Endovascular Treatment of Abdominal Aortic Aneurysm is not better than Conventional Surgery

To the Director

Eventually it seems to establish in the medical community criterion that elective treatment of abdominal aortic aneurysm (AAA) should be preferably carried out in endovascular way. Even in the field of vascular surgeons there is a tendency to let go the traditional approach at the expense of the endograft, which creates a potential fall in the supply of trained surgeons to solve in an "open" way an AAA. So far, the hospital and mid-term outcomes of endovascular treatment seemed to show the advantages of this procedure to prevent aortic rupture and furthermore, to improve survival compared to "open" technique. The less invasiveness in the approach, the passivity with which health insurance systems absorb the costs and the highest economic benefit that physicians, who use this method, collaborated to impose a treatment whose long-term benefits have not been demonstrated yet.

Recently they were published the long term outcomes to 6 years from the EVAR-1 (Endovascular Aneurysm Repair 1) controlled clinical trial, which included 37 hospitals in the UK, in a design with 626 patients per branch, chosen for endovascular treatment with second and third generation prosthesis versus "open" surgery for AAA. In this study, although hospital mortality at 30 days was lower with the endograft (1.8% vs. 4.3%, OR: 0.39, CI 95% from 0.18 to 0.87, p = 0, 02), this initial benefit disappeared at follow-up between 5 and 10 years, mostly due to the occurrence of fatal rupture of the endograft (HR 0.92, 95% CI 0.57 to 1.49, p = 0, 73), while in the same period, complications related to the prothesis and the need of reinterventions were three to four times more frequent with endovascular technique to "open" surgery, as well as incremental costs, which exceeded on average $ 4,500 dollars per patient.

For their part, the EVAR-2 long-term outcomes also bring into question the utility to treat with endograft patients with AAA because of their comorbidities they are not good candidates for "open" surgery. In this study, randomization of 404 patients to endograft or non intervention compared with a design of intention to treat, revealed that after 4 years, mortality relates to aneurysm was 3.6 deaths per 100 person-years in the first group versus 7.3 deaths per 100 person-years in the second one (HR 0.53, CI 95% 0.32 to 0.89, P = 0.02), whereas, there were not differences when all causes of death were considered: 21.0 versus 22.1 deaths per 100 person-years (HR 0.99, CI 95% 0.78 to 1.27, P = 0.97), which indicates that endovascular treatment did not improve long-term survival, since patients died from their comorbidities.

This long-term perspective is a warning about the correct way to analyze the evidence and influence with which the industry sometimes may handle the acceptance of innovations in the medical community. Expected ruptures with long-term endografts, the need to continuously monitor the proper functioning of the latter and reintervention in a substantial number of cases hide the fragilities and the safety of the procedure. And although we are convinced that new prosthesis will overcome these problems, these should demonstrate that overcame the drawbacks of the previous ones and did not create new problems. There is not to forget that medical progress is not always forward. Let's remember or else, the experience of coronary stents with drugs that reduced rates of restenosis, but incorporated the risk of acute thrombosis.

After the EVAR-1 and 2 studies, perhaps patients should know that with endovascular treatment, they have a lower intial risk, but there is a non-negligible possibility that their pathology has not been resolved and that over time it is probable a higher risk mortality. And maybe, covered systems should remember the opportunity cost that they are taking to accept a procedure which has not shown yet that it is better than the "open" surgery.

Drs. Raul A. Borracci[MTSAC], Diego Montero

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Endoluminal Treatment of Aortic Aneurysms. Do the New Studies Change our Clinical Practice?

Response to the Lettes to Editor "Endovascular Treatment of Abdominal Aortic Aneurysm is not better than Conventional Surgery," Drs. Borracci and Montero.

To the Director

Recently long-term outcomes have been published of the Endovascular Aneurysm Repair United Kingdom (EVAR-1 trial) (1) and Dutch Randomized Endovascular Aneurysm Repair (DREAM trial) studies. (2) They evaluated the evolution of a group of patients with an aneurysm of the abdominal aorta underwent at randomized way to conventional or endoluminal treatment of their aneurysm. The outcome of both studies was remarkably similar. Both concluded that although perioperative mortality at 30 days was significantly lower for the endoluminal group [the EVAR study, 1.8% vs. 4.8% (p = 0.02) or 2.3% vs intrahospital. 6% (p = 0.006)], these benefits disappear in the long-term by higher overall mortality and especially that associated with endovascular aneurysm group, as much in the period of 6 months to 4 years as in the period of 4 to 8 years , respectively. Endovascular treatment was also associated with a higher rate of complications

related with the graft, reoperations, and costs.

However, there are two fundamental points of this study that should be taken into account.

According to the UK Small Aneurysmal Trial, (3) patients selected for the EVAR-1 study had to have at least 55mm in aneurysmal diameter. The mean diameter was 64mm in the endovascular group. It has been demonstrated that endoluminal treatment outcome is directly related to the size of the aneurysm (to larger size, higher rate of complications, for example., device migration or endoleaks). Contemporaneously to the EVAR-1 study, Ouriel and et al. at Cleveland Clinic, (4) determined that 24 months after insertion of the graft, patients with an aneurysm whose diameter was greater than 55mm had a higher rate of device migration, endoleak, conversion, a lower survival and higher mortality related to aneurysm when they were compared with smaller aneurysms. Even the own authors of the EVAR-1 study, in a subsequent publication, determined that aneurysm size is a predictor of complications. (5) This would be enough to explain the higher rate of long-term mortality or reintervention of the endovascular group in EVAR-1.

The second important point in this publication is reffered to the used endograft. There is to consider that this study was developed between 1999 and June 2004. Many of these endografts have been modified, both in design and as in materials, in strict relation to complications associated with these "old" devices. Since six years ago, we have "last -generation devices".

In the EVAR-1 study, most of the deaths related to aneurysm occurred in the medium and long term (12 in 599 patients and 10 in 472 patients, respectively) after endoluminal treatment. In our experience (421 elective patients), we have not detected long-term mortality related to aneurysm. Patients that at 2 years after the procedure had not developed endoleaks, they did not present technical complications a posteriori. The only reintervened were those with persistent type II endoleak (from lumbar arteries). These results are consistent with those obtained in other population studies, meta-analysis or series. (6-12) Thus, recommendations led by the group of Cleveland Clinic are guided to a less aggressive follow up protocol when the irradiation and the nephrotoxicity are weighed with the incidence of long-term technical complications.

In conclusion, recent population studies show similar rates of reintervention between both approaches to include complications related to conventional aortic valve replacement, such as hernias, infections, pseudoaneurysms or intestinal obstructions. (6, 7)

We believe that there are many points to consider when talking about evolution in the short, medium and long term. Not just anatomical prescreening of the patient, the endograft used, the experience of the treating team or applied technology are key at the moment of determining outcomes. The EVAR-1 was useful to show that perioperative mortality, actually using older systems, is significantly lower in the endovascular group, even in low surgical risk patients. But the EVAR-1 is a study of patients with high anatomic risk, with the use of previous generation systems. The decision of which approach to use should be individualized. Patients should be well informed about the advantages and disadvantages of both types of procedures.

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Simultaneous Analysis of 2D *Strain*, Coronary Reserve and Wall Contractility during Dipyridamole Stress Echocardiography. Comparative Outcomes.

To the Director

There is not a work in the bibliography which has investigated the 2D *strain* during a stress echocardiographic study with dipyridamole, taking into account not only regional mortality but also coronary

reserve, so I believe that the Lowenstein study and et al.(1) is extremely interesting and original.

Prior to the use of 2D *strain* technique, the diagnosis of coronary disease in a test of urgency was based on the development of myocardial ischemia, it was revealed by echocardiography at impairment of regional thickening, and depended on a purely subjective determination

The incorporation of 2D *strain* allowed assessment of regional myocardial deformation (shortening or elongation) with a more precise analysis of myocardial fiber activity and incorporated as analysis variable the temporality of events (systolic or post-systolic regional shortening).

This technique allows the assessment of myocardial deformation, both in the longitudinal direction, generating a three-dimensional understanding of myocardial deformation, as well as radial and circumferential one. I agree with the authors in the use of longitudinal deformation for the detection of ischemia because it assesses the subendocardial fibers.

In this way we access to a segmentary analysis plotted by most teams as a bull's eye with a proper colour coding that allows us to easily suspect which is the culprit vessel.

The regional contractile status changes affect these measurements and to the extent that it impairs a segment shortening its regional coloration decreases, which changes from deep red to pale orange and even goes to a blue systolic coding (elongation: greater ending lenght in relation to the initial length).

There is more experience of the influence of inotropic agents on myocardial deformation, in so far as few data is found of the use of vasodilators as form of haste.

Sutherland and et al. (2) quantified changes in myocardial deformation with dipyridamole in normal, stunned and infarcted myocardial segments and they concluded that dipyridamole did not produce changes of myocardial deformation of the normal or infarcted segments and there was only a tendency to normalize the deformation of stunned segments.

In the Lowenstein study and et al.(1) was considered an ischemic response to dipyridamole in a reduction of the deformation (shortening "?) or an increase of non more than -5%; one could argue whether enough bibliographic support.

In patients without myocardial ischemia and in contrast to Sutherland and et al (2) segments increased their regional shortening significantly.

With regard to ischemic segments, it is striking how little difference in regional shortening values compared to basal ones (-19% vs. -18.5%), a finding, in my experience and being the anterior descending artery in study, of values of regional shortening very diminished.

In conclusion, I agree with the authors that the use of 2D *strain* is not intended to replace the visual analysis of motility, "there are alternative techniques, but rather complementary."

At this time of echocardiography, unlike other techniques that were presented as promising and did not survive the passing of time, I believe that 2D *strain* comes here to stay.

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Authors' reply

We appreciate Dr. Demian Chejtman's valuable comments on our study. It is true that there is certain reluctance to believe that dipyridamole is capable of producing contractile alterations and changes in ventricular deformation in response to an increase or a decrease in coronary flow and George Sutherland was always the upholder of this theory.

The study to which Dr. Chejtman makes mention was carried out experimentally in pigs and its results cannot be extrapolated to the clinical arena, because Sutherland, although uses the same dose of dipyridamole than us (0.84 mg/kg/4 min), he compares it with dobutamine but not randomly, but always after inotropic (for the long half-life of the vasodilator), which can reduce the response of dipyridamole by a phenomenon of preconditioning. It is not less important that he only evaluated the strain and strain radial rate exclusively at circumflex artery territory, while our study was based on longitudinal deformation (more precocious and more sensitive than radial deformation to assess ischemia) and the territory of anterior descending artery (ADA) in patients with different involved coronary territories.

The presented data in our study suggests that if coronary reserve is normal, the hyperemia is induced by dipyridamole increases myocardial deformation evaluated by longitudinal 2D *strain* in the ADA-dependent segments. If this is abnormal, vasodilator stimulation does not induce more deformation and is marked a perfusion defect.

In another study at our institution with 136 patients, we returned to confirm that the apical 2D *strain* follows the same trend of coronary reserve in the territory of the ADA during dipyridamole stress echocardiography. (1)

Statistically we have found (without previous bibliography) that the best cut to determine regional ischemia resulted in a lack of increase in the deformation or an increase less than 5%.

Finally, although there is isolated data about that coronary patients have at basal condition, a regional and global deformation lower than the healthy population, in our experience we have not yet been confirmed it in

segments with normal resting motility.

We understand that the main merit of speckle tracking is to allow the incorporation of the concept of deformation in the usual practice of echocardiography, based on the simplicity of its precurance and on the strength of its outcomes. Thus, in our laboratory is used systematically in stress echocardiographic studies and is applied to the analysis of synchronization, valvular heart diseases and cardiomyopathies.

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Assessment of Inappropiate Coronary Revascularization in a Highly Complex Cardiovascular Center

To the Director

I have read with great interest the study published by Albertal and et al. (1) In first place, I want to congratulate the authors for the initiative to assess the quality of the information and forms of revascularization carried out in their routine practice. Such type of periodic evaluations allow us to implement continuous improvements that help the patient and strengthen the institution. This publication is also important for understanding the community and as an example in using the RAM method (RAND / UCLA Appropriateness Method), which is little used in our environment. In the RAM method, (2, 3) the term "*appropriate*" refers to the relative weight of benefits and harms of medical or surgical interventions.

In order to enrich the discussion on this subject, I want to make some comments about the study. In first place, it is necessary to clarify that the RAM method is not applied as it were described, but changes have been made. For example, the simplification of the scenarios, since that more than 150 scenarios originally described, only 11 were evaluated. This reduction is produced partly because, as the authors say, they collapse in a group the adequate indications plus the doubtful ones. This approach incorporates the doubtful indications as adequate or at least inadequate. The RAM method refers that these uncertainties mean the need for new assessments of the patient or new research. It should not be defined a percentage of inadequate behaviours without reclassifing or excluding doubtful indications.

In this study is also incorporated the SYNTAX score (4) to analyze the modality of treatment in patients with multiple vessels. Even though, it is reasonable and interesting its inclusion, it should be considered that this score is not part of the RAM method.

For all these reasons, I consider that the outcomes observed in the study do not necessarily respond to appropriate or inappropriate indications according to the RAM method.

Recently modifications to the method have been proposed in the Council of Emergency "*Rafael Bullrich" Sociedad Argentina de Cardiologìa* (personal communication). In this case, a group of experts evaluated scenarios of acute myocardial infarction analyzing indications for coronariography and revascularization.

Finally, when comparing the outcomes with previous population studies (5) that have used similar methods of analysis, it is observed that the published data is better than the mean observed in the bibliography. This conclusion may be showing excellence in patient care, although it may also be reduced percentage of inadequacy by the collapse of doubyful qualifications together with the appropriate ones, or by changes introduced to the method in the assessment of indications. New researches should be carried out about the importance of the SYNTAX score in the adequacy of the indications to weigh out its added value.

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Surgery for Isolated Aortic Valve Replacement in Octogenarians: Evaluation, Operative Risk and Mid-Term Outcomes.

To the Director

Degenerative aortic disease is one of the most common causes of cardiovascular consultation in the elderly

population. Given the characteristics of this population beyond the traditional indications for valve replacement (symptoms, ventricular dysfunction, etc.), the inherent conditions to the patient (comorbidity, fragility) and to the physician (conviction of the procedure results) are critical in the time of decision- making.

The Piccinini study and et al. (1) based on local experience of a surgical experienced team allows us to take out interesting conclusions suitable for our environment: that age per se is not a condition to exclude elderly patients from an intervention, that the risk scores have a limited effectiveness in decision-making to undergo surgery or not a patient and that the surgeon's assessment together with the clinician is a needful synergism for this decision.

Likewise the Piccinini study and et al., (1) other series (2, 3) showed similar outcomes regarding postoperative morbi-mortality, as well as for improving the quality of life in this subgroup of patients: 82% with preserved ventricular function, only 11% with coronary history, predominantly women and mean logistic EuroSCORE of 9% ± 2.5%.

However, there are not controlled studies which assessment the outcomes between subjects undergoing surgery and non undergoing surgical subjects in relation to the preoperative risk.

Therefore, the publications are based on centers of much expertise, great volumes and liable of inclusion bias (the most "qualified") to express their outcomes.

Moreover, the approach of new surgical techniques, such as percutaneous valve implant, opens an interesting skyline management in this population with high comorbidity.

In the PARTNER study (4) (mean age 83 years, 55% women, 92% in FC III-IV, logistic EuroSCORE 28% ± 16%, 70% with coronary background), the events of the population with standard therapy were high (58% symptomatic at one year and an annual survival of 42%). Even though this population differs significantly from the Piccinini study and et al. (1), since it occupies one end of the group of octogenarian patients with aortic stenosis, its prevalence is higher each time.

With the increasing development and experience of the percutaneous technique and in the light of the results of the PARTNER study, (4) medical groups that assist this population have the obligation to define more properly the truly high-risk patient who is benefited with one or another surgery .

Clinicians, surgeons and cardiologists specialising in cardiac hemodynamics together, considering the favourable outcomes of the Piccinini study and et al. and similar series, we should be critical and prudent in decision-making, taking into account risk scores of the logistic EuroSCORE (overestimate) (5) and the Society of Thoracic Surgeons (underestimate) (6) they have limitations and do not consider certain patient's characteristics that are potentially relevant [calcified aorta ("porcelain"), "fragility", ...]. But on the other hand, we should also consider that in percutaneous implant still remains for us to know its durability (there are only communications up to 3 years of follow-up) and the potential complications inherent to this technique (aortic reflux incidence, reintervention).

It is in this context that the process of indication falls considerably in the synergism of the entire medical team (surgeons, clinical cardiologists and cardiologists specialising in cardiac hemodynamics), which should assess which one is the best candidate to be undergone surgery and which is the best technique to use in individual patients.

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Accelerated Ventricular Rhythm

To the Director

I have read with interest the article "Accelerated Idioventricular Rhythm. Chronology of its History", Perez Riera and et al. (1) since it is very connected with a study of my authorship, "Accelerated Nodal Pacemaker", (2) I want to make a brief comment.

In his study, Pérez Riera and et al. mention that the accelerated idioventricular rhythm (AIVR) is present in children without apparent cause, which coincides with my experience. They say, however, perhaps referring to adults, that it may happen with ischemia or in secondary way to digitalis toxication.

In my study in pediatric patients, the subsidiary pacemaker acceleration (such as nodal origin or the A-V or ventricular junction) are related to the following entities:

1. Digitalis excess, usually associated with hypokalemia.
2. Acute carditis, especially caused by rheumatic fever.
3. Post-surgical immediate period, especially when working on the interventricular *septum*.

An arrhythmia occurs in a non-paroxysmal way and is associated with A-V dissociation with catches if there is absence of added A-V block.

In those caused by surgery, it may be confused with blocking dissociation and falsely indicate a pacemaker. This may be avoided if it is observed that the heart rate is above 80 per minute and because there are catches.

In the case of carditis, this arrhythmia is detected very early. It was the fact that in our emergency room of Cook County Children's Hospital in Chicago, United States, these patients underwent an inmediate electrocardiogram that we could notice this arrhythmia.

Pick and Langendorf (3) had already observed in similar cases the existence of the association of acceleration and pacemaker lock. So much so that in these rheumatic carditis when tachycardia ceded it was "discovered" the long P-R which older books described as characteristic of this disease. In fact it was (the first-degree A-V block) the final stage of this story.

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ERRATA

Due to an unintentional error, Hernán Padilla, M.D. from the Department of Cardiovascular Surgery at Hospital Fernández, was not included as one of the authors of the clinical case "*Placement of an Implantable Cardioverter Defibrillator through a Transatrial Approach*", published in Vol. 78, Nº 6 of our *Argentine Journal of Cardiology*.