

DIOCLES: Some Caveats and New Questions. Response



DIOCLES: algunos matices y nuevas preguntas. Respuesta

To the Editor,

On behalf of the Scientific Committee and researchers of the DIOCLES registry,¹ we wish to thank Rosell-Ortiz et al for their letter and their interest in our study. The results are certainly promising in that they show a decrease in the in-hospital and 6-month mortality rates among patients with acute coronary syndrome (ACS) with respect to that recorded in the MASCARA study, the most recent large registry carried out in Spain,² and are in accordance with the progressive reduction in mortality due to ST-segment elevation acute myocardial infarction (STEMI) observed over the past 20 years. Undoubtedly, a number of factors have influenced this reduction, and the development of out-of-hospital emergency services is probably not the least important of them. Closely related to the latter aspect is the progressive incorporation of protocol-based networks for the management of patients with STEMI, in which prehospital care plays a major role.^{4,5} We agree with the authors of the letter in that the overall mortality occurring during the acute phase of STEMI is higher than the 6.6% recorded in our study,¹ as this value does not take into account prehospital mortality, both to ensure coherence for comparison with previous registries^{2,3} and because it is very difficult to reliably estimate its incidence.

We did not examine the possible differences in mortality in the overall group of ACS patients or specifically in those who also had STEMI in terms of the level of care provided by hospitals or the Spanish autonomous community, aspects of unquestionable interest.⁶ We will attempt to analyze the data from the DIOCLES registry in this respect, but the relatively small size of the population, especially in the subgroup with STEMI, will probably make it impossible to draw firm conclusions. In this subgroup, the overall management strategy applied is almost certainly more relevant than the technological level of the treatment hospital. Extensive evidence indicates that the development of efficient regional networks to care for patients with STEMI, and that include both primary percutaneous coronary intervention and a pharmacoinvasive strategy when this intervention cannot be performed promptly, improves the percentage of reperfused patients and decreases infarction-related mortality. It was not our objective (nor is the sample size large enough) to compare the mortality rate of the patients initially treated with thrombolysis with that of those whose initial treatment was primary percutaneous coronary intervention. In any case, the results of the DIOCLES registry show that, in Spain, there is margin for improvement in the application

of both reperfusion strategies.¹ As is logical, care provided by an out-of-hospital emergency service was associated with a more frequent use of prehospital thrombolysis (48%), especially if the treatment was administered in a fully-equipped ambulance (56%). However, even in this setting, a significant number of patients underwent thrombolysis in the hospital.

The DIOCLES data coincide with those of previous studies² in that the patients with unclassified ACS constitute the subgroup at highest risk, and we consider the attempt to identify the determinants of this greater risk, especially the modifiable factors, to be highly relevant. We appreciate the proposal of Rosell-Ortiz et al to carry out an in-depth analysis of the data in this respect.

José A. Barrabés^{a,b,*} and Alfredo Bardají^{a,c}

^aPrincipal investigators DIOCLES study

^bServicio de Cardiología, Hospital Universitario Vall d'Hebron, VHIR, Universidad Autónoma de Barcelona, Barcelona, Spain

^cServicio de Cardiología, Hospital Universitario de Tarragona Joan XXIII, IISPV, Universidad Rovira i Virgili, Tarragona, Spain

*Corresponding author:

E-mail address: jabarrabes@vhebron.net (J.A. Barrabés).

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Comments on Exercise Echocardiography and Multidetector Computed Tomography for the Evaluation of Acute Chest Pain



Comentarios a la evaluación del dolor torácico agudo mediante ecocardiografía de ejercicio y tomografía computarizada multidetectores

To the Editor,

I have read the article published by the group at the *Hospital Clínic de Barcelona*¹ with great interest, and would like to congratulate the authors publicly on their outstanding research endeavor.

Nonetheless, although the authors recommend “a balanced strategy” combining both techniques, in my reading of the article I detect an underlying conflict between them, and would like to make some comments related to this. These comments are intended in no way to diminish the authors’ extraordinary work, but rather to present “the current value” of computed tomography (CT).

- A limitation not mentioned by the authors is the long time elapsed between the conduct of the study and its publication. It may be that the intervening 6 years have brought no changes in exercise echocardiography and that older results thus remain applicable in 2014; however, developments in multidetector computed tomography (MDCT) during this period have been truly spectacular and exponential. Besides improved spatial and