Referee's comments in black. Author's response in purple italics

Referee 2 comments

Major Comments

Introduction:

Several key references, and some description therein, is missing in the introduction's description of the state of knowledge about orographic precipitation, its relationship to standing gravity waves, and the undulations of the melting layer. Regarding the first two, I refer the author to two widely cited review papers on orographic precipitation, Roe 2005 and Houze 2012, and two book chapters, Colle et al. 2013 and Stoelinga et al. 2013 (both are chapters from the same book). Regarding the melting layer and its variations in height with respect to the terrain, Minder et al. 2011 thoroughly explores the contribution of three mechanisms, two of which are described in this paper (albeit through a somewhat different lens). Although Minder et al. (2011) focuses on an idealized case where the snow line intersects the terrain, the discussion of mechanisms that modulate the altitude of the melting layer are highly relevant to this paper. Significantly more attention to prior literature and discussions about the mechanisms at play is necessary for this paper to adequately address its contribution.

The author thanks this referee for their careful reading and the many useful suggestions which have improved the paper.

Line numbers refer to those in revised manuscript.

I had not intended the introduction to cover the whole subject area of orographic enhancement of precipitation but agree there is value in citing the papers suggested in order to provide a comprehensive background, and have included references to all the papers suggested, mainly in the introduction. I have introduced ideas about the melting level relating to the paper of Minder et al. in the discussion in section 4.1 lines 536 onwards. I have also cited Stoelinga et al., 2013 in this section and expanded the discussion of diabatic effects of melting snow (paragraph line 549 onwards. This has prompted me to rework the analytical treatment in the Appendix to include an explicit diabatic term D as explained below).

Section 2 and associated Appendix:

These sections need considerable rewriting and reorganization to more clearly state why each equation is shown, how it is derived, and critically what assumptions are made in its derivation. In addition, much of the language surrounding the equations is vague and/or conversational; this section should be explicit and extremely plain with its language, for clarity.

To address the charge of conversational language in section 2 (now section 4) and the Appendix, I have reworded sections in various ways, including eliminating use of the pronoun 'we' as in 'we find that...' by using constructions in the passive voice, e.g. 'it is found that...'. I have replaced the conjunction 'so' where appropriate with the more formal equivalent 'therefore'. I do not list all the lines at which this is done.

I have also reworded Section 4 and the Appendix with a view to clarification. The equations are all derived from first principles and there is a balance to be struck between concision and

ease of following, but I have added extra explanatory text, e.g. lines 670- 671, 677 – 679 and Fig. A1 where positions and heights referred to in equations are shown diagrammatically. I have expanded the assumptions listed, e.g. lines 708 – 709, and for clarity and completeness extended the previously adiabatic treatment to one which caters for diabatic effects by introducing term D, line 736 onwards. This makes the treatment somewhat more complete and changes subsequent equations including the results in equations (5) and (A11) but leaves the overall isothermal, limiting case unchanged (equation 6).

Paper structure

The paper begins with its derivation of its precipitation trajectory mechanisms, followed by some discussion of those mechanisms, and then goes into a case study. This structure seemed back-to-front to me, and the story would have been significantly more clear had the paper been structured as follows: Following the introduction, the data and methods for the case study analysis should be clearly laid out including a description of the two models (UKV and GWM) and any other data used in the paper as well as a description of the particle trajectory software/process used (perhaps this is part of the GWM but it is not clear). Then, the case study could be described, and used to motivate the derivation of undulating melting layer+GW bunching enhancement. The last results section should then apply the enhancement to the case study (pages ~17-21 of the current paper) with some discussion of the value added of the new method, and the paper can then end with a conclusions section.

I have followed all these suggestions. The reordering of the paper structure necessitates rewording in multiple places to change backward and forward references to other parts of the paper, too many to list here. The data and methods section introduces a new source not included in the original submission, the Copernicus reanalysis data (CERRA, lines 64-69) which provide evidence for cloud and precipitation data relating to the case and also some verification of the UKV fields. Some discussion has been included, as suggested, in the results section (4.2) focussing on its worth as a conceptual model of a category of rainfall enhancement which seems to have been overlooked (lines 616 – 620)

Figure use and reference

In general, the figures should be referred to at specific sections of the text when they are discussed.

I have made some changes, including dropping the early reference to Fig. 8 before 5,6,7 (numbers now all change due to change in ordering).

Minor Comments

1. 25: I suggest adding ‘mechanisms’ to the text ‘One of the first to be described...’ so that it reads ‘One of the first mechanisms to be described...’

   Done.

2. 26: I suggest adding ‘moist’ to ‘... replenished by the ascent of air...’ so that it reads ‘... replenished by the ascent of moist air...’

   Done

3. Figure 1: I suggest you add a vector indicating the wind is blowing from the left.

Done

4. 57-58: This sentence requires considerable assumptions, e.g. that the evaporation doesn’t change across the interface, etc. More attention should be given to the assumptions made prior to each assertion.

   Assumptions elaborated, including evaporation, sublimation and melting (Lines 388 – 390).

5. 70: Is w_s assumed to be negative or does the negative sign before w_s in the equation capture the downward direction (i.e., the sign conventions used are not clear)?

   Clarified. w_s is negative so – w_s is positive. (Line 409)

6. Figure 2: I believe this figure is intended as a toy schematic for teasing apart the mechanisms, but this is not clearly described, and as such it is simplified to the point of being incorrect.

   I have clarified the assumptions made lines 420-425.

7. 81-83: These three sentences need some revision for clarity.

   Clarification as above and Lagrangian model expanded upon lines 86-90.

8. 96-98: This should refer back to Figure 1.

   Done, lines 441 (though now Fig. 1 is Fig. 13)

9. 104: Stout et al. 1997 should also be cited here.

   Done

10. 129: ‘So the magnitude of modulation...’ this is extremely conversational and needs to be revised for clarity.

    Deleted since not important.

11. 146-149: Where has this analysis been done? Is this testing not shown?

    More explanation lines 499 – 505, and Fig. 15 added which shows scatter plot of analytical vs modelled results. Not totally satisfactory since I didn’t find a way to highlight how variation in individual parameters influences different scatter plot

results but it conveys the overall sense of accuracy, along with quoted mean absolute error stats.

12. 220-224: This section of text poorly described and needs expansion for clarity.

Deleted since not important.

13. 251: This is, I believe, the first introduction of the UKV, and it needs to be defined.

Now introduced in new Data and methods section lines 58-60.

14. 254: A map of the analyzed rainfall should be included as one of the figures for the case study.

    A map of reported raingauge measurements colour-coded according to amount has now been included (Fig. 2). This is not analysed with isohyets, but I feel this sort of analysis can be misleading because we don't know what is happening in individual peaks and valleys, and colour coding goes some way to visualising spatial distribution at a glance. Time evolution of rain at Honister also included.

15. 274-279: This text which describes the UKV model should be moved into a section where data and methods are described (adjacent to the GWM description). Why were moist variable data unavailable?

    Done. Moist variable data were not presented to me as a menu option when extracting data. The shortfall has been supplied by using CERRA moist data, as in Figs. 6 and 7.

16. 293-294: 'Note that only the region...' is not a strong start to a new section. Transitions should be used to make the paper more readable.

Deleted since not important.

17. 300: Figures 5, 6, and 7 were not referenced before this reference to Figure 8.

Early reference to Fig. 8 dropped.

18. 308: This satellite imagery is not shown.

Reference to satellite imagery dropped.

19. 326-331: These experiments are presumably not shown; 'not shown' should be explicitly stated.

    Done (line 216).

20. 336-338: Since these are 24-hour averages, an alternative hypothesis would be that any diabetic/other effects that generated vertical velocity and precipitation occurred randomly through the domain at shorter time intervals and thus when averaged, their signal was largely removed.

Done (line 228).

21. 370-372: This sentence needs revision for clarity.

Done (line 269-273).

22. 381: principal->principle

As far as I can see the original is correct, so this is left unchanged (now line 282).

23. 413-416: The paper should refer to its equations for the enhancement calculation.

Done (line 327).

24. 526-545: This section of the appendix provides an example of what I'm suggesting in my Major Comment regarding Section 2 and the Appendix: This section describes an expression for the slope of an isotherm, but does not motivate this derivation by noting that it will be applied for a specific isotherm, the melting level. It also needs a bit more thorough defining and discussion for each equation shown (and for any inferences made between equations).

Along with the rewording to make it seem less conversational, I've tried to address this criticism by adding more explanation at various points through the Appendix to explain the approach, e.g.

Line 670, rationale added for derivation of expression of isotherm.

Lines 709 – 713, description added to prepare the reader for the derivations which follow.