RC4: Anonymous Referee #2

This study compares different Lagrangian models in their capability to decompose different terms in the heat budget, at the example of a specific event. I have several concerns regarding this paper, which prevent me from recommending it for publication. Sensitivity of the Lagrangian temperature anomaly decomposition to model choice: Lessons from the 2021 Pacific Northwest heatwave

Thank you very much for your careful review, detailed comments and constructive suggestions, which have greatly helped to improve the quality of our paper.

General comment:
In this paper, the authors test the sensitivity of the Lagrangian temperature anomaly decomposition of Röthlisberger and Papritz (2023) to the choice of Lagrangian model. More specifically, for the case of the 2021 Pacific Northwest heat wave, they compare the adiabatic, advective and diabatic contributions obtained from pure-trajectory models with those obtained from simulations with a dispersion model. In their analysis, the authors find substantial differences between the two model types: all pure-trajectory models consistently attribute the largest contribution to the heat anomaly to diabatic warming, whereas all simulations with the dispersion model attribute the largest contribution to adiabatic warming. The authors then show that this difference does not stem from differently simulated horizontal air parcel origins but from differently simulated vertical origins. More precisely, the trajectories from the dispersion model reveal much stronger subsidence than those from the pure-trajectory models, while all trajectories follow a similar horizontal path. In a last step, the authors identify turbulence, rather than convection or mesoscale effects, as the primary source of the discrepancy between the results of the two model types. They conclude that the study reveals a critical sensitivity of the relative contributions of the individual processes when the Lagrangian decomposition of Röthlisberger and Papritz (2023) is applied. Furthermore, they warn that the inferred dominance of any process may reflect the characteristics of the specific model rather than the underlying physics alone.

Unlike Reviewer 1, I do consider the study to be of value to the community — and notably for the very reason Reviewer 1 gives against it. Reviewer 1 argues that calculating pure trajectories in the boundary layer is essentially pointless; there is, however, an abundance of studies of heat extremes that do exactly this, neglecting any effects of turbulence. It is precisely in order to demonstrate how sensitive such trajectory calculations are that this study should be published. Of course, it has long been known that any trajectory calculation is prone to error and that certain assumptions inherent in the parcel approach may not be entirely valid. However, especially within the literature on Lagrangian studies of temperature extremes, there seems to be only limited awareness of the uncertainties associated with the Lagrangian diagnostics used, and the calculated numbers are often taken at face value. This paper highlights this concern.

One key question — deliberately provocative, I admit — that this paper raises is: if the decomposition of Röthlisberger and Papritz (2023) is so sensitive to the model used that it yields even qualitatively different results, is it then useful at all? Mayer and Wirth (2025) proposed a new
decomposition based on anomalous adiabatic, advective and diabatic contributions, which is argued there to be more useful than the decomposition of Röthlisberger and Papritz (2023). It would be interesting to know whether the different models would yield more similar results if this

anomaly based decomposition were applied. This seems plausible, because the respective model climatologies are subtracted, so that model-specific characteristics may cancel out. Such an analysis would be a valuable addition.

We sincerely thank the reviewer for their supportive and insightful comments. We are highly encouraged that the reviewer recognises the primary motivation behind this work: there is a rapidly growing body of research using the Lagrangian temperature anomaly decomposition approach to examine the physical drivers of heatwaves (e.g., Röthlisberger and Papritz, 2023; Oppong et al., 2025; Fernández-Alvarez et al., 2025, 2026; Lemburg et al., 2026; Pérez-Alacón et al., 2025) using pure-trajectory models (e.g., LAGRANTO) and dispersion models (such as FLEXPART). It is important to note that these studies have been focused on the absolute term contributions. As our manuscript shows, applying these two model classes to the same heatwave event yields different physical interpretations regarding the relative contributions of adiabatic and diabatic processes. This is a critical issue for the community, as the choice of model approach may influence the resulting attribution and, consequently, the interpretation of the thermodynamic drivers of a given meteorological event.

The reviewer raises an excellent and deliberately provocative question: If the Röthlisberger and Papritz (2023) decomposition is so sensitive to the model used that it yields qualitatively different results, is it useful at all? This method rigorously partitions the temperature anomaly along air-parcel trajectories, both mathematically and physically. The fact that it yields different results when fed trajectories from different models is not a flaw of the decomposition method itself, but rather a reflection of how air parcel trajectories are computed in pure trajectory and dispersion models. In other words, the Röthlisberger and Papritz (2023) sensitivity decomposition successfully shows the underlying uncertainties in the Lagrangian model (i.e., the presence or absence of turbulence parameterisation) feeding it.

Regarding the suggestion to consider the Mayer and Wirth (2025) anomaly-based decomposition: we agree that this is a highly relevant and important advancement in the field. As the reviewer points out, Mayer and Wirth (2025) focus on decomposing the potential temperature anomaly relative to climatology, arguing that this offers a more meaningful perspective on extreme events. However, there are two important distinctions to make regarding how their approach intersects with our study:

- **Eulerian vs. Lagrangian approach**: The Mayer and Wirth (2025) decomposition relies on an Eulerian tracer advection method (developed in Mayer and Wirth, 2023) to extract Lagrangian information on a fixed grid. This method does not need to calculate explicit Lagrangian particle trajectories. This approach is fed by gridded reanalysis data, such as ERA5. Because our study is specifically designed to assess the differences in Lagrangian models (pure trajectories and dispersion), switching to an Eulerian tracer framework would answer a different scientific question. In addition, note that the decomposition approach proposed by Mayer and Wirth (2025) is derived from the decomposition of Röthlisberger and Papritz (2023), which suggests that the Röthlisberger and Papritz (2023) decomposition is mathematically and physically rigorous. Furthermore, Mayer and Wirth (2025) analysed the PNW21 heatwave as a case study and found that the diabatic process dominated the heatwave development when examined in

absolute terms, which is in agreement with the resulting budget using Lagrangian pure trajectory models. This generates a new question: *Should we use the Mayer and Wirth (2025) method as a reference baseline to assess the Lagrangian temperature anomaly decomposition approach fed by pure trajectory and dispersion Lagrangian models?*

- **Cancellation of Model-Specific Climatologies**: The reviewer poses an interesting hypothesis: if we applied an anomaly-based decomposition to our explicit trajectories and subtracted the respective model climatologies, would the model-specific differences cancel out? While subtracting a model's climatology could certainly remove systematic, persistent biases, we argue that it would not fully reconcile the observed differences. Specifically, it is difficult to confidently interpret anomaly terms when the analysis of absolute terms yields such different results. More importantly, as noted by Mayer and Wirth (2025), the interpretation of the physical drivers of heatwaves depends fundamentally on the chosen analytical perspective, specifically, whether one analyses the absolute contributions of the processes or their anomalies relative to a baseline. However, this is a different research question that is out of the scope of the current work. Nonetheless, we agree that evaluating the adiabatic, advective and diabatic process anomalies relative to a climatological baseline provides valuable context. However, calculating a robust climatological baseline requires long-term backward air parcel trajectory datasets, which currently exist only for dispersion models, rendering this comparative analysis unfeasible for our current study.

In general, we view the anomaly-based perspective of Mayer and Wirth (2025) not as a replacement that renders the Röthlisberger and Papritz (2023) Lagrangian method obsolete, but as a highly valuable Eulerian approach. However, we believe that documenting the differences in the absolute budgets of the Lagrangian temperature anomaly decomposition approach is the necessary first step due to the increasing number of studies using it. In addition, we acknowledge that we do not establish which Lagrangian model class is most suitable for this kind of analysis. Overall, we hope that our current work generates more questions than answers in the community and can serve as a useful basis for future studies addressing them, helping to improve the robustness of Lagrangian heatwave attribution.

Overall, the paper is clear and carefully written, and most figures are presented clearly. However, do have a significant concern regarding how the study by Mayer and Wirth (2025) is presented and how the manuscript relates to it. My detailed comments on this major concern, together with some further thoughts and suggestions, are given below.

Major comments:

1) My main concern relates to the references to Mayer and Wirth (2025), which in places do not reproduce their findings accurately. I think that the references to Mayer and Wirth (2025) are important and precisely for this reason it seems essential to me that what Mayer and Wirth (2025) actually the state is reproduced correctly.

Thank you for your observation. Should the editor consider that resubmission of the manuscript is appropriate, We will revise the manuscript to clearly note the findings of Mayer and Wirth (2025).

L71–73: Here, the authors should state more clearly what is meant by "approaches" in Mayer and Wirth (2025). The key point of that study is the proposal of a new decomposition based on anomalies of the individual contributions, which is argued there to be more helpful for

understanding a temperature anomaly. This reasoning is not yet reflected in the current Manuscript.

We thank the reviewer for pointing this out. We agree that the manuscript should explicitly clarify these approaches within the Mayer and Wirth (2025) framework. We have revised the paragraph to clarify that these 'two approaches' refer to analysing absolute terms versus anomaly terms, both of which can be evaluated within their Eulerian tracer framework. Furthermore, we have explicitly incorporated their argument that evaluating the anomalies of individual contributions offers a far more meaningful perspective for understanding temperature extremes. Should the editor consider that resubmission of the manuscript is appropriate, we will revise this paragraph as follows:

Recently, Mayer and Wirth (2025) introduced a key theoretical development, using an Eulerian tracer advection method to extract Lagrangian information on a fixed grid. Their study demonstrated that the physical interpretation of heatwaves depends fundamentally on the chosen analytical perspective. Within this Eulerian framework, Mayer and Wirth (2025) evaluated two distinct approaches: analysing the absolute contributions of physical processes versus evaluating the anomalies of these individual contributions relative to a climatological baseline. They argue that an anomaly-based decomposition is more robust for understanding the physical drivers of temperature extremes. Furthermore, they showed that these two approaches can yield substantially different conclusions regarding which process dominates the development of the temperature extreme. These findings have profound implications for heatwave attribution, demonstrating that the identified physical drivers are highly sensitive to the chosen methodology. Overall, the Lagrangian temperature anomaly decomposition (Röthlisberger and Papritz, 2023) and the Eulerian approach (Mayer and Wirth, 2025) represent two distinct frameworks for examining the physical drivers of heat extremes.

L69/73: I may have overlooked an explanation, but I find the argument difficult to follow. On the one hand, the authors characterise Mayer and Wirth (2025) as a "key theoretical development" with "profound implications"; on the other hand, they retain the original approach of Röthlisberger and Papritz (2023) without further justification. This apparent inconsistency should be resolved explicitly, either by explaining why the earlier approach remains valid, by stating what the implications of Mayer and Wirth (2025) actually are for the present analysis, or by applying the decomposition approach of Mayer and Wirth (2025).

We thank the reviewer for highlighting this apparent inconsistency. We agree that our characterisation of Mayer and Wirth (2025) as a "key theoretical development" could inadvertently imply that the Röthlisberger and Papritz (2023) approach is no longer valid. This was not our intention.

By describing Mayer and Wirth (2025) as a 'key theoretical development,' we intend to emphasise that calculating anomalies relative to a baseline provides a different physical interpretation of heatwaves than calculating absolute terms. Nevertheless, this does not mean the Lagrangian temperature anomaly decomposition proposed by Röthlisberger and Papritz (2023) is flawed. This Lagrangian framework can be used to quantify the anomalous, rather than absolute, contribution of each physical process. Finally, it is worth noting that the Mayer and Wirth (2025) approach is based on an Eulerian framework, whereas the Röthlisberger and Papritz (2023) decomposition is purely Lagrangian.

Given the widespread and continued use of the Lagrangian temperature anomaly decomposition in heatwave research analysing the absolute term contributions, we believe our current study provides

useful information for the community by highlighting how the choice of model approach can affect the resulting physical interpretation.

Should the editor consider that resubmission of the manuscript is appropriate, we will better link the paragraph introducing the findings by Mayer and Wirth (2025) and the next one in the revised manuscript as follows:

While the choice of analytical perspective (absolute versus anomalous) introduces one fundamental sensitivity in heatwave attribution (Mayer and Wirth, 2025), a separate but critical uncertainty exists within the Lagrangian temperature anomaly decomposition.

L244/245: It is true that Mayer and Wirth (2025) report diabatic heating as the largest contribution during the Pacific Northwest heat wave, but this holds only for the decomposition in terms of absolute fields (i.e. the Röthlisberger and Papritz approach). That study also shows, however, that if the decomposition is applied in terms of anomaly fields, the relative importance of the contributions changes substantially, with horizontal advection emerging as the largest contribution. This distinction appears central to the argument of Mayer and Wirth (2025) and should be made explicit here.

We thank the reviewer for this comment. The reviewer is completely correct that Mayer and Wirth (2025) demonstrated that the relative importance of contributing processes changes when using absolute or anomaly terms. In particular, horizontal advection emerged as the largest contribution of the PNW21 heatwave when using the anomaly-based perspective. However, in the context of our study, we chose to highlight their findings using absolute terms because they are comparable with our results. Note that our study examined the differences in the Lagrangian temperature decomposition using pure-trajectory and dispersion models. Because we do not calculate anomalies relative to a climatology, comparing our absolute results with Mayer and Wirth (2025) anomaly-based results will also produce a methodological discrepancy.

L344/345: Here, the authors write that "Ultimately, the question of which physical process drove a HW event may have no unique answer; it depends on the perspective (and the model) from which the event is examined." I agree with this statement in principle. However, I do not see that the manuscript actually demonstrates any dependence on the perspective. In Mayer and Wirth (2025), "perspective" is used in the sense of the two different decomposition approaches. It would therefore be helpful if the authors clarified what they mean by the term here.

We thank the reviewer for this observation. We completely agree that the term "perspective" was used too loosely in this concluding sentence, making the argument difficult to follow.

In this specific context, we intended to summarise the two major methodological sensitivities: (1) the analytical perspective (i.e., whether the decomposition examined the contribution from absolute terms or anomaly fields, as demonstrated by Mayer and Wirth, 2025) and (2) the discrepancies associated with the Lagrangian model (pure trajectory or dispersion model) employed.

To resolve this ambiguity and ensure the conclusion accurately reflects the preceding discussion, we have explicitly clarified what we mean by these terms. Should the editor consider that resubmission of

the manuscript is appropriate, the revised sentence will clearly distinguish between the chosen analytical approach (anomalies vs absolute terms) and the underlying trajectory model.

Ultimately, the question of which physical process drove a HW event may yield no single answer; rather, it depends fundamentally on both the analytical perspective adopted (i.e., whether evaluating contributions from absolute terms or anomalies relative to a climatology, as demonstrated by Mayer and Wirth, 2025) and the type of Lagrangian model (pure trajectory or dispersion) employed.

2) This relates to my general comment and to comment 1): Would it be possible to extend the same analysis to the anomaly-based approach proposed by Mayer and Wirth (2025)? While the main challenge would be the need for a climatology of the contribution terms for each model, I wonder whether this could be feasible for at least a subset of the models. If I understand correctly, long-term datasets are available for some of them, which could make such an analysis achievable without an excessive amount of additional work. I believe this could be a valuable extension of the analysis. If such an analysis is not possible, I would at least expect a detailed discussion of this topic.

We thank the reviewer for this suggestion. However, performing an anomaly-based Lagrangian decomposition requires long-term air parcel trajectory data. While it is true that such long-term trajectory datasets are available for the dispersion model class, calculating a comparable climatology for pure trajectory models would require considerable computing time, making this analysis unfeasible for our current study, as we noted in the response to your general comment. This could be a further step in understanding the discrepancies in the Lagrangian temperature anomaly decomposition as a consequence of model choice.

Should the editor consider that resubmission of the manuscript is appropriate, we will add the following sentences into the last section of the revised manuscript.

In addition, as Mayer and Wirth (2025) demonstrated, analysing anomalies rather than absolute terms can yield different conclusions about which process dominates HW development. Building on this insight, future research should apply the Lagrangian temperature decomposition to evaluate process anomalies relative to a climatological baseline rather than rely on absolute values.

3) What I find interesting is that the two model types yield very similar horizontal pathways, yet the contribution from horizontal advection differs between them. I think this needs some discussion.

We thank the reviewer for highlighting this interesting result. We completely agree that the difference in the advection term warrants further discussion, particularly given the similarity of the horizontal pathways between the two model classes. Broadly, the advective contribution shows an overall agreement within each model class. Intra-model differences likely stem from differences in trajectory computation and the total number of simulated parcels. However, while less noticeable than the discrepancies between adiabatic/diabatic processes, the inter-model differences in the advective term are also likely related to parameterised processes in dispersion models.

Should the editor consider that resubmission of the manuscript is appropriate, we will add the following sentences in the revised version of the manuscript

It should be noted that there is an overall agreement in the advective contribution within each model class, where minor discrepancies likely stem from differences in trajectory integration, temporal and spatial interpolation, and the number of released parcels during the simulations. However, while less noticeable than the discrepancies between adiabatic/diabatic processes, the inter-model differences in the advective term are also likely related to the use of turbulence, convection and sub-grid-scale parameterisations in dispersion models.

Minor comments:

L82: I do not understand why the divergence in findings between models could be understood through the physical mechanisms typically associated with heat waves.

Thank you for this observation. Our aim was simply to note that the discrepancies between models could be understood by analysing the atmospheric circulation patterns and land-atmosphere feedback during the heatwave. We agree that the original phrase in the manuscript is confusing. Should the editor consider that resubmission of the manuscript is appropriate, we will remove it from the revised version of the manuscript.

Formula (1): Why does t_g carry the superscripts sea, adia and dia in the lower limit of the integrals? Furthermore, x_g (and other variables) are not explained.

Thank you for this observation. The correct lower limit is indeed tg. Should the editor consider that resubmission of the manuscript is appropriate, this typographical error will be corrected in the revised manuscript. Additionally, we will explicitly define the variable xg, which represents the position of the air parcel at time tg. The definitions of the remaining variables were intentionally omitted from this manuscript to keep the description of the Lagrangian temperature anomaly decomposition concise and readable. We refer readers to the original work of Röthlisberger and Papritz (2023) for an exhaustive discussion of those mathematical details.

L232: Well, it is not too surprising that Röthlisberger and Papritz reported very similar contributions, given that they used the same data. …

We agree with the reviewer that this consistency is entirely expected since both studies used the same data. We decided to include the findings of Röthlisberger and Papritz (2023) because their work is the foundational paper for the Lagrangian temperature anomaly decomposition, and it presents the PNW21 heatwave as a test case.

L266: What are Lagrangian parcels? Are parcels not always Lagrangian?

Thank you for this observation. This is a typographical error that will be corrected accordingly in the revised version of the manuscript.

Figure 3: It is not clear to me what is shown here. In particular, what is meant by a parcel distribution for LAGRANTO? Regarding panel (h): is this panel really needed?

We thank the reviewer for the comment. To clarify, the parcel distribution refers to the spatial and vertical position (longitude, latitude, and pressure) of the air parcel at time t=0 over the heatwave region.

Regarding panel (h): This panel is needed because the 3D scatter plots in panels (a-g) make it difficult to visually discern subtle differences in vertical dispersion and median pressure heights across the models. Panel (h) provides a quantitative violin plot comparison of the pressure distributions, allowing

readers to clearly see that all models show almost the same distribution of the parcels over the heatwave region.

Figure 4: What is the red rectangle in the left panel?
The red rectangle in the left panel indicates the geographical region shown in Figure 1 for reference. We will clarify this in the caption of Figure 4.

L399: "the anomaly" instead of "the warming" in "the largest fraction of the warming
Thank you for your suggestion. It was corrected accordingly