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In Reply.—We hope we can clarify the misunderstandings expressed by Kao et al. The null hypothesis in our study was that there is no segmental sensory change (as determined by standard clinical testing to pin-prick stimulus and cold) with epidural fentanyl. We initially used saline as a control but found that not only the fentanyl, but also the saline, produced segmental sensory changes.

We used the broad term "sensory change" to describe patient response to clinical testing. Despite the lack of clear scientific definition, the terms hypalgesia, hyperalgesia, hyposensitivity, and sensory change are frequently used throughout the literature describing effects of spinal opioids. At no time did we attempt to identify any differences in the quality of the sensory changes detected. Indeed, we would be interested to know what methods Kao et al. suggest for distinguishing sensory changes. "Anesthesia" described as complete loss of sensation was neither expected nor found. The term "analgesia" also in not suitable in this context. The cold pressor response test and the psychogalvanic skin reflex are also inappropriate.

We appreciate the attempts of Kao et al. to postulate the mechanisms of action of the epidural saline. Alteration of transmembrane potentials as well as pressure effects may indeed play a part in the sensory changes. However, these in no way determine the extent of sensory change. Local anesthetic solutions also act by altering transmembrane potentials. We also point out that epidural fentanyl diluted in 10 ml saline produces the same pressure effects as saline alone.

We agree that the word "block" in figures 2 and 4 of our article is not strictly correct. "Level of sensory change" would be more accurate, but "block" is commonly used when testing for dermatomal levels after spinal and epidural anesthesia.

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Early Reports of Pulmonary Aspiration during General Anesthesia

To the Editor—In his editorial "NPO after midnight for children—a reappraisal," Coté refers to the first reported pediatric anesthetic death, in April 1848. This was the well-documented case of Hannah Green. The frightened 15-year-old girl died less than 2 min after starting to inhale chloroform, while sitting in the operating chair, when the incision was made for removal of a great toenail. There is no evidence that aspiration of gastric contents occurred. No vomiting was observed by her medical attendants and, because of the sitting position, silent regurgitation of gastric contents into the pharynx was physiologically impossible. Autopsy revealed that the stomach was distended with food, but none was found in the bronchial tree, which contained bloody froth mixed with mucus. Simpson did not suggest that death was caused by aspiration of gastric contents; he claimed that it was the result of inhaling the brandy that was given for resuscitation, although the anesthesiologist stated that the brandy was administered after the girl had collapsed.

Snow's opinion, after reviewing the sequence of events, was: "From the lips becoming suddenly blanched, there is every reason to conclude that the heart was suddenly paralyzed." He documented 40 similar cases and concluded that the cause of death in every case was cardiac
CORRESPONDENCE

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In Reply—Dr. Maltby is certainly correct that Hannah Greener did not aspirate gastric contents.1 This case is an indication for how many years anaesthesiologists have been concerned with pulmonary aspiration, whatever the etiology. As I wrote, the patient may have aspirated brandy, since that was suggested by the words used by Simpson; my reference to this case was a clarification of what has been reported as pulmonary aspiration.2,3 It is always interesting to follow-up index cases such as that of Hannah Greener. She apparently also could have been the victim of ventricular fibrillation associated with light chloroform anesthesia.

In the original report, Simpson quoted Mr. Meggison, “I gave her some brandy, a little of which she swallowed with some difficulty.” Her father testified that “she moaned after the nail was off; he (Mr. Meggison) afterwards put some brandy in her mouth, and she rattled in her throat.” Simpson went on to state, “The attempt at swallowing was . . . I have no doubt an attempt at breathing . . . but it was impossible . . . in her weak and torpid state . . . to inspire through a medium of water and brandy . . . and . . . the liquid would be partially drawn into the larynx (she rattled in her throat).” The autopsy reported “lungs in a high state of congestion . . . bronchi filled with bloody froth . . . mixed with mucus, and a reddened larynx and epiglottis.” These findings could be consistent with either aspiration of the brandy or pulmonary edema secondary to attempts at inspiration against an obstructed airway.4

The true cause of death in the Hannah Greener case still remains speculative. The bottom line is that anaesthesiologists have been appropriately concerned with airway-related events for over 100 years. However, many of our best lessons are learned by reexamining previous experience.

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Linear Regression Is a Poor Descriptor of Accuracy

To the Editor—Urbanowicz and colleagues erroneously conclude that transesophageal echocardiography (TEE) provides a reasonable estimate of ejection fraction (EF) after cardiac surgery.1 This incorrect conclusion is based primarily on the common yet mistaken impression that good association implies good accuracy.

Linear regression is a poor descriptor of accuracy.2,4,5 Its proper uses