References

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2. Gribbin J: Hothouse Earth. New York, Groven Weidenfield, 1991
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In Reply:—Johnstone's letter concerns our Fluro-Ethyl® spray, (a nonflammable prescription topical anesthetic or skin refrigerant) and chlorofluorocarbons (CFCs) implicated in stratospheric ozone depletion.

Fluro-Ethyl contains 75% (by volume) dichlorotetrafluoroethane (114), a CFC also known as Freon® 114 or Dymel® 114, and 25% ethyl chloride. Johnstone is incorrect in stating that dichlorotetrafluoroethane is Freon 12. The chemical name for Freon 12 is dichlorodifluoromethane.

Freon 114 has been identified as a stratospheric ozone-depleting chemical. An accelerated phase-out is in progress for this chemical and for the other class I chemicals listed in the Clean Air Act Amendments of 1990, Title VI-Stratospheric Ozone Protection. The original phase-out, once set to be January 1, 2000, has been accelerated by Executive Order of President Bush. By the end of 1995, there will be no production of most class I chemicals; dichlorotetrafluoroethane is included in this phase-out. It also should be noted that this phase-out is far more strict than the United Nations-sponsored Montreal Protocol.

Johnstone states that other products, such as ethyl chloride, are available to anesthetize skin. However, ethyl chloride is flammable; Fluro-Ethyl® is not. Ethyl chloride, therefore, is sometimes not permitted in hospitals if not stored according to the recommendations of the Joint Commission on Accreditation of Hospitals.

It also should be noted that the current U. S. production of 114 is 50% of the 1986 volume, 360 million pounds. Fluro-Ethyl® uses 0.004% of this amount.

Gebauer is complying with the phase-out schedule. Regardless of the fact that we have no replacement at present for Fluro-Ethyl®, there will be no production of 114 in 1995; therefore, there will be no product.

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Risk of Ischemia in Patients Receiving Desflurane *Versus* Sufentanil: Sample Size and Clinical Significance

To the Editor:—Anticipating the arrival of desflurane into our clinical armamentarium, I eagerly read Helman *et al.*'s randomized trial of sufentanil *versus* desflurane in patients with documented coronary artery disease.[1] Helman *et al.* performed what appears to be a methodologically exacting study demonstrating that desflurane, like the other potent inhalational agents, can be used, with appropriate adjuncts, without unacceptable hemodynamic consequences in this patient population.

What is unclear to me as a clinical reader is their discussion directed at differences between the sufentanil and desflurane groups relative to adverse cardiac outcome. First we are informed that "our sample size was insufficient to detect a significant difference, and that even a "10-fold" increase in sample size would have been unable to detect a difference. But then the authors conclude that on the basis of a "relative risk" calculation, we should not rule out that desflurane could be associated with more prebypass ischemia than sufentanil in these patients.

What does "not significant" mean to the clinical reader? To me, it suggests that under the rules established by the investigators at the outset of the study, and subsequently validated by referees, they were unable to demonstrate a difference in the tested treatments. Clearly, Helman *et al.* were plagued with the common problem of a sample size insufficient to deal with a small "effect size" (*i.e.*, the strength of the treatment on the measured outcome). Another example of the Beelzebub of the clinical trial is, namely, does absence of evidence constitute evidence of absence?

What really disturbs me about Helman *et al.*'s paper is where it leaves us. First, the authors conclude that there was no significant