Peer Review History for 2024AV001297

Reviewer #1

In this manuscript, the authors provide an overview of recent atmospheric circulation changes. They discuss some examples in-depth, possible mechanisms, and known disagreements between models and observations. Providing such a summary is valuable since many in the climate science community could benefit from it. However, my main comments are first that the manuscript is currently written so that many phenomena are not discussed in depth. Second, the manuscript focuses too much on the mechanisms underlying the phenomena (although also here, the discussion remains flat in some cases), which leaves the reader not with the broad perspective such work could provide. Third, the mixing of "detected signals" vs long-term trends provides a false impression that should be fixed.

Major comments:

1. The authors re-define the term detection here as a long-term trend, which is not the canonical definition used in the literature (IPCC and the many papers on the subject). Detection is when a given signal (e.g., long-term trend) exceeds the noise. There are different ways to conduct detection analysis, and this is heavily discussed in the IPCC report. First, this would only confuse the general reader that you are suggesting that signals are detected, and such an error impression could propagate to wrong citations of this manuscript in the future. Second, given that you are not covering only signals that have been detected (in a formal manner), i.e., that they emerged from the background noise, in the long list of phenomena summarised in Table 1 and mentioned in the text, there could be phenomena that are simply due to internal variability and are not part of the forced response (i.e., they are not projected to continue in the coming decades). Because of these two reasons above, I suggest not to redefine detection and, indeed, discuss in more depth detected signals (that have emerged) or signals that show similar responses to what we predict.

2. Throughout the manuscript, the authors choose to focus only on a few examples of circulation changes, although this should be a perspective paper. The authors choose to put more weight on the mechanism part but do not go in-depth into describing all detected signals and their mechanisms. Specific examples are given below.

2a. In section 2 of detected signals, the authors go in-depth into the impact of ozone forcing on the flow but do not mention the other circulation changes in recent decades. Again, given that this is a perspective, the reader would like to know of all robust detected changes, not only the ones in Southern Hemisphere summer due to ozone. For example, changes in the Hadley cell strength in the Northern Hemisphere, storm tracks and jet stream during summer in the Northern Hemisphere, changes in storm tracks during winter in the Southern Hemisphere, etc. These should not just be mentioned but discussed in some depth.

2b. Related to the above, the discussion in Section 2 is cumbersome. You start by talking about the impact of ozone, then move to briefly mention Table 1, and then go back to the ozone story. I would again provide detailed information on each metric that has been detected (signal-to-noise ratio-wise).

2c. In section 3.1.1 on the upper tropospheric warming in the tropics, the authors too heavily refer to papers by the first author of this manuscript, while other studies should be discussed in more detail, including the impact of the upper warming on the Hadley cell width (Lu 2007 paper and the Held 2000 two-layer mechanism), Hadley cell strength (Chemke and Polvani 2019,2021 papers on the impact of static stability vs latent heating), winter North Atlantic storm tracks, etc.

2d. In section 3.1.3 on Arctic warming, the impact of high latitude warming on storm tracks during summer should be mentioned. Coumou et al. 2015 is a good starting point, as well as Chang et al., 2016, and the recently detected/attributed signal found in Chemke and Coumou 2024.

2e. In section 3.1.4, ocean processes were also found to affect the Hadley cell (e.g., Wang et al., 2018, Chemke 2021), and Southern Hemisphere storm tracks (e.g., Grise and Polavni 2014, Chemke 2022)

2f. Another impact of aerosol forcing is on the summer storms in the Northern Hemisphere found in Chemke and Coumou 2024.

2g. In section 4.1, more discussion is needed on each metric instead of just naming the discrepancies. These are interesting stories, and we could learn a lot from them. These are more relevant in such a perspective paper than the underlying mechanisms, in my opinion. Another example that is missing here is the Hadley cell strength. Also, in line 317, the first paper that identified the discrepancy between modeled and reanalysis trend in summer storms in Cuomou 2015, which was recently resolved in Chemke and Coumou 2024 (should also be added to Table 1). Lastly, it is crucial to discuss our limitation in observed flow changes, as we heavily rely on reanalyses that, similar to models, could also be biased.

3. I could not understand why aerosol forcing is an important chapter in this perspective. In a similar way, you could have done a chapter for each forcing agent separately. The aerosol forcing, while interesting, is actually the one that would likely have a smaller effect in the future. My suggestion is to remove this part.

4. The figures in the manuscript are referred to without any explanation. Please explain every symbol in each figure for the reader to understand the figure and your reference.

5. In addition to the above, there are several missing references. First, in the discussion on ozone's impact on the circulation in the Southern Hemisphere, the paper by Polvani et al., 2010 should be cited. Second, line 120 is missing a reference. Third, the discussion around line 121 should include the discussion in Barnes et al., 2014 on the delayed GHG response. Fourth, in several cases, when discussing mechanisms underlying flow changes, the authors refer to Shaw 2019. Another broader related paper that provides an overview of the mechanisms underlying large-scale flow changes in the tropics and extratropics is Vallis et al., 2015 (QJRMS).

Table 1. The Hadley cell intensification is most entirely in the Northern Hemisphere, not in both hemispheres.

Minor comments:

1. line 98. Thus -> Thus,

2. line 350: The general reader would benefit from it if you would refer to this in its canonical way: attribution analysis

Reviewer #2

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.

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.

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.

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.

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).

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.

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).

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

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.

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.

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).

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.

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.

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

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

Peer Review History for 2024AV001297R

Reviewer #1

The authors addressed all of my concerns. They revised the manuscript, which now provides a better perspective of our current knowledge. I list only a few minor comments below.

0. Not sure what the policy is in AGU advances, but I would remove references for manuscripts that have not undergone a peer review process.

1. line 31: "reducing uncertainties"; which uncertainties? Sounds vague currently.

2. lines 57-58: similar to the NH, in the SH, the storm track response is seasonally dependent, and you do not see a strengthening in both seasons.

3. line 93: remove "where"

4. line 112: I believe the reference here is Kang et al. 2024b, and not Kang et al. 2024

5. Table 1 under "Extratropical cyclone activity" the reference should be for Kang et al. 2024b, and not Kang et al. 2024

Reviewer #2

The paper has been substantially revised in response to the two reviews, which raised a number of common concerns with the original version. The paper now reads much better and provides a more balanced and useful perspective. I am happy to recommend publication, although I noted a number of typographical errors in Table 1 which should be corrected:

1. The entry "Extratropical cyclone activity" does not indicate the nature of the trend (i.e. increase or decrease)

2. The 11th row (the one citing Shaw et al. (2022) and Cox et al. (2024)) is blank for the first two columns

3. The row on weakening of upward vertical motion lists "upward motion" as the region

4. The penultimate row is incomplete and anyway seems like a repeat of the row three lines earlier

Peer Review History for 2024AV001297RR

[Version was not sent to review.]