through the skin and soft tissues. We believe that this maneuver is useful when method 3 fails, for example, in obese patients, and safer since deflation of the cuff is not required. Furthermore, this maneuver provides a simple safe way of tube position verification at any time during the endotracheal intubation.

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REFERENCES


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A Challenge to the Use of d-Tubocurarine Prior to Succinylcholine in Obstetrics

To the Editor:—May I present a challenge to your readers? I have noted over the years the repeated advocacy—especially in the North American literature—that an induction dose of suxamethonium be preceded by a small dose of intravenously administered d-tubocurarine. The rationale of this measure appears to be that the nondepolarizer prevents, in the obstetric patient, certain undesirable effects of suxamethonium: generalized fasciculations, with consequential increase in intra-abdominal pressure likely to enhance the prospect of passive regurgitation, and, subsequently, postoperative muscle pains.

I have reasonably well kept records of some 7,000 general anesthetics given for cesarean section in my service since 1968, and the recollection of close on 2,000 similar anesthetics personally administered before that date, and in none of these cases was the induction dose of 100 mg suxamethonium preceded by a nondepolarizer. Fasciculations, if evidenced at all, have almost always been of a very minor character, and never of an extent considered likely to pose the threat of passive regurgitation up the esophagus. Each of our patients is interviewed at least once subsequent to the day of operation, and the incidence of reported muscle pain is approximately 9%—in the great majority of these cases the reference is to mild discomfort in the shoulders or around the lower chest; in fewer than one in a thousand does the mother describe feeling “bruised all over.”

I appreciate, as Katz et al.1 showed many years ago, that North American patients, when treated in their own environment, respond differently to muscle relaxants than do British patients treated in the U. K., but I doubt that this contrast is pertinent to my current thesis. I believe that the prior administration of d-tubocurarine as described is a pharmacologic trespass possessing no merit, and invokes the hazard of unnecessary delay, plus the avoidable expense of an extra syringe and of the drug itself. Could any of your readers present a reasonable and compelling case for its continuance in obstetric anesthetic practice?

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REFERENCES


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A Patient Transfer Method: Try It, You’ll Be Glad You Did

To the Editor:—We recently have encountered a pleasingly simple and inexpensive variation of a technique for transferring patients from the operating table to a stretcher or bed. The technique follows the example of roller/conveyor devices but uses, instead, the ubiquitous green (the color doesn’t matter) garbage bag. The patient initially is rolled, using the draw sheet, 45–60 degrees away from the side to which transfer is to be...
Anesthesiology
57:550, 1982

Duplication and Fragmentation in Publications

To the Editor.—ANESTHESIOLOGY recently published an important study on the effects of epidural morphine by Bromage et al. 1 To my great surprise, I find the same study by the same authors published the same month in Anesthesia and Analgesia. 2 The “Materials and Methods” and “Results” sections are nearly identical in the two papers (they should be as is the same study). Different aspects are stressed in the two discussions. As the titles indicate, rostral spread of the epidural morphine is discussed in more detail in Anesthesiology, the non-respiratory side effects in Anesthesia and Analgesia. Lengthening of the discussion by one page in one paper should make the other paper unnecessary.

The authors even 1) refer to the Anesthesiology paper in the Anesthesia and Analgesia paper, and 2) indicate that the respiratory side effects from the study are going to be published in a third (!) paper. The latter suggests unnecessary fragmentation of information, while the first two articles are not even that. Publishing the same morphine concentrations in a table in one journal and as a figure in the other does not make them different studies.

How can the authors defend the submission of the first two papers with the signed statement (which I assume they sent to both journals) declaring that the manuscript has not been submitted for publication in whole or in part elsewhere. Even a brief look at the two abstracts would cause one to question this. Is the need for a long publication list so important that we should not have such duplication (and fragmentation to follow) of otherwise good scientific material?

Following the precedent established by these authors, I submit this letter to the editors of both journals.

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REFERENCES

In reply.—We are grateful for the opportunity to repudiate Dr. Steen’s allegations of deliberate duplication and fractionation of data. We agree with Dr. Steen that unnecessary fractionation and duplication of data is reprehensible. We also agree that our two recent papers to which he refers, in common with many others pub-