Author’s response to reviews

Title: Evaluation of left ventricular remodelling in young Afro-Caribbean athletes.

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Reviewer reports:

In first line we want to thank the reviewers to give us the opportunity to improve the manuscript. All the suggestions have been considered, and the modifications requested are now in the text.

We hope to have correctly interpreted and substantially modified the paper.

Considering the poor quality of the ECG images, we have removed the two images from the txt. This in agreement with the aim of the study that it is not addressed to the ECG exam.

Let me know if this is acceptable for you.
Reviewer #1: This is a study exploring the ethnic differences in footballer players by tissue characterisation and cardiac remodelling using echocardiography. A sub-cohort has also had follow up. Interestingly in the baseline groups, strain data showed no significant differences between black and white athletes, whereas the black athletes had higher wall thickness.

In Lines 50-51 the sentence "This ventricular adaptive remodelling is adequate, reversible characterized by a normal systolic and diastolic function" is grammatically incorrect. I recommend removing the word adequate.

Line 52 a preserved - a should be removed

Line 55 nowadays. This word should be removed.

Line 60 It's should read it is

Line 62 the words "a heart" should be removed

Thank you for your suggestions. The modifications requested in lines 50-51, 52, 55, 60 and 62 have been made.


Line 70 a reference needs to be included to back up this statement.

Sheikh, N. & Sharma, S. Nat. Rev. Cardiol. 2014;11:198–217 was included as a reference for this statement.

LV remodelling in black athletes has been previously described and is generally accepted. The numbers in each group are relatively low and probably explains why there were non-significant differences in LV mass. The interesting aspect is the persistence of LV remodelling in that black athlete, but this is not surprising.

We thank the reviewer to give us the opportunity to improve the paper. We are in agreement with this relevant suggestion and therefore we have implemented the manuscript in the last part.
To classify all Afro-Caribbean players into a subgroup known as "black athletes" is likely to be too-simplistic as there are likely to be ethnic differences (e.g. West African vs East African) between many of the players within this subgroup. This is an area which requires some evaluation and consideration. In this paper the terms Afro-Caribbean and Afro-American have been used interchangeably. This is incorrect and stricter definition of ethnic backgrounds are required.

Thank you for this important suggestion that underlies a sort of mistake and potential misunderstanding for the readers. The most part of the athletes investigated came from Africa and therefore the details of the population investigated have been modified. We have tried to clarify this in the text. Incorrectly, the “African Americans” term was used to classify all the “black athletes” while “African-Caribbean” was a more appropriate definition. We hope to have correctly interpreted you suggestion.

The ECG figures supplied are too small and difficult to interpret. Abnormal ECGs were found in at least 7% of WP and it would be useful to understand whether these athletes had normal wall thickness or strain patterns.

We are in agreement with your indication and considering that the ECG is not the fundamental topic of the study, we will prefer to withdraw the images, if this suits you intention.

The criteria for determining which players were selected for follow up needs to be explained to avoid potential for bias. If the rationale for follow up is not cogent, then I have reservations about drawing any meaningful conclusions. The authors need to explain why 47 BP did not have follow up. I suspect the follow up population consisted of players who continued to play and those who were lost to follow up represent a cohort who gave up playing football. If data were available for those players who detrained, the paper would be considerably stronger. Loss to follow up of over half the original cohort is a considerable weakness of the study.

As normally occur in the seasonal recruitment, some of the players can change their team. This is the principal reason to justify the absence of some subjects in follow–up. We have highlighted this aspect in the material and methods, and in the limits session of the paper (*).

In any case, considering that a small sample has continued to train regularly with the same work-load and same lifestyle like a homogeneous population, we have particularly focused on this small group. This aspect has been also clarified in the paper.

One of the papers conclusions is "Global Longitudinal Strain has not been specifically determined on Afro-American athletes yet". Given this statement, a fundamental drawback is the lack of follow up strain data.
The strain analysis has been made just in a restricted numbers of athletes and has been used exclusively to improve the message that contractile function was normal in the athletes.

In the reference section there are numerous typographical errors. For example, "subclinical left ventricular systolic dysfunction" or "electrocardiographic repolarization." Each incorrect reference needs to be re-referenced without such basic mistakes.

Thank you for your suggestions. All the typographical errors have been corrected.

In its current format this paper is not suitable for publication. It does not add to current literature and there are potential methodological flaws, which have not been acknowledged by the authors. I have not been able to highlight all the grammatical errors which need careful attention and correction. Whilst higher definition ECG figures are required, these do not add useful information to the paper and are not necessary. Examples of the strain curves obtained would be more relevant figures. The methodology section needs to provide the criteria for follow up. Comparative data for BP who detrained could strengthen the paper but without this I have strong reservations regarding suitability for publication.

Reviewer #2: This is a nice study about left ventricular remodeling in adolescent Afro-American athletes compared to Caucasian athletes. Data are interesting, especially for the unique characteristics of the population, but there are some significant methodological limitations that should be addressed.

Apparently, you excluded 20 subjects from your initial population. Why? Please explain the specific reason for each of the excluded subject.

As request on behalf of the reviewer n 1, and in agreement with him, we have explained the reason to investigate a restricted group (see *)

Who performed the echocardiograms? How many different operators? Do they have European certification for transthoracic echo? Did you assess their inter-observer variability?

Thank you for this check. It is very important for echo exam to have operators with a specific background. We have detailed all the characteristics requested, especially for the inter-observer variability in the text.
You state that differences in end-diastolic LV volumes are not significant because they may be due to inter-observer variability. Why this should not apply for all other measurements? This is a crucial aspect which should be clarified.

We have tried to explain, in first line and in the more comprehensible way, the best interpretation of data. We hope you are in agreement with us; however we respectfully accept any your additional modification.

The echo description lacks some important information. No data are provided about LA and RA volumes (at least dimensions), as well as no PASP values. The data missed are now in the tables.

Although the right heart is not the core of your analysis, it would be interesting to have a more comprehensive description of these hearts.

Yes, the RV is not the core of the investigation; however it has been studied as reported in the text.

You have strain analysis, but apparently you did not perform Tissue Doppler Imaging. Why? The echo exams should be stored so you should be able to recall some of this information.

We apologize for the data missing. The rational of this decision was that, considering the chambers function normal, the TDI calculation did not add any substantial information. If you think it could be determinant, we can try to recovery the data. Let me know about that.

It seems that strain analysis was performed in a subgroup. This should be better highlighted.

Thank you for your suggestion. We have now explained in the text the reasons why strain analysis was performed only on a minority of the subjects.

IVS wall thickness is different in the two study groups, but LVMI and RWT are not. You should discuss this, providing your possible explanations for this finding.

This is a strange aspect and of hard interpretation. In first line, the absence of the LVMI change, despite different and significant wall thickness, could be related to the different LV diameter found. If you consider this data determinant for the readers, we can add some sentences in the text.

RWT data should be added to the tables. Why did you choose a cut-off of 0.40 for RWT, which is different from ASE/EACVI recommendations (your reference 19).

The RWT data have been added. The reference has been updated with the last ASE version (2017).
Your definition of LVH is questionable, and you should explain to the reader why you didn't apply standard definition of LVH at echo.

Thank you for underlying this statement. We have referred the IVS values considering only the LV thickness, with the exclusion of the RV septum thickness wall. In this case, the value is in agreement with literature. Let me know if you prefer to adequate the manuscript to this reference.

A paragraph on study limitations is lacking. I suggest to add it.

Thank you for your suggestion. We have added it in the final part of the text.

The first evaluation of Table 4 seems different from Table 2. Please, explain.

Thank you to highlight this point. Table 4 shows data about the subgroup which was followed-up. The columns on the left side represent the first evaluation only of the followed-up subgroup, at time 0, while the columns on right side represent the second evaluation of the same subgroup, after 4 years. Table 2, in contrast, shows data about all the subjects evaluated at time 0, including the subgroup evaluated for the follow-up (left columns in table 4). We tried to clarify this aspect in the text too. If you think, this could be misunderstanding for the readers, we can try to modify the tables.

It would be interesting also to see whether there are significant differences between the first assessment and the follow-up within the same study group.

As consequence of the previous sentence, table 4 can be considered as “self explaining”.

In your statistical analysis how did you choose parametric and not parametric tests?

We apologize. The statistical analysis has been conducted considering the most appropriate test for unpaired data and the potential missing of some points of the results.

Please put all p values in the tables, even if they are non-significant.

Thank you for this suggestion. We put all of them in the tables.

The paper needs to be reviewed by an English mother-tongue

Comments from the Editors

We suggest to indicate the two groups as "Caucasians" and "African Americans", if this definition correctly describe your population.