Atmospheric Chemistry and Physics Discussions

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# Comment on acp-2022-335

Anonymous Referee #2

Referee comment on "Observing short-timescale cloud development to constrain aerosol–cloud interactions" by Edward Gryspeerdt et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-335-RC2, 2022

**General comments:**

In this well-written manuscript, the authors offer a new method for using MODIS cloud data combined with the CERES-CloudSat-CALIPSO-MODIS (CCCM) combined product and ERA5 reanalysis fields to assess short-term (several hour) changes in the relationships between cloud liquid water path (LWP) and cloud droplet number. The results focus on average cloud properties in 1x1 degree bins. There were a few places in the methods that need more clarification, but overall I believe the results will be helpful in understanding the factors contributing to cloud evolution over short timescales over large spatial scales, and that this information will provide an important constraint for models. I'm happy to recommend publication once the authors address the comments below.

**Specific comments:**

The introduction offers a nice assessment of the potential pitfalls in trying to identify aerosol impacts on the short-term development of clouds.

Methods: It might be helpful to mention more explicitly that the cloud droplet number is taken only from the top layer of clouds, whereas the LWP is a column property.

How did the authors account for cirrus clouds? Have the authors assessed how their results might differ if they exclude pixels with multi-layer clouds (e.g., see

https://doi.org/10.5194/amt-13-3263-2020)?

The ERA-5 wind results might really depend on altitude. Was some sort of cloud top height product used to identify the height of the cloud tops? Similarly, I am not sure how helpful surface winds are when many of the clouds of interest are at much higher altitudes (Fig. 5d).

Figure 1: For clarity, it might be helpful in the figure to say something like, "Low Nd (N < 60 cm-2)" and "High Nd (Nd > 60 cm-2)" instead of just high and low Nd.

In the methods it said that the study is only looking at oceanic data, but terrestrial data is shown and discussed later in the paper.

It would be helpful to have an indication of sample numbers in the various figures, and to mention whether there was any cutoff for excluding locations with low sample numbers (e.g., in Figs. 4, 5, etc.), which may add uncertainty to the extreme points.

L.63: "The LWP is calculated using all the available liquid pixels, as restricting the LWP retrieval to only the pixels used for the Nd calculation biases it towards higher optical depths, leading to a high LWP bias against passive microwave LWP (Gryspeerdt et al., 2019)." That makes sense, but does not solve the problem of the $N_d$ data then being potentially biased. Do any aircraft data support the LWP-Nd trends observed?

Line 104: "This would happen even without a causal Nd impact on LWP." I got lost here. Could the authors please provide a more thorough explanation for why that might be?

L117-120: Unless I am misunderstanding something, I don't really see enough evidence for attribution to pollution in all of the cases here. Maybe the text should be changed to something like, "is associated with pollution-dominated air masses in some locations"? For example, the Eastern Pacific area shows higher values, but is not typically thought of as a comparatively polluted region.

L.121: "In clean conditions (Nd>25 cm−3 )...." I agree that low Nd levels are often associated with reduced aerosol conditions and high Nd levels are often associated with high aerosol levels, but Nd is not a direct proxy for pollution concentrations so I suggest rephrasing. Same for labelling cases with initial Nd of 25-100 cm-3 as "moderately polluted" cases.

Figure 5: Why there are different data bins in each of the 4 panels?

Paragraph starting on L 168: Sea salt is not the only source of marine CCN – in fact it may not even be the main source of marine-produced CCN depending on location and time, e.g., https://www.nature.com/articles/s41598-018-21590-9 and emissions from these other sources also may be affected by wind speed.

The authors discussed "noise." Could they please discuss in greater detail what factors contribute to this noise?

Please discuss how the effects of systemic meteorological differences/changes at the different locations might affect the results. Does "noise" include this factor?

**Technical comments:**

L.62: "The Nd is calculated assuming **an** adiabatic cloud"

Flow field is sometimes spelled flowfield. Suggest standardizing, and possibly defining at first use.