Letters to the Editor

The Editor welcomes submissions for possible publication in the Letters to the Editor section that consist of commentary on an article published in the Journal or other relevant issues. Authors should:

- Include no more than 500 words of text, three authors, and five references
- Type with double-spacing
- See http://jtcs.ctsnetjournals.org/misc/ifora.shtml for detailed submission instructions.
- Submit the letter electronically via jtcvs.editorialmanager.com.

Letters commenting on an article published in the JTCVS will be considered if they are received within 6 weeks of the time the article was published. Authors of the article being commented on will be given an opportunity to offer a timely response (2 weeks) to the letter. Authors of letters will be notified that the letter has been received. Unpublished letters cannot be returned.

CorCap device in addition to mitral surgery in heart failure: Is it truly beneficial?

To the Editor:

The recent report about mitral valve surgery in heart failure[1] raises a number of interesting questions. While left ventricular (LV) end-diastolic volume and end-systolic volume in the treatment group decreased significantly more with the cardiac support device (CorCap; Acorn Cardiovascular, St Paul, Minn) than in the control group, other differences were statistically insignificant. Important end points such as survival, ejection fraction, and measurable indices of functional capacity (6-minute walking test and oxygen consumption), although presented as "better" in the treatment group, were not statistically different. The only adverse event that was nearing significance ($P = .06$) in favor of the control group, namely a neurologic deficit/stroke, is hidden in Table 2. The lack of significant advantages to the treatment group in the presence of a similarly stable mitral repair is intriguing. The CorCap device is enveloping both ventricles, defining a new upper limit to total (end-diastolic) biventricular volume. We are all familiar with the echocardiographic picture of the spherical shape of the globally enlarged LV, including the interventricular septum. Obviously, by enveloping both ventricles from the outside, the device does not restrict or reshape the septal component of LV dilatation. Moreover, assuming that the tension of the device is even and equal throughout, this means that we are expecting to limit LV free-wall dilatation by suspending it on the right ventricle (RV). Can we trust the RV with its much lower pressures and wall tension to successfully counteract and restrict either the systolic or the diastolic distention of the globally or even the regionally enlarged LV? It is likely that the greater reduction in LV volumes observed in the treatment group is the result of a new balance between the two ventricles. RV volume is restricted more than LV volume. The RV being dragged or "squeezed" toward the LV during each systole, and having decreased space for diastolic filling because of the rightward bulging, noncompliant ventricular septum, can now produce a smaller stroke volume. LV end-diastolic volume is consequently decreased, with some improvement in shape, but probably with minimal change in wall stress and ejection fraction beyond what has already been accomplished with mitral repair or replacement. The authors conclude that in addition to the clear benefit from elimination of mitral regurgitation, there is significant additional benefit with the CorCap device.

The question remains whether the mechanical preload reduction of the LV via RV restriction, which does not improve LV contractile function, outweighs the adverse events and the risks and technical difficulties encountered when such a patient requires heart transplantation.

Amir Elami, MD
Department of Cardiothoracic Surgery
Hadassah University Hospital
Jerusalem, Israel

Reference

1. Acker MA, Bolling S, Shemin R, Kirklin J, Oh JK, Mann DL, et al. Mitral valve surgery in heart failure: insights from the Acorn Clinical Trial. *J Thorac Cardiovasc Surg*. 2006; 132:568-77.

doi:10.1016/j.jtcvs.2006.09.058

Microbiologically documented nosocomial infections after coronary artery bypass surgery without cardiopulmonary bypass

To the Editor:

In the latest issue of the *Journal of Thoracic and Cardiovascular Surgery*, we read with great interest the article by Falagas and colleagues,[1] in which they evaluated the frequency, characteristics, and predisposing factors of microbiologically documented nosocomial infections in a well-defined subgroup of critically ill patients undergoing off-pump coronary artery bypass grafting. In this clearly and well-documented

article, it is mentioned that there is a statistically significant difference, in terms of mortality, between patients having a documented infection and the rest of the patients (23.8% vs 1.2%, $P < .001$).[1] These data, however, should be interpreted with caution. When considering the main characteristics of the study cohort, patients with infection versus patients without infection already differ in terms of age and underlying conditions before the onset of infection. The authors themselves demonstrated clearly a statistically significant difference concerning left ventricular ejection fraction (better in the patient group with lower mortality). This cannot be neglected because it has widely been demonstrated that such organ dysfunctions are indispensably associated with increased mortality.[2] Here a logistic regression model with adjustment for possible confounding factors, such as length of hospitalization before infection, age, and severity of illness, could be used to assess the potential causative effect of infection on mortality. With regard to this, we wonder whether the authors can provide further details on the rate of patients receiving appropriate antimicrobial agents. This has been shown to significantly improve patient outcome.[3-5] Furthermore, with respect to the severity of illness, a certain estimation by means of, for example, EuroSCORE could explain possible differences of outcome measures between both groups. We would appreciate if Falagas and colleagues could elaborate on their report, keeping those issues in mind.

Dominique M. Vandijck, RN, MSc, MA[a,b]
Stijn I. Blot, RN, MSc, PhD[a,b]
Jan I. Poelaert, MD, PhD[a]
Department of Intensive Care Medicine[a]
Ghent University Hospital
Ghent, Belgium
Healthcare Department[b]
Hogeschool Ghent, "Vesalius"
Ghent, Belgium

References

1. Falagas ME, Rosmarakis ES, Rellos K, Michalopoulos A, Samonis G, Prapas SN. Microbiologically documented nosocomial infections after coronary artery bypass surgery without cardiopulmonary bypass. *J Thorac Cardiovasc Surg*. 2006;132:481-90.
2. Blot SI, Depuydt P, Annemans L, et al. Clinical and economic outcomes in critically ill patients with nosocomial catheter-related bloodstream infections. *Clin Infect Dis*. 2005;41:1591-8.
3. Blot S, Vandewoude K. Early detection of systemic infections. *Acta Clin Belg*. 2004;59:20-3.
4. Colardyn F. Appropriate and timely empirical antimicrobial treatment of ICU infections—a role for carbapenems. *Acta Clin Belg*. 2005; 60:51-62.
5. Kollef M. Appropriate empirical antibacterial therapy for nosocomial infections: getting it right the first time. *Drugs*. 2003;63:2157-68.

doi:10.1016/j.jtcvs.2006.09.044

Reply to the Editor:

We appreciate the comments of Vandijck and colleagues regarding our study.[1] They raised the issue of the potential mortality attributable to infection in our study group (patients who underwent off-pump coronary artery bypass grafting). They consider that confounding factors, such as age, comorbidity, left ventricular ejection fraction, antimicrobial treatment, and EuroSCORE values, might have led to the observed statistically significant difference in mortality between patients with and without microbiologically documented nosocomial infection.

Our results regarding mortality of patients who underwent coronary artery bypass grafting are in accordance with the results of previous studies (a fact that is mentioned in the discussion section of our article).[2,3] For example, using data from the US Society of Thoracic Surgeons National Cardiac Database, Fowler and associates[2] showed that patients with major infection had significantly higher mortality (17.3% vs 3.0%, $P < .0001$) and postoperative length of stay of longer than 14 days (47.0% vs 5.9%, $P < .0001$) than patients without infection.

However, we agree with Vandijck and colleagues that infections are sometimes the consequence of other postoperative complications or comorbidity that predispose to infections. It should be emphasized that the majority of patients (4/7 [57%]) with sternal wound infection in our cohort had a history of previous sternotomy or postoperative invasive interventions at the surgical site.

We did not collect data to estimate EuroSCORE values for the group of patients who did not have nosocomial infection based on the design of our study, and these data are not readily available now to be analyzed. However, we performed an additional analysis of factors associated with mortality in our cohort of patients. Variables that were statistically associated ($P < .05$) with mortality in the bivariable analysis were entered in a backward, stepwise, multivariable logistic regression model. This statistical analysis revealed that independent risk factors for death were urgent operation, anemia (hematocrit, <34%), and low left ventricular ejection fraction on admission ($P < .001$ for all these variables).

We also agree with Vandijck and colleagues that appropriate antimicrobial treatment of postoperative infections is essential to improve patient outcome. It has been shown that inappropriate empirical therapy is associated with increased mortality, especially among patients with infections caused by multidrug-resistant bacteria.[4] In fact, old antibiotics, such as polymyxins, have been used recently to combat some of these infections.[5] Two of 21 patients with infection in our cohort (patients 2 and 17 in Table 3 of our article)[1] were infected with multidrug-resistant isolates; these patients did not receive appropriate empirical antimicrobial therapy until the results of the in vitro susceptibility testing of the isolated pathogens became available.

Matthew E. Falagas, MD, DSc[a,b,c]
Evangelos S. Rosmarakis, MD[a]
Sotirios N. Prapas, MD[d]
Alfa Institute of Biomedical Sciences (AIBS)[a]
Athens, Greece
Department of Medicine[b]
Department of Cardiac Surgery[d]
Henry Dunant Hospital
Athens, Greece
Department of Medicine[c]
Tufts University School of Medicine
Boston, Mass

References

1. Falagas ME, Rosmarakis ES, Rellos K, Michalopoulos A, Samonis G, Prapas SN. Microbiologically documented nosocomial infections after coronary artery bypass surgery without cardiopulmonary bypass. *J Thorac Cardiovasc Surg*. 2006;132:481-90.
2. Fowler VG Jr, O'Brien SM, Muhlbaier LH, Corey GR, Ferguson TB, Peterson ED. Clinical predictors of major infections after cardiac surgery. *Circulation.* 2005;112(suppl):I358-65.
3. Kollef MH, Sharpless L, Vlasnik J, Pasque C, Murphy D, Fraser VJ. The impact of nosocomial infections on patient outcomes following cardiac surgery. *Chest*. 1997;112:666-75.
4. Falagas ME, Kasiakou SK, Rafailidis PI, Zouglakis G, Morfou P. Comparison of mortality of patients with *Acinetobacter baumannii* bacteraemia receiving appropriate and inappropriate empirical therapy. *J Antimicrob Chemother*. 2006;57:1251-4.
5. Falagas ME, Michalopoulos A. Polymyxins: old antibiotics are back. *Lancet*. 2006;367: 633-4.

doi:10.1016/j.jtcvs.2006.10.013