This study provides a comprehensive look at daytime evolution of low cloud susceptibility to droplet number (N_d) perturbations. These susceptibilities are derived from snapshots of cloud and radiation fields taken from passive sensors (SEVIRI), onboard a geostationary satellite (Meteosat11), within a 10-by-10-degree box over the NE Atlantic region. The authors found persistent "U-shape" evolutions in cloud susceptibility, including cloud water, cloud albedo, and cloud fraction, from which they argue that polar-orbiting satellite derived cloud susceptibility underestimate the daytime-mean cloud susceptibility. They attribute the observed evolution in cloud susceptibility to two main hypotheses that they formulated and tested: i) clouds have "memory" of their past susceptibility signal associated with the cloud state they transition from; ii) cloud responses to N_d perturbations have a longer timescale than the satellite observing time interval (30 mins).

Overall, I found the study interesting and intriguing in some sense. Clearly, the authors have put in lots of thoughts on this problem and lots of efforts in interpreting these results. The manuscript is nicely constructed and organized. That being said, I do think there are some key/fundamental issues that need to be addressed and justified first. If these concerns and issues can be sufficiently addressed, I believe this work has the potential to provide new, impactful insights and make significant contribution to the field.

Major concerns/comments:

1. The authors adopted a methodology for deriving cloud susceptibility that uses spatial regression of cloud properties to N_d within satellite snapshots. This is done to minimize the impact from confounding meteorology. I found the extension of this approach to cloud fraction susceptibility is a bit unjustified, and I'm in general concerned about the appropriateness of applying this methodology to cloud fraction susceptibility. Cloud fraction is a tricky quantity by definition. It depends strongly on the spatiotemporal scale of one's investigation, for example, the distribution of daily cloud fraction will look very different from that of monthly cloud fraction at any given location on Earth. The degree of spatial aggregation also strongly affects cloud fraction, e.g., cloud fraction reduces to binary (zero and one) at pixel level. Overall, I find the discussion/explanations around CF susceptibility quite vague and often led to confusion, which could potentially be related to an ill-defined methodology. Some of my specific concerns are:
    a. How confident are you that, at 25-km resolution, variations in CF is not simply coming from the geometry of the cloud field, such that one scene captures the majority of a cloud while another scene captures only a small portion of the same cloud, resulting in a large difference in cloud fraction between the two scenes, while N_d has nothing to do with it.
    b. On lines 335-336, the readers are directed to the next section for explanations on the CF susceptibility pattern seen in Fig. 2c. In the next section, your discussion and hypotheses testing are targeted at CF susceptibility evolutions, while I'm still missing an explanation for the non-precip thin cloud that exhibit potentials in cloud fraction enhancement.
    c. Fig. 11 is another example that I am having difficult times wrapping my head around. While Fig. 11a and b suggest that the frequency of occurrence of cloud state in the LWP-N_d space is indeed changing throughout the day, the null contribution from

frequency changes shown in Fig. 11c troubles me a bit. I guess it's related to the pattern we see in Fig. 2c, which I still struggle to decipher.

2. Regarding the explanation for the "U-shape" evolutions, it seems a bit suspicious to me that all 3 quantities show the same shape of evolution. Moreover, you use cloud transition categories to explain the observed U-shape evolution, yet you see the similar "U-shape" for individual categories, suggesting to me that there are something fundamental that has not been teased out. This makes wonder if this is due to retrieval artifacts associated with relatively high solar zenith angle during early morning and late afternoon? Smally and Lebsock (2023) made a comparison between retrieved LWP from tau & r_e using adiabatic assumption with microwave imager retrieved LWP, onboard geostationary satellites. They found non-negligible bias even at intermediate SZA. The same bias applies for N_d retrievals as well, in a more pronounced way I suppose, given the high order dependence of N_d calculation on r_e. I wonder if this "U-shape" evolution can be explained by tau & r_e retrieval biases as a function of SZA. To me, even small biases can produce the trend you see towards higher SZAs (i.e. towards early morning and late afternoon). I suppose one can play with some synthetic data to rule this possibility out.

3. Assuming daytime evolution in cloud susceptibility is not SZA-dependent for now, an alternative explanation for the "U-shape" evolution is the change in meteorology, i.e., daytime evolution in boundary layer (BL) depth. From morning to early afternoon, BL deepens and becomes decoupled, enhancing the role of entrainment feedbacks in governing cloud susceptibility. In the afternoon, BL re-couples. This could possibly give you the "U-shape" evolution in cloud susceptibility. I wonder if the authors have considered this as an alternative explanation and tested it?

4. Regarding the "memory" hypothesis, I have to say that I'm quite lost in this argument, by the design of the analysis, by the inconsistency that I spotted when reasoning with this argument, and by the actual physical meaning of "memory" in the context of cloud susceptibility.
   a. Fundamentally, the concept of "memory" requires a Lagrangian perspective, however, I believe all the analyses done in this study are based on a Eulerian framework. This means that the "memory" in this study is actually the domain "memory" of the clouds that were previously present in the domain, not cloud "memory" of its past states. I believe one needs to at least do a grid-box tracking, if not individual cloud tracking, to support this argument, or show that clouds are stationary/semi-stationary in the study region.
   b. One example of the inconsistency that I observed is that in Fig. 5, if I follow the argument correctly, thick→thin transition will lead to more negative LWP susceptibility based on the LWP-N_d plot, however, at early morning where thick→thin is the most frequently occurring type, the overall LWP susceptibility is the highest (Fig. 4b). Furthermore, how do you explain the "U-shape" in the evolution that is specific to thick→thin transition clouds, they all have the same type of "memory" according to your argument.
   c. I wonder at what timescale do you expect these clouds to detach/decorrelate from their "memory" of past states? Existing literature seems to suggest a decorrelation timescale that is much shorter than your 2-hr window, on the order of 10 to 15 mins, for marine

boundary layer clouds. I wonder if you could elaborate on how does this decorrelation timescale fit in your 2-hr "memory" hypothesis?

d. I'm familiar with the concept that cloud have "memory" of their near-past state, in which clouds carry-over their past tendencies for a short period of time. However, I am afraid that I don't quite follow the argument that clouds have a "memory" of its past 'susceptibility.' I wonder if you could elaborate more on the physical mechanism of it. I believe the schematic (Fig. 8) you shown refers to the concept I mentioned, such that stronger evaporation & entrainment are carried over if the cloud just transitioned from thick to thin. It speaks to the temporal evolution of clouds, but how does this work for a "memory" of susceptibility, which is essentially a spatial regression, is less clear to me.

Minor comments:

- Line 36-37, 600, 669-671, the authors made the point that susceptibility derived from polar-orbiting satellites, Aqua in particular, underestimates the daily mean value of cloud susceptibly. While this is true based on the findings from this study, I would like to raise the point that this underestimation does not necessarily translate into an underestimation of the actual cloud response, as the simple arithmetic mean of susceptibility ('local derivatives') is not the same as a time integral of cloud responses, which is more relevant to the scaling up of radiative effects of ACI.
- Line 48, I guess you need more reference here than just Albrecht 1989, as it only speaks to the precip-suppression effect.
- Line 109, note that Gryspeerdt et al. (2021) did not use geostationary satellite but rather polar-orbiting satellites, i.e., Terra and Aqua.
- Line 114, I thought you did not "track" any cloud or cloud field and used with the Eulerian framework, I would be more precise here to avoid confusion.
- Line 137, reference for LWP calculation formular?
- Line 155, c_w is a function of pressure as well.
- Line 172-173, did you average tau and r_e first and then calculate N_d or calculate N_d first then average? This difference can induce huge bias in albedo susceptibility quantification as shown in Feingold et al. (2022, ACP).
- Line 175, note Zhou et al. (2021) used 2x2 grid box, if I recall correctly, worth double checking.
- Line 187, why cloudy pixels at cloud edge are set as clear?
- Line 218-219, why you opt for a Eulerian framework here? Isn't a Lagrangian framework the appropriate choice here as you want to investigate cloud "memory"?
- Line 254, what do you mean by "witnessed by clouds"?
- Line 266, I wouldn't use "hypothesis" here as these entrainment feedback mechanisms have been previously established, using large-eddy simulations.
- Line 280-281, what do you mean by "meteorological influences on clouds likely dampen the signal of the AIE…" I feel like you try to say meteorological confounding tends to obscure the AIE signal, probably need to rephrase here.
- Line 352-353, I don't think you have diurnal cycles, only daytime, right?
- Fig. 3c, the day-to-day (or spatial variation within the 10 by 10 box) variation is huge, at a given time of the day, is it possible to tell the sign of CF susceptibility with statistical significance?

- Line 439-440, I don't follow this argument, your susceptibility for each individual transition group shouldn't be affect by their relative frequency of occurrence at a given time, if I understand your method correctly, then why fewer samples in the thick/precip→thin groups lead to less difference between the groups?
- Line 454-456, again, why not using Lagrangian tracking in this assessment?
- Table. 1, I wonder if there are more effective ways of illustrating hypotheses than showing them in a table, the current structure of the table is quite difficult to decipher, at least for me. In particular the N/A boxes confuse me, and I don't see discussion of them in the main text.
- Line 478-481, these arguments are vague, if your "memory" argument stands, shouldn't we see a lagged evolution in thin clouds compared to thick clouds, instead of similar evolution?
- Line 488, why this is particularly prominent in the morning, I could not think of a reason following your "memory" argument.
- Line 567, worth repeating the question here.
- Fig. 10b, why the difference between thin→rain and thick→rain is reversed during midday, compared to early morning?
- Line 602, "instantaneous responses of warm boundary layer clouds…" I suggest rephrasing, as what you shown in the study is basically correlations (regression slopes), not actual responses (which refer to perturbation experiments).
- Line 621, "instantaneous CF adjustment rate" is a bit awkward, I suggest rephrasing (see comment above).
- Line 643, again, established mechanisms, not hypotheses.
- Font size too small in Figs. 3-7, 9-10. I suggest matching them to that of Fig. 11, which looks just fine.
- In Fig. 3-7, 9-11, basically all time-series figures, I strongly recommend replacing the horizontal lines indicating daytime mean values with lines indicating "0", and one can simply label the daytime mean values on each panel. The current horizontal lines are so seductive to be seen as the zero lines.

Reference

- Smalley, K. M., and M. D. Lebsock, 2023: Corrections for Geostationary Cloud Liquid Water Path Using Microwave Imagery. J. Atmos. Oceanic Technol., 40, 1049–1061, https://doi.org/10.1175/JTECH-D-23-0030.1.
- Feingold, G., Goren, T., and Yamaguchi, T., 2022: Quantifying albedo susceptibility biases in shallow clouds, Atmos. Chem. Phys., 22, 3303–3319, https://doi.org/10.5194/acp-22-3303-2022.