Note that in this response, the line numbers refer to the line numbers in the revision with track-changes; the reviewers' comments are in blue and italic; the responses are in black.

Reviewer #1

The paper is aimed to answering the question: how well the F10.7 and F30 solar fluxes represent solar EUV forcing in the thermosphere.

In the Introduction, the authors note that both proxies are often used in studies of long-term changes in the thermosphere and ionosphere.

Section 2 describes briefly the model and experimental data used in the analysis. The model simulation was conducted using the TIME-GCM model with the F10.7 and F30 fluxes as SA proxies. The set of thermospheric density from atmospheric drag observations (Emmert et al., 2021) was used to be compared to the simulation results. The measurements by the GOLD equipment onboard the SES-14 satellite were used to compare with the changes in the F10.7 and F30 fluxes in 2018-2024.

Figure 1 shows a comparison of the F10.7 and F30 solar fluxes over a period of 1961 – 2019. The most important point in Fig 1 is that during this period, F30 increased with respect to F10.7.

A comparison of the modeled (with F30 and F10.7 as a proxy) and observed (derived from satellite orbit changes) mass density for 1967 to 2019 is presented in Fig.2. It demonstrates distinctly that the ratio of two densities increases with time. However, the slope of the linear fit of that increase is much higher for F30 than for F10.7. If only a period to 1996 is considered, the above slope decreases for F10.7 but does not change for F30.The authors discuss the features seen in Fig 2 in terms of changes in the solar EUV fluxes with time.

Further, the authors compare in Fig. 3 the changes in GOLD Qeuv flux during 2018-2024 to the changes in the F30 and F10.7 solar proxies. Analyzing the Qeuv/F10.7 and Qeuv/F30 ratios, the authors note that the linear approximations of this ratio are different for the period of the extended SA minimum in 2018-2020 and the period after it.

Some aspects of the changes in the SA EUV flux related to the aforementioned results are considered in Discussion. For scientists involved in deriving long-term trends in the thermospheric and ionospheric parameters, the most important is the conclusion "…that the F30 flux is more suitable to be used as a solar EUV proxy in thermospheric modeling."

As far as a correct allowance for the SA changes is a very important step in attempts to reveal long-term trends in the thermosphere and ionosphere, the paper under review presents a very important study. I recommend publication of the paper with a minor revision.

My suggestions are as follows.

The increase with time in the ratio of modeled and observed densities in Fig 2c at the first sight leads to an inevitable assumption that it manifests a long-term trend in the density itself. In other *words: if the model gives a "correct" density without trends, the real density becomes lower during the later years due to a negative trend in it. I think that such impression could visit many readers, so the point deserves at least a brief comment in the text.*

Response: This is a great point, thanks! In the revision (lines 193-203), we added the following text: "Note the upward linear slope in the density ratio between simulated and orbit-derived mass density for the period 1967–2019 in Figure 2b, as well as in Figure 2c, which will be discussed later. This slope is not a long-term trend caused by increasing CO_2 concentrations. Both the simulated and orbit-derived mass densities include trends driven by rising CO_2 levels: the model simulations incorporated time-varying CO_2 concentrations measured at the Mauna Loa Observatory (Qian et al., 2006), and Emmert (2015) demonstrated that the height dependence of orbit-derived mass-density trends agree with model simulations of the impact of increasing CO_2. Thus, to a first approximation, changes in CO2 concentrations do not lead to a trend in the density ratio. This is the case for the earlier period from 1967–1996, which has a near zero slope (0.0007). The significantly larger slope of 0.0021 for the period 1967–2019 can be attributed to a change in the linear relationship between the density ratio and F10.7 after approximately 1996. This change arises from increased saturation of the F10.7 flux during the unusually low solar activity minima of 2008–2009 and 2019–2020. This will be discussed further later."

In my opinion, it is worth mentioning that the conclusion that F30 is better than F10.7 has been obtained by several research groups based on the analysis of changes in F2-layer parameter data.

Response: We revised the manuscript to make this point clear:

1. Lines 58-60: In addition, based on analysis of changes in F2-layer parameter data, several research groups have found that F30 is better than F10.7 in representing F2 parameters.
2. Lines 68-69: Danilov and Berbeneva [2024] also found that F30 is the best solar proxy to describe the foF2 behavior in the solar cycle.

Reviewer #2

Minor comments:

Figure 2: I found that the ratio of simulated and observed mass densities also have a clear solar dependence. So, I am curious about whether the calculation of linear trends shown here (dashed lines in Figures 2b and 2c) excluded the solar effect. If not, the inconsistency in the F10.7 case (Figure 2b) might also be introduced by the different solar levels in the starting and ending years.

In Figures 2b and 2c, the observed density is used to calculate the density ratios, normalizing the simulated densities for solar cycle variability. The ratio fluctuates roughly in phase with the solar cycle, indicating that the model tends to relatively overestimate mass density during solar maxima and underestimate it during solar minima (lines 208–212).

Notably, normalizing the simulated mass density using the observed density reduced the solar cycle signal from approximately 10 times the variation in mass density to about 1.25–1.5 times in the density ratio (Figures 2b, 2c). This substantial reduction in the solar signal, combined with the extensive sampling (multi-decadal data points), yielded statistically significant linear fits, as indicated by the F-scores for these linear regressions (1851 and 44, respectively). In the F-test, an F-value greater than 2.5 allows us to reject the null hypothesis, confirming the significance of these results.

Figure 3: The F30 is linearly correlated with F10.7, so the ratio of F10.7/Qeuv and F30*/Qeuv should also be in linear correlation because the two are both numerators. So, I am curious why the lines in Figures 3b and 3c are not strictly correlated.*

This is an excellent question. We believe the following two reasons explain why the lines in Figures 3b and 3c are not strictly correlated, despite F30* being linearly correlated with F10.7:

1. While F30* and F10.7 exhibit a linear correlation, there is some scatter around the linear fit (Figure S1a).
2. The linear relationship between F30* and F10.7 differs at low solar activity levels (F10.7 < ~90) compared to higher solar activity levels (F10.7 > ~90) (Figure S1a).

As a result, depending on the solar activity level (Figure S1d), the lines in Figures 3b and 3c are not strictly correlated (Figures S1b and S1c).

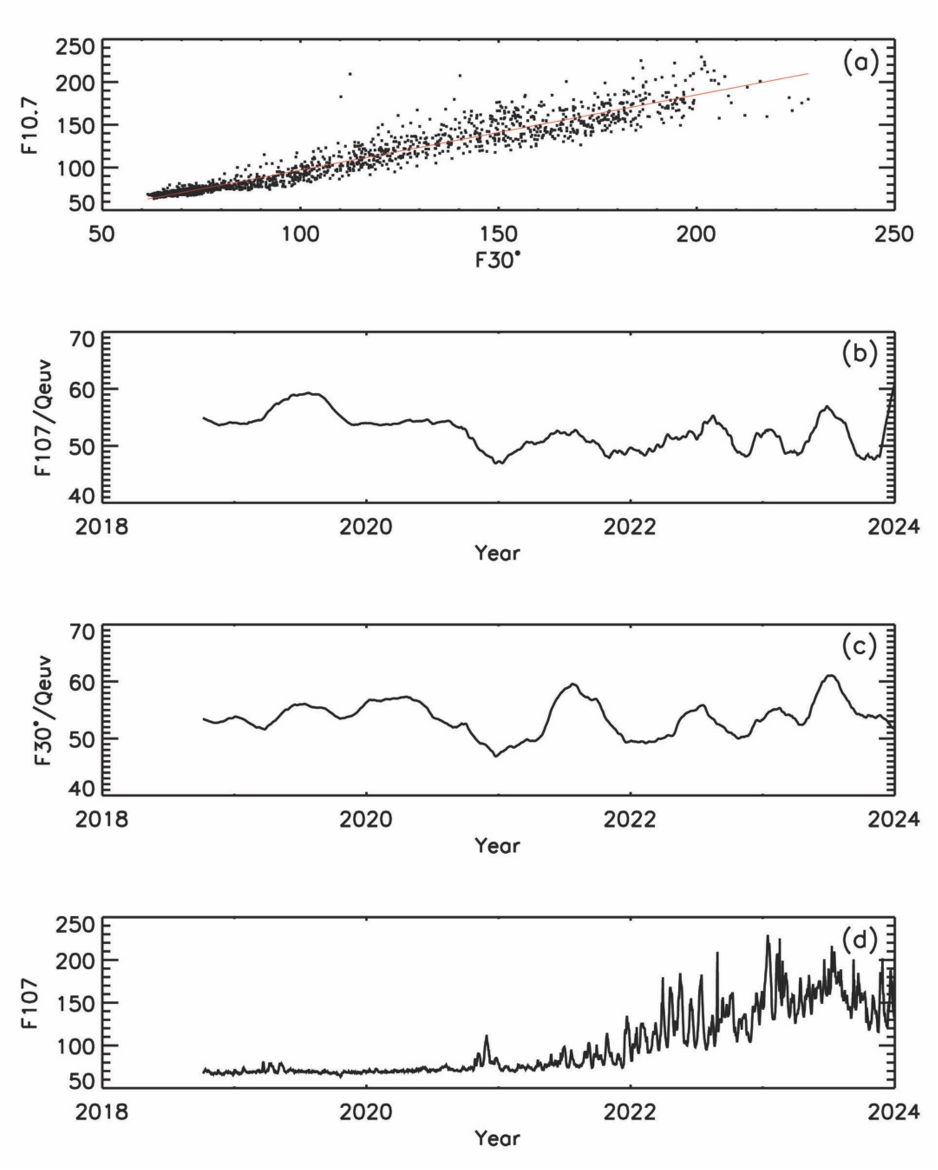

Figure S1: (a) scatter plot between F10.7 and F30*; (b) ratio of the 81-day averaged F10.7 and Qeuv; (c) ratio of the 81-day averaged F30* and Qeuv; (d) daily F10.7.

Please unify the symbols of "-" and "–" when referring to time periods.

Done (line 358).

Related Reference Suggestion:

Elias, A. G., Martinis, C. R., de Haro Barbas, B. F., Medina, F. D., Zossi, B. S., Fagre, M., and Duran, T. (2023). Comparative analysis of extreme ultraviolet solar radiation proxies during minimum activity levels. Earth Planet. Phys., 7(5), 540–547. DOI: 10.26464/epp2023050

Added (line 248; lines 430-432).

Reviewer #3

This paper presents an interesting study on the assessment of two solar activity EUV proxies for long term studies, F10.7 and F30, but it is incomplete and a couple of essential points are not discussed and quantified.

In your paper you do not discuss the problems in the Nobeyama data and their degraded quality since 2020. There are many outages and data problems since 2020 as can be seen on their website (http://solar.nro.nao.ac.jp/norp/html/ObsLogFrom2020.html). Since you are presenting small drifts, it is very important to take into account the quality (see Fig_f30p-vs-c_cycle23; the precision given in the CLS data file) and stability of the instrument calibration. There actually is a detailed paper on that for F10.7, but not for F30.

Reply: The problematic fraction of Nobeyama data since 2020 has little impact on our results. Firstly, as mentioned in the paper (Sec 2), the provisional radio flux data (with data quality flag of 1) were excluded from our analysis. Second, the analysis of trends in Fig. 2 is based on data up to 2019, i.e., preceding the problematic time interval of F30 data. In Figure 3, for the time interval since 2020, we have used F30 data from the CLS server. Accordingly, we have added the following note in section 3 in the revised manuscript: "Note that there have been observation interruptions of F30 due to instrumental issues at Nobeyama since 2020. These data gaps have been filled by the *Solar Radio Flux for Orbit Determination: Nowcast and Forecast* project of the Collecte Localisation Satellites (CLS) using the expectation-maximization interpolation method described in Dudok de Wit (2011). (For further details, see https://spaceweather.cls.fr/services/radioflux/)".

I miss a discussion of the CLS radio flux data file, which contains interpolated values in case of gaps or outliers (the flags are explained in the header of the CLS data file; see figures Fig_f30f_2023 and Fig_f30f_2019 with long periods with interpolated F30), and what consequences that may have for your study.

Reply: See our reply above.

You have used the conversion formula from the Yaya et al. 2017 paper (Dudok de Wit and Bruinsma, 2014 is erroneously given as reference), which is based on a regression from 1970-2014. Why have you not done your own regression? Because the period has a big impact on the regression parameters, and therefore certainly on your results via EUVAC. Do your results and conclusions change when you use other regression parameters? (see the regression results in figure Fig_f30c-f107c_linreg)

Reply: We found the conversion formula (F30* = 1.554 × F30 - 1.6, formula 1) on page 7 of Dudok de Wit and Bruinsma, 2017, not 2014, as Reviewer claims. However, since the same formula also appears in Yaya et al., 2017 (which is coauthored by the authors of the other paper!), we have now included a reference also to this paper at this point.

Our own regression using data from 1961 to 2020 yields F30* = 1.548 × F30 – 0.99 (formula 2). As an example, Figure S2 shows manuscript Figure 1 using formula 1 versus formula 2. Results using the two different formulae are hardly distinguishable in Figure S1. In fact, using our own regression does not change any of our results or conclusions.

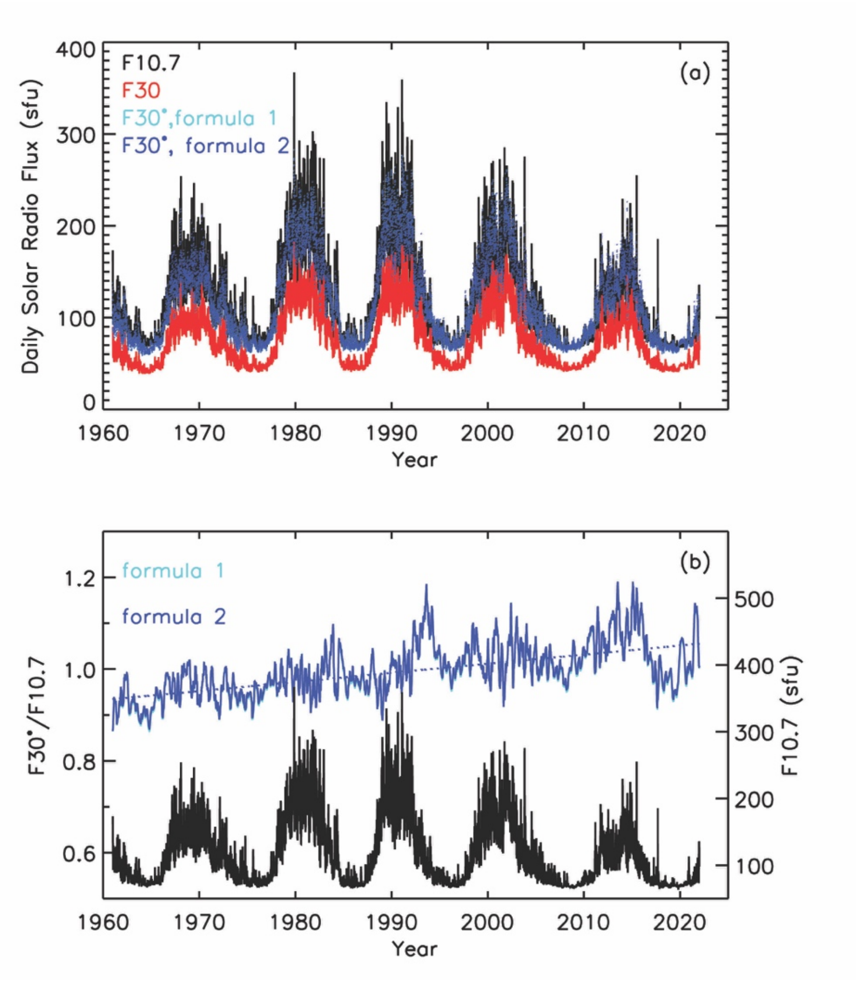

Figure S2: (a) Black: daily solar radio fluxes F10.7; red: F30; cyan: F30* using formula1; blue: F30* using formula 2. (b) Cyan: ratio of the 81-day averaged F30* and F10.7 using formula 1; blue: ratio of the 81-day averaged F30* and F10.7 using formula 2; Dotted line: linear fit to the ratios; black: daily F10.7 for reference.

There is also the question of instrument change, which may partly contribute to the difference, but which is not discussed in the paper:

Reply: Instrument changes in Canada and their possible effects, e.g., on calibration accuracy and data consistency of F10.7 have been thoroughly discussed by Tapping (2013), where the long-term continuity of F10.7 measurements was verified. In fact, the only recent study where the homogeneity of the F10.7 series has been questioned is Clette (2021) which suggested that there is a step-like change in the F10.7/sunspot ratio in 1980. They speculated, without any actual evidence, that this change is produced by a change in calibration introduced by the new team. However, as recently shown by Mursula et al. (2024), there is systematic long-term change in this ratio, not a step. Similar changes are found in several independent solar parameters, indicating a real change in the Sun. (For more details on the results and the physical explanation, see that paper). Accordingly, we see no reason to discuss the relocations in Canadian observations in this paper, since they do not cause any problems to F10.7 data quality.

Observations began on 6 November 1951 in Toyokawa at 8 cm, see Table 1. From 24 February 1994 to 14 May 1994, all but the observations at 8 cm were interrupted as the antennas were moved from their location at Toyokawa to nearby Nobeyama.

Reply: We clarified our data gap policy by adding the following sentence in Section 3: "Note that there have been observation interruptions of F30 due to instrumental issues at Nobeyama since 2020. These data gaps have been filled by the *Solar Radio Flux for Orbit Determination: Nowcast and Forecast* project of the Collecte Localisation Satellites (CLS) using the expectation-maximization interpolation method described in Dudok de Wit (2011). (For further details, see https://spaceweather.cls.fr/services/radioflux/)." We thank the Reviewer for noting on this shortage in our data presentation.

Figure 2: max in 1970 and 2012 are better with F10.7. F30 leads to too small densities in 1970, and too high in 2012. This drift in F30 has been detected and corrected in the DTM2020 paper (Bruinsma and Boniface, 2021). Calculation of DTM2020 density ratios with F10.7 and F30 and TLE densities at 250 km showed no trends, not with F10.7 nor with F30.

Reply: Indeed, Bruinsma and Boniface (2021) detected this drift, as shown in their Fig. 2. They removed this drift from the F30 index by a simple linear fit (their Eq. 2) by speculating that "The drift is most likely due to imperfect calibration of F30." However, they do not give any actual evidence for this claim. Thus, this trend removal can hardly be called a "correction". Despite this problem in F30, B&B (2021) use F30 since, as they write: "DTM2013 and DTM2020_Res are driven by F30 because the observed densities can be reconstructed with higher fidelity than with F10.7.." So, they use the F30 flux although they consider it so erroneous that it must be corrected. Accordingly, their treatment is not both unvalidated and inconsistent.
On the other hand, Mursula et al. (2024) offer a completely different interpretation: a natural and physically motivated explanation for the long-term drift between F10.7 and F30 (and several

other solar parameters) with no need to do any ad hoc "corrections" to the drift of F30 vs F10.7. The effects of this drift to the observations and modeling of thermospheric density are discussed in this paper.

Another point concerns thermosphere cooling due to increasing CO2 levels, which leads to lower densities mostly notable at solar minimum (decrease estimated at 2-5% per decade). That effect will also lead to a drift in the density ratios depending on how accurate your model takes that into account.

Reply: The upward linear slope in the density ratio between simulated and orbit-derived mass density for the period 1967–2019 in Figures 2b and 2c does not describe the long-term effect caused by increasing CO_2 concentrations. Both the simulated and the orbit-derived mass densities include the trend driven by the rising CO_2 level, and model simulations incorporate the time-varying CO_2 concentrations measured at the Mauna Loa Observatory (Qian et al., 2006). Moreover, Emmert (2015) demonstrated that the height dependence of orbit-derived mass-density trends agree with model simulations of the impact of increasing CO_2. The larger slope (about 0.0021) of the F10.7 model for the longer period 1967–2019 arises because the F10.7 model is unable to explain the very small density during the unusually low solar minima of 2008–2009 and 2019–2020 because of enhanced saturation of the F10.7 flux.

All points above should be discussed and clarified, and I therefore recommend moderate revision.