Overall Comment:

Many previous publications have shown observational links between tropical high clouds and the QBO. This study evaluates these connections in CMIP6 class models which can reproduce the QBO with some fidelity. Findings show several deficiencies in the model simulation of QBO influence on high clouds, particularly those in the TTL. By using a cloud-controlling factor analysis, the study also investigates the environmental pathways by which the QBO influences high clouds in both observations, and models. The study suggests that the largest reason for differences between models and observations is the modeled sensitivity of cloud fraction to the cloud controlling factor.

I found this paper enjoyable to read and generally found it easy to understand. However, there are a few locations and descriptions where the paper should be further refined. I would really enjoy a further explanation of the significance of the T_{UT} results, especially due to recent papers which have argued for the importance of S_{150} (Chen and Thompson, 2026). The paper as it currently feels like a lot of background before getting to any of the actual results. I thought the results were straightforward, so you may be able to cut a significant amount of the intro, data/methods, and preamble to Fig 1 in the results to have a more succinct story.

Major Comments:

1. This paper would benefit greatly from more discussion/analysis related to the statistical significance. Given that we only have 14 years of calipso data, and we are trying to test something related to the QBO, we only have ~6 independent samples (not accounting for complications due to the QBO disruptions). I know this partly motivates ridge regression as opposed to linear regression, but I still wonder if you can really fit 6 predictors to the model.
    a. When showing significance, are you accounting for autocorrelation?
2. Line 124: How is ENSO regressed out? Is it regressed out of the QBO index and all the latxlon datasets? Did you regress it out with a lag in the ENSO index? ENSO impacts on TTL clouds have been found to maximize after a few months (Tseng and Fu, 2017).
3. How is T_{UT} defined? Consider more clearly showing how you calculate T_{UT}, especially given its relevance in the later results.
4. Line 139: You change upper tropospheric stability to S_{150} related to the Chen and Thompson results. I am wondering if you should also change w_{300} and u_{300} to things more relevant for the TTL. For example, you might change w_{300} to w_{100} or u_{300} the shear between 300 and 100 hPa. Given that many of the thermodynamic changes driven by the QBO operate via changes in the vertical velocity in the upper-troposphere and lower-stratosphere, the w_{100} might be interesting to look at.
5. Equation 1: I found it difficult to understand what each term/symbol represented. Consider defining everything in the sentence immediately following. For example: "... where AMT represents the cloud fraction, r represents the individual grid cell, Θ_i represents the sensitivity of high cloud fraction to the ith cloud-controlling factor (X_i) and M is the total number of cloud-controlling factors (M=6)".
    a. Writing the equation for Θ_i as d(high_cloud)/dXi would have been helpful for me.
6. Fig. 1: I liked the comparison of models and observations here. It may be helpful to provide a difference row as well as the obs and modeled results.

    a. Consider reducing the hatching thickness as the current version feels too crowded
    b. Consider adding thin black lines as another contour at fixed levels to highlight changes in the magnitude
    c. For Fig S2, consider using the same colormap and colorbar as done in Fig 1 for ease of comparison
    d. For Fig S3, consider using same colormap and colorbar as Fig 1
7. L224: I noticed this at this point, but this is true throughout the paper. Consider reducing the emphasis on QBO westerlies reducing high clouds. While this is of course true, some studies have shown that the impact of the QBO on the TTL is actually much stronger during QBO easterlies (Tegtmeier et al., 2020). This is more subtle in this study because it is regression based, but may confuse the results. For example in L227-229, you state that the zonal asymmetries in the cloud impact are tied to QBO westerlies, when in reality, these are just the regression coefficients and may be dominated by easterly QBO variability
8. L294-295: This is a very interesting statement. I would like to understand this better. If this suggests that clouds respond separately to these 3 predictors, could you give more intuition or examples on how? More importantly, how do we square these results with the recent results of Chen and Thompson (2026) which seemed to argue that previous studies overstate the QBO-cloud coupling, and that studies should only look at the S_{150} variable? Is the flaw in that study that they only consider S_{150}? If so, it would be helpful to have some physical intuition on why T_{UT} is important. I know you expand on this slightly in L300-305, but having more discussion would highlight that stability alone is insufficient to describe cloud changes, and highlight the discrepancy with the Chen and Thompson study.
    a. I think Fig S6b shows that including all predictors with T_{UT} (Fig. 5b) gives a larger response than all predictors with T_{SURF} (Fig. S6b). Please correct me if I am wrong.
    b. How much of the variance between S_{150} is shared with Tut?
9. Fig 4: Given the importance of T_{UT} here, it should be included in this figure.
    a. Fig S7: I don't understand the T_{UT} sensitivity. Is it true that the reconstruction in Fig. 3b should be obtained by multiplying the pattern in Fig. S7aa? If I understand correctly, the positive values in Fig. S7a would suggest that an increase in temperature leads to an increase in cloud fraction. This seems opposite of what I would expect, and also contradictory to the results of Fig. 1. Please let me know if I am reading this incorrectly.
    b. Also, the T_{UT} sensitivity in S7 seems spatially disorganized compared to S_{150}. I think more interpretation of the results here would be helpful
10. L345-355: Could some of the zonal discrepancies between obs and models here be explained by differences in tropopause between obs and models? In observations, the QBO influence is much stronger above the tropopause, and much weaker below. If the tropopause is incorrect in models, this might prohibit QBO anomalies from extending into the troposphere. In observations the tropopause has longitudinal dependence, and this may be true for models as well.

11. Fig 5: Should the observation total in Fig. 5b be the same as the observational value in 5a? I am not familiar with the regression techniques used here, but if not, then does this change the interpretation of the role of T_{UT} from Fig S6?
12. L370-380: I noticed this here, but I think it is relevant throughout the paper. I am having trouble interpreting the %/std(QBO) units here. Sweeney et al. (2023) show variations in cloud fraction of about 2-5%. I know this is a composite difference compared to a regression coefficient here, but it still seems that there might be a discrepancy. Please correct me if I am misunderstanding.

Minor Comments:

1. L87: "...and so on" → "among other reasons (cite relevant paper)"
2. L98: "...underrespresentation of very thin clouds in CALIPSO-GOCCP compared to standard CALIOP-NASA products" → This seems like it could be important given that many of the TTL clouds are very thin. Is this something that may impact the results?
3. L159: Discussion on the QBO definition might be better for section 2.
4. L231: consider citing Tegtmeier et al. (2020)
5. L255: "with a proposed mechanism…" → "with one of many proposed mechanisms involving…"
6. L257: "Figure 2 indicates the strongest QBO-cloud relationship in observations occurs over the maritime continent…"
7. L257: Consider also citing Lin and Emanuel (2023)
8. Fig 3: Given that RH_{UT} is one of the three main contributors, consider moving it to row 3.
9. L311: "...do not align spatially in the MMM…" Do you mean that they do not align spatially with the observations or that the 3 predictors have less similar spatial patterns than those of the observations?
10. L322-323: My understanding is that this has been known from previous papers, consider citing them here.
11. L91–92: "(Swales et al., 2018**). has been developed" should read "…2018) has been developed."
12. L92–93: "generates outputs that is comparable" → "that are comparable."
13. L155: "20 years of MODIS data are also used from comparison" → "for comparison."
14. L257: "the QBO–cloud relationship in observations peak occur over maritime continent" → "…peaks over the Maritime Continent"
15. L355: "Figure S6 (first raw)" → "first row."
16. L401–402: "both cloud sensitivities and QBO modulation play part" → "play a part."
17. L403: "the cloud sensitivities to TUT and S150 varies" → "vary."
18. L104: "22 years of MODIS dataset are analysed" → "22 years of MODIS data are analysed." Also consider avoiding starting the sentence with a number.
19. L111: "global circulation models (GCM)" I am familiar with GCM meaning general circulation models, not global.
20. L24, L122 and throughout: inconsistent unit spacing ("10hPa," "150hPa," "440hPa") vs the spaced form used elsewhere ("150 hPa"). Standardize.
21. L53: "1 W/m2" → "1 W m^{-2}" (and superscript).
22. L124: "Niño3.4" → "Niño 3.4."

References:

1. Tegtmeier, S., Anstey, J., Davis, S., Ivanciu, I., Jia, Y., McPhee, D., et al. (2020). Zonal asymmetry of the QBO temperature signal in the tropical tropopause region. Geophysical Research Letters, 47, e2020GL089533. https://doi.org/10.1029/2020GL089533
2. Lin, J., and K. Emanuel, 2023: Stratospheric Modulation of the MJO through Cirrus Cloud Feedbacks. *J. Atmos. Sci.*, **80**, 273–299, https://doi.org/10.1175/JAS-D-22-0083.1.
3. Sweeney, A., Fu, Q., Pahlavan, H. A., & Haynes, P. (2023). Seasonality of the QBO impact on equatorial clouds. Journal of Geophysical Research: Atmospheres, 128, e2022JD037737. https://doi.org/10.1029/2022JD037737