This is the response of H.E. Rieder and L. Frossard on behalf of all authors (AC written in italic) to the comments of Referee 1 (R1).

First of all we thank the referee for his/her positive judgment and valuable suggestions leading to an improved version of the manuscript. The individual points raised by the referee are addressed in the point to point reply below.

Specific Comments:

R1: p 13204 L15: The authors might wish to add some references here (the term "commonly used") suggests that there exists a sufficient number of references.

AC: We included several references according to the referee's suggestion. The modified sentence reads: 'The most commonly-used covariates include: the 11-yr solar cycle, the Quasi-Biennial Oscillation (QBO), (linear) trends attributed to anthropogenic ozone depleting substances (ODS), and atmospheric aerosol load after volcanic eruptions (e.g., Fioletov et al., 2002; Steinbrecht et al., 2006; Vyushin et al., 2007; Schnadt Poberaj et al., 2011; WMO, 2003,2007,2011).'

R1: p 13206 L12: I suggest to replace Part 1 by "provided in the companion paper by Frossard et al."

AC: We changed according to the referee's suggestion.

R1: p 13206 L18: The acronym "ARMA" should be defined. Also a short description and a reference (possibly to Frossard et al.) would be appropriate.

AC: The term 'ARMA' is now spelled out, but since this is just an introductory statement, we prefer to give the references later when we define the ARMA process.

R1: p 13206 L19: The purpose of the design matrix is hard to understand here, because its meaning becomes obvious only later in the paper. I doubt that the use of the design matrix is necessary because an explicit formalism for calculation of $\mu(x, t)$ is given anyway. Perhaps the design matrix formalism is necessary to technically communicate the model used to a particular software for optimization of the β coefficients but for the paper I think its use only adds unnecessary complication, since the model is given explicitly in Eq. 2. In the paper, the design matrix is actually never used. One can easily go directly from Eq. 2 back to p13207 l1 or to Eq 3 without involving the design matrix.

AC: General remark: We substantially edited the statistics sections of both papers (Frossard et al. and Rieder et al.) to improve clarity and readability. We thank the referee for his/her critical review and valuable comments and suggestions especially on these parts. We hope that these sections are now much easier to access for a broad audience.

As for the design matrix, we keep using it because we think that its generality is not only useful for software implementation but also to illustrate the possibilities of these models. Moreover, it

allows providing a covariate-dependent expression that is independent of any statistical model, as is illustrated through the structure of the revised Section 2.3.

R1: p 13207 L2: This expression contains undefined terms; at least I did not find a definition of $\sigma(x)$. It should be said in the text that the extreme values $y(x, t)$ are a function of the expected (modeled) values, $\sigma(x)$ (whatever it is, probably some standard deviation) and ξ (probably an autocorrelated noise term; but the text says that this is used only in the ARMA investigation. What is ξ here?).

AC: As stated above we significantly edited the statistics sections of both papers to improve clarity and accessibility. All points raised here by the referee should have been solved in the revised version.

R1: p13208 L1: Here the definition of ARMA is given but the acronym is already used on page 13206.

AC: In the revised version the term 'ARMA' is defined at its first appearance in the paper.

R1: p 13208 L3: Are multiple linear regression models which consider error covariances (e.g. von Clarmann, Atmos. Chem. Phys., 10, 6737-6747, 2010 not called multiple linear regression models? I find this statement misleading (but I do appreciate that autocorrelations are considered).

AC: We thank the referee for acknowledging the treatment of autocorrelation in our analysis. The referee is right that several studies accounted for autocorrelation in multiple regression analysis. Von Clarmann et al. (2010) show ways to deal with autocorrelation when analyzing atmospheric data and point out that autocorrelation is frequently not accounted for in such analyses. To acknowledge previous work on this important topic, we added a short discussion to the corresponding section in Frossard et al., but since we restrict the statistics part of the present paper to a strict minimum, we just refer to Frossard et al.

R1: p 13208 L5 and Eq 3: "design matrix" see above.

AC: See the reply above.

R1: p 13208 L10: The notation in the beginning of this line is confusing and probably not known be many atmospheric scientists. ….

AC: As stated above we significantly edited the statistics sections of both papers to improve clarity and accessibility. In the revised version, this notation is not used anymore.

R1: p 13208 L13: I would prefer an explicit reference to the Frossard et al, paper here, see above.

AC: A direct reference to Frossard et al. is now provided according to the referee's suggestion.

R1: p 13209 L16: The meaning of the p-values should be explained. In this paper it is not even said that they refer to a z-test. A short description what the meaning of the p-value is and what we can learn from them would be helpful.

AC: We included a description of the z-test and p-values according to the referee's suggestion already in Section 2.3 of the revised version. There, we also provide a direct reference to the paper by Frossard et al., defining the statistical models in detail.

R1: p 13211 L19: Is there a particular reason to write the SATO-index with capital letters, although it is not an acronym but obviously borrows its name from its inventor?

AC: We changed to 'Sato-Index' in the revised versions of both parts.

R1: p 13214 L2: The statement on ozone transport may be a bit oversimplifying, considering the lifetime of ozone (c.f. Brasseur and Solomon, Aeronomy of the middle atmosphere, Springer, Sect 5.2.3.).

AC: We thank the referee for this comment. As stated in Brasseur and Solomon (2005) the primary region supplying ozone to the extra-tropics is a narrow transition zone, where dynamics and chemistry compete. In this transition zone air flows fast and retains relatively high mixing ratios and the pole-ward and down-ward meridional flow through that transition zone feeds ozone in the extra-tropics and dominates the extra-tropical column ozone. Therefore, we updated the statement to 'Overall the strong ozone "collar" following the Mt. Pinatubo eruption suggests that enhanced transport of ozone to mid-latitudes may have happened, most likely in relation with the concurrent strong warm ENSO phase.'

R1: Figures 1/2/3/5/6/9/10: The axis labels and tick marks are far too small.

AC: We will check on this during the production phase of the revised version, as this problem also depends on the final placement of the figures.