Reviewer's report

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

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Reviewer: Jared Magnani

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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.

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.

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.

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.

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.


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.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.