Response to Referees – My Response in blue

RC1: 'Comment on egusphere-2022-383', Anonymous Referee #1, 06 Oct 2022 reply

Review of manuscript "Biomass Burning and Gas Flares create the extreme West African Aerosol Plume which perturbs the Hadley Circulation and thereby changes Europe's winter climate"

This manuscript explores a hypothesis identified in the study that anthropogenic influence from increased biomass burning and gas flaring in West Africa has perturbed winter European climate via circulation changes associated with the frequency of continental high pressure blocking of North Atlantic weather systems.

The analysis identifies a specific seasonal aerosol plume that emerges each year in West Africa from December through to April, and introduces a terminology "the West African aerosol plume", within a proposed categorisation of 8 "continental-scale aerosol plumes" in existence in the present-day atmosphere.

The manuscript'a Abstract acknowledges the analysis is to-some-extent preliminary identifying the presnted information as "a first step" in relation to a specific tele-connection between the central Africa pollution and winter climate in Europe.

However, whilst the topic is interesting, and has the potential to progress to a pubishable analysis, several parts of the current manuscript are poorly worded and present too certain a narrative in relation to the magnitude and causality within the results presented: a set of individual trend analysis of different observational datasets.

Also, the climate influences from anthropogenic aerosol are well-known, whilst the manuscript seems to present the analysis as establishing something new.

This statement is incorrect.

Nowhere in the literature have I found any suggestion that aerosols over West Africa are the cause of the changes in Europe's winter climate. Indeed, references in my paper in section 1.7 [Schaller et al., 2016] and [Huntingford et al., 2014] suggest specifically that more work is needed to identify: (1) the anthropogenic source of the changes; and (2) the contribution of aerosols to the changes.

If Referee#1 can provide references showing that West African aerosols change the winter climate of Europe I am more than happy to acknowledge and discuss them.

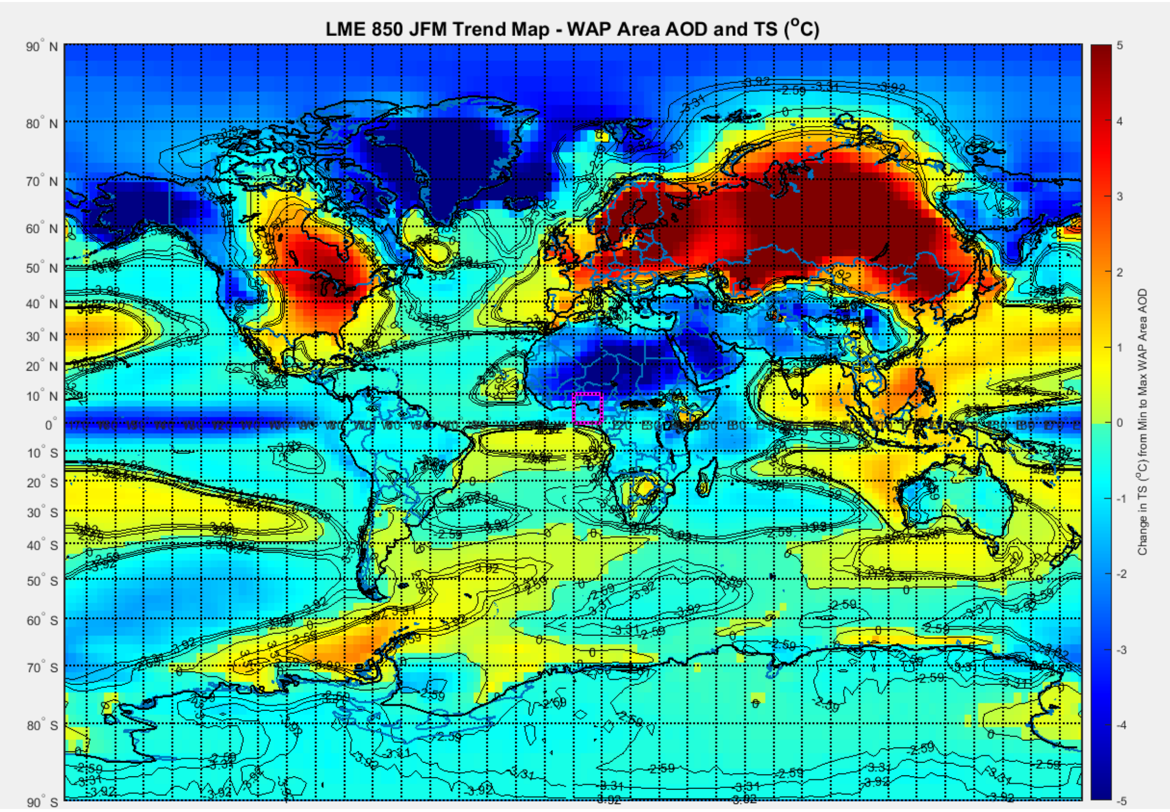
In its current state, the particular advance presented in this manuscript is not sufficiently explained. Whilst there is a moderate referencing of other related studies such as the Fawole et al. (2016b) analysis to establish signatures of gas-flaring within AERONET aerosol optical properties, the manuscript in its current form does not yet inform the reader of the specifics of the findings in relation to the relatively recent progression in understanding from being able to isolate the role that gas flaring black carbon and sulphate aerosol plays alongside desert dust and other aerosol types/sources within the West African aerosol plume.

I included an analysis of the sources of the WAP aerosols in the Supplementary Information submitted with the original paper which shows the aerosols are predominantly carbonaceous. I will add additional references to the revised paper which discuss the makeup of aerosols derived from gas flares eg [*Fawole*, 2016] and [*Conrad and Johnson*, 2017]

Overall, the manuscript reads like a report that has not yet been worked-up sufficiently to be at a publishable-level analysis, and requires further work to set out and establish a particular advance it represents within the current scientific literature on at least one of the two topics it addresses: the West African aerosol plume (and/or associated radiative effects), and/or its influence on European climate.

The purpose of the paper is to identify the cause of the changes in Europe's winter climate which, in the case of the UK floods, have caused numerous deaths by drowning. It is surely incumbent on the journal and Referees to facilitate the publication of new research into their cause which will enable the mitigation of the WAP as I suggest in section 5.2.1 of the original paper.

I will add LME trend maps from the eight LME simulations to the paper which show the variation in a parameter analysed from the minimum to maximum WAP AOD. The example from the 850 LME simulation below shows the variation in surface temperature due to the WAP. All the other LME simulations show the same result with little variation which demonstrates that the WAP is unquestionably the driver of the changes in the European winter climate.



Trend Map of the LME 850 simulation surface temperature difference between the minimum and maximum WAP Area AOD which shows significant warming in Europe north of the Alps and a massive arc of warming from Iberia to the Arctic and eastern Asia. The WAP Area is shown outlined in magenta.

A general area of improvement also is in the scientific writing style, where the precision and specific language used needs to reflect the multiple influences that combine both from the aerosol plume and its climate affects. Too often the current wording sometimes seems to indicate a unique causality of effect, via "creating" a system or effect.

This is because there actually is a unique causality between the West African aerosol Plume (WAP) and the changes in the climate of Europe in winter. The data from multiple sources shows the same effects with an infinitesimal chance of being wrong and, within the LME data, none of the eight forcing scenarios shows any significant variation from the others, showing clearly that the WAP is the driver of the changes.

AND, I note again, this is an existential problem for some of those who have been subject to flooding in the UK in recent decades.

The title for example refers to the West African Aerosol Plume, and whilst the definition presented in the manuscript is clear this terminology refers to a particular season, the reader cannot be expected to appreciate that within the title itself.

The second to last word in the title is “Winter” – is this not enough to clearly identify the fact that the paper addresses the Winter season which I define as JFM in the paper?

The climate influences the manuscript points to are currently presented only in the frame of correlations, and whilst that does not necessarily preclude the manuscript from being publishable, the narrative of the manuscript presents the correlations as indicative of a causal relationship.

I show that from the three different datasets used, the chance that all these results would show the same result and be wrong is 10^{-16}, a vanishingly small number which should be enough to satisfy any referee.

I will add LME trend maps to the revised paper which show the changes wrought by the WAP and which reflect the measured changes without correlation.

Although previous studies have indeed identified a strong signature of gas flaring aerosol (Fawole et al., 2016), this study explores only trends in total aerosol optical depth, then dependent on that previous finding, rather than this being a result established by this manuscript. So the title should not present that as the topic of this paper.

Gas flaring is an indisputably significant source of aerosols throughout the year in Nigeria as I show in the paper and in the Supplementary Information, and I can therefore see no reason to omit “gas flares” from the title.

A revised title should reflect at least some of the broader context for the research, and set out the main finding it addresses (rather than a simple causality and specifics of the datasets it analyses or methods it applies).

In my opinion the title should be reasonably short and clearly identify the topic and results which this title does.

When re-drafting the manuscript the author needs to ensure the narrative conveys the broader influences on the North Atlantic and European climate, with currently almost no recognition of the primary role of greenhouse gas changes and more general anthropogenic aerosol influences (see e.g. Booth et al., 2012).

The LME includes greenhouse gases and the individual simulation data provided in the original paper shows greenhouse gases (GHG) have no more significant effect than the other forcing agents analysed. If they did the GHG plot would be offset along the y axis of all these plots and it is not. Hence GHG's have no "primary role" in this significant change in Europe's climate and, whilst appreciating that this is difficult for researchers to accept, this is precisely what the data shows. I will add a comment to the paper stating that GHG's do not have a primary role in these changes using the eight LME simulations and trend plots as proof.

A potential future re-working of this manuscript could address trends in multi-wavelength aerosol optical properties, aligned to the AERONET analysis from Fawole et al. (2016), but the current analysis seems to assume 100% of the AOD comes from these sources.

The paper is already long and will be longer in its revised state and I am reluctant to add even more analysis re multi-wavelength aerosol properties which are covered in Fawole's Ph.D. thesis which I will include as a reference in the revised paper.

The changes in the climate of Europe are recent, and we are therefore looking for recent changes to the aerosol plume over West Africa which my paper shows to be from gas flares and biomass burning which were zero and significantly lower in 1950 as oil production in Nigeria did not start until 1958 and the population of West Africa was significantly lower in 1950 – 71 million cf 211 million in 2021 a three-fold increase!

I would advise the author also to present the analysis within established metrics for the effect he identifies is perturbed by this regional aerosol system, primarily the index for the mode of climate variability known as the North Atlantic Oscillation (e.g. Stephenson et al., 2006).

I will add a discussion of the NAO to the paper using the LME to show that the WAP affects sea level pressure in both the Azores and Iceland and thus the NAO.

The Introduction needs also to be refer to some of the extensive literature exploring changes in blocking frequency and European winter climate (e.g. Shabbar et al., 2001; Buehler et al., 2011; Breton et al., 2022). The manuscript would be strengthened also by explaining the role of gas flaring pollution, see for example he review paper by Fawole et al. (2016a), and references therein.

I will add a discussion of these references to the revised paper

I realise the author will be disappointed in this rejection and re-submission finding, but the manuscript has promise in identifying the specifc tele-connections between the under-appreciated gas flaring sources and European climate.

Disappointed – yes.

Anonymous Referee #2

Topic of this paper it self is interesting as in the future there will be interesting emerging aerosol patterns in Africa. This paper concentrates on Biomass burning aerosols on western part of Africa and their influences on European winter climate.

Whilst the paper does concentrate on biomass burning aerosols, the effects of the large flaring of “associated gas” which emanates from oil production in Nigeria cannot be ignored. This issue is addressed in the Supplementary Information submitted with the paper which I hope Referee#2 read.

General comments:

While the topic is relevant and interesting, the author interpred causality from only correlation from different datasets.

I show that from the three different datasets used the chance that all these results would show the same result and be wrong is 10^{-16}, a vanishingly small number, which should be enough to satisfy any referee.

However, I will add trend maps from each LME simulation to the revised paper which all show: First the same results; and Second the same change in pressure, precipitation and temperature as measured data thus demonstrating conclusively that the prime forcing agent of the recent changes to the winter climate of Europe is the aerosol plume over West Africa.

Example, one can include climate model analysis where individual aerosols sources and their effect are studied separately. Author does not presents any evolution of different aerosol species but assumes that observed trend is anthropogenic and not an example dust from sahara.

This is incorrect and I can only assume Referee#2 did not read the Supplementary Information submitted with this article.

In the Supplementary Information I show with references and data that: the sources of aerosols in the region analysed are biomass burning and gas flares in the oil production industry; and that dust from the Sahara, or more accurately, the Bodélé depression in the Sahel, and volcanic eruptions are not significant sources - lines 33 to 128.

I will add a reference to Fawole’s thesis to the revised paper. However, the Aeronet site used is at Ilorin which is some 500Km to the NNW of the gas flares in the Niger Delta and, as can be clearly seen from the aerosol maps presented, the prevailing winds flow south and west in this region and not all of the gas flaring aerosols will be detected in Ilorin.

Author should include also relevant previus studies and link these to new narrative of region aerosols and emergin aerosol patterns. The topic it self is relevant but this draft need much more detalied analysis to be accepted as scientific publication

I included in the introduction a discussion of previous studies of drought in Iberia, warmer temperatures in Europe north of the Alps and floods in the UK. A discussion of the sources of the West African aerosol Plume (WAP) is in the Supplementary Information, and the paper in section 1.3 includes a discussion of the evolution of the WAP due to increased population and oil production in southern Nigeria.

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