

Interactive comment on “Stratospheric carbon isotope fractionation and tropospheric histories of CFC-11, CFC-12 and CFC-113 isotopologues” by Max Thomas et al.

Anonymous Referee #2

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This is clearly a very important subject, and an important, unique data set. It is therefore critical to make the most of the opportunity which I feel is not being done at present. More work should be performed in order to put the results of the analysis and modelling into context. 1. The reader is left themselves to try to make sense of the discrepancy in the $\delta_{T(13C, CFC-113)}$ data before 1980. The authors write provocatively, 'While this discrepancy may be indicative of a change in $\delta_{E(13C, CFC-113)}$, it is premature to assign one.' I believe the discrepancy could also be indicative of faults with the sampling, analysis, and modelling and that either it is premature or it is not. The authors should decide if they have confidence in the conclusion and wish to defend it, or perhaps, based on statistics, model validation and so on, they would decide to withdraw.

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If they support the conclusion I would suggest doing more work to investigate what this change would be, for example change in manufacturer or process. 2. More work is needed to put the results into perspective. What is known now that was not known before? How will the results be used? Were the CFC budgets under-constrained, or will these results provide additional insight into stratospheric changes or processes in firn, or ? 3. Does the derived stratospheric photolytic fractionation factor match the predictions of theory and experiment? 4. There are a number of technical issues listed by another reviewer which should be addressed.

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