peutic approach on oxygen delivery and right ventricular perfor-
ance. This may allow us to decide appropriately whether an individual patient will benefit from inhaled NO.

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Warn Asthmas of Scuba Diving Risks

To the Editor:

I am writing to ask that very clear information and warnings be given to people with asthma (both newly diagnosed and chronic) concerning the risk of scuba diving. This concern stems from my husband’s recent and sudden death at the age of 47 from an asthmatic reaction subsequent to a relatively shallow scuba dive in Cancun, Mexico.

My husband was certified as a scuba diver in 1969 while in college. He had dived several times, but not for approximately 20 years. Apparently, certified divers are expected to know their own health risks and recertification and updating are generally not required, as they were not, in this case. Few questions are asked or warnings issued.

In my husband’s case, the dive instructor did indicate that asthma created some risk. However, because my husband had dived previously with no problem, while an asthmatic and while using inhalers, and was enough of a risk-taker to consider diving in the first place, he underestimated the degree of risk.

I am asking here that all physicians who treat people with asthma—and perhaps other lung diseases—tell their patients the degree of risk that scuba diving entails. The chronicity of asthma should be indicated as an additional risk. Even certified divers who have asthma should be asked to consult with their doctor prior to any dive. The standard waiver/release indicating that diving entails risk may be perceived as pro forma.

Information about diving with asthma, if communicated regu-
larly between physician and patient, may save lives or avert the tragedy of injury. Similarly, information could be included in the literature accompanying asthma inhalers.

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Wisdom in Video-Assisted Cardiac Surgery

What Can or Should Be Done?

To the Editor:

We read with great interest the recent paper by Tsai and colleagues (December 1996)1 reporting their preliminary expe-
rience with the application of video-assisted techniques in reop-
erative mitral valve surgery. We were impressed with the tech-
nical abilities of this group of enthusiastic surgeons. Nevertheless, we would like to express our concern regarding their patient selection and the conclusions of the report.

There is no doubt that the principle of minimizing the surgical incision has become widely accepted, and this has recently found its extension into the field of cardiac surgery.2-5 Yet, in cardiac surgery, there are as yet no data that confirm that smaller incisions are synonymous with less morbidity. All that can be said from the experience to date is that, in combination with the use of cardiopulmonary bypass, median sternotomy is likely to be associated with a higher morbidity than a lesser incision, but this remains to be substantiated.

Realizing this, if video-assisted cardiac surgery were to be an advancement on established practice, it would have to be advantage-
tous in terms of patient outcome. Moreover, the safety of these innovative approaches must be addressed inasmuch as surgical precision and clinical outcome cannot be sacrificed for the yet-doubtful pledge of benefits ascribed to reduced short-
term morbidity.

In the report of Dr. Tsai and colleagues,1 four very ill patients in need of emergency reoperative mitral valve surgery were selected. One cannot help immediately noticing the exceptionally prolonged cardiopulmonary bypass perfusion time, with a mean of 222 min (of particular significance is patient 4, who underwent mitral valve thrombectomy requiring 320 min of cardiopulmo-
nary bypass perfusion). Furthermore, the need for deep systemic hypothermia and hypothermic fibrillatory cardiac arrest in iso-
lated mitral valve surgery is by no means standard practice. Bearing these issues in mind, one is obliged to question the wisdom of subjecting critically ill patients to “experimental” surgery, and to question whether these patients fared better than those undergoing conventional surgery. Yet, the paper by Tsai et al1 offers no data to support their actions, since it neither presents the patients’ postoperative progress nor demonstrates any other advantage of the video-assisted approach.