Atmos. Chem. Phys. Discuss., 7, S5888–S5891, 2007
www.atmos-chem-phys-discuss.net/7/S5888/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the attribution of stratospheric ozone and temperature changes to changes in ozone-depleting substances and well-mixed greenhouse gases" *by* T. G. Shepherd and A. I. Jonsson

Anonymous Referee #1

Received and published: 11 October 2007

Review of "On the attribution of stratospheric ozone and temperature changes to changes in ozone-depleting substances and well-mixed greenhouse gases" by Shepherd and Jonsson

GENERAL COMMENTS

This paper presents a new approach to attributing both past and future stratospheric ozone and temperature changes. It makes the important distinction between attribution to CO2 and ozone (the traditional approach) and attribution to CO2 and ozone depleting substances (ODSs). The latter is a more robust approach as the coupling between ozone and temperature complicates attribution strictly to CO2 and ozone. The paper is well written and the arguments clearly presented. The paper presents new results that will be of interest to the ACP readership. I have only a few comments and once these have been addressed the paper will be suitable for publication.

SPECIFIC COMMENTS

Page 12328, line 23: I think you should come up with more precise wording rather than just 'atmospheric conditions'.

Page 12329, lines 7 and 8: I am surprised to see a paper with Ted Shepherd's name on it that states that significant ozone depletion started only in 1979.

Page 12330, lines 16 and 17: This is a key sentence in this manuscript i.e. in this formalism that you are developing in Section 2 you are assuming equilibrium conditions (I come back to this later). Anyway, at this point, when you say 'we are interested in long-term changes', you should say what you mean by 'long-term'. Is this decadal or many centuries?

Page 12330, equation (1): Consider the application of this equation in the non-equilibrium i.e. transient case when d(deltaT)/dt is not zero (actually I think my reasoning works under either case). The -c*deltaT term makes this into a first order differential equation which has an asymptotic response i.e. for a given fixed delta O3 and delta CO2, the time evolving delta T (which would start at 0) will tend towards/'saturate' to some equilibrium response. Now, I may be wrong on this but I would think that this 'saturation' happens because for a given impulse of CO2 (let's set delta O3 to zero for now), as time advances, the stratosphere cools and because CO2 emission of IR radiation depends on the ambient temperature, the IR emission by CO2 decreases. After a long enough time (when equilibrium is reached) the stratosphere has cooled to the extent that the IR emission by CO2 now, as in the case before the CO2 pulse to the stratosphere occurred, balances the UV/vis absorption by ozone. The key point here is

ACPD

7, S5888–S5891, 2007

Interactive Comment

that the cooling of the stratosphere by CO2 depends on the temperature perturbation itself so that equation (1) should be:

d(deltaT)/dt = 0 = a*deltaO3 - b*deltaCO2

with the b coefficient expanded as b=b_0 + b_1*deltaT to account for the temperature dependence of the CO2 cooling of the stratosphere, i.e.:

d(deltaT)/dt = 0 = a*deltaO3 - (b_0 + b_1*deltaT)*deltaCO2

which leads to:

d(deltaT)/dt = 0 = a*deltaO3 - b_0*deltaCO2 - b_1*deltaT*deltaCO2

If we now set b_0=b and b_1=c we get

d(deltaT)/dt = 0 = a*deltaO3 - b*deltaCO2 - c*deltaT*deltaCO2

which is very similar, but not identical, to your equation. That 'saturation' term at the end scales with CO2 in my case. I am not sure if I am right, but if I am, how does that change everything in Section 2 that comes after equation (1)?

While I am demonstrating my ignorance, let me just ask one other possibly silly question regarding equation (1): ODSs (I am thinking specifically now of CFC-11 and CFC-12) don't themselves induce any radiative cooling of the stratosphere in the same way that CO2, CH4 and N2O do, do they?

Page 12335, lines 7 and 8: It is not clear to me what you mean by 'Note that changes in CO2 or ODSs at a single altitude are not physically realizable.'

Page 12336, lines 7 and 8: All of the formalism developed in Section 2 was based on the assumption of the equilibrium response i.e. d(deltaT)/dt was zero and d(deltaO3)/dt was zero. Now it seems that you're going to be interpreting the TRANSIENT response in a CCM in terms of the Section 2 formalism. I am therefore a little worried. Should I be?

ACPD

7, S5888–S5891, 2007

Interactive Comment

EGU

Page 12338, lines 7 to 19: It is not clear to me whether these values have been derived within the formalism developed in Section 2 in anyway. If so, I would be worried for the same reason as outlined in the previous comment.

Page 12341, line 8: The problem with moving to a multiple linear regression analysis is non-orthogonality of your basis functions. The CO2, CH4 and N2O time series would be very similar in shape and the regression will not partition the variance across these three in a very robust way.

MINOR GRAMMAR AND TYPOGRAPHICAL CORRECTIONS

Page 12329, lines 21 to 23: Remove the parenthesis around this sentence.

Page 12336, line 14: Do you mean that the rate of increase in CO2 from 2010-2040 was approximately twice that from 1975-1995? If so, maybe just say that.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12327, 2007.