

Overview

This study evaluates the role of secondary ice production (SIP) in simulations of orographic cloud systems using available observations. The authors examine two independent cases—a Convective Case and a Stratiform Case—and compare a CONTROL simulation without SIP to an ALLSIP simulation that includes four major SIP processes. The authors conclude that including all four processes substantially improves simulation fidelity. However, the results do not support that broad conclusion. I appreciate the use of available observational data, which is too often very limited to constrain SIP using ground-based measurements alone. However, the manuscript cannot support claims of improved ice crystal number concentration (ICNC) without an observational benchmark. Improvements in radar reflectivity appear limited to particular portions of the cloud and are accompanied by compensating errors elsewhere. The claimed improvements in cloud structure are also difficult to assess because the evaluation is largely subjective and not statistical. The manuscript would also benefit from substantial organizational revision. It relies heavily on supplemental figures to support and explain the main results; material essential to the argument should be moved into the main text. The Conclusions are particularly problematic as they overstate the evidence for improvement, repeat points made earlier in the section, and provide too little discussion of how the findings relate to the existing literature. The manuscript therefore requires significant revision before it is suitable for publication in ACP. My comments are organized into Major Comments, Minor Comments, Technical/Semantic Comments, and comments on the Supplemental Material.

Major Comments

1. **Overall message:** The central message should be significantly recalibrated to match the results. Although more complete representations of physical processes may be expected to improve the simulations, such an outcome is not guaranteed. In these two cases, enabling SIP appears to produce modest changes relative to observations (where observations are available for comparison), and some apparent improvements seem to be compensated for by errors elsewhere in the cloud system. This result is indeed useful, as it documents the behavior of these parameterizations in cloud regimes that have not yet been thoroughly evaluated. However, the manuscript should present these results as a measured, largely neutral result unless the analyses demonstrate a clear overall improvement. In particular, the word “substantial” is used repeatedly throughout the manuscript, even though the differences between CONTROL and ALLSIP are often rather small. I request that such wording is tempered with a more neutral tone.
2. **Statistical robustness:** Given the rather small differences yielded by ALLSIP relative to CONTROL, this study would benefit from some more statistical comparisons that establish robustness. As one example, the distributions of LWP, IWP, and LWC could be tested with the Kolmogorov-Smirnov test to determine if the distributions of CONTROL and ALLSIP differ in a statistically significant manner. There are

numerous areas that would benefit from more quantitative comparisons as opposed to time-height cross sections and subjective qualification.

3. **Use of supplemental figures:** The manuscript consistently relies too much on supplemental figures to explain the results, oftentimes spending entire paragraphs discussing the supplemental figures. For example, Section 3.3 opens with the reader being required to consult the supplemental material for interpretation and discussion. Supplemental material should provide only supporting detail rather than evidence essential to follow the main argument. Please identify which results are central to the paper and move the corresponding figures or information to the main text. Specific suggestions appear below.
4. **Contextualizing your results:** Please explain more directly why the SIP processes evaluated here are expected to matter in this cloud regime, and how that expectation compares with studies of other cloud regimes. For instance, the limited contribution of drop shattering may be consistent with a scarcity of large supercooled drops, which are unlikely to be produced in this case due to limited warm-rain collision-coalescence. More broadly, the manuscript cites studies of other cloud regimes but does not contextualize how those mechanisms or outcomes compare. A focused discussion, probably in the Conclusions, is warranted to understand which findings may be regime-specific and why.
5. **Characterization of the stratiform case:** When the stratiform case is first introduced on Lines 259-264, it is described as favoring shallower clouds reaching about 6 km AGL. However, Fig. 8 shows cloud-top temperatures reaching -55 deg C, and the reflectivity time-height series include rather high precipitation-level reflectivities (> 20 dBZ in observations and up to 40 dBZ in simulations) with continuous cloud structures from the surface through 8 km. The shallower clouds only really occur late in the simulation, which overall appears to include appreciable embedded convection and associated precipitation. Please clarify the intended case description and reconcile it with the full time-height evolution. If available, simulated vertical velocity distributions for both cases could help characterize and compare their dynamical structure.
6. **Duplicate figures:** Fig. 5 and Fig. 10 appear to be identical. Please address.
7. **Evidence for increased and improved ICNC:** The text claims a large increase in ice crystal number concentrations (e.g., Line 642), but the manuscript shows only time-height profiles and does not quantify the distributional shift. Please add a quantitative comparison, such as joint temperature-ICNC distributions for each simulation and a third panel showing the difference in probability density for each temperature-ICNC bin, or an alternative suitable summary. Please also explain why higher ICNC would constitute an improvement. Although readers familiar with SIP may know that simulated ICNC is often low relative to observations, not all readers will, and this

study does not provide an observational ICNC benchmark (which is okay, because such observations are hard to come by, especially for ground-based instrumentation).

8. **Conclusions and study limitations:** The Conclusions need to be substantially revised to remove repetition and state the limitations of supporting evidence. The second paragraph on Page 25 (Lines 672-679) largely repeats the preceding paragraph, and the first sentence of the final paragraph on Page 25 again repeats points already made. Please remove or consolidate these passages to make clearer and more separable conclusions. The revised Conclusions should address (1) uncertainty in the implemented parameterizations, (2) the absence of an observational ICNC benchmark—which is understood outside of targeted aircraft campaigns but should be acknowledged, with unsupported claims of improved ICNC removed—and (3) how the findings compare with studies of other cloud regimes, including plausible reasons for differences. The conclusions should also distinguish changes in simulated quantities from demonstrated improvements in accuracy relative to observations.

Minor Comments

1. **Abstract:** The abstract follows a very familiar pattern for SIP sensitivity studies (increased ICNC and IWP, reduced LWP/LWC, changes in precipitation, and a hierarchy of SIP mechanisms) but does not foreground the more distinctive findings of this work. This is not inherently a criticism of the study, because it is important for the SIP community to explore these parameterizations and varying mechanisms in various cloud regimes, no matter how significant the findings are. I would suggest revising it to emphasize what is specific to these cases that hasn't been done before, and honestly reporting the differences and compensating nature of the changes where appropriate.
2. **Aerosol Treatment (Lines 164-170):** I was pretty confused here until I read the SM. As currently stated, there is no context for the reader about if CCN & INP are transported or subject to nucleation scavenging. I understand the desire for brevity, but I think the highest points of the aerosol information provided in Section 1.1 of the SM should be moved to the main manuscript.
3. **Parameterization uncertainty:** Please state that this sensitivity studies is conditional on the parameterizations implemented in the model. Most SIP studies rely on a small number of formulations without established alternatives. For example, Phillips et al. (2017a,b) may be the only tractable option for collisional breakup that the community currently has. The results should therefore not be read as a comprehensive estimate of process sensitivity that is independent of parameterization uncertainty.
4. **“Namelist options” (Lines 204-207):** The model namelist options do not contribute to the scientific argument and are relevant only to RAMS-ICLAMS users (which will not be the majority of your readers). If retained, move them to the supplement;

- ideally, simply provide the full settings in the data repository for readers seeking reproduction.
5. **Figure quality:** All figures could be improved by using a higher resolution (DPI). As it stands, much of the font looks choppy and textured. The font is not necessarily too small, but the low resolution makes legends and axes difficult to read.
 6. **Supplemental figure discussion:**
 - a. **Fig S2:** The use of 2 entire paragraphs (Lines 301-319) to discuss a supplemental figure is not warranted. Please move the figure to the main manuscript or move this discussion to the supplement. My suggestion is that this discussion is not important to the study and can be moved to the supplement.
 - b. **Figs. 4,9 & Fig. S5:** Figure S5 appears more informative for comparing reflectivity than Figs. 4 & 9. Figs. 3 & 8 already give the reader information on ice evolution, w/ Figs. 5 & 10 providing additional context. Consider moving S5a and S5b into the main text in place of Figs. 4 & 9, respectively, and moving the less informative figures to the supplement. This would also partly address the extensive discussion of the supplemental figure on Lines 366-383 and elsewhere. Please ensure that the revised main text contains the evidence on which the key claims depend.
 - c. **Fig. S6:** Discussed in too much detail to keep in the supplement. Perhaps combine it w/ Fig. 5 as a combined discussion w/ 3 panels.
 7. **Fig. 5, simulated LWC:** Please clarify whether the simulated LWC is a surface quantity or some type of vertical spatial average. If it is averaged, specify the averaging area and whether it is the same as the 5 x 5 km² region used for IWP & LWP.
 8. **Fig. 5 discussion (Line 441):** Please acknowledge that the peak LWC in ALLSIP is actually greater than in CONTROL. More generally, the CONTROL-ALLSIP differences appear very small relative to the discrepancy between either simulation and observations. That contrast is important and should be stated clearly. Statistical summaries would help establish whether the differences are robust; at a minimum, please report RMSE & MAE for LWC for each simulation relative to observations, with confidence intervals for the reported means and whether the differences between CONTROL or ALLSIP really matter. The differences are so small that I wonder if a CONTROL simulation with simple perturbed boundary layer thermodynamic variables (i.e., adding white noise) would produce the same magnitude of difference between the current CONTROL and a perturbed CONTROL as the magnitude of the current CONTROL vs. ALLSIP. I'm not suggesting you do that, but I'm not confident that the signal here is really that robust and may be as large as if initial conditions were minutely perturbed.
 9. **Figs. 6 & 11:** Please consider adding probability density functions of the production rates. The current vertical placement of the contours is useful to identify temperature

regions where the individual processes are active, but they provide no real quantitative comparison of the distributions and where they peak in their production rates.

10. **On the limits of DS production:** When discussing the lack of sensitivity to the drop-shattering process, please provide some physical context for the limited influence on ICNC, such as the lack of sufficiently large supercooled drops that this thermodynamic cloud regime does not support.
11. **Figure captions & legends:** The captions often repeat information already shown in the legends. Please shorten them where possible while retaining details needed to interpret the plotted data that is not redundant with the legends.
12. **Figs. S4 & S9:** On Line Line 509, the text describes Fig. S9 as showing the temporal evolution of total ICNC with values reaching about 60 L^{-1} . However, Fig. 8 shows values exceeding 1000 L^{-1} . This discrepancy does not make sense. Please clarify what quantify Fig. S9 shows and how it is calculated. Please check Fig. S4 for the same issue and clarify what “total ICNC” means here.
13. **Claims of improved spatiotemporal structure (Lines 534-536, 557-580, 559-560, 643):** Please explicitly identify what aspect of the spatiotemporal structure is better represented in ALLSIP than in CONTROL. Reduced reflectivity in some regions may be intended, but the cloud-layer boundaries do not appear meaningfully different, and changes at some heights seem to coincide with worse agreement elsewhere. If a specific improvement is supported, please point readers to the relevant feature and quantify it; otherwise, revise the claim.
 - a. **Reflectivity profiles (e.g., Line 643 & discussion of Fig. S5):** I do not believe that you can claim improved reflectivity profiles relative to CONTROL. This only occurs in the mean in the lower section of the cloud, with compensating errors higher in the cloud and worse agreement in the maximum profiles. This claim should be omitted.
 - i. **Line 385:** Is this a substantial reduction in bias? I know reflectivity is logarithmic, but the MAE & RMSE decreases do not appear to be super substantial. Also, I don't really see the value in reporting a bias reduction of a single reflectivity value. The profile structure matters here, where the only real improvement is below 3 km in the mean profile. Otherwise SIP only acts to increase bias (either above 3 km or in the maximum). I think declaring an improvement in the bias of a single averaged reflectivity value is very misleading here.
 - ii. **Lines 390-391:** On the statement “introducing a tendency toward enhanced reflectivity”. The way this statement is presented implies that this should be a target for improvement. The reflectivity profiles in Fig. S5 clearly show that enhancing reflectivity relative to CNTL is only valid for a portion of the profile.

- b. **IWC (Line 408):** I do not see “substantially enhanced” IWC. It looks rather modest according to Fig. S3. The maxima is indeed doubled, but I wonder what a PDF of IWC would look like. It would be more informative than these maps. I’m not convinced the distribution is actually that different and rather just the tail is changed.
14. **Reflectivity profiles:** Please describe how mean reflectivity profiles are calculated, including whether reflectivity is (appropriately) converted to linear units before averaging and then returned to logarithmic units. In addition, because reflectivity means are often strongly skewed towards the higher values, please consider adding percentile profiles (e.g., the 50th, 90th, & 99th) in addition to or in replacement of the mean and max. If Fig. S5 is moved into the main text as I suggest, these profiles could be presented as companion panels.
15. **Fig. 7:** The colors make the CBR mechanisms difficult to distinguish from the other three structurally different mechanisms. Consider assigning CBR processes as different shades of one color or using a common light color with distinct hatching for each sub-CBR-mechanism. In the legend, please prefix each entry with “CBR:” (for example, “CBR: graupel/hail on graupel/hail”) to identify the associated CBR process clearly as a sub-mechanism of CBR. As it currently stands, it is difficult to discern which processes are sub-mechanisms of CBR versus the other three structurally different mechanisms.
16. **Fig. 11 & discussion (Line 606):** The text identifies CBR as dominant, consistent with Fig. 7b, but Fig. 11 does not make this apparent. The most visible contours appear to be the pink/magenta SUBBR contours, which Fig. 7b indicates are a small fraction. Please check whether the purple CBR contours are simply obscured by the filled-contour color map that also includes purple hues. Please choose contour colors for the different mechanism rates that are sufficiently distinguishable from the colors used on the ICNC color map.

Technical/Semantic Corrections

- **Lines 21, 61, and elsewhere “droplet shattering” is used:** Generally, “droplet” is used to refer to a suspended cloud hydrometeor and “drop” is used to refer to a precipitating hydrometeor. I would change “droplet” to “drop” here, since the drop-shattering process is generally associated with precipitation-sized drops.
- **Line 41:** Saying “especially in the warmer temperatures below -15 deg C” is confusing, because some would interpret “below” as “less than”, meaning you’re stating temperatures *colder* than -15 deg C. I would rather state it as “especially for temperatures warmer than -15 deg C” or “especially in the warmer temperatures at altitudes below the -15 deg C isotherm).
- **Line 48:** Typo: “attribured” should be “attributed”

- **Lines 49-50:** Starting with “Although” and then using “yet” is a redundant conjunction. Remove one or the other.
- **Line 61:** No comma needed after “pair”
- **Line 61:** Note that this process is not limited to collisions between a drop and an ice crystal, just that the drop is triggered to freeze, which can indeed be due to immersion freezing triggered by INP.
- **Line 81:** It is not typical (nor intuitive) to introduce “ice water path” as “ice water content path”. I would just say “ice water content” here since the column-integrated quantity is not relevant for this argument.
- **Line 110:** The introductory phrase here makes it sound like your study too will be subject to such limitations. I think you actually mean something like “To address these limitations” or something like that.
- **Line 111:** Most readers are going to consult the Introduction and then skip to the Conclusions. Therefore, I’d recommend a short phrase here after introducing the CHOPIN campaign to state where the campaign was held (Mount Helmos, Greece). I know you state this in the first sentence of “Methods”, but many readers will skip the methods, so stating that earlier will be informative for those casual readers.
- **Line 119:** Since “objectives” is plural, “was” should be “were”
- **Line 129:** There should be a semicolon or period before “however”. A comma is not appropriate.
- **Lines 130-131:** Saying that changes in the distributions and number concentrations are directly reflected in the radar signal, as a defense for only looking at reflectivity, is ambiguous. I think you mean that changes in the number concentration and PSD shape affect reflectivity (primarily by the PSD due to the 6th-moment influence of particle size). Either way, this needs to be rephrased. “Radar signal” is not a synonymous term with “reflectivity”, which is a moment (as you state earlier).
- **Line 142:** Maybe add “chamber” after “(PINE)”?
- **Lines 134-135 & 148-149:** I’m a little confused by the instrumentation description here. It is not clear where the in-situ cloud microphysical measurements (lines 134-135) are located. Are they on the surface, or are they on the Helikite platform (line 149)? And what did the UAV probes sample? Was it relevant to this study? It’s probably not relevant to list instrumentation that isn’t pertinent to the study-at-hand.
- **Line 161:** You need to explicitly state here that this is a bulk microphysics scheme. I didn’t know that until I looked in the SM, and actually assumed it was a bin scheme since you said “resolves eight hydrometeor types.” To that end, I would actually say “represents” instead of “resolves” since the PSDs are not explicitly resolved.
- **Lines 172-173:** “may subsequently SIP” doesn’t make sense; do you mean “may subsequently modulate SIP”?

- **Line 198:** I know what you're saying by " $\Delta z_{\max} = 400 \text{ m}$ " but it is appropriate to textually define any symbolized parameter you introduce.
- **Line 210:** Please state what you mean by "fine mode" and "coarse mode". For example, are they represented by lognormal distributions with a geometric mean diameter and standard deviation? What are those representative diameters? I also don't think you describe in the main text or the supplement how aerosols are activated.
- **Line 205:** Please be more specific than just stating "the formulation of DeMott et al. (2010)". Since this is an integral part of this study, it should be stated how PIP occurs without just a reference.
- **Line 211:** So are only the surface values set to 600, 0.01, and 0.003 mg^{-1} , and there is an immediate exponential decrease with height, or is it constant to some degree through the boundary layer before that decrease starts?
- **Table 1:** If any variables are used in the table (e.g., Δx , Δz_0 , etc.) these should be explicitly defined in the Table caption.
- **Lines 219-220:** Please define what you mean by "characteristic size" here; it could be mass-weighted diameter, number-weighted diameter, median volume diameter, etc., and those details matter.
- **Lines 221-223:** Why are liquid hydrometeors required for the CBR process? Aren't these just collisions between 2 ice particles? I may need to remind myself of the Phillips et al. (2017a,b) parameterizations, but this inherently seems confusing.
- **Lines 249, 250:** "namely" here should be "named"
- **Line 255:** I would say that temperatures in the low core *at 500 hPa* reaching about -33 deg C. Same for line 260.
- **Line 258:** There is a blank parenthetical here. Please address.
- **Line 284:** "in-situ cloud radar" doesn't make sense. Radars are, by definition, remote-sensing, not in-situ. Also, you have "cloud radar" before and after "MIRA" which seems redundant.
- **Lines 284-288:** It seems you have only simulated the MIRA radar (~35.4 GHz) when the beam was vertical. So why even mention MXPOL and BASTA in Section 2.1? Probably no need to include unnecessary information.
- **Line 303:** When you say that "the model predicts drier conditions during the 12/11 00-08 UTC", do you mean between 11/11 18:00 UTC – 12/11 00:00 UTC? Based on Fig. S2, this looks like the temporal period where the model is too dry.
- **Lines 330-332:** You really don't need to state in the text all of these details that can be understood by looking at the figure and the legends, for example stating the logarithmic scale and ranges of the values on the colorbars. These are intuitive to the reader by looking at Fig. 3 and instead clutters the discussion.

- **Line 335:** Again, it is a bit confusing here to say “above the freezing level”, because it’s not super clear when you’re referring to temperature or altitude when you say “above”. I would state it rather as “resides at temperatures below freezing” or something like that.
- **Figure 4:** Why are there 6 panels instead of one long continuous panel for each row? It’s confusing to present it this way because separating the full 2 days into two panels implies that there is something fundamentally different about the different panels in each row.
- **Line 384 & following discussion:** What do you mean here by “statistical evaluation”? The text references some absolute values and error statistics, and suggests that these tests are performed on the time-averaged reflectivity profiles. Is this mean value just computed from the profiles? Wouldn’t it be more informative to show the error statistics at each vertical level? Either way, the method here should be more explicitly defined.
- **Line 390 and other references to “sedimentation-related features”:** I’m not sure what is meant by “sedimentation-related features” within the context of SIP. Is sedimentation relevant here because it’s necessary to perform most collisions that induce multiplication (assuming here that gravitational collisions are more important than turbulent collisions, at least)?
- **Line 429:** I would reiterate that these are differences in LWP here. Also, why not just state succinctly that LWP decreases by about 10% in both peaks?
- **Line 456:** “where temperature ranges in” should be “where temperatures range from”, though this is probably a moot point since the parameterization is designed to only operate in this range.
- **Line 546:** “During” implies a temporal perspective. I think you just mean “In the lowest...”
- **Lines 583-586:** The text states units of g/m^2 here to for LWP & IWP, but surely it should be kg/m^2 , as Fig. S10 has, right?
- **Line 589:** Again, I would tone down the language here. “Profound” is too aggressive based on what I’m seeing. It is fine to just state the statistics and let the reader interpret the significance based on the percentage difference.
- **Line 612:** The text uses “DRS” here but the legend has “DS”, with the latter being what is used throughout the rest of the manuscript.
- **Lins 612-614:** Regarding the DS process playing a more limited role, is this true? The cyan contours are *much* more noticeable in Fig. 11 compared to Fig. 6. And actually, looking at Fig. 7, there is a sliver of the pink/magenta DS shading in Fig. 7b that is nonexistent in Fig. 7a.
- **Line 634:** I do not understand what is meant by “consistently exceeds the experiment”. Please clarify.
- **Line 639:** I do not think it’s appropriate to say “successful” here. Also, in the opening sentence of the Conclusions, I would not introduce these SIP mechanisms with their

abbreviations and would instead spell them out, and I would state that these are implemented in addition to the Hallett-Mossop mechanism.

- **Lines 651-652:** This sentence is a complete repeat of the prior sentence.

Supplemental Material Comments

- **Line 10:** I would not say “such as” at the end of this line, because that implies you may not be mentioning all of the prognosed properties. Please be explicit in saying which species are single moment (mass only) and which are double moment (mass and number).
- **Line 16:** “with” should be “within”
- **Line 41:** “where F_{ρ} being” should be “where F_{ρ} is”; Also, I believe $E(x,y)$ should be the collection *efficiency*, right?
- **Line 54:** “very important the” should be “very important since the”
- **Line 58:** Suggest “mechanism” or “process” after “(HM)”
- **Line 65:** I’m confused by this notation. What exactly do the subscripts “S” and “L” refer to? Since rime-splintering occurs only for droplets smaller than 24 microns in diameter, I do not understand the parenthetical notation for whatever types of particles are larger than that size.
- **Line 67:** “interpolating” should be “interpolation”
- **Line 77:** I assume “rimming” is a typo for “riming”.
- **Line 77:** You say “as mentioned in the previous sections...” but none of this is actually mentioned in the previous section.
- **Line 89:** Can you put “A” in italics or something to make it more clear this is a parameter? Also you have a repetition of “with” at the end of the line
- **Line 90:** Again, “rimmed” fraction? Do you mean “rimed” fraction?
- **Line 91:** “presented on Table S2” should be “presented in Table S2”
- **Line 93:** There should be a comma after “Lastly”
- **Line 98:** “These constraints arise for” should be “These constraints arise from”
- **Line 102:** There should not be a comma after “Gautam et al. (2024)”
- **Line 106:** “Eq.” should be “Eqs.”
- **Line 108:** “, meaning those involved in collisions that do not result in collection” is confusing. Do you mean that some fraction of those involved in collision do not result in collection?
- **Footnote of Table S2:** You have not defined what “spatial planar” means up to this point.
- **Lines 138, 143:** “rain droplets” is confusing. Please use “rain drops” if you’re referring to precipitating hydrometeors or “cloud droplets” if you’re referring to suspended (non-precipitating) hydrometeors.
- **Lines 142-144:** Please state whether the drop-shattering process is induced by both immersion freezing and drop-ice collisions or just the latter.

- **Line 145:** “Droplet shattering” should be “Drop shattering.” Also, the acronym has changed here from “DS” in the main manuscript to “DRS” in the SM.
- **Lines 155-156:** “rain droplet” should be “rain drop”
- **Line 158:** “for larger droplets” should be “for larger drops”, especially if you’re talking about the millimeter-size range
- **Line 175:** “is specific latent heat of freezing” should be “is the specific heat of latent freezing”
- **Line 195:** “are fitting parameter” should be “are fitting parameters”
- **Line 196:** By saying “with respect to ice, and” you are implying that your list has concluded, but then after “Pruppacher and Klett, (1997)” you continue the list, so “and” should be moved to before the last item of said list.
- **Line 200:** I’m pretty certain now that by “rimmed” you mean “rimed”,.
- **Eqs. 16-17 & associated discussion:** You never state what the caret character means above “N” in these equations. Please do so.
- **Line 204:** There should not be a comma after “fragmentation rates”
- **Line 205:** I’m assuming you do not intend to have an underscore between “rho” and “r” and that you meant to use “r” as a legitimate subscript.
- **Line 207:** “span across” is redundant. You can simply say “span” here.
- **Line 215:** “cubic interpolating functions” should be “cubic interpolation functions”
- **Line 217:** “Eq. 16-18” should be “Eqs. 16-18”, i.e., the plural form
- **Line 225:** Do you mean the sublimation *mass* mixing ratio here?
- **Fig. S2:** The caption says simulations are dashed, but they look dot-dashed. In general the caption is confusing because you listed the observed wind speed as blue markers, but it’s hard to see any blue makers and how those may be different from the blue line itself.