

to pain and emotional stress during surgical, obstetrical, and certain medical procedures."\* This premise must be considered our first and most important aim. However, transgressions, exceptions, and deviations have all been noted.

We wish to express our concern about this opinion because numerous cases of awareness<sup>3-5</sup> have been reported during the last decade with apparently an ascending trend. Although this is not a new problem, as the first case was noted as far back as 1846,<sup>6</sup> nevertheless, for nearly 140 years these unexpected events have been considered undesirable, regrettable, and perhaps even liable.

In their study, Bogetz and Katz<sup>2</sup> concluded that awareness by multiple trauma victims is acceptable. The acceptability of awareness under those circumstances and their interpretation introduced reasons for debate, because surely we can find agents in our armamentarium (scopolamine, low-dose ketamine, or lorazepam, *etc.*) that under circumstances of hemodynamic instability may bring about anesthesia with minimal doses. Let's leave no doubt that we are not referring to periods of low cardiac output or no cardiac output at all (*i.e.*, cardiac arrest), when the inadequacy of cerebral blood flow barely keeps the neurons alive, much less allows the memory functions to go on. We are specifically referring to the phase of recovery when hemodynamic indexes are improving.

One other "twist" on the polemic that has not been considered is the possibility that the souls of Bogetz and Katz's patients were actually "watching" the resuscitative process in a manner similar to experiences related by some victims of cardiac arrest. Those latter patients have described their bodies being resuscitated while their vigilant souls "floated" overhead.<sup>7</sup> Were some of these patients truly "aware" because of insufficient anesthesia or is their recall a product of extraordinary surveillance while under stressful circumstances? Indeed, some patients who un-

dergo awareness during anesthesia may be reassured by the growing evidence on near-death experiences, which seem overwhelmingly positive and have elements—such as autopsy—in common with some awareness episodes.<sup>7,8</sup>

To decipher the mystery, it has become obvious that greater scrutiny would be desirable in all cases of awareness, including a psychiatric consultation and also, if necessary, hypnosis. As Marcia Angell<sup>9</sup> has noted, "pain is soul destroying," and since one of our primordial goals is to prevent it and/or treat it, to admit to its inevitable occurrence in trauma victims seems inadmissible. Her recommendation, "the quality of mercy, here of all places should not be strained" is indeed pertinent.

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\* Guidelines for patient care in anesthesiology. American Society of Anesthesiologists Directory of Members. Chicago, American Society of Anesthesiologists, 1985, p 494.

## Health Experiences of Operating Room Personnel

*To the Editor:*—Buring *et al.*,<sup>1</sup> in their abridged version of a report originally submitted to the American Society of Anesthesiologists, apply the relative risk approach to health data compiled from six selected epidemiologic studies previously conducted in the United States and abroad. Unfortunately, even this sophisticated method

does not permit firm conclusions, and once again we are left with only suggestive answers to the question, "What are the risks from exposure to waste anesthetic gases in the workplace?" It would appear that reanalysis of retrospective data from this selected group, or even from all of the more than 20 epidemiologic studies conducted

during the past decade, is unlikely to provide definitive conclusions. Axiomatically, it follows that those individuals previously unconvinced remain so, while the proponents draw support from the suggestively positive conclusions of this latest review.

Although there are serious limitations to the retrospective approach, it is unfortunate that both Buring *et al.* and Mazze and Lecky,<sup>2</sup> in their accompanying editorial, chose to eliminate from their consideration data presented in the National Dental Study.<sup>3</sup> Buring *et al.* excluded this study because "exposure (by dental personnel) to anesthetic gases differs from that of operating room personnel."<sup>1</sup> Likewise, Mazze and Lecky failed to discuss the implications of its findings since they assumed that scavenging of waste anesthetic gases is now universally practiced and therefore anesthesiologists are not exposed to the levels of waste gases that affect dental personnel.

Granted that dentists are not physicians, and dental assistants usually not nurses, the opportunity to employ an equivalent control group of professionals unexposed to inhalation anesthetics was unique. The study provided useful answers and went a long way in balancing out the difficultly controlled factors of stress, use of extraneous drugs, *etc.*, as well as many of the design flaws that were criticized by Buring *et al.* in the surveys they did review. Since most dentists who use inhalation agents use only nitrous oxide, health and reproductive information was computed for exposure to this one drug alone. This again is in contrast to the hospital-based surveys where workers were exposed to various combinations of halogenated anesthetics and nitrous oxide.

Both paternal (dentists) and maternal (dental assistant) exposure to nitrous oxide alone was associated with statistically significant increases in spontaneous abortions. Women directly exposed to nitrous oxide had a doubling in spontaneous abortion rates when compared with unexposed controls. Although paternal exposure to nitrous oxide was not associated with a higher rate of congenital abnormalities, the children of chairside assistants exposed to nitrous oxide had a greater incidence of birth defects. Dentists and chairside assistants exposed to nitrous oxide reported higher rates of liver, renal, and neurologic disease than nonexposed controls.

The neurologic findings of this study are especially important.<sup>4</sup> None of the epidemiologic studies discussed by Buring *et al.* had considered the entity of "nitrous oxide neuropathy." Mazze and Lecky, in referring to a publication by Layzer *et al.*,<sup>5</sup> stated that "nitrous oxide at much higher than waste concentrations; *i.e.* when used recreationally has significant adverse effects."<sup>2</sup> Unfortunately, they failed to discuss a second detailed article by Layzer that reports two patients denying recreational use but who suffered from "nitrous oxide neuropathy" after exposure to nitrous oxide in the workplace.<sup>6</sup> This article also in-

dicated that these findings had important implications for occupational health and that prolonged exposure to nitrous oxide in the workplace could lead to neuropathy.

Those of us that work in operating rooms should be concerned with the possibility of health and reproductive problems from waste gas exposure and specifically from nitrous oxide. As Buring *et al.* state, "no single epidemiologic study can definitively establish a cause-effect relationship due to the potential for chance, bias, or confounding to explain the results. . . ."<sup>1</sup> It is possible, yet by no means certain, that a large, carefully planned prospective study may be more helpful in providing definitive information. Such a study would be very expensive, requiring not only medical confirmation of each health event, but also the measured concentrations of individual anesthetic gases. Even then one is left with the necessity of controlling for additional factors of stress, smoking, drug and alcohol ingestion, responder bias, *etc.* The thousands of dollars spent on the earlier studies would be exceeded many times. If a prospective study is deemed feasible in the future, the dentist and dental assistant groups offer a prime opportunity for study.

Regretfully, the undersigned must take issue with the content of the editorial by Mazze and Lecky.<sup>2</sup> A more balanced presentation of both sides of the issue would have been helpful. The many animal experiments demonstrating reproductive hazards from chronic low-dose exposure to nitrous oxide during pregnancy and the increase in neurologic complications and the other health and reproductive problems identified in the National Dental Study cannot be ignored. As an aside, reading the original Buring article one fails to find any mention for their final editorial quotation stating "there is no basis at this time for federal government (OSHA) regulation of waste gas exposure or scavenging."<sup>2</sup>

Certainly, all agree, albeit for different reasons, that continued waste gas scavenging is indicated. Importantly, scavenging must be practiced as part of a total program that includes periodic air sampling and waste gas monitoring to assure success of the scavenging. Our goal and responsibility should be to document and achieve the lowest possible levels of exposure to nitrous oxide and other waste anesthetic gases. Conscientious adherence to such a program will minimize the health and reproductive hazards of the exposed population.

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*In reply:*—Drs. Brodsky and Cohen, in their letter commenting on the Special Article by Buring *et al.*,<sup>1</sup> suggest that failure to consider the National Dental Study<sup>2</sup> in their evaluation may have prevented Buring *et al.*<sup>1</sup> from reaching a definitive conclusion, *i.e.*, that trace concentrations of nitrous oxide are hazardous. In a similar vein, they comment that the editorial<sup>3</sup> we authored accompanying Buring *et al.*<sup>1</sup> also should have considered the National Dental Study<sup>2</sup> as well as relevant animal studies examining nitrous oxide administration during pregnancy.

Perhaps some explanations are in order. Buring *et al.*<sup>1</sup> did not consider the National Dental Study<sup>2</sup> because their charge from ASA was to evaluate the hazards of operating room exposure to waste anesthetic gases. Exposure of dentists and their chairside assistants to waste anesthetic agents occurs under different circumstances and is of greater magnitude than that of operating room personnel. Therefore, extrapolation of results from the dental population to operating room workers might not be appropriate. By not including the dental study, Buring *et al.*<sup>1</sup> wisely avoided one more confounding variable in a situation that has been plagued by such problems.

We also question whether the National Dental Study<sup>2</sup> can be considered more definitive than previous epidemiologic surveys of operating room workers. Without doubt, its overall design, response rate, and statistical approach are superior to that of the vast majority of previous surveys. However, in our opinion it shares some of the same fatal flaws of the other surveys, *i.e.*, lack of medical verification of questionnaire data; generally small differences in both absolute and relative risk, which, therefore, could have been due to misclassification or to response bias; and the possibility that differences may have been due to biased recall of past events. Any one or a combination of these factors could have led to the differences Cohen, Brodsky, and their co-workers reported in the National Dental Study.<sup>2</sup>

The omission of animal studies in the editorial was a deliberate one. Buring *et al.*<sup>1</sup> dealt only with human studies, and we restricted our discussion to humans as well. However, the overwhelming majority of animal studies performed with nitrous oxide in the waste gas range (25-500 ppm) have been negative. We are surprised that Brodsky and Cohen comment otherwise. It would require too many pages of this Journal to debate that point.

In summary, we believe that Buring *et al.*<sup>1</sup> and, hopefully, our editorial<sup>3</sup> have made useful contributions toward putting to rest the issue of the putative hazards of operating room employment. Buring *et al.*<sup>1</sup> point out the areas of increased risk (spontaneous abortion and liver disease) and their magnitude (small). They also note that these findings could have been due to response and/or recall bias and, in fact, they point out that they are not necessarily associated with exposure to waste anesthetic gases. They may be a consequence of operating room employment. Our editorial emphasizes some of the pitfalls associated with the conduct of epidemiologic surveys, particularly those that have dealt with the waste anesthetic gas issue. Perhaps the debate will end when the 10-year United Kingdom prospective study of the health of women physicians and dentists, now in its eighth year, is completed.

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