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Comment on acp-2021-344

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Referee comment on "Surface deposition of marine fog and its treatment in the WRF model" by Peter Allan Taylor et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-344-RC1, 2021

This paper explores the hypothesis that turbulent transfer of fog droplets to the ocean surface is more efficient than the transfer of water vapour. This sounds plausible, because "inertial impaction" on a rough surface is a possible mechanism for an additional sink of fog droplets. The authors of this paper paper include the mechanism in the framework of Monin Obukhov (MO) similarity and suggest to quantify the surface transfer of droplets through a roughness length for liquid water content. The latter is expected to be substantially larger than the roughness length for water vapour.

As pointed out in the paper, it is remarkable that most research models and Numerical Weather Prediction Models (NWP), do not account for droplet coalescence with the surface. At the same time most models overestimate liquid water content in fog near the surface, which could be due to the missing sink of liquid water.

The authors conclude from single column simulations and from 3D simulations with WRF, that results are very sensitive to the missing mechanism. Given the uncertainties in other mechanisms like microphysics and droplet settling, it is difficult to quantify the roughness length for cloud liquid water. It is therefore concluded that an experimental campaign is necessary to measure profiles of liquid water in fog and to analyse the data in the context of MO similarity. I completely agree and hope that such an initiative will lead the a more firm estimate of transfer coefficients of liquid water (or roughness length) to the ocean surface. Additionally, it would also be good to explore existing data over land because the probability of droplets to collide with vegetation is even higher than for the collision of droplets with water waves.

I have seen interesting critical comments on an earlier version of this paper, namely that: (1) the existence of a constant flux layer cannot be justified without observational support, and (2) the boundary condition Qc=0 may be wrong for liquid water in the form of droplets. I fundamentally disagree with both comments for the following reasons:
(1) A constant flux layer is a theoretical concept with a mathematical background in asymptotic theory. For small z/h (height divided by boundary layer depth) the equations of motion reduce to dF/dz=0 (with F for flux). The real question is: how small does z/h have to be to obtain meaningful solutions? We only know afterwards e.g. on the basis of experiments or numerical solutions and it also depends on the required accuracy. We know that Monin Obukhov Similarity (MOS, which is much more than just the constant

flux approximation) works very well for heights where z/h is not all that small. The key point is that the constant flux assumption is a mathematical concept.
(2) The boundary condition for liquid water transport by droplets is in my view correct. Qc=0 because droplets that hit the surface are absorbed by the water for ocean or stay on the vegetation as a water film for land. So, the surface is a sink for droplets and not a source as long there is no spray as in storm conditions, and as long as the droplets do not bounce back from the surface. The key point of the paper is that a rough surface is a more efficient sink for droplets than a smooth surface because droplets have inertia. As soon as the flow is curved, as over ocean waves, the droplets cannot follow the flow, hit the wave and are absorbed. The efficiency of this process depends on flow speed, drop size and curvature of the surface. The efficiency, which can be expressed empirically as a surface roughness, has to be determined experimentally.

For me, the idea presented in the paper is very interesting and elegant. Some scientists may have doubts about the value of this idea, and this is absolutely fine, but in that case, I am very much in favour of having the paper published as an innovative idea. Other scientists are free to write a comment, and hopefully a useful exchange of ideas emerges. ACP provides an ideal platform for such a discussion.

In conclusion, I very much welcome this paper. It describes a mechanism that is ignored in most models and it puts it very elegantly in the framework of MO similarity. The paper suggests a way forward to quantify the mechanism of turbulent droplet collision with the surface by an observational campaign. The paper is well written with an excellent overview of literature. It also discusses extensively other relevant processes, has clear conclusions based on model sensitivity experiments, and is highly suitable for ACP. Given that revisions were already made on the basis of earlier reviews, I suggest publication as is.