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Atmospheric Chemistry and Physics Discussions

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13, C3925–C3943, 2013

Interactive Comment

Interactive comment on "Simulations of the transport and deposition of ^{137}Cs over Europe after the Chernobyl NPP accident: influence of varying emission-altitude and model horizontal and vertical resolution" *by* N. Evangeliou et al.

N. Evangeliou et al.

nikolaos.evangeliou@lsce.ipsl.fr

Received and published: 19 June 2013

Anonymous Referee #1 Received and published: 30 April 2013 The authors modeled the emission and global transport and deposition of radioactive Cs from the Chernobyl nuclear power plant accident using the LMDzORINCA model at different resolutions. Concentrations and deposition quantities were compared to measurements and other studies in the literature. This type of study is certainly of value in the context of the simulation of the atmospheric transport of radionuclides.

The reviewer recommends publishing this paper with major revisions in response to the following questions and comments.

General Comments: Sec. 4: Although it's important to provide the technical specifications, too much detail, all available in other publications, not unique to this study and unnecessary for the scope of this paper is provided on the parameterisation of deposition processes in the model. This section serves as a digression and should be shortened and simplified.

Response: We agree with the reviewer. Therefore, we have transferred the chapter to the Supplementary Material-Methodology.

The treatment of statistics in Sec. 5 needs to be improved. For this point, also see individual comments below.

Individual Comments: Please find below individual comments prefixed by page and line number.

p7687 l1-3: It's not clear in the text that the model was nudged for this study. Please add additional details: What reanalysis data was used (eg. ERA-40), time constant?

Response: Corrected (P5-L10 MS with track changes)

p7687 l8: Please clarify in the text how Cs137 is treated in the model – "mostly" here is ambiguous. Also, if no gas phase chemistry is included in your simulation, sentence on line 6 p.7686 should be removed as it's unnecessary and may confuse the reader.

Response: We state in Page 6 – lines 12-13 that Cs137 is treated as an aerosol tracer. We remove the sentence on gas face chemistry (see manuscript in track changes mode)

p7687 l26-27: No need to quote each day and percentage. Just refer to Table 1 to improve legibility. Table 1: You refer to Devell et al, 2002 in the caption; yet only Devell et al. 1996 appears in the reference list. Further, the 1996 publication does not include

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the day-by-day or vertical profile for the emissions. Please provide correct references.

Response: The reference in the caption of Table 1 has been changed to Brandt et al. (2002). Percentage values in the “Emission estimates after the accident” chapter have been removed (p.7-L.32 –track change ms).

p7688 l21: Particle size distribution functional form should be added to the text for completeness. Fig. 6: What are the RËĘ2 values? No description is given in the caption or the text.

Response: Corrected (P2-L8 in Supplementary information now). Rˆ2 is now explained in the caption (p.31-L29)

p7693 l13:"altitude of the source" -> number of emission vertical levels; spread of emissions was greater -> emission distribution had more points; layers were denser covering lower distances-> layers were separated by shorter distances

Response: Corrected according to the reviewer’s suggestions (p.9-L. 6 and 7).

p7695 l8,9: From Fig. 7 the isosurface does not "dominate the higher layers across all Europe". For example, nothing appears aboveWestern Europe. Also, the 19-layers run rises to a higher altitude (lower pressure) than the 39-layers in the figure, in contrast to what is claimed in the text l.14,15. If this is not the case, both panels should be plotted against the same scale in the vertical for the panels to be comparable. A vantage point more similar to Brandt et al. is also needed to help facilitate the comparison. Overall, I don’t think that as it is Fig. 7 contributes much to the paper and it should be improved or removed.

Response: Obviously the 2 figures of different vertical levels had been placed in an opposite way. They are now correct and what is mentioned in the manuscript conforms to fig. 7. We appreciate the reviewer for his comment.

p7696 l8: What does "averaging" refer to? Should be clarified in the text.

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Response: Sentence has been removed, as it does not enforce the statements of the paragraph (p11-L.16, track change ms).

p7696 l14: "determination recoveries contrast between methodologies" is not clear.

Response: In order the sentence to be clearer, we link it with the previous sentences, which explain why the different methodologies used for the construction of the Atlas map might be responsible for some discrepancies between model and measurements (p.11-L.21 track changes ms).

Figs 8-10 captions: Website should be moved to references. Location (North Europe, etc.) should be moved to the first line. You mention "north, west, south-eastern", yet present "central-western, north, south-eastern". Sentence beginning "They were examined according..." appears in the text and it's superfluous to be included in each caption.

Response: These figures have been moved to the Supplementary Material of this manuscript in order to limit the manuscript in a way to be more easy-to-read for the readers.

p7697 l.5: "Educated guesses" needs to be elaborated upon. How were they calculated?

Response: The first estimation of the source term was based on a USSR report to the International Atomic Energy Agency (IAEA) in 1986 (Persson et al., 1986; Hass et al., 1990), where the source was estimated on the basis of summation of the material deposited within the countries of the former USSR. These investigations did not take into account the material deposited outside the former USSR and has since been corrected several times from other investigations with more than a factor of two. The emissions used in the simulations (see Table 1) were taken from Brandt et al. (2002) and they are based on Waight et al., 1995. The amount of release and temporal variation used in this study is similar to the estimates in De Cort et al. (1998). Here, we state that

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they are “educated guesses” because (a) an uncertainty of at least ±50% is used, and (b) the altitude of the emissions (which is very important and can change the transport regime extremely, as shown in the RG19L(S) simulation) is based on simple assessments. The last is mainly because tools such as back trajectories were not available in 27 years ago, and also, there was lack of information, while the national monitoring systems of the countries were not that developed like nowadays.

p7697 l.22: What do you mean by "the correlation coefficient at 95% confidence level"? A p-value needs to be computed for the test to decide significance at 95%.

Response: I agree with this statement. Here we mean that p was always lower than 0.05! It is now mentioned in the captions of Tables 3 and 4 and in p16-L11, 13, in p.12-L23, in p.13-L.4.

p7698 l.5: See previous comment on statistical significance.

Response: Clarified in the text. We appreciate the reviewer for this comment..

p7967 l.25: 0.81 does not appear anywhere in Table 3. What do you mean by "real emission altitude", when you also refer to "the emission altitude was taken into account" in l.22? The text needs to be clarified.

Response: The right value is 0.84 and is now included in the text. In the text we follow the pattern of (a) assuming surface emissions, and (b) real emission altitude, which means all the other simulations with the tracer emitted in a certain altitude (according to Brandt et al., 2002). Besides, we define in p.7-L.17 that “real emission altitude” means the simulations where emissions according to Table 1 have been taken into account.

p7699 l.1, Fig.12: It’s my understanding that the Pearson’s linear relation coefficient indicated the strength of the linear relationship but says nothing about the slope. Good agreement can be claimed if the lines fall close to the 1:1 slope. What is the case here?

Response: The Pearson test is the simplest statistical test and it is frequently used

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when similar quantities are compared. It is a measure of the linear correlation (dependence) between two variables X and Y, giving a value between +1 and −1 inclusive. It is widely used in the sciences as a measure of the strength of linear dependence between two variables. In our case where modeled and measured quantities are compared, the unique +1 dependence would follow the function y=x, whereas for -1, would give a reversely proportion dependence (completely wrong here!!!). The slope would give a glance of what is overestimated or underestimated against what, which it can be seen very easily in the relevant figure.

Fig. 20 caption: "Linear fitting" here refers to the 1:1 line? Should be made clear.

Response: Yes, it refers to 1:1 dependence and it has been clarified now in the text (p16-L.32).

Technical Corrections:

p7683 l9: be -> by

Response: Corrected (p.3-L.6)

p7686 l15: plane -> dimension

Response: Corrected (p.5-L.23).

Sec 5.1 title: versions -> resolutions

Response: Here, we have decided to maintain the term. For example, it refers to the zoom version of the model, which uses the same number of points in longitude and latitude as in the regular grid, but it stretches the grid over specific regions (as seen in Fig. 1)

Figs. 2-5: The captions read that every 10 days in May are shown, yet only one plot appears for May in each.

Response: Corrected (Fig.2-5)

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p7697 l.23: confident -> confidence

Response: Corrected (Tables 3 and 4 and in p16-L11, 13, in p.12-L23, in p.13-L.4)

p7699 l.23: Remove "consequently" p7699 l.25: Remove "briefly"

Response: Corrected (p.14âĂŤL.16 L.18)

p7700 l.1: appeared to be a local event -> was limited locally.

Response: Corrected (p.14-L.21)

p7700 l.25: is not able to estimate -> underestimates

Response: Corrected (p.15-L.11)

p7701 l.10: Remove "where"

Response: Corrected (p.15-L.25).

p7703 l.1: deficiencies -> discrepancies

Response: Corrected (p.17 – L.8).

The emission inventory used in the study (Devell et al.?) should be included in the abstract.

Response: Corrected (p.2-L.13)

Abstract l11: "The best choice for the model validation was the"-> "The model is validated for the"

Response: Corrected (p.2-L.12)

Abstract l12: Second sentence: no need for "However,"

Response: Corrected (p.2-L.20)

Abstract l24: "Atlas" here is ambiguous. Please add better description or reference

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Response: Corrected (p.2-L.22)

Anonymous Referee #2 Received and published: 29 May 2013 The authors present the simulations of the transport, wet and dry deposition of the Cs-137 released during the Chernobyl accident. The simulations were carried out with the coupled model LMDzORINCA at the European scale. Several configurations of the model were studied. Results were compared to the REM database and to other studies already published. The paper is interesting and addresses important questions to model the atmospheric dispersion of an accidental release. The reviewer recommends its publication after improvement.

GENERAL COMMENTS - To compute the wet deposition, it is essential to have realistic precipitation fields. The quality of the LMDz precipitations and more generally, the differences between the LMDz fields compared to the ERA-40 fields should be discussed. Does the vertical resolution of the simulations impact the precipitation fields (especially the convective precipitation fields) / the scavenging height?

Response: Below, we attach precipitation fields from observations and the model (Fig. 1&2). In the first one, the average precipitation in mm/d is shown for ERA40 (http://data-portal.ecmwf.int/data/d/era40_daily/) precipitation fields (2.5x2.5 degrees) for the year 1986. In the second picture, the average precipitation (in mm/d) from our model (for 1986) is shown for a horizontal resolution of 0.66x0.51 degrees. As you can see the difference in Europe (inside the zoom area) is very small! We have included relevant comments in the manuscript (Chapter 4.4). The average relative discrepancy (percentage) between model and observations ([mod-obs]/obs) is 8% in a box of 700x700 km and reaches 10% in 3000x3000 km centered in the plant. The difference was estimated after re-gridding ERA40 (2.5x2.5 degrees) to LMDZ resolution (0.66x0.51 degrees). The vertical distribution of scavenging is determined by the height of which precipitation is defined. In LMDZORINCA we used Emanuel's scheme (see supplementary materials) for convection, which has been defined in the 1-D column of the model, by comparing cloud properties and precipitation from experiments in mid-latitudes and tropics.

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- The uncertainties on the precipitation fields should be one of the reasons discussed in chapters 5.3 and 5.4 to explain the discrepancies between the observed and modeled deposition.

Response: Below, we compare precipitation fields from the model and observations (2 attached pictures, Fig. 3&4). In the first one, the average difference of precipitation in mm/d is shown between LMDZ model and ERA40 (http://data-portal.ecmwf.int/data/d/era40_daily/) precipitation fields (regridded from 2.5x2.5 degrees to the grid of LMDZ) for the year 1986. In the second picture, we calculate the same difference between our model (for 1986) using the annual average precipitation fields from the Global Precipitation Climatology Project (GPCC, http://www.esrl.noaa.gov/psd/data/gridded/data.gpcc.html) (in mm/day) for the period 1981-2010 (regridded from 0.5x0.5 degrees to the grid of LMDZ). As we explained before the relative difference in our region of interest is small and this is also show below in mm/d.

- The authors have to precise which parameterization they use for the horizontal and vertical diffusion processes. Do the vertical and horizontal resolutions impact the Cs-137 dilution? Does the choice of the parameterization for the diffusion may explain the differences between the simulations done with the different resolution?

Response: Paragraphs for the parameterization of diffusion and convection have been added (P4-L12 at the Supplementary Material of this article).

- Chapter 4: Why the wet deposition is parameterized assuming the Cs-137 behaves as a soluble gas and not as a particle? The particle size should influence the wet deposition.

Response: Our sentence was inaccurate and has been corrected. We thank the reviewer for pointing it out. We wanted to indicate that in-cloud scavenging of Cs was treated as for a soluble gas. In addition we account in the code for below-cloud scavenging and sedimentation of 137Cs. Hence 137Cs is treated as a sub-micronic

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aerosol.

- Chapter 5: The text should be improved: o Information is repeated.

Response: The manuscript has been significantly improved in order to be more precise and easy-to-read for the reader. Many parts and figures have now been moved to the Supplementary Materials and the most important parts have been maintained.

o The impact of the release height is too highlighted compared to the impact of the horizontal and vertical resolution. It is true that the influence of the release height is very important but the test of only 2 release heights so different is a bit extreme.

Response: We agree with this comment. The idea was not to simply to compare 2 extreme heights of emission but 2 real facts: Wrong information (which forces the use of surface emissions) versus educated guesses (that almost all the scientists that simulated the accident before have used). However, the most important part of this analysis is the consequences if decision-making has to be done after a major event like Chernobyl, which may save thousands of lives. This is the example we give for the city of Kiev.

o You should give the fac2, fac5... scores to be able to better compare your results with those of Brandt et al., Quelo et al., 2007. . .

Response: The actual goal here is not to compare our results with those of Brandt et al. but with the REM database. We use 3 statistical tests (Pearson, Spearmann, Kendal's Tau) and we also calculate biases in order to find the discrepancy from the measurement. These biases are discusses based on what Brandt et al. found instead. Insisting in a comparison with Brandt's paper would mean that we try to compete on the better response of the model, which is not the case in this paper. We have clarified the lines.

o The statistical analysis should be improved.

Response: We give 4 different statistical metrics (Pearson, Kendal Tau, Spearman

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correlation coefficient and the relevant calculated biases). We believe they are enough to prove that the model produces reliable results. However, if the reviewer has to suggest anything more specific, we would be willing to make further changes

INDIVIDUAL COMMENTS - The organization of the introduction should be improved in order to highlight the objectives of the study

Response: In Chapter 1 (introduction) we indicate the major findings reported after the accident by several scientists worldwide in terms of emissions and consequences. The last paragraph of the chapter has been used to highlight the objectives of the study in order to be clear to the readers. We do not know what the reviewer suggests here.

- P7683 the sentence L9 “the absence of reliable. . .” should be clarified.

Response: Here, the expression “lack of reliable information” has been used to comment what really happened in Europe after the accident. Other countries were commenting the detected radionuclide concentrations (although most of them did not have organized monitoring stations) and other countries were claiming that they did not detect the radioactive cloud at all (although it proved they did years after), in order not to panic the population. We now put a comment on that in the manuscript.

- P7685: “the already known patterns of the releases” is too strong as explain later the releases are highly uncertain.

Response: We changed the expression to “reported patterns of the releases”

- P7685 the last sentence is not useful.

Response: We understand the sense of the reviewer’s comment, and we have removed the last sentence.

- P7684 last paragraph: many studies have been performed in order to validate long range dispersion models with the Chernobyl accident: the Brand’s PhD work, the Quelo et al. 2007 (Atm. Env.) paper.

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Response: The reference of Quelo et al. (2007) has been also included in the reference list.

- Chapter 2: you need to indicate the met data you use to nudge LMDz and the temporal resolution.

Response: Corrected (Page 6-L.10 in the MS with track changes)

- P7687 L10: the references should be ordered according to the year of the paper.

Response: Corrected

-Chapter 4: which aerosol distribution do you use?

Response: Corrected (P2-L8 in Supplementary information now).

- P7691 first paragraph: you should compare your deposition velocities to the deposition velocities given in the Sportisse paper which are usually used to model the deposition of radioactive materials.

Response: This part has been now put to Supplementary Material of this paper. We have included the values of the deposition velocities from the paper according to the reviewer's comment. We appreciate for his help.

- Chapter 5: you use fallout to refer to the plume. Fallout is ambiguous since it is often used for the deposition. You should use "the plume" instead.

Response: We do not agree with this comment. I provide the official explanation as presented in several online dictionaries: "The slow descent of minute particles of debris in the atmosphere following an explosion, especially the descent of radioactive debris after a nuclear explosion". Since we talk about Cs-137, which is a particle, we believe it fits very well. However, if it still causes a problem, we would be willing to change the word in a next step of the reviewing process.

- Chapter 5.1: you should better highlight the similarity and the discrepancies between

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the different simulations.

Response: Some changes on this direction have been done on the manuscript. The description of the runs performed is being presented now in Chapter 4, where the main similarities observed are presented. If something more specific is to be changed, we would will to correct it.

- P7691 the sentence L9-10 is too reductive and unnecessary.

Response: Corrected according to the reviewer's comment.

- P7693 L25: "the cyclone observed . . .": was the cyclone really observed? Why it is not discussed before with the other simulations. Was it simulated?

Response: The cyclone was observed by the step-by-step transport of the Cs-137 plume and can be seen in the 2d-movie in the Supplementary Materials of this article. It is not discussed further because, in our opinion, it would not give any extra benefit to our discussion.

- P7695 first paragraph: the description of the 3-D illustration is not clear enough. I do not see the benefit. You should improve the analysis of the figure and try to give some possible explanations for the differences.

Response: We have changed this figure after determining some crucial mistakes. Also some parts in the manuscript that analyse it. We appreciate the reviewer for his comment.

- Chapter 5.2: You should improve the statistical analysis and your conclusions.

Response: This is too obscured. We have validated the comparison using 2 different statistical tests and also calculated biases in the relevant figures. We could do further changes if something more specific is asked.

- P7698 Add X, Y meaning.

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Response: X and Y here represent the variables being compared (model versus modeling). We have added a comment that.

- Chapter 5.3 P7700 L9-12 “It is unexpected. . .” You should remove this sentence or explain more precisely why you say that. What is usually done for crisis management?

Response: We do not understand the problem here. We provide an example of how the lack of available information after that major accident could have caused problems after using such modeling tools to predict the areas of the highest contamination. The official authorities were claiming that there was not any explosion in the beginning. Using this assumption we emit Cs-137 from the surface. And here comes the example of the Kiev city. According to our hypothetical simulation and taking into account the information of the first days following the accident, we estimate that there would be extreme contamination of the city that would lead to evacuation activities. Anyway, the paragraph has been changed in order to be more comprehensive.

- P7703 First paragraph: Do the other models have the same bias with the measurements? I have the feeling that the parameterization of the deposition and especially errors in the precipitation fields may explain those biases?

Response: To our knowledge model comparisons with REM database present similar to our biases. We provide evidence in some previous comment-responses why there is no problem with the parameterization of deposition or the precipitation fields used.

- Conclusions P7705: “e.g. using inverse modeling” you should add some references.

Response: Reference has been added. We thank the reviewer.

- P7705 “knowing the exact core. . .” I do not see why you add this point. It would be better to discuss what could be done to improve the scores of the model-to-data comparisons.

Response: We try to find the source of these discrepancies. Having seen what the other models find for the Chernobyl accident, we believe that our results can be con-

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sidered as very convenient. We believe that the 2 main sources of these discrepancies are (a) the source inventory (that it is known with a 50% uncertainty) and (b) the exact altitude that the emission took place (in this point, there are only guesses and rough estimations, although previous modelers have used the same estimations). This is exactly what we discuss in the manuscript.

- Table1: you should give the layer thickness.

Response: Corrected (Table 1) according to the reviewer's suggestion.

- Fig.7: You should plot the Brandt et al. Figure to help for the comparison. You should add a vertical scale.

Response: Obviously, Brandt et al. have used different software to create the 3d representation of the iso-surfaces. In both cases the altitude scale is not given. Therefore, we do not understand how it would help to "copy a figure on another". If the reviewer has something more specific to suggest, we would be glad to follow it in a next step.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C3925/2013/acpd-13-C3925-2013-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7681, 2013.

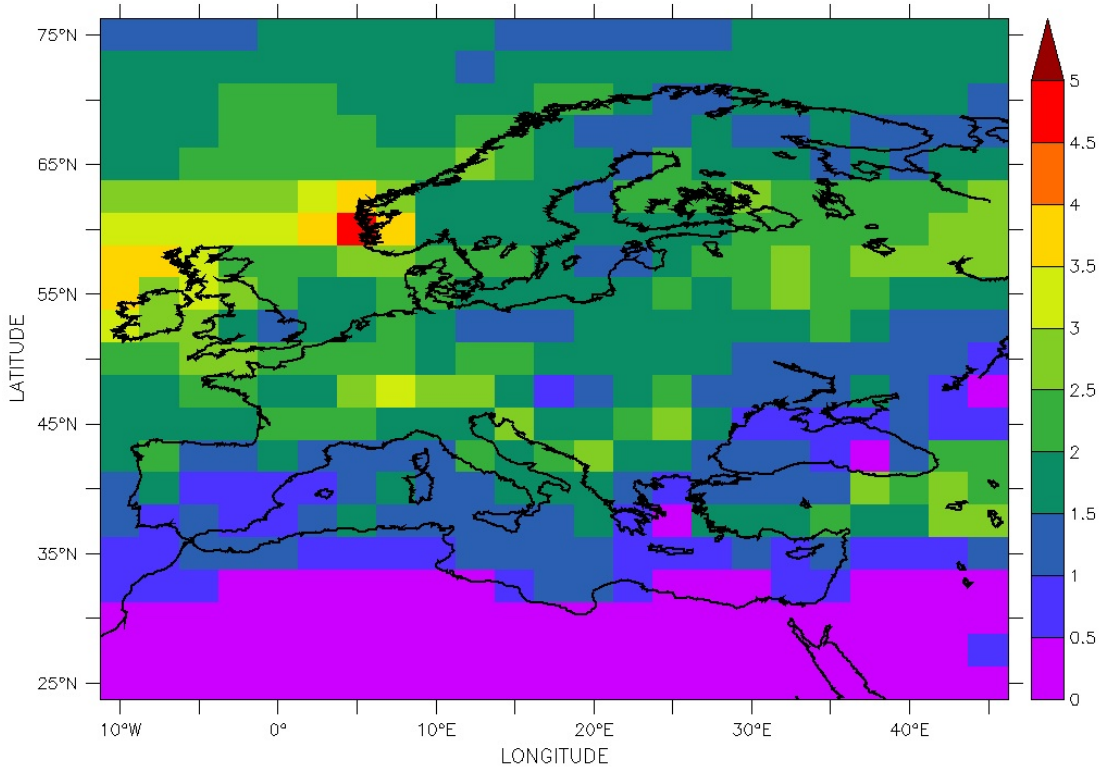

Fig. 1. ERA40 (2.5x2.5 degrees) average precipitation (in mm/d) over Europe in 1986.

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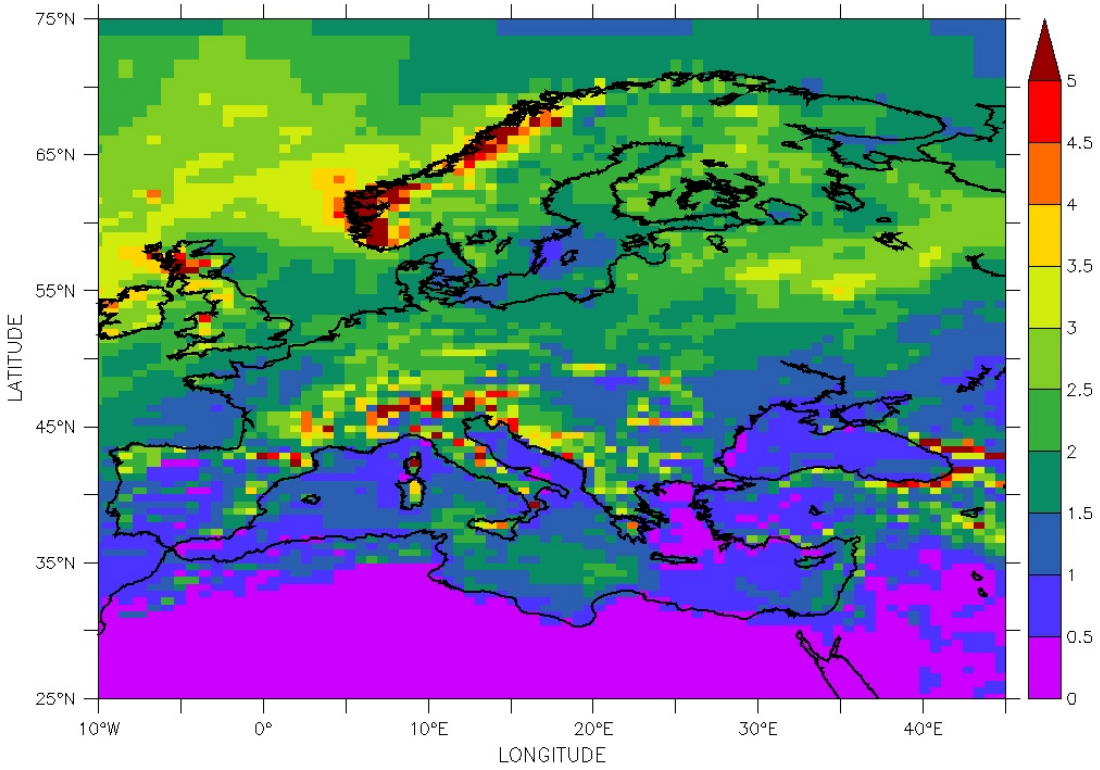

Fig. 2. LMDZ (0.66x0.51 degrees) average precipitation (in mm/d) over Europe in 1986.

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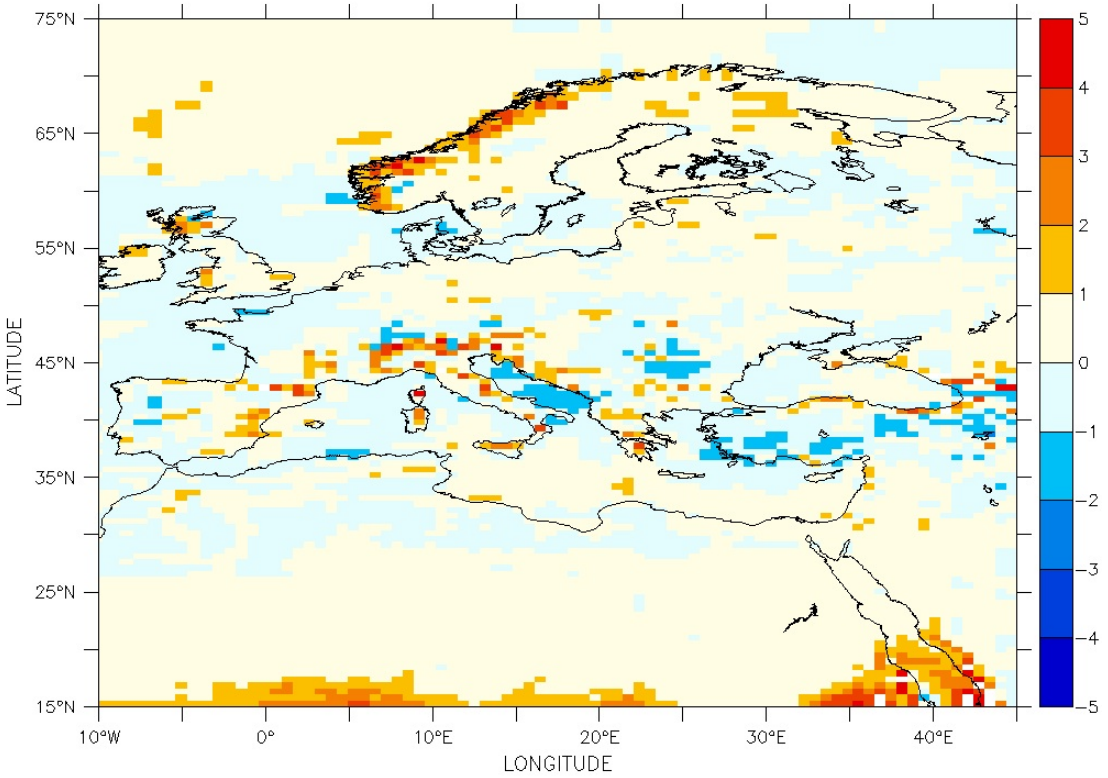

Fig. 3. Average difference of precipitation in mm/d is shown between LMDZ model and ERA40 for 1986.

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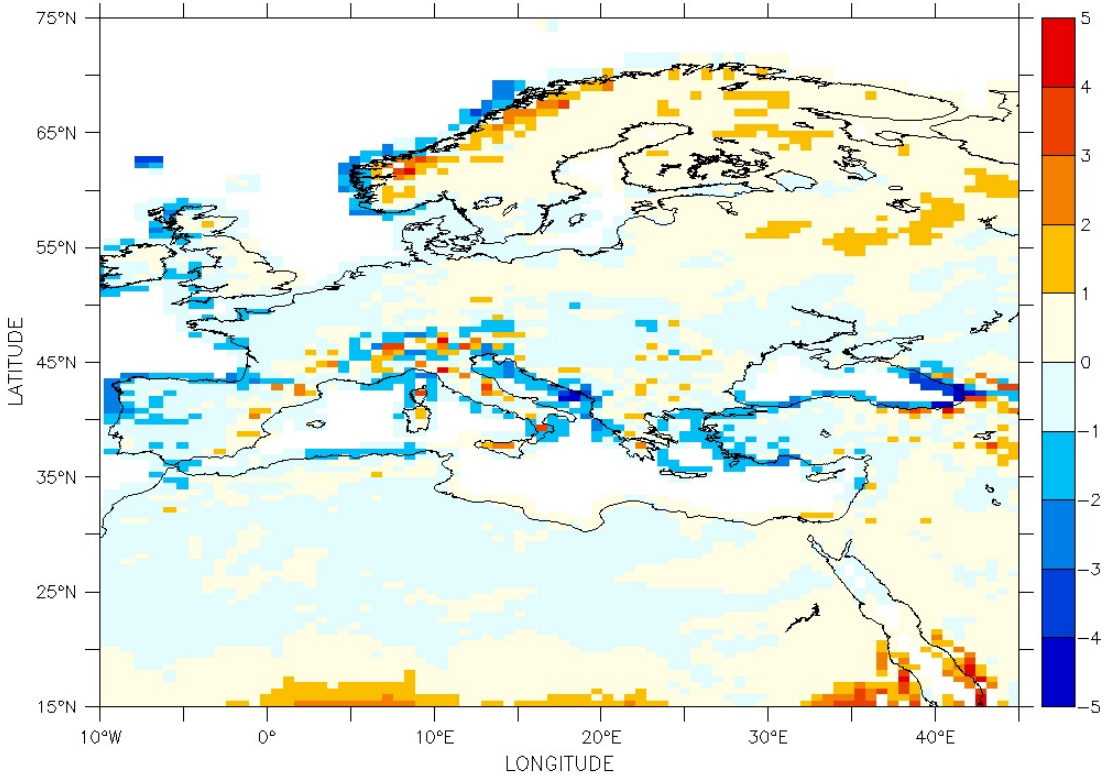

Fig. 4. Average difference of precipitation in mm/d is shown between LMDZ model and GPCC.

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