

where n is the sample size and p is the level of credibility, i.e., if 95% confidence is required, then $p = 0.05$.² If $n = 8$ and $p = 0.05$ as used by Sears, the maximum risk for significant bradydysrhythmias in a very large group of similar patients is actually 31%. Since bradydysrhythmias are undesirable and the true incidence could be as high as 31% based on Sears' study, we believe their conclusion would be more correctly stated that the incidence of bradydysrhythmias with a second dose of succinylcholine after a ketamine induction is (with 95% confidence) no higher than 31%.

When n is greater than 30, the computation is considerably simplified, and is referred to as the rule of 3.³ When $p = 0.05$, "if none of n patients shows the event about which we are concerned, we can be 95% confident that the chance of this event is at most 3 in n , (i.e., $3/n$)."² Thus, if Sears *et al.* had observed zero incidence in 50 subjects, they could have predicted the maximum incidence to be no higher than 6% ($3/50$) in a similar but large group.

When negative results are reported, the author should be careful about extrapolating results to the universe of patients as a whole.

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In Reply:—We agree with Benefiel *et al.* that our statement "the administration of a second dose of succinylcholine to healthy adult patients after induction with ketamine is safe with respect to cardiac rate and rhythm" may be too strong if only statistical analysis is considered. However, in clinical situations, other factors, such as patient's physical status, the pharmacological properties of the drug, or combination of drugs, must also be considered in the clinical conclusion of a particular study. In our study,¹ although the number is small ($n = 8$), we believe that our conclusion may not be too strong. This conclusion is not only based on the statistical analysis but also on the physical status of the patients (healthy adults) and on the pharmacodynamic property of ketamine as a drug with a sympathomimetic effect.²

The practical problem is the number of patients one should study before concluding that a technique is safe or dangerous. Based on our study, we feel it is appropriate to continue to use a second dose of succinylcholine after ketamine where required and continue to observe the patient response. If any serious side effects are encountered, we will of course report them. Thus far we still have not seen any problems with a second dose of succinylcholine in patients who received ketamine.

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To the Editor:—The recent publication of various articles and letters to the editor about succinylcholine and trismus has resulted in a re-evaluation of the use of succinylcholine and a reassessment of our clinical practice when trismus occurs after the administration of succinylcholine.¹⁻⁴ The major problem in this whole issue centers around the clinical judgment issue of what constitutes trismus. Unfortunately, Dorland's *Medical Dictionary* does not clarify the issue, nor is it clarified in any of

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Succinylcholine and Trismus

the papers that have been written. The reason is that it is a clinical judgment call with a broad spectrum of possibilities. At one end of the spectrum, the masseter muscle tone may be so increased that there is a complete inability to open the mouth. At the other end of the spectrum, there may be a mild increase in muscle tone that can be easily overcome. Van Der Spek has certainly thrown an entirely different light on the issue of succinylcholine and "trismus," with the finding