Letter by Ranucci et al Regarding Article, “Comparison Between Transcatheter and Surgical Prosthetic Valve Implantation in Patients With Severe Aortic Stenosis and Reduced Left Ventricular Ejection Fraction”

To the Editor:

We read with interest the article by Clavel et al1 that was recently published in Circulation. The authors presented interesting results, but we think that some points deserve to be highlighted. The authors could demonstrate that, in patients “considered at high or prohibitive surgical risk” treated with transaortic valve implantation (TAVI), the left ventricular ejection fraction (LVEF) recovers better than in patients who undergo surgical aortic valve replacement. This information is an important contribution to a hot topic; however, there are probably some problems with definitions in this study.

The first is related to the concept of “reduced” (title) or “depressed” (abstract) LVEF. The patients admitted to this study had an LVEF of ≥50%; this cutoff value is actually much higher than in the previous studies quoted by the authors,2–5 where the patients had an ejection fraction between 30%2 and 35%.3–5 In their series, the authors report a 30-day mortality rate of 12% in the surgical aortic valve replacement group. This value is quite high for this patient population. It is consistent with data collected 10 years ago in patients with a more severe LVEF reduction,2,4 but it is higher than the data reported by more recent studies (8% to 9%),3,5 again in patients with a more severely depressed LVEF (≤35% in both studies). On the basis of these recent data, we believe that a population of patients with an ejection fraction of ≤50% should have an expected mortality rate for surgical aortic valve replacement no higher than 8%, and the mortality rate reported by the authors for TAVI (19%) should be confronted with this value.

A second problem is the concept of high surgical risk. At present, this should correspond to a logistic EuroSCORE of ≥20 or a STS-PROM score of >10 for referring patients to TAVI, but Clavel’s study does not provide any information on patients at lower risk. Therefore, we believe that the information that LVEF will benefit more from TAVI than from surgical aortic valve replacement is interesting, but not necessarily useful in the selection of patients for TAVI, which should still be based on the overall mortality risk and not on the presence of a (moderately) depressed systolic ventricular function.

We recognize the quality of the study by Clavel and associates and the importance of their results, and we are sure that it was not the authors’ intention to spread a liberal message for TAVI indications. However, this is certainly a sensitive matter, and we believe that some aspects of this study are in need of clarification.

Disclosures

None.

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References

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Circulation. 2011;124:e205
doi: 10.1161/CIRCULATIONAHA.110.008417

Circulation is published by the American Heart Association, 7272 Greenville Avenue, Dallas, TX 75231
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Print ISSN: 0009-7322. Online ISSN: 1524-4539

The online version of this article, along with updated information and services, is located on the World Wide Web at:
http://circ.ahajournals.org/content/124/7/e205

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