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***Interactive comment on* “On the roles of circulation and aerosols in the decline of mist and dense fog in Europe over the last 30 years” by G. J. van Oldenborgh et al.**

Anonymous Referee #2

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Review of “On the roles of circulation and aerosols in the decline of mist and dense fog in Europe over the last 30 years” by G.J. van Oldenborgh, P. Yiou and R. Vautard
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General comments: The manuscript provides an original contribution to the study of fog and mist/haze events from the novel point of view of characterizing the influences of aerosols and large-scale atmospheric dynamics. The focus is on long-term trends in the occurrence of reduced visibilities, particularly extending the results of a prior study to dense fog events. Evidence of a reduction of reduced visibility events is clearly shown and the remainder of the paper aims to identify the extent to which reductions in

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aerosol emissions and changes in large-scale circulation are responsible for this reduction. This represents a challenging task given the complex nature of the phenomena involved and is carried out in a compelling way. The paper is also generally well written and figures are clear for the most part. Nevertheless, some clarifications needed in numerous parts of the paper (see below). Some general criticism can be formulated however. First, it remains unclear why the analysis does not include visibilities in the range from 200 m to 1 km (lighter fog) so that the full range of reduced visibilities is represented. The reason behind this should be justified. Second, an area where the work lacks completeness is the discussion related to dense fog. Fog is a phenomenon with an often overlooked complexity. It is often the case that when one refers simply to “fog” in fact “radiation fog” is meant. I believe it seems to be the case here. Reality is more complex however, with foggy conditions appearing at the surface as a result of a variety of mechanisms other than the classical radiative cooling under clear skies and light winds. Fog can even take place within synoptic-scale low pressure systems. Likely of relevance to the study under review, fog formation as a result of stratus lowering, and/or stratus interception by terrain in the more mountainous areas of Europe may be common occurrences. Such events are more likely related to mesoscale or local dynamical influences and conceivably less sensitive to aerosols than radiation fog. Such scenarios may be related to the unexplained variability found in the study. Also, the role of aerosols on radiation fog remains somewhat unclear in terms of whether they promote or inhibit formation of dense fog, depending on their size and chemical characteristics, type of fog etc. Therefore focusing on the correlation of sulfuric emissions implies that only certain types of aerosols are taken into account, which is another limitation of the study. In fact the title of the paper is a bit misleading in that respect. This should be briefly discussed and justified. Therefore it is apparent that only a subset of fog types and environmental influences on fog are addressed in this work. In my opinion, this does not diminish the relevance of the study, but this should be clearly acknowledged early in the paper (in the introduction), accompanied by a brief discussion on the elements not addressed in the paper with proper references given. Also, the possible

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impact on the results of uncertainties related to the aspects which cannot be easily included in the analysis, as outlined above, should be discussed in order to present the study in its proper context.

Specific comments: 1) The value of 40 % quoted in the abstract is not clearly supported by statements in the core of the text. Please revise the abstract to only reflect what is clearly stated in the text.

2) Line 21, p. 23988: the statement “. . .isolating the ground from upper atmospheric layers” is too general. Fog isolates the ground from the radiation emitted by the atmosphere, but fog-top radiative cooling acts to de-stabilize the layer, generating turbulent mixing within the fog layer therefore leading to a greater coupling (by turbulence) of the ground and the air above. What is really meant by this statement should be clarified.

3) Paragraph from lines 23 to 28 on page 23989 is unclear. Your aim is to provide a general description of the methodology, but I find myself rather more confused than enlightened. Why are daily patterns with fog/mist compared with seasonal mean patterns with anomalously large number of fog/mist days? I am unclear as to the reasoning. Please provide more details. What is meant by “concise description”? Again please provide more details.

4) The entire discussion about the role of fireworks on fog formation in Section 4 should be dropped as the evidence provided is not compelling enough to convince the reader that indeed the particles resulting from the fireworks were an important influence, rather than fog presence being merely the result of natural influences. Figure 3 does not show anything relevant to the discussion. The role of aerosols on fog should rather be more completely discussed from the point of view of established literature.

5) Line 19, p. 23994: in “weighted with the inverse error squared”, which “error” is referred to?

6) Line 6, p. 23995: “negative” correlations are spoken of, whereas positive values are

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shown in Fig. 4? Please clarify the discussion.

7) Line 14, p. 23996: Isn't the statement "...are also statistically significant for (dense) fog" a bit too strong or broad, given that results of 1/3 of stations with significance at $p < 0.1$ and 1/6 at $p < 0.01$ are presented in Section 3?

8) First paragraph of section 5.1, p. 23996 is unclear. It is said that composites of daily patterns of SLP are used, whereas composites of SLP anomalies are presented in Figs. 6 and 7. Please provide a more detailed description of your methodology: what is calculated and how, anomalies with respect to which reference? etc. Generally speaking, the discussion would be clearer if when anomalies are shown, that the reference patterns be shown as well. In fact, on the following page, a statement is made about the fact that anomalous gradients cancel out the climatological gradients, but the reader is not presented with any evidence of that.

9) Line 1, p. 23997: should the text refer to an anomalous geostrophic flow since large-scale SLP anomalies are discussed?

10) Line 17, p. 23997: a "southerly geostrophic flow" is described. Is an anomalous flow or the absolute flow referred to? If anomalous, please state clearly, if absolute, how can it be assessed from SLP anomalies? In that latter case, maps of climatological SLP patterns should be provided.

11) It is unclear what is sought after in section 5.2 and how the goals are different than presented in section 5.1. It is said that "check whether the high-frequency daily signal can be averaged to a lower-frequency seasonal signal". But the prior analysis was based on seasonal composites of daily SLP patterns. Doesn't that correspond to some kind of averaging already? What does the regression analysis provide that the prior analysis didn't? Please clarify.

12) Also, it remains unclear what the performed regression is or represents. The average of SLP values is calculated for every grid point of the re-analysis over Europe,

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for every season during the 30 years, the same seasonal averaging is done for the number of mist/fog days for one location (ex. De Bilt) and for each (grid point, number fog/mist days) pair, correlation is calculated? Is that it? Please provide more details about which processing is actually applied, how the regression is calculated on what and its meaning. Since it is not clear to me what is done here, I cannot provide further comment on the results of this section.

13) Line 14, p. 23998: the term “reasonable” is unclear in the context of a quantitative scientific analysis. Which metric is used to deem the correspondence between the patterns reasonable?

14) Line 19, p. 23998: the expression “beautiful day” should probably be replaced by “clear day” or even “clear night” to be more precise since what is implied is the formation of radiation fog.

15) First sentence, section 6.1: The explained variance shown in Fig. 15, not in Figs. 12-14.

16) Line 1, p. 24001: The sentence is confusing. If an association between fog and positive vorticity is obtained over seasonal time scales, but individual fog days are mostly characterized by negative vorticity, doesn't that simply says that seasonally averaged quantities are not good representers? At least for summer. Why not simply state that clearly as a conclusion to this section? Please clarify.

17) Lines 7 and 8, p. 24004: If trends are properly expressed as percentages of absolute numbers, negative numbers of days of fog cannot be obtained (an unphysical outcome!). The discussion of this paragraph should be reformulated and the reference to negative number of fog days taken out as a justification that the rate of decline of fog cannot persist.

18) Line 25, p. 24004: Proper references should be given with the statement ending with “. . .in agreement with micro-meteorological modeling studies”. Which studies are

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you referring to?

Technical corrections:

- 1) Line 23, p. 23992: In “. . . trends mainly due to chance” is an ambiguous statement. Do you rather mean that the detected trends are an artifact of data sampling ? Or something along those lines.
- 2) Line 3, p. 23994: typographical error: repeat of “these” in “one of these fog episodes”.
- 3) Fig. 4, p. 24013, please properly label x-axis (which variable is represented) and indicate units of the variable.
- 4) Fig 5. Caption on p. 24014 should read “The annual number of days. . .”.
- 5) Figs. 6, 7, 8, 9 should be more clearly labeled, with the name of the station put at the top of the figures, the caption “winter” and “summer” put on the left-hand side to designate upper and lower panels respectively. This would help the reader to grasp what is shown at a simple glance, rather than have to read to long caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23987, 2009.

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