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## ***Interactive comment on “Low-level jet characteristics over the Arctic Ocean in spring and summer” by L. Jakobson et al.***

**Anonymous Referee #2**

Received and published: 25 March 2013

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. 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

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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

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. 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. 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

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fields from the tether sondes (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  $\sim 12$  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 is present on the warm side of this thermal transition, its likely dynamical cause is more clearly identified.). 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 tether sonde soundings are available.

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  $\sim 6$ -12 h could be seen?

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

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maximum may be at a higher height than for the case of the stronger stability. The baroclinicity should reach at least  $z_a$  for these profiles since  $u_g$  decreases to this height.

Minor Comments 1. Abstract, l. 9-14: "...layer. Complex relationships between ...layer, but when inside the turbulent...mostly associated with transient..." 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. 3. Pg. 2, l. 51-52: "...sea ice. Near the coasts ..." 4. Pg. 3, l. 78: There were extensive tether sondes and kite sondes flown during AOE-2001 (Tjernstrom et al 2004). 5. Pg. 4, l. 108: replace "Hence" with "Therefore" 6. Pg. 4, l. 109-111: How far apart in time were the ascents and descents? 7. Pg. 4, l. 144: suggest "...800 m. The inversion top (230 m) is slightly..." 8. Pg. 5, l. 149 and l. 152: delete "each" 9. Pg. 5, l. 153-157: "... existence of a LLJ was the criteria for choosing the daily sounding...the highest sounding with a LLJ was chosen. All ...profiles were used in analyses ... 95 soundings, LLJs observed in 43 of these, and 25..." 10. Pg. 6, l. 177-178: The air looks to be less stable in profiles with LLJs. 11. Pg. 7, l. 207-208: "...than at the surface, the ... fulfilled. Thirteen cases..." 12. Pg. 8, l. 254: "...when the air mass traveled from ..." 13. Pg. 9, l. 280 suggest "...and four could have been related to wind..." 14. Pg. 9, l. 291-299: reference studies of baroclinic jets and jets in association with fronts. 15. Pg. 10, l. 303-304: "...in a single case was a jet ... unknown origin stronger than ..." 16. Pg. 10, l. 314-315: "...the stronger the wind the smaller the  $Ri(z)$ , and the smaller the  $Ri(z)$  the higher the  $z_{Ri}$ . 17. Pg. 11, l. 355: "...Denmark Strait..." 18. Pg. 11, l. 360-361: SHEBA soundings were smoothed over 1 minute to eliminate swinging motion effects. This time is about 450-500 m at 8 m/s ascent rate. The raw 1-s data is available, however. 19. Pg. 12, l. 379-380: I believe the AOE-2001 soundings were also smoothed, and hence only the strongest LLJs will be detected. 20. Pg. 12, l. 391: "...aircraft flight near the location of the last sounding..." 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

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diurnal variations near the North Pole to atmospheric diurnal tides from lower latitudes.  
22. Pg. 13, l. 405: "...associated with transient ..."  
23. Fig. 4: please label the panels  
24. Figs. 6 & 7: the scatter in these plots is very large, and any correlation is weak at best

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