Response to Dr. Gilles Bergametti's interactive comment on the on the manuscript "LES study on turbulent dust deposition and its dependence on atmospheric boundary-layer stability"

General comments: This article addresses a well identified but poorly solved problem, namely the effect of atmospheric boundary layer stability (ABLS) on the dry deposition of particles. Indeed if it has been noted that the dry deposition velocities of particles are higher in unstable conditions, no convincing physical demonstration of the reasons for this increase in deposition velocities has been proposed and consequently no parameterization of this effect exists until now. This paper addresses this issue.

The idea behind this paper is based on the fact that much of the particle deposition is induced by turbulent diffusion which is related to turbulent shear stress and that turbulent shear stress (both its average value and its fluctuations) are related to the stability of the atmospheric boundary layer, with unstable conditions resulting in higher turbulent shear stress and, more importantly, larger fluctuations. Because the dry deposition velocity does not depend linearly on the shear stress and because existing models describe the deposition velocity only as a function of the average shear stress (or average friction velocity), the effect of fluctuations in the shear stress is not properly accounted for.

To address this question, the authors perform 35 runs of a sophisticated Large Eddy Simulation (LES) model corresponding to various stability, wind speed and roughness conditions. This approach is well adapted to the problem and the results are convincing and important.

The results clearly show that under unstable conditions, especially when the wind speed is low, the shear stress shows a strong variability around its mean value, whereas under neutral or stable conditions, with strong wind speed, this variability is strongly reduced. The authors then look at the consequences of these results on dry deposition velocities. The results are clear: in stable or neutral conditions, there is no impact of the stability on the calculated deposition velocities; these are identical whether or not the fluctuations in shear stress induced by the stability conditions are taken into account. On the other hand, for unstable conditions, the deposition velocities integrating the shear stress fluctuations are nearly 50% higher than those based on the average shear stress value are.

The demonstration is convincing and clearly points out that the turbulent fluctuations of the shear stress are responsible for the increase in deposition velocities under unstable conditions.

The authors will then develop a parameterization allowing to take into account this effect in the dry deposition modules. The first step is to define the probability distribution function of the shear stress. The authors demonstrate that this distribution can be correctly reproduced by Weibull distributions and determine a relation allowing the calculation of the parameters of these distributions as a function of, in particular, the Monin-Obukhov length and the average friction speed. Finally, they apply the improved dry deposition model and show that taking into account the effects of instability leads to a significant increase in the deposition velocity of particles between 0.1 and about 2 µm in size.

This paper is highly interesting and provides physical basis for the understanding of the effect of the ABLS on particle dry deposition. The paper is complete since it does not only explain the role played by ABLS but it proposes a parameterization that can be used into dry deposition modules to account for its effects. The paper is well written, concise, correctly illustrated with the correct references.

I think the impact of this article is greater than the title suggests. Indeed, the work developed concerns all types of particles and not only dusts. A title like LES study on turbulent particles deposition and its dependence on atmospheric boundary-layer stability: application to dust could better reflect the real content of the work. In any case, I highly recommend publishing this article in ACP with minor revisions.

Response: We are most grateful to Dr. Gilles Bergametti for his time and effort in reading the manuscript, as well as for his encouraging comments and insightful suggestions. These comments are very valuable for us to improve our paper and approach the truth. Dr. Gilles Bergametti pointed out the basic theory behind this study and the usefulness of this study in clarifying the dependence of turbulent particle deposition velocity on atmospheric boundary layer stability. We thank Dr. Gilles Bergametti for his comment that this work applies not only dusts but also other particles. Therefore, we will change the title to 'LES study on turbulent particle deposition and its dependence on atmospheric boundary-layer stability' and modify the text accordingly.

Minor comments:

1) Line 24: Gregory (1945) is not in the reference list

Response: *Thanks. We will correct it in the revision.*

2) Line 35: a review paper by Fowler et al. (Atmospheric Environment 43, 5193-5267, 2009) should be cited. I think that the authors could have benefit to have a look at paragraph 7.6.1.3 (understanding the effect of stability and leaf properties on deposition velocities, p5258).

Response: Thanks. Paragraph 7.6.1.3 in Fowler et al. (2009) noted the gaps in observations that can better control stability conditions and the lack of testable hypothesis explaining the link between dry deposition velocity and atmospheric stability. It helped us to better understand the influence of stability and vegetation properties on deposition velocities, and further research needs.

Line 35 will be revised to 'It has been observed that the dry deposition velocities under convective conditions are larger than those under neutral and stable conditions when the background wind speeds are similar, but there is no convincing physical scheme in models to account for the effects of the instability (Fowler et al., 2009).'

3) Lines 39-40: the transition to dust is not well done, especially it is not clear why it is necessary to discuss about dust here. As mentioned before, and except if I miss something, the conclusions of the paper apply to any types of particles.

Response: *Sorry about this. We will replace the 'dust' using 'particle' in Lines 39-40, as well as in the conclusions part in the revision.*

4) Line 81: TKE (Turbulent Kinetic Energy)

Response: *Thanks. We will add the full name of TKE in the revision.*

5) Line 88: R_a is not defined

Response: *Sorry about this. R_a is the specific gas constant of air. We will define this in the revision.*

6) Line 112: change "grand" in "ground"

Response: *Thanks. We will correct this in the revision.*

7) Line 146: How are the rebound and collection efficiency computed? At least add a reference

Response: *Thanks. We will give formulas and references for the rebound and collection efficiency in the revision.*

8) Line 226: 1-c and 1-f instead of 1a-c and 1d-f

Response: *Thanks. We will correct this in the revision.*

9) Line 227: the choice of 1.46 µm for the particle size should be explained in few words

Response: *WRF that we used calculates the deposition velocity by default for particles with diameters of 1.46 µm, 2.8 µm, 4.8 µm, 9 µm and 16 µm. In the text, the particle size of 1.46 µm is chosen as an example because the particle of this size is most sensitive to turbulent diffusion compared to the other four sizes.*

10) Line 242: Li et al., 2020 and not 2020a

Response: *Thanks. We will correct this in the revision.*

11) Line 274: it should be useful for the reader to know how $1/L_o$ is computed in the simulation and also to give its value in table 2 (with only figure 4, it is not easy to connect $1/L_o$ to the other parameters)

Response: *Thanks for the suggestion. 1/L_o is the reciprocal of the Monin-Obukhov length which is defined by* $L_o = \frac{-\bar{\theta} u_*^3}{kg\overline{w'\theta'}_0}$ *with* $\overline{w'\theta'}_0 = \frac{H_0}{\rho_a c_p}$. *Thus,* $1/L_o = -\frac{kg\overline{w'\theta'}_0}{\bar{\theta} u_*^3}$. *In the revision, we will give the computed formula of 1/L_o in the text and its value in table 2.*

12) Line 333: again I think that the conclusion is not specific to dust
Response: *Thanks for the suggestion. We will correct it in the revision.*

13) Line 353: the authors mention that change in surface roughness may affect the variation of the shear stress. They have simulations for two different roughness lengths and have provided in Appendix a figure similar to figure 3 but for z_o=0.76 mm. Even, if the wind speed are not exactly the same, they could discuss a little more how change in roughness changes or not the pdf of shear stress.

Response: *The surface roughness length mainly reflects the fact that the surface topography changes the turbulence structure near the surface, which affects the mean wind profile. Since the simulations cannot delineate the surface topography in detail, we vary the roughness length to simulate different wind profile conditions. However, this does not fully reflect the effect of the change in surface topography on the turbulent structure and the particle deposition process under this effect. We thank the reviewer for this comment and we will examine this issue in detail in future work.*

14) Line 377: author contributions. Change YPS in YP to be consistent with the name of the authors used after the title.
Response: *Thanks. We will correct it in the revision.*

15) Lines 419-420: delete this reference not cited in the text
Response: *Thanks. We will delete this reference not cited in the text in the revision.*

16) Lines 435-436: delete this reference not cited in the text
Response: *Thanks. We will correct this in the revision.*

17) Lines 443: change 2020a by 2020
Response: *Thanks. We will correct this in the revision.*

18) Lines 444-445: this reference is in duplicate
Response: *Thanks. We will delete this in the revision.*