Anonymous referee #1 comments are in black and author responses are in purple italics.

General comments:

The author introduces an interesting thought experiment regarding the role of orographically-induced gravity waves on precipitation distribution across the Isle of Man and the Lake District of northwest England. Unfortunately, the data presented in support of these ideas is insufficient to justify publication in a peer-reviewed journal. The author relies on relatively coarse operational model output that does not include any moisture variables. Additionally, verification of this model output is virtually absent, which significantly limits its credibility, especially when discussing orographic precipitation processes. A separate, higher resolution gravity wave model is also employed, whose results seem to be consistent with the operational model output. However, this does not mitigate the aforementioned lack of model validation. It also does not mitigate the absence of moisture variables in the analysis dataset. The author has to make numerous assumptions in his analysis to get around this limitation. I find many, if not most of these assumptions to be dubious.

Ultimately, the author makes assertions that are not supported with credible evidence. As a result, my recommendation is to reject the manuscript for publication.

Thanks very much for reading my paper and taking time to comment on it. I address these general comments beneath in response to your specific points below. As a general point, I wanted specifically to put cloud microphysical processes to one side in my treatment, not to minimise their importance, but to indicate that I am elucidating a process which arises due to the dynamics/kinematics; that what I describe occurs independently of much of the microphysics, can be considered quite separately as a complementary process, and does not require high vertical resolution model data to examine. Incidentally, the main point of the paper was to draw attention to the rainfall intensification/redistribution mechanism, illustrating it graphically and deriving an analytical quantification for it, since I don't think this has been done before. You don't explicitly comment on the main analytical result, that rain rates can be increased by a factor approaching rain fall speed divided by snow fall speed through the effects I elaborate.

1. sections 2 and 3: The author goes into great detail about how gravity waves and an oscillating melting layer can impact the spatial distribution of precipitation across orography (i.e., Figs. 1-2; Equs. 1-4). He calls the areas of precipitation where the "trajectory" lines are closer together more "intense" than the areas where the trajectory lines are farther apart. If precipitation intensity is based on an areal integration, this may be an appropriate interpretation. However, precipitation intensity is typically based on precipitation rate, which is a mass flux for a given vertical column. The only way that precipitation intensity can be enhanced is by adding mass to the volume of hydrometeors through cloud microphysical processes. The author is not really addressing precipitation enhancement; rather, he is addressing precipitation redistribution.

But if you redistribute a given mass of precipitation into a smaller area, its intensity necessarily increases whether the area is the size of a raingauge or a river catchment. There seems to be a substantive difference of opinion between us here since I don't agree with your assertion that the only way precipitation intensity can be enhanced is by adding mass through microphysical processes. I think I am using the same definition of 'intensity' as you, i.e.

precipitation rate as commonly represented in mm/hr. Where precipitation trajectory lines become closer together, precipitation which originated over a given depth in a column is concentrated into a column of narrower cross-sectional area, with increased water mass flux relative to that which would occur in the absence of the effect. Mass flux is measured in kg per metre squared per second (numerically equal to 3600 times the mm/hr value), so is defined in terms of a mass passing through an area (rather than a volume) per unit time. If you reduce the area over which it falls, a given mass of precipitation results in a greater intensity. Perhaps I did not expand on this point fully enough (and I hope I'm not labouring it here), but I had assumed that it was demonstrated in Fig. 1 and accompanying text lines 56-59. Unlike enhancement through cloud washout, the total area integrated water mass is unaltered; rainrate and therefore rainfall totals are just intensified by being concentrated in a smaller area.

I wonder whether the use of the word 'enhancement' is the cause for confusion, since in the context of rainfall it is often used in specific sense of adding mass to existing hydrometeors e.g. through washout. But here I was simply using the word consistently with its definition as 'intensification' or 'increase' in the same way one might talk about enhancement of convection or enhancement of a jet. Perhaps I should have made all this clear or used another expression such as 'rainfall intensification' or 'rainfall focussing' but I did try in several places to draw the distinction with microphysical mechanisms. Whatever it is called, I think I have presented strong evidence that it occurs and can have a dramatic effect on measured rainfall rate, increasing it severalfold.

2. sections 2 and 3: Does the author have any observational evidence to support the notion that a melting layer can oscillate as he hypothesizes? For example, are there any radar studies that show a bright band that oscillates in such a manner?

In case there is any confusion I did not wish to imply that the melting level oscillates in the examples given, but that it varies in height with horizontal distance – the examples I give are steady-state. I do not have specific observational evidence but it is an inevitable consequence of adiabatic temperature response to vertical velocity in a stable airmass where temperature decreases with height, and can be verified with a simple thought experiment. It also comes out explicitly in my analytical treatment (e.g. eqn A6) and is clearly evident in the model output shown, where the close mirroring of the orography by the melting level would be too coincidental to be ascribed to anything else.

3. L274-279: This paragraph outlines the very limited nature of the data available to the author. This data was in the form of 24-hour mean fields from a single operational model simulation. The data was limited to horizontal and vertical winds and temperature. No moisture variables were available (i.e., water vapor, precipitating ice and/or liquid water). While the horizontal resolution was 1.5 km, there were only 14 vertical levels (~600 m resolution near the melting level). No attempt at validating the model output is evident. This dataset is clearly insufficient for addressing the processes discussed by the author.

I address the two points made here, (1) absence of verification and (2) sufficiency of model output for addressing the processes discussed.

(1) I concede that no attempt was made at validating the model output, but this would be difficult given the absence of observations of vertical velocity. Given that it is a short period forecast it is considered that the model is unlikely to have been incorrect in the overall signal, I have taken it as 'truth' for the purposes of revealing likely flow and thermal

structure. I agree that it would have been good to have the model rainfall predictions for verification but unfortunately I cannot get them. I should have stated more clearly that I was taking the model as a proxy for 'truth' and justified that choice. As such it is an internally consistent source of data which supports my mechanism in generating significant intensification of rain in the right area and qualitatively explains the west-east variation in reported rainfall values as given in Fig. 10. It seems more than coincidental that there was extreme rainfall at Honister. The advantage of such a simple model with no cloud physics (beyond specified fall speeds) over a sophisticated cloud physics model is that the output results largely from effects treated in the analytical approach, which is what is being tested.

(2) I should clarify that the UKV model from which output was drawn had 70 vertical levels and was a state-of-the-art mesoscale model which was used to inform the successful issue of flood warnings at the time. However, I was unavoidably using a dataset with only 14 of those levels. I do not agree that this dataset is insufficient for addressing the processes I discussed because the output reveals well-defined structures in the vertical velocity field which were much larger than the vertical resolution available, so it seems to me that it is not a serious shortcoming; whilst being potentially problematic for determining lapse rate over small distances, as I drew attention to, any such insufficiency would have the effect of lessening the mechanism I was investigating. Despite this, there is clearly a lot of detail evident in the model melting level.

So I contend that the model output was adequate for my purposes, that of tracing nominal precipitation trajectories, thereby giving reasonable corroboration for my derived equations, and revealing large scale dynamical structure to compare with the gravity wave model. If I had been doing a study of microphysical precipitation mechanisms then this would clearly be different and the absence of humidity data would obviously have been a serious shortcoming.

4. L333-338: This paragraph makes a sweeping assertion: "However, the generally good correspondence between this output and the UKV, despite all the caveats, along with the remarkable steadiness of rain rate at Honister, strongly suggests that gravity waves were the main driver for vertical velocity over the period of extreme rainfall and therefore for the rain itself." The operational model output and corresponding gravity wave model output do not provide sufficient evidence to support this assertion. In particular, the lack of moisture variables eliminates evidence of possible alternative explanations based on cloud microphysics. The author does not present evidence about the depth of precipitation and whether there are hydrometeors aloft that could be influenced by the vertical velocity patterns described. For all we know, the precipitation could be very shallow in nature.

Clearly cloud microphysical processes are indispensable to precipitation production, but my starting point is that significant ascent (upward motion) is necessary to produce the rapid cooling and supersaturation required for significant precipitation, and that once formed the hydrometeors would move largely with the wind – both these points seem uncontroversial to me though perhaps I should have expanded on them. I tried to indicate (e.g. line 340) that I was not dealing with the microphysical part of the problem, but acknowledge its importance as an additional factor (e.g. line 145, 461). Large scale dynamic / kinematic explanations for drivers of precipitation are in general clearly separated from the microphysical; your point about alternative, microphysical explanations to the gravity wave explanation seems to be at cross-purposes, in the way that someone might say of another situation 'how do you know that it was the cold front which caused the rain and not microphysical processes?'. I would *go further and say that for this case cloud microphysics cannot provide a good alternative explanation for the cause of the rain because without strong ascent to provide rapid generation of solid/liquid water, no microphysical process could generate such large totals.*

By the same token it seems highly unlikely that the precipitation was very shallow in nature since all empirical evidence I'm aware of indicates that deep layers of precipitation-producing cloud are necessary for very heavy rain. Perhaps I should have made all this clearer.

5. L461-463: The author states: "Of course, augmentation of rainfall by wash-out of droplets from the feeder cloud, essentially a cloud physics problem, is not included in this, neither is the growth by riming whilst ice particles are in near suspension over the windward slopes as supercooled cloud droplets rise around them." This apparent "disclaimer" does not make any attempt to diminish the significance of these processes. It is quite possible that these processes are the dominant factors in the precipitation distribution associated with the case.

Yes, I certainly agree that cloud microphysical processes are very likely significant in the enhancement of precipitation in this case and could be the dominant factors for intensity if not distribution. I'm not sure what you mean by 'disclaimer' but it was not meant in any way to deny the contribution of cloud physical processes, rather to indicate again the limits of my quantitative treatment. They have generally been well documented and their operation is quite well understood. I merely wanted to point out that before one even considers them, the mechanism to which I have drawn attention, which to my knowledge has not been described before, is likely to have intensified precipitation by a significant factor. Whatever enhancement multiplication factor might be attributable to microphysics, it is acting on top of the effect which I have elucidated.

6. L494-496: The author reasserts an unsupported conclusion that the rainfall in the case study was "principally driven by gravity wave motions".

If one accepts that heavy precipitation must be driven by ascent, and that a pure gravity wave model reproduces the primitive equation model's pattern of ascent quite well, then I hope I have presented quite strong, if not overwhelming, evidence that the precipitation was caused by gravity waves. Of course one could also tackle the problem from a different angle, take the dynamical processes as a given and consider the precipitation as being caused by a whole train of microphysical processes from ice nucleation, sublimation, riming, aggregation, cloud washout etc. for which you would need a much more complex numerical model, which is perhaps what you are thinking of. This was not my purpose. The effect I'm investigating is complementary and, as far as I'm aware, has not been described before. I'm not setting it up as a rival explanation for rainfall enhancement, just an additional factor which reveals a surprising quantitative relationship and, I hope, an illuminating conceptual model.