tients are probably less prone to a possible combined effect of statin and succinylcholine, simply because they have less muscle mass. If this clinical effect in fact existed, we would been observing a pandemic of perioperative renal failure caused by myoglobinuria during the past several years, since statins are prescribed frequently for the elderly population undergoing surgical procedures in the United States,[3] and succinylcholine has not been contra-indicated in the same population. In contrast, postoperative myoglobinuria leading to kidney injury is not a common clinical entity, being only reported in few case reports that attributed inappropriate patient positioning as a possible cause.[4]

Scientific writing techniques teach us that certain parts of manuscripts are particularly powerful in conveying the manuscript's message, and the title is definitely one of them. This fact has led certain peer-reviewed journals to restrict the use of conclusive titles by authors in order to limit the influence of the author's conclusion on readers' conclusions. I personally believe that conclusive titles are important and should be allowed to point out important study results based on scientific evidence, but this was not the case in Lee's editorial.

If readers are mislead by the title of Lee's editorial, it could lead to a change in practice that may increase the use of high doses of rocuronium in substitution for succinylcholine, and certainly could create a favorable clinical setting for the widespread use of sugammadex. Despite early favorable safety studies, sugammadex lacks the several decades of clinical experience of succinylcholine, which were crucial to understand the safety profile of succinylcholine. This change in practice would substantially increase the market for sugammadex because of the high prevalence of statin use among surgical patients. I understand that Lee has demonstrated in a well designed, industry-sponsored study the beneficial reversal effects of sugammadex on rocuronium-induced neuromuscular block, compared with spontaneous succinylcholine,[5] and therefore he might have a negative personal experience with succinylcholine. This was further confirmed by Lee's suggestion that succinylcholine should be removed from the anesthesia practice: "After all, many inexpensive anesthesia drugs have been removed from anesthesia practice, why not succinylcholine?" Again, another strong statement, supported by not enough evidence. It is unknown if patients are willing to pay the cost of sugammadex in cases where high doses of rocuronium are used instead of succinylcholine. It is also unknown the effects high doses of rocuronium can have on operating room utilization costs in countries were sugammadex is still not available, such as the United States. Also important to note is that succinylcholine postoperative myalgias can be reduced by a number of low-cost interventions, such as the perioperative use of another cheap drug, lidocaine.[6]

Succinylcholine has been used for several decades by anesthesiologists. Although it has well established contraindications, such as in patients with a history of malignant hyperthermia and spine cord injury, it is also a cheap and highly efficacious drug with clear indications for its use by anesthesiologists. Based on the current literature, there is no evidence that succinylcholine should be avoided in patients receiving statins. There is evidence after Turan *et al.*'s study that succinylcholine is likely safe for otherwise healthy patients taking statins.

Gildàsio S. de Oliveira, Jr., M.D., M.S.C.I., Northwestern University, Feinberg School of Medicine, Chicago, Illinois. g-jr@northwestern.edu

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We Do Not Have Evidence that Avoidance of Succinylcholine in Patients Receiving Statins Will Improve Outcomes

To the Editor:

It is always helpful when a clinical study demonstrates that a commonly used medication lacks a potentially harmful side effect. Such was the case in the recent article by Turan *et al.*, in which it was demonstrated that succinylcholine produced an inconsequentially greater release of myoglobin in patients receiving statins than in patients not receiving statins.[1] Importantly, there was no difference in plasma potassium, plasma creatine kinase, or postoperative myalgias. Equally importantly, there was no comparison group in which patients receiving statins were randomly assigned to receive either no relaxant or a relaxant other than succinylcholine.

Curiously, the editorial by Lee that discussed the study by Turan *et al.* was titled "Succinylcholine Should Be Avoided in Patients on Statin Therapy."[1,2] Where did Lee find the evidence for this conclusion within the study by Turan *et al.*? Where is the evidence that avoiding succinylcholine and using either no relaxant or an alternative relaxant would result

in reduced release of myoglobin or better outcomes for patients? Where is the evidence that future patients will benefit if we follow Lee's advice to update the advantages and disadvantages of succinylcholine in light of the study by Turan *et al.*? Should we interpret this study to say that statins should be withdrawn in patients who will require succinylcholine? I think the most prudent course is to interpret the data in the same way as the authors: "the effect of succinylcholine given to patients taking statins is likely to be small and probably of limited consequence."

John F. Butterworth IV, M.D., Virginia Commonwealth University, Richmond, Virginia. jbutterworth@mcvh-vcu.edu

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In Reply:

I welcome de Oliveira's and Butterworth's balancing views on whether succinylcholine should be avoided in patients on statin therapy. Succinylcholine has proven controversial again, as it has repeatedly for decades. With no intention to suggest a contraindication, my standpoint remains, "Why succinylcholine at all?" *versus* "Why not succinylcholine?" Since its introduction, so many relaxants of better pharmacological profile have been developed that I believe succinylcholine would be more valuable if it is used only when it is advantageous or specifically indicated.[1] This is not the case in patients on statin therapy.[2] A minor disadvantage is still a disadvantage, and therefore undesirable, considering that it can easily be replaced.

I appreciate de Oliveira's concern about using a statement instead of a question in the title of my editorial. This was specifically considered before its submission for publication. After so many have been raised, why bother just raising another question? Instead, I opted to raise a point, and justify it with a balanced review of the history, economics, and pharmacological profiles of succinylcholine, which I have followed for decades.[1] Specific indications for succinylcholine were updated.[1] Contrary to de Oliveira's perception that I might have a negative personal experience, I have always advocated for succinylcholine where it is advantageous. For example, I still suggest that if one dose of succinylcholine has worked well in a patient, it is quite handy to extend its use for as long as significant Phase II block can be avoided. Most serious problems with succinylcholine occur with the first dose, when its advantage of rapid recovery and low cost has not yet been fully exploited. Also of note is that in obstetric anesthesia, where rapid-sequence induction-intubation is often indicated, the rapid onset and offset features of succinylcholine often make it the relaxant of choice.

My statement, "many inexpensive anesthesia drugs have been removed from anesthesia practice, why not succinylcholine," as quoted by de Oliveira, should be read in its context. It was made specifically against the cost-saving argument for succinylcholine. Unless specifically indicated, a dose of succinylcholine followed shortly by a nondepolarizing relaxant is often a waste, an unnecessary risk, and expensive for a few minutes of relaxation. The procurement, stocking, dispensing, and recording of succinylcholine usage are no less expensive than other relaxants, especially considered on a per-minute basis.

According to Butterworth, "we do not have evidence that avoidance of succinylcholine in patients receiving statins will improve outcomes." I would not wait for a large-scale outcome study to note the new evidence that succinylcholine adds to statin-related muscle damage, which admittedly appears minor in a limited study. I would neither expect a large-scale outcome study to show any new advantage of succinylcholine. Butterworth further asked, "Should we interpret this study as to say that statins should be withdrawn in patients who will require succinylcholine?" Possibly, but the question is not germane if succinylcholine can readily be replaced to begin with. How many patients on statin therapy "require" succinylcholine?

Chingmuh Lee, M.D., Harbor-UCLA Medical Center, Torrance, California. chingleeucla@ucla.edu

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