Response to Reviewer 2

Thank you very much for your time and effort dedicated to our manuscript! With the help of your advices, we have prepared a restructured, more balanced and more readable version of our manuscript. Our responses to your comments are marked in red below.

General Summary: This study utilizes tethersonde data collected during the Tara drift in 2007 to examine characteristics of atmospheric low-level jets over the Arctic Ocean. While the data utilized is reasonably unique, the study doesn’t provide much additional information on Arctic Ocean LLJs than is already available from the few previous studies.

We have now better summarized the new results in the beginning of the Discussion and Conclusions section. These include the low occurrence of LLJs, the properties of LLJs related to baroclinicity (a low core height but a deep jet), the fact that about half of the LLJs were associated with a frontal passage, and the fact that some LLJs are simply due to gusts (which can not be diagnosed on the basis of standard radiosonde soundings).

The data and arguments used to indicate causality is weak and may often be unreliable, as suggested by the fact that more than half of the observed LLJ cases seem to be of unknown origin. The description of the LLJ structure is straightforward and OK, though the lack of confidence in the causality analysis may mean that grouping the LLJ structure into causality categories is meaningless. As mentioned by the authors, the LLJ structure seen in this study is not much different than seen in other studies. However, some additional analyses may help understand the causality better and could at least provide some unique analyses of a few specific cases. It is recommended that some additional analyses be done, and that some of the awkward phrasing that occurs in more than a few spots be corrected.

Rating: Accept only after additional analysis is done and major comments addressed

We have carried out the additional analyses suggested by the Reviewer and addressed all the comments as explained below.

Major Comments

1) This study is weak in its assessment of the causes of the LLJs. For instance, an unclear distinction is made between "frontal" LLJs and "baroclinic" ones. Obviously, frontal zones are baroclinic zones.

We have reconsidered the analyses and discussion on fronts in the manuscript. With a help of additional analyses based on Tara surface observations and time-height cross-sections of the sounding data, we have distinguished many more fronts that on the basis of the ECMWF analyses. We now make it clear that a front itself is not a causal mechanism for LLJ generation, but a front provides a favourable environment for other forcing mechanisms to act, because (a) non-occluded fronts are baroclinic, (b) in case of a cold front, the cold air mass typically penetrates below the warm air mass, building a stably stratified layer in between, which favours the generation of inertial oscillations, and (c) wind in the cold air mass is very often gusty. We have modified Sections 3.4 and 3.5, as well as related text in the Abstract and Discussion and Conclusions.

Numerous studies have documented the presence of LLJs in the reverse horizontal thermal gradients in the warm sector just in advance of surface cold fronts (e.g., Browning and Harrold 1970; other work by Browning; Carlson 1980 (who called it the warm conveyor belt); Hobbs and Persson 1982; Crook 1987; Kotroni et al 1993; Roux et al 1993; Groenaas and Skeie 1999; Nieman et al 2004; Wakimoto and Murphey 2008, among many others). At least some of these should be mentioned.

Thank you for the references! We have added some of them to the manuscript.

Furthermore, the assessment of baroclinity is made using ECMWF analyses. How reliable are these analyses? Were there 1-2x daily synoptic soundings from Tara? If so, were these assimilated into the ECMWF analyses? If not, the ECMWF analyses are primarily dependent on model output, especially in the lowest 1500 m, so it is questionable how realistic the thermal gradient fields produced by these analyses are. ECMWF does not use a snow layer on its sea ice and does not have a realistic sea-ice thickness, so the conductive heat flux will likely be heating the surface too much (hence, the 2 C warm bias in the lowest 400 m found by Jakobson et al 2012). ECMWF reanalyses (also based on the ECMWF model) produce too little supercooled liquid water in clouds during the spring, producing negative biases in the surface longwave radiative fluxes. The combination of errors in fluxes such as these are likely to impact the evolution of the low-level thermal fields as air moves towards Tara in the Central Arctic from the periphery where assimilation data is available, so it is unclear what such erroneous fluxes will do to the horizontal low-level temperature gradients. I would recommend the following: For the cases (days) when several ascent/descent profiles are available, time-height sections of the thermal, moisture, and kinematic fields from the tethersondes (and incorporating any synoptic soundings from Tara available during these times) should be computed. These analyses can be used to provide a better understanding of the synoptic/mesoscale context of the LLJs and their evolution over a time scale of 12 h That is, the temporal evolution of these fields can be used to represent their spatial variability, which is probably a better assumption than relying on the ECMWF analyses. (i.e., if the low-level thermal field changes and a LLJ are present on the warm side of this thermal transition, its likely dynamical cause is more clearly identified.).

No radiosonde soundings were made at Tara, and the tethersonde data have not been assimilated into models. We agree that the ECMWF analyses, as any model analyses, are liable to errors in the central Arctic. We have followed the reviewer’s suggestion and computed time-height sections of the thermal, moisture, and kinematic fields from soundings and the weather mast. These were very helpful in checking if fronts / baroclinic zones appeared in the observations (see our response to Major comment 1). We have modified the manuscript accordingly (Sections 3.4 and 3.5). See also our response below.

These analyses can also be used to validate the temporal evolution (and hence the likely spatial distribution) of the ECMWF thermal and kinematic fields. This would be the validation of the temporal evolution of the low-level thermal field in addition to the statistical validation done by Jakobson et al 2012. Based on such analyses, it can be determined whether it is useful to use the ECMWF thermal gradient fields for the cases when only one or two tethersonde soundings are available.

The idea of validating the temporal evolution of the ECMWF fields is good in principle. We tried it, but it included two problems: (1) the small number (12) of soundings within a time interval of 6 h (the temporal resolution of the ECMWF fields), and (2) the small magnitude of changes in the fields between the soundings. Due to the first problem, the results are not statistically representative, and due to the second problem it is impossible to reliably convert the temporal tendencies to spatial gradients. This is because we cannot assume that the observed changes are just due to advection of stationary fields; the changes may also be strongly affected by Lagrangian changes related to turbulence, radiation, and condensation. Also subsidence and changes in wind direction may often play a role.

We note that in the revised manuscript the ECMWF fields are the primary basis only for the analyses of baroclinicity. In the analyses of frontal passages we now primarily rely on Tara observations, and only use the ECMWF fields as supporting material to compare with. Considering baroclinicity, a sufficiently large horizontal temperature gradient is needed to generate a LLJ, and it is more probable that the ECMWF analyses catch such synoptic-scale features than that they catch much smaller gradients that were often detected when comparing the successive tethersonde soundings. Some confidence to the ECMWF-based baroclinicity is also generated by the fact that baroclinicity was twice as common in cases of a LLJ as in cases without a LLJ.

2) The lack of wind direction is a problem for identifying inertial oscillations. Weren't measurements made of the changes in tether orientation with time during the ascent and descent (e.g., through the use of 2 gps points)? Even if the true direction is in error, the changes in direction could be used. Furthermore, the lack of temporal sets of profiles is a problem for looking at the evolution of the wind speed which could show better if there is an inertial oscillation. Were there any "IO" cases that had several ascent/descents so the evolution over 6-12 h could be seen?

Our tethersonde system did not contain GPS sensors, so we could not detect the changes of direction. We had three inertial-oscillation cases where also another sounding was done after 2 – 4 hours, and there was no jet anymore. Such a temporal evolution of the LLJ indeed supports the diagnose of inertial oscillations, because the jets related to synoptic-scale baroclinicity usually do not disappear so quickly and these jets were not related to gusts, as there were no major changes between the profiles of the ascending and descending soundings (Section 3.2).

3. pg. 13, l. 410-413: The argument that the altitude of the horizontal temperature gradient is kept low by strong stability doesn't hold in general. While the horizontal temperature gradient could be produced by differential surface heating, it can also be produced by flow confluence at any height without being impacted by vertical mixing. The height of a baroclinic jet is then

determined by the ambient stability and the height of the baroclinicity. If the baroclinicity is deep but the stability strong, the height of the LLJ will be low, as it is determined by the frictional retardation of the stronger geostrophic winds below. If the baroclinicity is shallow but stability weak, the wind maximum may be at a higher height than for the case of the stronger stability. The baroclinicity should reach at least za for these profiles since ug decreases to this height.

Thank you for pointing this out! We have rewritten this part following the interpretation suggested (Page 14, lines 463 – 468).

Minor Comments

We have edited the manuscript as suggested by the Reviewer in his/her 24 minor comments. We provide specific answers to the following minor comments:

2. Abstract, section 3, l. 396-397, and elsewhere: It's not clear what the difference is between LLJs associated with baroclinicity and those associated with fronts? – see major comment 1. If there is a difference, this should be clarified.
See our response to Major comment 1.

6. Pg. 4, l. 109-111: How far apart in time were the ascents and descents?
The time difference at the jet core height was never larger than 1 h. This information is added in the end of Section 3.3.

10. Pg. 6, l. 177-178: The air looks to be less stable in profiles with LLJs.
Indeed, on average the air seems to be less stable in profiles with LLJs, but we did a statistical analysis and it showed that the difference is really insignificant due to quite large variations (Page 6, lines 180 – 181).

14. Pg. 9, l. 291-299: reference studies of baroclinic jets and jets in association with fronts.
Reference studies are mentioned and compared to in "Discussion and Conclusions" section (Page 14, lines 455 – 456; 462 – 463).

21. Pg. 13, l. 399-400: Was data checked for diurnal variations? Tjernstrom et al (2004) found some subtle diurnal variations, as did Sverdrup (1932?) for data from the Maud. Sverdrup attributed these diurnal variations near the North Pole to atmospheric diurnal tides from lower latitudes.

We did not detect clear signs of a diurnal cycle. This may be partly due to the limited amount of sounding data, and its distribution over a long period from spring to summer. We added a reference to Tjernström et al. (2004) (Page 14, lines 451 – 453), but not to Sverdrup (1932) because we unfortunately do not have access to it.

24. Figs. 7 & 8: the scatter in these plots is very large, and any correlation is weak at best.

Yes, considering all 25 jets, there was no correlation ($r = 0.004$) between the height of the jet core (z_j) and z_{Ri} (Figure 7). We tried to point out that if the inversion base temperature (T_b) is lower than –15°C then there is a high correlation ($r = 0.95$; $p = 0.052$). Although it is based on very limited data, it fits well to Andreas et al. (2000) and Walter and Overland (1991), and is therefore worth mentioning (Page 11, lines 347 – 349). Also it seemed interesting for us that a jet core with higher than average wind speed ($U_j > 7.1$ m s^{-1}) occurred more often inside the turbulent layer (Figure 7) (77% of these cases showed $z_j < z_{Ri}$) and jet cores with smaller than average wind speed ($U_j < 7.1$ m s^{-1}) appeared above the turbulent layer (83% of these cases showed $z_j > z_{Ri}$) (Page 11, lines 349 – 353). Actually there was significant correlation in Figure 8 ($r = 0.62$; $p < 0.01$). We pointed out that LLJs with the core above the turbulent layer had a correlation coefficient of 0.72 ($p < 0.01$) (Page 11, lines 360 – 362).