Review of TUFFC-05633-2013

I. Summary

The authors suggest a combination of two MV beamforming techniques; the beamspace adaptive beamformer and the eigenspace adaptive beamformer. This combination achieves increased contrast (like the eigenspace beamformer) with reduced computational complexity (compared to MV).

Establishing a high-performance, low-complexity method is both scientifiically interesting and practically desirable. Unfortunately, the paper has some unfortunate flaws because the authors have not correctly identified all the effect of their new method. My current impression is that the actual interesting aspects of the new methods are not enough to warrant publication as a full paper, but the conclusions could be rewritten as a letter or a correspondence if certain changes and better comparisons are made.

II. List of concerns

1) The authors state that "(...) contrast improvement has not yet been satisfactory". Where does this conclusion come from? Why was resolution improvement deemed satisfactory, while contrast improvement was not?
2) If the motivation of the authors is to find a method that has the resolution of MV and improved contrast without significantly increased computational complexity, the interesting alternatives are found in the world of coherence-based beamformers (where MV is combined with a scalar, SNR-dependent coherence factor). The authors must either state (convincingly) why these methods have not been considered, or include them in the simulations. To leap ahead a bit, I am already convinced that MV/CF-based methods are sufficiently related to "pure" MV methods that a comparison is warranted in this context.
3) In Sec. III-C, the authors claim that reducing the number of rows in $\mathbf{T}$ from L to P will reduce the complexity of the eigendecomposition from $O(L^3)$ to $O(P^3)$. However, the eigen-decomposition problem actually remains unchanged, independent of the beamspace reduction that is applied afterwards. This is easily seen as the matrix $\mathbf{U}_s$ in (22) is the same as that in (10), and must be acquired the same way with an identical complexity of $O(L^3)$. In other words, the only complexity reduction in B-ESBMV is due to the calculation of $\vec{w}_{B-MV}$, which has already been established in [15]. Therefore, it is not at all surprising that B-ESBMV executes quicker than MV. What would be surprising is if it executed faster than B-MV.
4) The MV result in Fig. 7 b) should be explained; why are the noise/interference levels for MV (and also sometimes for ESBMV) seemingly higher than for DAS? Is this related to image normalization?
5) The authors must get their manuscript proof-read by a native English speaker. There are quite a lot of spelling errors, clumsy sentences, and formulations that must be corrected. There are too many obvious examples to go into further detail (like "passed without distortionless" $\rightarrow$ "passed without distortion").
6) The statement "(...) DAS beamformer has a wide mainlobe and high sidelobes" is somewhat unclear. An adaptive beamformer cannot cheat the fundamental mainlobe-width/sidelobe-level tradeoff, but they can rather use all degrees of freedom in an optimal manner. I obviously see that the authors are trying to express that "DAS is limited", but they should find another way.
7) The adaptive beamformers are perhaps "conserving", not "enhancing" useful signals while suppressing interference. Enhancement of useful signal content in the output is done through interference suppression, not as a separate process.
8) Is lowering the computational complexity of a method the same as making the method easier to implement?
9) "always" is a somewhat strong modifier in the sentence "(...) study of low complexity adaptive beamformer has always been an area of great interest (...)". I assume the authors mean after the introduction of adaptive beamformers to ultrasound (even then, it took a few years to catch on).
10) The reason for discarding Synnevg's low-complexity data-dependent beamformer because it depends too much on the predefined weights and is hard to handle in practical use is both vague and undocumented. If the authors know of studies that indicate these shortcomings, they should cite them. Otherwise, they should stick to valid reasons for discrediting existing alternative techniques, or refrain from discrediting them at all.
11) While I agree with the authors that the constrasts of the ESBMV beamformers in Fig. 6 are superior to DAS and MV, I would challenge them with the fact that the shape of the cyst is clear and the edge is easy to judge for all six images, including DAS and MV. Could some more extreme scenarios be used as examples?

III. Conclusion

In my opinion, all of the above points can be corrected with the possible exception of two important points:

- The suggested method might be out-performed wrt. both contrast and computational complexity by well-known coherence-based methods. This should be investigated.
- The suggested method does not actually contribute any non-trivial reduction in computational complexity than what is already given by the beamspace dimensionality reduction of B-MV, which has already been published and established. If so, then it is a trivial combiantion of two existing methods, constituting a (brief) correspondence at best.

The authors should first of all approach these two points, and if they are successful correct the remaining points to change my mind about publication.