General comments

The paper presents the results of experiments to study the possible effect of microplastic fibres on soil properties and soil erosion. Even if there already some studies focused on changes in soil properties, it is surly one of the first studies to tackle this subject, at least with respect to water soil erosion. Overall, I find this an informative study on an important subject: How does MP effect soil and potential soil erosion? In my view the experimental design is sound, laid out well, and the results are overall well presented and be summarized. I would like to thank the authors for this interesting study and the results. When discussing the results, I miss the clarification that this study is about laboratory experiments that may not always reflect reality. We are still in the knowledge level of estimating.

Authors (ACs): We would like to thank the anonymous Referee for insightful comments which helped us to increase the quality of our manuscript

Specific comments

Nevertheless, I have four fundamental critics to the paper which should be clearified:

- This study deals with microplastic fibers – as written in the title. This is not always clear stated in the paper. Especially the introduction is very generally about MP and not focused on fibers. Since the present study deals purely with fibers, I would also go into more detail on fibers in the introduction. Also in the methods and results/discussion it is often not clear you talk about microplastic at all or microplastic fibers in detail. It always has to be clear stated.

ACs: We modified the text following the reviewer's suggestions. Moreover, where appropriate, we changed the term “microplastic and MP” to “polyester microplastic fibers or polyester MP fibers”

- The preparation of the fibers (size/shape) used in the experiment are described in the methods. After this description the fibers are mixed with soil via a blender. This makes the size/shape of the used fibers (still fibers at all?) unclear again and that is dangerous for the entire interpretation of the results. I expect a statement in the methods that justifies the use of a blender and how you deal with the lost information about size and shape of the fibers.

ACs: Many thanks for bringing up this point. We used the blender in order to provide a homogenous distribution of the plastic material in the soil. It is not so easy to chop fibers, this requires special equipment, since the fibers are very malleable. Nevertheless, before setting the protocol, we have done several tests varying the mixing time and we evaluated the fiber status using a stereomicroscope. The visual image analysis of our protocol showed that changes to fibers shape, form and size were negligible. We added the following information in the new version of the manuscript: "*We chose to incorporate the fibers into the soil using a blender to provide a more homogeneous distribution of the fibers in the soil. We tested the impact of mixing time to establish a protocol which ensured a homogeneous distribution of the polyester MP fibers into the soil, and that preserved the integrity of the MP fibers (which was evaluated through visual inspection using a stereo microscope Zeiss Stemi 2000-C; Fig. 1a and b)*."

- Can the method described in this study for selecting the water-stable aggregates be cited? And with it results are compared? To my knowledge, in soil research, soil aggregates <53 µm are referred to as water-stable. In this study it is aggregates up to > 2 mm. I don't think that all water stable aggregates identified by different methods and defined with different sizes can be compared 1x1 in the discussion. On the one hand, I expect an explanatory statement on the aggregate selecting method used in the method section. On the other

hand, I expect a realistic classification of the results (water-stable) in previous literature on soil erosion and soil aggregates in the discussion.

ACs: Many thanks for the comment. As the request for clarification was raised by two Referees, we decided to remove the “stability indices” calculated on both new-formed aggregates (WSNFA(%)) and MWD (Sindex). We are now aware that those indices could have led to misinterpretation. We decided to report data obtained with both dry sieving and wet sieving (now Fig. 5 and 6) and added the references about the percentage of newly formed aggregates and MWD

- I would be more open with the final conclusions of this study. In the discussion and conclusion, everything needs to be considered in context that the results are based on a microscale laboratory experiment and may not present natural conditions. I generally lack the courage to "don't know". It should be dealt with more consciously that the results provide estimates and do not yet close any knowledge gaps. Even with the results of this study, we still know too little about the effect of micoplastics on the soil ecosystem.

ACs: We agree with the Reviewer. We are aware that this research could not answer all questions about the effects of the presence of MP fibers in soil. It is a contribution that provides new information and sets the stage for subsequent research and insights. We have modified the text in various parts stating that the experiment was conducted on a microscale and also that, in natural conditions, the effects can be different considering the innumerable interactions that can occur among the various components of the agroecosystem

Here are some more suggestions I have for the authors:
Material and Methods:

- The description of the differences in the soils could be more precise. In the description clarify with comments like "this is the soil with most soil carbon" or with indication by value. Or shorten the text and refer to the table for comparisons.

ACs: We have clarified the description of the three soils by firstly reporting their general characteristics and then the specific traits of the sampled soils.

- The information that the recorded procedure could not be continued because of the pandemic does weak your method description. I would leave that out and accordingly only say that the samples were incubated for 6 months. If you are sure that your samples were okay, I see no reason to be suspicious of this statement.

ACs: We have removed this part from the revised version of the manuscript

- The soil samples are dried to determine the BD. I assume at a temperature of 105 ° C. Can it be confirmed that this temperature has no effect on the MP-soil interaction? Even if polyester has a melting temperature of 150 ° C, e.g. 100% polyester fabric must not be washed above 40 ° C, as the fibers can then shrink or melt. Melting would, however, affect the analyzed parameters especially soil aggregation. I would add a sentence to this context to the methods.

ACs: Thanks for the comment. The dry soil bulk density, BD (g cm^{-3}), was calculated by measuring the volume at the end of the experiment after oven-drying the sample at 105 °C for 24 h. Given that polyester can withstand temperatures as high as 150 °C and the added

amount of fibers (0.5%) was low, we were confident that the drying process had no detectable effect on the measured bulk density. We added a sentence in the new version of the manuscript: "*Given that polyester can withstand temperatures as high as 150°C and the added amount of fibers (0.5%) was low, the drying process was not expected to have a significant effect on the measured BD values.*"

- Fibers with a diameter of >2 mm were prepared. Are you sure they can move down to a sieve to 0,106 mm? Is it not obvious you will "analyse" a rise in water stable soil aggregation and by that overestimate this effect? You have an idea about the movement behavior of the fibers in the sieves?

ACs: The fiber, as already reported in the text, had the following characteristics: "*The mean and the standard deviation, (SD), of the fiber length were 2.87 mm and 0.31 mm, respectively, and the mean and SD of fiber diameter were 87 µm and 3 µm, respectively.*". We visually analyzed the soil samples in the various sieves after the wet sieving procedure and we found that almost all the MP fibers were in the sieves with a mesh greater than 600 µm. We have no reason to believe that in dry sieving the microplastic fibers passed through smaller diameter sieves. We considered the recovered MP fibers in the calculations to estimate the stability of the new-formed aggregates (weight of the aggregates found in the wet sieving minus the recovered plastic). We did not consider the recovered MP fibers in the dry sieving. However, although this may have led to a slight underestimation of stability, the differences are certainly minimal considering the low weight of the fibers recovered in the sieved samples (on average 0.2 g out of a total of 40 g)

Discussion

- Broaden your discussion due to the soil erosion. At present the discussion focuses on the differences between soils. While that is perfectly fine (but always be focused on fibers) it would be good to place the topic of the rain simulations also in a somewhat broader framework. The size of the plots you studied implies that you mainly measured interrill erosion. This also implies that your results may would change depending on scale. You should discuss this. What are the implications at a real landscape scale?

ACs: Many thanks for this very important comment. The investigation was performed at the microplot scale and we hesitate to extrapolate the results to larger scales since active erosion processes are scale-dependent. We suggested that our results could help to delineate the hypotheses that deserve consideration in larger scale investigations. We clarified this aspect in the text. The following part was added in the revised version of the manuscript: "*We are aware that active erosion processes vary with the measurement scale (Cammeraat, 2002). At the microplot scale adopted in this investigation, erosion is expected to be due to the rainfall impact and the interrill flow (Bagarello & Ferro, 2004) and it should be a transport-limited process as a consequence of the reduced rain impacted flow and the limitation of flow velocity (Boix-Fayos et al., 2006; Chaplot & Le Bissonnais, 2000). Therefore, the collected data provide information on particle detachment and the early stage of their transport that can be expected to occur in upland agricultural soils during intense rainfall events. However, in agricultural fields, rill erosion can dominate total soil erosion due to the simultaneous occurrence of long slopes (dozens of meters or more) and the exposure of bare soil surfaces to rainfall in some periods of the year (Rejman & Brodowski, 2005). The effects of polyester MP fibers on rill erosion require specific testing but, also according to this investigation, it could be expected that rill development is hindered since the presence of the contaminant we used makes the soil intrinsically less erodible.*"

Technical corrections

There are more small comments and some textual suggestions in the annotated pdf attached.

ACs: Thanks for these additional suggestions which we considered in the revised version of the manuscript and further improved the overall quality of our manuscript.