Surgical Treatment of Acute Type A Aortic Dissection: In-Hospital Outcomes and Long-Term Follow-Up

To the Director

It is indeed interesting to get acquainted of local outcomes for the treatment of such a complex condition as the acute type A aortic dissection. For that reason, we congratulate Dr. Spampinato et al. for their effort, and for publishing their work. (1)

The treatment for this condition is a point of interest for our Department; we have already conducted an analysis about both in-hospital and follow-up predictors, which we would like to share as a contribution.

Our group retrospectively analyzed the risk factors for in-hospital mortality on 123 patients treated for TA-AAD between 1995 and 2003. Then we prospectively evaluated mortality in the follow-up, and its predictors, on a series of 155 patients, with a median follow-up of 5.3 years.

Both studies were presented at the Argentine Congress of Cardiology in 2004 and 2006.

Our population is very similar to that presented in Spampinato et al.'s study: mean age was 62 ± 11 years; 20% were over 70, and the proportion of diabetic and hypertensive patients was equal.

On hospital admittance, 26% were in shock; this contrasts with the authors' experience, in which only 6% were in shock. In the International Registry of Acute Aortic Dissection, this figure reaches 16%, so the low percentage reported by the authors is surprising. (2)

Our in-hospital mortality rate was 34.5%, including 7 patients who died before undergoing surgery. The incidence for postoperative complications was slightly lower: postoperative neurologic incidence was 10.5% (13/123), low cardiac output syndrome (LCOS) was 15.4% (19/123), infection was 20.3% (25/123). 40% of the patients had renal failure.

Incidence of reoperation due to bleeding was 13% (16/123). This piece of information, which is relevant in any presentation of surgical series, is not included in the work, and it would be interesting to know it. The reason is that when we identify variables associated with mortality, admission in shock, reoperation due to bleeding, sepsis, and LCOS are detected as independent variables.

In contrast, the IRAD risk score (3) does not include postoperative variables, and we consider it relevant to know what influence it could have on mortality.

In another study, we conducted a follow-up on 155 patients, which was 98.7% completed.

In this series, 24.5% of the patients died, most of them due to a related cause (18.7% of the total). 20% of the patients developed aneurysma, 6% had aortic rupture, and 20% required reoperation, most of them due to valve dysfunction.

Independent mortality predictors in our follow-up series included the development of new aneurysm, reoperations, and rupture. No significant associations with age or perfusion times were found.

Our figures are clearly higher than those reported by the authors.

We ask ourselves up to what extent losing 11% of patients during follow-up could have contributed to that low percentage, or if our postoperative imaging follow-up should have been more intensive in order to prevent ruptures or perform earlier reinterventions.

In the discussion, the authors attribute higher mortality rate –compared to some international series– to the older-aged population and to diabetes. This could be reasonable from the logical but not from the statistical point of view, because the age CI 95% includes the median of the series cited by the authors, which indicates that populations are not necessarily different. On the other hand, neither age nor diabetes were independent predictors of mortality among their patients.

Alberto Domenech, MD[MTSAC, FACC]

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Authors' reply

The contribution of Dr. Alberto Domenech's group is interesting; we appreciate their interest in our work, published by our Department.

First thing to point out is the similarity between the populations studied. The difference regarding admission in shock could be in part due to the fact that patients who died on admittance before surgery were not included in our work, because its aim was to evaluate the outcomes of surgery for TA-AAD, and not the progress associated with the condition itself. In their work, Domenech et al. report an in-hospital mortality rate of 34.5%, including 7 patients who died before surgery; among these patients, there was probably a high percentage of unstable patients, in shock –among other instability variables associated with higher in-hospital mortality rates. (1) Excluding these

patients, the group of Domenech et al. probably has a rate lower than 26%, closer to the one published, between 12% (2) and 16%. (1) However, our rate of 6.35% is still low, and it could be explained by the fact that, in our work, 87% underwent early surgery (< 24 hours), and this complication had no time to develop. In the work by Trimarchi et al., the average intervention interval since the onset of symptoms was 79.3 for patients in shock before surgery. However, this was not particularly analyzed in our work, and it could be due to a bias, since it was a retrospective analysis.

Intrahospital reoperation incidence in our work was 9.7% (6 out of 63), slightly lower than the one reported by the group of Domenech et al. In the International Registry of Acute Aortic Dissection (1), it was 12.8%; in the work by Martín et al., it was 9% (9/98), and despite it was not predictor of in-hospital mortality, 6 out of 9 patients died.

Domenech et al. obtained similar results in the follow-up, except for the need of late reoperation, which was higher than the one reported in our work (20% versus 5.4%), Undoubtedly, the loss of 11% of the patients in our follow-up is a bias to be considered. However, mortality for cardiovascular reasons (21.6% versus 18.7%) and aortic rupture (8% versus 6%) were similar in both works. On the other hand, periodical imaging evaluation can identify aneurysms likely to rupture, or valve dysfunction, and result in early reintervention, probably with lower mortality.

In the discussion, the differences of mortality rate were not directly associated with or attributed to the age of the population. While confidence intervals include the averages, in theory –and taking into account that age is always a predictor of mortality– it seemed appropriate to explain the mortality variability reported in the different series (from 15% to 35%). Only one contrastive analysis of the different populations, including the one in our series, was conducted. In the work by Rampoldi et al., (3) the mean ages between survivors and deceased patients were 59 ± 13.5 versus 62.9 ± 14.3 years respectively ($p < 0.01$); these intervals were similar to those reported in our discussion, and it is an interesting point about which to put forward at least one hypotesis.

In closing, it is really necessary to have a multicenter registry to pose many of these questions.

Ricardo A. Spampinato Torcivia, MD,
Hernán Cohen Arazi, MD MTSAC

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2007 National Registry of Admissions due to Heart Failure

Torniamo all antico e sará un progresso.
Giusseppe Verdi

The article written by Fairman, et al (1) about admissions due to heart failure (HF) deserves my following comments.

60% of the patients had systolic dysfunction. 40% of them had AF. After 90 days, readmittance was of 24.5%.

Now, the DIG study demonstrated that digitalis did not increase survival but reduced hospitalizations. I cannot understand the idea that hospitalization reductions do not imply pathology deterioration; but today, other studies demonstrate that if high doses are avoided mortality due to HF decreases (2), and that digoxin is indicated not only in systolic but also in diastolic heart failure, (3) particularly if HF is present, as it is usually the case.

Why were those patients not administered digitalis? Why was only 52% of them administered loop diuretics? Why was only 47% of them administered ACE inhibitor? Is it not related to the fact that 70% of the patients are readmitted due to systemic or pulmonary congestion (with 18% of COPD)?

And even worse, if what we are trying to do is reduce admissions, why was only 30% discharged with digoxin when the DIG and RADIANCE studies –among others– showed that it is effective for that purpose?

Digitalis intoxication is on the verge of extinction, and reducing its use results in serious cost-effective consequences for the elder population. (4) In these cases, would it not be appropriate to go back to what is traditional?

Despite most Argentine cardiologists do not seem to notice it, the guides still recommend digoxin and diuretics for HF and AF.

Dr. Fernando J. Peliche, MD MTSAC
Río Gallegos

P.S.: Of course, this criticism does not apply to the authors –who tell us what they've found– but to the HF management on the part of the cardiologists.

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Authors' reply

We thank Dr. Peliche for his interest in our work.

As commented in his postscript, this survey reflects the reality of how the centers linked to the Argentine Society of Cardiology (SAC) work, but it does not imply that patients included are wrongly or insufficiently medicated. As it is well known, registries include an old-aged population, with greater proportion of women and higher incidence of cormobidities. This entails the fact that the best possible medication is far from the ideal one. However, we agree; it is a warning as to how we handle our patients in the outpatient practice.

Indication for digoxin may vary. On the one hand, the evidence that sustains almost all the indication emerged from a study (DIG) (1) where the rate of use of beta-blockers is inconsiderable, given the time in which the study was carried out. On the other hand, the use of digoxin was the necessary inclusion criteria in the first clinical trials on enalapril, and the current rate of use of digoxin is below 40-45%, even in interventional studies exclusively with systolic dysfunction. (2) In his editorial about the DIG study (3), Packer predicted a reduction in the use of digoxin would be expected as a result of the advent of new drugs with additional effects on mortality (such is the case of beta-blockers).

Moreover, it is true that the use of digoxin in the said DIG study was associated to a lower rate of admission, and that earlier studies (PROVED and RADIANCE) show that stopping digoxin is associated to a higher rate of decompensation.

According to current data, we understand that it is provoking to attribute to the low indication of digoxin certain responsibility in decompensation, although its relevance is uncertain.

Due to the rooting use of digoxin, it is improbable that its low indication was the result of lack of knowledge, but rather a consequence of an active attitude of not prescribing.

We understand the valuable letter from Dr. Peliche as a stimulous in order that those of us who manage patients with heart failure can question ourselves the therapeutic decision for each situation in an active way.

Enrique Fairman, MD [MTSAC], **Jorge Thierer, MD** [MTSAC], **Leandro Rodríguez, MD**

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Inflammatory Activity in Multiple Atherosclerotic Plaques of Patients Dying of Acute Myocardial Infarction

To the Director

The work of Dr. Ricardo Sarmiento, et al., published in N° 2 Volume 77 of our *Revista* (1) is a valuable contribution of information on both the physiopathological and clinical aspects of atherosclerosis, and it is highly topical, since cardiologists –those specializing in physiology or in clinical or interventional areas– focus on this issue.

By studying 58 patients who died of acute myocardial infarction, they did the kind of excellent and in-depth analysis of coronary histopathology and lesions; we are used to this kind of analysis by this group of researchers, from the books we studied with, (2) written by our dear teacher Dr. Carlos Bertolasi. The following conclusions are drawn:

1. Thrombosis and plaque accidents were more frequent in arteries responsible for the infarction.
2. The inflammatory process was present both in arteries responsible for the event and in those uninvolved.

The outstanding point is that between 40% and 50% of the arteries uninvolved in infarction showed some degree of complication, and that the inflammatory process is diffuse.

For a long time, several authors –particularly some from Argentina– have defended the concept of "inflammated" or "vulnerable patients" which results from a concatenation of processes from the plaque –with the same adjectives– to the vessel, to the blood, and finally to the subject. (3) This warns us about the fact that we are not treating a focal process but a systemic disease, both the atherosclerosis and the events that cause it. For this reason, the therapy should take all these factors into account when treating these acute patients, and not just revascularization.

We should deactivate the inflammatory process caused by risk factors and its most feared consequences, such as hemorheologic changes, and changes in neuro-hormonal regulation to get optimal therapeutic results through a more holistic approach.

All those who work in diagnostic imaging related to atherosclerosis notice that atherosclerotic plaques and vascular disease are somehow non-homogeneous, and that they have some sort of territoriality; this study takes the credit of capturing it with the gold standard: the anatomic pathology.

Finally, I agree with the authors in that it would be very interesting to analyze the material with the immunohistochemical techniques to differentiate immune cell lineage. In this way, we might recognize if all the lesions are in the same inflammatory stage, and improve the way to determine these patterns and the mechanisms involved. I hope the authors will be able to perform it if they still have the material available.

Considering this work in the context of the bibliography, and being aware of the limitations in our sphere, this publication dignifies our cardiology, and constitutes an outstanding element for learning about physiopathology of atherosclerosis.

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Estimation of the supply and demand for cardiologists in Argentina

To the Director

Argentina's society has little –and not very reliable– statistical resources. For instance, we all know the current problems at INDEC (National Statistics and Census Bureau); this situation generates distrust and uncertainty when it comes to different epidemiologic diagnoses, and of course the conclusions or solutions arrived at are also affected. I would like to point out the significance of the issue dealt with in the article *"Estimation of the Supply and Demand for Cardiologists in Argentina"*, by Dr. Raúl A. Borracci, et al.; (1) it would be extremely important to have a situational diagnosis about the need of specialists in different medical areas –not only in cardiology, where the number seems to be excessive, but also in areas where there seems to be a deficit in professional training , as in the case of family doctors. This study estimated the number of cardiologists in Argentina, and its relationship with the needs of the population, according to a model of demand. The number of cardiologists per million inhabitants seems to far surpass the indexes obtained and recommended in Europe and in the United States. The same surplus is present in the individual analysis of each province, and in the number of new in-training cardiologists, according to the authors' conclusions. I want to point out the case of Tierra del Fuego, the province where I work as cardiologist. It has been estimated that there are 412 cardiologists per million inhabitants, for a recommended of 39 cardiologists, a number that places this province in the third place with an excessive number of cardiologists nationwide. The authors remarked that their work design might be limited, but since the result was so conclusive, they would not bias it. It is highly probable that this is the situation in the rest of the country, but in the case of Tierra del Fuego, the margin of error maybe excessive, since the real number of cardiologists on active duty for an estimate of 120,000 inhabitants was 13 for the same period covered in the study. I am sure of the authors' good faith, but I am doubtful about the accuracy of the registries used to set up the database for Tierra del Fuego. Sometimes, being too confident with registries that should be correct and accurate –but they are not– may lead us to wrong or exaggerated conclusions on the topic in question. I am intending to collaborate on such an important issue, and contribute with information that helps analyze the supply and demand for cardiologists in Argentina, and in Tierra del Fuego in particular.

Dr. Ignacio J. Grané
Cardiologist

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Authors' reply

We appreciate Dr. Ignacio J. Grané's interest on this issue, and his contribution, in which he points out the error in the number of cardiologists on active duty in Tierra del Fuego. The number of cardiologists determined in our study corresponded to the fellow members in the SAC register (23 fellow physicians, most of them from Ushuaia, Río Grande, and Tolhuin), and to an estimate of physicians who perform as cardiologists but are not registered, based on a population of 111,736 inhabitants. The new calculation using the data provided by Dr. Grané would be of 116 cardiologists per million inhabitants, for an optimal of 39 professionals; it would place this province with an excess of cardiologists of 197%, similar to that of the province of Jujuy.

Contributions like this one from Dr. Grané are indispensable to organize the information of the country about specialty resources, and are also an example for others to follow in this collaborative effort.

Raúl A. Borracci, MD MTSAC,
by the authors

Time delays in Performing Primary Angioplasty in Patiens Transferred with Acute Myocardial Infarction: a Health Care Issue

To the Director

The work presented by Dr. García Escudero, et al. (1) about the time delays in referring patients with acute myocardial infarction for primary angioplasty is very interesting, since it is currently a key topic for debate in all international scientific fora related to cardiology, public health and implementation of health care policies, due to the prognostic impact of reperfusion times on ST-segment elevation acute myocardial infarction (STEMI). In the United States, a program called D2B Alliance (Door to Balloon) has been created; it includes the American College of Cardiology (ACC), the American Heart Association (AHA), and the National Heart, Lung, and Blood Institute, as great partners in the planning for reducing times to primary angioplasty. During delays to percutaneous reperfusion, many difficulties are encountered, including geographical, weather, method-availability, and health care difficulties. The city of Buenos Aires does not encounter geographical, weather, or availability difficulties; however, it does encounter health care difficulties, which are dealt with in this study.

It should be mentioned that it is valuable that this study has been carried out in a public hospital of Buenos Aires, and that this topic –which is key at international level– is also focus for analysis to improve healthcare for the lower-income population.

The authors divide the periods into different stages, separating 'consultation time' –which they call 'patient time'– as opposed to 'health care time', which is the delay from the moment the patient makes a consultation in the health care system to the first ballon inflation in the culprit vessel.

What calls the attention –and it is also mentioned by the authors– is that the reported patient time is 90 minutes, when the time reported in international studies is about 180 minutes (2) and the 2005 SAC survey (3) reports a consultation delay of 240 minutes.

This is probably due to the fact that the study focuses only on patients who were referred to primary or rescue angioplasty. There is another group of patients who might have been treated with fibrinolytics in consultation centers. And there is one more question –and concern– about the number of patients we are not informed of, who consulted too late or did not have an adequate diagnosis, thus not having the chance to reperfusion.

As regards medical care time, there are different stages to be analyzed: ambulance transport time of 31 minutes is appropriate, and correlates with other studies published, and admittance in hemodynamics room for first inflated balloon time of 35 minutes is also appropriate and correlated, and varies according to the anatomic/technical difficulty in each patient. However, times related to diagnosis and to the therapeutic decision and its implementation are responsible for the extended medical care time of 200 minutes, that is, almost 3 hours and a half, which mean loss of viable myocardium.

To have an impact on these points, the project of a door-balloon program is under development, started up by the Ministry of Health of the City of Buenos Aires and the Cardiology and Hemodynamics departments of the city hospitals; based on the model implemented in Denmark after the DANAMI 2 study results, (4) it seeks to optimize the times from initial consultation to reperfusion, by means of early diagnosis through outpatient ECGs with telemetry reading and selective referral of STEMI patients to centers performing primary angioplasty. In this way, delays in diagnosis and in reperfusion are eliminated.

Pablo E. Pérez Baliño, MD,
Néstor A. Pérez Baliño, MD MTSAC

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Heart Rate Contribution in Soccer Players During the Competence Period

To the Director

I would like to comment on the very good work by Dr. Motta and Dr. Angelino, published in the *Revista* that you run.

To my mind, Dr. Domingo A. Motta and Dr. Arnaldo A. Angelino have published a very important work related to sports and the heart: *"Heart Rate Contribution in Soccer Players During the Competence Period"*. (1) In this work, the authors analyze the cardiovascular system reaction to competitive effort in 108 young soccer players aged 17 on average. While

they focus mainly on heart rate (HR) behavior, they include other very important variables like oxygen consumption, and lactate production, which increases when the aerobic regime is exceeded and it becomes an anaerobic physiology.

I believe that sophisticated and complete studies like this work are the best and most objective way to analyze any sports practice.

In 1978, the Division of Cardiology of the *Hospital de Niños "Dr. Ricardo Gutiérrez"* studied the relation between HR and the duration of the effort until exhaustion in children from different age groups under the age of 15, and a group of phyisically trained children, in order to have them as a reference to be compared with those with different congenital heart defects, operated or not. (2) Subsequently, and up to present time, other similar elements have been added to those included in the work we mentioned above.

The authors demonstrate and arrive at the conclusion that the methodology used allows to evaluate training and physical adaptation of the studied young soccer players.

Based on the experience of our group initiated by Dr. Marta López, Dr. G. G. Berri, Dr. H. Faella, (3) and at present by Dr. Inés Abella and Dr. A. Leveroni, (4) I would like to express my concern, which could be studied by the group of the present work; it has to do with the aptitudes in those considered remarkable athlets in all kind of competitive sports. Undoubtfully, in addition to their natural technical skills, they have nervous mechanisms that allow them to bring down their opponents in a fraction of a second –a thousandth of a second, I would say. That is for a different chapter, and it deserves another kind of study.

What concerns me is how to detect these super champions' natural capacity objectively, with respect to metabolic and cardiovascular adaptation changes with the studies (or others) the authors propose. I was thinking that it could be detected through the study of a curve and the point that crosses it at the moment in which the aerobic mechanism becomes anaerobic. I think that it will place higher for the best athletes than for the majority of the not-so-talented athlets.

Once again, I would like to mention that this work has also motivated me to write these insights and recommend that an emphasis be made on these studies in any other route of investigation, so as to predict objectively who the athlete standing out over the rest will be.

Alberto Rodríguez Coronel[MD]
Ex Chief of the Division of Cardiology at *Hospital de Niños*

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Authors' reply

First of all, we would like to thank Dr. Alberto Rodríguez Coronel for his highly favorable words, as well as for his constant spirit of research and his remarkable contributions based on his experience.

We consider that measuring direct oxygen consumption and respiratory equivalents for O_2 and CO_2 to determine the anaerobic threshold (AT) –like aerobic-anaerobic transition area– still constitutes the selective study for high-performance athletes.

High maximal AT and oxygen consumption allows to identify greater capacity to maintain exercise mainly in aerobic regime and in economy of effort regime in the most talented athlets. In addition, monitoring the heart rate, time, and AT speed is a specific training indicator to increase it.

We believe each athlete has his/her own submaximal and maximal heart rate rhythms, and that considering heart rate in intervals of percentages of theoretical maximum heart rate loses specificity as training and adaptation indicator.

In addition, measuring heart rate in the cross of aerobic-anaerobic transition (AT) –as our reader proposed– would be most suitable in order to be more precise when being used in training and in evaluating the adaptation to metabolic and cardiopulmonary resistance.

In the ergometric tests with indirect oxygen consumption performed in our work, the constant curve registration of the heart rate allowed us to observe its usefulnesss. If we consider it regarding the loss of its linearity with the gradual and progressive increase of the intensity of the effort –submaximal heart rate–, the changes observed on displacement speed, times and distances during the competitive cycle allowed us to observe its better adaptation in cardiopulmonary resistance.

We have found no fixed absolute or relative values of submaximal and maximal heart rate intensities, predictors of higher performance in cardiopulmonary and metabolic resistance, isolated from other indicators.

This being all, we appreciate Dr. Rodríguez Coronel's contributions, and we hope to have met the expectations arised from our work.

Domingo A. Motta, MD,
Arnaldo A. Angelino, MD [MTSAC]

Atrial contraction recovery after left MAZE III surgery

To the Director

I wish to congratulate Dr. Matías Tinetti et al. on their study about MAZE III for AF surgery associated with cardiac surgery. (1) Although the number does not seem to be large, it is a very valuable experience in our sphere.

I have a question about the surgical technique utilized, because it implies additional opening and exposure of the left atrium in aortic and coronary patients, and this may be demanding for the surgeon when the atrial volume is low. Is this a reason for having included only one coronary patient in the analysis group?

In the editorial, Dr. Carlos Labadet points out that outcomes are encouraging, with 87% in sinus rhythm and 80% in mechanical function. (2) However, in the discussion the authors state that mechanical function recovery was low.

Follow-up protocol set controls at one, three, six, and twelve months. However, follow-up average was lower (9 months), and the authors mention that atrial mechanical function was 80% after follow-up. The graphic in Figure 2 does not show the last mandatory control to meet the protocol requirements. If mechanical contraction and rhythm curves are analyzed, it is clear that the outcome is inadequate at one month, but then the curves rise at three months; values decrease at six months and then increase at the end of the registry. Example: at 9 months, 87% in sinus rhythm versus 69% in mechanical function. If the final outcomes are added, the result is 87% versus 80%. There seems to be no statistical significance, especially for the last difference*. This lends weight to the hypothesis that, in fact, rhythm and mechanical function coincide with the passing of time, so the conclusions arrived at in this work are valid only for the early stage of the follow-up.

It seems to me that Tissue Doppler technology does not have statistical weight in this study in order to recommend its systematic use in MAZE procedure. Its use would be limited to clinical research, and to those few patients at very high risk of continuing with oral anticoagulation treatment. It is evident that ablation surgery is widely justified in spite of its cost, since it does not worsen morbidity, and it was very useful for rhythm and mechanical function management at late follow-up.

A fact about nomenclature –perhaps not very important– is that the name MAZE III was used for the last version of the purely surgical procedure (in English, "cut and sew") and was replaced by the name MAZE IV (Washington University) when radiofrequency or microwave ablation, cryoablation, or similar methods, were utilized. (3, 4)

* P values (chi-square): difference 1: 87% versus 69%: $p = 0.19$; difference 2: 87% versus 80%: $p = 0.75$.

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Does severity of preoperative symptoms predict operative risk in mitral regurgitation?

To the Director

The work of Vaccarino et al. published in the *Revista Argentina de Cardiología* highlights the clinical-prognostic value of dyspnea in the intraoperative and late evolution of patients with mitral regurgitation. (1) The group of patients with severe mitral regurgitation associated with left ventricle systolic dysfunction should be referred to surgical treatment, mainly when the surgery outcomes match international standards. (2) Asymptomatic patients with intermediate parameters of systolic function and high feasibilitiy of valve repair constitute a special group that can be surgically treated in specialized centers (class II). Beyond the work limitations related to the retrospective design and the variables posed by dyspnea (which is clearly discussed by the authors), it is essential to highlight the importance of exhaustive clinical monitorization of symptoms, and to avoid –as far as possible– the delay of interventions in symptomatic patients. This aspect –as detailed in this work and other pioneer works– is related to the outcomes. (1, 3, 4) In this regard, no randomized studies showing us that early surgery is better are available, despite more symptomatic patients have worse outcomes. However, success in valve repair and reduction in perioperative mortality make early surgery more attractive. Com-

Falta traducción de texto en la Fig.

pared to the publications mentioned, (3, 4) the work of Vaccarino et al. shows lower rate of repair and higher perioperative mortality, but with higher late survival. It would be important to know if follow-up for 100% of the patients was achieved, and what the preoperative repair expectation was for group 1 (since if the valve cannot be repaired, one should be cautious of operating asymptomatic patients). I congratulate the authors, and highlight the importance of having the outcomes from Argentine surgery centers available, in order to weight up the risks of surgery.

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Authors' reply

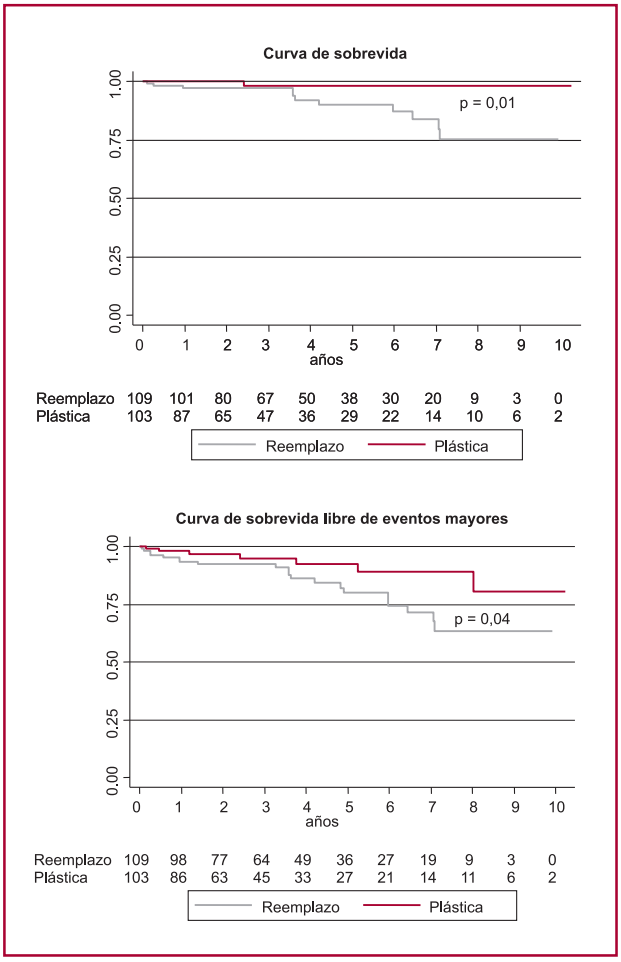
In response to Dr. Sergio J. Baratta's letter about our article *"Does severity of preoperative symptoms predict operative risk in mitral regurgitation?"*, the optimal time to indicate surgery for severe mitral regurgitation on asymptomatic patients is controversial, and the American College of Cardiology/American Heart Association guidelines recommend early indication in highly specialized surgical centers. The conservative strategy with medical treatment and intensive cardiac follow-up is recommended for selected patients, paying special attention to signs of ventricular dysfunction, atrial fibrillation, and/or pulmonary hypertension. (1)

In our country, there are no randomized or comparison trials of these two strategies for the management of asymptomatic patients in order to draw conclusions.

In 1996, Ling et al. (2) reported the 10 years follow-up of 229 patients with severe mitral regurgitation due to flail leaflet. In this observational study, 45 patients died (20%) under medical treatment, and severity of symptoms at the time of diagnosis was obviously the severity of symptoms at the time of diagnosis was the major predictor of death; however, patients in functional class I-II had an annual mortality of 4.1%. More importantly, early surgery was detected as independent predictor of higher survival in the multivariate analysis.

In a study carried out by Enriquez-Sarano et al. (3) of 456 asymptomatic patients with severe mitral regurgitation and under medical treatment, freedom of cardiac death was 22% ± 3%, heart failure freedom was 14% ± 3%, and atrial fibrillation freedom was 33% ± 3%. Patients with effective regurgitant orifice ^{3} 40 mm^2 presented higher mortality at 5 years than the expected one for the general population in the United States census (78% versus 58% ± 9%; p = 0.03).

In a recent publication, Kang et al. (4) prospectively analyzed two groups of 127 patients, assessed by propensity score with asymptomatic severe mitral regurgitation and preserved ventricular function (one group under conservative medical treatment and the other one under early surgery). In the 7 years follow-up, cardiac mortality in the surgery group was 0%, whereas the conservative group presented 12 deaths (5%), p = 0,008. Actuarial survival at 7 years due to cardiac reasons was significantly higher in the sur-



gery group than in the conservative group (99% ± 1% versus 85% ± 4%; p = 0.007). In the conservative group, the multivariate analysis of mortality and rehospitalization due to heart failure identified age (OR: 1.02, IC 95% 1.01-1.04; p = 0.005), degree of pulmonary hypertension (OR: 1.87, IC 95% 1.22-2.87; p = 0.003), and the regurgitant orifice area (OR: 2.06, CI 95% 1.11-3.82; p = 0.02) as independent predictors. Patients were under medical treatment, with clinical and echocardiographic checks every 6 months, and they were referred to surgery only when they presented dyspnea, ventricular dysfunction (ejection fraction £ 60%, end-diastolic diameter ³ 45 mm), pulmonary artery pressure estimated by color Doppler ³ 50 mm Hg, or atrial fibrillation occurrence.

As mentioned above, mitral valve repair feasibility is directly related to the experience of the surgical center. Over the past ten years, experience in valve repair increased, and our surgery group has also specialized on this subject matter. In our work, actuarial survival at 10 years for patients with mitral valve repair was 98%, with 95.5% of follow-up (see Figures).

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