REPORT ON THE REVISED MANUSCRIPT
"ON WEIGHTED MULTIVARIATE SIGN FUNCTIONS"

I appreciate authors' detailed response to my comments, as well as the work invested in the revision. It is clear that the quality of presentation and clarity has improved, however I still believe that significant changes are necessary.

My main concern is that authors still insist on using the true, unknown distribution $\mathbb{F}$ in the definition of the estimators (for instance, on page 2, it is stated "for clarity...we fix $\mathbb{F} = \mathbb{F}_X$..." While this statement on page 2 is followed by the correct remark that $\mathbb{F}$ is typically unknown and is replaced by the empirical distribution $\mathbb{F}_n$, I can't understand why authors do not follow a natural approach and use $\mathbb{F}_n$ everywhere to begin with, as it is common in statistics in general. Although it is claimed that $\mathbb{F}$ can be replaced by $\mathbb{F}_n$ under "very standard regularity conditions," authors do not rigorously verify these regularity conditions (labeled B1-B3 in the paper) and simply state that "these properties are easily satisfied for weight functions derived from standard depth functions..." I respectfully disagree with the fact that the verification is easy, as it involves uniform law of large numbers-type requirement (B2) which often requires non-trivial justification. I encourage the authors to expand the discussion of the main assumptions (including A1-A3 and B1-B3), and include more concrete and justified examples where these technical conditions are satisfied.

With the approach taken by the authors, results such as Theorem 2.1 have little value: why are we interested in the properties of a weighted spatial median that uses the true distribution of the data as an input? And it was not immediately clear to me that this result remains true if $\mathbb{F}$ is replaced by $\mathbb{F}_n$, the empirical distribution function.

Finally, it was not clear to me why should one use the proposed dispersion estimator $\hat{\hat{\Sigma}}$? How does it compare to existing alternatives, say Maronna's and Tyler's estimators? And why is Corollary 3.5 useful – can we say anything concrete about ARE here?

Some additional questions and remarks are given below.

(1) On page 1, you use μ_x depending on x to define S_i. What is x here, and how should one choose it? Also, could you comment on the choice of "suitable centering parameters $\tilde{\mu}$ and μ_x" on page 1?
(2) Page 2: why $R(X_i; \mu, \mathbb{F}) = S_i$ if $W(x, \mathbb{F}) = 1$? It was not clear to me.
(3) Result of Corollary 2.2 should be scale-invariant, in a sense that multiplying the weight W by a constant should not change the right-hand side of the inequality. However, it seemed to me that the numerator and the denominator scale differently which I could not understand. Moreover, why the condition $k/\lambda^2_{\min}(\Psi_{2W}) < \lambda_{\min}(\Psi_1)/(\lambda_{\max}(\Psi_{1W})\lambda^2_{\max}(\Psi_2))$ is easy to satisfy? Can you present concrete examples of weights with this property? In the next paragraph, you say that choosing $k = \exp(-p^2)$ allows to make asymptotic relative efficiency as large as possible. However, we can always scale bounded weights to satisfy this condition, which appears to be too good to be true.
(4) In Corollary 2.4, you use small-o notation. However, it is unclear what this means, since Σ is a fixed matrix and there is nothing asymptotic in the definition. Is p assumed to tend to infinity here? What about the rest of the paper?

(5) On page 7, you use MAD, the median absolute deviation, applied to $\mathbb{F}_Z$, the distribution of a random vector. I am only aware of the median absolute deviation for the distributions on the real line – an extension to $\mathbb{R}^d$ requires further comments.

(6) In Lemma 3.2, you make an assumption $\mathbb{E}|X-\mu|^{-4} < \infty$. What is the class of distributions satisfying this condition?