Review of Kang and Chun (2021) Contributions of equatorial planetary waves and small-scale convective gravity waves to the 2019/20 QBO disruption

A nice follow-up of the author's 2020 ACP paper about the 2015/16 QBO disruption (KCG20), the submitted manuscript has a very similar structure and the same analysis tools, making a comparison of both events easy to the reader. The manuscript is high quality and I'd even say almost ready for publication.

I only have a couple of general remarks:

- In some places the discussion comparing the 2019/20 to the 2015/16 event can be extended a little, referencing more here and there (see individual comments below).
- Although the authors follow a similar structure to KCG20 and the main differences between both QBO disruptions are summarized in section 4, I think it would be very helpful to add a short subsection at the very end of the results with a discussion about the most striking differences between both QBO disruptions (see the last individual comment for a potential figure). I think this would involve very little restructuring, the figure shouldn't be a lot of work either, and the readers would benefit a lot from having all this information concentrated in one subsection (compared to being spread throughout the whole paper).

–> This should be viewed as just a suggestion, I leave the decision to the authors since the paper is fine anyways in the current format.

Minor and technical corrections:

Title: remove 'planetary' since your equatorial wave filters include shorter wavelengths than w1-3.

p.1, l.8: 'that occurred in 2015/16' –> of 2015/16

p.1, l.11: I suggest starting a new paragraph after '… reanalysis data'

p.2, l.55: 'anomalously AND sufficiently strong...'

p.4, l.95-105: very nicely condensed and pointing to KCG20 where needed, good job.

p.4, l.108-109: just to be sure, is this a difference/improvement from KCG20, or still the same?

p.5, l.128: 'shear anomaly and westerly anomaly…' –> do you mean easterly? The red curve is to the left of the climatology July-December at 100-150 hPa

p.5, l.129: 'and January 2020, respectively.' –> remove for clarity + see previous comment.

p.5, l.133-134: 'Close to the equator…' –> it has the lightest shade of green there in Fig 2a, so calling this 'anomalously strong' might be a bit inflated? Or do the authors mean 20-30 hPa?

p.5, l.135-140: I'd be very interested to see this figure for October, where the westerly jet (and the shear zone) at ~30hPa is still strong –> perhaps a good addition to the supplement, to see more detail about the wave forcings in the period leading to the disruption.

Also, the authors note that overall Kelvin wave activity propagating from the troposphere is above average, but still less than the 2015/16 event. Maybe would be worth adding here that there was no strong ENSO this time.

p.6, l.162-163: ‘This implies that the ADVz can help QBO disruption…’ –> Could the authors elaborate on how this would work? At first, it is a bit counter-intuitive as in principle it should act to increase the WQBO period length, right?

p.6, l.170-172: Towards the end of this paragraph, since the authors compare to the 2015/16 QBO disruption, it would be nice to have some previous references added and perhaps shortly discussed there.

p.6, l.174-180: A short comparison of the dominance (or not) of MRG and IG, to the 2015/16 event would be helpful in this paragraph.

Also as in the previous comment, a couple of references can be added in the discussion.

--> A different option: instead of the two previous comments, include an additional paragraph devoted to pointing out the most important differences to 2015/16, among all wave types.

p.6, l.177: ‘… reversing the sign of the zonal WIND in the later stages.’

p.7, l.202: ‘… the meridional component becomes weaker,…’ ?

Fig. 6c: remove 'latitude' at the bottom, replace with m/s. Also, I recommend that the vertical scale of 6c, and the latitude scale of 6b, are forced to match those of 6a for consistency.

p.9, l.255-260: Maybe add a small comment whether the boxed regions are similar to those of the 2015/16 disruption, since the box in the NH was not included in your previous paper whereas the SH boxes in both studies are close to each other.

(optional) If Fig. 9 doesn't show anything remarkable, I'd move it to the supplement.

p.10, l.298-299: specify / make clearer in this sentence: the IG EPFz 70hPa for the 15/16 case was significantly larger than climatology and the 19/20 case.

p.11, l.356-357: You need to mention ENSO here, as the much more enhanced spectrum in 2015/16 can be partly attributed to the strong el Nino then.

p.12, l.346-347: This is a bit speculative, I think a discussion about lack of el Nino conditions (vs strong El Nino in 2015/16) would help the argument that by elimination global warming may have helped the formation of westerly anomalies, but as you mention later, this needs further research and is beyond the scope of your study.

p.13, l.388-389: One thing that could nicely discern this is doing an additional figure, same as Fig. 4 but for 20-30 hPa, and comparing both 15/16 and 19/20 events.

I suggest at least adding such a figure into the supplement, but I'd even support making a small subsection in the main manuscript about it - however I leave it up to the authors.