We would like to thank Michael Diamond and the two Anonymous Referees for reviewing the revised manuscript. Following are our point-by-point responses to their latest comments. Line numbers in our replies refer to the "track-changes" version of the manuscript.

Reply to Referee # 1 (M. Diamond)

The authors have done a nice job responding to the reviewer comments and the resulting manuscript is in very good shape. I have a few remaining comments that I hope the authors will consider before publication. -Michael Diamond

1. For the displayed uncertainty, please specify the confidence interval. (I.e., is this one propagated standard error or two?)

Reply: The displayed uncertainties correspond to one standard error. This is now clarified in the captions of Figs. 2, 4, 7, 9, S1, S2, S3, S6, S7, S9.

2. Lines 216-218:

a) The Tippett results are specifically about the "invisible" ship tracks method utilizing air mass tracking, not all track methods (like those picking out visibly identified tracks). I'd suggest adding something like: "More recently, Tippett et al. (2024) showed that the W response onto aerosol perturbations from ship emissions is weak on average, after correcting for biases in prior research <based on tracking ship-affected air masses (Meinshausen et al., 2022, 2023)>, related to correlations between wind and cloud properties." Probably also worth citing Toll et al. (2019).

Reply: We rephrased this sentence as suggested (lines 226-228).

b) I'd recommend briefly discussing the visible track literature as well given the scope of the statements you're making. There are numerous possibilities, but the most comprehensive for visible tracks globally would be: Toll, V., Christensen, M., Quaas, J., and Bellouin, N.: Weak average liquid-cloud-water response to anthropogenic aerosols, Nature, 572, 51-55, 10.1038/s41586-019-1423-9, 2019.

Reply: The main conclusion of the Toll et al. (2019) study regarding the effect of visible ship tracks on *W* was added in this discussion (lines 228-299).

Reply to Anonymous Referee #2

The authors addressed most of my concerns. However there are a few important lingering aspects that were not fully resolved in the revised manuscript.

1. Abstract: It is not sufficient to say that the contribution of this manuscript is the use of geostationary cloud retrievals while listing findings that are also reported in other articles. Again, the abstract needs to document the novel contribution of this paper. In reply you did mention some new results, which should be added to the abstract.

Reply: We have included these additional novel contributions in the revised abstract.

2. I still do not see a statistical analysis that tests the statistical significance of the magnitude change over the shipping corridor. This analysis of the signal significance is different from the uncertainty analysis they provide. Basically, a statistical test needs to be applied to demonstrate that the changes over the corridor are statistically different from zero. Standard procedures can be found in statistical and climate analysis books (e.g. Wilks et al., von Storch & Zwiers).

Reply: A statistical significance analysis of the corridor effect, separately from the uncertainty propagation, was indeed missing. We have now included it in the time series average, monthly effects, and the profiles of the corridor effects before and after 2020 (lines 171-173, 206-208, 216-217, 232, 268-269, 311-313, 326-328, 385-387 and Tables 1 and 2).

Other comments

Line 133: daytime instead of day-only?

Reply: We changed day-only to daytime (line 121).

Line 233: Strong inversion would make cloud -op entrainment less likely, so it is not easy to reconcile the idea of strong inversion and enhanced entrainment, it is conflicting.

Reply: The referee is right, that our phrasing is inaccurate. The first part of the sentence is a general characteristic of stratocumulus clouds, namely that they have a strong inversion at the top. The second part of the sentence describes the consequence of an increase in droplet number concentration: increased entrainment and decreasing liquid water path. We removed the first part of the sentence and slightly reformulated the second part (lines 221-222).

Line 252: "driven by a modification of the air circulation across the corridor", what physical mechanism could explain this process?

Reply: This phrase was added to clarify how the presence of the corridor could have an effect on its sides. Across-corridor circulation patterns have been described before for different mechanisms (Wang and Feingold, 2009). In our case, we do not want to speculate on mechanisms that cannot be justified based on our analysis. Therefore, this phrase is removed (line 244).

Figure 4: is there an annual cycle in the shipping activity?

Reply: Unfortunately, we do not have seasonally resolved data related to shipping activity to answer this question.

Line 331-332: Changes during the daytime are strongly modulated by shortwave radiative heating. Contrary to the explanation provided by the authors, entrainment rate peaks during nighttime.

Reply: Both our first attempt to give a plausible explanation of the slight decrease in r_e during the day, and our second attempt to clarify it, caused remarks by two Referees pointing to possible misconceptions and inconsistencies and highlighting the complexity of the issue. Given that this explanation is not crucial for the main findings of the study, and to avoid possible inaccuracies, we decided to remove this sentence (lines 331-334).

Line 348" This contrasts with"

Reply: Corrected (line 349).

Line 354: "however the latter…" what is the latter. It is not clear from the sentence.

Reply: With “the latter” we mean observations in more frequent time intervals, such as CLAAS-3. We have replaced “the latter” with “diurnal observations” to clarify this (line 354).

Reply to Anonymous Referee #3

The authors have made a thorough revision of the manuscript. I believe this work represents an advancement in the field with respect to the analysis of the diurnal cycle using geostationary data.

In my initial review, I pointed out that the lack of response in τ is intriguing, as it might suggest, for example, that the Twomey and LWP effects cancel each other out. If this is the case, it is particularly interesting, as radiative forcing is ultimately what matters. In the revision the authors addressed this point by performing an analysis of τ as well as of the effective cloud albedo. The results for cloud albedo were similar to those for τ, as expected. What does this imply for the radiative impact of the shipping corridor? I think that this point should be emphasized in the discussion.

Reply: The absence of any apparent impact on τ and the consequent implication on the radiative impact are now mentioned in the summary and conclusions section, as well as in the abstract (lines 15-16 and 460-461). However, given the discussed limitations in our methodology, we refrain from definitely concluding that there is indeed no impact on these parameters (see also our reply to the comment below).

One more point on that regard is that the authors suggest in the revised manuscript that the absence of an apparent corridor effect in τ could indicate a limitation of their methodology. If so, I wonder how does this limitation apply to the rest of their results.

Reply: We imply here that our method will not work well if the corridor effect does not manifest over a smooth (across-corridor) background. In this case, while the ship emissions may still have an effect on τ, this effect cannot be quantified using our methodology. When a smooth background is present, we are confident that our method provides meaningful results on the quantification of the corridor effect.

Please note that a correction is needed in the caption of Figure S4 regarding τ. Also, in 372 of the track changes you should add S before Figs. 7.

Reply: We corrected both.

I also want to comment on the authors' response to my remark from the first review: "Line 252-256: Can you provide a reference for why cloud thinning would lead to a smaller re?." - Thin Sc clouds do not necessarily indicate limited cloud growth, they could also result from cloud dissipation. In such cases, the cloud particle size does not necessarily increase with height, and the clouds are not adiabatic. This might affect your interpretation.

Reply: Thank you for this remark. The intension of that sentence in the first submission (“Liquid r_e decreases slightly in the morning (Fig. 5c), probably due to the overall thinning of clouds”) was to provide a plausible explanation on the slightly decreasing r_e during the day. Two Referees pointed deficiencies and possible misconceptions in both this initial phrasing and our attempt to clarify it, highlighting the complexities of the underlying mechanisms. Given that an explanation of this plot is not crucial for our main findings, we decided to remove this sentence (lines 331-334).