Response to Maarten Ambaum

This work addresses an important problem in atmospheric dynamics, namely the dynamical processes underlying the formation of the baroclinic zone, as diagnosed by the slope of isentropic surfaces. A new perspective is introduced by looking at the slope evolution in phase space. It is found that the upper and the lower troposphere exhibit very distinct (in some sense opposite) dynamics for the slope evolution.

My own work also looks at phase space dynamics of the storm track and it will be no surprise that I find this approach of great interest. However, the manuscript left me wondering in the end, what the real conclusion was regarding the role of diabatic effects in slope dynamics. The writing is somewhat evasive and vague at many points so it is hard to interpret what the authors are actually saying.

I would imagine that a more clearer layout of the arguments at those points should clarify sufficiently what was specifically meant. Below I comment on the manuscript in order of line number, not in order of importance.

We thank Maarten for such a rapid and thorough review of our work. We hope to have addressed all the concerns raised, which helped us improve the readability of the manuscript. Here we provide a line-by-line response.

Specific comments

1) **l.12: "... rendering the phasing ..."; did you mean something like "... suggesting that this phasing is ..."; I also assume that "phasing" actually means the order in which processes occur; perhaps that would be a clearer and simpler way of putting it?**

   Our use of the verb render is perhaps not very common. To improve readability, we rephrased as follows: "The same phasing between diabatic and tilting tendencies of the slope is observed both in upstream and downstream sectors of the North Atlantic and North Pacific storm tracks. This suggests that the opposite behaviour between near-surface and free troposphere is a general feature of midlatitude storm tracks."

2) l.88 Presumably you meant $d\theta/dz < 10^{-4}$... not $> 10^{-4}$

Yes, thanks for spotting the typo. We have fixed it.

3) l.92:"... does not affect our analysis as we exclude land grid points." Given that the cold air for the all-important cold air outbreaks is sourced on the land, it seems to me that it is strange to exclude land points: as a local tendency it is perhaps not important, but as a source of required air masses it is. How is that reflected in your equations or your analysis? It seems to me that the anomalously high diabatic cooling over land is absolutely crucial in providing a source of the gradient in the baroclinic zone.

The isentropic slope and its tendencies are computed everywhere and we then exclude land grid points when computing spatial averages. So, the contribution of diabatic processes over land enters the domain of interest over the ocean through the slope entering the domain. We exclude land grid points to avoid the signature of orographic effects, which can dominate the response in TILT and thereby confuse the analysis. We have edited the manuscript to make this more explicit. "Over land, the largest amount of masking occurs in correspondence with high orography, though this does not affect our analysis as we exclude land grid points when calculating spatial averages."

4) Fig 1: Perhaps label the figures which are free troposphere and which are lower troposphere? Also: you use the word "hatching" in this figure and figure 9, but you actually show a stippling. (I thought I'd check the dictionary on my computer, just to be sure. This is the entry: "hatching | hatʃɪŋ | noun [mass noun] (in fine art and technical drawing) shading with closely drawn parallel lines: the miniaturist's use of hatching and stippling.")

We have labelled the figures accordingly, explicitly mentioning the pressure levels between which the vertical average is taken (we realised we actually meant 900-825hPa for the near-surface, so we have changed that in the text as well). Thanks for pointing out the difference between stippling and hatching,

one never stops learning! In our defence, the relevant command in Python uses 'hatches' for both, still two wrongs don't make a right, so we have corrected this in the revised manuscript.

5) l.132: "... mainly due to orographic effects advected ..." I do not know what the process is that you are describing. Please be specific what you actually mean. How does orography swap the sign of these effects, and how does this advection work?

By orographic effects, we refer to gravity waves and other mesoscale features excited by mountain ranges that can result in significant TILT. At instances when TILT is generally weak over most of the spatial domain, these orographic effects can dominate the domain-averaged value for the tilting term. To reduce the impact of these undesired orographic effects, we adjusted the domains accordingly. We have rephrased the manuscript as follows to clarify this point:

"There are instances where they change sign, most often for TILT rather than DIAB. This is mainly due to orographic effects (e.g., gravity waves and other mesoscale features excited by mountain ranges) advected over the ocean that can result in significant positive TILT and thus dominate its domain-averaged value, especially when TILT is generally weak. To reduce the impact of these undesired orographic effects, we adjusted the domains accordingly."

6) Fig.2: Looking at the lower panel, it would be of interest to add a third line indicating "DIAB + TILT"; it is after all the lack of compensation between those two terms that provides the tendency for the slope. In that respect, could you please comment, perhaps somewhere around l. 135, whether the IADV term is smaller than the DIAB + TILT combination? If DIAB and TILT are largely compensating each other then their individual magnitudes do not matter that much. It seems to me that the mismatch between DIAB and TILT is the crucial variable. (Of course, the phase space analysis is diagnosing that mismatch in detail.)

We have looked in more detail into the discrepancy between DIAB and TILT.

Figure AR1 is an extended version of the manuscript's Fig. 2, which also includes time series for DIAB+TILT (Fig. AR1c) and for the advection term, ADV (Fig. AR1d). We show ADV instead of IADV as we have reformulated the slope tendency equation (Equation 2) in its Eulerian form rather than Lagrangian, in response to the other Reviewer. In the Eulerian form, the advection term also includes local slope tendency due to the adiabatic advection of slope by the flow.

We notice that the sum of DIAB and TILT tends to be on the positive side, although it does oscillate considerably. In particular, positive (negative) values appear to correspond to a higher (lower) slope (Fig. AR1a), though it remains somewhat hard to link visually. The magnitude of the advection term is substantially smaller than DIAB and TILT, while it is more comparable to that of DIAB+TILT but still visibly weaker. As we deem it an interesting result in itself, we have decided to comment upon it in the revised manuscript, including the following lines on line 135 of the original manuscript:

"We also notice that the sum of DIAB and TILT tends to be on the positive side (not shown), with positive (negative) values appearing to correspond to a higher (lower) slope. While the advection term is substantially weaker than DIAB and TILT (not shown), its magnitude is comparable to that of DIAB+TILT, which suggests that part of the imbalance between DIAB and TILT is compensated by advection."

However, we have not included DIAB+TILT in the revised manuscript as it does not really add much to Figure 2 and, while it is true the mismatch is expected to be more relevant to changes in slope, here we want to focus on the phasing between DIAB and TILT and the physical mechanisms behind it.

7) Fig.3: This is a crucial figure, but it is also very dense and hard to read. Some separate comments:

It feels more natural to me to put the free troposphere images at the top. I do not see any problem with describing the bottom plots first, Sn. 5.1, then the top plots, Sn 5.2.

Could you add a couple of arrows, perhaps to Figs 3a and 3e, to indicate the direction of flow? Could you add a 1-1 line (or rather a line with slope -1) to indicate the line in phase space where TILT and DIAB exactly compensate and will not contribute to slope

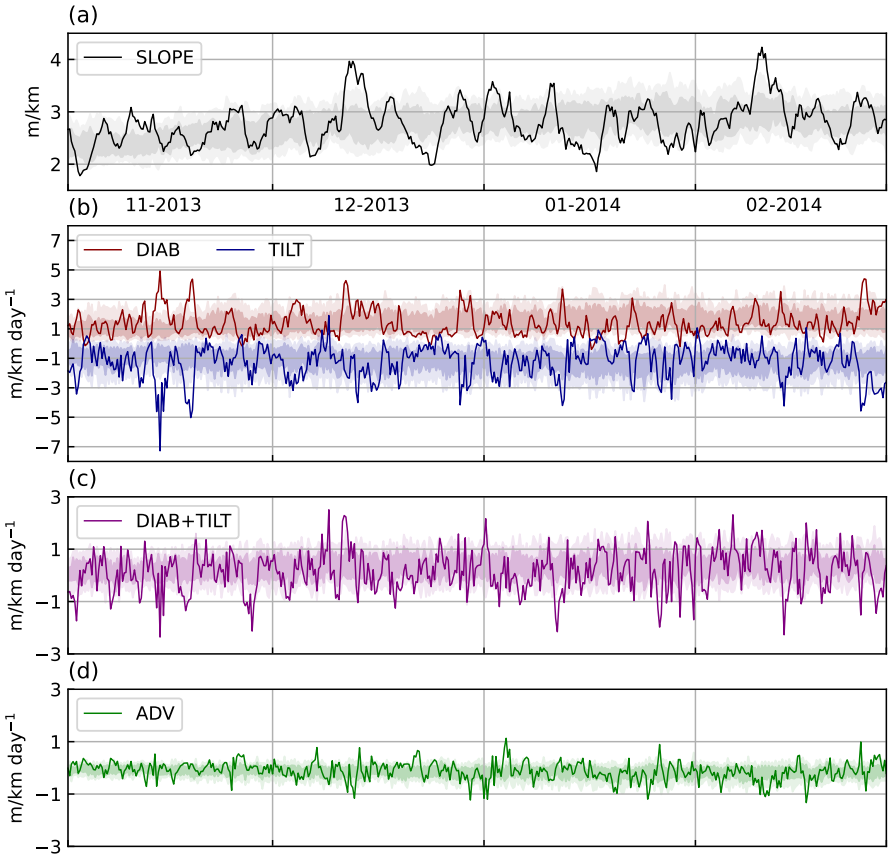

Figure AR1: Time series of free-tropospheric (a) isentropic slope, (b) DIAB and TILT, (c) DIAB+TILT, and (d) ADV, all spatially averaged over the GSE region. Solid lines represent a sample season (NDJF 2013-14) while light (dark) shading represents the interdecile (interquartile) ranges over the climatological period (1979–2017).

tendency. In this sense, phases (i) and (iii) are defined by the maximum distance from this line, and perhaps phases (ii) and (iv) need to be defined as lying exactly on this line.

We have inverted panels so that the free troposphere and near-surface phase

portraits are shown in the upper and lower panels, respectively. We have also added arrows to the leftmost panels to illustrate the direction of the phase space circulation more clearly.

A line indicating where TILT and DIAB compensate is not particularly useful, especially in the near-surface where TILT is considerably stronger than DIAB. At an earlier point, we envisaged using a more objective method to determine the four different stages. In particular, we opted to define them by looking at the value of the kernel-averaged slope: increasing, maximum, decreasing, and minimum. However, the transition from one stage to the next was somewhat harder to follow and consequently required a larger number of panels to be able to link them. That by itself is perhaps indicative that such 'analytical' and elegant definitions do not always correspond to the most relevant points in the phase space.

To summarise our results efficiently and concisely, we focused on the existing sets of four points in the phase space, whose selection aims to enhance the visualisation of the transitions between distinct stages.

8) l.176-179: I found this a very confusing bit. Are you describing your phase space portraits or are you actually speculating on mechanisms? What do you mean by "driving": a description of what happens in the figures, or some causal mechanism? If a physical mechanism, you would need to be more specific: what processes are actually happening? If describing the figure, perhaps use a different word to "driving". Perhaps "leading"?

Here we describe the phase portraits and highlight one aspect that appears counter-intuitive at first, as we observe mean slope to increase while TILT is stronger than DIAB, which would be expected to coincide with a decreasing slope. One way to resolve this apparent contradiction is to re-frame the causal link between slope and TILT, namely that a steeper slope could enhance TILT and DIAB. However, at this stage, this is simply a speculation, as we cannot say much more by looking at the phase portraits alone. Composite analysis indicates that the advection of cold air over the ocean corresponds with a surge in TILT and DIAB, so the advection likely plays a more relevant role in the near-surface. We have expanded the discussion on lines 176-179 of the original manuscript to make this point clearer.

"Given that the net effect of TILT on the slope is negative, it might at first appear counter-intuitive that the near-surface slope increases with TILT. One

possible explanation for this apparent contradiction is that the steepening of mean slope actively contributes to the strengthening of both TILT and DIAB. Using phase space composites, we shed light onto the physical mechanisms behind the phase space circulation (see Section 5)."

9) l.182: "...driving storm development": DIAB is a tendency for isentropic slopes, not for storm development.

We wanted to stress the leading role that DIAB plays in the free troposphere, leading in time both on slope and TILT. The steeper the slope, the more favourable the conditions for storm development, which is captured by TILT. We have rephrased the text as follows: "The interpretation is thus more straightforward, as the increase of slope with DIAB underlines the primary role of DIAB in generating enough slope for the development of baroclinic instabilities, which in turn is associated with an increase in TILT."

10) l.200: Just a personal comment regarding style which, obviously, you may completely ignore. This paper is not easy to follow because of all the shorthand notation: TILT DIAB IADV KOE GSE ENP ENA TCWV CAO, ... Please consider writing this stuff out in the text: there is no gain in shortening it, and it is a real pain to follow the text when using all this shorthand. I know it is a device used by many authors, but I believe that a paper should be as easy as possible to follow; Mike Mcintyre would agree with me on that. Do you really want readers to be trying to decipher what a CAO over the KOE is?

While we agree with the Reviewer's sentiment on this point, we argue that there is a gain in introducing the shorthand notations used in this paper and their removal might make some paragraphs unnecessarily wordy or unclear.

For instance, the use of geographical tags helps us distinguish between the different domains, especially when describing composites: we might be referring to the Eastern North Pacific both in KOE and ENP composites. Hence we decided to retain GSE, KOE, ENA and ENP, which are presented in Table 1 so that it should be easier to find their definition.

As for CAO and TCWV, they are quite common abbreviations. In particular, TCWV is the shorthand notation used in ECMWF documentation, so it is obvious we are using this specific abbreviation.

Finally, DIAB and TILT are easier to refer to compared to diabatic and tilting tendencies, especially when we mention increases/decreases of such tendencies.

11) l.204: "TILT follows the advancing cold air front, while DIAB intensifies further upstream.": It really would help if you explained these things a bit more specifically: why/how does TILT "follow" the advancing cold air? It is often not clear whether you are trying to describe the plots or to explain the plots. Could a schematic help?

Here we were describing the spatial distribution of TILT and DIAB in the composites and, specifically, referring to the fact that at this stage tilting tendencies (TILT) strengthen in correspondence with the advancement of cold air masses over the ocean, which is consistent with the circulation associated with geopotential anomalies. To clarify this point we have rephrased the text as follows: "The spatial distribution of strong TILT follows the advancing cold air front, while DIAB intensifies upstream to the west of peaks in TILT."

12) And I remain somewhat puzzled: if "TILT follows the advancing cold air front" would this then not be manifested in the IADV term? I probably misinterpret the physical processes underlying these two terms, but it surely is an indication that it is not obvious what you are actually saying here.

The isentropic advection term (IADV) represents along-flow changes in the slope of an air parcel. In the absence of processes that actively modify the slope (either diabatically or adiabatically), the slope of the parcel would change as it moves into an environment with a different slope. The advection of a cold air mass concurs with physical mechanisms that deform isentropic surfaces, thus TILT, or actually its spatial distribution, can 'follow' the cold front and not be captured by the advection term. In an Eulerian perspective,

the local slope tendency is also affected by the advection of slope (Papritz and Spengler,2015). However, by definition, TILT would still exclude advection and only measure the tilting contribution to the local tendency. To avoid confusion, we have rephrased the text to avoid using the term 'follows': "The spatial distribution of strong TILT trails behind the advancing cold air front, while DIAB intensifies upstream of peaks in TILT (i.e., to their west)."

13) l.218: "... the suppression of cyclonic activity, consistent with a weaker slope.": I am not sure what you are saying here: a weaker slope corresponds to reduced baroclinic growth. This is not obviously the same as saying that a suppressed cyclonic activity is consistent with a weaker slope. What do you mean with "suppression" anyway? Perhaps you simply meant "represent the anomalously low cyclonic activity..."?

The use of the term 'suppression' was perhaps a bit misleading so we have rephrased as suggested.

14) l.245: What is the purpose of the hyphens around "phases" Are they the opposing phases of the Rossby wave or not? (I think they are.)

We have removed the hyphens.

15) l.246: "... constitute a defining feature ..." : I do not know what this means. Can you be more specific? In what sense is it a "defining" feature? With this phrase it certainly doesn't sound like you managed to add physical understanding to the role of diabatic effects in the evolution of the storm track, which is probably underselling the results presented.

As we are able to capture the evolution of a Rossby wave packet across the North Atlantic and Pacific oceans through composites based exclusively on changes in DIAB and TILT, our results suggest that the phasing between

DIAB and TILT, with DIAB leading TILT, characterises the propagation of such Rossby wave packets. Perhaps our use of 'defining' here is not generally intelligible, so we rephrased it as follows to make it more explicit. "These composites are based exclusively on the mean value of DIAB and TILT, which implies that their evolution is inherently linked to how DIAB and TILT co-evolve. Through composites at different stages, we are able to reconstruct the propagation of a Rossby wave across the North Atlantic and Pacific oceans, and the specific phasing between DIAB and TILT, with DIAB leading TILT, appears essential in its propagation."

16) l.262: "... of moisture availability": Moisture availability seems to imply that DIAB is a moisture limited process. Do you have evidence for that, or do you mean something else?

In the same section, we show how peaks in DIAB correspond to an increase in total column water vapour, which suggests that moist diabatic processes underpin a substantial fraction of the diabatic tendency. However, from reanalysis alone, we cannot determine whether moisture is actually a limiting factor, which is a hypothesis that would require sensitivity experiments to validate. We have made it clearer in the revised manuscript that our results are consistent with such a hypothesis, though further work would be needed. Specifically, we rephrased lines 262–264 as follows: "The close relationship that we found between DIAB, precipitation, and TCWV is consistent with the hypothesis that moisture availability plays a crucial role in the evolution of cyclones and is most likely linked to pulses in storm track activity, as suggested by Weijenborg and Spengler (2020). However, sensitivity experiments are needed to validate this hypothesis."

We also added a line at the end of the conclusions, which points more explicitly to possible future work.

17) l.272: "neither": Double negative; it needs to be "either".

We have corrected this in the revised manuscript.

18) l.273: "... actually condition DIAB and TILT": what do you mean with the verb "condition"?

We refer back to the dictionary entry for condition (verb): *have a significant influence on or determine (the manner or outcome of something)*, so here we mean that changes in the near-surface slope lead to changes in DIAB and TILT rather than the other way round.

19) l.275: " driving": "Driving" or "leading" (as in "leading in time")? Driving seems to imply a causal effect; if so, you need to explain how this causality works. The rest of the sentence seems to just describe the figure, but I think you developed a much clearer physical picture of why DIAB leads TILT in this case.

Here we meant to briefly summarise the main results for the free troposphere, namely that phase portraits indicate that the phasing between DIAB and TILT suggests the former leads on the latter. By itself, this result would not help infer any causality, which arguably would make the use of the term 'driving' somewhat speculative at this stage. However, when we inspect composites in the phase space, we are able to visualise the typical structure of the atmospheric flow at different points in the phase space. These composites suggest that the initial increase in DIAB occurs primarily to the south/southwest of the cyclonic anomalies that will then deepen and become part of a wave packet stretching along the oceanic basins. The diabatic generation of slope on the downstream side of these cyclonic anomalies contributes to the increase in mean slope which then fuels their further development and triggers TILT as these cyclones consume more and more of the excess baroclinicity/slope.

We agree the original wording was not as clear as it could have been and we have rephrased the entire paragraph in light of this and further comments below, also by the second Reviewer.

20) l.279-281 "... suggesting that DIAB in the free troposphere is a distinctive aspect of the evolution of storm tracks, while the development and progression of anomalies in the composites for the near-surface are more contingent to the specific spatial domain

considered.": There are many words here, but it is not obvious what is actually said. Most people would agree, without any hesitation, that diabatic effects play an important part in the evolution of storm tracks (I am not sure what a "distinct aspect" is –distinct from what?). I also do not understand the final part of this very long sentence. What are you trying to say?

We acknowledge this sentence was poorly phrased. We have rephrased it to link it back to the discussion already presented in Section 5.2.

"In particular, the evolution of the anomalous circulation (Z1000 and Z500) across the different stages identified in the phase portraits is reminiscent of a Rossby wave packet propagating over the North Atlantic and Pacific Ocean basins. As composites are based exclusively on changes in DIAB and TILT, the specific phasing between DIAB and TILT appears thus an essential feature of these Rossby wave packets."

21) To conclude, after reading this, it is not obvious what the main take-home message is of the paper. I think it is the distinct dynamics between the two altitudes. My take on it is that TILT and DIAB mostly compensate on average, but at lower levels TILT leads DIAB due to the low level cold air advection being followed by sensible heat fluxes, while at higher levels DIAB leads TILT because the forced latent heat release produces some counteracting secondary circulation. Perhaps I misread the gist of the paper, but I think that it should be more clearly laid out what this take-home message is.

We have partly rephrased and expanded the concluding section, hopefully laying out more clearly the main results of our study, which include the conclusions already mentioned by the Reviewer.