Anesthesiology
79:189, 1993

In Reply:—I appreciate the comments on our study[1] offered by Ruta and Mutch. However, I disagree with some of their statements. First, our study was unique in comparing the effects on organ blood flows of hemorrhage superimposed upon three techniques of deliberate hypotension. The study was designed to demonstrate the effects of hemorrhage, not the effects of hemorrhage combined with other drugs or resuscitation techniques. In contrast, their study, demonstrated the effects of resuscitation following hemorrhage during deliberate hypotension with isoflurane only.[2] Additionally, only regional cerebral blood flow and renal blood flow were reported in their study.

Second, I believe that their study demonstrates one reason for our study design: resuscitation may influence different vascular beds in a heterogeneous manner. For example, despite their criticism of our work that brain and kidney blood flow could be restored to prehemorrhagic values by their different resuscitation methods, examination of their paper shows that blood flow was restored to brain structures in a differential manner and that renal blood flow was not restored at all!

Third, they rightly point out that our animals hemorrhaged during isoflurane hypotension had a significantly greater *p*H value compared with our other hemorrhaged animals. From this, Ruta and Mutch would conclude that hemorrhage during hypotension with deep isoflurane is associated with the least metabolic compromise of the three techniques. We did not measure any specific metabolic parameters and so cannot comment this possibility. However, I wonder whether they would apply the same inference to their own data in which the *p*H decreased in each group following resuscitation?

Though Ruta and Mutch are correct to imply that the direct extrapolation of our data to the clinical situation should be done thoughtfully, the same can be said of most, if not all research.

Richard J. Sperry, M.D.
Department of Anesthesiology
The University of Utah
50 North Medical Drive
Salt Lake City, Utah 84132

References

1. Sperry RJ, Monk CR, Durieux ME, Longnecker DE: The influence of hemorrhage on organ perfusion during deliberate hypotension. ANESTHESIOLOGY 77:1171–1177, 1992

2. Ruta TS, Mutch WAC: Regional cerebral blood flow following hemorrhage during isoflurane anesthesia in the rabbit: Comparison of techniques to support blood pressure. ANESTHESIOLOGY 70:978–983, 1989

(Accepted for publication March 28, 1993.)

Anesthesiology
79:189–190, 1993

Spinal Anesthesia for a Former Premature Infant Undergoing Upper Abdominal Surgery

To the Editor:—Spinal anesthesia for former premature infants with significant residual pulmonary pathology has been recommended for surgery below the diaphragm, particularly lower extremity (*e.g.*, club foot repair) or lower abdominal (*e.g.*, herniorraphy or orchiopexy) operations. We report a case of upper abdominal surgery performed safely with a spinal anesthetic.

The patient is a 14-month-old boy requiring gastrostomy placement and bilateral inguinal hernia repair. His medical history is significant for a preterm delivery at 28 weeks' gestational age with a birth weight of 1,060 g. His neonatal course was complicated by severe infant respiratory distress syndrome requiring prolonged intubation and intermittent jet ventilation and air leaks requiring chest tube placement. He also had multiple bouts of sepsis, thrombocytopenia, multiple transfusions, chylothorax, jaundice, intraventricular hemorrhage, and perforated necrotizing enterocolitis.

At the time of this operation, he had no acute infectious problems, was developmentally delayed, required 0.5 L nasal cannula oxygen support, and weighed 3.48 kg. His medications included Hydrodiuril, spironolactone, phenobarbital, Cholestyramine, Kaopectate, terbutaline, prednisone, and theophylline. He had no allergies.

His physical examination revealed an undernourished, relatively macrocephalic boy. He had a normal airway, good air entry bilaterally without adventitial sounds, and a regular heart rate without murmurs. His hematocrit was 32%, and his electrolytes were normal.

He was brought to the operating room, where he was monitored with electrocardiogram, automated blood pressure cuff, pulse oximeter, precordial stethoscope, and end-tidal carbon dioxide. He was positioned in the right lateral decubitus, and a 22-G, 2-inch Quincke point spinal needle was inserted into the subarachnoid space *via* the L4–L5 interspace. Four milligrams hyperbaric tetracaine with an epinephrine wash was injected. The needle and syringe were left in place for 5 s prior to removal, to avoid leakage of medication into the needle tract.[1]

The electrocautery ground was placed while the child was on his

side. He was then log-rolled onto his back. There were no significant hemodynamic changes as a dense sensorimotor block to a T4 level developed. The gastrostomy began 5 min later and was followed by the inguinal hernia repairs. There were no episodes of inadequate anesthesia, desaturation, apnea, or hemodynamic instability. The child received normal intravenous hydration through a preexisting intravenous catheter. No sedatives were required. He was returned to the pediatric intermediate care unit. His residual block began to recede approximately 2.5 h after block, and by 4 h after block, he exhibited baseline sensorineural function.

We chose a spinal anesthetic after considering risks and benefits. In reviewing the current literature, much attention has been focused on considering peripheral lower extremity and lower abdominal surgeries as appropriate for subarachnoid blockade in pulmonary compromised infants. Specific reference to upper abdominal surgery, particularly as regards pulmonary function intraoperatively and postoperatively, is lacking. Rice *et al.* have researched the effect of subarachnoid block in high-risk infants undergoing lower abdominal surgery and suggest that, although breathing mechanics are altered, there are no significant changes in transcutaneous carbon dioxide or arterial oxygen saturation with sensorineural block to T4 levels.* Given this child's history of difficulty in separation from mechanical ventilation, we elected to attempt subarchnoid block and proceed to tracheal intubation and a combined general and regional anesthetic if required.

Our case suggests that subarachnoid block may be an acceptable anesthetic for relatively simple upper abdominal surgery, allowing many of the same benefits portended for lower abdominal and peripheral lower extremity surgery.

Brian K. Gingrich, M.D.
Director, Pediatric Anesthesiology
Nassau County Medical Center
2201 Hempstead Turnpike
East Meadow, New York 11554

Reference

1. Abajian JC, Mellish RW, Browne AF, Perkins FM, Lambert DH, Mazuzan JE Jr: Spinal anesthesia for surgery in the high risk infant. Anesth Analg 63:359–362, 1984

(Accepted for publication March 28, 1993.)

* Rice LJ, Bacon G, Newmark: Spinal anesthesia does not compromise ventilation or oxygenation in high-risk infants (abstract). ANESTHESIOLOGY 71:A1021, 1989.

Anesthesiology
79:190, 1993

Anesthesiologists and Substance Abuse

To the Editor:—While Schmidt and Schlesinger are to be commended for their attempts to bring greater accountability to the distribution of controlled substances at their hospital,[1] they are to be loudly condemned for their statement, "Substance abuse is more common among anesthesiologists than among any other medical specialty," citing Spiegelman *et al.*'s 1984 paper.[2] What this source states is that anesthesiologists are over-represented in substance abuse treatment programs, but it is a vast leap of science and fact to say this equates to higher rates of abuse among anesthesiologists than any other medical field. What it may mean is that anesthesiologists are more likely to recognize a problem in themselves and self-refer to those treatment programs.

Among the many things we as anesthesiologists must be vigilant of is the transmutation of rumor and innuendo into fact. If we are not, then we can expect those less expert than ourselves to worry needlessly about their anesthesiologist citing our own journal as the source of those fears.

George M. Woods, M.D.
1835 Vallejo Street, Apt. #304
San Francisco, California 94123

References

1. Schmidt KA, Schlesinger MD: A reliable accounting system for controlled substances in the operating room. ANESTHESIOLOGY 78:184–190, 1993
2. Spiegelman WG, Saunders L, Mazze RI: Addiction and anesthesiology. ANESTHESIOLOGY 60:335–341, 1984

(Accepted for publication March 30, 1993.)

Anesthesiology
79:190–191, 1993

In Reply:—Our reference to Spiegelman *et al.*'s[1] paper was not a "transmutation of rumor and innuendo into fact." Several anesthesiology textbooks review this and other literature and reach the same conclusion. Berry and Katz[2] state, "There are more addictive diseases among anesthesiologists than among any other medical specialty." According to Arnold,[3] "The disease is more common in some spe-