surement. If the measurements could, somehow, have been repeated, since a similar experiment has not been done, the aspects of consistency and reliability could be determined. Finally, patients with gastrointestinal disorders should have been studied as well. After all, it will be these patients, especially those with hypersecretory diseases, who will benefit the most from prophylactic administration of ranitidine.

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REFERENCE

1. Manchikanti L, Colliver JA, Grow JB, Demeyer RG, Hadley CH, Roush JR: Dose-response effects of intravenous ranitidine on gastric *p*H and volume in outpatients. ANESTHESIOLOGY 65: 180–185, 1986

(Accepted for publication November 6, 1986.)

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66:442, 1987

In Reply:—We would like to thank Keith P. Kittelberger for his comments. In response to his question about whether intra- or inter-observer error may have played a part in measurement of gastric contents, this was not a blind study. We believe, however, that observer bias was not a major factor in this study, as those evaluating the stomach contents, as well as laboratory technicians determining the *p*H, were not aware of premedication. To address the questions of repeating the measurements, as well as studying the patients with gastrointestinal disorders, it will require two separate studies, and we welcome other investigators to conduct such studies.

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Questions Concerning Aspiration Prophylaxis Study

To the Editor:—I am disturbed by an apparent inconsistency in the anesthetic management of patients in the study by Manchikanti *et al.*, dealing with the effectiveness of intravenous ranitidine in outpatients.[1] The purpose of their study was to establish the dose of ranitidine required to reduce gastric contents below 0.4 $ml \cdot kg^{-1}$ and to elevate gastric *p*H above 2.5. Inherent in the study design is the need to have a control group with no pharmacologic protection and groups in which pharmacologic protection may be incomplete. The authors fail to state whether an endotracheal tube was used. Yet, a gastric tube was passed which would further increase the risk of regurgitation. I find it unfortunate that pharmacologic protection is emphasized without a single comment concerning the most important aspect of aspiration prophylaxis, that of proper airway management.

There are also larger questions which could have been addressed. Should all patients be considered at risk for regurgitation and aspiration? If not, how do we identify those who satisfy the laboratory criteria of risk? Should all patients at risk receive pharmacologic protection (the authors do advocate this)? If it is not agreed that all patients at risk require pharmacologic protection, then should they all be intubated? If not, are we dealing with a laboratory definition of aspiration risk which is not accepted as a clinically significant definition of aspiration risk?

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REFERENCE

1. Manchikanti L, Colliver JA, Grow JB, Demeyer RG, Hadley CH, Roush, JR: Dose-response effects of intravenous ranitidine on gastric *p*H and volume in outpatients. ANESTHESIOLOGY 65: 180–185, 1986

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