Dear author,

Thank you for your revised m/s alongside details of your edits and response to the second review. The inclusion of the range bin uncertainty is helpful. However, I do not believe you have adequately addressed the review comments. The critical issue relates to point 3 made by the referee, which you do not address at all in your response. The question is whether, and to what extent, error sources in derived accumulation are auto-correlated in space (see referees comments). This is a critical issue that you do not discuss at all. It is critical because if, as seems reasonable and likely, there are, for example, systematic errors in layer date along a flightline or systematic bias in density spatially, the assumption that the errors are normally distributed is invalid and the errors must be summed, rather than taking the RMS.

You have provided no evidence and no argument for why the date and density errors should be randomly distributed. The assumption that the range bin errors are randomly distributed seems more plausible but again is unproven. This issue is not insurmountable if, as the referee suggests, you are more qualified in the statements you make.

The uncertainty in MAR is unknown. It is impossible for me to deduce (as you claim) from figs 4, 5 and 10 what the relative magnitude of the differences between MAR and the radar-accumulation rates are. Further, your errors are all one sigma, while a two sigma uncertainty would likely encompass most/all the differences between MAR and the radar data. In other words, you have not demonstrated here that the radar data are “right” and the MAR data “wrong” or that you can use the former to evaluate the latter. It is reasonable to compare the two but not to claim that the radar data can evaluate the performance of MAR. You need, therefore, to reword the discussion of this. It is a comparison, not an evaluation.

Specific points (page and line numbering as for authors response)

p4,l20 dominate -> dominant

p5, l11 a prediction is something relating to the future. You mean a reconstruction as this is about the past.

p7, l20 it is unclear here whether 25 km refers to ERA-interim (the last noun) or MAR. need to reword

p9, l23. This statement assume that there is zero uncertainty in the measurement, which is typically ~10% for near surface density.

p10, l7 missing “of”

p11, l1. modeled -> modelled

p11, l10 dominate -> dominant

pll, l17. This is a **very** bold and unproven assumption. You need to justify this, e.g. “Here, we assume that the errors are uncorrelated and normally distributed. While we acknowledge that there may be spatially correlated errors along flightlines, for example, averaged over the ice sheet, and several seasons, we believe this is acceptable to first order.”

p15, l4-15. replace predict with reconstruct. This section is problematic. Lines 8-10 imply that the RCM is somehow tuned to observed accumulation rates, which is incorrect. L-14-18 are also problematic and incorrect. This claim about lateral boundaries is repeated later (P17, l20-29). If this was correct then it should be a systematic trait of the model but it is not. 2010 shows larger values

WRT the radar data and 2012 shows good agreement. The boundaries have not changed in these years.

P15, l15 rather than saying “underestimate”, or “overestimate” you should say that MAR produces higher/ lower estimates relative to the radar data.

P15, l19 Figure 10 -> Figure 11

P16, l23 “are useful for evaluating” -> “have the potential for evaluating”

p17, l14 modeling -> modelling

p17, l17 here you acknowledge that there is a systematic bias.

p17, l20-29. See earlier comment. This is not correct.