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Geoscientific Model Development Discussions

Interactive comment on "An observation-constrained multi-physics RCM ensemble for simulating European mega-heatwaves" *by* A. I. Stegehuis et al.

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Answers to Reviewer #1Âă:

This article presents and discusses the performance of a large multi-physics ensemble configured with the WRF regional climate model system in representing two major heat wave episodes observed in recent years across the European continent. The paper attempts to objectively assess which configurations perform best in reproducing the heat wave characteristics with the purpose to identify a few best performing configurations which might be recommendable for application in studies on the role of heat wave in Europe under climate change conditions in summer.

The paper is reasonably well written, although I came across quite a few sentences which I found difficult to understand. In addition the manuscript also contains a number of assumptions or claims that go without solid argumentation (or argumentation at all) and are not backed up by references. Some of the figures are difficult to view, in particular the Figure 1 and the figures displaying scatter plots. But with some work that can easily be improved.

- We thank the reviewer for the useful comments and suggestions. As concerns figures, they are now adapted to the suggestions of both reviewers.

A major concern, however, I have with the followed methodology is that the authors have chosen to leave out the land surface scheme from their considerations and to restrict their construction of a multi-physics ensemble to what they refer to as the atmospheric physical schemes. There are probably quite a few large-scale weather phenomena that are rather insensitive to the details of a land surface scheme, but for sure such approach does not hold for heat wave conditions across Europe. There is an overwhelmingly large amount of literature that points to the role of land surface processes and their effect on land-atmosphere exchanges in the weeks or months ahead of the onset of a heat wave episode across (a subregion in) Europe, so I really can't understand why the authors have chosen to pursue this approach. The more so as the authors themselves write in their concluding remarks." ... a limitation of this study is the use of only one land-surface scheme; the five selected WRF configurations may actually all compensate for systematic errors of the NOAH land surface scheme ..." etcetera.

- This paper is designed as a methodological paper. Our intention was to test as many physic schemes as possible, including land-surface schemes. However, at the moment we performed the study 4 different land-surface schemes were available: NOAH, RUC, Pleim-Xiu and the 5-layer thermal diffusion scheme. The last one, non-physical, is designed for test cases and cannot be used in realistic situations. The Pleim-Xiu scheme comes with a dedicated set of other physical schemes and does not allow in most cases

to combine different possibilities. We performed an ensemble of simulations using the RUC scheme, but we found that it provided extremely sensitive fluxes, with large latent heat fluxes at the beginning of season and extreme subsequent drying in summer months. Sensible heat fluxes also appear overestimated. Comparisons with several FLUXNET sites are now explicitly shown in Figure 1. So even if temperatures of heat waves would match observations in a few combinations of physics schemes, we would almost be sure that this would be for wrong reasons. We also experienced technical problems while running several of the RUC simulations. This made us decide to only use one land-surface scheme and focus on the atmospheric physics processes. We believe that this can be very useful to the many users of the WRF model to examine this sensitivity and to be aware of best-performing physics combinations using NOAH land surface as it is very widely used. We added some sentences to the methodological section, and added a figure with the comparison RUC vs NOAH (new Figure 1).

In contrast to the use of a single land surface scheme there is a multitude of atmospheric physics schemes examined in this study that can be exchanged for one another, in particular there are six different boundary-layer/surface-layer schemes, but it is not at all made clear to the reader in what aspect they differ or how different they are. To a lesser extent similar considerations apply to the other physics schemes as well. Too many, unclear what their differences are, and in a way accidentally selected because these schemes happen to be implemented in the WRF system, which doesn't help in making this a "clean" ensemble, meaning that there is no way in which the various members of the ensemble can be neatly discerned from each other in some model physics phase space. In that respect, this ensemble is not so very different from the multi-physics ensemble approaches mentioned in the paper.

- We would have liked to test all possible physics combinations, but this was not possible due to the large number of combinations. The selection was however based on a strategy. First we performed preliminary tests to see the behavior of the physics and to exclude least-advanced ones (such as the land-surface scheme without soil mois-

ture). Then we looked into the different schemes to see the physics behind, in order to choose those that were most different from one another. For example in the cumulus options we used only one of the two Arakawa-Schubert Schemes, the 'new' one and left the 'old' one out of our study, and in the planet boundary schemes we tried to use different physics (K2 non-local; TKE; MF). We might not have explained in detail on what we based our decision. We added a part to the methodological section 'physics schemes' in order to explain better. The methodology developed here is innovative. As compared to previous studies, our ensemble is very large, a lot of combinations were tested, and furthermore one originality is that our ensemble is dedicated to simulate heat waves, which has (in our knowledge) not been done before.

To conclude, in my opinion ignoring variations in the type of land surface scheme in building up a multi-physics ensemble makes the approach that is followed quite out of balance, in particular when such ensemble is meant to draw conclusions for the model ability to represent heat wave episodes. I would argue that the authors should first carry out the future study they announce in their "Concluding remarks" in which they intend to investigate the performance of a joint permutation of different atmospheric physics schemes and land surface schemes. On the basis of the results from that study they could then have solidly zoomed in on what they want to present in this study.

- We hope to have answered most concerns, and justified our choice to focus on atmospheric processes only.

MAJOR COMMENTS:

1.) The title of the paper should already contain a reference to WRF to directly inform the reader what the paper is about. WRF is not synonymous nor equivalent to RCM, thus conclusions drawn for WRF can probably not immediately be generalized to an arbitrary RCM.

- We propose to change the title into : 'An observation-constrained multi-physics WRF ensemble for simulating European mega-heatwaves.

2.) Following my general comment on the omission of having varied the land surface scheme which makes the role of the NOAH LSM that is being used even more relevant I want to bring up some issues on the initialization of soil moisture. The authors write that soil moisture is directly taken from ERA-Interim. The authors also state that varying the initial date (1st of May against before 1st of May) results in variations in temperature outcomes of less than 0.5 C, at least for the August 2003 case. I would argue that this result does not at all imply that soil moisture is adequately initialized, it only indicates that there is little sensitivity regarding the date of initialization. My concerns are the following. âĂć ERA-Interim employs TESSEL as the land surface scheme which is rather different from the NOAH LSM. How was the actual mapping of TESSEL soil moisture onto NOAH soil moisture be carried out. Through interpolation of soil moisture (in relative volumetric units) or through interpolation of a soil moisture index taking account of the soil moisture field capacity and wilting point parameters in the respective schemes? The 2nd approach is obviously preferable. Please mention in the text what approach is followed in this respect. âĂć But even if they did, there are issues of soil water buffering capacity. Soil depths in both schemes may be difficult so, while properly mapping soil moisture content from TESSEL to NOAH, the resulting soil moisture columns might still be quite different. The authors should mention this point.

- Initial soil moisture is obtained from interpolation of ERA-Interim data on the soil layers, accounting for the field capacity. We verified that soil moisture did not differ much in the upper layers from TESSEL, but we agree with the reviewer that initialization is an issue. However, as heat waves occur 2 months after initialization, memory from this initial condition is mostly lost. We performed initial condition experiments to select the starting time of the runs, and found that 1 May does not provide very different results from 1 April, to verify absence of spin-up sensitivity.

âĂć Also, soil moisture must still be predominantly regarded as a model (or module) specific quantity. It is poorly constrained by observations and in the context of data-assimilation often treated as a free parameter that can be used (or abused) to repro-

duce observed near-surface parameters like temperature and relative humidity. We agree with the reviewer. This is one of the reasons which also led us to focus on atmospheric processes, having this in mind. So, on the basis of these grounds I would argue that there is considerable amount of uncertainty that can be attributed to the initial soil moisture profile used as the starting point for the set of WRF-simulations. I think the authors could benefit from this situation. Instead of taking a second and, even, third, land surface scheme, (still the most recommendable option, though) or altering the formulation of the land surface scheme, they might bring in the potential role of soil moisture on the evolution of the seasonal slice simulations by perturbing the initial soil moisture profile. For example, an option is to use plausible dry and wet perturbations of soil moisture initial profiles to examine the sustainability of the 5 or 55 best performing configurations that came out of their current exercise. (I don't think it is necessary to redo all 216 configurations that have been done so far).

- We did a new experiment as suggested by the reviewer. We made simulations with our 5 best performing configurations initializing the soil moisture differently. Instead of starting with the normal/original amount of soil moisture, we took out 20% of soil moisture (in all layers) in the first experiment, and added 20% of moisture in the second one. We found that drying the soil all along the column led to a general increase of all temperatures in the heat wave period, and a wetting led to a general decrease of all temperatures (regardless of physics schemes combinations). This confirms the sensitivity of temperatures to the land surface scheme. However, all perturbed runs still perform relatively well, suggesting that, despite the temperature shift, variability remains well simulated. We added a description of this sensitivity test in the discussion section, and added an extra figure to the paper (Figure 8).

3.) The Russia-region is quite close to the eastern boundary of the EURO-CORDEX domain. This is potentially problematic in simulations of anomalous circulations like those that give rise to heat wave episodes, where there might be a conflict between the circulation sustained by the regional model and the forcing imposed at the near-by

eastern boundary. Please, make clear in the text how you paid attention to this aspect. Did you choose a model domain that is actually much larger than the EURO- CORDEX domain? Did you broaden the boundary relaxation zone? Anything else?

- We did not take this point into account. However for the calculation of the variables over Russia we excluded the pixels too close to the border of the domain.

MINOR COMMENTS: (in indicating the line number I refer to the line number value in the pdf-document of the discussion paper as outlined by GMD).

1.) It seems to me that the proper spelling of “heatwave” must be “heat wave”; similarly “mega-heatwave” should be spelled as “mega heat wave” (see Wiktionary).

- In different papers we find different spelling of 'heat( )wave'. We chose to use the version without space, but now changed it into the version with the space, as suggested by the referee.

2.) Page 7862, line 2. “Climate models are often not evaluated ...” This is a too strong statement. Take e.g. Vautard et al. and Kotlarski et al. who evaluated models contributing to EURO-CORDEX.

- We changed this sentence into: 'Climate models are not often evaluated...', to make the statement less strong.

3.) Page 7862, line 6: “sensitive” Sensitive to what? The most sensitive physics (e.g. Convection, micro-physics etc.). This has been adapted in the text: with the goal of detecting the most sensitive physics.

4.) Page 7862, lines 7-9: these 55 combinations that can reproduce” can so because they satisfy an pre-imposed criterion. Please mention that the criterion is less than 1 degree bias during the heat wave episodes.

- We changed the sentence into : 55 out of 216 simulations combining different atmospheric physical schemes have a temperature bias smaller than 1 degree during the

heat wave episodes, the majority of the simulations showing a cold bias of on average 2-3 degrees Celsius.

5.) Page 7862, line 11: The statement “..and short wave radiation is slightly underestimated” seems to contradict the results discussed in the paper which clearly show that the model simulations in the mean overestimate the observed radiation, and that this hold for the majority of the five best-performing configurations. Please, restate.

- We changed 'underestimated' into 'overestimated'.

6.) Page 7863,lines 13-14: according to Weisheimer et al., 2011, the enhanced sophistication combined in land surface hydrology, convection and radiation proved key (their words) for a successful reforecast of the 2003 summer in Europe.

- We adjusted the sentence and added that radiation needed to be adjusted as well, as is indeed described in the paper of Weisheimer.

7.) Page 7863, line 15: What is meant with “easily”? Please explain.

- This has now been explained: “because model biases are mixed with sensitivity to initial conditions”

8.) Page 7863, lines 15-19: Sentence starting with “However ...” This a a very long and difficult sentence. Please, cut into pieces and clarify. E.g. what is meant with “ the effect of the representation of physical processes.”

- We cut the sentence and rephrased in: However seasonal forecasting experiments do not easily allow the assessment of model physical processes underlying extreme temperatures during heat waves. This is partly due to their sensitivity to initial conditions. These may inhibit the effect of the representation of physical processes in reproducing the exact atmospheric circulation when starting simulations at the beginning of the season.

9.) Page 7863, line 23: “hindcast simulations” → “evaluation experiments” in CORDEX-

compliant terminology.

- We changed 'hindcast simulations' into 'evaluation experiments' as suggested by the reviewer.

10.) Page 7863, line 26-27: Regarding the line "For some models ...for the 21st century", I am wondering where it comes from. It is not a conclusion taken from Vautard et al. 2013, because that paper was only about ERA-Interim forced RCM- simulations. I am also wondering what the authors intend to say with this line. Because I do not immediately see how the size in bias can be connected with the projected temperature change in the coming century, be, please, more explicit in what you want to say.

- We decided to remove this sentence because indeed it is a bit confusing.

11.) Page 7863, line 27- page7864, line 3. "Individual ... internal variability". Again this is very long sentence, and I do not quite understand what you mean to say with the second part starting with " because ..." Please, clarify.

- We cut this sentence into different parts and adapted to make the second part more clear: 'Individual mega heat waves were reproduced by most models. However, it was difficult to infer whether these models could also simulate associated processes leading to the extreme heat waves. The exact same events with similar atmospheric flow and its persistence could not be reproduced due to internal variability of the models.

12.) Page 7864, line 28: please change "using the same model" into the "using the same model system", because these groups are not using the precise same model.

- This has been changed.

13.) Page 7864, line 29: What is meant with "democracy-driven". Please clarify.

- We meant that instead of using all available models ('classical multi-model ensemble'), in some experiments different parameterizations of one model are used that are selected by different research groups. Using all these parameterizations from the dif-

ferent groups in one experiment, leads to a sort of democracy driven choice.

14.) Page 7868, lines 8-12: This a again a very long sentence. Please, break up in parts. A part of the sentence is removed. It is now: 'From this final ranking, and in order to propose a reduced multi-physics ensemble of five combinations, we successively selected the highest-ranked schemes'.

- The last sentence was not longer applicable, as the 5 highest ranked configurations differ already by two schemes from each other.

15.) Page 7868, line 22: It is not entirely clear to me what criterion was used by the authors to determine the extreme configuration. Is it only based on "daily mean temperature"? Throughout the paper there is only one set of two extreme configurations (am I right?), which is used in Fig 1a-h and Fig S2. Or are there separate sets for France and Russia? I think it would be very helpful if you explicitly state how these extreme combinations are configured. (I might have missed it, but I couldn't find it spelled out).

- The two configurations are simply chosen to show the consistency of 'warm' and 'cold' simulations. They are chosen based on daily mean temperature over France during the 2003 heat wave. They are not separate sets for different regions or years, because that would eliminate their goal: to show the consistency. We added this explication to the text: 'In Fig. 1, we select two extreme configurations (blue and red lines), based on daily mean temperature over France during the 2003 heat wave. Interestingly, they are extreme in all regions and years, indicating that each combination tends to induce a rather large systematic bias.'

16.) Page 7868, lines 23-24. The "large bias" mentioned in these lines is certainly not always large, specifically not for the extreme configuration on the warm side. Please mention.

- We adapted the sentence to mention this point: 'In Fig. 1, we select two extreme configurations (blue and red lines). They are interestingly extreme in all three cases,

indicating that each combination tends to induce a rather large systematic bias. This bias however, is different for the 'warm' and 'cold' extreme configuration'.

17.) Page 7869, lines 9-12: "The two selected extreme ... misrepresentation of the land water supply" What is meant with "land water supply"? Soil moisture content or evaporation/evapotranspiration from the land surface to the atmosphere? I find the argument presented in this line indeed quite suggestive.

- We mean to say that the temperature bias in the two extreme configurations seems not to be due to too much or to less rain (water supply). For example if the 'cold' extreme would have had way too much precipitation, it could have been a reason for the low temperatures, but this does not seem to be the case. To make this more clear we changed the sentence into: 'The two selected extreme combinations are reproducing precipitation overall without a major bias. This suggests that the temperature bias in these two extremes is not explicitly caused by misrepresentation of atmospheric water supply from precipitation.'

18.) Page 7869,' line 12: What is meant with "soil moisture", and also in Figs 2 and S3? Soil moisture of the top soil layer (how thick) or averaged over the whole soil column?

- With soil moisture the moisture over the whole soil column is meant. To clarify this point we added it between brackets: 'However soil moisture (the soil moisture over the whole column) does show a strong relation to temperature biases in model simulations.'.

19.) Page 7869, lines 17-19: this sentence "This indicates ...summer precipitation" precisely underscores why there should have been at least two different land surface schemes included in this study.

- We agree with the reviewer that to answer this question different land surface schemes could be used, but because of reasons mentioned above we chose not to do so.

20.) Page 7869, lines 20-21: "For solar radiation ... approximately 100W/m2 ..." Difference in solar radiation over France and Russia, or differences over one region within an physics-ensemble. Also, solar radiation over Russia is not shown in Fig 1g or 1h.

- There is a mistake in the sentence. It should have been over France for 2003 and 2007 instead of over France and Russia. This is now adapted. Solar radiation data over Russia is very scarce, and so we were not able to compare the model simulations with radiation observations over this area, as is explained in the methodology section.

21.) Page 7869, lines 21-28: Apparently there is a discernible overestimation of solar radiation in the warmest extreme configuration which is not translated in an overestimation of near-surface temperature. So accordingly the authors suspect there is a cooling mechanism without mentioning what that mechanism would be. This is the interesting part. Is it compensated by an overly large reflected solar radiation (unlikely) or is it participated differently over sensible and latent heat flux than in nature, such that latent heat flux is overestimated. Yet, this is not giving rise to more precipitation (no large precipitation bias, see above), nor to more clouds (positive solar radiation bias), nor is it drying out the soil (because the excess latent heat flux continues, otherwise the partitioning of excess solar radiation would go into sensible heat flux giving rise to higher near-surface temperature.) Please, try to identify what this cooling mechanism could be.

- We are not sure about the cooling mechanism. It might partly be the reflection of the solar radiation, but maybe more importantly and overestimation of latent heat flux (which does not necessarily need to lead to higher precipitation rates). However, it is almost impossible to be sure about this, due to the scarce observations of the land heat fluxes and clouds.

22.) Page 7870, line 3: Please rephrase "In order to identify the most sensitive schemes for the development of heatwaves ..." as "In order to identify the parameterizations (or parametric schemes or physics schemes) to which the development of

heat waves is most sensitive ...". Schemes themselves are not sensitive! Check the remainder of your manuscript wrt the use of "sensitive".

- We rephrased the sentence.

23.) Page 7871, lines 23-24: "The overestimation ...for other regions and years ..." I tend to disagree, I find the latent heat flux figure for Russia 2010 (Fig S5e) not very different from the result shown for France 2003 (Fig. 3d). I am wondering how the France-2007 time series for latent heat flux looks like in this respect. Is that comparison available?

- Yes, it is available. The latent heat flux in France 2007 seems still to be overestimated, although maybe in a lesser degree (especially during late summer) than France 2003. However, we also looked at the Iberian Peninsula and Scandinavia, and especially in Scandinavia the latent heat flux seems not to be overestimated. We added an additional figure in the supplementary material to strengthen the statement.

24.) Page 7871, line 26: "cross-validation" → "cross-comparison" (also page 7873, line 7, and in first line of the caption of Table 3)

- These two cases are changed.

25.) Page 7872, line 7-11: The first sentence of the section "Concluding Remarks" is again a very long sentence. It is also not a a very adequate line. Why using the word "small" in front to set, you considered all available combinations in this context. Also the phrase "with a given accuracy thresholds for temperature, precipitation and shortwave radiation" is not clear to me. What kind of thresholds have been used for precipitation and short wave radiation?

- We adapted the sentence to make it more clear: 'In this study we designed and analyzed a large multi-physics ensemble. It is made of all possible combinations of a set of different atmospheric physics parameterization schemes. They were evaluated on their ability to simulate the heat waves of 2003 and 2010 using the regional climate

model WRF based on temperature, precipitation and shortwave radiation'.

26.) Page 7872, line 23-27: the conclusion might be that the performance of a configuration is related to its ability to adequately represent cloud parameters (cloud amount, liquid water, etc.) or cloud-radiative interactions. In that respect I am wondering which parameterization within WRF is representing the stratiform – or layer-clouds? Can you comment on that.

- Stratiform/layer clouds are described in the micro physics schemes, together with other cloud parameters such as particle types. The interactions between clouds and radiation are mostly described in the radiation schemes. However, the performance of the configurations seem to be more sensitive to the convection schemes, where the convective clouds are described.

27.) Page 7873, line 4-6: replace "schemes" by "configurations" or "combinations" or "members"; "scheme" refers to a single parameterization, that is not what is meant here.

- This is now replaced.

28.) Page 7873, lines 11-16: That points to the heart of the matter as I already mentioned under general comments.

- We agree, but hope to have better explained our choice of using only one soil scheme.

29.) Page 7873, lines 17-26: Please mention explicitly that the conclusions from your investigation are only valid for heat wave conditions. There is no guarantee that the constrained ensemble is also better performing for e.g. wet summer conditions or winter conditions.

- The ensemble was also constrained for the summer of 2007, which was a wet year. So although it is true that the ensemble was mostly trained for heat wave conditions, it also performs relatively well in a wet summer year. Winter conditions were not tested in this study, although primarily results of a next study indicate that winter temperatures

(and precipitation) are also quite well simulated. We now mention however that the configurations were not tested on winter conditions.

30.) There are two schemes in Table 1 assigned with number (6), namely WRF-SM6 and Tiedtke. Is that correct?

- Yes, this is correct. For all different physics (radiation, micro-physics, convection, planet-boundary, and surface physics), the schemes are numbered starting with '1', so the schemes can indeed have the same numbers.

31.) I would strongly recommend to split Fig 1 into three Figures, because it is very difficult to read. Fig 1i becomes Fig 1, Figs 1a-c become Figs 2a-c, use column- format like Figs 2a,c,e. Figs 1d-h become Fig 3, also column-format is preferred.

- We changed the order of the figures. Figure 1 is split into several figures as was suggested by the reviewer.

32.) Figs 1d,e,f: Preferably use same y-axis range and start at 0.

- We changed the y-axis range, as suggested by the reviewer.

33.) Figs 1g,h: Preferably use same y-axis range

- We changed the y-axis range.

34.) Fig 2, but also Figs S3 and S4. It is quite hard to distinguish the points by their different colours. It would help to choose different plotting symbols as well.

- We adapted the figures. Now different schemes are represented by different colors and different symbols.

OTHER POINTS:

1) Page 7862, line 7-8: "55 Out of ..." → "55 out of ..."

Corrected

2) Page 7862, line 13: "4" → "four"

Corrected

3) Page 7863, line 1: Use "evaluated" instead of "validated"

Corrected

4) Page 7863, line 26: leave out "Celsius"

Corrected

5) Page 7864, line 22: "with different set" → "with different sets"

Corrected

6) Page 7865, line 11: "the number ... were limited"→ "the number ... was limited"

Corrected

7) Page 7867, line 1: "Tawari" → "Tewari"

Corrected

8) Page 7868, line 11: "to keep"→"in order to favour"

The part of the sentence with 'to keep' has been removed.

9) Page 7869, line 1: "maximal" →"maximum"

Corrected

10) Page 7869, line 5: "during heatwaves years" → "during heat wave years"

Corrected

11) Page 7869, line 6: "in a lesser extent" →"to a lesser extent"

Corrected

12) Page 7869, line 7: "findings found" →"findings reported"

Corrected

13) Page 7869, line 22: “under” →”below”; “the middle of the simulations” → “the mean value (the median value?) derived from the simulations”

Corrected

14) Page 7870, line 2: “how temperature clusters” → “how resulting temperatures are clustered”

Corrected

15) Page 7870, lines 18-19: “affect radiation before heatwaves” →“affect radiation prior to the onset of heat waves”

Corrected

16) Page 7870, line 23: “of Sect. 2” →”introduced in Sect.2”

Corrected

17) Page 7870, line 24: “model-data” → “model-observation”

Corrected

18) Page 7871, lines 1-2: “The same is found ..”→ “The same is not only found ...”. Please also indicate for each statement the season and region. “for the years 2007 and 2010 in Russia” probably should be interpreted as “for the years 2007 in France, and 2010 in Russia”.

Corrected

19) Page 7871, line 21: “... are largely overestimating ...” →”... are found to considerably overestimate ...”

Corrected

20) Page 7871, line 23: “Tiedke”→”Tiedtke”

Corrected

21) Page 7871, line 25: “fairly simulated” → “fairly well simulated”

Corrected

22) Page 7872, line 27: “before” → “prior to”

Corrected

23) Page 7873, line 1: “feedback” → “feed back”

Corrected

24) Page 7873„line 4: “atmospheric schemes” → “atmospheric physics schemes”

Corrected

25) Caption of Table 1: “Physic schemes” → “Physics schemes”

Corrected

26) Caption of Figure 1: “Daily time series of temperature” → “Time series of daily mean temperature”

Corrected

27) Caption Supplementary Figs 2: “2a-d” → “2a-f”

Corrected

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