performed.[4] Soppa and colleagues might, therefore, achieve even better surgical results using the sutureless technique with significant improvement in patient outcomes.[5]

We fully agree with Dr Soppa that sutureless aortic valve replacement is an ideal option for redo surgery, such as was recently suggested by our preliminary data in this patient subset.[6]

We believe that sutureless aortic valve prostheses have the potential to shorten the surgical time, and future research will determine whether this advantage will also translate into better outcomes in high-risk patients. Sutureless aortic valve replacement has been shown to be associated with improved survival compared with transcatheter aortic valve implantation, owing to the lower or no rates of residual aortic regurgitation. Only randomized prospective studies comparing the 2 surgical techniques will allow definite conclusions to be drawn regarding this issue.

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Reply to the Editor:

With all due respect to the clinical competence of Drs Delaere and Van Raemdonck, we would like to address their pointed critique as not only unsubstantiated but also demonstrably false, which is both disturbing and damaging to the field of tracheal transplantation.[1]

The most disturbing comment is "more than half of the patients died within a 3-month period."[1] This is incorrect. Of our first 9 clinical applications using a natural scaffold, only 1 died within the short-term period, and the death was unrelated to the transplantation. A report detailing these cases is under review for publication. We can firmly suggest tissue-engineered tracheal replacement is not "destined to fail" as evidenced by survivors beyond 67 months.[2]

Second, the editorial states "Tracheal bioengineering was not tested in animal models," which is untrue, based on our previous publications. In fact, in 1994, we described the surgical technique for, and revascularization of, tracheal allotransplantations in pigs, published in this Journal.[3] To avoid immunosuppression, several large and small animal models and in vitro airway transplantation studies, not requiring immunosuppression were then completed and published in peer-reviewed journals (the number exceeded the reference limit). All have supported the readiness for ethical clinical application. Additionally, advances in neoangiogenesis, epithelial differentiation, stem cell biology, and systemic and in situ regenerative processes have been reported.[4,5] From this sound preclinical evidence, human airway transplantation has been approved by national and local regulatory bodies in 6 countries, including the US Food and Drug Administration, widely regarded as the world's toughest regulatory body.

Finally, Delaere and Van Raemdonck suggested "dissemination of misinformation" could be avoided with "clear visualization of the trachea." Video endoscopy, high-resolution computed tomography scan images, and photomicrography of the regenerated respiratory epithelium, 5 years after transplantation and without an airway stent in place have, in fact, been published,[2] and whose evidence cannot be disputed.

We value the comments of Delaere and Van Raemdonck and other leaders in this field. We do not expect undisputed acceptance of our approach; however, we would appreciate a certain degree of collegiality and respect for our unceasing efforts to push for an innovative and scientifically sound solution for a vexing clinical problem. The trachea is "one of the most difficult organs in the human body to replace." Rebuilding an identical copy of the native airway might not be possible; however, creating an ideal, nonimmunogenic replacement is. The best strategy for replacement and regeneration has yet to be determined. Tissue-engineered tracheal transplantation is still in its experimental phase, far from routine clinical application, and awaits the results of an ongoing clinical trial (www.clinicaltrials.gov). However, the assertions that our preclinical and translational advances in tracheal transplantation are "misleading and unrealistic" are overreaching, given the extensive published data supporting the cells-to-bioartificial scaffold interactions and documented long-term survival of our own patient series.

Finally, the editorial questions whether the trachea is really the first bioengineered organ. This claim has never been made by us, but rather in

a New York Times article describing our work. Dr Anthony Atala has a much better claim to this milestone achievement.

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Reply to the Editor:

We thank Dr Macchiarini for commenting on our editorial published in the *Journal,* and we acknowledge his team's motivation and efforts to advance tracheal replacement.

In his response to our editorial, Paolo Macchiarini refers to several publications, thus undoubtedly convincing many readers of his views. However, in not one of these articles have mortality rates been published. Furthermore, we cannot follow his suggestion to rely on an unpublished article to obtain this information. Nor indeed can we refer to its content, although we have been in a position to read it. However, the unfortunate results after some of the treatments with "bioengineered" tracheas have reached investigative journalists of *Science*[1] and other media.[2,3]

More important, the purpose of our editorial was to inform the scientific community that regeneration of a viable trachea resulting from applying bone marrow cells to a decellularized or a synthetic scaffold in the absence of any blood supply is based on hope and belief and not on scientific evidence. None of the publications that Macchiarini cites in his response provide scientific evidence for his claims. We therefore strongly warn against further unethical human experimentation. The ongoing clinical trials will show whether or not this warning was justified.

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MODIFIABLE RISK FACTORS FOR ACUTE KIDNEY INJURY AFTER CORONARY ARTERY BYPASS GRAFTING

To the Editor:

We read with interest the recent article by Ng and colleagues[1] that identified modifiable risk factors for acute kidney injury (AKI) after coronary artery bypass grafting (CABG) in an Asian population. They showed that preoperative anemia and intraoperative lowest hematocrit were potentially modifiable risk factors independently associated with postoperative AKI. In the design of this study, however, some important data regarding patient perioperative management, such as intraoperative hemodynamic changes, fluid volume, and use of vasoactive medicines, were evidently missing. It has been shown that intraoperative systolic blood pressure decrease relative to baseline is independently associated with postoperative AKI in patients undergoing CABG.[2] Furthermore, the combination of intraoperative hemodilution anemia and hypotension can synergistically act to increase the risk of AKI after cardiac surgery.[3] Campbell and associates[4] have demonstrated that fluid volume before cardiopulmonary bypass can contribute significantly to intraoperative hemodilution anemia and that restricting fluid volume before cardiopulmonary bypass can attenuate intraoperative hemodilution anemia and decrease the need for transfusion in patients undergoing CABG. In addition, perioperative inotropes, vasopressors, antiarrhythmics, and diuretics may also influence development of AKI after cardiac surgery. We therefore argue that optimizing perioperative management, such as intraoperative avoidance of excess fluid volume, hypotension, and renal arterial vasoconstrictive drugs, should be importantly modifiable factors in decreasing the occurrence of postoperative AKI in patients undergoing CABG. We believe that the results of this study would have been more informative had these factors been taken into account.

Ng and colleagues[1] did not mention the specific timing of postoperative creatinine measurements. It was also unclear whether continuous creatinine measurements were performed. It is therefore difficult to determine whether the cases of AKI reported in this study were due to intraoperative or postoperative factors. Although serum creatinine lags behind acute changes in renal function, AKI (defined by serum creatinine >10 mmol/L greater than normal values)