The editor and reviewer's comments are in normal font and **the authors' responses are in bold font.**

Editor

L35: I would think throughfall per definition always has contact to the vegetated surface before it drips to the ground, so I would remove may or may not.

This is not true, throughfall can be split into three types: free throughfall, splash throughfall and canopy drip. Be definition, free throughfall is the proportion of throughfall that does not contact the canopy surface and, thus, maintains the same drop size distribution as open rainfall. For more throughfall partitioning details, please check https://doi.org/10.1002/hyp.13432 and https://doi.org/10.5194/hess-24-4675-2020.

L50: change concentrated to concentrates?

Change accepted.

L51: This sentence is a bit vague to me. Maybe good to clarify whether you mean the infiltration into the soil (i.e. the amount and whether it infiltrates as stemflow or matrix infiltration around the bole) or the infiltration pattern.

With our sentence we are referring to the stemflow infiltration pattern in the soil. In other words, the two things mentioned by the editor are intrinsically linked.

L145: "amount and rate of stemflow corresponds", should be correspond

Change accepted.

L154: reviewer asked whether the concentration is standard, you reply with yes and a citation. Please also add the citation to the article then.

"(Flury and Flühler, 1994)" has been added.

L187: Digitized images where were then further …"

The sentence has been corrected. "where" was changed to "were".

L190-191: I would suggest to adapt the explanation a bit, it was apparently not clear to the reviewer. I think the confusion stems from the fact that in this description it is not very clear that this is about the fraction of the profile which is stained. And make it clear that this is not equal to the total stained area.

We changed the text to "Dye coverage (Dc, %) (Flury et al., 1994) is the proportion of the dye-stained area in relation to the total area of the soil profile under consideration. It was calculated in 10-mm depth increments (dye coverage profile) and also for the entire profile (total dye coverage)." In addition, our text makes clear the distinction between Tsa, which quantifies the dye-stained area in square centimeters (cm^2), and Dc, which represents a fraction.

Fig 6: the moisture content is changing most in the top layer towards the end of the experiment. The description and explanations you give about the moisture content development in that top layer are still not clear.

We have added the following sentence: "As expected, the shallower probes exhibited the most pronounced SWC response with a maximum increase ranging from 14% to 20%. This is because the surface soil layer (0-10 cm) is the first to become wetter and by continuously receiving stemflow water contributes to its quicker saturation."

Reviewer

40, now 43: RC: Naming something a key topic, more citations should be given to prove that. - AC: The citations of Levia et al. (2011) and Van Stan et al. (2020) are the latest published books on forest hydrology with emphasis on throughfall and stemflow effects. We do not think that more citations are needed. – RC2: I do not find it very convincing to evidence that "stemflow has become an active topic of research in recent years" (only) with a more than ten years old book on forest hydrology. There are much more recent and specific (on stemflow) papers and review papers, which I would find more fitting to illustrate an active research topic.

The book by Van Stan et al. (2020) is one of the most updated compilations of global information on the subject and the book by Levia et al. (2011) despite being 10 years old is a book that shows the advances and knowledge gaps in rainfall interception processes from different viewpoints, that are still being studied to this day. We added the following relevant citations:

- **Tucker, A., Levia, D. F., Katul, G. G., Nanko, K., & Rossi, L. F. (2020). A network model for stemflow solute transport. Applied Mathematical Modelling, 88, 266-282.**
- **Magyar, D., Van Stan, J. T., & Sridhar, K. R. (2021). Hypothesis and theory: fungal spores in stemflow and potential bark sources. Frontiers in Forests and Global Change, 4, 623758.**
- **Tonello, K. C., Campos, S. D., de Menezes, A. J., Bramorski, J., Mathias, S. L., & Lima, M. T. (2021). How is bark absorbability and wettability related to stemflow yield? Observations from isolated trees in the Brazilian Cerrado. Frontiers in Forests and Global Change, 4, 650665.**
- **Pinos, J., Latron, J., Levia, D. F., & Llorens, P. (2021). Drivers of the circumferential variation of stemflow inputs on the boles of Pinus sylvestris L. (Scots pine). Ecohydrology, 14(8), e2348.**

37: RC: "overland, preferential or subsurface flow" This combination is odd to me. Please reconsider what your point is. AC: We change the sentence to: "overland, preferential or subsurface matrix flow". RC2: These categories still do not make sense to me. Preferential and matrix flow are both subsurface.

We changed the sentence to: "overland or subsurface flow".

167 and following: RC: I would prefer using correct mathematical symbols and equations, meaning that one metric can not be symbolized by several letters (e.g. A_s instead of TSA). In any case, however, the quantities used in equations should be consequently printed in italics also outside of the equations. - AC We agree that the symbols should be all in italic, in both equations

and text. However, it is not true that math symbols cannot have several letters. There is no such rule, indeed, the Cauchy number is an example of a symbol that has 3 letters. We have changed to upper and lower letters as suggested: for instance: DC to Dc. – RC2: You are right, there are no global rules to this. Some journals have rules, besides this, much depends on what is common in a field. I did not mean using lowercase instead of uppercase, but subscripts instead of abbreviations (the formatting got lost here). Using long abbreviations without subscripts just makes the formula much more unclear, and is also not standard practice. I want to reference to how formulas are put here: https://doi.org/10.1016/j.tplants.2020.04.003, https://doi.org/10.2136/vzj2015.09.0131. It is just a hint; if the authors are very attached to this formatting, let them keep it.

We prefer to keep our format which is also consistent with various subject-related articles in the literature.

207, now 218: RC: "no relevant…" What does that mean, what are the implications for this study? No change in soil physical properties does not mean that no differences in hydrological processes will occur or nothing interesting will happen below this depth? – AC: We chose 30 cm because it was the maximum depth chosen for the excavation of the soil profiles after the experiment. Yes, no change in soil physical properties does not mean no differences in hydrological processes. However, we do not expect to find different results at deeper depths in the vadose zone. We changed "no relevant" to "no significant". – RC2: I think it is fine to stop at a depth of 30 cm for the study, but it is wrong to argue with no changes in soil properties. It is not logical and sounds like an excuse where no excuse is needed. I personally would expect that processes change with depth, also with the same soil physical properties, and I am sure not the only one.

Yes, we can agree with the reviewer's comment here. No change to the manuscript was made, however, because the reviewer comment was related to our responses in the response document, which was not in the manuscript itself.

233, now 242: RC: Please use WRB instead of US SoilTax. When using a classification system, do not forget the reference. - AC: We are actually not using US SoilTax, but the USDA Soil Texture Classification. This is not the same. – RC2: You are right, I overlooked this. Still, I am missing a reference.

The following reference has been added:

USDA (2017). Soil survey manual. In: Soil Survey Division Staff, Soil Conservation Service Volume Handbook 18. U.S. Department of Agriculture (Chapter 3).

251, now 262: It would be great to have some measure of variance to being able to characterize and compare. – AC: Thanks for the suggestion, but as we only have one sensor per location and the water flow pattern is spatially variably, we do not have replicated measurements, and the variance for measurements cannot be calculated. Figure 5, Figure S1 and Table 1 provide an idea about the differences in water contents among sensors. – RC2: As you say, the pattern is spatially variable, and a measure of variability would give a number to quantify exactly that. You have

n=16, which is enough to calculate a statistical variation. You could e.g. give a temporal evolution of the spatial variance in the two depths.

There are not 16 probes in two depths as mentioned by the reviewer. There are 16 probes at one depth (0-30cm). We calculated the coefficient of variation among the probes and added the following sentence regarding variability: "The spatial variability of SWC showed a decrease in the coefficient of variation throughout the experiment from 17% to 11% towards the end."

259: Please set the maximum difference in SWC in relation to pre-stemflow values. AC: This is what was done. “the maximum difference in SWC (i.e., difference between the max SWC and the pre-stemflow SWC” is shown in Table 1., and 264 ff.: RC: Please give absolute additional to relative values. – AC: These values are listed in Table 1., and 294: “SWC” Please again also give absolute numbers. – AC: Again, what would be the point of adding these values? The absolute values are listed in Table 1. – RC2: It is difficult to get a full picture of the observed processes, when you just name relative values or differences. Only the combination of both an absolute value (SWC at the beginning or end of the experiment) and a relative value (or difference) gives the reader the possibility to understand the processes (at a high pre-stemflow water content, a small max difference means something else than at a low pre-stemflow water content). You give all these results in the tables, but you name some of them in the text, too. The flow of your results text relies very much on these named results, and therefore it is good for them to offer the full picture. I hope I could clarify what I meant in those comments.

We agree that both absolute values and relative differences are needed to understand the hydrological responses described in our manuscript. We do list these values in Table 1, and we are interpreting some of these values in the text for a better description of the changes in SWC.

272: RC: “because…” This is a good thought. Maybe you can find more references to compare stemflow infiltration with and without throughfall and go into more detail here. - AC: Thank you for this wonderful suggestion, however, we are not aware of any studies that analyze stemflow infiltration with and without throughfall. If the reviewer is aware of any, please let us know as it would be of great interest. – RC2: As some stemflow studies investigated stemflow infiltration under natural rain events, and some applied stemflow artificially, you have the differentiation of inclusion and exclusion of throughfall.

We now understand the reviewer’s comment. However, we think that it would be difficult to compare studies with natural events and studies with simulated events, because the throughfall will compound with the stemflow and make analyses of stemflow infiltration patterns challenging. Given the already extensive length of our manuscript, we opted to not include a discussion of this topic.

307 (“The distribution…”) – 313: RC: I find this paragraph too long and the statement rather trivial. The flow mainly taking place laterally due to the low (non-existent) slope inclination can be said in one sentence and does not need to be compared with studies located on stronger slopes, because this is comparing apples with oranges. – AC: The sentences will be modified and shortened for better understanding as follows: “We observed the greatest dye coverage in the immediate vicinity of the tree (at 10-cm distance), which suggests that the majority of the

stemflow infiltration water was directed vertically. This is likely a consequence of the flat area at our experimental site.” In addition, we disagree with the reviewer's comment "comparing apples with oranges". Relying on the observations of reviewer 1, it is certainly valid and useful to compare our results with the few available in the literature on trees on slopes. – RC2: You can compare different things, but then it is very important to name the differences. If you do not name the differences, then it is comparing apples and oranges. It is the same as it makes no sense to compare stemflow infiltration area, when you do not refer to stemflow amount and intensity (and tree species, soil type etc.). Without the preconditions, the numbers are not interpretable. And in your case, you can mention and discuss stemflow infiltration on slopes (I do not know what reviewer 1 said about this in particular), but it for sure makes more sense to compare your own results in a flat area (what, as I understood, it is) to other studies in flat areas.

The purpose of our statements in the manuscript regarding stemflow infiltration patterns on slopes is simply to inform the reader what has been observed on slopes, whether that finding is trivial or not does not really matter. We could not find a reference for flat slopes, that is why we did not include a literature comparison.