Prosthetic detachment after aortic valve replacement is one of the most frequent complications requiring reoperation.¹ Many factors, including endocarditis, aortitis, anatomic characteristics, and surgical techniques, are thought to predispose a patient to this complication. However, the intrinsic anatomic factor is considered critical for an adverse event of this kind. The aortic annulus between the middle portion of the right coronary sinus and the middle portion of the noncoronary sinus corresponds to the area above the membranous part of the interventricular septum and the right trigone of the cardiac skeleton. This embryologic origin of the aortic annulus may be a reason for its intrinsic weakness. The base of the noncoronary leaflet does not appear to be embedded in the ventricular muscle and seems to be higher in the aortic root plane.² However, the lack of muscular tissues presumes that this sector has hypodynamic properties, transforming it into a point of anchorage because of the greater dynamic energy developed by the other 2 bases. In addition, the mechanical changes start from the lowest point of each sinus of Valsalva during the cardiac cycle.³ Different dynamic characteristics cause the least expansion of this weak sector. The cooperation of the anatomic, mechanical, and pathologic factors may adversely increase to put stress on the base of the noncoronary leaflet, transforming it into a major site of prosthetic detachment: the dissection of the intrinsic weak annulus into the left ventricular outflow tract and a major flail-like rocking motion of the prosthesis.

Taking into consideration the reconstruction with a valved conduit is helpful because the rigid prosthetic valve does not apply direct pressure to the aortic annulus and the flexible tubular prosthesis may cushion the stress. This modified translocated Bentall technique provides better flexibility and elasticity of the aortic annulus than the standard Bentall procedure.⁴ The technique also has the advantage

of being more hemostatic, because it is easier to place a suture into the cuff of the conduit rather than placing the rigid aortic valve prosthesis on the native annulus. Bioprosthesis of a valved conduit is the better choice of reoperation for aortic prosthetic detachment caused by endocarditis. Moreover, the modified Bentall procedure has often been used to prevent valve detachment in prosthetic detachment caused by aortitis.⁵ Thoracic and cardiovascular surgeons may choose to use this technique.

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SILVER NITRATE: TO DISCOVER ALREADY DISCOVERED...

To the Editor:

We read the article by Stratakos and colleagues¹ with great interest inas-

much as the treatment of bronchial fistulas is of utmost importance to both the pulmonary and thoracic surgery divisions. The exact method of silver nitrate (SN) application through the videobronchoscope shown by the authors is very valuable on one hand. However, on the other hand, it is rather evident for us, and we have treated many patients without publishing our experience owing to its obviousness. We appreciate this work as a well-planned scientific trial to prove the application of SN to heal bronchial fistulas, but its use has been known for longer than the 25 years postulated by the authors. Melfi, Schverlich, and Tambornini² in 1954 used SN for endobronchial treatment of tuberculosis cavities. The same method of treating the opened bronchi was described by Guzeev³ in 1965. Worth mentioning is the toxicity induced by SN, sometimes even demanding lobectomy.⁴

In conclusion, we thank the authors for reminding us to use SN for smaller bronchial fistulas, but let us focus on finding new ways to heal the fistulas or preventing this complication during the operation, rather than reminding us of the old known methods.

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Reply to the Editor:

We appreciate the interest expressed by Kubisa, Grodzki, and Wójcik in our recently published study concerning silver nitrate technique for the management of small bronchopleural fistulas.¹ We are glad to learn that other colleagues as well have experience with this method.

As we noted twice in our study, although this technique was described once, 25 years ago,² nothing has been reported since on the subject, and it is certain that younger generations of pulmonologists and thoracic surgeons alike are not acquainted with it. In an extensive review on the endoscopic management of bronchopleural fistulas published recently as well, nothing more than the above was reported.³

The references mentioned by Kubisa, Grodzki, and Wójcik were all published during the 1950s and 1960s, before the advent of the flexible bronchoscope, and referred mainly to complications of endobronchial tuberculosis, which is a completely different situation (and topic), ideally rare nowadays.

To our knowledge, based on a systematic research of the literature, our report is the first in the modern era to present this safe and effective technique for endoscopic management of such a serious and difficult-to-treat complication.

With regard to complications and safety, we have already described possible casualties in our report,¹ and we are currently working on developing a new bronchoscopic silver nitrate applicator for safer and simpler use with modern flexible bronchoscopes.

We totally agree that much work has to be done in the domains of pre-

vention and new techniques. However, we also believe that revisiting old ideas and techniques (if they are forgotten and continue to be valid) is important and can be of considerable help for clinicians in their daily practice.

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PNEUMONECTOMY AFTER INDUCTION RADIOCHEMOTHERAPY: IS IT TIME FOR A META-ANALYSIS?

To the Editor:

The discussion regarding the safety and feasibility of pneumonectomy for non-small cell lung cancer after induction chemoradiotherapy (IT) is a long-standing, widely debated issue, in particular, in the past 2 decades.

We read with great interest the review from Krasna,¹ in which pneumonectomy after IT is finally judged as a risky procedure (especially if right-sided) that, until further validation (via prospectively gathered data),

should be “used with caution in experienced centers.”

We wish to express our viewpoint and ask the authors about their opinion on the basis of our own long-term experience with IT,² our recently reported results on pneumonectomy after IT,³ and in the light of other recently published authoritative reports.^{4,5} In particular, we have evaluated the outcome-related data of 85 (49 after IT) consecutive standard pneumonectomies in a 14-year period. Operative mortality and morbidity do not seem to be directly associated with IT; besides, among the clinical, surgical, and pathologic features, the right-sided pneumonectomy showed a worse long-term survival in the overall population regardless of the prior application of IT. Substantially, the same old questions still stay unanswered: Is pneumonectomy a feasible and safe procedure after IT? Are there criteria to stratify the correlated risk after IT-pneumonectomy? Do long-term oncologic results justify this treatment?

On top of this, it is impossible not to consider that a complete resection (thus including the pneumonectomy option) must be attempted if a radical chance of cure is sought and the conditions for resectability are met or re-met after IT.

Still, the indication for pneumonectomy after IT is not yet strictly evidence-based, and a prospective approach is difficult to imagine given the substantial impossibility to design a trial with a proper control group: radical resection in NSCLC, in fact, has no ethical comparator alternative. As well, when the concept of “experienced center” is analyzed, the criteria to define it are in doubt? If present, are these criteria validated? What is the referral benchmark of this validation: survival or mortality/morbidity? What is the correct (ethical) approach to the information given to the patient when a pneumonectomy is offered after an IT?

In our opinion, in reality, the “experienced centers” are those in which the