

Reply to Referee #2

Structural warming biases distort extreme rainfall intensification estimates in event attribution

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Preprint egusphere-2026-3044 — *Weather and Climate Dynamics*

RC2, 27 July 2026 — <https://doi.org/10.5194/egusphere-2026-3044-RC2>

The referee's comments are reproduced in full and in their original order, followed by our replies.

The paper discusses the influence of different perturbations used in the pseudo-global warming experiments to attribute the recent Valencia flood event. The authors compare perturbations derived from CMIP6 models with the perturbation derived from ERA5 reanalysis data, and found that the response is much larger with the ERA5 derived perturbations. They attribute this stronger response to too distinct differences: a much weaker dry lapse rate stabilisation and a stronger land sea temperature contrast in ERA versus CMIP. Given the upcoming field of attribution using similar techniques this is an important and relevant finding that deserves publication. The paper is in general well readable, and figures are clear. However, I also found some fairly major concerns about the interpretation of the results, so I recommend major revisions.

Reply: We thank the referee for a constructive report and for the positive assessment of the work's relevance. The comments have shaped the revision substantially, and we address them in turn below.

Major comments

Major comment 1

The link between changed perturbation is not very clear at the moment. Increased instability (in ERA vs CMIP6) will likely force stronger upward motions (stronger convection), possibly leading to more latent heat release and adjustment of large-scale flow features (as described e.g. in Nie et al., and Lenderink et al. 2017). As the authors rightly point out these dynamical response are relevant when they are a direct response to the changed humidity content of the atmosphere (or systematic changes in temperature gradients), and can therefore be "attributed" to warming. Yet, I did not find any information on how enhanced stability could lead to a stronger response in the paper, and would be good to at least discuss the mechanism and possibly provide supporting information from the model results.

Reply: This is a good point, and we have taken it up in the revised text.

Our causal chain already ran from the vertical structure of the warming to the lapse-rate change and the low-level moistening, and from there to instability. The referee's observation extends it by one step. The ERA5 perturbation gives a median CAPE increase of +28 % against +12 % under CMIP6, a contrast that follows from where the warming and the moistening are placed in the vertical. Greater instability permits stronger updrafts and greater latent heat release, which reinforce one another and can in turn modify the large-

scale flow aloft, including through the associated modification of the thermal wind (Nie et al., 2018; Lenderink et al., 2017). We have added this step to Sect. 3.4, where it bears on two separate things. It completes the thermodynamic chain towards the precipitation response, which previously stopped at instability and now carries through to latent heating and updraft intensity. And it supplies a third candidate contribution to the 500 hPa wind response, alongside the land–sea warming contrast and a possible displacement of the cut-off low. Being itself a response to the altered thermodynamic state, it is attributable to warming in the sense the referee sets out. We present it as a candidate rather than as a demonstrated mechanism, since we have not isolated its contribution.

We have added the two references the referee suggests, together with Lenderink et al. (2025).

Major comment 2

The second mechanism the authors propose is the adjustment due to the thermal wind balance. Although temperature gradient change due to the surface conditions, I guess that the model quickly adjust temperatures due to the soil scheme, boundary layer turbulence and convection, and the large-scale dynamics. It is therefore a bit hard to understand how the initial perturbation leads to larger rainfall in the ERA5 experiment without more specific information on dynamical changes of the system.

Reply: We would first separate two questions, because the larger rainfall in the ERA5 experiment does not require a dynamical explanation at all.

The thermodynamic response imposed by the initial conditions is sufficient on its own. A bottom-heavy warming profile steepens the lapse rate and, at fixed relative humidity, concentrates the moisture increase in the lower troposphere. Both raise convective available potential energy, which is what we find: CAPE rises by a median of +28 % under the ERA5 perturbation against +12 % under CMIP6. In an event already producing deep convection, that difference in available energy and low-level moisture is enough to account for the difference in rainfall. The convective-scale dynamics of course respond, since stronger updrafts are the direct expression of that instability and moisture. It is the large-scale dynamics that are an additional contribution rather than a necessary link in the chain.

On the design itself, we perturb temperature only, in three dimensions and at the surface, holding relative humidity fixed. The three-dimensional wind and geopotential fields are bitwise identical between the factual and counterfactual initial-condition files, as is the surface pressure field. The initial state is therefore not in hydrostatic or geostrophic balance with the modified temperature structure, and the model establishes that balance during the first hours of integration, through precisely the processes the referee lists. The counterfactual is not a state we impose and hold, but a thermodynamic starting point that the model brings into its own balance early in the integration, well before the event itself. Whatever dynamical difference then appears is part of that adjustment, whether through the imposed temperature gradient (minor comment 3) or through the latent heating of comment 1.

The thermal-wind interpretation itself is addressed in our reply to minor comment 3, where we set out the calculation that replaces it.

Major comment 3

I am slightly concerned that the sharpening of the surface pressure gradient west of Valencia with warming in the ERA runs (visible in Fig 4 a, c, Fig S13a,c) is responsible/associated with the strong response of rainfall. But, if the authors can show or make it plausible that this response is not directly inherited from the ERA perturbations, but a dynamical response

as a indirect results from the perturbation (e.g enhanced moisture and greater instability) this concern could also be a strength of the paper.

Reply: The sharpening of the pressure gradient is weaker than it appears. If it were driving the rainfall response there would be a corresponding signal in the near-surface flow, and there is none: Fig. 5e, f show 10 m winds essentially unchanged in both perturbation families. The significant wind response is at 500 hPa, and only under the ERA5 perturbation (median +13 %).

At that level the referee’s concern does become testable, in the form of a possible displacement of the system, and Referee 1 raised it directly. We therefore computed the 500 hPa wind difference maps, which we have added to the revised Supplement. Under a displacement the area-integrated change would be near zero. Instead the mean change over southern Spain is $+0.63 \text{ m s}^{-1}$, the flow is strengthened over 61 % of the area, the normalised asymmetry is +0.49, and the pattern is a coherent band of intensification rather than a pair of opposing lobes. We nonetheless retain a displacement or reshaping of the cut-off low as one of the candidate contributions in Sect. 3.4.

On whether the response is inherited, it is not. As set out in our reply to comment 2, the perturbation modifies temperature only. The three-dimensional wind and geopotential fields are bitwise identical between the factual and counterfactual initial-condition files, as is the surface pressure field. No pressure-gradient anomaly is imposed at initialisation, anywhere in the domain, so the sharpened gradient cannot have been inherited from the perturbation.

The response is therefore emergent rather than inherited, which is the reading the referee identifies as a potential strength of the paper. We have added an explicit statement of this to Sect. 3.4, where the dynamical response is discussed, since the submitted version left it implicit.

Major comment 4

Given the above, I think it would be good to be somewhat more moderate in the claims with respect to how general the results are and how they could be interpreted. It could be worthwhile to also look at discussion in Lenderink et al 2025 in this respect. I would also be a bit more reluctant to the generalise to the other cities, although I agree that the mismatch in lapse rate change is quite important, but many other factors (like land-sea mismatches) could play out differently.

Reply: We accept this and have moderated Sect. 4.3, but we would first distinguish two things, because the submitted text claimed less than this reading suggests.

What Sect. 4.3 claims is a direction. Where CMIP6 warming is top-heavy relative to an observationally constrained estimate, a CMIP6-derived counterfactual will understate instability and low-level moisture, so the attributed response will be weaker. That follows from thermodynamics rather than from the synoptic setting of this case, and Fig. 8 shows the bias is not confined to the western Mediterranean.

What the section does not claim is a magnitude. We nowhere state that an event in London or Los Angeles would show a response of the size found for Valencia. How strongly precipitation responds to a given structural bias is event-dependent, and in a predominantly stratiform event, or one strongly forced by the large-scale dynamics, the sensitivity to low-level stability will be weaker than in a convectively dominated case such as this one. Referee 1 raises the same question from a different direction, noting that extreme precipitation elsewhere may be predominantly stratiform or governed by atmospheric rivers or

extratropical cyclones.

The revised section states this distinction explicitly, presents the size of the effect as requiring testing on other event types rather than as a result, and says that other factors, land–sea contrasts among them, may act differently elsewhere.

Minor comments

Minor comment 1

l 87: constant relative humidity. Do you use the relative humidity change from CMIP/ERA, and adjust absolute humidity according to temperature and relative humidity change (as is common), or do actually use unchanged relative humidity (so same RH for both C and F experiments).

Reply: The latter. Relative humidity is held unchanged between the factual and counterfactual experiments, and specific humidity adjusts to the perturbed temperature under that constraint. At 2 m, where ERA5 provides dew-point temperature rather than relative humidity, the perturbation is applied to the dew-point, so that its change follows from the temperature change alone. Relative humidity is not itself perturbed, and no relative humidity change from ERA5 or CMIP6 enters the construction. We have rewritten the sentence in the Methods so that this is unambiguous, since we agree the submitted wording admitted the referee’s alternative reading.

Minor comment 2

Figure 2: It is a bit unsatisfying that these are yearly mean results, and perhaps would be better to show results for the appropriate season (here, or in the supplement).

Reply: We have added the figure to the revised Supplement, as a September–November version of Fig. 2, referred to from the main text. It confirms that the contrast is not an artefact of annual averaging: in autumn the CMIP6 excess over ERA5 is $+0.13^{\circ}\text{C}$ at both 300 and 700 hPa against only $+0.05^{\circ}\text{C}$ at 1000 hPa, so the top-heavy character of the CMIP6 warming is if anything more pronounced in the season of the event than in the annual mean.

We would explain why Fig. 2 is nonetheless annual. The paper is organised from the general to the particular, and that applies to the temporal scale as well as to the spatial one. Sect. 3.1 is not about Valencia, and not about autumn. It establishes that CMIP6 distorts the vertical structure of warming as a global, year-round property, for which the annual mean is the appropriate measure. The event enters in Sect. 3.2, and the perturbations actually imposed in the experiments are shown in Fig. 3. Those are built from monthly signals interpolated to daily resolution and aligned with the event, so the seasonal information the referee asks for is already there, at the point where it bears on the result.

Minor comment 3

Line 275. The link between upper flow and thermal winds looks to be rather speculative

Reply: All three referees raise this same objection, which is more friction than we had anticipated. The wording was more assertive than it should have been, and a statement of this kind is better quantified than asserted. We would note that the passage carried little weight in the submitted paper. It occupied two sentences of Sect. 3.4, was framed

as a consistency statement rather than a claim of attribution, and could have been deleted without altering a figure, a number or a conclusion. Having said that, we have quantified it. The revised Sect. 3.4 states the outcome qualitatively and points to a new figure in the revised Supplement showing the implied increment. We set out the calculation and the numbers here.

From the imposed δT we integrate the thermal-wind relation to obtain the geostrophic increment between 1000 and 500 hPa,

$$\delta \mathbf{v}_g(p) = \frac{R}{f} \mathbf{k} \times \int_p^{p_0} \nabla_h \delta T \, d \ln p',$$

evaluated on the ERA5 grid with f varying with latitude, truncated at the local surface pressure, and with δT smoothed to 150 km before differentiation, identically for both perturbation families. The horizontal gradient is taken at every level and then integrated, so no single-layer gradient is used to infer the wind aloft.

The result is shown in Fig. R1. Under the ERA5 perturbation the implied increment is organised as a south-west to north-east band, strongest over northern Morocco at +6.9 % of the factual 500 hPa wind speed. Over the Valencian Community it averages +3.3 %, against +0.1 % under CMIP6, compared with a simulated response of $\sim 13\%$. It is therefore of the right sign and order, and clearly capable of contributing, but it cannot account for the simulated difference on its own. It now appears as one of three candidate contributions rather than as the explanation.

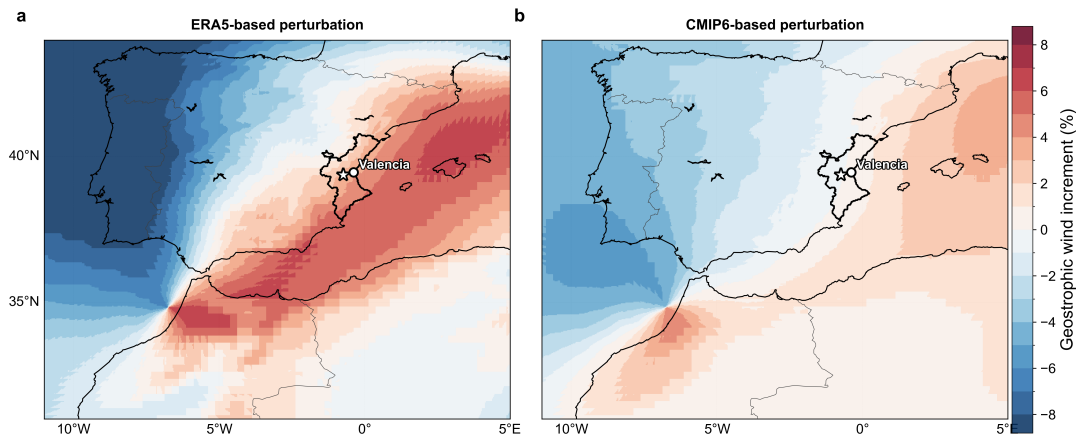


Figure R1. Geostrophic wind increment between 1000 and 500 hPa implied by the imposed temperature perturbation, for (a) the ERA5-based and (b) the CMIP6-based perturbation. The increment is a vector field determined solely by $\nabla_h \delta T$, and is shown projected onto the factual 500 hPa flow, as a percentage of the factual wind speed, so that positive values strengthen that flow. The projection is applied identically to both panels. The outline marks the Valencian Community, the star Turís and the circle the city of Valencia. This figure has been added to the revised Supplement.

Minor comment 4

Line 330. I believe that Engelbrecht et al use ERA to do counterfactual PGW type simulations

Reply: We agree, this was an oversight on our part, and we have corrected it. Engelbrecht et al. (2025) do build their counterfactual from ERA5 trends, as do Wang et al. (2018) for Hurricane Harvey using a different reanalysis and a different period. Our claim of novelty

on this point was therefore too strong. We now cite both at that line and state plainly that reanalysis-derived counterfactuals have been used before.

We have rewritten that line and the related text in Sect. 4.1, which made the same claim in stronger terms. What we retain as our contribution is not the use of a reanalysis-derived counterfactual but the comparison of the attribution outcome against the CMIP6-derived alternative for the same event, which those studies do not make. That comparison was already the point of our paper, so the correction affects how we position the work relative to the literature rather than its findings.

Minor comment 5

L 360. If I understood correctly, 1-h precipitation is area averaged and are not local intensities, which is a bit of the weird statistic in terms of impact. Did you also looked at small scale hourly precipitation?

Reply: The referee’s reading is correct. The hourly statistic is an area average over the Valencian Community, not a local intensity. And no, we did not look at hourly precipitation at the grid point. At that resolution the location and timing of individual convective cells vary too much between members for a 9-member ensemble to constrain the point maximum. At daily accumulation the grid-point maximum is robust enough to report, and we do report it as Pmax in Fig. 5.

We have made the definition explicit in the text, together with the limitation it implies: our hourly results do not speak to changes in local rainfall intensity.

Minor comment 6

In general, I think a greater effort could be done to refer to existing literature on PGW experiments.

Reply: We have expanded the treatment of the pseudo-global-warming literature in the Introduction and in Sect. 4.1. We now cite [Lenderink et al. \(2025\)](#) where the PGW approach is introduced, and [Wang et al. \(2016\)](#), [Wang et al. \(2018\)](#) and [Engelbrecht et al. \(2025\)](#) among the studies that build counterfactuals from observed trends. The two [Wang et al. \(2016, 2018\)](#) studies apply the same detrending strategy to the 2016 Louisiana flood and to Hurricane Harvey, with a different reanalysis, and [Engelbrecht et al. \(2025\)](#) follow them.

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