Figure 1. Immunofluorescent staining on cells grown out of cardiac tissue. Cells incubated with CD45-FITC (A, lower left) and C-kit-phycoerythrin (A, upper right). 4,6-diamino-2-phenylindole (A and B, upper left) was used to stain nuclei. A, Outgrown c-kit⁺ cells do not coexpress CD45 and c-kit. B, Cells are incubated with CD105-FITC (lower left) and c-kit-phycoerythrin (upper right); 95% of the cells do express CD105 and a subpopulation coexpresses c-kit. DAPI, 4,6-diamino-2-phenylindole.

outgrowth. So the difference in the expression profile between our c-kit⁺ cells and that detected by Pouly and colleagues¹ cannot be the result of cell processing.

Our findings indicate that c-kit⁺ cells, present in right atrial appendages, coexpress CD105 but are CD45⁻. It is therefore unlikely that these cells are mast cells because our data indicate that they are probably cardiac progenitor cells.

Remco Koninckx, MSc

Karen Hensen, PhD

Jean-Luc Rummens, MD

Marc Hendriks, MD

Virga Jesse Hospital, Hasselt, Belgium and

University of Hasselt, Diepenbeek, Belgium

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Reply to the Editor:

We thank Koninckx and colleagues for their comments. As stated in our article, data were obtained from both endomyocardial biopsies and atrial appendages, and these 2 sampling sites yielded concordant data. However, the major difference is that we performed in situ detection and characterization of cells, whereas Koninckx and colleagues cultured cells for 2 weeks before immunostaining. Such a time interval can change

the cell phenotype and delete some cell populations that do not survive under these conditions. The latter phenomenon could explain why Koninckx and colleagues did not find any mast cell in their myocardial tissue cultures, whereas it is well established that the myocardium does contain such cells. Because a minor component of the c-kit-positive cells could have represented a subset of cells different from mast cells, we also tested them for other markers of stemness (CD105, islet-1, and MDR1). However, in our hands, these markers remained negative. The data of Koninckx and colleagues suggest that after a period of culture, c-kit-positive cardiac "stem" cells can be identified, but it would be clinically relevant that they provide a quantitative estimate of these cells to assess whether this number allows one to reasonably envision their use for therapeutic purposes.

Julia Pouly, MD

Patrick Bruneval, MD

Philippe Menasche, MD, PhD

Hopital Europeen Georges Pompidou

Department of Cardiovascular Surgery

Paris, France

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Aprotinin; An economy of truth? To the Editor:

I write to comment on the editorials by Drs Sundt¹ (April 2008) and Westaby² (March 2008). Dr Sundt is perceptive but fails to consider that the observational studies suggesting a danger with aprotinin may have had bias in the analysis.

At the advisory committee meeting of the Food and Drug Administration (FDA) held on September 12, 2007, Dr Mangano allowed the FDA access to the McSPI (Multicenter Study of Perioperative Ischemia) data set. Whereas Mangano and col-

leagues³ used a propensity score that was based on likelihood of bleeding, the FDA reanalysis of these data used stratification according to risk of adverse outcome. The FDA analysis showed no increases in relative risks (RRs) for death (RR 0.91, 95% confidence interval [CI] 0.54–1.53), heart failure (RR 1.05, 95% CI 0.75–1.47), myocardial infarction (RR 1.10, 95% CI 0.88–1.39), or renal dysfunction (RR 1.26, 95% CI 0.76–2.11) when data from 1222 aprotinin-treated patients were compared with those of 1307 patients who did not receive the drug.⁴

At the same FDA meeting, Dr Karkouti, who used matching of pairs of data, showed that the inclusion into the model of cardiopulmonary bypass variables (time and circulatory arrest) and transfusion (>4 units of red blood cells and fresh-frozen plasma) removed the statistical effects of aprotinin on renal function.⁴ The Toronto data have never shown any other mortality or morbidity risk. Dr Funary also presented the North West Consortium analysis, which showed that any apparent effect of aprotinin on adverse outcome is lost when red blood cell transfusion numbers are included as a confounding variable.⁵

In the article from Shaw and colleagues⁶ of the Duke University Medical Center, the populations of patients receiving aprotinin or ε-aminocaproic acid (EACA) were hugely different. No matter how clever the statistical modeling, clinicians will recognize that there must be differences in management and outcome between a patient with isolated myocardial ischemia undergoing primary, elective revascularization (given EACA) and one undergoing a nonelective reoperation for heart failure associated with valve pathology (who would likely receive aprotinin in about 70%-80% of cases worldwide). Despite this, Shaw and colleagues⁶