3. Both the text and the legend clearly indicate that after the initial response, therapy was started and no further data points were included for that patient.

We agree that “the plasma concentration of fentanyl or sufentanil that blocks undesirable responses to painful stimuli in all human patients has not been determined.” What we question is whether such a concentration exists in a clinically useful dose range. Drs. Stanley and Bailey’s description of the enormous doses of fentanyl required to block response to pain in dogs seems to corroborate our opinion.

Further studies with extremely large doses or perhaps even more potent opioids will be necessary to put this issue to rest. Then the related issue of appropriateness of these doses in terms of side effects and cost will have to be addressed. For the present, we remain convinced that in the clinically relevant dose range of available potent opioids, no evidence of a dose–response relationship exists.

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(Accepted for publication November 2, 1990.)

In Reply—Stanley and Bailey miss the mark in their analysis of the findings by Philbin et al. Philbin et al. gave a loading infusion along with a constant maintenance infusion of sufentanil in the manner suggested by Wagner1 as a means of quickly reaching a stable plasma concentration—a highly desirable condition in which to relate drug concentration to effect. The expected consequence of this infusion regimen is an initially very high drug concentration that rapidly declines as the drug equilibrates between plasma and well-perfused tissues (distribution phase). Thereafter, plasma and brain concentrations decline more slowly until a steady state is reached. A delay between the initial loading infusion and measurements of drug concentration and effect is necessary to allow relatively stable conditions to be achieved. The sequence employed by Philbin et al. was appropriate to their purpose.

Observations of response/nonresponse were made for sternotomy when the plasma levels of sufentanil were relatively stable. Reliable “anesthesia” (i.e., suppression of hemodynamic responses to a potent noxious stimulus) was not evident in the range of 10–16 ng/ml concentrations of sufentanil in plasma. The proportion of responders was virtually the same in the concentration ranges of 2–5 mg/ml (7 of 13), 5–10 mg/ml (7 of 14), and 10–16 mg/ml (4 of 10). I doubt that anyone would be willing to administer the very large doses of sufentanil necessary to produce and maintain plasma concentrations greater than 10 mg/ml and still accept a 40% response rate. And if not 10–16 mg/ml, how much sufentanil is required to use it as a “pure anesthetic”? That is the primary practical point made by Philbin et al.

Importantly, de Lange et al. did not use “pure” sufentanil to achieve satisfactory anesthetic conditions in their cardiac surgical patients. Their patients were premedicated with a large oral dose of lorazepam (0.08 mg/kg).2 We have found that this large dose of lorazepam makes it relatively easy to finish the induction of anesthesia and to maintain it with a moderate concentration of opioid in plasma.3

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(Accepted for publication November 2, 1990.)

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Anesthesiology

Nystagmus Following Epidural Morphine

To the Editor—We read with great interest the report of Fish and Rosen1, in which a case of vertical nystagmus in a patient who had received epidural morphine was described. We recently also observed such a case.

A 67-year-old male, of weight 75 kg and height 168 cm, underwent bilateral total knee replacements for osteoarthritis. His past medical history was unremarkable; specifically, he had no neurologic disease and was taking only a nonsteroidal antiinflammatory medication. His anesthetic was performed with lumbar epidural anesthesia using 0.75% bupivacaine HCl. During the surgical procedure he received midazolam 10 mg. No other hypnotics, sedatives, or analgesics were administered. In the recovery room he received a loading dose of 2 mg preservative-

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Routine Testing for Latex Allergy in Patients with Spina Bifida Is Not Recommended

To the Editor:—Moneret-Vautrin et al., in their discussion of three cases of intraoperative anaphylaxis in children with spina bifida, state that “prick tests and RASTs are reliable for detecting latex allergy.” They conclude that such tests should be performed preoperatively on all patients with spina bifida.

Unfortunately, there are no studies to support this statement. We do not yet know the prevalence of clinical rubber allergy in patients with spina bifida; recent surveys suggest that it is between 18 and 26%. Turjanmaa et al. have found that the commercially available latex RAST is only 53% sensitive, and no sensitivity or specificity data are available for percutaneous latex testing. We therefore have no data whatever on the predictive value of these tests.

Until prospective studies identify the risk factors and predictors of intraoperative anaphylaxis, physicians must continue to rely on tools that are of demonstrated efficacy. We must obtain accurate histories from our patients, and carefully inquire of patients with spina bifida and their parents whether there have been any unusual, idiopathic, or perioperative allergic reactions in the past. Patients with such a history should be offered preoperative prophylaxis against immediate hypersensitivity reactions and should be spared, whenever possible, unnecessary cutaneous and parenteral exposure to natural rubber products.

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REFERENCES

In Reply.—Slater and Mostello rightly question the specificity and the sensitivity of these prick-tests because until now no data have been available. We have studied 907 physicians, surgeons, nurses, and hospital employees using both a questionnaire and prick test to a latex emulsion. In 18 cases, allergy to latex was suspected on the basis of clinical symptoms. Prick tests in all 18 were positive (sensitivity 100%). In 889 subjects with a negative clinical history, 883 prick tests were negative (specificity approximately 99%). In 3 of the 6 subjects with a