Response to Reviewer 3

Comments and Suggestions for Authors

The manuscript is interesting as several approaches were used to assess the treatment capability of different photo-processes. However, there are some critical aspects (major revision) that must be considered by the authors to publish the manuscript in this journal.

Answer: Thanks to the reviewer for the time dedicated to evaluating our work and the useful comments that helped us to improve the manuscript quality. The responses to the specific comments are presented below.

What is the test water matrix?

Answer: Based on the reviewer's question, in Section 2.3 of the revised manuscript was clarified that we used distilled water (please see line 132).

UVA intensity is 90 W/m2? Are the authors sure about this value? It is much higher than the usual one and an efficient disinfection by only solar light should be observed (minutes of treatment).

Answer: Thanks a lot for the comment. Considering this, the irradiation measurement was carefully repeated. Thus, we found that the actual UVA intensity was ~44.3 W m^{-2}. Consequently, the correct value was added to the revised version of the manuscript (line 129).

Did the authors measure the Ag Leaching during and after the processes? Leaching has to be measured and considered in these systems as the metals can play a critical role. Can the silver precursor species adsorbed on the oxidative face leach during the process and be the reason for the disinfection improvement shown as Ag is a powerful disinfectant agent?

Answer: As the reviewer indicated in the comment, the control of the Ag leaching is relevant if the concentration in the solution is high. We usually control the metal leaching in our photocatalytic processes when we apply new materials, measuring the Ag leaching by ICP-MS, and for materials such as Ag/TiO_2, the released amount of Ag is low (less than 4% of the initial load of silver in the catalyst or equivalently ~ 20 μg L^{-1}); however, the disinfecting effects by the leached Ag cannot be neglected (we should mention that considering the reviewer comment, this information was added to the revised manuscript on lines 367-371).

A comparison with the commercial, effective and inexpensive TiO2 P25 catalyst has to be included.

Answer: The objective of this research work is to evaluate the effectivity of new catalysts based on TiO_2 and silver synthesized by expert researchers from the Institute of Materials Science of Seville (Spain) so for this reason our catalyst used as control of Ag/TiO_2 is the TiO_2 synthesized and already used in this research work. In our research group, we continue working with TiO_2 P25 in different applications but we didn't want to evaluate this catalyst in this study to avoid misunderstanding the main objective of the research.

Fig 2d. Revise microbiological units and change the icons to make it easier to distinguish the treatments on the graph. Disinfection by only solar light very slow.

Answer: Thanks a lot for the recommendation. Fig. 2d was adjusted as requested by the referee (please see lines 341-342).

The PDS concentration is 0.01 mmol L-1? Or 0.01 mol L-1?

Answer: We confirm that the PDS concentration is 0.01 mmol L^{-1}; indicating that a low amount of the peroxydisulfate is enough to achieve the improvement of the photocatalytic performance.

AA activity removal was performed replicated? There are not statistics in the graphic and it is a measurement with high error associated.

Answer: Thank you very much for the observation. Considering this comment, in Section 2.3 of the revised manuscript, it was clarified that "In the figures/plots, the average values of the experimental replicates are reported, and the coefficients of variation for such repetitions were lower than 5% in the case of LEV degradation and these were lower than 10% for the microbiological tests" (lines 135-138).

Figure 3b. Are the authors sure that radicals can be generated in the dark by the interaction between TiO2 and PDS? TiO2 without light is inert.

Answer: We agree with the reviewer that the interaction between TiO_2 and PDS in the dark does not produce radicals. Then, based on this comment and a careful revision of Fig. 3, we noted that due to a similarity in the colors used to represent the sulfate radical and the PDS/TiO_2 system, confusion is caused (generation of radicals in the dark). Hence, in the revised manuscript, Fig. 3 was properly adjusted to avoid such confusion (please see the adjusted version of Fig. 3).