Letter to Hans Georg Borst

Axel Haverich, MD

Dr Haverich

Dear Hans,

Now that it has been six years since you left your office (and the operating room) at the medical school here in Hannover and as you approach your 75th birthday, we know you have become more selective in your reading of medical articles. But I would like to draw your attention to one article, for various reasons. First, this article addresses the (hitherto thought) never-ending controversy about medical versus surgical therapy for acute type B aortic dissections, a question in which you were always extremely interested. Second, it (again) originates from the Stanford group, whose members have been philosophically determined to answer this question for some decades. Third, a (even for surgeons easily understandable) statistical tool called “propensity score analysis” is now used to “upgrade” a retrospective analysis into a prospective study, something you always dreamed of during your active career. Finally, you will be surprised to know that the ultimate answer to the question how to treat aortic type B dissections has been found. However, new technology renders the question partly outdated (of course), posing new questions to keep the gray substance of the next generation of surgeon philosophers active. As both your former resident and current successor, I would desperately like to hear your opinion regarding the paper of Craig Miller’s group.¹

During the first months of my training, you taught me the differences between acute versus chronic and type A versus type B aortic dissections. You always preferred the Stanford classification system for two reasons: (1) its indicative value for the type of surgical approach; (2) its inherent prognostic information. Our policy then and for the next 20 years was the one exercised by most surgical groups, including Stanford: medical treatment for uncomplicated acute type B dissections and surgery for all type A dissections. Rupture and shock, malperfusion of distal organs including paraplegia, and uncontrollable pain were the only indications for surgery in distal dissections. This policy was disseminated to all residents, the medical public in Germany, and, of course, our cardiology partners. When asked to justify this approach, we quoted numbers associated with operative risk compared with medical treatment, focusing on blood pressure control alone: 25% early mortality after surgery compared with 16% with pharmacologic therapy.² The decision to avoid surgery for this indication, however, was also based on tremendous technical difficulties. Problems related to proximal clamping, distal anastomoses, and bleeding control after prolonged pump runs disqualified this operation to probably the least attractive one next to limb amputation during my residency. Ongoing malperfusion, paraplegia, and incomplete replacement of the diseased aorta were some of the early and late complications amplifying our aversion to the operation. In light of an operative mortality rate of 57% in the early experience of the Stanford series, did we really want to see surgical outcome equalling or topping that of medical treatment at that time? We have to keep in mind that deep hypothermic circulatory arrest, a clinical tool revived by Griep and coworkers³ in 1975, was not routine then, and even experienced radiologists were not able to perform catheter-based interventions for malperfusion.

In this context, I find it remarkable that the Stanford group could reduce their operative mortality from 57% 30 years ago to less than 10% over the past 10 years. Of course, many groups may not be able to achieve this degree of excellence, even taking into account that, in this article, only patients *without* malperfusion were included in the surgical treatment group. This selective measure certainly has

From Medizinische Hochschule Hannover, Klinik für Thorax-, Herz- und Gefäßchirurgie, Hannover, Germany.

Received for publication April 4, 2002; accepted for publication July 18, 2002.

Address for reprints: Axel Haverich, MD, Medizinische Hochschule Hannover, Klinik für Thorax-, Herz- und Gefäßchirurgie, Hannover D-30623, Germany (E-mail: haverich@thg.mh-hannover.de).

J Thorac Cardiovasc Surg 2002;124:891-3

Copyright © 2002 by The American Association for Thoracic Surgery

0022-5223/2002 \$35.00+0 12/1/128574

doi:10.1067/mtc.2002.128574

contributed to obtaining better results in this series than in other centers. Furthermore, the introduction of cardiopulmonary bypass and a more aggressive use of profound hypothermic circulatory arrest⁴ in the past decade may explain in part the significant improvements in outcome. Catheter-based interventions for malperfusion have been available in most institutions since the early 1990s. This important adjunct most probably has influenced outcome in both surgically and medically treated patients. Unlike those in the Stanford group, patients with acute type B aortic dissections who presented to other hospitals and who were treated locally according to our recommendations were usually not admitted to our hospital. Therefore, such patients were never included in our studies; the same probably holds true for many other centers. Aortic dissections originating beyond the origin of the left subclavian artery occur more frequently in patients during their 6th or 7th decade of life.⁵ The high degree of age-related comorbidity in these patients made our decision for medical treatment easier. For type A dissections, the mean age usually ranges between 50 and 55 years.

The younger age as well as the dismal outcome of patients with medically treated type A aortic dissections left no doubt that urgent surgical therapy was indicated in these cases. The outcome in the literature shows wide variations, clearly depending on indications and techniques as well as on experience and exposition.

For type B dissections, we believed that surgery should indeed be advocated in some nonemergency cases. You, Hans Georg, developed the elephant trunk technique to facilitate distal aortic replacement after ascending and/or aortic arch surgery.⁶ This idea was born from the dire consequences associated with aortic repair in Marfan patients, who usually required more than one reoperation once aortic dissection had occurred. Palma and associates⁷ invented an elegant and at the same time aggressive and defendant approach, further developing your technique. Via median sternotomy, they introduced the elephant technique (in normal arch anatomy) for patients with acute type B aortic dissection. You were both proud and enthusiastic after your visit to Brazil, and we were ready to introduce the concept. Just as the Stanford group argues today, we believed then that we had patients in our cohort for whom elective surgery was deemed appropriate. On the basis of data regarding late reoperation in Marfan patients after medical treatment,⁸ the Stanford group conclusively advocates early intervention in these and other younger age victims of acute type B dissection.

The method of endovascular stent grafting has been shown to be an attractive alternative in patients with aneurysms of the abdominal and thoracic aorta, especially those with significant comorbidity. The value of this method for dissecting aneurysms is still questionable because our ex-

perience is limited and long-term follow-up data are lacking.

In Hannover, we introduced a surgical technique whereby an aortic arch prosthesis is attached to a stented graft for the descending aorta, placed through the open aortic arch during hypothermic circulatory arrest and antegrade selective cerebral perfusion. This procedure can be performed through a conventional median sternotomy and combines the concepts of the elephant trunk principle and endovascular stenting of descending aortic aneurysms or dissections. Its advantage lies in the fact that the stented graft, unlike a conventional elephant trunk prosthesis, can be securely anchored at the desired level distal to the descending aortic aneurysm, thereby allowing thrombus formation within the space between the graft and the wall of the aneurysm.⁹ Hans Georg, I think we both welcome the idea per se of advocating early repair of the aorta, because our own results both in aortic type A dissection and in type B dissection clearly suggest the high risk of late reoperation in these patients. Such an evaluation, however, has to be stratified according to the underlying disease. Looking at the total series at Stanford, the incidence of reoperation was, in general, not higher in medically treated patients. Future investigations, therefore, have to identify patients at higher risk of aneurysm expansion, for example, connective tissue disorders compared with atherosclerotic aortic disease. Because catheter-based techniques of aortic repair are less invasive, future investigations also have to define the role of this concept in acute type B dissections. Previous reports suggest stent-graft placement to be an alternative in chronic type B dissections; however, data related to the acute stage of the disease remain scarce.¹⁰ Our own experience from 1990 comprises 122 patients, of whom only 14 had acute type B dissection. For many good reasons, therefore, stent-graft placement does not appear to be the ultimate answer in acute type B dissection at this point. High urgency cases based on complications of the disorder have to be clearly separated from elective cases. The role of surgery should increase; better techniques may also provide better results. I would fully concur with Craig Miller's group in demanding prospective randomized and, because of the low number of patients seen in individual institutions, multicenter trials comparing surgery and stenting. Hans Georg, would you also agree with such a proposal?

If one looks at the statistical analysis used in this report, the Holy Grail of medical statistics, prospective randomized trials, have, however, clearly seen better days. The analysis includes 39 patients in a similar pretreatment risk situation compared with medically treated patients. The 39 patients were operated on over 36 years. Surgical mortality decreased from 57% to 10% over the decades; the comparison with 119 medically treated patients has been found to be statistically sound. This secret has a name: propensity score

analysis. Hans Georg, let me briefly explain this to you. You simply perform a multivariate analysis defining risk factors *for the type of intervention*, in this case surgical versus medical treatment. In this study, shock was a strong denominator for surgery. If you exclude patients with significant predisposing factors for either one of the treatment options, you are left with 2 segments (out of 5, named quintiles) of the initial population that would have qualified for both treatment modalities. In this study, the number of medically treated patients is reduced from 158 to 112, the number of surgically treated patients, from 72 to 39 by using propensity score analysis, but the remaining patients being compared are similar in terms of risk of dying. You then compare the data as you would in a prospective randomized trial. This is what I called “upgrade” in my introduction.

Of course, Eugene Blackstone¹¹ has introduced this mathematic method into statistical tools for cardiothoracic surgery. The first two papers to which it was applied were published in the January issue of this *Journal*, both on ischemic mitral valve surgery. I was impressed by the power of this statistical method. But, looking at the current grading system for evidence-based medicine (grade 1 being “expert opinion,” grade 2, “single-center study reports,” grade 3, “multiple single-center reports,” and grade 4, “prospective randomized multicenter trials,” where does this new method find its place? And how does it actually differ from retrospective matched pair analyses? At present, for me as a cardiothoracic surgeon with only a borderline background in medical statistics, the value of the new method remains unclear. Using tremendous computing facilities, it certainly allows for censoring data sets in retrospectively analyzed patient cohorts. This may be especially important in acute type B dissections, where prospectively randomized multicenter trials are extremely difficult to perform. In this case, the method has been helpful to clearly show us that surgical treatment of acute type B aortic dissection is definitely *not* significantly different in outcome compared with medical

treatment. After publication of this important message, we will now start operating on acute type B dissection in all Marfan patients and candidates younger than 60 years. Once prospective trials are organized using durable stent grafts not associated with high endoleak rates, and not wandering with time, and without structural failure at midterm, we would embark on such trials. This is what a landmark paper does to a surgeon. What do *you* think, Hans Georg?

References

1. Umaña JP, Lai DT, Mitchell RS, Moore KA, Rodriguez F, Robbins RC, et al. Is medical therapy still the most optimal treatment strategy for patients with acute type B aortic dissections? *J Thorac Cardiovasc Surg.* 2002;124:896-910.
2. Wheat MW Jr, Harris PD, Malm JR, Kaiser G, Bowman FO Jr, Palmer RF. Acute dissecting aneurysms of the aorta: treatment of results in 64 patients. *J Thorac Cardiovasc Surg.* 1969;58:344-51.
3. Griep RB, Stinson EB, Hollingsworth JF, Buehler D. Prosthetic replacement of the aortic arch. *J Thorac Cardiovasc Surg.* 1975;70:1051-63.
4. Kouchoukos NT, Masetti P, Rokkas CK, Murphy SF, Blackstone EH. Safety and efficacy of hypothermic cardiopulmonary bypass and circulatory arrest for operations on the descending thoracic and thoracoabdominal aorta. *Ann Thorac Surg.* 2001;72:699-707; discussion 707-8.
5. Wheat MW. Acute dissecting aneurysms of the aorta: diagnosis and treatment. *Am Heart J.* 1980;99:373-87.
6. Borst HG, Walterbusch G, Schaps D. Extensive aortic replacement using elephant trunk prosthesis. *Thorac Cardiovasc Surg.* 1983;31:37-40.
7. Palma JH, Almeida DR, Carvalho AC, Andrade JC, Buffolo E. Surgical treatment of acute type B aortic dissection using an endoprosthesis (elephant trunk). *Ann Thorac Surg.* 1997;63:1081-4.
8. Kodolitsch Y von, Raghunath M, Karck M, Haverich A, Nienaber CA. Das Marfan Syndrom: Therapie bei kardiovaskulären Manifestationen. *Z Kardiol.* 1998;14:191-6.
9. Heijmen RH, Deblie IG, Moll FL, Dossche KM, van den Berg JC, Overtom TT, et al. Endovascular stent-grafting for descending thoracic aortic aneurysms. *Eur J Cardiothorac Surg.* 2002;21:5-9.
10. Dake MD, Kato N, Mitchell RS, Semba CP, Razavi MK, Shimono T, et al. Endovascular stent-graft placement for the treatment of acute aortic dissection. *N Engl J Med.* 1999;340:1585-6.
11. Blackstone EH. Comparing apples and oranges. *J Thorac Cardiovasc Surg.* 2002;123:8-15.