trathecal "protective" drugs, spinal cord cooling, cerebrospinal fluid drainage, systemic hemodynamic manipulations, and distal shunts. Unfortunately, no method(s) has proved to be effective (although few have been tested systematically), and none is widely employed.

One approach would be to devise a better method for detecting the onset of spinal cord ischemia, so that potentially useful therapies might be applied promptly to those patients who would benefit most. This requires some means of monitoring spinal cord function monitoring. Somatosensory evoked potentials (SSEPs) recorded from the scalp after peripheral nerve stimulation have been widely employed, but this method has serious limitations. It is insensitive to selective motor tract ischemia (*i.e.,* there is a documented potential for false negative recordings), and SSEP can be abolished by peripheral nerve (as distinct from spinal cord) ischemia, leading to false positives (*i.e.,* a loss of waveform when there is no actual spinal cord ischemia). Motor evoked potentials (MEPs; using either magnetic or electrical stimuli delivered at the scalp) would seem to offer some advantages, but their extraordinary sensitivity to anesthetics and temperature, for instance, has prevented their widespread use.

In the current issue of ANESTHESIOLOGY, Stühmeier *et al.* (page 1170) present their clinical experience with a method that has obvious promise. Thoracic and lumbar epidural catheters were placed, allowing both the stimulus and the conducted response to be applied to and recorded from the spinal cord itself. This avoids some of the problems that plague both standard SSEPs and MEPs. It can be argued that this method has no false negatives, because any loss of potentials should represent true spinal cord ischemia (even if this is not followed by paraplegia), and their data indicate that almost 70% of patients indeed have some degree of ischemia. More importantly, they showed that patients for whom the electrospinogram was lost within the first 15 min after aortic occlusion are at very high risk for clinical cord injury (30%). Given this, it might be reasonable to ask that the surgeon wait for 15 min after aortic clamping before "permanently" dividing the aorta—although it remains to be seen just what alternative surgical approach might be used in such a situation, particularly since the placement of an axillofemoral shunt did not appear to be of great benefit. Perhaps the method would be better used to define a patient population in which therapeutic interventions might be tried without the statistical "dilution" encountered when large numbers of low-risk patients are included in a study.

It is important to emphasize that the authors have not shown that the electrospinogram is "better" than traditional SSEPs (and it would have been helpful if standard SSEPs had been recorded simultaneously), nor have they shown a clear benefit from such monitoring, because they made no effort to intervene when the electrospinogram was lost and because their overall incidence of paraplegia is not clearly different from that reported by others. However, if this method were applied properly in the context of systematic trials of various treatments or protective maneuvers, it might aid in substantially reducing the number of catastrophic neurologic complications of otherwise lifesaving surgery.

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Effects of Inhalational Anesthetics on Biochemical Events in Growing Neuronal Tips

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ANESTHESIOLOGISTS have had a long-standing concern that inhaled anesthetics given during the developmental period might produce adverse outcomes. In well defined animal models, the administration of inhaled anesthetics during the perinatal period has been associated with morphologic and behavioral abnormalities. The type and severity of these teratogenic effects depend on the animal model examined and the anesthetic administered. For example, marked anomalies in the fetal central nervous and skeletal systems develop after exposure of pregnant rats (on the 9th day of gestation) to nitrous oxide. Pregnant rats given other gaseous and volatile anesthetics do not exhibit an increased incidence of fetal macroscopic lesions, although more subtle (*e.g.,* behavioral) changes in the offspring remain possible.

In this issue of the journal, Saito *et al.* (page 1338) attempt to provide the biochemical explanation behind the influence of inhaled anesthetics on the developing nervous system. The authors examined developing neural tips (growth cones) in neonatal rats after exposure to clinical concentrations of nitrous oxide or halothane. These growth cones are thought to be responsible for the pathways taken by developing neurons and the establishment of appropriate synaptic connections in the developing or regenerating nervous system.

One-day postnatal rats were exposed to 75% N_2O or 1% halothane for 6 h. After anesthetic exposure, growth cone particles were isolated from brain on postnatal days 2, 3, 4, and 5. Anesthetic exposure did not alter body or brain weight compared to controls. However, halothane or nitrous oxide decreased the activity of protein kinase C (by about 30%) and the phosphorylation of two specific proteins (by about 70%) in growth cones isolated on the 2nd postnatal day. The decreases in protein kinase C activity and protein phosphorylation mainly were reversible by the 5th postnatal day.

Does the action of anesthetics on growth cone particles explain all the teratogenic effects of anesthetics? The answer is no! Because halothane and nitrous oxide have similar biochemical effects on growth cone particles, the gross central nervous system lesions produced in the fetus after exposure of pregnant rats to nitrous oxide (but not halothane) cannot be explained by an action on growth cones. Neither can the behavioral and/or synaptic abnormalities produced in rodents by low (*e.g.*, 10 ppm) halothane concentrations (studies quoted by Saito *et al.*) be attributed to anesthetic action on growth cones, since 500-fold higher halothane concentrations (0.5%) had no detectable effect on protein kinase C activity. Finally, it needs to be noted that animal studies of anesthetic-induced teratogenicity may not apply to humans. There is no definitive evidence that exposure of pregnant women or newborns to inhaled anesthetics produces deleterious effects on fetal or neonatal development.

The provocative findings of Saito *et al.* demonstrate an anesthetic effect on growth cones in developing rat brain and suggest that early exposure to inhaled anesthetics may influence the development of neural networks. A causal link between anesthetic effects on neuronal growth cones and the teratogenic/behavioral effects of anesthetics remains to be proved.

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