

but did not continue during maintenance of anesthesia with isoflurane or reappear on recovery supports a role for nitrous oxide, since the effect of nitrous oxide would be most prominent on induction (*i.e.*, before the development of anesthesia with the more soluble isoflurane). Perhaps nitrous oxide produced the seizure and isoflurane subsequently suppressed the convulsive activity. Isoflurane has been shown to possess anticonvulsant activity.² Such a hypothesis also is consistent with the absence of convulsive activity on recovery, since isoflurane would be eliminated more slowly than nitrous oxide and thus would continue to suppress any tendency toward nitrous oxide-induced seizures.

The publication by Krenn *et al.*³ may be used to support the hypothesis that nitrous oxide rather than isoflurane produced the seizure seen. Krenn *et al.* administered halothane three times to a 5-year-old boy, twice with nitrous oxide and once without nitrous oxide.

Convulsive activity occurred during induction on both occasions when nitrous oxide was given but did not occur when halothane was given alone.

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In reply:—Dr. Eger makes a valuable and welcome point. We were unaware of the reference by Krenn *et al.* at the time the manuscript was originally prepared. It certainly does support the possibility that nitrous oxide rather than isoflurane may have produced the seizure observed in our patient.

It is noteworthy that, since publication of the manuscript, I have communicated with an established investigator who years ago was a junior member of a research team that prepared one of the early reports cited in my manuscript. On condition of strict confidentiality of my source, that investigator shared with me his recollection that "two or three" unpremedicated subjects experienced seizures during isoflurane anesthesia but were not re-

ported in the published series. The reason for the failure to report the data was attributed to "the pressure under which we were working."

Information presented in this manner cannot be investigated, therefore, it is of questionable value. Nonetheless, coupled with our finding in one patient, it reinforces the notion that perhaps a new and carefully structured critical investigation is warranted.

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Simple System for Portable Positive End-expiratory Pressure

To the Editor:—Positive end-expiratory pressure (PEEP) frequently is required when transporting ventilated patients to and from the intensive care units and operating rooms. The two most common systems used in conjunction with a nonbreathing resuscitation bag are the Magnetic PEEP valves (Instrumentation Industries) and PEEP Accessory (Puritan-Bennett Corporation). These products are internally complex and require

testing and calibration prior to use. We have assembled an accurate, lightweight, inexpensive, and simple-to-use system that has been employed successfully in our institution (fig. 1). It avoids the necessity of monitoring PEEP levels in transit and limits the risk of undetected disconnection because all parts are simple in function and within the operator's view.

An expiration diverter (Laerdal Medical Corporation)