Reviewer's report

Title: Cardiac Function Assessed by Myocardial Deformation in Adult Polycystic Kidney Disease Patients

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Reviewer: Bert Bammens

Reviewer's report:

Dear dr. Lassen and colleagues,

I have read with interest your manuscript "Cardiac Function Assessed by Myocardial Deformation in Adult Polycystic Kidney Disease Patients".

The paper is well-written and the clinical and scientific questions inspiring the reported research are clear. I feel that this study indeed shows that the assessment of left ventricular global longitudinal strain by advanced echocardiography may offer a more sensitive way to detect cardiovascular implications of PKD in an earlier stage.

I have some questions that may need to be answered in a revised version of the manuscript before publication:

1/ There is no control population in the study. It would be good to see whether CKD from other causes has another impact on the studied parameters. In other words: are the findings unique to ADPKD as a renal diagnosis? The fact that GLS does only weakly correlate with eGFR, does not exclude the impact of kidney dysfunction per se on the cardiac parameters.

This is the major drawback of the study. Also the relationship with hTKV does not hold in multivariate analysis. So, it is not clear whether the findings are ADPKD specific and whether they reflect ADPKD-specific pathogenetic mechanisms rather than the impact of renal dysfunction in general.

2/ Many subgroups of patients were excluded from the trial (prior kidney transplant, pregnancy, uncontrolled diabetes, current participation in a trial...). Which proportion of the total number of ADPKD patients was this? Any risk of selection bias? Potentially the patients participating in trials have different characteristics as compared to others?
3/ The authors correctly point to the potential impact of "reverse causation" with regard to the association between beta-blocker use and GLS. Nevertheless, they suggest that "alternative antihypertensive classes may be preferred... ...for the treatment of hypertension in ADPKD", which would imply direct causality. I would rather stick to the first hypothesis, also taken into the account the low numbers.

Kind regards,

Bert Bammens, MD, PhD

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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