transannular and right ventricular patching equally affect late functional

REPLY
We appreciate the opportunity to reply to the letter sent by Dr.
d’Udekem and colleagues concerning the article we recently
published in JACC.

Although we are grateful for their interest in our work, we
would like to address the issues they have raised. First, their
statement that “...all patients operated on... showed neither
clinical improvement nor regression of ventricular volumes after
reoperation...” is incorrect. As our article clearly stated, most of
our symptomatic patients had a significant clinical improvement
postoperatively (24% NYHA class ≥ III preoperatively vs. 0%
postoperatively, p < 0.001) (1). Unfortunately, this could not be
confirmed objectively (the same mean duration of exercise and
external workload was achieved both pre- and postoperatively).
Furthermore, although the mean right ventricular volume and
function did not improve postoperatively, about one-third of our
patients did show such improvement as depicted in Figures 1, 2
and 3 of our article (1). Unfortunately, the relatively small number
of patients made it impossible to determine which of the many
factors analyzed favored such a response. Finally, we disagree with
the statement that "the... effect of this [bulky bioprosthesis]... is
to further increase the size of the right ventricular cavity..." and
impede contraction of the pulmonary infundibulum by virtue of its
extensive transannular patching. They are correct that we do
enlarge the outflow tract to accommodate as large a prosthesis as
possible, but the patch to do this is largely from the annulus
distally. Proximal to the annulus, the patch extends into the
infundibulum a distance of 10 to 15 mm. We do not believe that
a 10 to 15 mm incision below the pulmonary annulus would have
an important impact on right ventricular volume and function. We
do agree, however, that any incision in the right ventricle should be
avoided or minimized as much as possible in order to potentiate
maximal postoperative functional recovery.

The issue of pulmonary valve replacement in adults late after
repair of tetralogy of Fallot remains a controversial one and we
again thank Dr. d’Udekem and colleagues for their interest in our
work.

Judith Therrien, MD
The Sir Mortimer B Davis–Jewish General Hospital
3755 Chemin de la Cote-Sainte Catherine
Room E-206
Montreal, PQ H3T 1E2 Canada
E-mail: judiththerrien@hotmail.com

Gary Webb, MD
William G. Williams, MD

REFERENCE
GD. Pulmonary valve replacement in adults late after repair of
tetralogy of Fallot: are we operating too late? J Am Coll Cardiol

A Real Smoker’s Paradox
The article by van Domburg et al. (1) in the September issue of the
Journal presented detailed but not unexpected findings with regard
to the hazards of cigarette smoking. However, I wonder whether
the authors are aware that in Figure 2 of their paper, there is a
graph which implies that the survival rate of cigarette smokers who
quit after their bypass operation is significantly better than those
who continue to smoke, but also significantly better than those
who have never smoked. One would have to conclude from this
data that the best chances of survival are among those who smoke
up until the time of their surgery and then quit, rather than never
to smoke at all. This would truly be a smoker’s paradox if in fact
it is correct!

I would appreciate some explanation from the authors.

David Shander, MD
Rose Medical Center
4545 East 9th Avenue
Suite 150
Denver, Colorado 80220

REFERENCE
1. van Domburg RT, Meeter K, van Berkel DFM, Veldkamp RF, van
Herwerden LA, Bogers AJJC. Smoking cessation reduces mortality

REPLY
We appreciate the comments of Dr. Shander regarding our recent
article in JACC (1). In his comments he concluded that “the best
chances of survival are among those who smoke up until the time
of their surgery and then quit rather than never to smoke at all.”
We have proven that patients who quit smoking after bypass
surgery are significantly better off than those who continue to
smoke. However, in our study we only compared the patients who
quit smoking with patients who continued smoking. Because of its
irrelevancy, we did not compare the patients who quit smoking
with patients who did not smoke. Furthermore, we did not use the
term “never smoked” but used the term “nonsmoking.” We did not
distinguish between patients who have never smoked and ex-
smokers (patients who had stopped smoking before the time of
surgery), and we combined these two groups into one nonsmokers’
group at the time of surgery. The smoking habits at the time of
surgery did not influence survival during the follow-up period.
This smoker’s paradox is partly explained by the difference in
baseline characteristics such as an age difference (smokers were
four years younger than nonsmokers). Another explanation could
be selection bias, as many smokers tend to die of fatal myocardial
infarctions before they have the chance to undergo coronary bypass
surgery (2).

Finally, the survival rates of the nonsmokers were probably
positively influenced by the ex-smokers. In conclusion, the worse
condition of the nonsmokers as compared with the smokers at the