Referee Report on "The Q-Method for Symmetric Cone Programming" by Yu Xia and Farid Aliadeh.

This paper attempts to provide a global convergence analysis for the Q-method (originally proposed for SDP by Alizadeh-Haeberly-Overton) for symmetric cone programming by borrowing ideas from the analysis of the infeasible interior-point algorithm for LP due to Kojima-Megiddo-Mizuno (KMM). A second, but similar, algorithm based on a further modification of the central-path neighborhood is shown to rectify the problem of unbounded iterates in the first algorithm. The second algorithm has some potential for warm starting.

Overall, the problem addressed is a relevant and important one, the paper is readable (with room for improvement in the exposition), and the references are comprehensive. However, the authors have glossed over important technical issues, hence trivialising, in some sense, an otherwise difficult and challenging convergence analysis. The paper is not publishable until these issues have been satisfactorily resolved.

Main Issues.

1. In the middle of page 26, the authors casually assume that the search direction is uniformly bounded. But this is a key step that has to be proved, not assumed! This boundedness claim is simply not true even from a 'regular' point (unless you impose some notion of 'sufficiently regular' that holds uniformly over all iterations — certainly no reason to expect this to be true.) In fact, a typical convergence analysis would require a stronger result, namely a quantitative (up to order) bound on the search direction in terms of μ, the duality gap.

2. Remark 3.1 is not as innocuous as it seems. Cutting the step length to maintain regularity has to be done carefully for at least two reasons. (i) The bound η depends strongly on the distance to irregularity. It is crucial to quantify this since η appears so prominently in the rest of the analysis. (ii) Regularity is complicated nonlinear function of steplength. A large step length may bring an effective decrease duality gap and infeasibility, but result in a more irregular iterate from which only poor progress may be possible in the next iteration. A step length selection strategy must therefore carefully balance these criteria. The suggested approach of simply cutting step length by a fixed fraction is likely to be inadequate.

3. Even if this simple step length selection strategy were adopted, there is nothing to prevent the step lengths from approaching zero. (The paper's claim to the contrary seems fallacious.) It is possible to conceive of a pathological scenario where a bad iterate results in a bad search direction along which a step length of, say, 1/2 is taken. The next iterate, also a bad one, could result in (nearly) the same search direction along which a step of only 1/4 may be possible, and so on, thus producing step lengths convering to zero. This is an unlikely situation, but there is nothing in the analysis to preclude it.

On the whole, the paper lacks a rigorous quantitative analysis that cleanly establishes how the chosen neighborhood leads simultaneously to bounded search directions, sufficient decrease in duality gap and infeasibility, all the while ensuring that iterates stay sufficiently regular. Loose hand-waving arguments result in the peculiar and unconvincing convergence result stated in Theorem 4.1.

Other Issues.

4. Below (2): "forms a square system". With the complementarity condition stated as $XZ = 0$, this is not a square system.

5. Page 3, 3rd line: "second order asymptotic rate of convergence may be lost". This sentence is somewhat misleading. While it's correct that Newton's method is applied to a different function in each step (hence quadratic convergence is not an obvious consequence of Newton's method), it is true that many of these families of algorithms enjoy quadratic or superlinear convergence under suitable assumptions.

6. Section 2: This 15 page review of Euclidean Jordan algebras is tedious and distracts from the main point of the paper — the convergence analysis. It should be possible to distill only the essentials (perhaps even in an appendix), with appropriate references to Faraut & Koranyi or Koecher's lecture notes for the rest. In fact, it suffices to have a clean analysis for just SDP; the rest is a straightforward generalization with the Jordan algegra machinery.

7. Page 14, 3rd line: If the dilation group simply acts by positive scaling, I don't see why Jordan frames aren't preserved.

8. Example 18: $GL_n \otimes GL_n$ is obviously not isomorphic to GL_n. You need better notation here to denote the set of transformations $X \to PXP^T$ with $P \in GL_n$.

9. Lemma 3.3: You could mention that this generalizes the result of Alizadeh-Haeberly-Overton for SDP. Also, it's not clear how this result fits into the convergence analysis. You claim at the end of the proof that the Q-method is therefore numerically stable near the optimal solution provided nondegeneracy, strict complementarity and regularity holds at the solution. Yet, the convergence analysis doesn't particularly rely on any of these properties.

10. Theorem 4.1: This is an unusual convergence theorem. When could the condition $\|F^n w\| \geq d > 0$ be violated? Shouldn't the algorithm be fixed somehow so that this condition is always satisfied?

11. Page 28, 7th line from the bottom: How do you define $\|(F^{m_i})^{-1}\|$ for the nonlinear function within the norm?

12. Section 4.3: How do iterates get unbounded? With Slater's condition, the solution sets are bounded. Theorem 4.1 shows convergence in a finite number of steps. Thus it seems that the condition in Theorem 4.1 could be violated, and that this would lead to unbounded iterates, although this connection is not very clear. At any rate, it's disconcerting to encounter the possibility of unbounded iterates immediately after a convergence theorem was proved!

13. Section 5: The interesting experiments for the Q-method are not randomly generated problems, but ones that involve eigenvalue coalescence at the optimal solution. Since the convergence analysis doesn't assume regularity at the solution, you could study how the algorithm performs in this case, or when started close to an irregular point. Numerical results on SDP's would be especially instructive. If the second algorithm does indeed have some potential for warm starts, numerical evidence to this effect is required.

A few typos are scattered throughout the paper, but the items above need to be addressed first.