Please note that the line numbers referenced in this response refer to **the version of the manuscript with tracked changes**, as they may differ from the ones in the clean version.

Response to Referee #2

Minor Comments

(1) P.3 L.65 "This paper uses the second version of the ATSR-AATSR"

Readers may be interested in the characteristics of this data set when compared with other observations such as ISCCP or MODIS. If the authors could comment on this, that will be helpful.

The ATSR-AATSR data set is popular in Europe as it's developed and operated by ESA, but we agree that adding its characteristics will be helpful. We've added a sentence in L.87-89.

(2) P.5 L.109 "... it is fixed to be 500m"

It would be helpful to readers if the authors explain why the LCL is fixed to be 500m, rather than being estimated.

LCL is estimated by a simple approximation of dew point and itself. First of all, we have to emphasise that the difference between EIS and LTS is dominated by the variability in lapse rate rather than that in LCL or the geopotential height of 700 hPa (Wood and Bretherton, 2006). Based on that we can do the simplification in calculating LCL. An additional clarification is put in L.130-134.

(3) P.5 Figure 2 caption "low cloud fraction (kappa) from 2003 to 2014"

This appears inconsistent with the statement in L.70, that low cloud fraction data for the period from Jan 2003 to Dec 2011 is analyzed.

It was a typo and we've changed it to 2011 now.

(4) P.6 L.143 "C_D=0.0015 the drag coefficient"

Readers might wonder why the drag coefficient is assumed to be a constant value of 0.0015. Additional clarification would be helpful.

The drag coefficient C_D is assumed to be constant in this study, as its variability is relatively small and sensitivity to it is not our focus. Previous studies have shown that C_D varies little under moderate wind conditions (<10 m/s) (Vickers, Mahrt, and Andreas, 2013; Kochanski, Koračin, and Dorman, 2006). Our analysis shows that the mean 10 m wind in the studied regions ranges from 1.7 to 7.3 m/s, with extreme values ranging from 0 to 12.2 m/s. Based on this, we initially selected a value of 0.0015 for C_D, referring to Fig 5 in Vickers, Mahrt, and Andreas (2013), which represents the maximum of C_D within the range of extreme values.

However, following your comment, we see that this choice may lead to a debate which we wouldn't expect to. To avoid distraction, we decide to choose a classical and specific method to define C_D. Hence, we now changed C_D to be 0.0012 referring to Large and Pond (1981). Figures 3 and 7 in the manuscript have been updated accordingly (with no significant change), and a clarification has been added to L.173–174.

(5) P.7 Table 1 caption "The dominant contribution to EIS is denoted by a bold font"

It would be helpful to readers if the authors explain in more detail what the dominant contribution means. Does it mean that magnitude of the correlation or the standard deviation at one pressure level is larger than the magnitude at the other level?

It refers to the magnitude of the correlation. We've rephrased the sentence to be "The level with the higher correlation is denoted by bold font" in the caption of Table 1.

(6) P.7 L.158 "explain most of the variability in EIS"
Readers might wonder if this statement is supported by Table 1, because magnitude of the correlation is smaller at 700hPa than at 1000hPa at NP and SI on interannual timescale. Additional clarification would be helpful.
It's a bit misleading to use the word "most" here. We've rephrased the sentence to be "In the higher latitude regions of the NP, NA and SI, variations of θ_{700} are mostly larger than variations in θ_{1000} in both the seasonal and interannual data and explain a large part of the variability in EIS in those regions, particularly on seasonal timescales".

(7) P.7 L.171 "this correlation does not imply causation"
Here the authors argue that the correlation does not imply causation. Does this argument apply to both seasonal cycle and interannual variability, or does it apply to interannual variability only? Additional clarification would be helpful.
I've phrased it in L.203-206.

(8) P.8 Figure 3
I suggest that the authors give definition of the primes for EIS, Q_c, LHF_c, and W_E.
I've added an explanation that "The primes indicate deviations from the mean of the respective regions on the corresponding timescales." in the caption.

(9) P.8 L.187 "a clear relationship between Q_c and omega_700,c"
It would be helpful if the authors write the pressure level at which the Q_c is evaluated. Is it 700hPa?
Yes, it's 700 hPa. It's been added here (L.222).

(10) P.10 L.204 "Upwelling areas are restricted to the coastal regions where the wind-stress curl is large"
In Figure 7(b), there appears to be no upwelling along the equator in the Pacific and the Atlantic. Readers might wonder how the SST cold tongue is maintained. Additional clarification would be helpful.
We cannot show the upwelling along the equator due to the change of sign in the Coriolis parameter as the Ekman pumping velocity in this paper is calculated from wind stress. Referring to equation 7, w_E cannot be properly calculated near the equator where f is approaching zero. Hence, we masked the values in the current Figure 7(b) near the equator but it's not explicit as it is covered by the gridline. Hence, we remove gridlines, extend the masked region to avoid misleading, and state it in the caption of the current Figure 7.

(11) P.10 L.207 "variations in near-surface geopotential gradients are not the primary driver of changes in theta_1000"
It may be a good idea to show scatter plots for variations in geopotential gradients and variations in theta_1000, for both seasonal and interannual timescales, so that readers can better follow the argument.
I've added a new Figure 8 showing the variation relationships between geopotential gradients and θ_{1000} (the left column).

Typos
Thanks for pointing them out. All the typos mentioned here have been corrected.