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 Taxus 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 thick-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 (I2) 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 reviews.5

References


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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

References


2. As you can see, this is the evolution of the CPVA approach and toward 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. 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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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., 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). 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