Review of the manuscript "Ozone and DNA active UV radiation changes for the near global mean and at high latitudes due to enhanced greenhouse gas concentrations", by Eleftheratos et al.

The manuscript presents results from the global chemistry climate model ECAM on changes regarding DNA weighted UV radiation with regard to changes in total ozone, cloud cover and surface albedo. UV data derived from the model are validated with respect to past UV data from 13 ground-based stations providing solar UV measurements while the simulated ozone is compared to SBUV ozone.

The results are discussed as three groups, defined by North and South high latitudes and middle latitudes, and model results are retrieved at the locations of the 13 ground-based stations.

The key findings presented in the manuscript are that model and measurements agree fairly well, giving support to the simulations of the future scenarios. Cloud cover is generally decreasing, leading to increased solar radiation, apart from the high latitudes, where no significant changes are observed. UV trends are a combination of ozone changes (mostly ozone recovery), and cloud cover changes, while at high latitudes, decreased surface albedo in the second half of the next century have a significant influence on the surface UV radiation.

The manuscript is well written, the references are extensive and cover the current status of the field as far as I can judge. The results are interesting and therefore the manuscript is in principle worth to be published.

However I have serious concerns with the novelty of the research and its added value with respect to already published papers, foremost the one published in 2020 by the same main author, Eleftheratos, K., Kapsomenakis, J., Zerefos, C. S., Bais, A. F., Fountoulakis, I., Dameris, M., Jöckel, P., 821 Haslerud, A. S., Godin-Beekmann, S., Steinbrecht, W., Petropavlovskikh, I., Brogniez, C., Leblanc, T., Liley, 822 J. B., Querel R., and Swart, D. P. J.: Possible Effects of Greenhouse Gases to Ozone Profiles and DNA Active 823 UV-B Irradiance at Ground Level, Atmosphere, 11, 228, doi:10.3390/atmos11030228, 2020.

The authors discusses this manuscript at length, so they are aware that there is a need for distinction. However it seems that the main difference in this manuscript with respect to the previous work are the addition of a few ground-based stations at which the model results are analysed (13 instead of 5, of which 4 are identical). The conclusions of the manuscript are very similar to the previous manuscript, with some differences by distinguishing three latitudinal bands.

Since the model used in the analysis is a global model, the restriction to 13 specific sites must be for good reason. The only reason I can see is that this allows comparison between the ground based UV stations with the model at these locations, in order to validate the model. I am not convinced by this argument for the following reasons:

- The model has a resolution of 3°x3°, which is a huge area for which the point measurement has to be representative for. I doubt that this is the case for several stations where the surrounding area is inhomogeneous, such as mountain tops (MaunaLoa, Sonnblick, Zugspitze), or in valleys (Aosta), or by being in a town with very heterogeneous surroundings (sea, mountains, tropospheric ozone and aerosols such as Athens).
- The correlation for DNA weighted UV irradiance is actually not very good, and the figures in the supplementary material show quite different behaviour. A general comment is that the correlation is not the only measure for the agreement between two datasets, but also the slope between two datasets (from a scatter plot), are significantly different from one,

showing that the model results disagree quite significantly from the measurements, see Table S1 with a summary of the statistics. For me these comparisons do not support any validation of the model.

- The comparisons were performed for past to present data, using a model with prescribed dynamics. However the results of the manuscript are obtained using a free-running model, which as the authors write themselves, has serious shortcomings. The Appendix A discusses this fact, which is appreciated.
- ozone and Cloud cover are obtained from satellite measurements, which also give a global product.
- The authors themselves mention that some stations might not be very representative, being close to the shore (Barrow and Palmer, line 493).

Therefore the benefit of restricting the analysis of the model results to only 13 point locations does not compensate for the results obtained if the model results were analysed as a whole, for example in latitudinal bands, or by selecting specific regions where future changes are expected to be very different (Europe versus Asia, Sahara, …).

Some specific comments:

- The simulations of the future climate did not take into account possible solar variabilities (grand minimum, as discussed in Anet, J., Rozanov, S. Muthers, et al., Impact of a potential 21st century "grand solar minimum" on surface temperatures and stratospheric ozone, Geophys. Res. Lett., 40, 4420–4425, doi:10.1002/grl.50806, 2013, and Arsenovic, P., Rozanov, J. Anet, et al., Implications of potential future grand solar minimum for ozone layer and climate, Atmos. Chem. Phys., 18, 3469–3483, doi: 10.5194/acp-18-3469-2018, 2018.
- Future changes in aerosol loading are expected to be significant in some areas of the globe, having a strong impact on the UV radiation reaching the surface.
- The significance of the results are mainly described by correlation coefficient and p values. The supporting figures, for example Figure 1, however show that the variability between the model and ground based stations is very large. Even though scatter plots are also not the method of choice, they would give a better indication how two datasets would scatter, and the slope and associated fitting uncertainties would give an indication on how well the two datasets agree. I would have preferred the authors to have used other metrics as well, such as uncertainties (at the 95% confidence level) derived from the statistical models.
- The statistical approach of using a MLR technique is interesting, but why did not the authors include in equation 7 also the surface albedo, instead of treating it separately in the following section?
- The datasets would have been ideally suited to be analysed using the very powerful Dynamical Linear Modelling (DLR), for example, Alsing, (2019) dlmmc: Dynamical linear model regression for atmospheric time-series analysis. Journal of Open Source Software, 4(37), 1157, https://doi.org/10.21105/joss.01157, and Laine, M., Latva-Pukkila, N., and Kyrölä, E.: Analysing time-varying trends in stratospheric ozone time series using the state space approach, Atmos. Chem. Phys., 14, 9707–9725, https://doi.org/10.5194/acp-14-9707-2014, 2014.
- Figure 1: 50N-50S, there is a striking difference between model and measurements around the year 2012, of about 15%, which seem not be seen in either ozone or cloud cover. Did the authors investigate this feature?
- In the figures from the supplement, the model variabilities of the DNA weighted irradiance are much larger than the corresponding measurements for : Barrow, Villeneuve d'Ascq,

Aosta, Lauder, Ushuaia, while they are in better agreement for Summit, Thessaloniki, Boulder, MaunaLoa, or Alice Springs. For Athens, between approx. 2012 and 2015 the measurements are significantly higher than the model results, why is that so?

- The cloud cover from Modis/Terra and the model show no correlation for most stations, apart for example for Aosta, which is slightly better. Can the authors provide some comments why some stations show better agreement than others?