring is applied and where sugammadex is available, I would invite coworkers to experiment with deeper levels of block to gain their own experience and, by performing aforementioned missing studies, add to the scientific knowledge base of our specialty.

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Lawyers Choose Specific Experts for Many Different Reasons

To the Editor

Radvansky et al.¹ studied expert witness qualifications in one dimension, namely that of scholarly impact. I have reviewed, as an expert witness, approximately 500 medical malpractice cases over 44 years, and I think I understand the many varied reasons why lawyers pick specific expert witnesses. First, as Radvansky et al.¹ point out, a high scholarly impact in a particular field can be important and was undoubtedly a major factor in my being selected as an expert witness in many anesthesia for thoracic surgery, airway, and obstructive sleep apnea management cases. However, sometimes a single off-the-beaten-path article or a letter-to-the-editor can be the sole determinant of whether an expert is asked to review a case. For example, my single article about 4 cases of neural injury after interscalene block under general anesthesia² and 2 very short letters-to-the-editor on the hazards of externally pressurizing cell-saver reinfusion bags^{3,4} were great magnets for lawyers to send me cases to review in these areas. As Radvansky et al.¹ point out, even though an individual's entire publication record may have nothing to do with the issues of a given case, a good publication record still makes the author appear to be a desirable expert witness.

Second, the expert who is chosen to testify must agree with the lawyer's position on the case. There are many experts who are not asked to participate in a given case after review because they cannot support the side they have reviewed the case for, a circumstance I have encountered many times in my career. This aspect of expert witness activity is completely unaccounted for by the article of Radvansky et al.¹

Third, the ability to communicate with a jury is vitally important to the success of a given side, and in the minds of many lawyers, this consideration far outweighs the importance of publication record. As a possibly related factor, physical appearance may play a role in expert witness selection.

Fourth, many experts are chosen simply by good word-of-mouth from one lawyer to another, and so the selection

by the second lawyer has nothing to do with publication record.

Fifth, many lawyers pick experts based on previous jury decisions in similar cases; thus, the lawyer knows beforehand the direction and quality of an expert's testimony with regard to a specific issue. Sixth, and related to this previous point, I have no doubt experts are sometimes chosen because the lawyer knows what the expert is going to say based on previous direct experience and work with the expert.

Seventh, any "expert" can become more "expert" on any given issue after he or she gets into the case; thus, selection of experts may be based on the experts' willingness to educate themselves. Cases involving pure judgment on general issues are often like this.

Eighth, a relatively obscure (and inexpensive) expert is picked in a few recurring situations: plaintiff lawyers will use such "experts" to simply file an affidavit supporting the case just to get the case going in the litigation process, but they never intend to use that expert to actually testify. Such "experts" may then aid plaintiff attorneys in getting nuisance awards and aid defense attorneys in reducing awards in hopeless cases by threatening a lengthy and expensive fight.

Ninth, some experts are chosen from private practice to simply opine on the practice in a given community.

Finally, some defendants want faculty from their residency to be their experts; scholarly impact is usually high.

Thus, the choice of experts by lawyers is a complex and multifactorial matter, and it will likely vary from case to case and from lawyer to lawyer. Scholarly impact, although an important determinant in some cases, may be relatively unimportant in many other cases.

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In Response

We thank Benumof¹ for his interest in our analysis and for valuable commentary into some of the reasons why lawyers choose specific experts in malpractice litigation. His insights into the varied reasons experts contribute to cases complement our findings and

Optimal Surgical Conditions in Laparoscopic Surgery: Just Relax and Lower the Pressure

To the Editor

In their recent review, Kopman and Naguib¹ suggest that the benefits of deep neuromuscular block (NMB) may be nonexistent. This is a surprising conclusion, given the increasing amount of literature on this topic that does show a significant clinical benefit from deep NMB. The most important study in this respect, by Martini et al.,² demonstrated in a blinded, randomized, controlled trial that the use of deep compared with moderate NMB is associated with an improved quality of surgical conditions in retroperitoneal laparoscopies (retroperitoneal prostatectomy, and nephrectomy) as determined by an experienced surgeon on a 5-point surgical rating scale.² Moreover, the peri- and postoperative cardiorespiratory conditions of the patients that received deep NMB were not compromised.

Another study showed that the use of deep NMB compared with no NMB improved surgical conditions for laparoscopic cholecystectomy by a motionless surgical field and better visibility.³ Apart from inducing the absence of interfering muscle contractions, deep NMB at post-tetanic count values ≤ 2 may allow for increased intraabdominal volume at lower insufflation pressures.^{4,5} This is important because a low pressure pneumoperitoneum (8 mm Hg) may be more advantageous than a standard pressure (12 mm Hg) in terms of the adverse impact on the surgical peritoneal environment.⁶ Guidelines from the European Association for Endoscopic Surgery recommended the use of the lowest intraabdominal pressure possible rather than the use of a routine pressure.⁷

We contend that both surgeons and anesthesiologists agree that the level of NMB, assessed with quantitative neuromuscular monitoring, should be aimed at optimal surgical conditions with the lowest possible pressure. This would see the prospects of improved outcome, including the potential to minimize the adverse effects of high intra-peritoneal pressure on inflammation/peritoneal fibrosis, and less pain in the days after surgery.^{6,8}

However, in this interesting field of neuromuscular management in relation to cavity pressure, cavity volume, and patient outcomes, there are several challenges. When assessing surgical conditions, objective measures should be developed, which may result in eliminating the discrepancy between anesthesiologist and surgeon ratings of optimal surgical conditions. The surgical rating scale of Martini et al.² is a first step in this direction. Furthermore, it is important to find the correct balance between insufflation pressure and cavity volume. A deep NMB may result in larger volumes at the same pressure, leading to overexpansion of the patient. This pneumoperitoneum-associated

expansion of the abdominal cavity is related to visceral pain and other physiological changes, affecting the post-operative outcome negatively.⁸ Finally, in relation to the use of deep NMB laparoscopic surgical procedures, more data are required on objective end points regarding outcomes of interest, such as surgical time, incidence of complications, postoperative patient outcome, graft function, pain, or other outcomes specific to the type of patient and surgery.

The study by Martini et al.² provides unambiguous evidence of benefit in one setting to deep NMB. Indeed, while we agree that further studies are needed to confirm the benefits of deeper levels of NMB in laparoscopic surgery, a multidisciplinary approach in the development of this new paradigm in laparoscopic surgery will be beneficial to the patient.

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Deep Neuromuscular Blockade for Laparoscopy: A Different View

To the Editor

We read with much interest the manuscript by Kopman and Naguib¹ reviewing the impact of deep neuromuscular blockade (NMB) on surgical conditions during laparoscopy. It represents an important contribution to the literature. However, we believe that clarification is necessary.

In their conclusion, the authors stated that “there is little or no evidence to suggest that using deep block (as opposed to block of moderate degree) for laparoscopic surgery will improve surgical operating conditions.”¹ This statement contrasts with the conclusion of a recent systematic review by Madsen et al.² indicating that “there is a good evidence that deep NMB, compared to moderate NMB, is associated with optimal surgical conditions” during laparoscopy. This conclusion was based on the results of 3 randomized controlled trials showing beneficial effects of deep NMB during laparoscopic surgery.^{3–5} In the study by Dubois et al.,³ optimal to excellent surgical conditions occurred in 90% of patients receiving deep NMB (train of four [TOF] count ≤ 2) but only 66% of those receiving moderate NMB (TOF count > 2). Deep NMB also significantly reduced the incidence of unacceptable surgical conditions.³ Similarly, Staehr-Rye et al.⁴ reported optimal surgical space conditions in 28% of patients with deep NMB (posttetanic count 0–1) but only 4% of those receiving moderate NMB (TOF count ≥ 2). In addition, Martini et al.⁵ demonstrated a significantly higher incidence of poor surgical conditions with moderate NMB (TOF count 1–2) than that with deep NMB (posttetanic count 1–2): 18% and 1% of patients, respectively. Maintaining deep NMB at posttetanic count 1–5 is desirable for optimal surgical conditions during laparoscopic surgery.⁶

Optimal surgical conditions are the result of synergistic effects of anesthetics, analgesics, and neuromuscular blocking agents carefully titrated during general anesthesia. On the basis of the literature, deep, compared with moderate or shallow, NMB is emerging as a distinct opportunity to improve laparoscopic surgical conditions.

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In Response

We appreciate Dr. Carron's interest in our article, and we agree that “Optimal surgical conditions are the result of synergistic effects of anesthetics, analgesics, and neuromuscular blocking agents carefully titrated during general anesthesia.” However, in their letter, they quote the statement of Madsen et al.¹ that “there is good evidence that deep neuromuscular block compared to moderate neuromuscular block is associated with optimal surgical conditions.” We do not concur with this assertion.

Of the 3 references^{2–4} that Dr. Carron cites in support of Madsen et al.'s conclusion, 2 have serious flaws in their protocols.^{2,3} We discuss these deficiencies at considerable length in our review.⁵ To give 1 example, in the article by Staehr-Rye et al.,³ the authors conclude that “Deep neuromuscular blockade was associated with surgical space conditions that were marginally better than with moderate muscle relaxation during low-pressure laparoscopic cholecystectomy.” However, at a point half-way through the surgical procedure, twitch height (T1) in the moderate neuromuscular block group was 47% of control (a train-of-four count of 4 with fade); and at the 75% time point, T1 was 89% of control (a train-of-four ratio > 0.40).⁶ Thus, the authors were really comparing deep versus very shallow or minimal block for a considerable portion of the surgical procedure.

In response to de Boer et al., we think that they have misread our position. We do not deny that neuromuscular blocking agents may have a valuable role to play in achieving satisfactory operating conditions for laparoscopic surgery. Thus, we fail to see the relevance of the study by Blobner et al.,⁷ in which the authors compared surgical conditions under deep block with no block at all. To repeat, we do not believe that Madsen et al.'s article is authoritative. Simply labeling an article a “systematic review” does not guarantee that the study was conducted or reported with due rigor. A review and its conclusions can only be as good as the references it includes and the data it attempts to analyze. The article by Martini et al.⁴ was the only study that Madsen et al. cites that reasonably supports the hypothesis that deep versus moderate block may achieve superior conditions for the surgeon. The mean difference (\pm SD) in the rating scores between deep block (a post-tetanic count of 1 or 2) and moderate block were, however, very modest—only

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0.7 units (4.7 ± 0.4 vs 4.0 ± 0.4 , respectively). Furthermore, a potential weakness of this protocol was its small sample size ($n = 12$ per group). The rating scale used is, at best, a surrogate marker. The study did not find any differences in patient outcome. In fact, to the best of our knowledge, no study has identified that maintaining deep neuromuscular blockade improves surgical outcome or reduces complication rates. We are not convinced that it is reasonable to generalize from a single study of limited sample size showing a weak difference in surrogate markers in lieu of clinically important differences in patient outcomes or incidence of adverse events. Indeed, the utter lack of important differences in clinical outcomes or incidence of adverse events in any of the cited studies is more consistent with the evidence showing no benefit.

Regrettably, investigators keep asking the wrong questions. For example, we think it is rather pointless to compare clinical conditions for laparoscopy during deep neuromuscular block versus no block at all.^{8,9} Such protocols do not reflect the reality of routine anesthetic practice. To summarize our position, with the exception of the article by Martini et al.,⁴ we have not been able to identify any studies that compare operating conditions for laparoscopy performed under deep neuromuscular block versus moderate block maintained until the end of surgery. Thus, we stand by our statement that the relative benefits of a sustained deep neuromuscular block over a sustained moderate block for laparoscopy are as yet unproven. The available data suggest that there are no important clinical benefits.

In our practice, where sugammadex is not available, if the surgeon says conditions are less than satisfactory, we then take action. We administer additional relaxant, opioid, hypnotic, change the ventilatory pattern, or some combination of these. Problem solved. None of the cited studies consider the possibility that the surgeon and the anesthesiologist might actually communicate during surgery to maintain optimal surgical conditions without overdosing the patient.

Finally, de Boer et al. suggest that further studies regarding this question are required. We believe that a prerequisite for any additional research is identifying a clinical problem that needs to be addressed. We cannot countenance intentionally administering an overdose of rocuronium to research subjects undergoing laparoscopic surgery in hopes of solving a nonexistent problem.

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Laparoscopic Surgery and Muscle Relaxants: Is Deep Block Really Not Helpful?

To the Editor:

I have read the review by Kopman and Naguib¹ with great interest! However, although comprehensively written by 2 of the most distinguished authors in their field, I dare to disagree with the format in which some of the studies are represented and with the review's conclusions. I will confine my detailed criticism to the 3 most relevant headings from the original review.

NONLAPAROSCOPIC SURGERY: ARE RELAXANTS ALWAYS NECESSARY?

The authors make the point that “lessons learned from general surgery have applicability to laparoscopic surgery as well.”¹ The first reference discussed in detail is a study by Tammisto and Olkkola,² which concludes that “as anesthesia deepened, less intense block was required.” In the context of volatile-based anesthesia, this is not surprising. However, I would like to draw attention to a quote from the same article²: “We conclude that there is a linear relationship between the end-tidal concentrations of enflurane and the degree of neuromuscular block ... However, due to huge interindividual variation, certain ‘overdosing’ of neuromuscular blocking drugs is necessary to guarantee adequate muscle relaxation of abdominal muscles during all stages of upper abdominal surgery.” Depth of anesthesia therefore appears to be not