**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Sensing Hadley cell with space lidar" *by* W. Sun and B. Lin

S. Davis (Referee)

sean.m.davis@noaa.gov

Review of "Sensing Hadley cell with space lidar"

Several recent studies have presented evidence of tropical widening, using various diagnostics for the latitudinal edge of the tropics. The authors propose a novel new definition of the Hadley cell edge based on the occurrence of subvisual cirrus clouds as measured by space-based lidar, and suggest that long-term measurements from space could be used to measure tropical widening. These ideas are worth pursuing, as there is a relative dearth of direct measurements of tropical widening that don't rely on reanalysis, which can be problematic for trend detection. However, as currently written the paper inadequately describes the proposed tropical edge definition and properties, and fails to show how reliable this methodology is in defining the tropical edge relative to other more established methods of defining the Hadley cell edge, e.g., based on

the mean meridional streamfunction. Overall, this paper appears to be incomplete and hastily put together. I cannot recommend publication without a significant expansion of the description of the edge definition, and some effort to validate/evaluate the proposed definition. Below are several major comments that should be addressed, along with some specific comments.

Sean M. Davis

Major comments:

1. At several points throughout the paper, the authors seem to confuse the cause of high altitude tropopause-level subvisual cirrus clouds, associating them with the vertical mass flux and vertical moisture transport in the Hadley cell. While SVC do occur at latitudes in the general region of upwelling in the Hadley cell, they reside in the TTL above the region of significant upward mass flux that most people would consider to be the traditional Hadley cell. Also, there is very little evidence that their occurrence is due to moisture transport from convective systems. On the contrary, their occurrence is related more to the cold temperatures encountered in the TTL, and the non-occurrence high-altitude SVC at higher latitudes seems to be a simple consequence of the change in tropopause height with latitude. This begs the question of why not just use tropopause height to identify the tropics, as was done in previous work? Some discussion on these point should is needed in the paper, as it affects the interpretation of the high-altitude SVC as being an exact marker of the Hadley cell.

2. Description of methodology – There seems to be many details missing here. Why are CCCM data being used instead of just CALIPSO data? Does the identification really require the use of both CERES and MODIS data? If so, this would seem to be a real weakness of the methodology (that a coincident imaging radiomter is required in addition to a space-based lidar for this method to work). However, I don't think MODIS is required.

It appears from the description that the authors are requiring MODIS cloud fraction <

1% and CALIPSO cloud fraction 100% to identify a 'thin cirrus'. If this description is accurate, I have two questions: 1) Why are you requiring MODIS cloud fraction < 1%. What you are essentially saying is that the lower atmosphere has to be clear, but that the upper trop has to be cloudy. Why not just use a CALIPSO cloud fraction threshold for scenes with tops > 15 (or some other value) km? It seems like you are needlessly biasing the results by only considering scenes with clear sky below.

2) Is the CALIPSO cloud fraction threshold really 100%? What do you even mean by cloud fraction for the case of CALIPSO? For CALIPSO and MODIS, cloud fraction should be clearly defined. My guess is that a MODIS cloud fraction is f = xcloud/xtotal where xcloud is the number of cloudy pixels in the scene and xtotal is the total number of pixels. For CALIPSO, which views a 'curtain', does 100% just mean that there is a cloud somewhere in the vertical profile at one point along the 'curtain', or is it the number of cloudy profiles in the curtain divided by the total number in the curtain?

3. Lidar sensitivity requirements – One could imagine that lidars with different detection limits for thin clouds might come up with different latitudinal extents of the tropics. As shown in Davis et al (In situ and lidar observations of tropopause subvisible cirrus clouds during TC4, JGR, 2010), CALIPSO misses a significant fraction of the thinnest SVC. While this is not necessarily a problem for tropical edge detection, it may lead to constraints on future lidars. In other words, the tropical edge could occur at a different latitude if lidars (on future satellites) with higher sensitivities were to be used. This could cause spurious widening trends in the tropical width. It seems possible that a definition of the tropical edge could be constructed that did not exhibit this sensitivity. Either way, potential sensitivity issues should be discussed, and ideally the tropical edge definition should be as insensitive as possible to potential future increases in sensitivity of space-based lidars. Related to this, the issue of nighttime vs. daytime data should be discussed in more than just a passing sentence as it currently is.

Specific comments:

Title – The title exhibits poor grammar, and is imprecise. One example of a more appropriate wording would be "Sensing the Hadley cell edge with space lidar". Also, the paper doesn't really establish that it is specifically the Hadley cell edge that is being identified. Depending on the changes made to the paper, it may be acceptable to substitute the more general "tropical edge" for Hadley cell.

Page 1, Line 1 – "reliable" – This paper does not establish that the Hadley cell could be reliable measured. No data are presented validating the approach, so it is a stretch to call it a "reliable" method.

Page 1, Line 9 – I would dispute that the tropical widening has been unambiguously attributed to global warming. It should be at least acknowledged that ozone depletion may be involved (e.g. ,Polvani et al., 2011, Son et al., 2010, etc).

Sentence starting Page 2, Line 2 – This is an extremely long run-on sentence.

Page 2, line 18 and Figure 4– "common clouds. . . cannot be used for tracing the large-scale air flow in the general circulations." I disagree with this statement, and Figure 4 seems to show that there is a transition in cloudiness from the tropics to extra-tropics. Why couldn't one just simply use a threshold of the MODIS ice cloud height (e.g., 10 km) to identify the tropical edge? Also, the OLR methods used in Hu and Fu (2007) are partially based on the fact that the subtropical dry regions are characterized by infrequent cloudiness, which (along with temp/humidity profiles) affects OLR and causes a peak in the subtropics. So "common clouds" really can be used for indicating the tropical edges.

Last page, line 10 – "mean maximum" ? What does this mean? Do you mean the mean cloud top height?

Last page, and figure 4 – Why is colatitude being used here. You should just convert to latitude.

Figure 1 – This looks very similar to the figure on Wikipedia and other educational sites,

such as http://www.srh.noaa.gov/jetstream//global/images/jetstream3.jpg Their repro-
duction may be okay, but they should be acknowledged at the very least, if that is
indeed what this figure is based on.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 16599, 2011.

C7147