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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REFERENCES
1 Böttiger BW, Bach A, Böhler H, et al. Acute pulmonary embolism: clinical, pathophysiological, diagnostic, and therapeu-
91-97
4 Böttiger BW, Motsch J, Dörsam J, et al. Inhaled nitric oxide selectively decreases pulmonary artery pressure and pulmo-
5 Blanch L, Baigorri F, Fernandez R, et al. Efecto vasodilata-
757-59
7 Dantzker DR, Bower JS. Alterations in gas exchange follow-
8 Barbera JA, Roger N, Roca J, et al. Worsening of pulmonary
gas exchange with nitric oxide inhalation in chronic obstruct-

Warn Asthmatics 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 ex-
perience with the application of video-assisted techniques in reop-
erative mitral valve surgery. We were impressed with the
technical abilities of this group of enthusiastic surgeons. Neverthe-
less, 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 advan-
tageous 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.

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