Keywords: Tracheobronchoplasty • Tracheobronchomalacia • Tracheobronchopexy • Mounier-Kuhn syndrome

We are grateful for the letter from Dr Nagarajan Muthialu regarding our case report on tracheobronchoplasty after a trial of silicone stenting and we appreciate his kind comments about the importance of this procedure to improve the airway dynamics [1, 2].

Fortunately, in our centre, patients selected for surgery have been managed only by a polypropylene mesh attached to the posterior membranous wall of the trachea and main bronchi and reinforced with sequential rows of mattress 4–0 polydioxanone II sutures placed in a partial thickness fashion with satisfactory outcomes.

This described surgical technique has been enough to stabilize and add rigidity to the membranous wall in the case of membranous malacia. Moreover, when the sutures are tied, the membranous wall is plicated and made narrower which reconfigures the normal D-shape of the trachea. When the membranous wall is associated cartilaginous deformation, the surgeon needs to estimate the degree of reduction in the width of the membranous wall that will re-create the D-shape of the trachea.

Nevertheless, in the case of cartilaginous malacia with severe deformations of the cartilage or, in the case of failure to reconstruct the D-shape of the trachea by a posterior mesh reinforced with sequential rows of sutures, we would not hesitate to opt for either an anterior tracheobronchopexy [3] or a 3-dimensional printed bioresorbable external airway splints [4–6] to provide adequate rigidity and radial support to maintain airway patency, in previously selected patients.

All our patients were aged between 35 and 50 years. We have not had experience in treating tracheobronchomalacia in the paediatric age group. In conclusion, we should treat each patient individually and define the anatomic form of tracheomalacia presented in each case.

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What we see depends on what we look for

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Keywords: Aortic valve stenosis • Surgical arotic valve replacement • Transcatheter aortic valve implantation

With great interest we read the meta-analysis on mortality rates after transcatheter aortic valve implantation (TAVI) and surgical aortic valve replacement (SAVR) by Barili *et al.* [1]. The results presented are of great importance and provide a shadow of doubt on the current enthusiasm on TAVI.

Implementation of new technology is risky business as the use of new devices might be related to complications not previously seen with the procedure considered as gold standard. An interesting observation can be made when the recently published 5-year results of the PARTNER 2A trial are studied in detail [2]. Prosthetic aortic valve performance after TAVI seems non-inferior to SAVR. However, treatment should not only focus on the resolution of the primary abnormality itself but also on the resolution of the consequences thereof. As a result of higher outflow gradient seen in aortic valve stenosis, compensatory changes in left ventricular volumes (higher end-systolic and end-diastolic volume) and mass will occur. Successful resolution of aortic valve dysfunction should thus also result in normalization of left ventricular volumes as well as mass regression. The results provided by the PARTNER 2A trial, however, show that TAVI was inferior in stimulating volume and mass regression when compared to SAVR. These results suggest that while the aortic valve is effectively treated with TAVI, SAVR is more effective at treating the disease as a whole. Whether this is a consequence of higher rates of paravalvular leakage or other complications more often seen with TAVI (e.g. intraventricular conduction abnormalities) warrants further study. No matter the underlying cause, late results should in theory be in favour of SAVR, as excellently shown by Barili *et al.*

As a last thought, it should be acknowledged that the performance of TAVI prostheses has improved and that the results of TAVI are far more dependent on the type of prosthesis implanted than the results of SAVR. The study by Barili *et al.* presents the best data on the performance of this technology but is clearly limited by the drawbacks of the available literature. At this point, the level of evidence cannot be considered sufficient to support changes in the way the majority of patients with aortic valve stenosis should be treated.

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Reply to Tomšič and Klautz

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Keywords: Aortic valve • Transcatheter aortic valve implantation • Surgical aortic valve replacement

The development of new technologies for healthcare therapeutics is challenging because expected outcomes may not be consistent with the results obtained by *in vitro* simulations and animal model testing, which could be completely overturned once transferred to humans. Indeed the natural course and the needed timeline for evaluating clinical effectiveness are often too

longer than those of marketing strategy. The comment by Tomšič and Klautz on our article [1, 2] sheds light on a critical issue, and we agree that 5-year results of PARTNER 2A trial are a good example [3]. The implantation of an aortic prosthesis should not only ensure the safety and effectiveness of treating the valve disease but also guarantee long-term results at least comparable to the standard of care, which so far is represented by surgical aortic valve replacement (SAVR). The emphasis on the non-inferiority in the composite outcomes overshadows the lack of expected reversal of myocardial hypertrophy and volumes, as well as the disadvantage of transcatheter aortic valve implantation (TAVI) in terms of reoperations and rehospitalization. A critical appraisal of 5-year changes from the baseline of echocardiographic parameters reported in the Appendix [3] brings to light a lack of reversal of hypertrophy and volumes in the TAVI group and a very significant difference with surgery (*t*-test *P*-values <0.0001 for left ventricular end systolic volume, left ventricular end diastolic volume and left ventricular ejection fraction; *P*-value 0.0003 for left ventricular mass), although the unexplained high quote of missing data means that conclusions should be drawn with caution. In addition, even the key message should be critically weighted considering all the reported results. Landmark analysis shows that TAVI is a risk factor for the primary end point (death from any cause and disabling stroke) after 2 years, with a 27% higher hazard compared to surgery [hazard ratio (HR) 1.27, 95% confidence interval (CI) 1.06–1.53; $P < 0.05$; Fig. S4] [3]. These data have also been confirmed not only in transthoracic access but also in transfemoral cohort, as presented at 2019 EACTS meeting (HR 1.23, 95% CI 1.00–1.52) [4]. Hence, the global non-inferiority cannot be considered a balanced key message, as it does not hold these emerging drawbacks of TAVI.

New devices can be expected to provide better outcomes; nonetheless, a breakthrough change in clinical practice, which is not supported by long-term follow-up data, cannot be justified by predicted hypothetical results. The unexpected can be around the corner and could lead to an unexpected worse treatment option for patients.

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Aortic root surgery; . . .sparing the valve

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Keywords: Aortic valve repair • Aortic valve surgery • Aortic root surgery • Sparing the aortic valve • Bicuspid aortic valve • Aortic valve insufficiency

We read with great interest the study by Beckmann *et al.* [1] entitled 'Aortic valve-sparing root replacement in patients with bicuspid aortic valve: long-term outcome with the David I procedure over 20 years'. We would like to congratulate them on their excellent surgical results and their well-documented article. The benefits of preserving the aortic valve, tricuspid or bicuspid, is well known in the international literature. The risk of reoperation has to be weighed against the risks and benefits of prosthetic graft replacement [2]. Mechanical valve prostheses have the disadvantage of life-long anticoagulation with associated risks of bleeding and thromboembolism [1, 2]. The rate of bleeding after mechanical valve implantation is reported to be 16% in 10 years and 61% in 20 years [2]. Thromboembolic complications occur in 10% of patients after 10 years and 24% after 20 years [2]. With these data in mind, we have to repair any pliable aortic valve, tricuspid or bicuspid. There are well-described techniques with good mid- and long-term results [3, 4]. We would like to comment on two issues of the article by Beckmann *et al.* The graft used in all patients of the above-mentioned series was straight; we believe that the graft should mimic the sinus of Valsalva to have normal blood flow through the valve and the 'synthetic' aortic root. The grafts mimicking the sinus provide a more physiological and less turbulent flow that could destroy the repaired valve. Then, they did not use the caliper of Schäfers that aims to perform a standardized repair of the aortic valve, either tricuspid or bicuspid [5, 6]. The mid- and long-term results are better after the introduction of this tool [4, 5]. We consider it extremely useful to measure the coaptation area of the cusps. Of course the results in patients with Marfan syndrome or other connective tissue diseases, in patients with acute aortic dissection, could not be as good as in patients with simple dilatation of the root and regurgitation of the valve. In conclusion, we would like to suggest the use of grafts mimicking the sinus of Valsalva and the caliper of Schäfers for better long-term results in aortic root surgery and repair of the bicuspid valve. Then, if the cusps are pliable, the aortic valve, either tricuspid or tricuspid, has to be repaired.

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Reply to Baikoussis *et al.*

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