

Express² (Boston Scientific), respectively] have a negligible impact on the analysis. Our findings of a more pronounced anti-restenotic effect ($P < 0.001$) in Cypher vs. Taxus have actually raised concerns that most of the relative risk reduction apparent with Cypher was due to the severely inferior performance of Bx Velocity (a well-known thick strut BMS) when compared with the substantially superior performance of Cypher [when compared with Express² (a thinner strut BMS) and Taxus, respectively].

Using results reported from Pache *et al.*¹ (Figure 1A), it can now be shown that the benefits obtained from Cypher vs. Bx Velocity are far greater than those obtained from Cypher vs. thin-strut BMS, thus casting a shadow of doubt on our indirect comparative results ($P < 0.0001$ for statistical heterogeneity, when comparing the estimates obtained with thin-strut BMS vs. thick-strut BMS).

Conversely, data recently reported from several direct head-to-head Cypher vs. Taxus randomized trials and demonstrating a lower risk of restenosis with Cypher vs. Taxus ($P < 0.0001$) appear largely in agreement with our previous indirect adjusted findings (dating back as early as June 2004),⁴ both at graphical inspection and consistency testing with heterogeneity and inconsistency (I^2) tests (Figure 1B). Nonetheless, some degree of overestimation cannot be dismissed and is likely due to the difference between thin- and thick-strut BMS, as correctly pointed out by Pache *et al.*¹

In conclusion, the authors should be complimented for their independent research effort and for having reminded us that any piece of new clinical knowledge can provide important and relevant insights, both by itself and when combined with appropriate analytical methods in systematic overviews.⁵

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Giuseppe G.L. Biondi-Zoccai
Interventional Cardiology Unit
St Raffaele Hospital
via Olgettina 60
20132 Milan, Italy
Tel: +39 3408626829
Fax: +39 0226437339
E-mail address: gbiondizoccai@gmail.com

Pierfrancesco Agostoni
Interventional Cardiology Unit
AZ Middelheim
Antwerpen
Belgium

Antonio Abbate
Department of Medicine
Medical College of Virginia
Richmond
VA
USA

Flavio Airoldi
Interventional Cardiology Unit
St Raffaele Hospital
via Olgettina 60
20132 Milan
Italy

Antonio Colombo
EMO Centro Cuore Columbus
Milan
Italy

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Critical role of bare-metal stent controls in trials of drug-eluting stents: reply

We appreciate very much the interest shown by Dr Biondi-Zoccai and co-workers in our study¹ and share their opinion about the need of a careful and comprehensive evaluation of the real benefit of new technologies. This was also the rationale of our trial.¹

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randomized trial. *Eur Heart J* 2005, in press. Published online ahead of print February 28, 2005. doi:10.1093/eurheartj/ehi098.

Adnan Kastrati
Deutsches Herzzentrum
Lazarettstr. 36
80636 Munich
Germany
E-mail address:
kastrati@dhm.mhn.de

Jürgen Pache
Deutsches Herzzentrum
Lazarettstr. 36
80636 Munich
Germany

Alban Dibra
Deutsches Herzzentrum
Lazarettstr. 36
80636 Munich
Germany

Albert Schömig
Deutsches Herzzentrum
Lazarettstr. 36
80636 Munich
Germany

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Towards a unified strategy for atrial fibrillation ablation?

We read with great interest the recent editorial by Cappato¹ in the *European Heart Journal*. In the last few years, most electrophysiological groups, including the pioneering group of Bordeaux, progressively and substantially changed their initial strategy of atrial fibrillation (AF) catheter ablation from focal or pulmonary vein isolation to more extensive lesions, aiming for a better success rate and lowering risks. We started performing circumferential pulmonary vein ablation (CPVA) simultaneously to the first approach described by the Bordeaux group and have not substantially changed CPVA technique overtime mainly because of the initial high success rates and minimal risks obtained in patients with both paroxysmal and chronic AF. Additional lines were added to avoid iatrogenic left atrial tachycardia from January 2002 and the final data on 580 patients have been recently published in *Circulation*.² As you can see, this is the evolution of the CPVA approach and

everybody knows, with a few exceptions perhaps, the results of our experience. Cappato commented on the occurrence of left atrial flutter as being a rather common adverse event after CPVA and thus a limiting factor of this technique. This is not now scientifically accurate. Unfortunately and surprisingly enough, Cappato has not reported in his editorial the aforementioned randomized study addressing this important and specific issue.² The CPVA approach using additional lines in the posterior wall and mitral isthmus, which is from 2002 the current technique, has an incidence of left atrial tachycardia of only 3.9% when compared with 10% with CPVA alone.

We strongly believe that the two initial different strategies of the two pioneering groups on AF catheter ablation have now quite similar success rates and go towards a unified strategy, that is the CPVA approach.

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Carlo Pappone

Department of Cardiology
Electrophysiology and Cardiac Pacing Unit
San Raffaele University Hospital
Milan 20132
Italy
E-mail address: carlo.pappone@hsr.it

Vincenzo Santinelli

Department of Cardiology
Electrophysiology and Cardiac Pacing Unit
San Raffaele University Hospital
Milan 20132
Italy

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Towards a unified strategy for atrial fibrillation ablation?: reply

I thank Drs Pappone and Santinelli for their interest in my editorial titled 'Towards more effective techniques for catheter ablation of atrial fibrillation: to aim for electrical

disconnection of pulmonary veins or not?'¹ The authors observe that adding ablation lines at the left posterior wall and the mitral isthmus to the anatomical circumferential ablation (ACA) catheter technique for the treatment of paroxysmal or persistent atrial fibrillation is associated with a significantly lower incidence (from 10.0 to 3.9%) of iatrogenic left atrial flutter during long-term follow-up² and that this lower incidence may justify an extended use of their technique. I have to admit that the manuscript quoted by Pappone and Santinelli had not been published yet at the time of final submission of my editorial to the *European Heart Journal*. Nevertheless, the arguments raised by the authors offer an interesting opportunity for debate.

Although the technique proposed by Pappone and Santinelli to limit the incidence of left atrial flutter in these patients is of interest and accurately investigated, the following observations should be carefully considered before it can be proposed in clinical practice. First, a 4% incidence of left atrial flutter in response to ACA is not a poor figure, particularly if one considers the drug refractoriness of this arrhythmia and the deterioration in quality of life that it may produce when compared with pre-ablation. Secondly, this figure is obtained at the expense of additional pulses deployed in the left atrium, outside of the area delimited by the ACA design. Of interest, recent findings indicate that post-ablation left atrial flutter originate and perpetuate within the territory delimited by the ACA design, and that re-isolation of the pulmonary vein (PV) antrum effectively prevents left atrial flutter recurrence.³ As a result of this observation, PV electrical disconnection rather than empirically designed ACA would appear to have a better rationale for prevention of late left atrial flutter; also, PV electrical disconnection would not require adding ablation lines at the left posterior wall and the mitral annulus. Finally, the data from Pappone and Santinelli reflect the experience of a single centre and are in need of confirmation from a multi-centre experience. As outlined in a recent survey conducted worldwide on catheter ablation of atrial fibrillation, the results obtained in daily clinical practice from a heterogeneous set of EP laboratories show considerably lower efficacy than those reported from pioneering centres;⁴ I would not be surprised, if the same observation held true with regard to the incidence of post-ablation left atrial flutter in patients receiving ACA technique.

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Riccardo Cappato

Arrhythmia and Electrophysiology Center
Policlinico San Donato
University of Milan
20097 Milan
Italy

E-mail address:

riccardo.cappato@grupposandonato.it

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Plasma levels of B-type natriuretic peptide in children and adolescents with aortic valve stenosis

We read with interest the recent article of Weber *et al.*,¹ which investigated the relation of N-terminal (NT) pro-B-type natriuretic peptide (BNP) to progression of aortic valve disease. According to their results on a large cohort, the authors considered NT-proBNP as a suitable biomarker for the evaluation and monitoring of patients with aortic valve disease. On the basis of our own experience in paediatric patients, we would like to add a note of caution regarding the diagnostic accuracy of natriuretic peptides in aortic stenosis.

In the last 4 years, we have measured BNP in almost 200 healthy children and more than 400 patients with congenital heart disease using the Triage BNP test (Biosite Inc., San Diego, California, USA).² Within these examinations, we analysed 25 infants, children, and adolescents (aged 6 weeks to 27 years, median age 9.9 years, 18 males, and seven females) with aortic valve stenosis, with or without mild to moderate aortic insufficiency, but without additional

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