Referee Report: Theory of Semidefinite Localization for Sensor Network Localization

I am pleased that the authors, in revising their manuscript, having attempted to provide a more comprehensive survey of the relevant literature. Unfortunately, the current version contains several embarrassing misstatements. In the hope that the next revision can be accepted, I offer the following specific objections and antidotes:

1. Having noted the seminal work of Schoenberg [4] and Young and Householder [9], which established a characterization of Euclidean distance matrices (EDMs) in terms of positive semiefinite matrices, the authors then state:

    > "Such a characterization forms the basis of various multidimensional scaling algorithms (see, e.g. [14, 31, 32])."

    This sentence is misleading in two ways:

    (a) It is too broad. It would be more accurate (and no less efficient) to state that "Such a characterization forms the basis for the classical approach to multidimensional scaling..."

    (b) The citations are poorly chosen.

    i. The authors' reference [14] is an expository book that surveys various approaches to multidimensional scaling (and related techniques). Section 2.2 is devoted to classical scaling, but the book emphasizes other approaches that are not based on the relation of EDMs to positive semiefinite matrices. If the authors feel that they must cite this work, I suggest something along the lines of "See Section 2.2 of [2] and the references therein..."

    ii. For classical multidimensional scaling (CMDS), the seminal references are [5] and [3].

    iii. For reasons discussed below, the authors' references [31, 32] (Trosset's work on extensions of CMDS) are more relevant to the present work than can reasonably be inferred from this passing reference. In this sentence, I think that it suffices to cite [8], which unifies separate extensions in [6] and [7]. Of these, [6] is more closely related to traditional nonmetric approaches to multidimensional scaling, while [7] is concerned with EDM completion and the problem of missing data.

2. The authors then state:

    > "However, there is no guarantee that these algorithms will find a realization in the required dimension if some of the pairwise distances are missing."

    This sentence is also misleading:

    (a) It conveys the impression that the authors do not understand the nature of multidimensional scaling, the purpose of which is to construct configurations of points in a specified number of dimensions. In contrast to techniques that attempt to realize pairwise distance information exactly in as many dimensions as needed, multidimensional scaling techniques approximate pairwise distance information in the required number of dimensions.

(b) While it is true that many multidimensional scaling techniques cannot accommodate missing data, there are many that can. In particular, if by "these algorithms" the authors mean the algorithms cited in the previous sentence (the natural interpretation, in my opinion), then the authors are simply wrong: the whole point of [7] is that it extends CMDS, which cannot accommodate missing data, to a technique that can!

3. The authors subsequently state:

> "In addition, Alfakih and Wolkowicz [4, 5] have related this problem to the *Euclidean Distance Matrix Completion* problem and obtained an SDP formulation for the former. Moreover, Alfakih has obtained a characterization of rigid graphs in [1] using Euclidean distance matrices and has studied some of the computational aspects of such characterization in [2] using SDP. However, these papers mostly address the question of realizability of the input graph, and the analyses of their SDP models only guarantee that they will find a realization whose dimension lies within a certain range. Thus, these models are not quite suitable for application. In contrast, our analysis takes advantage of the presence of anchors and gives a condition which guarantees that our SDP model will find a realization in the required dimension."

This is the crucial passage in which the authors characterize the nature of their contribution, and it's fine as far as it goes. However, it omits some important information, thereby conveying a somewhat distorted sense of the literature.

(a) The authors' references to work by Alfakih and collaborators are extremely relevant, but the crucial paper in this body of work is [1]. It certainly should be cited. (It is [3] on the authors' list of references, but was not cited in the quoted passage.)

(b) It is misleading for the authors to build their case on the deficiencies of algorithms for EDM completion without explicitly mentioning [7]. Consider the following:

i. Most of the work of EDM completion is graph-theoretic. I know of exactly two papers that propose numerical algorithms for EDM completion, [1] and [7].

ii. Both [1] and [7] were published in the optimization literature; in fact, both appeared in *Computational Optimization and Applications*, in consecutive years.

iii. Both [1] and [7] exploit the connection between EDMs and positive semidefinite matrices.

iv. The approach in [1] uses an SDP model but does not find a solution in a specified number of dimensions. In contrast, the approach in [7] finds a solution in a specified number of dimensions, but does not use an SDP model. Hence,

v. From the perspective of EDM completion, the present contribution is an algorithm that uses an SDP model *and* finds a solution in a specified number of dimensions. However the authors may have originally conceived their contribution, and whatever exposition they may have chosen topresent it, what results is sufficiently symmetric with respect to [1] and [7] that the connection to each should certainly be acknowledged.

References

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