The manuscript is focused on approximations of the inversion strength in situations when low level clouds occur. These approximations are designed to estimate the real inversion strength using relatively coarse spatial and temporal resolution datasets such as a reanalysis. Two metrics in particular have been proposed as a means to relate cloud cover to the prevailing meteorological conditions, that relate to inversion strength, one is the Lower Tropospheric Stability parameter (LTS; Klein and Hartmann, 1993) and the other the Estimated Inversion Strength (EIS; Wood and Bretherton, 2006). LTS is the difference in potential temperature between 700 hPa and the surface while EIS is a modification of LTS where the potential temperature change along the moist adiabat above the lifting condensation level is removed. The authors discuss situations where neither LTS nor EIS are a good fit for the real inversion strength and propose a correction to EIS where the variations in potential temperature below the LCL are removed to avoid a strong deviation from the real inversion strength in situations when the PBL is decoupled, as well as above the inversion to avoid errors caused by deviations from the moist adiabat over a relatively wide distance above the LCL. Using high resolution radio soundings at a site in continental United States, they demonstrate how their estimated inversion strength 1) performs better than EIS or LTS for characterizing the actual inversion strength and 2) offers a stronger correlation with low level cloud cover. They then explore how the three metrics relate to clouds geographically as well as for different temporal resolutions. The whole analysis is extensive, very thorough and captivating, but covers a lot of grounds and can be overwhelming at times. However the demonstration and justification for this new parameter is convincing and will be a useful addition in the quest for the most important cloud controlling factors. The figures are well done and compelling while the verification process precise and careful, so in essence, this is a good manuscript and there is no need for additional scientific work. But to become really accessible, the presentation of the results could be much improved and should be simplified with some reorganization.
Therefore before the manuscript be accepted for publication, there are a number of improvements to be made. My main comments and suggestions are as follows, with more specific comments listed below:

1) While the work is logically presented, there are some presentation issues. There is quite a lot of ground covered by the paper, maybe this could be streamlined. There are two aspects in this analysis that are covered: 1) which metric can approximate inversion strength the best and 2) where and at which temporal resolution does inversion strength correlate with low level cloud cover. The separation between the two should be clearer to help follow the narrative, and avoid the repetition of how much of an improvement the EISp metric is.
2) The definition and construction of the new inversion strength estimate proposed here is very confusing, I strongly encourage the authors to rethink their explanations in section 3. In fact this crucial section could be much improved if it is divided into a first part that explains the theory behind EISp and how it is calculated. Then a second part that explores how it relates to inversion strength, and a comparison with LTS/EIS respective relations to IS using the SGP data. Finally this can then be expanded with a comparison to IS using the IGRA and ENA data. Once you have made the case that EISp is a superior measure of inversion strength, you can move on to a section 4 that explores its relationship with clouds.

3) There is little discussion of the expansive work and knowledge accumulated on the physical reasons for a strong link between LTS or EIS and cloud cover, so sometimes it seems that the fact that this link exists comes as a surprise. I made a few suggestions below of papers that would help in the interpretation of the results, some are already cited.
4) The distinction and classification of coupled, decoupled and clear sky profiles is confusing, there are different metrics used and these appear at different points in the manuscript, which is quite confusing. Because this distinction is at the center of the whole argument in favor of EISp, the matter merits its own section. A subsection in section 2 dedicated to the subject would be a helpful addition and thereafter the method would not need to be explained again.
5) The language can be confusing at times, and if simplified this should help a great deal follow the argument. Some suggestions of changes are given in both lists below.

Specific comments:

1. Line 9: "At the southern great plains site" needs some additional information such as where it is, and which agency/project runs it. Given this is the abstract, it might be easier to replace here with something similar to "at a ground-based site in north America" for example.
2. Line 53-54: if inversion strength is not well predicted by LTS or EIS at high temporal resolution but quite well at longer time scales, what could be the reason? This should be discussed.
3. Line 56-57: this sentence is unclear, especially "may not be conserved but with a stable layer". Please rephrase.
4. Line 70: "ARM" has not been introduced yet, what is it and which agency runs this? For the title of the subsection I suggest replacing "ARM sites" with "ground-based sites" or a short introduction of section 2 detailing the sort of data to be presented would be great and it could be used to introduce ARM etc..
5. Line 71: so here "SGP" is used, I do not think it has ever been said it means Southern Great plains. Also why use this site? It is inland and therefore quite different from the environments usually associated with low clouds such as the subtropical stratocumulus regions. Why isn't ENA your primary site instead? This needs to be explained at the outset.
6. Line 72: "the ARM" is odd, just "ARM" would suffice. But it should really be "established by the Atmospheric Radiation Measurement program of the Department of Energy", unless it is already said in an intro (see comments above).
7. Line 95: do you keep profiles that have low clouds but with other clouds aloft also present?
8. Line 97-99: here you mention separating coupled and decoupled PBL conditions but do not explain how it is done until much later in the paper. So either explain the method here or refrain from introducing the classification until it becomes relevant.
9. Line 102-103: the sentence "our results…" is unclear and incorrect. How about replacing with "this will be verified and discussed later".

10. Lines 103-107: finally the reason for choosing SGP over ENA is explained but the explanation is confusing. And sounds very ad hoc and not very scientific. I think that your point is that the work focuses on the strong relationship between cloud cover and inversion strength, but this relationship is not true everywhere and in particular it is not observed at the ENA or weak. But it is found at the SGP, making this location a good place to explore a refinement of the inversion strength estimate. I wonder why the ENA site is mentioned at all?
11. Line 115: another acronym that is not defined: "OWS"
12. Line 170: "as the height of three fourth of the greatest …." Needs to be rewritten, it is really hard to understand what is done.
13. Line 172: "these two methods both works" is incorrect, consider replacing with "these two methods work" or "these methods both work", BUT either way, it is confusing: does this mean the two methods give the same result? Which method do you actually use? Why mention two if you only use the one?
14. Line 176-179: the technique used to get the inversion strength for coarse vertical resolution radiosoundings is confusing, please clarify.
15. Line 188: replace "in consistent with that" with "as"
16. Line 193: as for section 2, a short intro in section 3 would help situate where the analysis is going next and why.
17. Line 224: why use "180 m" and not "150m" as in Jones et al? not clearly explained.
18. Line 225: so is this method the same as that used in Jones et al 2011 or something else? If it is then just say so. If it is not the same, then justify it.
19. Line 228: "large positive skewness": not sure I understand why this is? This should be discussed.
20. Line 230-231: this sentence is not entirely clear nor correct. By construction, LTS and EIS will include the term (theta_LCL-theta_0) regardless of its value. What you're trying to say I think is that in situations where this term is not zero, such as clear or decoupled PBLs, this will cause LTS and EIS to deviate from the real inversion strength value. No? Please rephrase accordingly.
21. Line 232: "even in the coupled PBLs: do you mean "decoupled" instead? Or is "even" misplaced? Unclear.
22. Line 241: "could easily overwhelm the real IS" is weird. What you simply mean is that this term will cause an overestimate of the IS estimated with LTS or EIS, so why not rephrase this sentence with this in mind.
23. Line 259-260: this sentence seems very obvious, no clouds present suggests adverse conditions for cloud formation indeed. Not sure what the point is?
24. Figure 2 and discussion: this is for all seasons for the full period with SGP data, correct? Might want to remind the reader.
25. Line 282-283: I wonder if the better relationship between RH and LTS compared to EIS does not come from the fact that EIS is meant to approximate inversion strength so necessitates that an inversion be present, when LTS is meant to represent static stability and can still relate to clouds when there is no inversion (or very weak). Have you tried to check LCC vs EIS by separating situations with IS >0 from those with IS=0 or <0?

26. Line 339: for the ERA5 vs SGP sonde comparison, what is the time resolution of the ERA5? 3-hourly or hourly? Is the same time of day used?
27. Line 343: “while decreasing RH…” is unclear, please clarify.
28. Line 346: language not very scientific, replace “does a better job on estimating the IS” with “offers a better fit to the real IS”
29. Line 373: language not very rigorous, replace “not bad” with e.g. “remarkably close to that explained by IS”
30. Section 4: again an intro would help follow the narrative.
31. Section 4.1, title not exact, only a few locations are chosen, this is not true worldwide, please rephrase.
32. Table 2, caption: what does “excluding 7-day means” mean?
33. Line 390: “globally” is again too vast, maybe “in other locations” or “in maritime locations” would be more appropriate?
34. Line 391: here the coupled vs decoupled distinction is done again, and cloud base height is mentioned (as having been used earlier). This was not clear. Maybe best to always use the same definition no? could this alpha parameter be tested for the high resolution soundings as well?
35. Line 396: unclear, is this parameter introduced by Wood and Bretherton? Or by you but happens to be consistent to what they had done?
36. Line 397: “the decoupling degree is in proportion…” is awkward. You simply mean that when alpha is zero the PBL is coupled, and when it is not, it is decoupled. Because a very small value suggest a state very close to coupled, you use 0.2 as a threshold to separate the two conditions. Please rewrite this paragraph in a similar fashion if my interpretation is correct. That being said, a physical explanation of what the formula (#12) is expressing is missing and should be added to help understand what it represents.
37. Line 424: I am not sure why the second sentence only mentions EISp, clearly there are no cloud data in high elevations regions, namely Tibetan Plateau and Andes (maybe others but these two are the most obvious) so just say so, the reader can see the analysis cannot be done in these areas for sure.
38. Line 425-432: this whole discussion of Fig 8 is unclear and hand-wavy at best. I cannot clearly see what the authors are trying to convey here. It might help to overlap the LCC contours (as solid lines) on the LTS/EIS/EISp maps and possibly draw boxes around the areas under focus here. But the whole paragraph needs clarification for sure.
39. Line 436: “poor fault tolerance” is unclear, what does this mean?
40. Line 438: language not very rigorous, consider replacing “but these will not happen with the EISp” with “The performance of EISp is less dependent on surface type” (assuming that this is what is meant).
41. Line 438-439: the whole sentence could equally say that LTS can explain LCC variations over land and EIS over ocean nearly as well as EISp. While it is clear EISp is performing better and more uniformly, your analysis still demonstrates where LTS and EIS have advantages and explains why they have been relied upon for quite some time.
42. Line 449/Fig 9: what is “all time scales” exactly??

43. Line 459: you could add reference to the Klein et al. 2017 review that summarizes all the work done on the environmental factors that impact LCC in addition to IS. And this diversity explains the non geographical uniformity of the effect these parameters have on LCC, and in particular explains your result that “IS-LCC relationship is not uniform but varies with regions even over oceans”. I would add that SST plays an important role and it is not uniform either.
44. Line 468: I would suggest revisiting the work of Klein and Hartmann (1993) to better understand why LTS and LCC are related despite LTS not being well correlated with IS (again LTS is not a direct measure of inversion but of static stability).
45. Line 490: what is sensitivity here, the slope?
46. Lines 496-498: I do not understand the point of this last sentence, just that EISp is the only metric that exceeds 80% of the variance explained for all locations? Please consider simplifying the sentence.
47. Line 525: again the sentence “Unfortunately..” is unclear, what is this discussing exactly? is little known about this because it has not been explored or the authors do not have a satisfying explanation? “Limited knowledge” implies there is some, so what is it?
48. Lines 529-530: it might be useful to remind the reader what the actual definition of LTS is and remind what KH93 had in mind. Also see Kawai and Teixeira (2010, JCLI, doi: 10.1175/2009JCLI3070.1).
49. Line 530-531: again, see comment above, IS is not the only cloud controlling factor, see also Myers and Norris (2013, JCLI, doi:10.1175/JCLI-D-12-00736.1), cited in your manuscript.
50. Line 564: “questionable” should be replaced with “not recommended”, but as conveyed in the comments above, there is a large amount of literature on the role of various meteorological metrics on cloud properties, I doubt that cloud feedback is estimated with only inversion strength in mind. But if it is, a citation should be added.

Typos/grammar:

1. Line 101: remove “oceans”, redundant.
2. Line 144: “away about 2.8km” is incorrect, replace with “within about 2.8 km”
3. Line 148: here and in other places, “metrices” should be “metrics”
4. Line 173: remove “s” at end of “jump” and “increase”
5. Line 208: “could compute” is odd, do you mean “would include”?
6. Line 209: “as the IS estimates” is awkward, do you mean “in the IS estimates”?
7. Line 209: Remove “in general”, awkward.
8. Line 229: “strength” is not great, how about “value”?
9. Line 297: remove “that” at the beginning of this line.
10. Line 320: replace “three” after “among” with “them”.
11. Line 341: “close” should be “closer”
12. Line 392: insert “parameter” at the end of the sentence (before the “:”)
13. Line 395&398: I believe “2004” should be “2006”, unless there is a missing reference in the list.
14. Line 488: “composites LCC to them” is odd, maybe “the slopes of the linear regression with LCC”?

15. Line 489: “besides” should be “in addition” I think
16. Line 499: “cross” should be “across”
17. Line 529: “the worst estimation of the IS” should be “correlates the least with IS”
18. Line 545: “wrong” should be “erroneous”