Reviewer #2 (Formal Review for Authors (shown to authors)):

This Perspective concerns the response of atmospheric circulation to climate change. When I first saw the title, abstract and first author I did a double-take, since a very similar-sounding Perspective had just been published in Frontiers in Climate led by the same first author: "Regional climate change: consensus, discrepancies, and ways forward" (doi: 10.3389/fclim.2024.1391634). Apart from one exception in addition to the first author, the co-author teams on the two papers are completely different. And despite the similar-sounding topic, the perspective taken in the two papers is also completely different. In the Frontiers paper, the perspective might be characterized as bottom-up, i.e. beginning at the regional scale, trying to make sense of the observed changes, and focussing on discrepancies between observations and climate models. In this paper, the perspective might be characterized as top-down, i.e. beginning with the predictions from climate models, and assessing the extent to which they are confirmed or refuted by observations. The common ground between the two papers lies in the role of atmospheric circulation in the regional changes discussed in the Frontiers paper, which is, not surprisingly, widespread.

I can definitely see value in the present paper, but it needs to be made clear that it is written by theorists for other theorists. For example, as is acknowledged on line 448, there is a heavy emphasis on the zonal mean. That is because theories are much simpler for the global mean than for regional aspects of climate change, and the theorist has the advantage of being able to choose the observable that matches their theory, in contrast to the practitioner who needs to find a theory that matches a given observable. Yet nobody lives in the zonal mean, and especially in the Northern Hemisphere, the zonal mean is a very poor guide to the behaviour in a given region. Since AGU Advances targets broad audiences, it would be helpful to the non-specialist reader for the paper to make this perspective explicit, and compare it with the perspective in the Frontiers paper. I am not arguing for one over the other; both perspectives clearly have their value.

Thank you for the valuable comments which significantly improved the manuscript. Our responses to your comments are presented in bold. The line numbers referenced in our response correspond to those in the revised manuscript.

We view our paper as written by theorists for the broader AGU community. We think it is important for the AGU community to know the state of the science as regards emerging atmospheric circulation signals, including our understanding of the mechanisms. A lot has changed since previous assessments (IPCC 2021, Shepherd 2014). We now make this perspective explicit in the Introduction and put more emphasis on regional signals in the manuscript. In particular, we note that 11 out of the 18 signals listed in Table 1 are regional signals. We also added discussion of regional mechanisms (stationary wave responses) to section 3 (see lines 179-197).

We added a reference to the Shaw et al. (2024) Frontiers paper and made it clear what the distinction between that paper and this one is. Here the emphasis is on the emerging

atmospheric circulation signals, which are part of a growing list of regional climate change signals discussed in the Frontiers paper. In the revised paper we discuss signals that have been attributed to human activities in more detail (see lines 85-121) and the mechanisms related to those signals (see lines 123-211). Those aspects were not covered in the Frontiers paper.

It also needs to be made clear that the definition of "detection" used here seems quite liberal, and certainly more liberal than is used for the classical thermodynamic aspects of climate change. On lines 70-71 it is defined as "a statistically significant linear trend over the satellite era or longer". I think that is perfectly fine, but I suspect that the significance tests do not allow for multi-decadal variability, and probably do not account for multiple testing. Moreover, it is well established that for improbable hypotheses (which would apply to any case where the observed trend disagrees with a consensus model prediction), the p-value is a massive underestimate of the probability of the null hypothesis being true. For all these reasons, it seems quite possible that many of the statistically significant observed circulation trends could have arisen from internal variability, as is indeed acknowledged within the paper. Again I feel this needs to be made clear at the outset so as not to mislead the reader.

Thank you for the suggestion. Based on the feedback from both reviewers we revised the paper to define signals as statistically significant long-term trends over the satellite era or longer. We revised Table 1 to clearly indicate which signals have been detected (based on signal-to-noise) and attributed to human activities based on definitions from the IPCC (see Table 1 caption). We also added text to Section 2 to highlight circulation signals that have been attributed to human activities (see lines 85-121).

Thus, I am happy to recommend publication of this paper, provided these concerns are addressed and it is made clear that whilst this paper may be relevant to many aspects of regional climate change, it is not about regional climate change.

As we mentioned above the paper has been thoroughly revised following the reviewer comments. We note in the text that 11 out of the 18 signals listed in Table 1 are regional signals. In addition many circulation signals in the Southern Hemisphere are zonally-symmetric leading to similar impacts across longitudinal regions. In the revised paper we make this clear and also discuss mechanisms relevant to regional signals in section 3 (see lines 179-197).

I also have to say that I find a more than typical emphasis on the work of the first author, as if it is accepted fact. This is combined with frequent use of the royal "we", where the authors appear to be speaking on behalf of the scientific community, while promoting their own work. I would urge more objective wording.

The manuscript has been thoroughly revised and the references have been expanded significantly. We removed several instances of “we”. The remaining instances are in the Introduction and refer to the plan for the content of the paper.

Detailed comments:

Line 22-23: The wording of this Key Point, with the "While", suggests that the second clause contradicts the first clause. I feel that is misleading. Assuming that the "more uncertain" is a comparison with thermodynamic changes --- although this is not explicitly stated, and needs to be --- for the most part the degree of alignment of theory, climate models and observations remains qualitatively different (recognizing that the distinction between thermodynamic and dynamic can sometimes be blurry). This is readily seen by looking at the high confidence statements in the SPM of the last IPCC WGI report. Acknowledging this does not undermine the paper in any way, and in fact strengthens the second and third Key Points. A simple fix is just to remove "thought to be" (also on line 61).

The key points were revised (see lines 20-25).

Line 38: The sentence starts with "Regional climate change signals", but most of the signals discussed in the paper are for zonal-mean quantities. You should therefore drop the word "Regional" here, since it is misleading: nobody lives in the zonal mean. In any case, you say "in many regions" later in the sentence.

We revised the manuscript to add more discussion of regional circulation changes. 11 out of the 18 signals listed in Table 1 are regional signals and in addition Figure 1 shows clear regional signals. While the theoretical explanations to date have mostly focused on the zonal-mean there is some theoretical work on stationary waves, which we now cite and review (Hoskins & Woollings 2015, Wills et al. , 2019). We added text to discuss mechanisms related to regional climate change signals (see lines 179-197) in Table 1. We also discuss the importance of building up regional understanding (see lines 318-326).

Line 58: Is there such a consensus on weakening of the Hadley circulation? See Lionello et al. (2024, https://nyaspubs.onlinelibrary.wiley.com/doi/10.1111/nyas.15114).

Yes, there is a consensus on the weakening of the Hadley circulation, at least in the Northern Hemisphere. Chemke & Yuval (2023) attributed the weakening signal to human influence (see lines 117-119)

Line 102: You should make clear that the asterisks refer to Table 1, as this was not immediately obvious to me.

We revised Table 1 and removed the asterisks.

Figure 1: The units should be indicated explicitly, perhaps in the caption. Also the stippling is not particularly clear, e.g. many white regions are stippled, and there does not appear to be stippling in the regions with the largest changes.

We fixed the units and stippling in Figure 1.

Box 1: I find this Box to be a rather uncompelling example of what the authors are advocating for in this piece. The premise of the piece is that circulation signals are now emerging from the noise, which merit attention by large-scale atmospheric dynamicists. By "emerging from the noise" the authors mean a statistically significant trend. In Box 1, they suggest that an apparent recent "de-emergence" of the impact of ozone hole recovery on the SAM is such a case. But if one removes the last three data points from the figure, then the observed record seems perfectly consistent with the earlier fit. I can hardly imagine that any significance test could reject the hypothesis that the anomalously low values seen in the last three years occurred by chance (especially if one accounts for the multiple-testing problem, which is rarely done). I would suggest removing Box 1 entirely since I cannot see what purpose it is serving, and it seems inconsistent with the approach taken in the rest of the piece.

We removed Box 1 and expanded section 2 to discuss atmospheric circulation signals attributed to human activities (see lines 85-121), including those for ozone depletion. The discussion on the de-emergence of the ozone recovery signal in circulation was mostly removed, and we agree that it is likely sensitivity to end points rather than a robust result that the emerging signal disappears when taking more recent years into account. Rather, this result emphasizes the role of internal variability for detecting the emergence of signals, in particular for such short periods with changes in forcing. We emphasize the role of end point sensitivity now in the text.

Figure Box 1: The updated piecewise-linear fit (the red dashed line) should anyway be performed for the entire period, not just from 2000 onwards, because it is physically inconsistent to have sequential linear trends that are discontinuous (think of the famous "climate skeptics' view of global warming", which is a staircase of zero-trend lines).

Thank you for your feedback. Indeed, the way the lines in Fig. 2 were displayed seemed to indicate that the updated trends were calculated sequentially. However, the updated trend lines were performed with the same continuous piecewise linear fit method. In the updated version of Fig. 2, we now include the updated trend line for both periods to make this more clear. Moreover, the Figure has been updated to include the most recent season of 2023/24.

Lines 146-147: This sentence "Many dynamical mechanisms have been proposed to explain the robust circulation responses predicted by generations of climate models (Shaw, 2019)" seems much too sweeping. Shaw (2019) considered only the changes in zonal mean mid-latitude circulation. Yet this wording (and similar wording earlier in the paper) might give the impression that generations of climate models have been consistently predicting all kinds of circulation changes. We must not forget that the IPCC AR4 attributed the observed NAO trend (up to 2000) to climate change, and had to walk that attribution back in subsequent reports. That should give any dynamicist pause, and shows that multi-decadal variability is grossly underestimated in statistical significance tests. Much gets swept under the carpet, especially in the NH, when one looks at the zonal mean. Indeed Lionello et al. (2024, earlier citation) claim that the Hadley cell has contracted rather than expanded over many land regions of the NH (which is where we care about the Hadley cell width). I would urge the authors to not be so tendentious in their wording, to ensure that their statements are not misleading in their scope.

Thank you for your feedback. We added a reference to Wills et al. (2019) who discussed robust stationary wave responses, which affect regional climate. We added more discussion of stationary wave mechanisms related to the signals in Table 1 to the revised manuscript (see lines 179-197).

The reviewer's cautionary tale about the wintertime NAO is a good reminder. An updated analysis of the wintertime NAO trend shows the signal is statistically significant in observational products but coupled models exhibit a discrepancy: they do not capture the signal (see Fig. 3c,d in Blackport & Fyfe 2022). Thus, while there is clearly a signal that has emerged from the noise using observational products, attributing the driver to human influence using single forcing simulations is not possible because the models do not capture the signal with fidelity. The revised manuscript highlights signals that have been attributed to human activities using single forcing experiments. Ultimately it seems that after ~20 years the detection and attribution exercise has advanced considerably thanks in large part to large ensembles that can be used to quantify noise and single forcing simulations that can quantify human influence.

Line 164: It is unclear what "the poleward shift" refers to here. I first assumed the subtropical jet, but the rest of the sentence suggests it refers instead to the extratropical (eddy-driven) jet.

The text was removed as part of the revision.

Line 519: You need to specify here that you are talking about global models, since in limited domains they have existed for some time.

We added "global" to clarify we are referring to global models (see line 363).

Table 1: Two of the rows are missing some entries.

We meant for the entries to be repeated from above. We added the entries from above to avoid confusion.