We thank reviewer 1 for their comments and suggestions which have helped to improve the representation of the study. We specifically address their points below and revise the text accordingly.

This is a very well written paper which contains the development of a new parameterization for lightning flash rates for use in chemical transport models and contains a comprehensive evaluation of this new scheme as well as four existing schemes. The new parameterization uses the cloud ice content from the ECMWF ERA-Interim reanalysis, along with the upward cloud mass flux and cloud fraction to estimate cloud ice flux, which is an important component of the non-inductive theory of cloud electrification. The authors found that including the cloud fraction data in this calculation was an important feature. The results show that by a variety of statistical measures, the cloud ice flux scheme outperforms the existing schemes in comparison with lightning flash observations from the Lightning Imaging Sensor (LIS).

One significant piece of information that is missing from the paper is a description of how the cloud ice is computed in the ECMWF model. Is it computed through some interaction of the convective and microphysical parameterization schemes?

RESPONSE. We thank the reviewer for noting this omission and we have added an additional paragraph in section 2.1. The scheme is based on that of Tiedtke (1993). The microphysical parametrisation indeed interacts with the convective scheme through using detrainment from convection as a source of cloud condensate. The phase of cloud condensate is then diagnosed according to a temperature-dependent function. We have described this in our new text.

Otherwise, my comments are more minor, and are listed below:
p. 17825, lines 7-8: Need to be explicit and put the names of the schemes here, rather than make the reader look ahead to all of the subsections to find them. I suggest a wording such as, " ....for the cloud top height, updraught mass flux, convective precipitation (polynomial), and convective precipitation (linear) parametrisations of Sections 3.1 - 3.4, and 1.09 for the new cloud ice flux scheme of Section 4.

RESPONSE. We have adjusted the text as suggested.

p. 17828, lines 15-16: .....been introduced into large-scale lightning parametrisations due to lack of sufficient microphysical detail in global models. Improved representation of cloud ice in global models now allows....

RESPONSE. Thank you for the suggestion of referring to the microphysical modelling ability of models. We do agree with the statement. The positive results from this study suggest that ERA-Interim has modelled microphysics sufficiently well to consider using an ice-based lightning parametrisation. However, we have decided not to include this amendment because we do not wish to suggest that further improvements to the microphysical schemes are not important. The results here encourage the consideration of such a parametrisation but further improvement of microphysical schemes is essential to understanding the origin of biases in the approach as well as enabling the introduction of graupel in order to complete the mechanism.

p. 17831, lines 8-9: Need to note that the cloud ice flux method only improved the correlation with LIS lightning flashes by 0.06 over cloud-top height, which is a non-microphysically based method.

RESPONSE. We have adjusted the text as suggested.

p. 17833, line 6: Minnesota should be changed to New Mexico

RESPONSE. We have adjusted the text as suggested.

p. 17840: The authors conclude that the updraught mass flux and convective precipitation (polynomial) schemes performed the poorest. Both of these schemes were developed using data from another reanalysis. I wonder if that might play a large role in their poor performance here when used with the ECMWF ERA-Interim reanalysis? Some comments concerning this issue would be appropriate on this page. At a minimum, perhaps lines 14-15 could be modified to "....found to perform poorly for the metrics used here, at least when applied using the ECMWF ERA-Interim data."

RESPONSE. We agree that that the performance of the polynomial parametrisations would not necessarily be the same in some other reanalyses and models and have acknowledged this as suggested. We do note that similar poor performance has been found in other studies though (Tost et al., 2007, Murray et al., 2012). We encourage the further testing of the findings with other models and evaluation studies in the conclusion.

p. 17847: Figure 1 caption: .....annual LIS totals for each of the 10-year, 5-year, and 2-year climatologies, respectively.

RESPONSE. We have adjusted the text as suggested.

p. 17852: Figure 6: An issue that has appeared in the application of most lightning schemes and with most input meteorological data has been the overestimate of flashes in the Amazon Basin and the underestimate in Central Africa. The cloud ice flux method does not appear to have solved this problem! This result should be mentioned in the text. The authors note in the text the Central African underestimate, but should also point out that the Amazonian problem still exists, but it is not as severe as with the cloud-top height approach.

RESPONSE. Thank you for the suggestion. The improvement but not complete solution of South American biases by ICEFLUX has been stated in a new paragraph in section 6 along with relevant discussion of the LIS SAA biases (a related point raised by reviewer 2).