Author's response to reviews

Title: Scarcity of atrial fibrillation in a traditional African population: a community-based study

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Author's response to reviews: see over
Dear Editor,

We thank the reviewer for the constructive comments on the abovementioned manuscript. Hereby we submit a revised version of the manuscript, in which the changes have been underlined.

Our responses to the remarks of the reviewer are given below in italics. Changes in the manuscript are referred to by line numbers and by reference numbers corresponding with the revised version of the manuscript.

We hope to have answered the comments and questions satisfactorily. If necessary, we are glad to respond to further inquiries.

We look forward to your response. Thank you in advance.

On behalf of the coauthors,

Yours sincerely,

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RESPONSE TO THE COMMENTS OF THE REVIEWER

Reviewer

Koopman et al present a revised manuscript describing the AF in a Ghanian cohort. The manuscript has made considerable progress and the authors are to be commended for conducting an investigation in the prevalence of atrial fibrillation in an African community. To my knowledge, we have very limited insight into AF and its prevalence and complications in Africa. The report by Koopman et al raises important hypotheses about the etiology of AF, the role of environment versus genetics, and AF surveillance methods in resource-limited communities. I previously commented primarily on the use of the reference cohort (the ATRIA Study); thank you to the authors for integrating several comments and suggestions. Here are further comments for the authors:

1. Population-based versus community-based. I would hesitate to call the project population-based, which indicates that the cohort is representative of the African population. The study examines a defined population in Ghana rather than Africa. Suggestions are to state the country or region in the title (rather than African) or to use the term community-based (in my opinion more appropriate, as the study selected for individuals>50 years and not the population) for population-based.

   We have changed “population-based” into “community-based” in the title of the revised manuscript (line 2) and have made corresponding changes in the manuscript (lines 37, 54, 85, 98, 99, 113, 202, and 312).

2. I suggest deleting "unfortunately" from the second sentence of the abstract. It is precisely because the study of AF has been limited to Western countries that your study is insightful and important; the deficit of studies is the expression of a need for your current work.

   We have removed this word in the abstract of the revised manuscript (line 35; see also line 78).

3. Reference population, page 5, NHANES. It would actually be erroneous to state that the general population of the US was used as a reference population. Rather, NHANES, a survey designed to conduct representative sampling of the US, was used as the reference population. The implication that Koopman et al used the entire US population for reference would be problematic.

   We have changed the wording of this fragment in the revised manuscript as proposed (lines 155-156).
4. ATRIA study. Since the present study identified 3 cases of AF, is a limited number, did not conduct longitudinal assessments, I find it problematic to compare the incidence of AF at all between the Ghanian cohort and the ATRIA study. My thought is that the differences in study design, including AF ascertainment, are too enormous. My suggestion for the authors, and we can discuss further by email, would be not to use such a different study in terms of magnitude, design, and methodology for reference. I am of the mind that the work performed in Ghana can stand on its own. The only place where an implicit comparison is made between the present cohort and ATRIA is Figure 1, which in my view communicates little about the present study's results (please see next comment, 5). Other comparison between the present cohort and ATRIA or other studies (consider also the ARIC cohort; see Alonso 2009 AJC) can be done in the discussion. Such an approach avoids confusing the readers and seeming to present a comparison between two such very different studies.

As proposed, we have deleted Figure 1 from the revised manuscript and now discuss the different prevalences of atrial fibrillation as found in the Ghanaian study population and as known for the general population of the USA in the discussion section (lines 214-218).

5. Because only 3 of 921 study participants had AF, Figure 1 communicates little. An alternative may be to bring the supplementary table into the main manuscript because it provides granular detail on the 3 individuals diagnosed with AF.

As proposed, we have brought Supplementary Table 1 into the manuscript as Table 2 (lines 188-189 and 520-526).


We have corrected this sentence in the revised manuscript (line 157).

7. Page 9, line 273, sentence is not clear. What is the study population to which the authors are describing genetic similarity between individuals of Western and West-African ancestry? The reviewer suggests some word-smithing to improve the paragraph. For instance, the phrase "genetically little susceptible" is awkward.

We have clarified this fragment in the revised manuscript (lines 269-280).