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Interactive comment on "Future Arctic ozone recovery: the importance of chemistry and dynamics" *by* E. M. Bednarz et al.

Anonymous Referee #3

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This article is based on an interesting study of the projection of future Arctic ozone using ensemble simulation from the UM-UKCA chemistry climate model. While other studies have been performed on this subject (e.g. WMO, 2011; Langematz et al., 2014), the originality of the study lies in the use of ensemble simulation, which allows the authors to estimate the intrinsic variability of the stratosphere, together with the impact of ozone depleting substances decrease and climate change on Arctic ozone. The paper is well written and informative for the projection of future Arctic ozone and I recommend publication in ACP, provided that important comments for improvement are taken into account.

Main comments

âĂć The main focus of the study is on the respective contribution of chemistry and

dynamics on future Arctic ozone. In that respect diagnostics have been set up in order to evaluate the importance of chlorine chemistry in future ozone loss and the authors argue that halogen chemistry can still play a substantial role after mid-century. Since this result is rather intriguing, it deserves more attention in the article. A whole section is dedicated to the case study of winter 2063 but it is somewhat descriptive and does not demonstrate fully that the chemical loss is linked to halogen chemistry. For example, is the observed loss coherent with the known relationship between Cly levels, chlorine activation and PSC volume (e.g. Rex et al., 2004)? What is the role of nitrogen chemistry that can sometimes be important in the Arctic mid-stratosphere as shown in Kuttipurath et al., 2010? A quantification of PSC volume and a figure similar to Figure 2 but showing observations in order to demonstrate the skills of the model to simulate halogen chemistry would be useful.

âĂć Since the Arctic ozone loss is computed over the 65-90°N latitude range, it encompasses some loss from non-vortex air. This issue is acknowledged by the authors but would need some quantification.

âĂć From Figure 1, it seems that the interannual variability of Arctic ozone from the ensemble simulation is larger than the natural variability as seen from the observations. Can the authors comment on that and provide some statistics on this issue? In addition a more substantial description in section 2 of the skills of the UM-UKCA model in terms of polar ozone simulation is needed: e.g is there a cold bias of the polar stratosphere? How the strength and duration of the Northern vortex compare with observations, . . .?

âĂć Temperature trends: No mention is made of the evolution of the occurrence of sudden warmings in the ensemble simulation. It is thus difficult to distinguish radiatively induced with dynamically induced temperature trends. This issue should be addressed.

Minor comments

P5 l24: it is not clear how the 11-year solar cycle is simulated over the 21st century.

P7 l21: the products of the reaction are wrong: it should be Cl + O2.

P6 l4: This sentence is not so clear. The computational efficiency relates to the use of the diagnostics for evaluating halogen induced ozone loss.

P9 l21: what is the contribution of slowing of gaz phase ozone loss cycles compared to changes in stratospheric transport in the earlier recovery of Arctic ozone?

P11 l4-13: In line with my major comments, the causes of the drops of Arctic ozone in late century, and the comparison with Langematz et al. (2014) study should be better substantiated.

P12 l10-18: a chemical loss of 40 DU is similar to the current Arctic ozone losses, while chlorine levels in 2061-2080 will be lower by more than a factor of 2. What PSC volume is necessary for such extreme loss?

P16 l13: what is the justification for the PV value to define the vortex?

Figure 5: Case study years (2060 and 2063) should be highlighted in the figure.

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