Report on the paper OPTL-D-22-00348

"On a threshold descent method for quasi-equilibria"

The authors propose a simple threshold descent method for solving a special class of quasi-equilibrium problems in metric spaces. The convergence of the method is proved without the usual assumptions considered in the literature. Moreover, some existence results are established as corollaries of the convergence theorems. The method is applied to scalar and vector generalized quasi-equilibrium problems and to some classes of relative optimization problems.

The paper is interesting and well written, the solution approach is new and very simple, the proofs seem to be correct. However, I have the following comments:

1. Page 4, line 28: Add a "." at the end of the sentence "We first discuss some conditions ...".

2. The authors state in the abstract and introduction that the proposed method converges without the usual convexity and monotonicity assumptions. However, the convergence results proved in the paper involve some monotonicity assumptions, as the cyclic antimonotonicity (that implies the key assumption (C1)) and the Brezis pseudomonotonicity. I think the authors should better clarify this point and a comparison between the monotonicity assumptions exploited in the paper and the classical notions of (strong, pseudo) monotonicity considered in the literature could be useful to the reader.

3. The beginning of the proof of Theorem 2 is not clear. The authors write "First let us prove that assumption (C1) holds". However, in the following lines they prove by contradiction a slightly different property, i.e.,it is finite the number of points z_k such that (9) holds. Probably this latter property is sufficient in the proof, but condition (C1) has not been proved. The authors should clarify this point.

4. In Section 4.2 the results proved in Section 3 are applied to Generalized Nash Equilibrium Problems. However, I have several doubts about the assumptions of Proposition 1.
   First, I think assumption i. about the income functions is very restrictive. In fact, the authors write in Remark 2 that assumption i. holds if the income functions are additive, but in this case the GNEP results to be equivalent to a potential GNEP where each utility function u_i only depends on his/her own strategy x_i.
   Then, I think assumption iv., in particular $x \in D(x)$ for every $x \in X$, is very restrictive as well. The authors write "It seems reasonable to assume that the players can keep the current state if necessary, which gives $x \in D(x)$ for all $x \in X$". However, it is well known that very often this assumption is not satisfied in GNEPs (very simple counterexamples

can be built even for GNEPs with joint constraints).

The authors should at least comment these assumptions more in detail and consider whether to delete Section 4.2 because it is not very meaningful.

In conclusion, I think the paper needs a major revision before it can be published. The paper should be modified according to the above observations.