This study explores the link between concurrent hemispheric heatwaves (high Land Surface Temperatures, LST) and extremely weak Meridional Heat Transport (MHT) events. Authors present analysis for these combinations during boreal summer and winter, describing in detail the associated dynamical patterns across multiple variables, which they relate to existing literature. This descriptive approach is thorough, but the interpretation could be strengthened by drawing clearer connections to the study's main research questions and hypotheses, which are not clearly formulated to begin with.

While the study tackles an important and timely topic, its current presentation limits the clarity and potential impact of its findings. The manuscript would benefit greatly from a clearer framing of the study's goals and novelty early on, as well as a more integrated structure that better guides the reader through the analysis. In particular, introducing the central mechanism earlier and using schematic summaries would help contextualise detailed descriptions and improve overall readability. I therefore recommend a substantial restructuring to enhance narrative flow and ensure the scientific significance of the work is fully conveyed. See general comments for suggestions intended to be constructive.

***We thank the reviewer for the insightful evaluation of the manuscript and constructive criticism. We take full responsibility for not having conveyed the main message of the analysis in a sufficiently clear way, and for the lack of structure that the reviewer perceived in the way arguments were exposed. This is in line with the criticism raised by reviewer 1, and prompt us to substantially rethink the organization of the work, and to highlight the main message in a more logical way. As mentioned in the reply to reviewer 1, the restructuring of the manuscript would lead to a tentative revised structure that would look like this:***

- ***An Introduction, explicitly mentioning in a clear and revised way what are the aims and scope of the present work, and what is its novelty compared to existing literature;***
- ***Data and Methods;***
- ***Results: for conditional occurrences, separately for boreal Summer and boreal Winter;***
- ***A discussion section providing a physical interpretation of results, making explicit connections with dominant planetary scale waves, building upon previous work, with the help of case studies that will be detailed in the Supplementary material;***

- ***Summary and Conclusions: evidencing the main take-home message of this manuscript, and the lines of research that are opened by this analysis;***
- ***Figures 1, 4, 5, 6, 9, while those regarding case studies will be moved to the supplementary material;***

***We answer briefly the specific points raised by the Reviewer, highlighting how we are planning to achieve this revision.***

**General Comments**

1. The introduction currently reads as though there is some confusion or inconsistency in how the authors position their work relative to existing literature and understanding. Authors write that they intend to explore link of LSTs to extremes in planetary-scale Rossby wave anomalies, which is a classic subject in atmospheric dynamics, yet at the l. 44 write "the literature [...] primarily focused on dynamical drivers," implying that in this study they will not do so, but isn't this study also examining dynamics? Perhaps the intention is to highlight that prior work has largely focused on Rossby waves as responses to atmospheric variability, without explicitly linking them to MHT processes. If so, I suggest clarifying this distinction. I recommend placing greater emphasis on the novelty of incorporating one large-scale thermodynamical mechanism (i.e. MHT). To strengthen this point, it would be helpful to include a statement highlighting that, to their knowledge, no prior study has specifically explored this linkage. That said, the study also engages with wave dynamics (e.g. wavenumber analysis), so care should be taken not to present the work as focused solely on thermodynamics. Emphasising the integration of thermodynamical and dynamical perspectives would more accurately reflect the study's contribution. Also, is there a well-established hypothesis connecting moisture heat transport to (concurrent) heat extremes in Rossby wave anomalies? Or are the authors proposing or testing a new mechanism? This lack of clarity makes it hard to follow how the study's "process-based investigation" (l.45) advances our understanding of co-occurring hemispheric heatwaves. Formulating specific research questions would go a great way to improve the clarity and goal of the paper.

***Thank you for the detailed criticism about the positioning of the work. This gives us the opportunity to specify what are the motivations behind this analysis, in the context of existing literature (most of which has been mentioned in the introduction):***

- ***zonally integrated meridional heat transport extremes are related with the excitation of planetary-scale waves in both seasons (Lembo et al. 2019);***
- ***heatwaves in the Northern Hemisphere are often associated with concurrent events spanning the most part of the mid-latitudinal channel (e.g. van Loon and Thomson, 2023);***
- ***a clear and robust identification of heat transport extremes would allow to provide a coherent interpretation of how heat transports are related to concurrent heatwaves through the action of planetary-scale waves;***
- ***the mechanism underlying this relation can be understood through the knowledge of changes in the dominant weather regimes that was discussed in a previous companion paper (Lembo et al. 2022);***

***Given these premises, while existing literature has mainly focused on the dynamical drivers behind the occurrence of hemispheric heatwaves, we want to highlight how meridional heat transport extremes link with (some of) these types of heatwaves through the excitation or dampening of specific planetary-scale waves. This relates the thermodynamic background energy flows to the occurrence of such events. We did not aim to provide any hypothesis about the role of moisture, as we think it falls beyond the scope of the present work.***

***We will substantially revisit the Introduction, addressing this suggestion and comments from the other reviewer.***

2. Regarding Section 4

   The explanation of the proposed mechanism is introduced only in the results section, and primarily through a case study. While I understand the use of a case study as a narrative device, in this context it would be more effective to introduce the mechanism earlier – ideally in the introduction – to better frame the study and guide the reader through the analysis. This could mean swapping the position of Sections 4 and 5, so that first we are presented with the overall picture, and then an example to show what a typical (or not typical) case might look like.

***Agreed. We will swap sections 4 and 5 (and the arguments in section 6) to reflect whit suggestion.***

This section contains detailed descriptions that can be difficult to follow, but it remains unclear what they amount to. Adding a schematic (possibly even at the expense of Fig. 2) summarising the

key processes, stages, and mechanisms – whether in the case study or the general explanation, whichever helps improve readability – would greatly help readers grasp the overall picture more easily. One schematic for the summer case and another later for the winter case.

***Agreed. We will provide a depictive scheme in the Discussion section, trying to explain what is the proposed mechanism underlying the relation between MHT extremes and hemispheric heatwaves.***

3. As it stands, Sections 4–6 are very description-heavy, referring to multiple subfigures, variables, and seasonal differences. Because the results and discussion are presented as separate sections, it becomes difficult to digest and interpret the findings. A more integrated structure – e.g. combining results & discussion – could improve readability and help contextualise the findings as they are introduced. The schematic illustration suggested above could also aid in guiding the reader through the narrative.
4. Another possible restructuring tactic would be to subsume the current Sections 3–6 into a single Results section, with subsections (e.g. 3.1–3.4) capturing the existing material.

***We agree with the reviewer that the Results section could be substantially improved, making it more integrated to the Discussions section. We prefer addressing this by taking on point 4. suggestion, as this makes it easier to place the case studies in the context of the analysis.***

5. Towards the end of the study, authors suggest that the findings could help improve the predictability of certain events, it is currently not clear how this connection is established or supported by the results. That said, this might become more self-evident once the structure and contextualisation of the manuscript have been revised.

***Apologize for the confusion. We did not mean to imply that our results facilitate better predicability of hemispheric heatwaves. We refer to "tipicality" at the end of section 7 to emphasize that some conditional occurrences are somehow typical and relate (cfr. Lembo et al. 2022) to dominant weather regimes. We cannot infer how predictable this occurrences are given some values of MHT. We will state it more clear how this related to the typicality of heatwaves in the revised manuscript.***

**Specific comments**

l.5 “other conditional occurrences” is too abstract and uninformative – what are these other occurrences?

***We simply refer here to other combinations of LST and MHT extremes (e.g. cold LST-weak MHT, warm LST-strong MHT…). We will revise the phrase to clarify this.***

l.17 and 24 The use of 'evidence' as a verb is technically correct but sounds unusual in contemporary academic English and may read as non-idiomatic. A more standard phrasing such as 'shows' or ‘highlights’ or 'provides evidence for' would improve readability.

***Agreed. We will try to avoid using the verb “evidence” as much as possible.***

l.38 “underlying conditions” is vague here, as it appears to group together two quite different types of factors: fixed geographic characteristics (orography and land-sea contrast) and low-frequency or slowly varying boundary conditions (sea surface temperature anomalies). The phrase could be amended to be more precise.

***Agreed. We will revise the phrase in order to be more specific.***

ll.51-6 See general comment recommendations for re-structuring.

l.80 Numbering missing for equations pertaining to wavenumber decomposition.

***Apologize for the inconvenience. We will make sure that the equations are correctly labeled.***

l.93 add space before ‘with’

***Agreed***

l.96 add space after *index*"

***Agreed.***

Sect. 2.2.2. Some key details are missing or unclear in this paragraph for full reproducibility. Detrending and deseasonalisation are mentioned, but the specific methods used aren’t explained. It’s also unclear how the declustering is done – what parameters or window are used? Finally, more detail is needed on how the EVT-based convergence algorithm works in practice.

***Agreed. Some details of the procedure are not explicit, as also outlined by a community comment. We will revise the manuscript in order to account for the missing explanation on the methodology.***

l.111 This sentence implies a directional, possibly causal relationship. I suggest using "association" instead of impact if the paper only shows statistical associations, and no causal analysis. If authors had introduced a clear hypothesised mechanism, then using terms like "impact" might feel more justified – even if causality isn't fully proven.

***Agreed. The sentence can read as a bold statement about causality, although it is not. This point was also raised by the other reviewer. We will revise the text in order to reflect that we are mainly describing statistical associations that we aim to interpret.***

l.114 "composite mean of such quantities in coincidence of LST|MHT" is a bit wordy – could simply write "composite mean of LST|MHT"

***Agreed. We will revise the text accordingly.***

ll.111-114 "…whether the composite mean … is random or reflects an emerging pattern." Is somewhat vague. What is the null hypothesis? i.e. are co-occurring MHT extremes associated with significantly different conditions *during* LST extremes?

***The null hypothesis is that the set of conditional occurrences is a random set of occurrences with fixed size, and that the anomalies thereby shown are non significant.***

l.155 what counts as a "consistent deviation"?

***Agreed. We should refer to it as "significant". We will revise the text accordingly.***

l.118 Would suggest adding a reference to Wilks (2016).

***Thanks. We will add the suggested reference.***

ll.119-20 Significantly different the OVERALL distribution? So are you testing LST|MHT against LST|$\bar{}$MHT$\bar{}$ or climatology? Please clarify.

***By "overall distribution" we refer here to "climatology" for what concerns masks applied in Figure 1 and B1, LST95 events for what concerns Figure 4 and Figure 6.***

***Apologize for not stating it clearly here. We will revise the manuscript in order to be more precise.***

l.122 “weak MHT extreme” sounds contradictory. If the intended meaning is “extremely weak MHT values (i.e. very low MHT, and not non-extreme or moderate), then a clearer phrasing would be “extremely weak MHT” or possibly “strongly negative MHT anomalies” or “lower-tail MHT extremes.” I suggest considering one of these alternatives *throughout the manuscript*.

***Agreed. We used the term “extremely weak MHT” in several instances, so we would stick with this terminology, whenever we do not use the related symbol.***

Table 2. Last row highlights values below 10%, in contradiction with caption.

***Thank you for spotting the inconsistency, that was also detected by reviewer 1. We will correct the caption accordingly.***

ll.126-9 What is the implication for your study of the statement in the sentence starting “However, […]” with regard to the strength of the association between MHT -> LST ?

***We carried out the same analysis by both including and excluding <5 days lasting heatwaves, but we did not notice significant qualitative differences. Therefore, in order to preserve a larger number of instances, and for consistency between boreal Summer and Winter, we preferred maintaining all heatwaves with any duration. We will revise the analysis on only >5days and we may include this as an additional Appendix, in case we would notice that there would be anything worth highlighting.***

l.128 “consecutive days” is more standard phrasing in this context.

***Agreed. We will revise the sentence accordingly.***

ll.130,133 “weak MHT-warm LST” is the same as $LST_{95}|MHT_{\downarrow}$, correct? I suggest sticking with the latter ‘symbolic’ notation throughout the text for clarity and consistency. Reverting to mixed phrases like the former can easily lead to confusion, especially when other conditions (like strong MHT) are also discussed.

***Agreed. We will try to use the symbolic notation everytime it is possible.***

ll.130-33 One of the study’s key claims regards $LST_{95}|MHT_{\downarrow}$ accounts for 30% and 40% of warm LST days in JJA and DJF, respectively. However, this paragraph suggests

that these figures come from a fraction of heatwaves, whose total durations make up that proportion of LST days. If so, I think this deserves clearer framing – the current phrasing risks implying that individual $LST_{95}|MHT_{\downarrow}$ days make up that share, rather than those days occurring within longer events. I recommend clarifying whether the percentages refer to discrete days or to the cumulative duration of events in which $LST_{95}|MHT_{\downarrow}$ conditions are observed. This is a presentation issue, not necessarily a flaw in the result, but it risks misleading interpretation.

***Indeed, we claim that the conditioned events are related to 30.4% >5days events in DJF, 42.5% in JJA. This is not meant to say that all days belonging to the heatwave are characterized by a conditional occurrence. We will state it very clearly at the beginning of this paragraph and elsewhere in the text, where relevant.***

Figure 1. I suggest reordering the figure panels so that the same variable is shown side by side across seasons. This would make seasonal differences easier to compare at a glance.

***This is a very good point. Thank you for the suggestion. We will certainly implement it.***

l.156 The interpretation of wind speed anomalies is not straightforward when trying to infer changes in the jet stream's absolute strength or position. A positive anomaly simply indicates stronger-than-average winds at a given location, but this can mean very different things depending on the climatological mean – e.g. in climatologically weak-jet regions, it may reflect modest flow or jet extension, while in strong-jet regions, it could signal true intensification or a shift in jet position. I suggest supplementing the V250 fields with contours of absolute wind fields or some baseline seasonal climatology.

***Agreed. A similar suggestion was also provided by reviewer 1 and will certainly find the best way to implement it. We may have to use a different strategy than what proposed, given that the contours could be shaded by the significance masks, but we will address it fully before submitting the revised manuscript.***

ll.177-8 The authors attribute the pattern to strong blocking based on SLP anomalies. However, since blocking is generally diagnosed using mid- to upper-tropospheric geopotential height or potential vorticity. I suggest either supporting this claim with such fields or rephrasing to avoid implying a definitive blocking event based on surface pressure alone.

***Agreed. Thank you for raising this point. As we acknowledge that the blocking is usually computed from geopotential height fields at 500 hPa (cfr. Davini et al. 2012), we should refer to these patterns more properly as high/low pressure systems. Nevertheless, as this point was raised by reviewer 1 as well, we realise that the manuscript would benefit from a quantitative estimate of blocking index over the usually active regions, we will provide this, based on the Tibaldi-Molteni index and successive updates (e.g. Davini et al. 2012) and compare this with what can be drawn out of considerations about SLP fields, especially in Summer (cfr. Katsafados et al. 2014).***

ll.186-7 See my comment regarding l.156.

***Agreed. We will make the climatological fields explicit in the revised figures and text.***

Figure 3. I recommend increasing the font size of all text and labels, as well as increasing the thickness of curves in Fig. 3b to improve visual communication. Additionally, Fig. 3a title says MHT anomalies, but the caption says absolute MHT. Please clarify.

***Agreed. We will increase the size of axis labels, the thickness of lines in Figure 3b, and replace the title of panel 3a with the correct one. Apologize for the lack of visual clarity.***

l.219 Are authors making this claim based on the one case?

***This point was raised by Reviewer 1 as well, therefore we apologize for not making it clear enough that we hereby refer to the results from White et al. 2022 regarding the 2021 NW Pacific heatwave, and we mean here to confirm that this argument can be generalized to this class of cases for boreal Summer. We will revisit the phrase accordingly.***

l.258 Double-check WCD's stylistic guide for dates: I think "19 January 2007" without 'th' may be correct.

***Sure. This is a typo and we will certainly correct that.***

l.274 "O" letter instead of "0" number in ENSO

***Thanks for spotting it. We will revisit the text accordingly.***

ll.299-300 What are the implications of these observed differences? Clarifying this sentence would strengthen the connection between the diagnostics and the study's core questions – which, as currently presented in the introduction, are not that clear and could benefit from sharper formulation.

***Thank you for this comment. We realise that we should dig a bit further in the interpretation of these differences. Therefore, we will expand this paragraph and be more specific about how this is relevant for our understanding of MHT extremes role for hemispheric heatwaves.***

ll.322-6 This is precisely the kind of information that could be part of a summarising schematic.

***We think that this summarizing sentence is useful in this context, in order to avoid the reader being confused by the chain of arguments above. We will make sure to repeat or at least mention more explicitly this concluding remark in the Conclusions.***

ll.328-31 An interesting hypothesis that would be worthwhile testing in future research!

***Thanks! We do in fact plan to investigate these connections in future climate change scenarios across CMIP-class models.***

l.345 The phrase "other combinations" is vague and uninformative. I recommend briefly restating the specific combinations for clarity, even if this involves some repetition.

***Agreed. We will try to recall the conditional occurrences combinations briefly at the beginning of the Conclusions.***

l.348 Once again, I would hesitate to mention causality.

***Agreed. This hints once again at the necessity of clarifying the aims and scope of this work, that will be among the main efforts required to revisit the manuscript and prepare a new version of it. We take full responsibility for this lack of clarity and plan to convey the relevant message in a more explicit way in the conclusions.***

Figure 7b and various in panels Figure 9. Some anomalies exceed the colorbar range, resulting in blank or clipped areas in the plots.

***That is true. We were a bit hesitant in changing the colorbar range, in order to maintain a range that is coherent across different figures and avoid shading the relevant results. We will revisit the best combination of colorbar range that allows to convey the message and fit in the whole range of anomalies in each panel.***