Reviewer 1

English language and style

( ) Extensive editing of English language and style required   
( ) Moderate English changes required   
(x) English language and style are fine/minor spell check required   
( ) I don't feel qualified to judge about the English language and style

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| --- | --- | --- | --- | --- |
|  | Yes | Can be improved | Must be improved | Not applicable |
| Does the introduction provide sufficient background and include all relevant references? | ( ) | ( ) | (x) | ( ) |
| Is the research design appropriate? | ( ) | (x) | ( ) | ( ) |
| Are the methods adequately described? | ( ) | ( ) | (x) | ( ) |
| Are the results clearly presented? | ( ) | ( ) | (x) | ( ) |
| Are the conclusions supported by the results? | ( ) | ( ) | (x) | ( ) |

Comments and Suggestions for Authors

# Summary  
  
The authors propose an algorithm to extract velocity profiles from pulsed wave Doppler traces, using image processing. The algorithm is based in a thresholding operation that aims at finding the edge of the profile.  
  
I have two major concerns: 1. The work has not been put in context of similar methods in the literature, and 2. There is no comparison to ground truth traces (e.g. from experts) so there is no indication of how accurate the method is.  
  
# Major comments  
  
The authors justify the study based on the tediousness of manual delineation. However, there is no description of how much manual input (e.g. defining RoI, baseline, etc) is there in the proposed workflow, or whether the proposed workflow is less tedious than current clinical practice.  
  
Moreover, the results are not compared to any reference, even when manual delineations could be made available sice they say that 2 specialists were involved in the study. How does the proposed method compare to the manual tracings of these specialists?  
  
The methods and the results are mixed; there is quite a bit of methodology in the results/discussion section that should be re-organised.   
  
Last, there is a relevant body of literature on detection of the Doppler trace  that outperform the Canny edge detector significantly, and should be included in this study. The proposed algorithm must be put in comparison to those. For example:  
  
Dhutia, Niti M., et al. "Open-source, vendor-independent, automated multi-beat tissue Doppler echocardiography analysis." The international journal of cardiovascular imaging 33.8 (2017): 1135-1148.  
  
Biradar, Nagashettappa, M. L. Dewal, and Manoj Kumar Rohit. "Automated delineation of Doppler echocardiographic images using texture filters." 2015 2nd International Conference on Computing for Sustainable Global Development (INDIACom). IEEE, 2015.  
  
Zolgharni, Massoud, et al. "Automated aortic Doppler flow tracing for reproducible research and clinical measurements." IEEE transactions on medical imaging 33.5 (2014): 1071-1082.  
  
Wang, Zhe, et al. "Automatic tracing of blood flow velocity in pulsed Doppler images." 2008 IEEE International Conference on Automation Science and Engineering. IEEE, 2008.  
  
# Minor comments  
  
Doppler ultrasound modes include: 1D continuous wave Doppler (CWD), 1D pulsed wave Doppler (PWD) and colour Doppler imaging (CDI). In this study, PWD is used and it should be mentioned.  
  
(Materials and Methods)   
  
In section 2, the authors describe the Doppler formula as Af = ft-fr, and then discuss about the change in period. It would be informative to relate frequency and period here.  
  
Authors state that images were acquired at 30fps. However, Doppler waves like the one in Fig. 1.b, have  a temporal axis which may be different from the BMode image acquisition frame rate. In this case, it is more appropriate to talk about temporal resolution of the Doppler wave (which may have implications in validity of derived quantities, such aa velocity gradients).   
  
In Step 2, authors describe the Doppler wave as "the Doppler shift". What the PWD trace shows is the distribution (in frequency) of the Doppler shift, due to variation in insonation angle within the resolution cell and to particles moving at different velocities and different directions. Each will show a different Doppler shift hence the imag shows the overall distribution, for each time instant, as a gray-level column. Please rephrase appropriately.  
  
Step 3 is not a processing step: retrieving the intensities does not need any calculation, just querying the image. If interpolation was done, please state so.   
  
Fig. 1: It is unclear why the histogram there is relevant and what the red arrows are pointing at. This should be indicated in the Figure's caption.  
  
Step 5 is an edge detection algorithm. As such, an algorithmic description (e.g. using pseudocode) would be a more appropriate way of describing it. Also, The reader is directed to figure 2 to follow the algorithm: this figure should be labelled consistently with the description, i.e. the horizontal axis should be Y (instead of vertical pixel) and so on.  
  
Fig. 2: Label the baseline (dashed grey line)  
  
How is the baseline detected from raw images?  
  
Fig. 3 should be put first, as an overview, then followed byu the details of each block. Also, there are many ways of carrying out image smoothing. Also, it is not clear why the smoothing block is dashed. Figure 3 should be described in its caption.  
  
(Results)  
  
Smoothing is described in the results section, but this is clearly a methods step. Re-organise the paper to include this in the methods section. Why was a moving average used (i.e. convolution with a square step kernel), instead of other, more common smoothing methods such as convolution with a Gaussian kernel?  
  
The results in Fig. 5 show agreement between the two methods, but they could be both wrong-can authors provide any validation with a ground truth, e.g. manual delineations from the 2 specialists involved in the study?  
  
Smoothing is a low-pass filtering operation, so the fact that there are fewer high-frequency components is only trivial. This should not be part of the results/conclussion.  
  
Authors also say (in the results) that edge detectors were used for comparison (Fig. 7) but no quantitative analysis was made. Canny edge detector is usually applied on smoothed images (even more so here, where authors are smoothing the datafor their own method). I believe that results in Fig 8 where obtained from the original images, without smoothing. Please, re-compute those images after smoothing.  
  
# Phrasing/typos  
  
p2 - "In a Doppler echocardiography" -> In Doppler echocardiography or In a Doppler echocardiography examination  
p2 - "using Doppler effect" -> using the Doppler effect  
p2 - "In doppler cardiography," -> In Doppler cardiography,

Submission Date

17 May 2019

Date of this review

21 May 2019 14:46:04

Reviewer 2

English language and style

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|  | Yes | Can be improved | Must be improved | Not applicable |
| Does the introduction provide sufficient background and include all relevant references? | (x) | ( ) | ( ) | ( ) |
| Is the research design appropriate? | ( ) | (x) | ( ) | ( ) |
| Are the methods adequately described? | ( ) | (x) | ( ) | ( ) |
| Are the results clearly presented? | ( ) | ( ) | (x) | ( ) |
| Are the conclusions supported by the results? | ( ) | (x) | ( ) | ( ) |

Comments and Suggestions for Authors

In this paper the authors propose a method to extract positive and negative peak velocity profiles from Doppler echocardiographic images.

The proposed approach is based on intensity calculations and two different thresholding methods have been proposed and tested.

In general, the paper is well written and clear. However, there are some minor points to address.

1.    A minor mistake is in raw 81: the range is [0,255] and not [0, 256].

2.    The authors should explain and motivate the meaning of “consistent” in raw 225.

3.    Information about the hardware and software resources used to implement the system

4.    And finally, but more important, the paper presents only some examples without any groundtruth and any quantitative measure useful to better evaluate the performance of the proposed method. A visual analysis of the results is not enough to confirm the goodness of the algorithm. At least the authors should add a visual and numerical comparison with a possible groundtruth proposed by experts.

In my opinion the paper is suitable for publication in the Journal after just the suggested minor corrections.

Submission Date

17 May 2019

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