Author's response to reviews

Title: The CYP2J2 G-50T Polymorphism and Survived Myocardial Infarction in Patients with Cardiovascular Risk Profile

Authors:

Jan Börgel (jan.boergel@rub.de)
Daniel Bulut (daniel.bulut@rub.de)
Christoph Hanefeld (christoph.hanefeld@rub.de)
Horst Neubauer (horst.neubauer@rub.de)
Andreas Mügge (andreas.muegge@rub.de)
Jörg T Epplen (joerg.t.epplen@rub.de)
Tim Holland-Letz (holland-letz@amib.ruhr-uni-bochum.de)
Martin Spiecker (spiecker@kardiologie-marl.de)

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Author's response to reviews: see over
To the editor:

We are herewith sending to you the second revision of our manuscript: “The CYP2J2 G-50T Polymorphism and Myocardial Infarction in Patients with Cardiovascular Risk Profile”.

You can find a List of Corrections, based on the Comments of the editorial board:

Comments from our editorial board:

"I would not agree with reviewer 2 that a retrospective study design cannot answer the question whether a genetic polymorphism is associated with coronary disease risk. Most case-control studies in coronary disease that are sufficiently large to provide reliable results regarding genetic associations are of necessity retrospective; prospective studies would have to be unmanageably large to generate the numbers of cases required.

Editor: “However, the authors break their study design in analysing the OSA and angiography groups together, I'm not sure why they do this. So the study design is suboptimal in that respect.”

Author: Both study groups were collected by the same research group and the same data collection method. Research of both study groups focussed on the genetic mechanisms in patients with risk factors for cardiovascular disease. In the OSA – group the epidemiologic associations of OSA on hypertension, Dyslipidemia and myocardial infarction were analysed and in addition the modulating effects of e.g. beta-2-receptor polymorphisms on these relationships were investigated (see for example: Bartels & Börgel et al., BMC Medicine 2007; 5:1).

Since both groups were collected by the same method and intention – and in addition both represent patients with on average high cardiovascular risk profile – we combined both groups for the analysis of the CYP 2J2 G-50T Polymorphism on myocardial infarction.

Editor: “I'm also in agreement with reviewer 2's point regarding data reporting. It's not correct for the authors to suggest that there is an association here, when the p-value isn't even conventionally significant at the 0.05 level, much less at the very stringent significance levels appropriate for genetic association studies.”

I would think the conclusions of the manuscript need to be altered before it could be accepted ....

Author: Since, we investigate only one polymorphism in this combined population, no correction (e.g. Bonferroni) is necessary, and therefore a level of significance of 0.05 would be enough. However, since this is not achieved, the text in the conclusions of abstract and manuscript text were changed as mentioned by the editor. Now, it is clearly stated, that the multivariate analysis is not significant. However, we would like to (partially) justify the need for further research with our result being close to the border of significance.
**Editor:** …and the limitation of the two-group ascertainment and joint analysis mentioned. Also the authors need to acknowledge that their numbers are very small and therefore the power to detect even a quite strong association is very low."

**Author:** Limitations were changed according to the recommendation of the editor. Since we found significant results in the past in these study groups, we would like to de escalate the superlative character of the sentence (e.g. very low -> low, very small -> comparatively small).

We would like to thank you for your sophisticated and constructive criticism and recommendations!

We are looking forward to your answer.

With our best regards,

Jan Börgel, MD