

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.

LUBOS TRINER, M.D., PH.D.  
Associate Professor  
Department of Anesthesiology

Anesthesiology  
57:549, 1982

### 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, post-operative 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

Anesthesiology  
57:549–550, 1982

### 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

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.*<sup>1</sup> 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?

J. SELWYN CRAWFORD, F.F.A.R.C.S., F.R.C.O.G.  
Consultant Anaesthetist  
The Birmingham Maternity Hospital  
Queen Elizabeth Medical Centre  
Edgbaston, Birmingham B15 276  
England

#### REFERENCES

1. Katz RC, Norman J, Seed RF, Conrad LA: Comparison of the effects of suxamethonium and tubocurarine in patients in London and New York. *Br J Anaesth* 41:1041–1047, 1969

(Accepted for publication June 14, 1982.)

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

made. The plastic bag is placed on the table beneath the patient (and the draw sheet) and the patient is returned to the supine position. Transfer to a stretcher or bed of similar height is effected by pulling on the draw sheet. The head and lower legs must be supported during transfer but the larger part of the patient's mass slides without being lifted. An assistant should hold and maintain tension on the trailing end of the draw sheet to prevent a fall in the event of separation of table and stretcher. The method is less cumbersome and less uncomfortable for the patient than roller/conveyor devices and is much preferable to a multi-person lift (especially for transfer to a wide bed). "One, two, three, heave" becomes "one, two, three, and a slow slide." The latter is safer for all concerned. The patient's weight is never "airborne;" the slide can be interrupted easily to unhook the inadvertently snagged iv or urinary

catheter; and the popping of facet joints and nuclei pulposi and the choruses of "Oooh, my back!" rarely are heard.

This technique may well be in wide use already, but the authors have not encountered it previously. We would be pleased to give credit to the originator but he/she is unknown to us. The "Man from Glad," perhaps?

JOHN C. DRUMMOND, M.D.

*Assistant Clinical Professor of Anesthesia  
University Hospital, University of  
California, San Diego*

HILDA SAGER, R.N.

*Staff Nurse, ICU, The Green Hospital of  
Scripps Clinical, La Jolla, California*

*(Accepted for publication June 14, 1982.)*

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.*<sup>1</sup> To my great surprise, I find the same study by the same authors published the same month in *Anesthesia and Analgesia*.<sup>2</sup> The "Materials and Methods" and "Results" sections are nearly identical in the two papers (they should be as it 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 as to warrant 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.

PETTER ANDREAS STEEN, M.D.

*Department of Anesthesiology  
Ullevål Hospital, Oslo, Norway*

#### REFERENCES

1. Bromage PR, Camporesi EM, Durant PAC, Nielson CH: Rostral spread of epidural morphine. ANESTHESIOLOGY 56:431-436, 1982
2. Bromage PR, Camporesi EM, Durant PAC, Nielson CH: Non-respiratory side effects of epidural morphine. Anesth Analg (Cleve) 61:490-495, 1982

*(Accepted for publication July 29, 1982.)*

Anesthesiology  
57:550-551, 1982

*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-