

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.

G. J. van Oldenborgh et al.

oldenborgh@knmi.nl

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We would like to thank the reviewer for his very careful review of our article. Please find below the answers to his/her comments and how these have affected the main text.

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

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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 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).

Thank you.

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.

We did the analysis for all visibilities in this range. However, we chose to present only the two extremes for clarity, and because a meteorologist at KNMI (Geert Groen) advised us that the 2 km visibility is very important for air traffic and the 200 m cut-off for road traffic.

We added the justification and a note that the results for the other cut-offs can be obtained from the authors, and added a note of caution on the 100 m value in the description of Fig. 4.

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.

In the context of this study, the term ‘fog’ simply refers to visibility less than 500 m and ‘mist’ to situations with visibility less than 2 km. The majority of these cases are prob-

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ably radiation fog, but on coasts there are clear signs of advection fog, as (implicitly) noted in the text when discussing summer fog at the North Sea shores.

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.

We found that mountain station had completely different fog characteristics than lowland stations (trend, dependence on circulation), because low visibility there is, as you suggested, the effect of terrain intercepting clouds. We therefore excluded stations above 1000 m. The remaining stations had a more homogeneous behaviour.

Because our observational database does not distinguish between different kinds of fog, for this statistical analysis we just analysed all low visibility days in the same way, trying to find some mechanisms that affect their occurrence.

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.

The emission patterns of SO₂ and NO_x are so similar that Fig. 4 looks virtually identical if we use NO_x emissions as a proxy for aerosol loading. For this coarse statistical analysis the details of the aerosol subspecies are not so important. In the future this will likely change, as sulphur emission seem to be easier to control than nitrogen emissions, which also originate from e.g. intensive agriculture. The roles of different

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aerosols on fog formation is an interesting subject for further research.

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 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.

We have adjusted the introduction based on these recommendation to read as follows.

‘In this paper we first extend the trend analysis to dense fog (less than 200 m visibility). We group all causes of low visibility and do not distinguish between different mechanisms (e.g. radiation fog, advection fog, stratus lowering, aerosol haze), but exclude mountain stations where low visibility is mainly due to clouds.

Next we perform a statistical analysis of two factors that are likely candidates for explaining the trend in low visibility: aerosols and atmospheric circulation. These factors are used in meteorological fog forecasting systems.

For aerosols we compare the spatial and temporal patterns of the trend in low visibility with the patterns of SO₂ emission trends in Europe. The results are very similar when we use NO_x emissions, as the trends in these are strongly correlated over the period considered. The statistical analysis therefore does not enable us to study the different effects of different types of aerosols. We do consider the possible effects of changes in urbanisation using the same spatial comparison technique.’

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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.*

We chose the opposite solution and explicitly added the 40% to the discussion of Fig. 15, on which this statement is based. From the figure it can be seen that one third of the stations show correlations $r > 0.6$ for 2 km visibility in winter. The underlying data show that 25% of the stations have a correlation larger than $\sqrt{0.4}$, justifying this number.

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.*

Changed to 'isolating the ground from atmospheric layers above the fog' to take into account the mixing within the fog layer.

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.*

We have clarified his paragraph to:

'The atmospheric circulation is first investigated using the daily pressure field. We consider the daily large-scale weather patterns affecting fog and mist oc-

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currences at three stations for winter (October–March) and summer (April–September). This reveals which daily circulation types lead on average to low visibility at these stations. Next we compare these daily circulation patterns to the seasonal mean patterns associated with anomalously large number of fog or mist days in that season (relative to the trend). The most important features of these seasonal sea-level pressure (SLP) patterns turn out to be the local gradients at the station where the low visibility has been recorded. These gradients (the geostrophic wind) are used as a concise description of the large-scale circulation to investigate its effect on the number of low visibility occurrences at all stations.'

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.*

We have deleted the example of the New Year's fog. The climate research community is not convinced that aerosols affect fog, judging from reactions to presentations of this research at conferences. We could not find more literature from the fog community to support our claim than the papers cited in this discussion.

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

Changed to 'weighted with the inverse square of the error on the trend (from the fitting procedure)'.

6. *Line 6, p. 23995: “negative” correlations are spoken of, whereas positive values are shown in Fig. 4? Please clarify the discussion.*

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'We also investigated the spatial correlation of low visibility trends with an estimate of the trends in urbanisation over 1976–2006. The fraction with urban land use on a 0.5° grid 1976–2005 (Hurt et al, 2006) has been averaged to the same 2.5° grid as the emission data. From the meteorological experience that fog occurs less frequently in built-up areas we expect that more urban land is associated with less fog and mist occurrences. The spatial correlations between urbanisation trends and low visibility occurrences are indeed negative for all ranges, although not all are significant at $p < 0.05$ (one-sided). For easier comparison with the aerosol signal the sign of the correlation coefficients has been changed to positive in Fig. ??.'

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?*

In section 3 we discussed the significances of the trends, i.e., how likely would a chance distribution of trends as observed be if there are no physical trends. The conclusion was that there is clear evidence for trends in low visibility observations. Having established that the trends are real, we try to find factors associated with those trends, and the association with aerosol emissions is statistically significant at all ranges (Fig. 4), justifying this statement.

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.*

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In order not to add too many panels we have added a figure with the climatological SLP patterns in front of the composites of anomalous SLP. As the anomalies are smaller than the climatological gradient in most of the domain this is clearer than showing the full anomalies. We have also explained the procedure in more detail in the text, using this new figure.

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

Yes, this error has been corrected.

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.*

The climatological geostrophic flows in Switzerland are weak. The description has been made more precise, with reference to the added figure of the climatological SLP fields.

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.*

We have clarified the reasoning behind this by rewriting the first paragraph of 5.2. As is shown later, the correspondence fails for summer fog, which is associated with other weather types on the seasonal scale than on the daily scale. This can be traced back to the required preconditioning of wetness, which is opposite to the clear-sky conditions on the fog day itself. A similar problem is encountered

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when studying the associations between coastal showers and sea surface temperatures: high temperatures intensify the showers, but these high temperatures result from a dry summer.

The new introduction reads: 'In the previous section we established the daily circulation patterns associated with single low-visibility days. The next step is to investigate whether the seasonal mean pressure patterns of seasons with many fog or mist days are similar. In other words, we check whether the high-frequency daily signal can be averaged to a lower-frequency seasonal signal. This is not necessarily the case, as fog occurrences may depend on preconditioning that has different characteristics than the weather on the fog day itself. Only if the resulting seasonal mean circulation patterns are similar to the ones found in the composites can we use these as predictors for the number of low-visibility situations, and proceed towards the goal of attributing part of the downward trend to changes in circulation. If they disagree a more sophisticated analysis taking both the preconditioning and the weather on the fog day itself into account will be needed.'

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, 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.*

We have added an extra paragraph to explain the analysis in somewhat greater detail:

'As a measure of the effect of the seasonal mean circulation on low visibility we use the regression of the seasonal mean pressure on the number of dense fog

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or mist days. The maps show how much higher or lower than average the sea-level pressure is at each grid point when the number of dense fog or mist days at the station under consideration is one higher. A simple high-pass filter has been applied (year-on-year differences) to eliminate the influence of the trends to first order.'

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?*

Added field correlation coefficients over the domain as a measure of the agreement.

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.*

It has been replaced by 'clear-sky day'.

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

Thank you for pointing this out.

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.*

The requested clarification has been added to the text: 'This implies that the seasonally averaged circulation alone does not give a good description of the occurrence of mist and fog in the summer season.'

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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.*

We have deleted this paragraph.

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 you referring to?*

We have added a reference to the section in which these are discussed.

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.*

Indeed. We changed the sentence to 'In these regions fog is rare and the trends mainly due to chance fluctuations of the weather in a small sample.'

2. *Line 3, p. 23994: typographical error: repeat of "these" in "one of these fog episodes".*

Thank you.

3. *Fig. 4, p. 24013, please properly label x-axis (which variable is represented) and indicate units of the variable.*

The label 'minimum visibility [m]' has been added

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4. *Fig 5. Caption on p. 24014 should read "The annual number of days. . .".*

Added 'annual'.

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.*

Done, also in the new figure with climatological pressure patterns.

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

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