Report on the paper JSPI-2018-61
Consistency of the minimum triangular distance estimator...
by Cassetti et al.

The authors consider a specific parametric family of distributions depending on two parameters α and γ. Assuming that γ (the shape parameter) is known, they prove the consistency (in probability) of a minimum distance estimator for the parameter α based on the minimization of the so-called "triangular distance" between a nonparametric density estimator of the underlying density and the assumed parametric density.

I find that the manuscript suffers from the following, quite serious, drawbacks

1. **Lack of motivation**. There is today a large literature on minimum distance estimation, including methods based on distance between density functions. I must confess that I never heard before of the so-called "triangular distance" but, still, I don't find in this paper any convincing reason why this distance (or discrepancy measure) should be sometimes used instead of other much more popular, simpler and better motivated distances. The authors mention a paper of theirs (item [16] in the references list) to motivate the interest of the triangular distance based on an empirical study. However, such study is too incomplete to provide a convincing motivation since, in particular, no other minimum distance method is considered besides that based on the triangular distance. The, more indirect, motivation based on the use for hypothesis testing given in reference [24] is still more unconvincing (I cannot understand the meaning of Table V there).

2. **Lack of relevance of the provided theoretical result**. Even in the most favorable situation, if we assume the total correctness of the proved theoretical result, one can hardly say that this result is very relevant. It only establishes weak consistency for a minimum distance estimator (under a very specific model) as a quite direct corollary of a well known-classical result by Parr and Schucany (1982, Th. 1). In addition, **the shape parameter γ is assumed to be known**, which is not a very natural assumption in practice.

3. **Overwritten and somewhat imprecise theoretical development**. I have the feeling that the whole theoretical development might be simplified and improved in several aspects. For example,

   - Proposition 1 looks rather unnecessary: it seems to me that this result follows immediately from the well-known Scheffé's Theorem (which establishes that the pointwise convergence of density functions to another density function entails the L_1-convergence).
   - In the Definition 1 (of the triangular distance) I don't understand why one needs that both densities have a common support.
   - Since the authors establish expression (III.2a) in a rather formal way, devoting a lemma to it (Lemma 3.1), one might assume that such expression is not completely trivial. However, no proof is given.
   - By contrast, the considerations before Proposition 5 are rather obvious and I don't see why the authors have chosen to present them in the general framework of a locally compact parametric space (which is clearly not needed here). Also, Proposition 5 itself looks a rather immediate consequence from the identifiability property of the distance and the continuity of the distance with respect to the parameter.
   - The use of non-standard kernel density estimators (based on "asymmetric kernels") should be better motivated, and perhaps backed with some experimental results. The reasons mentioned by the authors at the beginning of Section IV do not seem convincing enough to me. In particular, a precise reference should be given on the claim made about the optimal convergence rate: I was unable to find it in the paper by Bouezmarny and Scaillet, 2005; on the other hand the results in Chen (2000) (which are not formally stated, anyway) seem to apply to the case of bounded support. It is well-known that standard kernel density estimators provide **universal almost sure** consistency in L_1 (which is the kind of consistency needed here); see, e.g., the classical book by Devroye and Györfi (1985). The authors should clearly motivate the need of using another class of estimators (not well studied yet in the statistical literature) which, apparently, only yields consistency

in probability.

Overall, I don't think that this paper is suitable for publication in JSPI. Since no empirical results are provided (a comparison with other minimum-distance estimators would be welcome) the assessment of this manuscript must be done on the sole basis of the provided theoretical result which, by the above mentioned reasons, is somewhat insufficient to deserve publication in a research journal.