**Detailed Response to Reviewers**

Dear all reviewers,

**Responses to Reviewer 1**

**1.**

**Reviewer’s Comment:**

General: Of course, a more throughout validation of the proposed method with clinical pathological cases would be required to state on the potential of the proposed method. Several typos were noticed, especially in Discussion, they must be corrected.

**Author’s Response:**

A subsection has been added to the discussion section to discuss the limitations of pathological applicability of these methods. Typos have been addressed, similarly to comments from Reviewer 3 and have been changed as follows:

[TYPOS CHANGED XX]

**2.**

**Reviewer’s Comment:**

P6, L77: Justify the "multiplicative" nature of wave scattering interference. This is unexpected in linear acoustics.

**Author’s Response:**

We agree that “multiplicative” is not mathematically accurate in describing the summed contributions of the scatterers that cause speckle. The description of speckle has been changed.

**Change in Manuscript:**

“Speckle occurs due to multiplicative scattering of acoustic waves in a material, resulting in a granular appearance on the image.” to “Speckle occurs from interference of randomly distributed microscopic scatterers, resulting in a granular appearance on the image”.

**3.**

**Reviewer’s Comment:**

P6, L79: This is also incorrect: "B-mode image processing attempts to remove speckle". Speckle usually refers to the stochastic nature of B-mode images.

**Author’s Response:**

We agree the statement in the manuscript was inaccurate. Image processing in B-mode is acknowledged to not specifically aim to remove speckle. We do know that logarithmic compression and frequency-dependent attenuation are used by some image processing algorithms and these nonlinear transforms make it difficult to study the original speckle. To state this more accurately than the original manuscript, the statement has been changed.

**Change in Manuscript:**

“B-mode image processing commonly attempts to remove speckle, but speckle contains information of the acoustic interactions in tissue…” to “to produce B-mode images, manufacturers often image enhancement algorithms, such as logarithmic compression, which alter the nonlinearly alter speckle patterns. However, the original speckle pattern contains information of the acoustic interactions in tissue…”

4.

Reviewer’s Comment:

P6, L82: Can we really define the absence of signal in shadows as speckle? Speckle occurs to the constructive and destructive wave interferences; if there is no signal (background noise) can it be considered as speckle? Maybe but it could be relevant to talk about "random speckle", if it is a white Gaussian noise. It would be relevant to perform a review on ultrasound background noise statistical characteristics to clarify the nature of the "random speckle"; even though it is one of the objectives of your work to define the Nakagami pdf of random speckle in shadows. It has already been addressed in the "old" literature (probably not using the Nakagami modeling but likely Rayleigh pdf).

Author’s Response:

We agree with the comment that if there is an absence of signal in shadow regions, there is no “speckle” in the sense of the interference interactions between scatters that can be observed on aa raw RF image. A clarification has been added that the differentiation in the shadow region or non-shadow regions is not by the speckle in each region but by the distribution of the scatterers. In the case of a non-shadow, we expect it to resemble ultrasonic speckle and in the case of the shadow, we expect a lack of ultrasonic speckle and be composed of electronic background noise instead.

**Change in Manuscript:**

“Speckle can then characterize different regions, such as a region of tissue or a region of signal loss in a shadow.” Has been changed to “Analyzing the presence of speckle can then be used to characterize an acoustic shadow as we expect non-shadowing regions to contain speckle resembling known statistical distributions and

shadowing regions to resemble background noise from the lack of acoustic interactions with scatterers.” [XX need some background on ultrasound noise”

5.

Reviewer’s Comment:

P7: Eq. 1 is adequately described but most ultrasound scientists are presenting the shape parameter as the "m" parameter. It may be relevant to clarify that "the shape parameter is also known as the Nakagami "m" parameter". Parametric images of the "m" parameter have been widely described. For Eq. 1, why not using the same nomenclatures as the cited reference?

Author’s Response:

To be consistent with literature, the shape parameter description has been changed.

**Change in Manuscript:**

*“µ* is a shape parameter” has been changed to “m is the shape parameter or Nakagami “m” parameter”.

6.

Reviewer’s Comment:

P7, L108: "width of a single RF data point and length of three time the pulse length". Normally one uses window sizes defined by the 2D correlation length of speckle to avoid redundant statistical characteristics. What is the width of a single RF data point considering the fact that standard beamforming was likely performed and two transducers were used? Can you define the number of RF lines in your images; which may correspond to what you defined as "a single RF data point"? Because you used a curvilinear transducer, the width of "a single RF data point" changed as a function of depth if it corresponds to the distance between RF lines.

Author’s Response:

[xx should I change it to pulse length??]

7.

Reviewer’s Comment:

Results based on the scale parameter of the Nakagami pdf are reported (the scale parameter being related to the echo scattered power); what about the shape parameter defining the signal-to-noise ratio? This parameter turned out to be of no value to define shadows?

Author’s Response:

The Nakagami shape parameter was found to not be a consistent indicator of shadow or non-shadow. In many images. The signal-to-noise ratio was observed to be higher in most non-shadowing regions compare to most shadow regions, however, the range of shape parameter values in shadow regions overlapped with non-shadowing regions too much for Otsu’s method to provide a threshold to accurately separate the two regions. Empirically, the scale parameter was sufficient in combination with Otsu’s method to achieve the accuracy reported and the shape parameter was unused any further. To clarify this, a figure has been added showing the bi-modal distributions of both the shape and scale parameters in shadow and non-shadow regions, which show the significant overlap in the shape parameter between the two regions. In addition, a line has been added to comment on this, “The Nakagami shape parameter, ‘m’, was also investigated, though there was not sufficient delineation between parameter values in shadow and non-shadow regions for this parameter to be effective in thresholding. The distributions of the two parameters are displayed for shadow and non-shadow regions in Figure [XX]”

(This figure is also addressed and shown in response to comment 15)

8.

Reviewer’s Comment:

P8, L127-129: Can you clarify that the same window sizes as the RF data analysis was used here for B-mode entropy calculations? It seems to be the case.

Author’s Response:

A line has been added to clarify this.

Change in Manuscript:

“with the window size fixed as three times the pulse width, $\eta$, as defined has been changed to “with the window size fixed as three times the pulse width, $\eta$, as defined in Eq. 2. This is the same window size as the RF analysis.”

9.

Reviewer’s Comment:

P8, L138: By "intensity of pixel" are you referring to the gray level between 0-256?

Author’s Response:

Yes, we acknowledge the poor wording of the original phrase. The statement has been changed to address the gray level.

Change in Manuscript:

“is the intensity of pixel” has been changed to “is the gray level, or intensity, of pixel”

10

Reviewer’s Comment:

Results: To compute RF and B-mode parametric maps on which thresholds were applied, did you use a single image frame or several frames to increase the robustness of the methods?

Author’s Response:

Yes, three frames were averaged for the method. A clarification has been added to state this.

**Change in Manuscript:**

“This was performed on an averaged RF signal from three image frames.” has been added and “B-mode analysis was performed on an averaged image from 3 image frames, similar to RF analysis.”” Has been added.

11.

Reviewer’s Comment:

Two transducers were used for this study (3.3 and 11 MHz upper frequencie s, approximately). It is concluded that the shadow detection method is robust to system settings and transducer frequency used. According to the paper listed below, Nakagami statistics are influenced by the transducer frequency over a range of 10-58 MHz. Can you conclude that the proposed method would remain valid over a wider range of frequencies? This should be discussed.

Author’s Response:

We sincerely thank and appreciate the reviewer for providing their expertise and the literature source to strengthen our understanding and limitations of the methods. The equipment available did not provide lower than 3.3MHz or higher than 13.3MHz. From the provided paper, the 36MHz and 58MHz frequencies used displayed a large transient drop in the Nakagami coefficient, and stabilizing to a Nakagami parameter value below the 10MHz frequency. The provided paper concluded that in the 36-58MHz range, the “neither the K or Nakagami distirbutions adequately fit the experimental results” [2]. This would provide difficulty in detecting acoustic shadows with the Nakagami distribution in the higher frequency range and we cannot conclude that the Nakagami modelling can be accurate for shadow detection. In most clinical uses, frequencies range from 2-15MHz [3] (except in biomicroscopy in optical scans where frequencyes range 50-100MHz, though shadows are not expected here [4]) and the higher frequency deviations from Nakagami distributions may not be a significant issue. Regardless, this limitation is acknowledged and has been added in the discussion section. The two references in this response have also been added to the references section.

(References not added to manuscript)

[4] Ultrasound Biomicroscopy in Plateau Iris Syndrome. Pavlin, Charles J. et al. American Journal of Ophthalmology , Volume 113 , Issue 4 , 390 – 395

Changes to Manuscript:

Added to the discussion section “There is a limitation with analysis using the Nakagami distribution in that the Nakagami distribution modelling scatterers change depending on transducer frequency. Previous literature observed that the 36-58MHz frequency range, the Nakagami $m$ parameter decreased near the theoretical lower limit compared to a higher Nakagami $m$ parameter value at 10MHz signal (Cloutier et al. 2004.) This was reported to be due to the spatial organization of the cells being "on the order of a fraction of the wavelength" and a Nakagami distribution cannot model the scatterers of red blood cells at this frequency. Due to this and from limitations of the equipment used in our study, we cannot conclude that shadow detection with Nakagami analysis will be accurate in higher frequencies beyond the values tested. Future studies are required to analysis the performace of shadow detection in higher frequencies. Diagnostic ultrasound commonly uses a frequency range of 2-15MHz [Jensen et al. 2007] and the shadow detection method is expected to not be applicable in most use cases without issues from the high frequency behaviour of the Nakagami distribution.”

Added references:

[XX] Cloutier, G., Daronatand, M., Savé Ry, D., Garcia, D., Durand, L.-G., & Foster, F. S. (2004). Non-Gaussian statistics and temporal variations of the ultrasound signal backscattered by blood at frequencies between 10 and 58 MHz. Acoustical Society of America, 116(1), 566–577.

And

[xx] Jensen, J. A. (2007). Medical ultrasound imaging. Progress in Biophysics and Molecular Biology, 93(1–3), 153–165. https://doi.org/10.1016/j.pbiomolbio.2006.07.025

12. Reviewer’s Comment:

P11, L212: Reverberation artifacts were not considered in this study. A strong mismatch in acoustic impedance usually results in reverberation depending on the insonification angle. Those reverberations would be superimposed over shadow regions. Please consider this issue and provide explanations why your methods would work in these conditions.

Author’s Response:

We agree that reverberation occurs and a “shadow region” can exhibit bright reverberations and that this needs to be addressed for our method . An explanation has been added in the discussion section that the method, which looks for the beginning of a shadow boundary, takes into account the directional information of ultrasound, which will avoid “false negatives” of detecting non-shadow below a shadow. Once the algorithm detects a shadow boundary, the axially deeper regions are considered shadow even with reverberation artifacts. Figure 2a) shows a scan with a reverberation artifact and has been referenced in the discussion section as well.

Changes in Manuscript:

Added to the discussion section “As both RF and B-mode images search for a threshold for the start of a shadow, it is possible to misinterpret a beginning of a shadow as a reverberation artifact. Reverberation at a shadow boundary would cause a similar bright region followed by a dark region, which visually appears like a shadow boundary despite being an artifact in a shadow region. This is addressed by considering directionality when searching for the start of a shadow boundary such that the first shadow boundary when traversing down a scanline is interpreted as a beginning of a shadow and any further shadow boundaries are interpreted as reverberation artifacts. Figure 2 shows shadow detection with a reverberation artifact underneath a shadow caused by the radial joint.”

13. Reviewer’s Comment:

Results: Because different settings and transducers were used, thresholded RF and B-mode parametric images likely differed. Provide thresholded values used for all reported results. How did you determine those thresholds? You likely used the same training set and test set and you likely optimized the thresholds for each type of images, settings and transducers to minimize the classification error or optimize the Dice coefficient; this is not clear. How would be blindly select the threshold for a new image? This is an important issue that should be addressed and included in a Limitation Section.

Author’s Response:

We acknowledge that the description of thresholding is not clear. It is true that the threshold for any image is different, though no training was used to determine an optimal threshold. The thresholds were automatically computed for each individual image, with no manual tuning for each image. Once a Nakagami scale map or entropy map was computed, the map automatically processed by Otsu’s method to determine a threshold value for the Nakagami scale parameter or entropy, assuming that there are shadows, which we expect to result in a bi-modal distribution for both maps. Clarification has been added to the methods seciton.

Changes in manuscript:

Changed “To detect shadows, Otsu's method was applied on the entire image to automatically compute a threshold for the Ω parameter.” to “Then, for each ultrasound image, Otsu’s method was applied to its Nakagami Ω map to automatically compute a Ω threshold for each individual image as we expect separate distributions for shadow and non-shadow regions.”

Changed “Next, Otsu's method is applied similarly to compute a threshold entropy value” to “Next, Otsu's method is applied onto the entropy map of each image to automatically compute a threshold entropy value, similar to RF analysis.”

14. Reviewer’s Comment:

In discussion, clarify the challenges that would be required to apply your methods for diagnostic purpose where shadowing is often indicative of a severe pathological condition. Your study did not consider small shadows produced by calcium nodules; how would you address this?

Author’s Response:

The reviewer’s is important as a motivation for shadow detection was to detect pathological conditions that may exhibit acoustic shadowing. Large shadows from gall and kidney stones exhibit shadowing behavior similar to the dataset study - one large black streak following a hyperechoic boundary. For these cases we expect shadow detection to be applicable as the methods have been designed to target patterns similar to these shadows. Small calcifications are inconsistent as some exhibit the characteristic shadow streak and some exhibit brighter regions even after the boundary, such as in placental ultrasound where calcifications are important to detect [5]. In these cases, diagnosis by automated shadow detection may not be dependable. A comment was added that the design of the study did not allow for data collection from clinical cases of pathological conditions exhibiting acoustic shadows and should be done in the future to continue validation of these methods.

[5] Abramowicz, J. S., & Sheiner, E. (2008). Ultrasound of the Placenta: A Systematic Approach. Part I: Imaging. Placenta, 29(3), 225–240. https://doi.org/10.1016/j.placenta.2007.12.006

Changes in Manuscript:

Added “There is a limitation for diagnostic usage of the proposed shadow method in cases where acoustic shadowing does not exhibit the characteristic bright boundary followed by a dark region. In cases where there is partial or incomplete shadowing, such as small calcifications in the placenta (Abramowicz et al. 2008). In these cases, there is a resemblance of a shadow, where the calcification is brighter and the region below is noticeably darker, but not with a brightness difference as extreme as shadowing from the ulna and the regions below retain speckle similar to tissue. Although calcifications are pathologically important to recognize, the proposed shadow detection method would likely be unable to detect the partial shadowing from these calcifications. The proposed method would be applicable only in cases of more complete shadowing, which would still be practical for significant gall and kidney stones, for instance.”

15. Reviewer’s Comment:

P13, L253: Indicate the pulse widths that were used as priors in your models (maybe not in Discussion but in Methods).

Author’s Response:

Pulse widths have been added to the methods section to improve clarity.

Changes in Manuscript:

Added “The pulse widths were XX for the linear and XX for the curvilinear transducers.”

15. Reviewer’s Comment:

P13, L259: You are referring to the speckle distribution; this brings the following point. In many applications in the literature, speckle statistics were modeled as single or mixture of probability density distributions for segmentation purpose or tissue characterization. Because knowing the probability distribution of shadows might be important for those applications, it would be important to report histograms of Nakagami parameters and entropy within the segmented shadow areas. Giving mean +/- SD is not sufficient.

Author’s Response:

We agree that mean +/- SD is not sufficient to portray the distribution and behavior of the Nakagami parameters. We thank the reviewer as the suggestion would certainly help presenting the data better. A figure has been added showing the histograms of Nakagami shape, Nakagami scale, and Nakagami entropy parameters.

Changes in Manuscript:

Added the following figure:

Added caption:

Histograms of investigated parameters in different regions including the a) Nakagami *Ω* parameter for shadowing and non-shadowing regions, b) the Nakagami *m* parameter for shadowing and non-shadowing regions, and c) Entropy for shadow boundary and non-boundary regions. The Nakagami *Ω* has a more noticeable separation between shadowing and no-shdoawing region compared to the Nakagami *m* parameter. The entropy distribution also has a noticeable separation between boundary and non-boundary regions, motivating the detection of shadows by thresholding the Nakagami *Ω* and Entropy parameters.

Reviewer’s Comment:

Acknowledgments: Because the manual shadow detection is your benchmark measure, it is surprising that this was not considered in the authorships.

Author’s Response:

The acknowledged trainer was indeed an important contributor and provided valuable from sonography experience. The authorships were determined by any extended effort in data acquisition, novel contributions to the method, and supervision. The acknowledged manual detection trainer did not manually segment the all the images and provided training for two hours to the authors for manual segmentation and was not considered for authorship.

Changes in Manuscript:

No changes made regarding this comment.