Reviewer’s report

Title: Unfiltered signal averaged P wave analysis - further validation of the method

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Reviewer: Damian Redfearn

Reviewer’s report:

General
Holmqvist and colleagues poorly describe a number of studies under the collective heading of “unfiltered signal averaged p wave analysis – further validation of this method”. The title is somewhat misleading as there is no evidence of ‘validation’ of the method as such, rather the group investigate the mean change in P wave duration with additional filtering techniques on 48 high resolution 12 lead ECG recordings from normal individuals. They observe a ‘significant’ increase in the mean PWD with additional filtering of the same signal. The authors then state they determine the ‘reproducibility’ of a P wave morphology classification procedure by examining the intraobserver and interobserver variability. The ‘error’ in P wave parameters was calculated for differing recording lengths with little difference found.

The paper lacks a clear hypothesis and general direction. The link between unfiltered signal averaged P wave and increased PWD with filtering and comparison between studies is unclear. The authors clearly state that most studies use similar bandpass filtering methods and yet the difference in mean PWD between studies remains. Thus it would appear to be more than the issue of bandpass filtering. Indeed they elude to this in the last paragraph on P wave duration in the discussion.

The statement that the method used has received little attention is simply untrue. Many investigators have examined the best methodology for SAPW analysis both time and frequency domains eg Gondo N, Stafford PJ, Steinberg JS etc.

There is a deficiency of explanation of a complex topic that would make this very difficult for the general cardiologist. For example the filtering and signal averaging as noise reduction techniques requires some introduction before the concept of bandpass filtering can be introduced.

The concept of reproducibility is overstated. The group appear to study an isolated recording from 48 normal individuals (pulled from a cohort of 131; it is not clear why the remaining 83 were not studied). They examine the interobserver variability but do not determine reproducibility of the technique over time with either an immediate second recording or any later recordings. The group do however examine this in the context of cardiac pathology in addition to the normal controls. This could be made clearer in the text.

The discussion is interesting and mentions the concepts of noise cut-off levels, this was not studied but would be interesting to examine in a rigorous manner.

The authors would be better to concentrate on one or other of the studies mentioned in this paper. The concept of additional filtering increasing PWD is interesting, although not entirely novel, but does not fit with the rest of the paper. Moreover the introduction lacks direction and a statement of clear hypothesis, what is it the authors want to test?

The ability to derive data from low resolution ECG machines that is reproducible and meaningful is highly desirable. The automated classification algorithm together with previous work looks promising. The authors should pursue this line of validation with further recording from their subjects and an estimate of reproducibility over time and between the recordings.

Figures and tables are clear and of a high standard.

Recommendations
Remove the section on filtering, add data on reproducibility between recordings in the same individual and combine with the data presented on observer variability. Concentrate more on the application of this technique to standard ECG recordings and validation of this tool.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Title – misleading and not relevant to the filtering
Recording length. It is unclear how the parameters were measured, by hand, screen callipers? The findings that a 10sec low resolution recording can present similar results to 6 minutes of high resolution with regard
to noise reduction is hard to grasp. However noise reduction is not measured, merely the gross morphology of each derived orthogonal lead. It is unclear if the 6min recording is compared with a second 6min recording. Moreover the relevance of this to the rest of the study is unclear.

Automated classification. This is poorly explained, is each lead divided into equal thirds, one assumes 1 represents a positive deflection, -1 a negative deflection and 0 isoelectric. A figure would help here.

The reproducibility should be measured according to the coefficient of reproducibility as define by Bland, Altman.

Page 7 para 2 line 2: As mentioned above there are many studies examining the differing methodology, these should be acknowledged and this statement qualified.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
Minor Essential Revisions
The methodology of signal average P wave is generally good; however the authors should mention how the P waves and QRS complexes were determined to be similar, was this template matching, if so how many points, was there a modulus difference algorithm employed?

The results of P wave duration need to refer to the population examined, presumably the 48 normals. The last statement in the paragraph is unclear and is not elaborated on in the discussion. The inference is that the technique produced longer P waves but as the populations are not well defined this should be removed unless supported by data of the populations studied and preferably with an explanation.

Page 6, para 3, line 1. 96% needs definition, is this a % coefficient of reproducibility, if so this should be added to the statistical section of the methods.

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Discretionary Revisions (which the author can choose to ignore)
Page 8 para 1 line 4. This is a bold statement and might benefit from some moderation.

What next?: Reject because too small an advance to publish

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare I have no competing interests.