Dear Dr. Endl,

We thank the anonymous referee very much for their constructive feedback. We have modified our paper in response to this feedback, which has materially improved the paper. We have **bolded** all changed text within the manuscript. Below, we present a point-by-point response to the points made by the referee (referee text italicized/unindented, our text unitalicized/indented). Further, we highlight a few changes we made to the manuscript independent of the referee’s feedback.

*Dear Editors and Authors,  
  
I have carefully reviewed the manuscript entitled "UV Spectral Characterization of Low-Mass Stars With AstroSat UVIT for Exoplanet Applications: The Case Study of HIP 23309" by Ranjan et al. and have concluded that it merits publication in the Astronomical Journal. I have several recommendations outlined below that I believe will substantially improve the utility of the paper. None are mandatory, but I strongly recommend that the authors implement the changes I suggest for the benefit of the readership and the impact of their work.  
  
I wish to particularly compliment the authors on their objectivity. With a few minor exceptions, they communicate the performance of the ASTROSAT instruments without appearing to try to "sell" it to the reader. This is challenging to achieve for anyone who has spent great effort in developing an instrument, yet I consider such objectivity critical to maintaining the credibility and productivity of our field.   
  
Respectfully,  
Anonymous*

We thank the referee very much for their kind words and for their constructive criticism, which has materially improved this manuscript.

*# Major Science Recommendation  
  
1. The NUV spectrum appears to flatten at red wavelengths (> ~2400 Å), in contrast with all of the comparison spectra and with all M and K star NUV spectra that I have seen (such as those in Walkowicz et al. 2008). The text does not address this disagreement. I suspect this is a systematic error resulting from the instrument/analysis. I strongly recommend that the authors address this issue by exploring causes of the flattening. Note that activity should cause the spectrum to appear to flatten toward short wavelengths in the UV, not toward long ones.*

Text

**I would really value input as to whether the referee’s feedback is correct. In Walkowicz et al. 2008, I see a few spectra which appear to flatten at longer wavelengths, viz (Figure 2):**

* **GJ 4053**
* **GJ 3192**
* **GJ 3512**
* **GI 487**

**Similarly, the AD Leo IUE spectrum used by Segura et al. 2005 (Figure 1b), Segura et al. 2010 (Figure 2), Rugheimer et al. 2015 (ApJ, Figure 1) looks pretty flat from 200-300 nm.**

**But on the other hand, maybe there is a resolution issue? AD Leo looks pretty flat in the IUE spectrum, but more sloped in the HST spectrum (Walkowicz et al. 2008 Figure 1).**

**The referee appears to be an expert, so I’d really like to address their comments in a substantive way, not just “get past” them – this may be an opportunity to identify and correct an error we are making.**

*# Minor Science Recommendations*  
  
*2. I and other readers will likely find more information on the host star's stated youth helpful. Is this evaluation based solely on the star's activity? Are there age estimates in the literature? What is the rotation period?*

The age of the star is based on its membership in the Beta Pictoris moving group (Messina et al. 2010, Pineda et al. 2021), which is estimated at an age of ~24 \pm 3 Myr based on isochrone fitting (Bell et al. 2015). We have added a sentence to the introduction, stating this: “The age estimate for HIP23309 is based on its membership in the β Pictoris moving group (Messina et al. 2010; Pineda et al. 2021), which is 24 ± 3 Myr old (Bell et al. 2015)”

We have listed a rotation period of 8.6 \pm 0.07 days in Table 1, with credit to Messina et al. 2010.

*2a. Okay I looked for TESS data on this system. There is clear rotational variability with a period of 8.5 {plus minus} 0.5 d from trough to trough in the data taken in September and October of 2018. You could add this to the text. A more precise period measurement is no doubt possible with a periodogram or similar analysis, but for the purposes of this paper, I don't think it is necessary.*

We have added a rotation period to Table 1 (see above comment)

*3. I don't think interpreting the continuum flux offset as a noise floor is accurate and is a likely misconception of noise floor. The noise floor is the level below which flux (assuming a certain integration time) will be undetectable. However, it should be noise, which means it should average to zero. If integrating the continuum yields a value that is within ~1 sigma of zero then perhaps the offset is truly noise. If it yields a value that is over 2 sigma and over the HST data for the star, then this is likely a background subtraction or other issue with the instrument/data reduction.*

**Prasanta + Mayank, it sounds like they are saying this offset has to be due to reduction. Is this something we can track down? Or, should we just say that this is an issue and we don’t have a solution?**

*4. An alternative to systematics that could explain the flux variations is that the star's emission is actually varying (not flares but "quiescent" variations). I don't think this is likely to explain the 2x FUV continuum offset as both the lines and continuum would likely vary in step, but it is a good candidate for explaining the NUV variations. Rotation can cause ~10% variations in GJ 436, an M star that is less active than HIP 23309, (dos Santos et al. 2019). Factor few variations on timescales of ~10 min are also possible (the HAZMAT paper from Loyd et al. 2018). I think it would be worth addressing this possibility in the discussion. If the NUV variation is caused by an 8.5 d sinusoidal variation due to rotation, then I estimate the minimum amplitude of the variation (minimum since we don't know the phase of the signal) to be 60%, which I don't think rules out rotational variations.*

We thank the referee for these valuable comments. We indeed wondered if the NUV variations might be physical. The problem is that we did not locate any studies on the NUV variability of HIP 23309 or similar stars, so we do not know the expected scale of variations. There is extensive work on the FUV variability of M-dwarfs, including the references cited by the referee, but it was not clear to us how to map the amplitude and timescale of the FUV variations to the NUV variations.

**Sebastian,**

* **Is there any way to map FUV variability to NUV variability?**
* **Is this “Factor of a few variation on a timescale of minutes” from Figure 2 of Loyd et al. 2018?**
* **Do you understand how the referee got this 60% number?** I tried writing the flux as F(T)=F\_0+Asin(phi+2pi\*T/8.5 days), evaluating at F1=F(0) and F2=F(12000 s=0.13 day), setting 0.1=(F2-F1)/F1, and solving for A/F\_0 the fractional amplitude of variation. I got A/F\_0=0.1/(sin(phi+0.096)-1.1sin(phi)), and numerically minimizing on Wolfram Alpha I saw a minimum of 70%. I have no idea how the referee decided 60% was a reasonable variation.

*5. A brief aside: The discussion around line 180 regarding the radiative transfer assumptions of prebiotic chemistry is very intriguing. I presume there are not yet any updated results for M stars or you would have cited them. The Rimmer et al. 2021b reference appears to focus on the young Sun. I hope the authors are considering updating past results on M stars! By the way, listing the albedo as 0.9 < 1 is confusing to me. Is this meant to imply that the correct albedo is 1? Or simply noting that albedos are only valid in the range of [0,1]?*

1. Rimmer et al. are aware of the issue. We are leaving it up to them to update their past results at their own pace. However, when relevant to our own work, we are showing the results with the correct radiative transfer calculation.
   1. Note that not updating Rimmer et al. 2018 is additionally justifiable because there is the possibility of a more stringent bottleneck elsewhere in the chemistry; Rimmer et al. are exploring this possibility as well. They view their 2018 criterion as a limit below which the chemistry will definitely fail, but it is possible that this limit can be made more stringent (increased, ruling out more stars). So, there is a case to be made for deferring an M-dwarf update paper until the chemistry is more thoroughly explored.
2. We cite Rimmer et al. 2021b because it is in this paper that they note the error in their earlier radiative transfer treatment (page 17, first column, bottom of page). This citation is necessary because it validates our statement in this paper.
3. The problem arises because the main treatment of two-stream radiative transfer is not valid for omega\_0=1. The standard mitigation is to impose a ceiling on omega\_0 of 1-epsilon, where epsilon is small. For example, in the codes ultimately derived from the Kasting model, such as EXO-P, epsilon=1E-3. In the earlier Rimmer code, epsilon=1E-1. This works fine when either the absorption optical depth >> the scattering optical depth, or the scattering optical depth <<1. However, for early Earth which lacks UV-absorbing ozone and oxygen, a regime is unveiled in the NUV where the scattering optical depth >> 1>> absorption optical depth. In this case, even epsilon=1E-3 leads to errors, and we instead set epsilon=1E-12, which produces very good agreement with the pure scattering limit over the range of parameter space relevant to early Earth-like planets (Ranjan & Sasselov 2017).

To clarify the matter, we have updated the text

“single-scattering albedo ω0 = 0.9 < 1 when calculating Rayleigh scattering, which underestimates surface UV in the unique radiative transfer regime unveiled on anoxic early Earth-like planets”

To

“a ceiling on single-scattering albedo ω0 of 0.92 when calculating two-stream radiative transfer, which underestimates surface UV in the unique regime unveiled on anoxic early Earth-like planets”

We have also moved the reference to the P. Rimmer personal communication to footnote 2 as it is mostly superfluous given the Rimmer et al. 2021b citation, and because upon re-reading it seemed a bit aggressive to us.

Actually, we would appreciate feedback from the referee/editor as to whether any part of this discussion feels like an attack – that is not at all our intention, we respect Rimmer et al. very much, our goal is to make a constructive point about the value of characterizing M-dwarf UV fields as oppose to criticizing their work.

*6. The text mentions Lya as being a cause of error, but I did not catch any mention of how the modeling dealt with the lack of Lya in the UVIT spectra. Was the flux at all wavelengths shortward of the UVIT band set to zero? An explicit statement of how this was handled would help.*

Yes, that is correct. To clarify, we have added the following sentence in Section 4.1, where we describe SED construction: “We set the stellar flux at wavelengths shorter than the limits of the measured SED to 0.” Section 4.2 states that it follows Section 4.1 in constructing the 2 additional SEDs.

*7. Similarly to (6), were the rescaled HST spectra rescaled over their full wavelength range? If so, could this have affected the thermal structure of the atmosphere in a way that would propagate into the chemistry?*

Yes, that is correct (see Figure 7). However, we do not self-consistently calculate the T-P profile, and therefore do not capture this additional effect. As we note from lines 197-203, our calculations are photochemistry-only sensitivity tests following Kasting et al. 1997, designed to test the importance of accurate UV estimates only, as opposed to self-consistent climate-photochemistry calculations. In any case, it is unphysical to rescale the entire spectrum as we have done in this crude sensitivity test; to do this in full detail, one would need to build a stellar atmospheric model, determine which knobs to turn to match the observed FUV, and propagate those changes To clarify this, we have added the following sentence to the final paragraph in Section 4.2: “For simplicity we scale the SEDs across all wavelengths, but only the UV wavelengths have any impact on our models since our photochemistry-only sensitivity test neglects climate adjustments.”

# Language  
  
*8. Line 254 could be read to imply that NUV measurements are still possible, but that is not the case, right? If so, then presumably only archival measurements are available. I recommend you revise the text to avoid any confusion.*

That is correct, new NUV grism measurements are no longer possible, but archival NUV grism analyses are possible, as are new NUV photometry measurements. We attempted to clarify this in line 264, where we state “Our work suggests a role for archival NUV observations and archival and future FUV observations…”

To further clarify, we have updated “Measurements from the UVIT NUV grism can inform studies of planetary habitability because it…” to “Archival measurements of stellar UV from the UVIT NUV grism can inform studies of planetary habitability because they…”. Further, we have removed the word “ongoing” from the last sentence of the Introduction (i.e. “potential of UVIT for ongoing studies” 🡪 “potential of UVIT for studies”)

*9. The introduction mentions the paper will discuss implications for the INSIST and UV-SCOPE mission (concepts). However, the conclusions only mention INSIST.*

We thank the reviewer for pointing out this error. We had originally discussed UV-SCOPE in the manuscript as well, but as that mission concept was unfortunately not selected we removed that discussion. The text noted by the reviewer was retained by accident.

We have completed removal of mention of UV-SCOPE by removing “and UV-SCOPE” from the Introduction.

*10. Line 59: "It remains preferred" seems vague.*

We aren’t sure how to do much better. The problem is that studies seeking to quantify the impact of using estimated SEDs compared to accurately measured SEDs are extremely scarce; the Teal et al. 2022 study we cite is the only thorough study we are aware of. Teal et al. 2022 concluded the *necessity* of using measured SEDs, but we did not find their arguments fully convincing because significant effects on the observables were only found in one of the two endmembers they studied, and it is not clear how representative that endmember is. We do however fully support their qualitative point that it’s better to use measured SEDs when possible. For this reason, we chose this weaker phrasing. We would value alternative suggestions that the referee might offer.

*11. Line 76: Excellent agreement seems like a stretch given the continuum errors and flattening of the NUV flux at red wavelengths. Regardless, excellence is subjective so I think you should be epxlicity about who makes this judgment. How about "and find agreement at levels that we consider satisfactory for use in photochemical modeling."*

We have updated “show excellent agreement” 🡪 “find agreement at levels that we consider satisfactory for use in exoplanet photochemistry studies”

*12. Your readership might not be familiar with the ISRO acronym.*

We have adjusted “ISRO” to “Indian Space Research Organization (ISRO)”

*13. The wording in the second clause of the sentence on line 165 needs some work.*

We were not quite sure what work was needed. We have attempted to clarify the text as follows: we have changed

“Efforts to experimentally determine if flare UV can substitute for quiescent UV are now under way1; if they cannot, then life search on exoplanets may constrain the pathways towards the origin of life (Ranjan et al. 2017; Rimmer et al. 2018)”

To

“Laboratory efforts to experimentally determine if flare UV can substitute for quiescent UV in HCN photohomologation are now under way1. If flares cannot compensate for low-steady state UV, then life search on M-dwarf exoplanets may enable tests of theories the origin of life (Rimmer et al. 2021a)”

*14. I had to look up "coterminous." Contemporaneous might be more easily understood to your readers.*

We have updated “coterminous” 🡪 “contemporaneous”.

*15. Line 68. Consider stating here that NUV observations are no longer possible. I initially interpreted this to mean that only one NUV optic could be used any longer.*

We have added the sentence “The NUV channel is now inoperative and only archival studies are possible; the FUV channel remains operative” to the end of the paragraph.

# Figures  
  
*16. The gray line in Figure 3 is quite challenging to see.*

Gray line in Figure 3 changed to green.

*17. The error bars in all figures add a great deal of clutter. Consider using them on a subset of data points or plotting errors as separate lines (e.g., dotted lines of the same color).*

We thank the referee for this suggestion. We prefer to retain the point-by-point error bars without alteration, so that we are not accused of hiding any aspect of our data. However, we also see the referee’s point regarding clarity.

We propose the following solution: We expand Figure 3 into two plots: a top plot showing the data in linear scale with error bars, and a bottom plot showing the data in log scale without error bars. We believe this is more readable without compromising on showing the errors. We have correspondingly updated the caption to Figure 3 with the sentence “Top plot shows data in linear y-scale with error bars; bottom plot is identical to top plot except with log y-scale and omission of error bars for easier viewing.”

*18. The error bars in Fig 5 are especially distracting. Consider setting a floor to the error bars so that they impinge less on other lines on a log scale.*

Figure 5 contains no error bars (none of our log plots did, they were unreadable with error bars). Instead, what the referee is likely noticing in the left panel of Figure 5 are points where the unbinned UVIT FUV fluxes are <=0.

To alleviate this problem, we replace the unbinned UVIT FUV fluxes with the binned UVIT FUV fluxes from Figures 3 and 7. We have further restricted the wavelength ranges of the figures to 1290-1710 A and 2000-2950 A, to match Figure 7 and the inputs into the prebiotic chemistry and atmospheric chemistry calculations.

# CHANGES INITIATED INDEPENDENT OF REFEREE COMMENTS  
  
 Line 237: “useless” 🡪 “of limited utility” [since archival observations may still yield useful results].

Line XXX: “permit limited accurate photochemical modeling” 🡪 “permit limited accuracy in photochemical modeling” [Eliminate awkward phrasing].