

Review of Oscillations in atmospheric water above Switzerland

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General comments

Although the paper is based on a very valuable dataset (a continuous time series of CF, ILW, and IWV from the TROWARA at Bern for the period 2004-2016), and the data analysis methods used are certainly valid and well established, I am disappointed in the physical interpretation of the obtained results. The manuscript reads more as a summing up of findings, and often no explanations are given:

- On page 5, line 31: "it is surprising that the power spectrum of the zonal wind has strong annual harmonics reaching up to the fourth harmonic". Couldn't you try to explain this surprise?
- on page 6, lines 4-8: "The amplitude of the annual oscillation was strongest around 2010 to 2011. The semi-annual oscillation is strong from 2010 to 2014. ... Similar to CF, the SAO in ILW is strong from 2010 to 2014." What is so specific about this 2010 to 2014 time period to explain the strongest amplitudes in the annual and semi-annual oscillations of those parameters?
- On page 6, lines 31-32: "The amplitude maxima are at a period of 7 days for CF, 6 days for ILW, 365 days for IWV, and 17 days for u". Can you relate the 6-7 and 17 day periods to atmospheric/weather event phenomena and what causes the difference between those periods for CF/ILW and u, given your earlier argument that those 3 variables are closely connected?
- On page 7, lines 2-3: "Figure 8 shows the mean amplitudes as function of the month and the period. The climatologies of CF, ILW and IWV show some similarities with increased amplitudes in the period range 5-10 days from spring to fall." Is this period related to synoptic scale weather events and why not in wintertime?

On the other hand, if the authors tried to explain some of the findings, their interpretation is too suggestive.

- on page 5, from line 32 onwards: "Since lower tropospheric wind is a major player for cloud formation and transport processes we suggest that the spectral components in the zonal wind spectrum are possibly the cause for the annual and semi-annual oscillations in the power spectra of CF and ILW". Did the authors investigate other causes?
- on page 6, lines 11-12: "It is surprising that the AO of the CF is almost in anti-phase to the AO in ILW which peaks in July. We think that convective cumulus clouds are responsible for the high ILW values in July". It is obvious that convective cumulus clouds form mostly in summer, but can

you make this thought more scientifically sounded (observations of clouds, CAPE index calculation etc.)?

- On the same page, the interpretation in lines 16-21 is also made on a lot of assumptions, which are not proven by the authors: "It is obvious that the climatology of u is rather similar to the climatology of CF in Figure 5. It seems that the strong eastward wind in December and January transports stratus cloud layers to Switzerland. Related to the study of Nuijens and Stevens (2012) we may argue that an increase in the lower tropospheric wind u leads to a deepening of the cloud layer. In addition, one may argue that an eastward advection of moist air from the Atlantic towards the Swiss plateau and the Alps occur which leads to a maximum of CF in winter". To my opinion, these statements might only be made after a careful cluster analysis of the trajectories of the air parcels arriving at Bern. I think that the entire paper would greatly benefit from such an analysis.
- On page 7, lines 3-4, it is stated: "The climatology of u shows a 20-day oscillation in winter which is possibly related to a Rossby wave". If you make such a statement, you should argue this.

Due to the lack of physical interpretation of the observed frequencies, the focus of the paper now lies on the demonstration of the analysis technique of identifying periods in the power spectra of CF, ILW, and IWV. The authors should investigate more time in the interpretation of the identified periods and the links between CF, ILW, IWV, u , and other variables, e.g. based on an identification of the origin of the air masses arriving at Bern. I would therefore suggest to make a major revision of the paper.

Specific comments

- On page 4, lines 29-30: "Finally, we like to mention that the CF, ILW and IWV measurements of TROWARA at Bern are representative for the Swiss plateau". Do you have references or arguments for this statement?
- Fig 2: the authors point to the similarity between the normalized power spectra of CF, ILW and u . This is certainly true for the two peaks around the fourth harmonic. But what about the rather strong frequency around 2.4 cycles/year in both the CF and ILW, which is not so prominent in the u ?

Technical corrections

- Page 2, line 15: there are two "the" before seasonal
- Page 5, line 24: I would write "information" instead of "informations"
- Page 5, line 31: there are two "to" at the end of this line.