studies with longer follow-up are necessary to demonstrate whether these differences have a significant clinical effect. With the present study, we wish to highlight that surgical aortic valve replacement is still the best choice for patients with aortic valve stenosis. However, new therapeutic options such as TAVR and SU-AVR can provide good results in select patients. A center that is able to offer their patients all these therapeutic alternatives can select the most appropriate technique, tailoring the choice to each patient and considering all crucial characteristics such as age, comorbidities, frailty, and anatomy. A particularly careful evaluation is needed for patients in the “gray zone,” who can benefit from either technique. An experienced “aortic team” will be able to make the most appropriate choice. The limitations of the present study were mainly related to the retrospective nature, the different procedures conducted at different centers, the inclusion of TA-TAVR–only patients, and the small number of patients in the SU-AVR cohort.

In conclusion, our data have shown that no main differences exist in the outcomes among SAVR, TA-TAVR, and SU-AVR. SAVR was associated with a significant reduction in postoperative AR compared with TA-TAVR. The latter, however, showed lower transaortic gradients. A trend was seen toward less AR in the SU-AVR group than in the TA-TAVR group; however, this difference was not statistically significant. Future, larger, and, possibly, prospective studies are needed to confirm our preliminary results.

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Discussion

Dr Martin Misfeld (Leipzig, Germany). I would like to thank the American Association for Thoracic Surgery (AATS) for the opportunity to discuss the report by Dr D’Onofrio and colleagues and Dr D’Onofrio for supplying me with the report in a timely manner.

The present study is a comparison of 566 patients undergoing TA-TAVR in 20 Italian cardiac surgery centers and 38 patients undergoing SAVR in 3 centers and 349 patients undergoing
conventional aortic valve replacement in 1 center. It represents an extension of a study presented at last year’s AATS meeting and has recently been published in the *Journal of Thoracic and Cardiovascular Surgery*, in which comparisons were made between patients receiving a transapical Edwards SAPIEN valve and those who underwent implantation of the Perceval S sutureless valve. To the best of my knowledge, the present report is the first to compare these 3 patient groups simultaneously.

Dr D’Onofrio and colleagues have used sophisticated propensity score matching to compare these 3 groups. However, the overlapping treatment period of the 3 patient groups and the lack of randomization very likely resulted in significant differences among the groups. The patient factors that were not measured, such as frailty, porcelain aorta, or other risk factors for conventional surgery, were undoubtedly different among the groups. Although these unmeasured patient factors could explain the elevated mortality observed in the TAVR group, they are less likely to have affected the observed hemodynamic differences among the groups (ie, the lower transvalvular gradient and greater incidence of AR in the TAVR group).

I have 4 questions for the authors.

First, what were the criteria for deciding whether patients underwent TA-TAVR or sutureless valve implantation in the elderly high-risk subgroup and what patient-related features would have made you decide that 1 technique would be definitely more suitable than the other?

Second, the paravalvular leak rate for the sutureless group was greater than that usually mentioned in published studies. Do you have an explanation for this?

Third, did the sutureless valve patients have lower crossclamp and cardiopulmonary bypass times than those undergoing conventional AVR?

Finally, is it time for a randomization trial between sutureless valves and TAVR?

Once again, I would like to thank the Association for the honor of being able to discuss this unique and important study.

**Dr D’Onofrio.** Thank you for your kind comments and questions.

Talking about your first question, actually, each of these techniques has pros and cons for high-risk patients. I’m talking about transcatheter and sutureless aortic valve replacement.

Transcatheter is, of course, less invasive. It can be performed on the awake patient, especially if performed through transfemoral access, and does not require cardiopulmonary bypass or aortic crossclamping; thus, it is definitely less invasive. However, the rate of paravalvular leak is still high.

In contrast, SAVR requires cardiopulmonary bypass and aortic crossclamping. With this technique, the hemodynamic results in terms of paravalvular leakage have been much better.

I think that the choice between these 2 alternative techniques should be tailored to the characteristics of each patient, considering age, comorbidities, and, as you mentioned, frailty and other factors that usually are not included in risk scores.

Regarding the incidence of paravalvular leak, it was high in the sutureless group, that is true, but even mild AR was included in these analyses. The patients with mild AR were actually the first 30 patients who received the sutureless valve in our country. It was at the very beginning of the learning curve. Currently, we have been observing a progressive decrease in paravalvular leakage as our experience in sizing and the implantation technique has improved.

The crossclamp and cardiopulmonary bypass time was 44 minutes and 69 minutes, respectively, in the sutureless group. Again, this was the very beginning of our experience, and we now are observing that the crossclamp time in these patients can be as low as 20 minutes with the improvement in experience.

Of course, a trial would be an excellent idea. However, as you mentioned, these are 3 techniques for 3 different groups of patients. Thus, it would not be easy to design a prospective study to include all 3 techniques in similar patients.