EGUsphere, author comment AC2
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Reply on RC2

Bing Cao et al.

Author comment on "Equatorial waves resolved by balloon-borne Global Navigation Satellite System radio occultation in the Strateole-2 campaign" by Bing Cao et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-381-AC2, 2022

[I am Editor for this paper. One of the referees who originally agreed to provide a report on this paper has failed to respond. Therefore I am acting as a referee myself.]

This is an interesting paper setting out algorithmic details of extracting temperature information from balloon-based radio occultation during one of the preliminary STRATEOLE balloon flights. It is clear that when the main STRATEOLE campaigns are under way, with radio occultation available from many balloons on long-duration flights, this technique will provide much useful new information on temperature profiles in the TTL. This paper provides much useful preliminary information.

My only reservation about the paper is that it has been submitted as a Research Article, and indeed I as Editor accepted in on that basis, but having looked at the paper in more detail it does seem to me to be more a "Measurement Report' than a 'Research Article' -- see the description of different paper types at:

https://www.atmospheric-chemistry-and-physics.net/about/manuscript_types.html

In raising this I am not trying to obstruct publication of the paper. But I do ask the authors to consider whether the paper would be more appropriately published as a measurement report. (As far as I am aware, all the standard bibliographic databases make no distinction between the different manuscript types and I believe that the category of 'Measurement Report' was introduced to acknowledge the value of this kind of contribution and to avoid publication delays where, e.g., referees or Editors were reluctant to recommend publication of a potentially very valuable paper because of confusion about whether it contained sufficient new scientific results to the publishable.)

We understand the arguments for suggesting a Measurement Report, however we believe the results are new and of high potential interest, and the first reviewer showed no reluctance in recommending publication. We included an additional statement on the main results in the abstract at line 13:

"The results illustrate the difference in Kelvin wave period (20 vs 16 days) in the Lagrangian versus ground-fixed reference, and as much as 20% difference in amplitude compared to COSMIC-2, both of which impact estimates of momentum flux."

The fact that the equatorial wave results come from a new type of observation (balloon-

borne radio occultation) and clearly demonstrate what new information this type of data can bring should be more than sufficient to qualify as new scientific results.

l12: 'A short dataset from the extra Galileo and GLONASS ... planned follow-on' -- the only mention of this in the text is almost a repeat of this sentence -- l588-589 -- saying that no details are given in this paper. It doesn't really seem appropriate to mention this point in the abstract.

Line 505-510: We provided more details on the Galileo, GLONASS, and Beidou observations, to better emphasize the significance of doubling the number of profiles. This increased density of sampling provided by the additional constellations is a key factor for distinguishing the advantages of BRO relative to the quasi-random sampling of spaceborne RO.

These capabilities will be further enriched by recovering data of multiple constellations, including Galileo, GLONASS, and Beidou. 24 hours of data were retrieved from the Galileo and GLONASS constellations sufficient to demonstrate quality comparable to GPS. 12 hours of data were retrieved from Beidou. Due to data transmission limitations, however, it was not possible to transmit the additional constellation datasets from the entire flight. The data were sufficient to verify the number of occultations from each constellation. On average, there were ~2 profiles retrieved per hour from the GPS constellation. Including data from all GNSS constellations more than doubles the number of profiles.

l11-16: There is really no concrete information on results here in the abstract. That makes the status of 'Research Article' difficult to justify.

We included an additional statement on the main results in the abstract at line 13:

“The results illustrate the difference in Kelvin wave period (20 vs 16 days) in the Lagrangian versus ground-fixed reference, and as much as 20% difference in amplitude compared to COSMIC-2, both of which impact estimates of momentum flux.”

l35-52: This is quite a long introduction that could be shorted significantly simply to say, with appropriate references, that the QBO is important and but aspects of it are poorly understood, one being the quantitative role of different waves in driving the QBO winds changes in the lower stratosphere.

We removed Lines 42-44, 48-53.

l41: 'uncertainty in period and amplitude'?

Line 43 changed to:

leading to uncertainty in the evolution of the QBO period and amplitude in a changing climate.

l43: 'somewhat muted in forecast models compared to observations' -- I think that you are really talking about free-running global climate models here -- not models that start from an observed state. I think that most readers will interpret forecast models as the latter.

We removed this statement when we shortened the introduction.

l76-78: 'For example, the difference ... 40 per cent difference in the force in the QBO' -- this seems to be very specific quantitative statement and I wondered what calculation exactly had been done to arrive at it.

Line 74: Changed to

"For example, in QBO shear zones wave vertical wavelengths will shrink wherever the wind approaches the wave phase speed. If the wave can survive to higher altitude where density is lower and the wave has shorter vertical wavelength, the wave can impart a significantly stronger force on the QBO flow (Vincent and Alexander, 2021)."

l83: 'tangent point ... drifts' -- I suppose that this is standard terminology but it is potentially a little confusing since you have also referred to the 'drift' of the balloon itself. Perhaps consider later in the paper whenever 'drift' is used whether it is always absolutely clear -- to a non-expert reader -- whether it refers to the position of the balloon or the position of the tangent point.

l4 and throughout text:

To avoid the potential confusion between the balloon drifting and the tangent point drifting, we choose to use 'float' for the balloon and keep 'drift' solely for the tangent point.

l87-88: I assume, on the basis of the previous statement re 500km, that short here implies say 1000 km or less and larger-scale implies say a few 1000 km.

It would be useful to have some indication.

Line 85 changed to:

The slanted nature of the profiles also presents difficulties in resolving wave properties in the presence of horizontal scale variability shorter than about 1000 km, without explicitly considering the drift.

l120: Just to be clear -- the implication here is that this technique works ONLY for quasi-hydrostatic waves. (Any comment about that re implications for estimating QBO momentum fluxes.)

Tropical waves typically have a broad variety of periods and scales, some are hydrostatic and some are not. Those waves collectively contribute to the forcing QBO. This study focuses on waves with longer periods and larger scale, which are part of the wave spectrum.

l182-186: You refer to various intervals when different radio occultation signals were recovered and note that these are indicated by dashed boxes on Figure 1, but there doesn't seem to be any information on which box corresponds to which interval.

L180: A note is added in the text to clarify the boxes and periods

The recovered dataset includes GPS only data from 6 December 2019–22 December 2019 (largest box on the left on Figure 1), 1 January 2020 (box in the middle on Figure 1), 9 January 2021, and 11 January 2021–14 January 2021 (box on
the right on Figure 1).

l185: 'data' is needed only once.

The second one is removed.

l200-201: I suppose that 'epoch' is being used here in a technical sense. What does it mean.

Line 197: changed to:

The final calculation was solved for the coordinates of the balloon antenna at each time sample.

l236: 'loess' -- I suggest capitalising this -- even if it is not capitalised in program scripts.

Changed to LOESS.

l307: 'consecutive' -- should this be 'regular' (in contrast to 'random')

Line 322: The BRO profiles are not really regular, they are random as well. But they are closely aligned with the balloon position, thus making them continuous. Changed to:

Multiple irregularly spaced but temporally and spatially consecutive profiles form near-parallel transects ~400 km wide along the trajectory.

Figure 4: 'lowest tangent point further from the balloon path' -- I think that this means that for each derived profile the lowest point on the profile corresponds to the horizontal location furthest from the balloon path? Is that correct? Perhaps make slightly more explicit.

Fig. 4: Yes, this interpretation is correct. Changed to:

Red and blue lines denote rising and setting BRO profiles, respectively, that are projections of slanted profiles with the highest tangent point on the balloon path and the lowest point furthest away.

l364: 'BRO profile matches the ERA5 profile well for this case, which we attribute to the fact that they both consider the same tangent point drift' -- on first reading I was slightly confused by 'for this case' because I interpreted the sentence as implying that 'same tangent point drift' applied to this case, but perhaps not to other cases. But in fact my understanding is that construction of ERA5 profiles with the same tangent point drift as BRO is a standard feature of your calculation.

Line 361: Your interpretation is correct. ‘For the case’ we mean the results here only apply to the comparison for the set of profiles. We removed “for this case”.

l372: add 'temperature' -- i.e. 'depress the cold point tropopause temperature by ~1K'?

Added.

l423: You give 'quadrature in zonal wind and temperature' as permitting identification as a Kelvin wave here -- then at l478-479 you essentially repeat the same point and cite Andrews et al. Remove repetition?

The second instance is removed.

l494: 'was used in the calculation' -- do you mean in the Sato and Dunkerton calculation?

L489-491, Yes, it refers to the calculation in Sato and Dunkerton 1997. We reword the sentence to make it clear.

This is near the values deduced from radiosonde data in Sato and Dunkerton (1997, Fig. (7)) during the westerly phase of the QBO, which was about 0.1–0.2 mPa at around 20 km.

l499-504: This statement seems to be more appropriate for the concluding Section.

We leave it here because this is where the intrinsic frequency of the observations are described.

l594-604: As my comment above re l35-52, there is a certain amount of general motivation here that is pretty much the same at the end of the paper as it was at the beginning -- i.e. the results in the paper, apart from showing temperature retrieval from BRO is possible, have not really focused this motivation.

Line 583-587: added text to focus on the results more:

However, the BRO observations with slightly higher vertical resolution and denser sampling retrieved as much as 20% higher amplitude temperature variation associated with the wave. The BRO observations present the advantage that the waves are naturally measured in the intrinsic reference frame. The BRO profiles show an intrinsic period of 20 days for the Kelvin wave as compared to 16 day period for the Kelvin wave in the ground-referenced COSMIC-2 dataset. Both the difference in amplitude and the difference between periods determined from the BRO vs COSMIC-2 datasets affect the calculation of momentum flux.