**Review of paper acp-2015-41**
**Variability of aerosols forecast over the Mediterranean area and during July 2013 (ADRIMED/CHARMEX), L. Menut, G. Rea, S. Mailler, D. Khvorostyanov, and S. Turquety**

Dear Editor and reviewers,

We acknowledge the reviewers for the time spent to evaluate our work. We also acknowledge the Editor, Dr. S. Kazadzis, for its comments. We made all proposed changes in the revised manuscript: **the manuscript is shorter, with less figures, more explanations about the observations and the statistical scores, more bibliography and was completely rewritten, then 'english checked'.**

There is some common remarks which can be synthesized:

1. *The framework of the study was not enough described.* More generally, our work takes place in the CHARMEX program and this paper is thus submitted to the CHARMEX special issue of ACP. This program being a multi-year program, our study focusses on a specific part of this program, the ADRIMED project (June and July 2013). But the overview paper by [Mallet et al.] describing the whole project and the instrumental set-up is in late and not yet submitted to ACPD. This is why we have to add informations about the project, even if, normally, a citation to the paper of [Mallet et al.] would have been sufficient.

2. *Is this study a continuation of a previous study?* This is partially right and this was certainly at the origin of some misunderstandings. The reviewer #1 considers that the topic is not new enough to deserve a new article and the reviewer #2 considers we have to add details (but already published). These two views cannot be satisfied simultaneously, being contradictory. The first paper, [Menut et al., 2015] being now published, we think that the best way is to be more clear and precise to show that the present study has very different scientific questions and deserves an independent publication.

3. *This is a model validation study.* Not really. The validation study was the main topic of the previous paper, as well as others papers in the special issue of ACP-AMT. In this study, our goal is to quantify the spread of the forecast and to estimate if the forecasts are far from the observations (when available) or not.

4. *The word 'variability' is not preperly used:* In fact, this is much more the spread of the forecast and this was changed in the manuscript. To follow the Editor and reviewers recommendations, the title is now **Aerosol forecast over the Mediterranean area during July 2013 (ADRIMED/CHARMEX)**

5. *The English has to be completely checked and improved*: this was done and the whole paper was completely rewritten and ckecked. The paper is shorter, with less figures and more conclusive text.

We have also to write that we (the authors) were shocked by some sentences in the two reviews, especially the review#2. This is difficult to revise a paper when a reviewer wrote your work was done in an 'amateurish way', dealing with 'evident' questions and not up to date with the international literature. This kind of sentences is more insulting than motivating. In addition, the reviews are quite short and offers no real improvement, no suggestion, no scientific discussion and no reference to add.

Finally, please note that our answers are in blue in the text and after each reviewers remark.

Best regards,
Laurent MENUT
July 3, 2015

## Message from the Editor

S. Kazadzis (Editor)
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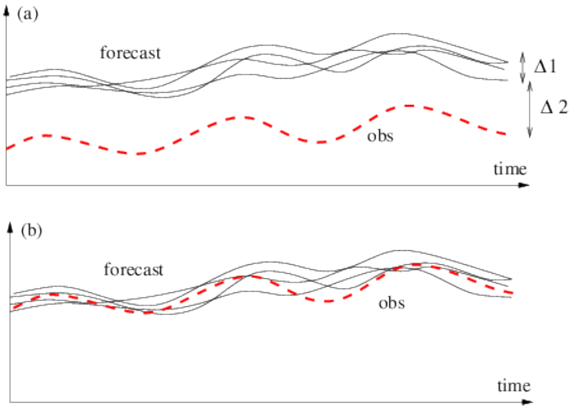
Received and published: 17 June 2015

Dear authors,
Here is an additional document with suggestions, trying to summarize and smooth a bit the reviewer comments.

### General comments

I agree with the two reviewers that the document needs improvement on the English language. This has to be fixed in the revised manuscript. This problem has also an impact on the discussion of the results.
The manuscript was completely rewritten, then checked for the english.
I do not agree with a point of the reviewer that this is exactly the same work as the previous Menut et al paper as this is an evaluation of the forecast scheme of the two models just using the same data. However, I do agree with the fact that there is a duplication of figures etc. So I suggest to focus on the comparison itself and not to duplicate time series etc.
In this study, we want to evaluate the spread between the forecast and see if this spread is far from the reality, i.e with the measurements when they are available. The little figure below try to illustrate this: $\Delta 1$ represents the spread between the forecasts and $\Delta 2$ represents the difference between the forecast ensemble and the observations. The key question is to explain the forecast scores: if $\Delta 2$ is large, it means that the model is biased (fig.(a)). A wrong 'pollutants' forecast is thus not due to the chaotic spread of the forecast. If $\Delta 2 << \Delta 1$, it means that the model is not particularly biased and the forecast accuracy mainly depends on the model forcings: the ability to have a robust forecast of the main forcings, i.e the meteorology and the emissions (fig.(b)).
Note that, following the Editor recommendation, the time series were removed from the manuscript, since the useful informations to estimate $\Delta 2$ are contained in the biases results.




The presentation and the form of the sections and sub-sections can be significantly improved. AOD and AOD maps can go together, also meteorological parameters, emission, aerosols can be the three main sections. Repetition of time series and variability in every section can be avoided.
The sections were completely revised in order to be more clear. We have now three main sections for the results: (1) meteorological variables, (2) emissions, (3) aerosol.
The term variability is used in a wrong way most of the times in the manuscript, this makes it very difficult for the reader to follow arguments about model-measurements differences and sources of uncertainties. (see also below).
The term 'variability' is used here to characterize the spread of the various forecasts. It is now replaced by 'spread'.
If the topic is accuracy of aerosol forecast then i miss the impact on the accuracy of the emissions and the meteorological parameters on the aerosol forecast model accuracy.

Yes, the main goal is the accuracy of the aerosol forecast. This is difficult to estimate the accuracy of the emissions and the meteorological parameters on the aerosol forecast: to quantify this, it would be better to make a sensitivity using several emissions and meteorology and quantify the variability of the modelled aerosol. This is often studied, using Monte-Carlo methods for example. But here, this is not the purpose of the paper. We are using the meteorological forecast as it is and we want to quantify the spread of the forecasted aerosols using these several forecasted forcings.
Title: The current title does not described the context of this work. I would use simply an evaluation of wrf and chimaire over mediterranean for July 2015. Or something like that.
We think it is important to keep, at least, the word forecast because this is a very specific kind of study. The model evaluation was much more the topic of the prevous paper done for the direct analysis of july 2013 over Mediterranean.
Abstract "The goal of this study is to know the reason why the model does not always simulate in advance what is finally observed: is it due to systematic biases in the models used or to a too large variability due to the real non-linear nature of the meteorology and chemistry?"
This is a difficult question to answer. I would rephrase this as a direct sentence on the objectives of this project which is more or less the presentation of the performance of the models used and the investigation of the reasons behind observed deviations. I would add some numbers in the abstract in order to include information on the final accuracy and the main findings. The abstract now it is more or less described in a "general" way.
We added some quantifications in the abstract. Indeed, the question is not simple, and we tried to formulate it in a better way. But the goal of the study is just to quantify is the model is able to provide satistactorily forecast, and if not, why.
Introduction 10343 line 26: Both reviewer's do not like the question that is raised here. I would say that the question itself is not easy since it is not easy (or possible) to clearly separate model errors and the (atmospheric) system's non linearity. In addition, I would say that if this is really the question you want to raise then it is not answered by this paper.
Yes this is the real question we are adressing. Thus, we added more elements in the paper to better answer this question.
I would make things more easy and I would suggest that you are searching for model-measurements deviations and the quantification of the sources of errors.
We quantify the forecast errors and the main goal is to see if this can be link to the forecast errors of the meteorology and the emissions.
Section 4 I would name this section Time series and statistical results. I would also add a second paragraph describing the way that you are presenting the analysis. Time series, statistical results, statistical parameters used, short discussion, more discussion in the conclusion section etc.. Also you don't have to mention variability in every paragraph but just the variable name would be enough There are several cases in the text that the discussion is difficult to follow: For example: "Figure 2 shows that the variability between the forecast leads is lower than the differences between the observations and the model." The variability between two things have no meaning, moreover if these two things is a. a forecasted parameter and b. the difference of this forecasted parameter with the measurement. In general the term variability is misused throughout the document. I would suggest to refer more to the difference of model vs forecast. If this parameter is highly variable means that the variability of each of the parameters (forecasted and measured) differs.
There are several minor comments raised by the reviewers that have to be taken into account.
We agree and they were all taken into account.
I would suggest a major revision including all the above. I do agree with a reviewer comment that the material exist but is not publishable in this form. Practically after this new revised document is ready scientific points raised here and in the reviewer's reports could be answered within the responses to the reviewers accompanied by the full new manuscript.
All questions were treated below. We made major revision, since numerous figures were removed, more references were added and the text was completely rewritten.
best regards SK

## Answers to reviewer #1

Received and published: 8 June 2015

**General comments:**

1. As mentioned several times by the authors, this article has a companion paper:
Menut, L., Mailler, S., Siour, G., Bessagnet, B., Turquety, S., Rea, G., Briant, R., Mallet, M., Sciare, J., Formenti, P., and Meleux, F.: Ozone and aerosol tropospheric concentrations variability analyzed using the ADRIMED measurements and the WRF and CHIMERE models, Atmos. Chem. Phys., 15, 6159-6182, doi:10.5194/acp-15-6159- 2015, 2015.
In particular, the simulation period, domain, model system and configuration, observations and general topic are either the same or similar, as also evident when comparing both titles.
Similarities in the two papers are also evident in the Results they present: both include the time series of atmospheric temperature and AOD (e.g. Figures 2 and 10 of this manuscript vs. Figure 8 and 14 of the companion article), and a part of model evaluation (e.g. Table 2 vs. Tables 4 and 5, or Table 4 vs. Table 8). Also, I couldn't help noticing the phrase "to understand the several types of meteorological variabilities influencing ozone and aerosols concentrations", which is only relevant to the previously published article and not to this manuscript, as this does not deal with ozone.
The analysis of june-july 2013 in the Mediterranean, using measurements and modeling was the topic of the previous study (as well as other papers currently submitted in the Charmex special issue of ACP/AMT). This is not the main topic. Of course, the region and the period are the same, but this is common, when you have a field experiment, to have several studies dealing with different scientific questions. In our case, the goal of the present study is to evaluate if the forecast was good and, if not, if this mainly due to the model biases or to the chaotic evolution of the forecasted meteorology and emissions.
Lastly, and as noted by the authors (cf. pp. 10344 lines 13-14) the added value of this study is the comparison among the 4 simulations/day. Thus, taken into account that most of the above information is duplicated, a lot of text could have been shortened, and/or moved to supplement and/or removed (pls. see specific comments below), the remaining part -to my view- cannot support a stand-alone article.
The added value is not only to add 4 simulations in place of one. The added value is to compare the forecast between them and to estimate if these forecasts are far from the reality or not. This is a double discussion.
Thus, my suggestion would have been to merge these two papers to one. In this way, the extra work of this study, which is mainly the inter-comparison among the 4 simulations and the observations, would have been a subsection in the Results of the already published article. Now, that the other article is already published in ACP, I am afraid I have no alternative but to recommend its rejection from publication.
If the main argument to reject the paper is that this is not new compared to the previous one, this is only because the reviewer did not understand the main goal of this one. If this is only due to problems with the wording, we hope that this new manuscript will remove this feeling. The paper was completely rewritten.
2. The language is the second major issue of this article: is not at all fluent and precise. A lot of grammar mistakes (e.g. precipitations, informations, contraints, variabilities, aerosols optical depth, the fires emissions), syntax errors, awkward and inappropriate expressions (e.g. "Aerosols sources and sinks studies remain difficult", "to act at the right time and place to reduce the anthropogenic part of the emissions.", "Logically, ...", "the same conclusion was done" ) are present. Even if I would have favored the publication of this article, I would have definitely asked for a major review so that its overall readability is improved. The English copy-editing services of Copernicus could have been a solution.
The text was completely checked and rewritten.

**Specific comments:**

The title ("Variability of aerosols forecast over the Mediterranean area during July 2013 (ADRIMED/CHARMEX)" is rather a general statement than a clear reflection of the main aim of the paper. Moreover, when one compares it with the one of the companion paper ("Ozone and aerosol tropospheric concentrations variability analyzed using the ADRIMED measurements and

the WRF and CHIMERE models") it can be shown that the current manuscript should have been a part of that article and cannot stand as a second paper.
The abstract is written using awkward expressions and sentence structures (e.g. "in order to help scientists to decide...", "Each day, a simulation of four days is performed", "This variable is at the origin of ..."). This affects the reader's comprehension of the text.
The overall presentation is not well structured and clear:
The aim of an introductory section is to provide previous and supporting information for the specific topic and aim of the current publication. Thus, it should not elaborate on general information, e.g. regarding the pollution over the Mediterranean (the first introductory paragraph), or phrases irrelevant to the main topic, e.g. the composition of aerosols (the second paragraph), or a generic review in model evaluation (third paragraph). On the other hand, each of its paragraphs should aim to cover a different aspect of the points that are analyzed in the results' sections, previous studies, i.e. useful information about the same topic, and finally a number of goals of the current study.
Please see answer to the Editor.
There is no section "methodology" or "results". The observed parameters used for comparisons are not mentioned. A lot of text is spent to present the models, although widely known and/or presented in Menut et al. (2015). Statistic measures are not presented in advance. Overall, the methodology is the same as in the companion paper, thus even if the results were to support a separate article, a major revision should be made, so that text is replaced by tables, and/or shortened, and/or moved to the supplement, and/or completely removed.
A new section was added to present the statistical scores used.
Many results are not interpreted and/or analyzed in a sufficient way (e.g. temperature biases, differentiation of wind speed variability per station, variability in wind direction), thus -to my view- the way they are presented cannot support safe conclusions.
The discussion of the results was improved.
Inversely, the two main conclusions reached in the current manuscript are not satisfactorily supported by the results and are found incompetent to support this article.
Many parts of the paper should have either reduced or combined or eliminated. E.g. a) the first three paragraphs of the Introduction (pls. see above comments), b) theoretical reference to processes in the results sections, such as the paragraph in Sect. 4 and 4.1, 5.1 (first paragraph), 6. Most of such information, should be either placed in the methodology, or where attempting to explain the results, or else should be removed. c) First (and the half of next) paragraph of conclusions does not reflect conclusions of this work.
The conclusion was completely rewritten as well as the abstract. The results are more clearly described and quantified.
From my personal experience, the number of references is small. As far as giving credit to similar findings from other researchers, I have concerns: no results and/or findings of similar studies are cited in the results sections (4-7). Abbreviations for the statistical parameters used (e.g. RMSE) are not defined.
Abbreviations are now all defined. We added new references, but this is clear there is not a lot of publications on this specific topic. If we are wrong, please propose references.

## Answers to reviewer #2

We acknowledge this second reviewer for its complete reading of the study. Independently of some agressive and non constructive sentences, we made the revisions and we precisely answered to all remarks.

### Summary

The manuscript presents a verification study of the WRF/CHIMERE modelling system. The study is of interest for the scientific community
The word 'verification' is not appropriate. The goal of this paper is not to 'verify' or validate the model against observations, but to see if its forecast variability is lower or greater than its biases compared to observations.

**Recommendation**

Accept after major revision. I would like to stress out that I support the potential publication of this paper due to its scientific interest. On the other hand, many aspects of the manuscript need to be extensively improved; otherwise I will not be able to support final publication. As such, I strongly advice the authors to take into serious consideration all of the following major and minor remarks in order to improve the quality of the presentation of their work. To state it even more clearly, the quality of the manuscript must be strengthened a lot or else it would be extremely difficult to be finally accepted.
We made a lot of corrections in the manuscript and we hope the reviewer will appreciate them.

**Major remarks**

1. The use of English must greatly improve if the paper is to be published in ACP. I urge the authors to advice a native English speaker for doing so. Further, they need to advice similar papers in order to improve the overall presentation of their work.

   The English was improved even if there is no 'native English speaker' in our team, lab. So, we did our best. We also tried to see where information was provided in a "rather amateurish way". The reviewer giving no specific indications, we also did our best to be more 'professional'.

2. The manuscript's abstract needs to be thoroughly revised. Rather simplistic questions (e.g. P10342, L8-10) must be avoided and the scope of the study must be properly highlighted. In addition, a concise summary of the key findings should be present.

   The abstract was completely rewritten. But we don't think that the question is 'simplistic'. The fact that for most models, we don't know if a wrong forecast is mainly due to biases or natural spread is a real question. And studies pointing out this quantification for atmospheric composition are rare (contrarily to meteorology where studies are more numerous).

3. The "Introduction" section of the manuscript requires extensive revision. First of all, the authors need to expand the review of literature that is relevant to their study. Second, and probably most importantly, the aim of the study needs to be properly highlighted and justified. Instead of setting their aim in the frame of a simplistic question (reviewer's personal point of view), I would suggest that the authors attempt to present the key objectives of their study with regards to what is currently known (i.e. literature), thus highlighting the added value of the paper.

   We agree that the feeling of a 'simplistic question' is just the reviewer's personal point of view. In general, when a reviewer points out a lack in the bibliography, he is able to propose some references. In this review, there are only criticisms but no useful suggestion. We made a complete bibliograph and, to our knowledge, such kind of studies are rare and are all cited in this paper. Nevertheless, please note we added new references: they are not exactly in the scope of this paper but their addition is interesting.

   [Wyszogrodzki et al., 2013] and [Zhang et al., 2013] for the WRF forecast performances
   [Hollingsworth et al., 2008] for the GEMS forecast system,
   [Marécal et al., 2015] for the MACC-2 daily ensemble forecast system,
   [Wang et al., 2014]: for the gain of data assimilation in case of aerosol forecast in the Mediterranean basin,
   [Pérez et al., 2006]: for the interest to have coupled dust/radiation models in case of aerosol forecast

4. Section 2, "The observations", is hardly useful in its current form. Although some information about the measurement stations is provided in Table 1, further details must be given. What were the measured variables? What was the sampling interval? What are the specific measurements mentioned? This section could be also enhanced by moving the information about the specific field campaign, currently placed in "Introduction".

This section was improved and we proposed more details about the measurements. These details are also in the paper [Menut et al., 2015] and we detailed this point during the first phase of the review for this paper in ACPD. It seems that the reviewer did not waste time to read this previous paper. Thus, we have to take informations of the previous paper to put in this one. The only lack in this paper is the missing description of the E-OBS data and this is now corrected.

5. To my point of view, Section 3.1 is written in an amateurish way, far away from the quality standards that a manuscript needs to meet for being published in ACP. The description of the modelling system components' is almost chaotic, while the use of terminology and language is too simplified. For instance: "The first step is to calculate regional forecasted meteorology", "they are then used for several calculations:" etc. I urge the authors to re-write this section from scratch.

   We don't really understand why the description of the modelling system is done in 'a amateurish way'. A more technical description has no sense for ACPD. Perhaps in GMDD, but this is not the goal of this paper to describe FTP procedures, shell scripts and the complete model configurations.
   Nevertheless, the section 3 was reorganized to be more clear about the used tools. The bibliography was also updated but we don't know many papers about this specific topic of the 'atmospheric composition predictability'. During the review period of ACPD, we wrote: "Any help will be welcome during the ACP review phase". But in this review, the reviewer only writes that the text is not correct but suggests nothing to improve it.

6. Section 3.2: To my view, this is not a proper presentation of the configuration adopted for WRF. Again, information is provided in a rather chaotic way that is very hard to follow. The authors should consult similar "modelling papers" to view how the setup of a modelling system should be properly presented and justified.

   We tried to improve the presentation of the WRF forecast use. But we are not sure to really understand what is a "proper presentation", the reviewer only writing that our presentation is not correct, but not proposing new ideas or references. In fact, our presentation is close to already published papers and it was not a problem for their publication. The reviewer wrote about 'similar papers'. What papers? Articles about forecast? meteorological forecast? Atmospheric composition forecast? or about modelling systems? There is a lot of papers about forecast modelling chains but this is not the goal of this paper. The modelling chain presented in this paper was already used for other studies such as [Menut et al., 2009], [Menut and Bessagnet 2010] and these papers are already referenced in this article. A lot of people is using this system with CHIMERE and this is already referenced with [Honore et al., 2008] and [Rouil et al., 2008], among others.
   When a reviewer considers that well-known papers are missing and have to be cited, it is recommended to give more details about this papers. In absence of reference proposed by the reviewer, we changed the text to be more clear and according to the articles we know.

7. It is not very supportive for the manuscript to continuously refer to a previous publication, when presenting the modelling system. I agree that credit should be given to a previous work, nevertheless this should be done with caution and not continuously to avoid presenting information that could be useful for the interested reader.

   We understand this remark and we reduced at the maximum the citation to the other paper (which used the same models to analyze the same period and the same region).

8. Discussion of results (Section 4 and thereafter) significantly lacks quality. One striking example:

   - P10349, L22-23: The statement that it is difficult to find any explanation and no relative information is available, is too simplistic. A low mean bias could be computed from large biases in both directions (i.e. both negative and positive). A bad model behaviour always occurs for some reason, either related to initialization data or to physics representation. Hence, to just state that model performance is bad but there is no clear reason for this degrades the quality of the manuscript. Overall, Section 4 does not contain a single reference to any previous work, as part of a discussion for the results obtained in this study. Are the computed verification metrics within the ranges observed in similar past studies? What has been found in past studies, regarding model performance? These questions need to be properly addressed in order to enhance discussion of results. The same comment is valid for Section 5, which also does not include any reference to similar past studies.

If you know some interesting references, and since this is not our case, please give the complete reference. We agree that the explanation is light. This is also obvious that a low mean bias remains a "mean" and that model error compensations are probably acting. Logically, the more we are close to the event, the more the forecast has to be good. Here, this is not always the case and this clearly means that some large scale model errors counteracts against the logical benefit of data assimilation. But we tried to change the end of this paragraph to be more precise.

9. Conclusions are not presented properly. Mainly, this is because the presentation of results lacks any discussion in terms of the international literature. Simplistic and amateurish conclusions exist:

   - "the answer is certainly a bit of each": this is something to say during an oral presentation and something that it cannot be written in a manuscript considered for publication.

   This sentence was not the only conclusion: it was the beginning of a sentence explaining the two conclusions. But this sentence was removed, the paper having been completely rewritten.

**Minor remarks**

1. P10343, L1: "In this context, ...". This sentence does not really fit in this place, as it does not "connect" well with what is written before and after it. Please, consider removing it and placing it where appropriate.

   Ok, this was changed.

2. P10345, L8-10: I would prefer a better description of the modelling system, instead of just referring to a previous publication that utilized the same system. In fact, I do believe that this lowers the quality of the manuscript's presentation.

3. P10345, L11-19: Does this paragraph add anything to the manuscript, especially at this place? My opinion is that it does not. Consider revising accordingly.

   In the first submitted version, this paragraph was in the Introduction and this reviewer asked to change that. This paragraph explains this study is not our first work regarding meteorology and atmospheric pollution forecast. In this version, this paragraph is in the "forecast configuration" section. We think this paragraph is important and at the right place now. Of course, this is always possible to write some sentences at several places, but in this particular case, the reviewer has only expressed his "opinion" but has not given real argument.

4. NCEP/GFS appears as an abbreviation but no prior definition is given. Revise accordingly.

   Yes, this is an abbreviation. The text was corrected to define this acronym: National Centers for Environmental Prediction (NCEP), Global Forecast System (GFS). Please note this is well-known for people working in the field of meteorological modelling. It is even directly used in titles of articles as, for example: *Sun, R., Moorthi, S., Xiao, H., and Mechoso, C. R.: Simulation of low clouds in the Southeast Pacific by the NCEP GFS: sensitivity to vertical mixing, Atmos. Chem. Phys., 10, 12261-12272, doi:10.5194/acp-10-12261-2010, 2010.*. Moreover, we added this reference, a good description of the GFS system being included.

5. E-OBS data are used for verification. Have they been previously defined? What are these data? Where do they come from? How many stations (?) have been used for the verification?

The E-OBS data are very known in Europe for the model/observations comparisons. But we agree that these data are not sufficiently described in this paper and a new paragraph was added.

6. P10350, L19-20: When computing percentage differences there is no reason to keep in mind the units of measurement. Revise accordingly.

   In fact, this is important. When temperature differences are calculated, this is crucial to remind the unit. Because 1% of difference has not the same meaning in Celsius degrees and in Kelvin. The two units being commonly used in publications, this is more precise to remind what unit we are using.

7. A separate section for the different results obtained? Consider revising by using a general "Results and discussion" section, and sub-sections for the various parameters examined.

   The sections for the results are already organized to be easy to read and understand. To make an unique section "Results and discussion", with sub-sections, would change nothing, apart add some confusion in reading the manuscript. This remark sounds like an editing problem only. In this version, the results are ordered in order to properly sort the results, from the meteorology to the concentrations of aerosols.
   4. Meteorological parameters
   5. Emissions
   6. Aerosol
   Finally, the 'conclusions' part summarized all these results in the section 7.

# References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[Marécal et al., 2015] Marécal, V., Peuch, V.-H., Andersson, C., Andersson, S., Arteta, J., Beekmann, M., Benedictow, A., Bergström, R., Bessagnet, B., Cansado, A., Chéroux, F., Colette, A., Coman, A., Curier, R. L., Denier van der Gon, H. A. C., Drouin, A., Elbern, H., Emili, E., Engelen, R. J., Eskes, H. J., Foret, G., Friese, E., Gauss, M., Giannaros, C., Guth, J., Joly, M., Jaumouillé, E., Josse, B., Kadygrov, N., Kaiser, J. W., Krajsek, K., Kuenen, J., Kumar, U., Liora, N., Lopez, E., Malherbe, L., Martinez, I., Melas, D., Meleux, F., Menut, L., Moinat, P., Morales, T., Parmentier, J., Piacentini, A., Plu, M., Poupkou, A., Queguiner, S., Robertson, L., Rouïl, L., Schaap, M., Segers, A., Sofiev, M., Thomas, M., Timmermans, R., Valdebenito, A., van Velthoven, P., van Versendaal, R., Vira, J., and Ung, A. (2015). A regional air quality forecasting system over Europe: the MACC-II daily ensemble production. *Geoscientific Model Development Discussions*, 8(3):2739–2806.

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