Reviewers Comments & Authors Replies

Manuscript No.	Paper T-SP-18051-2014, submitted to *"IEEE Transactions on Signal Processing"*
Title	"Traffic Aware Resource Allocation Schemes for Multi-Cell MIMO-OFDM Systems"
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The authors would like to thank the associate editor and the reviewers for their valuable comments on the manuscript of the paper, which have been greatly helpful to improve the paper quality. Based on the comments, we have made several major revisions to the paper, following the suggestions of the reviewers. In what follows, the comments are listed, each followed immediately by the corresponding reply from the authors. The revisions in the revised manuscript are highlighted using blue color and the authors responses are also presented in blue color text. Following is the summary of the revision made on the manuscript in accordance with the reviewers comments.

1. We have provided additional information to improve the continuity in the algorithm formulation from the SINR expression as suggested by the reviewers.

2. We have included the discussion for the MSE based reformulation for different norms in the objective function.

3. We have rewritten the reduced complexity spatial resource allocation (SRA) in Section III-D for better readability.

4. We have shortened the ADMM approach as suggested by the reviewers in Section IV-B.

5. Rigorous convergence analysis is provided for the centralized algorithm in Appendix A on the supplementary document.

6. We have included Section V-C on the queue behavior over multiple time instants. We have added Fig. 4 to show the performance of the proposed scheme over the existing precoder design algorithms.

7. We have addressed the issues in the citations and also provided additional references to prove the convergence of the centralized algorithm as suggested by the reviewers.

Due to the strict page limitation imposed on the resubmitted manuscript, we have included all Appendices on the supplementary document in the manuscript central. In what follows, the comments are listed, each followed by the corresponding reply from the authors. Unless otherwise stated, all the numbered items (figures, equations, references, citations, etc) in this response letter refer to the revised manuscript.

Response to Reviewer - 1's Comments

Comments: The response to the reviewer's concerns are generally satisfying, except the convergence proof.

We thank the reviewer for providing valuable and insightful comments.
For the resubmitted manuscript, the reviewer still has the following concerns

1. Considering the length of the manuscript, it would be better to shorten some parts that are not new in this manuscript, e.g. III.A. More space can be left for convergence proof, which is very important.

Reply: We thank the reviewer for the comment.

2. In convergence proof (48), why does the 2nd inequality hold? In fact, to prove the feasibility of $m_{k+1}^{(i)}, w_*^{(i-1)}; m_k^{(i)}$, the part between 2nd and 3rd inequality is not necessary, "¡=0" directly follows the 2nd inequality since the solution $m_{k+1}^{(i)}, \gamma_{k+1}^{(i)}$ is the optimal solution, and therefore feasible.

Reply:

3. The solutions SCA iterations $m_k^{(i)}$ does not necessarily converge. In fact m has compact feasible region, and thus $m_k^{(i)}$ has limit points for any specific i. However $m_*^{(i)}$ does not necessarily exist(the whole sequence $m_k^{(i)}$ may be not convergent). Similar problem happens to $w_k^{(i)}$.

Reply:

4. Strict monotonicity with respect to the objective function f should be rigorously proved. Note that to guarantee the uniqueness of the beamformer iterates, (52) instead of the objective function is used.

Reply:

5. Note that the conclusions [32, Thm 2] and [26, Thm 10] have lots of assumptions. To invoke these reference, explicit exposition should be provided to show that these conclusions can be applied to our problem. The same questions occur to the proof in Appendix B, where conclusions in [11] [36] and [37] are used. Too many details are omitted to make the proof convincing and clear.

Reply:

Response to Reviewer - 2's Comments

The authors have addressed many of my previous comments. However, there are still several major issues that need further clarification.

We would like to thank the reviewer for providing valuable comments.

1. The revised paper did not address my previous comment about how to select the sub-channel ordering. I understand that finding the best sub-channel ordering requires exhaustive search which has extremely high complexity. But it is important to provide a guidance on what would be a good choice of sub-channel ordering. For example, can we achieve a good performance by using a low complexity ordering algorithm such as a greedy sub-channel ordering algorithm?

Reply:

2. The authors mentioned that the signaling overhead of the distributed algorithm can be reduced by using a smaller number of iterations $J_{\max}$. But still, you didn't answer my question about whether the signaling overhead of the distributed algorithm is smaller than the centralized algorithm. You should first analyze the signaling overhead of the distributed algorithm for fixed $J_{\max}$ and the signaling overhead of the centralized algorithm. Then you should point out under what $J_{\max}$ the distributed algorithm will have less signaling overhead than the centralized algorithm. Is it possible that the distributed algorithm always has more signaling overhead than the centralized algorithm even when $J_{\max} = 1$? Finally, there is a tradeoff between performance and signaling overhead ($J_{\max}$) for the distributed algorithm. For the same signaling overhead (we can control $J_{\max}$ to make the signaling overhead of the distributed algorithm approximately equal to that of the centralized algorithm), does the distributed algorithm achieve better performance than the centralized algorithm?

Reply:

3. If the authors can't prove the convergence of the ADMM algorithm (or the decomposition approach via KKT conditions) in Section IV.B, then at least, you should discuss the property of the fixed point of the algorithm. For example, does there exist a fixed point of the algorithm? If so, is the fixed point of the algorithm unique? Is any fixed point of the algorithm also the optimal solution of the original problem in (20)? Assuming that the ADMM algorithm converges to a fixed point, will the interference vector in (39) converges to the actual interference in the network? These questions must be clarified in the paper. Otherwise, it is not clear how the ADMM algorithm is related to the original problem in (20). Similar questions should also be answered for the decomposition approach via KKT conditions.

Reply:

Response to Reviewer - 3's Comments

The authors have introduced changes in the manuscript that improved the paper's quality. Additionally, the authors have taken into account the reviewers' comments giving clarifications and modifying the content when required. More specifically, the following aspects have been treated:

We thank the reviewer for reading the manuscript and providing valuable comments. The comments are really helps to improve the manuscript.

1. Convexity of problem (16). The paragraphs surrounding (16) allow a better understanding of the usage of the additional variables, i.e. gamma and beta, to remove the equality constraint in (2). For the reviewer remains however unclear, the procedure/criterion to determine the operating point for the parameter $\tilde{\beta}$ required in (19) and used in the convex subproblems (20) and (21).

Reply:

2. Proof of convergence. The proof of convergence introduced by the authors in the Appendix seems correct and enhances the content of the manuscript.

Reply:

Additional Comments -

(a) - The reviewer considers that closing statements regarding the applicability of the proposed schemes are missing. Since the results are quite similar (when not identical), which formulation is preferable between the centralized schemes? Which one for the distributed solutions?

Reply:

(b) - The last discussion in section IV-C could benefit from restructuring. The information on how to obtain a practical distributed precoder design and to avoid backhaul exchange is too condensed and difficult to understand.

Reply:

(c) - For the simulation results, why not to unify configurations when possible? Having to read a different configuration for each graph is cumbersome and no additional comparisons are possible between figures. E.g. PL uniformly distributed between [0,-6] dB in Fig. 2 and [0,-3] dB in Fig. 3.

Reply:

(d) - In Fig. 1, the description of the system model does not agree with the statement of N = 3 subchannels.

Reply:

(e) - In Fig. 4(b), the performance of Q-WSRME seems to be (in average) worse than Q-WSRM. However, that should not be the case, since Q-WSRME is taking into account the over allocation. Any reason for this?

Reply:

(f) - p 6, col 2, row 49: it should be $t_{l,k,n}$ instead of $t_{l,n,k}$

Reply:

(g) - p 10, col 1, row 28: is it λ a dual variable?

Reply:

(h) - p 10, col 2, row 59: typo wHith

Reply:

(i) - In general, a grammar check is recommended, several mistakes with respect to singular and plural nouns have been observed, e.g. -p 6, col 2, row 56 "... for A fixed receiverS" is not correct.

Reply:

Response to Reviewer - 4's Comments

This reviewer's concerns have been addressed, and this manuscript is now deemed fit for publication.

First, we thank the reviewer for providing valuable and insightful comments. The comments are really helpful in improving the manuscript.