Anonymous Referee #1

We would like to thank you sincerely for your precious support to correct the text, and all your suggestions. Before answering to your questions, we must confess that there was an error in the coding of the deposition process : the deposition velocity was mistakenly multiplied by the volume of the grid, corresponding to a ratio of 25 for all the simulations at 5m resolution (so a deposition velocity of 50 cm/s instead of 2 cm/s was actually applied), and to a ratio of 4 for the simulation at 2m resolution (noted DX2). Consequently, the deposition effect was overestimated.
All the simulations except the one without deposition (called NDG) have been run again and most of the figures have been updated. For the REF simulation (with a deposition velocity of 2 cm/s), the discrepancies with the observed microphysical fields are a bit stronger (cloud mixing ratio and droplet concentration more overestimated), but the DE8 simulation (deposition velocity of 8 cm/s as it was requested by one of the reviewers) presents a significant improvement. The signature of the fog onset at elevated levels in the REF simulation is not so marked, and is more evident in the DE8 simulation, showing that both the tree drag effect and the deposition are necessary to reproduce the formation of fog at elevated levels. The new results do not modify the analysis of the fog event and the conclusions of the study.
The text has been also reduced to answer to the reviewers : the sensitivity test on the initial conditions has been removed, as well as the corresponding figures. The length of the text has been reduced as expected. Lastly, the text has been revised by an english native speaker.

*Recommended disposition: The manuscript requires major revisions before publication in Atmospheric Chemistry and Physics.*

*General comments:*

*The manuscript presents results from various LES of a radiation fog event observed at a complex site. The simulations were aimed at identifying the main dynamical factors affecting the simulated life cycle of the fog layer, including its microstructure. The research is an original contribution toward a more complete understanding of the complex interactions shaping the evolution of a fog layer, as well as the identification of possible improvements of numerical models necessary for more accurate fog forecasts. The work is also a good example of how carefully crafted simulations can provide some insights into specific features often present in observations taken at complex sites. The discussion is comprehensive and generally well-structured, with major findings clearly emphasized. Some parts of the discussion could be shortened to further improve the clarity of the overall presentation. It is the opinion if this reviewer that major revisions are however needed before the manuscript can be accepted for publication.*

*More specifically:*
*1. First and foremost the written English is not of sufficient quality, which provides for a difficult read of the manuscript. It appears that the text was not put through a basic grammar check that most text editors have available. I highly encourage the authors to have the text revised by an English speaker to ensure appropriate terminology and sentence construction. There are also numerous opportunities to make the text more concise and clearer.*

Additionally to all your suggestions, the text has been revised by a native speaker of English.

*2. As this is a central aspect of this study, the parameterization of fogwater deposition on the tree canopy should be more clearly described and justified. In particular, the use of a drag term on momentum and TKE to represent the impact of a tree barrier on the flow and associated turbulence,* *while the use of a parameterization of fog water deposition which is entirely independent of the flow and turbulent characteristics (i.e. constant deposition velocity) may appear as incompatible. Hence, the chosen formulation should be more clearly justified and contrasted against the work of von Glasow and Bott (1999).*

You are right that the tree drag parametrization is sophisticated while the parameterization of fogwater deposition is simplistic.The tree drag parametrization has been introduced quite a long time ago in the model (Aumond et al., 2013) and validated on different cases (Bergot et al., 2015a). On the contrary, the deposition process is not taken into account in most of the models, especially NWP models. The first step here is therefore to have a first approach by examining the importance of this process, considering a simplistic formulation. As the conclusion is that this term is essential to correctly reproduce microphysical fields of the fog cycle, the next step in a further study will be to have a more sophisticated formulation as in von Glasow and Bott (1999). The text has been modified like this :

« In addition to droplet sedimentation, fog deposition is also introduced which represents direct droplet interception by the plant canopies. In the real world, it results from turbulent exchange of fog water between the air and the surface underneath, leading to collection (Lovett et al., 1997). In numerical weather prediction models (NWP), this process is most of the time not included, such as in the French NWP model AROME (Seity et al., 2011) whose physics comes from Meso-NH. As a new process to introduce, only a simple formulation of the deposition process is considered here as a first step, in order to perform a sensitivity study. The fog deposition flux $F_{DEP}$ is predicted at the first level of the atmospheric model (50 cm height) for grassy areas, and over the 15 m height for trees, in a simplistic way following Zhang et al. (2014b): $F_{DEP} = {}_{a}V_{DEP}$ with = $r_c;N_c$ and where $V_{DEP}$ is the deposition velocity. In a review based on measurements and parametrizations, Katata (2014) showed that $V_{DEP}$ values ranged from 2.1 to 8.0 cm/s for short vegetation. Here $V_{DEP}$ is assumed to be constant, equal to 2 cm/s. A test of sensitivity to this value is presented below. Water sedimentation and deposition amounts are input to the humidity storage of the surface model. A more complete approach in a further study would include a dependance of $V_{DEP}$ on momentum transport as in von Glasow and Bott (1999) and also on LAI.»

The simplistic formulation of the deposition process and the necessity to improve it was already underlined in the conclusion : « In this study, the deposition term was introduced quite crudely and this would need some refinements in further studies. It would need to take account of the wind speed and the turbulence , and it could also consider the hygroscopic nature of canopies. By analogy with dry deposition, it would also be better to take droplet diameter into account, assuming that this field is correctly reproduced. Other studies have also shown that fog water deposition is strongly enhanced at the forest edge, becoming up to 1.5-4 times larger than that in closed forest canopies (Katata, 2014), so it could be interesting to simulate the edge effect of fog water deposition. ».

*Specific comments:*

1. *Throughout the text, replace "trees barrier" by "tree barrier" or by "barrier of trees".*
   OK

2. *Use of past tense to describe some aspects of the simulations throughout the paper is awkward. You may have performed the simulations in the past, but their characteristics remain true now. Please revise your use of the past tense throughout the manuscript.*
   OK

3. *Throughout the manuscript, replace "ponctual" by "point".*
   OK

4. *Abtsract line 2: Revise with "during the ParisFog"*
   OK

5. *Abstract line 3: Please specify which aspect of "dynamics" you are referring to Boundary layer?*
   Yes, it has been corrected by « the dynamics of boundary layer »

6. *Abstract line 5: deposition of what? Please specify for greater clarity.*
   Yes, « deposition of droplets »

7. *Abstract line 7: We should read "as in observations" rather than "like in the observation".*
   OK

8. *Abstract last sentence: I would suggest re-wording as: : : :"necessary to accurately represent the fog life cycle at very high resolution" for a clearer statement.*
   Yes, thank you.

9. *Introduction line 18: How do you define "local dynamics"? and why do you not seem to include turbulence in that category?*
   I mean by local dynamics local flow due to orography for instance. This has been corrected by « local flow »

10. *Introduction line 19: Please rewrite with "understanding of fog processes" rather than "fog processes understanding".*
    Yes.

11. *Introduction, line 20: Sentence is without a verb.*
    OK, « can be referred » has been added.

12. *Introduction, line 22: measurements (please use plural).*
    OK

13. *Introduction, line 22: "and set liquid water content": ? I do not understand. Please revise.*
    OK, set has been replaced by report.

14. *Page 2, line 5: use "as shown by Nakanishi"*
    OK

15. *. Page 2, line 6: Here, need to add "to study some aspects of the characteristics of a fog layer". Nakanishi was not the first to use LES in general, as you seem to imply.*
    OK, this has been corrected by : « Many important features of fog have also been characterized using one-dimensional (1D) modelling (Bergot et al. (2007), Tardif (2007), Stolaki et al. (2015) among others). However, to study some aspects of the characteristics of a fog layer, it has become necessary to explicitly simulate turbulence motions in 3D as shown by Nakanishi (2000) who was the first to use a large-eddy simulation (LES) for fog. »

16. *Page 2, line 8: "a turbulence scheme"*
    OK

17. *Page 2, line 11: Use of "stripes" is not appropriate. Maybe use "banded structures" and specify in which field(s) theses structures are observed.*
    OK, but « stripes » was already used by Bergot (2013). This has been corrected by : « During the formation phase, small banded structures, identified by Bergot(2013) as Kelvin-Helmotz (KH) billows, occur in the middle of the fog layer on dynamical and thermodynamical fields. »

18. *Page 2, line 14: Replace “move” by “relocate”.*
    OK

19. *Page 2, line 18: the word “Hence” is superfluous.*
    OK

20. *Page 2, line 26: The use of “allowing to represent” is not proper. Change to "allowing the representation of"*
    OK

21. *Page 2, line 30: replace “it” by “values”*
    OK

22. *Page 3, lines 3-4, sentence beginning with “Sensitivity tests will: : :”: This has been said already. Please remove sentence.*
    The sentence has been removed.

23. *Page 3, line 5: Replace “sophisticated microphysics” by “sophisticated microphysical parameterizarion scheme" to be more precise.*
    OK

24. *Page 3, line 6: Replace “taking into account” by “while accounting for".*
    OK

25. *Page 3, line 6: We should read “such as forests”*
    OK

26. *Page 3, line 14: winter of*
    OK

27. *Page 3, line 20: wind does not flow from a "side", rather from a direction. Also, "this side" implies that information about wind direction has been provided to the reader, which wasn't. Please revise your sentence(s).*
    OK, this has been corrected by : : « Zaïdi et al. (2013) demonstrated the impact of the tree barrier on the observed flow when the wind was blowing from this direction, and our case study was in this configuration. »

28. *Page 3, line 21: It is mentioned that the reader should refer to the study by Stolaki for a description of the instrumentation, yet the the entire next paragraph is devoted to just that. Please revise your text*
    The reference related to Stolaki's study for the description of instrumentation has been removed.

29. *Page 3, line 23: Get rid of “At the surface”. 30m is not "at the surface" in this context.*
    « At the surface » has been removed.

30. *Page 3, line 31: We should read “Aerosol particle measurements”, not “particles measurements”*
    OK

31. *Page 3, line 33: What type of profiler? I suppose it is a microwave profiler. Please be more precise with your statement.*

Yes, this is a RPG-HATPRO water vapour and oxygen multi-channel microwave profiler : this information has been added.

32. *Page 4, line 5: 1000 UTC "on the following morning"? Please be more precise.*
It has been added.

33. *Page 4, line 9: I am not clear as to why fog events were not classified as stratus lowering. 150m for initial cloud formation does seem high to be a radiation fog. Please explain.*
You are right that the distinction between radiative fog and cloud lowering is not easy to make. Fog classifications traditionally use the Tardif and Rasmussen (2007) method. They differentiate stratus lowering from radiative fog by the wind speed and the cloud ceiling. If the wind speed at 10m is lower than 2.5 cm/s before the formation and the cloud ceiling is less than 100m then the fog is supposed to be radiative. Our measured wind speed at 10m is under 2.5cm/s but our cloud ceiling is higher than 100m (150m). However according to Dupont et al. (2012), the lowering of a stratus is due to a cooling at its base by evaporation of sedimented droplets. Considering fall speed of 2.2 cm/s (Roach et al, 1976) it would necessitate at less 10 hours for the cloud to reach the ground. Moreover we do believe that the cloud formation at 150m is due to the modification of the flow caused by the tree barrier resulting in an important vertical mixing on a significant depth. So we conclude that this fog is a radiative one.
We propose the text :
« As underlined by Stolaki et al. (2015), this characteristic is very common at Sirta and 88% of the radiation fog events during the field experiment were also elevated. However, they were not classified as stratus lowering as they were followed rapidly by formation of fog at the surface. A delay of 30 min between the formation at 150 m height and at the ground seems too short to be a stratus lowering, which is mainly driven by the evaporation of slowly falling droplets that cool the sub-cloud layer (Dupont et al., 2012). This suggests that this type of radiation fog could be linked with, and specific to, the configuration of the Sirta site. »

34. *Page 4, line 12: Replace "according to" by "following"*
OK

35. *Page 4, line 14: Use of "moistening" could lead to confusion. Is "moistening" referring to an increase in *relative* humidity (due to cooling) or increase in absolute humidity (water vapor content)? Please be more precise.*
You are right that it was confusing. The increase in relative humidity is associated to the cooling, as we can see below on the dewpoint temperature : the difference between temperature and dewpoint temperature reduces slowly until the fog formation. « as well as a moistening » has been replaced by «inducing an increase in relative humidity ».

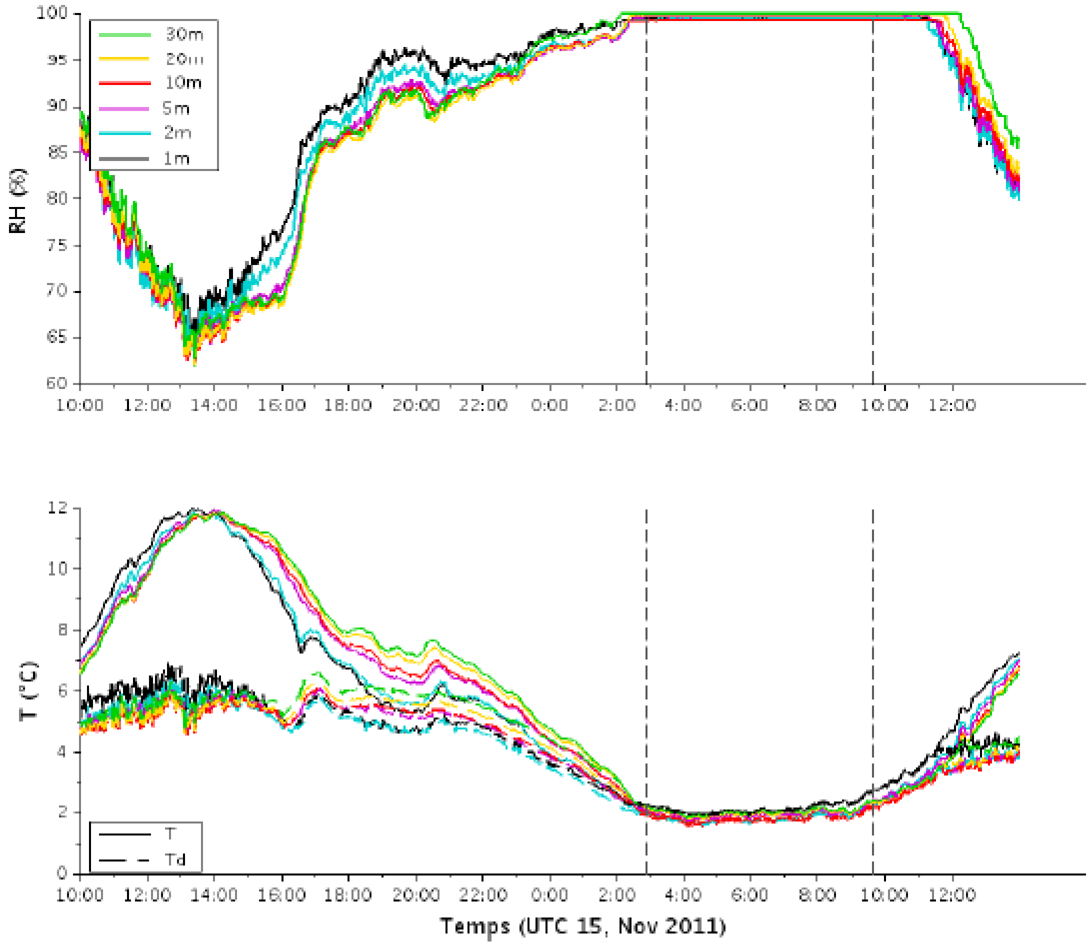


*Figure : Temporal evolution of observed relative humidity (a) and temperature and dewpoint temperature (b) from 10 UTC the 14th of November to 12 UTC the 15th.*

36. *Page 4, line 19: Not sure I understand the meaning of "temperature convergence" in this context.*
    This has been corrected by : « At 0230 UTC, the apparition of fog at the ground was associated with a temperature homogenization in the first 30 metres, called temperature convergence by Price (2011) and corresponding to a neutral layer.»

37. *Page 5, line 2: "fog droplet microphysics" is awkward wording in this context. Perhaps "fog microstructure" is more appropriate?*
    « liquid droplet » has been removed.

38. *Page 5, line 5: "leaded" is not proper English.*
    OK, it has replaced by « brought ».

39. *Page 5, line 6: LWC and Nc decreased at 3m but visibility remained constant? Please explain.*
    In fact, LWC and Nc decrease but visibility increases slightly. This has been corrected.

40. *Page 5, line 15: Can you be more precise in your description. Not sure that "between" means in the context of a size distribution.*
    OK, this has been corrected by : « During the dissipation phase (in green, at 0700 UTC), the concentration of larger droplets fell but remained higher than initially. »

41. *Page 5, line 29: "at the instrumental site »*

OK

42. *Page 6, line 1: By "It" you mean "The drag approach"? Please be more precise.*
    *Yes : «* The drag approach consists of introducing an additional term in the momentum and TKE equations »

43. *Page 6, line 5: "a combination of the product" is confusing. A product already is a "combination" of terms. Simply say that it is a product of the fraction of vegetation with LAI and a weighting function. I would suggest that you show an equation for greater clarity and since it is a central aspect of your study.*
    The sentence has been corrected but we have not introduced an additional equation as the formulation is exactly described by the sentence : « $A_f$ ( z) is the product of the fraction of vegetation in the grid cell by the leaf area index (LAI) and by a weighting function representing the shape of the trees, as presented in Aumond et al. (2013). »

44. *Page 6, line 7: The vertical profile of what exactly? Please be more precise in you statement.*
    Yes, this has beeen corrected : see 43.

45. *Page 6, line 7: "atlantic broad leaved trees": Where does that information included and how? Again, please be more precise in your statement. Perhaps refer to the equation that will show how Af is expressed.*
    This information does not refer to the weighting function but to the vegetation cover introduced for the trees, which will be considered by the land surface scheme. This has been clarified by : « The trees introduced in the simulation domain for the land surface scheme correspond to Atlantic coast broad leaved trees » instead of « We have considered atlantic coast broad leaved trees ».

46. *Page 6, line 11: Aren't "activated CCN" droplets? Please clarify the difference between Nc and Nccn.*
    At the beginning, concentrations of activated CCN and droplets are equal but then droplet concentration is modified by several mechanisms as break-up, evaporation, autoconversion, accretion and sedimentation so concentrations of activated CCN and droplets become different. This point is presented in the 2 references relative to the microphysical scheme : Khairoutdinov and Kogan (2000) and Geoffroy et al. (2008).
    Another point is that according to the Köhler theory, for a given maximum supersaturation Smax, aerosols activated are exactly those with a critical supersaturation lower than Smax. Thus, to determine the number of aerosols really activated at time t, we first compute the number of activable aerosols for Smax. The number of aerosols really activated is then the difference between the number of activable aerosols and the number of aerosols previously activated during the simulation. This point has been added.

47. *Page 7, line 16: How is droplet concentration and cloud mixing ratio taken into account in LW and SW calculations? Just provide appropriate references.*
    The radiative transfer is computed with the ECMWF radiation code, using the Rapid Radiation Transfer Model (RRTM, Mlawer et al. (1997)) for longwave and Morcrette (1991) for shortwave radiation. Cloud optical properties for LW and SW radiation take account of the cloud droplet concentration in addition to the cloud mixing ratio. For SW radiation, the effective radius of cloud particle is calculated from the 2-moment microphysical scheme, the optical thickness is parametrized according to Savijärvi et al. (1997), the asymetry factor from Fouquart et al. (1991) and the single scaterring albedo from Slingo (1989). For LW radiation, cloud water optical properties refer to Savijärvi et al. (1997).

48. *Page 8, line 2: I think here you rather mean that the **reduction** in visibility is*

*underestimated. Please revise your statements.*
No, on Fig.6, the green curve is below the black one ; the parametrized visibility according to Zhang underestimates the observed visibility.

49. *Page 8, lines 8 and 9: .Variables are not transported. Perhaps simply write "momentum is advected"*
OK

50. *Page 8, line 11: Awkward use of past tense.*
The past tense throughout the manuscript has been replaced by the present tense.

51. *Page 8, sentence on line 17-18: 1) soil moisture not moistening*
OK
*2) Used the same point measurements to initialize soil variables across the entire domain? Please justify this approach.*
Soil measurements are available in one place. As we consider a flat terrain and only two cover types (grass and trees) in the simulation, it makes sense.

52. *Page 8, line 30, sentence with "good degree of confidence": This is not clearly justified. Please more directly and clearly address the possible shortcomings or impact of using this on your results. You should convince the reader that this mismatch does not adversely affect your results.*
A representation of the activation is a crucial point of this study as it directly links the calculated supersaturation to activated aerosol concentration. Usually, to find the Cohard et al. (2000c) parameter values, a fit is made on the aerosol lognormal distribution. Thanks to the CCNC, we get the exact curve of the evolution of the activated aerosol concentration, but only for supersaturation above 0.1 percent. As the activation in a fog layer is supposed to be under 0.1 percent, an instrumental method has been developed by Mazoyer et al. (2016) to retrieve the activation spectrum under this value. Using the combination of both information provides the exact activation spectrum, meaning that there is no shortcoming to use this method.
We have addressed this point more directly :
« Nevertheless, considering that the activation spectrum is deduced from measurements, it includes a good degree of confidence. » has been replaced by : « Deducing the activation spectrum from measurements provides the exact solution. »

53. *Page 9, lines 3-4, "good degree of confidence" : Compared to surface observations? Please be more precise with your statement.*
«degree of confidence » has been replaced by « agreement with observation »

54. *Page 9, statement on lines 5-6: making some assumption of ergodicity here? Taking time averages of point observations to compare to area-averaged simulated fields? Please describe more clearly the assumptions you are making and justify.*
No, it does not correspond to some assumption of ergodicity. The horizontal variability study (Fig.9a for instance) shows that the domain near the surface can be decomposed into 4 meriodional bands with similar characteristics inside each one : the first one upstream from the trees, the second one corresponding to the barrier, the third one downstream the trees and the last one far downstream the trees. The instrumented area is located inside the third one so we have averaged the simulated fields on this band to compare to the measurements.
You are right that the sentence was not clear. We propose :
« It should be emphasized that observations localized at one point will be compared to simulated fields averaged over a horizontal area located downstream of the tree barrier (blue contour area of Fig. 1b) representative of the instrumented area. We will indeed

see that the simulation domain is divided into 4 parts with significant differences between them, but similar characteristics inside each one.»

55. *Page 10, line 22: What does "reducing the spectrum" mean? I do not know what a reduction in the spectrum mean.*
We wanted to say that the number of larger droplets has been reduced. This part has been simplified and adapted to the new results : « During the whole fog life cycle, the model overestimates droplets with a diameter larger than 4 m and underestimates the smaller ones.»

56. *Page 10, line 24: "leaded" is not proper English*
OK.

57. *Page 10, line 24: Awkward use of "weakness". Maybe replace by "underestimated"*
OK.

58. *Page 10, line 24: What do you mean by "surface cloud water amount by sedimentation"? Do you mean to say "amount of water deposited on the surface by sedimentation"?*
Yes. This part has been simplified and the new comment is : « The cloud water deposition rate at the ground presents a maximum of 0.36 mm/day while the maximum of droplet sedimentation rate is 0.08 mm/day, meaning that the deposition is the main contributor to the cloud water amount at the ground. »

59. *Page 10, last sentence: Maybe an important point here about usefulness of more sophisticated formulations of visibility diagnostics for models. Your simulation results indicated that a simpler formulation based solely on LMC is adequate given the difficulty in simulating Nc. Perhaps this finding could be expanded upon here.*
Thank you. It has been added : « This explains why a simpler formulation of visibility based solely on $r_c$ is usually more adequate given the difficulty of simulating $N_c$ for the models.»

60. *Page 11, line 15: "allows to decompose formally" is awkward. Maybe change to "serves as a basis for decomposing"*
Thank you.

61. *Page 11, line 19: "consecutively to the flow", you rather mean "related to the flow perturbations"?*
No, we just mean that the layer of TKE deepens slowly due to the tree barrier. It has been corrected.

62. *Page 12, line 10, use of "rc" I believe you used "LWC" before. You should remain consistent throughout the paper.*
Yes, we agree. Only cloud mixing ratio is now only used throughout the paper.

63. *Page 13, line 8: drawning? Please revise*
Yes, replaced by « bringing »

64. *Page 13, sentence on lines 10-11 is unclear. Please revise.*
«The fog forms at the surface upstream from the trees, and 500 m downstream, while it appears first at elevated levels between both » has been replaced by « The fog forms at the surface upstream of the trees, and 500 m far downstream, while it appears first at elevated levels over the intermediate area between the trees and far downstream (Fig. 9d).»

65. *Page 13, line 31: statement with "even if measurements" is unclear. You mean"...probably overestimated, although this cannot be confirmed as measurements ..."*
Yes, thank you.

66. *Page 14, sentence on lines 24-25 is confusing. Please revise.*
We propose : « The main differences in dynamics between NTR and REF appear first on total TKE, with ~~the absence of stronger values in the first 40 metres in NTR, as they were restricted to the immediate vicinity of the ground~~a thinner layer of TKE values higher than 0.5 $m^2/s^2$ and smaller maxima (Fig. 8b). »

67. *Page 16, line 5: "removed fully deposition" should be replaced by "removed deposition altogether" for proper wording.*
Yes, thank you.

68. *Page 16, line 18, LWP was largely overestimated. Where? At the surface? If so, how is LWC at surface positively correlated to the depth of the fog layer? Please provide a clearer explanation.*
LWP (Liquid Water Path) corresponds to the LWC integrated on the vertical. As LWC is overestimated near the ground (Fig.13) and as the fog layer is deeper, LWP is overestimated. It has been completed by : « Due to the larger amount of cloud water near the ground, the dissipation at the ground is delayed by more than one hour . »

69. *Page 16, line 21: Is DE5 based on deposition on a grassy surface only, or is deposition over the entire tree canopy considered as well? In the context of this section, this text is not clear. Please clarify.*
DE5 was related to grass and tree canopy as it was like in REF. DE5 has been replaced by DE8 (deposition velocity of 8 cm/s) to answer to the newt point, and the principle has been clarified.

70. *Page 16, line 21: Why not use a value of 8 cm s-1, the upper bound suggested by Katata?*
OK, DE8 has been run and is presented instead of DE5. As explained in the introduction, the previous mistake on the deposition velocity has induced some modifications and now the DE8 simulation presents a significant improvement compared to REF.

71. *Page 16, line 22: Replace "diminution" by "reduction".*
OK.

72. *Page 16, line 29: Replace "the remove of" by "neglecting" for proper wording.*
OK.

73. *Page 17, line 26: I do not think "preformation" is a word. Maybe you mean "initial formation"?*
Thank you.

74. *Page 17, line 27: A DSD does not "move". Maybe "characterized by higher concentrations of larger droplets"*
Yes, thank you.

75. *Page 17, line 30: "dilutes" is not properly used here. You rather mean "decreases" or "diminishes".*
OK

76. *Page 17, line 30: Also this reduced effect impacts which field(s) in particular. Please*

*clarify.*
This has been clarified.

77. *Page 17, line 32, "fog slightly deeper": Please revise as "a slightly deeper fog layer"*
*OK*

78. *Page 18, lines 26-27: I do not understand the statement "diverged on the fog life cycle in the same way". Please revise your statement.*
This part has been removed as the text was too long.

79. *Page 18, lines 27-28: Not a very clear statement. Please revise. And be more explicit about what you mean by "dynamical conditions".*
This part has been removed as the text was too long.

80. *Page 19, line 10, "as the wind overcame this obstacle": Awkward formulation. Maybe "and associated perturbed mean flow and turbulence conditions" would be a clearer statement.*
OK, « overcame » has been replaced by « met »

81. *Page 19, line 17: replace "meeting" with "encountering" or "reaching".*
OK, "encountering".

82. *Page 19, line 17: use of the expression "dynamical gradients" is not specific enough. Do you mean "wind shear" in particular?*
Yes, thank you.

83. *Page 19, line 18, "became well-marked": This is awkward wording. Do you mean "became prominent"?*
Yes, this has been corrected.

84. *Page 19, line 23,"homogeneous". Where? Throughout the fog layer? At the top of the layer? Please be more precise in your statement.*
No, inside the cloud layer : « The cloud droplet concentration became quasi homogeneous in the fog layer when averaged over time but extremes of droplet concentration occurred locally near the top of the fog in the radiative cooling layer, with maxima preferentially upstream of the crests of the waves rather than downstream, in the ascent area. »

85. *Page 19, line 24: "evolved" rather than "involved"?*
Yes, thank you.

86. *Page 19, line 29: "damaging the visibility diagnostic" is awkward wording. Maybe "worsening visibility diagnostics"?*
Yes, thank you.

87. *Page 19, line 31: "The removal of the deposition process" is awkward wording. Maybe replace "The removal of" by "Neglecting".*
OK.

88. *Page 20, line 4, "Endly": You mean "Finally" or "Lastly"?*
Yes, lastly.

89. *Page 20, lines 4-5: The use of "reduce much more the number concentration" is awkward.*

*Change to "reduce the overestimated droplet number concentration" for a more precise statement.*
*OK.*

90. *Page 20, line 8: In what way "simulations remain very challenging"? Please explain.*
    They are very challenging due to the importance to represent correctly surface heterogeneities. This has been corrected.

91. *Page 20, line 12: I suggest you replace "cannot be neglected anymore" by "should not be neglected"*
    OK, thank you.

92. *Page 20, line 20: We should read "dewfall" instead of "dewfal"*
    Yes, thank you.

93. *Page 20, line 23: Change "no one" to "none" for appropriate wording.*
    OK

94. *Page 20, line 23: Change "to reproduce correctly" to "in correctly reproducing"*
    OK.

95. *Page 39: The citation of Hammer is not accurate. That paper has now been fully published and the citation should now indicate : Atmos. Chem. Phys., 14, 10517- 10533, doi:10.5194/acp-14-10517-2014*
    OK, thank you.

96. *The formatting of citations is inconsistent throughout the References section. In particular, the names of journals sometimes uses capital letters (as should be) and sometimes not. Please revise.*
    Yes, it has been done.

    *Note: Only the most important text corrections have been suggested. A much greater number of possible corrections have been omitted due to time constraints for the reviewer. I strongly recommend that the text be reviewed by someone with a higher proficiency in English.*