Review of Smalley et al "A Lagrangian Analysis of Pockets of Open Cells over the Southeast Pacific"

The manuscript provides an observational analysis of pockets of open cells (POCs) over the southeast Pacific during a three-month period using retrievals from the geostationary satellite GOES16 and other satellite/reanalysis products. Once the POCs are identified, the authors use MERRA2 reanalysis winds to track how their size and cloud and environmental properties change during the POC evolution, from before its formation to after its demise. The authors also run trajectories that start 24 hours before or after initial POC formation in cloudy overcast scenes to compare how the POC trajectories differ from a "control" trajectory case where no POCs form.

Although there have been a couple previously published, satellite-based analysis of POCs (Wood et al. 2008; Watson-Parris et al., 2021), the novel aspect of this study is the use of Lagrangian trajectories to examine the temporal aspect of clouds, particularly how they form and how they dissipate. Information of their temporal duration and area are new and interesting. However, I found that much of the manuscript focuses on the results that have been confirmed by past observations (more rain rate, larger cloud drops, lower aerosol or cloud droplet number concentration in POCs) rather than highlighting the new findings and its implications. I believe that the authors can make significant contributions to answering what conditions favor POC formation and what sets the size of POCs, which other observational studies have not been able to discuss at length. In the current manuscript, the discussion of these results seems minimal. In addition, there are a number of errors and issues associated with the methods and points of clarification that I would like to see the authors address.

Given that some of the methodological fixes potentially impact results and interpretations, I recommend major revisions before the manuscript is published.

Major comments

Lack of emphasis and discussion on the temporal evolution of POCs
The authors point out the novelty of their analysis is in capturing the temporal evolution of POC formation, maintenance, and dissipation. And based on the title, I expected to see more emphasis and discussion on such themes. However, most of the results of the paper seemed to focus on confirming past findings about the POC vs non-POC cloud and environmental conditions that have already been reported by previous studies, which did not use a Lagrangian perspective. That POCs have less clouds, larger cloud droplets, less aerosol/cloud drops, more intense rain rates, and that they have similar large-scale meteorological contexts as non-POC regions have been reported previously by examining POC conditions and the regions surrounding POCs, without looking at a Lagrangian perspective.
I believe that the authors are however well positioned and should focus on answering some questions that other studies have only lightly touched on or have not addressed. First, they can better answer the origins of POCs and whether there are any environmental precursors that make it much more likely for POCs to form. For example, are there necessary conditions for POC formation? The authors touch on this in Figure 13 and 14, and mention that there appear to be no environmental precursors to POC formation, but do not elaborate further about what it means

with respect to what previous studies have found (e.g., Wood et al., 2008; Yamaguchi and Feingold, 2015) and whether the authors find evidence to support or go against what has been proposed in those studies.

The authors are also well positioned to answer the downsides of relying on polar orbiting satellites to identify and characterize POCs. Do we miss anything by just observing POCs twice a day, except for the lack of temporally tracking their evolution?

The other set of questions that the authors are able to address relates to the growth of POCs. For example, what sets the growth of POC size? And for larger POCs, do they grow rapidly or steadily over time? Are POC sizes just determined by how long it takes for the POCs from formation to reach the edge of the stratocumulus deck? When POCs grow, do they have a preferred direction of growth relative to the 925hPa trajectory?

LTS & LCL calculations

For the LCL and LTS calculations, the surface air temperature is typically used, rather than the surface temperature (Romps, 2017; Klein and Hartmann, 1993). For the LTS, it seems that the sea surface temperature is used and potentially also for the LCL. I suggest the authors redo the analysis for these. Based on previous studies that have shown surface air temperatures tend to be colder in POCs due to precipitation evaporation, I would expect a lower LCL and stronger LTS over POCs vs non-POCs.

Furthermore, my impression from the reading the manuscript is that the boundary layer qv and LCL are frequently discussed as environmental factors (alongside large-scale subsidence or sea level pressure differences), when internal cloud and boundary layer processes (precipitation and boundary layer decoupling) can have significant impact on these variables. Please provide further explanation for why the authors consider them as environmental factors mainly determined by the large-scale meteorology, rather than internal boundary layer processes.

Analyses in Section 3.2

The synoptic comparison between no-POC and frequent (>7) POC days is interesting, but how robust are the results? In other words, how similar are the synoptics among frequent POC days vs no POC days compared to the difference between frequent and no POC days? There is no discussion about how large the POC vs non-POC differences are compared to the variance within each category.

It was also not clear to me what specific question the authors want to answer by examining the synoptic and environmental conditions. My impression is that the purpose of this section is to ask whether there are certain environmental conditions that favor POCs vs just common stratocumulus clouds. The statement at the end of the section (L250-254), which points out that many of the synoptic differences are consistent with conditions that favor more stratocumulus seems to muddy whether those synoptic differences we observe in the difference plots are indeed conducive for POCs specifically. Can the authors perform more in depth analysis with the available data (or by including other months) to solely compare non-POC days when there are large StCu cloud decks to see whether there are certain conditions that actually favor POC formation?

Note also that some things like, lower AOD, lower LCL, and higher qv might be a symptom of, rather than a cause behind having many POCs during a day, because POCs are cleaner, have lower LCL, and moisture near surface values compared to non-POC boundary layers.

Specific/minor questions

L8 – Although it also shows up in the conclusions, I believe that 104km2 is a typo. Please correct.

L90-91 – Would the authors provide a sentence to explain why POCs that appear to develop in response to gravity waves are excluded?

L117- 258 POCs seems to be a lot for a 3 month period, compared to 23 identified by Wood et al. (2008) over a two month period. Are these all separate POCs that form? How many of these merge with each other? Looking at Figure 9, I can potentially see how one can reach 258 POCs over a ~90 day period but cannot imagine a stratocumulus cloud deck with 20+ POCs embedded within it, especially since Figure 2 and 3 show just one POC. Would the authors show a snapshot with multiple POCs in one day so that we can see the shape and size of POCs that are identified on a frequent POC day, in addition to the classical cases shown in Figure 2 and 3?

L121 How sensitive are results if the 0.5degx0.5deg box is relaxed to something like 1x1 deg? Are all subsequent comparisons of variables (clouds, environment) averaged over the 0.5 deg x 0.5 deg box following the wind trajectory starting from initial formation? Or are they averaged over the whole POC?

L126-127 – how many POCs intersect a new POC? I assume this means that a POC merges with another POC? How often do these occur?

L155-156 - Are LWP and N derived during the nighttime? I haven't heard of such work before. Are there previous studies that have evaluated the validity of using nighttime optical depth and re retrievals to calculate LWP and N?

L159 – degree sign

Figure 4 – Why is the color scale cutoff at 10-2 mm/d-1 when the AMSR rain probability plot shows a 0.1 mm/d precipitation threshold?
And what does the right figure show? At 0.1 mm/d, we get a probability of ~2% and at 10 mm/d a probability of ~80%. Is it the probability of the rain rate being greater than the value along the x-axis? Are the rain rates area-averaged? Please provide some more explanation.

L190 – For calculating LTS, the surface AIR temperature should be used rather than the sea-surface temperature, since it's a measure of the atmospheric column instability.

L191 – Similar to the comment for L190, is the surface air temperature used or the sea-surface temperature?

L221 – What local times have previous studies found POCs to form at?

L233 – Figure 9, rather than Figure 11.

Figure 9 caption – median counts, rather than duration.

Fig 12 – there are large discontinuities along the day/night boundary in N, re, optical depth. How many of these are due to retrieval differences and how many from actual cloud changes?

L271 – I believe the authors mean reduced N, rather than elevated N.

L271-272 – How do the authors reconcile these observations with the rain rate plot, which seems to indicate similar rain rates? Can the authors comment further about this?

Figure 14 – The bins don't seem to overlap. Is there a reason for this? Would the authors provide more explanation for the choice to weight the frequency by the bin-mean rain rate? I can see that by weighting the distribution by rain rate, one can discern the difference in mean-precipitation rate between POC and nonPOC trajectories, but it makes it more difficult to ascertain the relative frequency of certain rain rates occurring in POC and nonPOC trajectories, especially before the formation of POCs (ie, asking whether exceeding a particular rain rate makes it X% more likely to form a POC).

L304-307- How good is MERRA2 reanalysis in capturing and characterizing boundary layer characteristics in POCs? I suspect that satellite retrievals of boundary layer characteristics are not well captured due to the large gradients. For example, does MERRA2 capture the boundary layer decoupling that is frequently observed in POCs? If not, I would be suspicious of the boundary layer output being compared.

L315 – "…have a maximum area larger than 104 sq km… " – The value here is likely wrong, given that the minimum size is 0.5degx0.5deg. However, I am curious how sensitive is this size to the 0.5x0.5deg minimum threshold used to identify POCs. I suspect not so much, but I am curious, given that the results here differ from Watson-Parris et al. (2021).

L317-318 – Precipitation is discussed as if it's solely driven by boundary layer dynamics and microphysics, but certain environmental conditions do make precipitation formation more conducive, as was discussed by Wood et al. (2008). LWP, re, and N appear to be different before POCs form (L271-272). Are there POC and nonPOC differences in Fig 10 or 15 that would be conducive to increasing LWP or decreasing N?