A review of **"Simulation of a spaceborne metastable helium lidar"** by Chuan Huang et al.

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Overview: The paper presents a discussion of the feasibility and potential performance of a space-based resonance lidar targeting the metastable helium layer in the Earth's thermosphere and exosphere. In principle, this can be a meaningful contribution, though the feasibility of such an instrument is in my opinion overstated in the paper. There is an alarming lack of references to appropriate literature. My recommendation is a major revision of the paper; my concrete concerns are listed below.

Major concern: The calculation of saturation is incorrect. The derivation presented here is valid only for lidars employing broadband lasers -- i.e. lasers with bandwidths larger than the Doppler-broadened absorption line that they are targeting. (Also, the derivation presented here is based on the derivation by Megie et al. (1977) and mirrors very closely the presentation given in Chu and Papen (2005). Those sources should certainly be cited; not to cite either is borderline plagiarism.) An approach to estimating saturation for lidars employing narrowband lasers was published by von der Gathen (1991) in reference to sodium resonance lidars; our group refined this approach and extended it to helium and iron resonance lidars in a recent publication (Geach and Kaifler, 2026). To summarize the implications of that paper: saturation is significantly more severe than the approximations from Megie et al. would imply, particularly for helium resonance lidars. Furthermore, the degree of saturation is highly wavelength-dependent and can therefore lead to substantial errors in inferred winds and temperatures. This represents a significant design constraint for future helium resonance lidars. This needs to be properly considered in order for an assessment of a spaceborne lidar to be meaningful.

Minor concerns:

The helium resonance lidar was first proposed by Gerrard et al. (1997) – that work should be referenced here.

Line 99: Various variables are not defined (eta_a, eta_2, tau, G, T_a). lambda is used to signify the wavelength of the laser, but subsequently, lambda_L is used.

Line 105: For completeness, it should be stated what N_det and N_A are. The choice of symbols is a bit confusing, since (if I understand correctly) N_sig has units of counts, whereas N_det and N_A have units of counts/time. The repetition rate (f) is also not defined – particularly important since soon after f is used to mean the frequency of the laser or resonance frequency of the transition. Please go through all equations and make sure that all variables used are adequately defined.

Line 125-126: 'on the order of tens of nanoseconds' → 98 ns.

Lines 194-198: Not clear why a discussion of the solar optical background is relevant for a satellite mission. The continuum background arises due to solar scattering in the lower atmosphere, which would not affect a satellite looking upwards.

Lines 200-201: For which time of day are the densities calculated?

Lines 209-216: The variation in density shown in Figure 4 is substantially smaller than that observed in Geach and Kaifler (2025). How do you account for the discrepancy? Those measurements were made at a different longitude – does your model predict such strong longitude-dependency?

Lines 227-229: The background will presumably be dominated by solar illumination of the helium layer rather than astronomical sources – just as is the case for ground-based systems with sufficiently narrow-band optical filters. Given the planned orbit along the terminator, the helium layer above the satellite will be constantly illuminated, leading to a high, passive, in-band background.

Lines 278-280: I believe this is trying to convey that there must be an offset between the telescope and laser to account for the lateral motion of the satellite over the time of flight of each pulse – but that could be more clearly communicated.

Line 284-285: The reference to Ren et al. (2017) is odd, since the detector technology employed in that work is not the one referenced here (i.e. SNSPD). Also, a key limitation of SNSPDs is the active area of the detector, which places constraints on the etendue of the instrument – this should be addressed.

Line 309: This equation implies perfect knowledge of the background counts – when the background is imperfectly known (it is, after all, estimated from noisy measurements), the background subtraction also contributes to the final uncertainty.

Figure 7: SNR depends of course on the vertical and temporal resolution – these should be stated (an integration time of 1 minute is given in Table 2 but it should be explicitly stated that this is the integration time used in Figure 7, if that is the case).

Line 322: If I understand correctly, a noisy profile was generated, then this noisy profile was resampled 1000 times, with new noise realizations each time. Is this not double counting the noise? Why not use a noiseless profile in the first step, and add the individual noise realizations to that?

Lines 323-324: I don't understand why this trick of integrating over 60 km but repeating every 20 km is used. It seems to imply, incorrectly, that a vertical resolution of 20 km is achieved, but this is misleading. The separate measurements are not independent. If the authors wish to show a resolution of 20 km, then they should use vertical bins of 20 km; if this results in insufficient SNR, then they should use vertical bins of 60 km spaced at 60 km. (Otherwise, why stop at 20 km resolution? One could just as easily use a sliding window of 60 km repeated at intervals of 1 km, or even smaller, implying an even higher vertical resolution.)

Line 317-318: Figure 9 should be referenced here, to indicate the form of the prescribed profiles. Why are such large LOS winds used? Also, do you assume 1 minute integration? How are is the integration time split between the three observation wavelengths – is this fractional integration time already accounted for?

Figure 9: Can you explain the apparent bias in the LOS wind?

References:

Chu, X., & Papen, G. (2005). Resonance fluorescence lidar for measurements of the middle and upper atmosphere. In T. Fujii & T. Fukuchi (Eds.), *Laser remote sensing* (pp. 179–432). Taylor and Francis Group.

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Geach, C., & Kaifler, B. (2026). Saturation effects in narrowband resonance lidar measurements, Part 1: Theoretical consideration of sodium, iron, and helium lidar systems. *Journal of Geophysical Research: Space Physics*, https://doi.org/10.1029/2026JA035269.

Gerrard, A. J., Kane, T. J., Meisel, D. D., Thayer, J. P. & Kerr, R. B. Investigation of a resonance lidar for measurement of thermospheric metastable helium. *J. Atmos. Sol. Terrestrial Phys.* **59**, 2023–2035 (1997).

Megie, G., Bos, F., Blamont, J., & Chanin, M. (1978). Simultaneous nighttime lidar measurements of atmpospheric sodium and potassium. *Planetary and Space Science*, *26*(1), 28–35. https://doi.org/10.1016/0032-0633(78)90034-X

von der Gathen, P. (1991). Saturation effects in Na lidar temperature measurements. *Journal of Geophysical Research*, *96*(A3), 3679–3690. https://doi.org/10.1029/90JA02420