

## ***Interactive comment on “Radiative forcing from modelled and observed stratospheric ozone changes due to the 11-year solar cycle” by I. S. A. Isaksen et al.***

### **Anonymous Referee #1**

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This paper attempts to give an estimate of the additional radiative forcing that solar cycle fluctuation may cause via stratospheric ozone changes induced by photolysis changes. This is, in itself, useful work as respective radiative forcing calculations are sparse and have usually not been included in recent studies of the stratospheric solar cycle impact (e.g., Matthes et al., Pap. Meteorol. Geophys 2003; Langematz et al., GRL 2005, Austin et al., ACP 2006). The main findings are described as 1) there being general agreement in observed and simulated ozone change vertical profiles from the solar cycle, and 2) a additional RF being more than one magnitude smaller compared to the RF induced by direct radiative absorption (about 0.2 W/m<sup>2</sup>).

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However, I cannot recommend publication of this manuscript in anything like its present form. The methodology lacks a stringent consistency and is often poorly described. At least, a lot of important information is missing. In particular, the way of transferring of the ozone change data from the CTMs to the radiation codes is hardly convincing and the discussion of sensitivities remains a patchy business.

I also think that (given the known sensitivity of even the sign of stratospheric ozone RFs to details in the change pattern) the ozone change distributions entering the study should be chosen very carefully. An outdated pattern as that of Haigh (1994) "for comparison" might better be replaced by more recent work.

This said, I nevertheless give a list of detailed comments hereafter, in order to provide some guidance for a potential revised version of the paper.

Detail comments:

p.4354, l.19: What is meant by "...influence atmospheric ozone directly..." ? From my point of view the direct influence of solar variations to ozone is via changes in the photolysis rates, while I would classify other impacts (via temperature-dependent reaction rates, transport changes due to dynamic feedbacks, or chemical feedbacks) as "indirect". Do the author's agree (see p.4355, l.7) ? Please, make it clear throughout the paper which aspects are covered in the present paper, and which are not.

p.4354, l.25: It is true that the climate impact of ozone changes due to solar cycle variation has remained uncertain, but in view of the paper's scope (radiative forcing) I recommend to be precise here and make it clear that other aspects (local stratospheric temperature, surface temperature, dynamic feedbacks) of climate change are not addressed here (the next sentence indeed continues with radiative forcing).

p.4355, l.6: Mention here that it is not intended to address in the present paper all aspects of possible solar variation impacts on climate, in particular the cosmic ray - cloud interaction (or remove the sentence on that subject completely).

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p.4355, l.9: No comma behind "variations"

p.4355, l.24: Mention already here that including stratospheric temperature adjustment is essential to get the sign of the net effect correct (p.4362, l.20).

p.4556, section 2: The section is headlined with "Atmospheric processes" but contains only information on "chemistry" with some remarks on "radiation". I understand that radiative heating, subsequent temperature and ozone reaction rate changes, and dynamic feedbacks do not play a role within the scope of the paper. Therefore, "Atmospheric chemistry" appears to be the proper headline. If the authors like to retain the current headline, there should be two (small) sections on radiation and dynamics, explaining to which extent these processes are included and what their omission means for the limits of this study.

p.4556, l.18: "...range below  $\lambda$ 200 nm ..."

p.4556, equation R2: It should be mentioned what "M" means.

p.4557, l.13,15: Reformulate the sentence ending with "...where flux variations are smaller", as it is difficult to understand.

p.4357, l.18: "The model calculations give ...": Those described in 3.1.1, 3.1.2, or is this a priori information entering the CTM simulations ?

p.4357, l.20: Active chlorine is enhanced...": Why ? Give or refer to a reaction formula.

p.4358, l.1,2: Give reaction formula for NO<sub>x</sub> to HNO<sub>3</sub> conversion.

p.4358, l.12: An experiment design using GISS model winds to drive the tracer transport, while using NCEP observed temperatures to control the model PSCs obviously lacks the required consistency. It also seems that the same wind climatology has been used to drive the solar maximum and solar minimum runs, where distinct simulations (or pre-selected observations) for either case might have been preferable (and available !).

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p.4358, I.14: What do 21 layers mean for the vertical resolution in the lower and middle stratosphere ? Is it possible to resolve the vertical structure of the ozone change pattern (as shown in e.g., Austin et al., 2006), and of the driving wind data ?

p.4358, I.18: Which chlorine loading was chosen ?

p.4358, I.24: What is the actual number of evaluated simulation years ?

p.4359, I.4: "For comparison" is too diffuse a statement here. What are the merits and shortcomings of the two model systems ? (See also introducing remarks.)

p.4359, I.7: How many levels ?

p.4359, I.11: I wonder if the simulations of Haigh (1994) could use as an input the results provided by Lean et al. (1997), see p.4356, I.8,9 . Please, give a discussion of consistency (see also introducing remarks.)

p.4359, I.11: What is the actual number of evaluated simulation years ?

p.4360, section 3.2.1: I would like to see the analysed zonal mean cross section of the ozone change pattern (in volume mixing ratio) to get a impression of the data basic resulting in the Figure 2 profiles.

p.4360, I.6,7: Which years, hence, were actually chosen ?

p.4360, section 3.2.2: Are there large differences to the section 3.2.1. results for the overlap period ? I would like to see the zonal mean cross section.

p.4360, Figure 2: The averaging has been done for the percentage differences, haven't they ? I find it rather daring to claim a general similarity between the profiles here, except perhaps for Fig. 2c. Considering this, it is necessary to recall the main reasons for the deviating structure of the SBUV data here and give a discussion of their reliability (p.4361, I.7)

p.4361, section 4.1.: Confirm that the UiO radiation code produces the stratosphere

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adjusted radiative forcing. Has the code been used in previous ozone RF studies or compared with other codes ? Is the ozone consistent with the ERA-40 ozone in the UoR code (p.4361, l.21) ? If not, what does this mean when implementing percentage changes ?

p.4362, section 4.2.: Please, describe how the profiles have been implemented into the radiation codes. Have there been 3 simulations (SH, NH extratropics, tropics) for each case and the results converted into a global mean ? If yes, why did the authors not use the basic zonal mean ozone change distributions ? How were the (polar) regions treated that are not covered by the ozone observations ? Is there any notable contribution from polar regions at all (possibly the authors may give values for all 3 domains).

p.4363, l.16: "...as the RF from the ozone changes in the stratosphere"

p.4363, l.22: Does this mean that the ozone change pattern have been used in finer latitudinal resolution, or have just more calculations been done for more latitudes but the profiles as in Fig. 2 been retained ?

p.4364, l.9: "...this does not indicate significant dynamic effects." This is a problematic statement not supported by the extent of agreement found in Fig. 2. The most critical region (lowermost stratosphere) is not adequately covered by the observation (let alone the differences below 35 km altitude in Fig. 2b).

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