This paper describes the implementation of a troposphere-stratosphere iodine scheme in the SOCOL CCM. The scheme is based on the well-used CAM Chem scheme. I think that after the suggested revisions the paper will largely serve the purpose of documenting the model iodine scheme and will be suitable for publication in GMD.

I thought that the evaluation of the model was somewhat superficial and some of the text was not clear. The whole paper would benefit from a thorough proof-reading (see some examples below).

Specific Comments

(1) General. Use of the word 'loss'. The manuscript mentions ozone 'loss' throughout, including the abstract (line 5). Really, what is shown is the ozone difference between a model run with and without iodine. The word loss (like 'depletion') to me implies some time trend or change. (There is also photochemical production/loss rates but that is not what is being shown either). If there was no change in the iodine emissions then this difference would always be present (subject to trends in reaction partners), it was just that models were not so accurate without it. So, I suggest reading through the paper and being clear what is being shown by the difference between the model experiments.

(2) Line 6-7. Confusing because the number range quoted is globally averaged so we have no idea of the maximising value at high latitude.

(3) Line 10. Confusing because the sentence appears to be about the lower troposphere but then discusses 50 hPa. Maybe change 'and' to 'but' and explicitly state that 50 hPa is in the stratosphere.

(4) The importance of iodine (or not) depends not just on how much ozone might be destroyed by iodine but by any time trend in the abundance. I don't think these results 'constrain' anything – they show the sensitivity.

(5) Line 135. Hadley Centre (spellings)

(6) Line 149. Use of word 'recur' not clear to me.
(7) Line 207. 3 x CFC11

(8) Line 211. Why ‘correspondingly’?

(9) Line 236. Write dt with Delta as in the equation.

(10) Line 253. Experiments. It is commendable to have run 10 ensemble members for each experiment but I cannot see that much use was made of the variability between them. It could be interesting to know how large this variability is. On this point, it is not clear if the SD in e.g. Figure 1 includes this or is just based on the zonal mean of the ensemble mean?

(11) Line 261 ‘COMPARED to present-day’. Also, why is this a worst case? You cannot assume that. It is just an assumption to investigate the sensitivity.

(12) Line 264. ‘we’ -> ‘were’?

(13) Line 276. Figure 1 caption. Last line, why ‘ozone’?

(14) Lines 286 - 291. ‘peculiarities’ and text around this. This is not clear. The aim of GMD papers is to explain model behaviour and assumptions like this. The text needs clear rewriting to explain if the non-conservation is an issue and at what point it happens or is forced. The blue profile in Figure 1 looks very fixed on 1 pptv. Is that coincidental? The red line seems to show a bit more variation.

(15) Line 300. The evaluation with the TORERO data is very crude. If there is a reason for this (e.g. free running CCM) then please state it. Why not sample the model like the observations? The ‘doubling’ assumes the same length of day/night. Roughly ok for the equator but this is just Jan/Feb so will be biased at other latitudes.

(16) As a general point the model evaluation with observations is very brief. What about data from other sources, e.g. the balloon and ground-based data mentioned in the introduction?