

## ***Interactive comment on “Growth-deviation model to understand the perceived variety of falling snow” by J. Nelson***

**J. Nelson**

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Reply to Referee 1 General comments (labeling a-d below is my own)

a) About there being many assumptions, my view is that one must start somewhere. And this is a start - I have found no previous studies in which the origin of the crystal diversity is modeled. For a clearly messy situation - complex growth forms in a turbulent cloud - one must start with an idealized model. Otherwise, readers become bogged down in details and gain little understanding. Moreover, in the revised manuscript, I make it clear where more data is needed (data that would likely advance other topics in cloud physics), and describe how the new data can be incorporated into the model.

On the other hand, about giving more justification to the model assumptions, this is a reasonable suggestion and I provide details for particular cases below.

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b) About humidity effects being 'dismissed', they are not entirely dismissed. The cloud is assumed to be at liquid-water saturation, so the humidity varies with temperature. Of course, this means that temperature and humidity are not independent. However, I have taken the practical view; measured growth rates are available only for the case of liquid-water saturation, and air in mixed-phase clouds is mostly liquid saturated. Possible errors in using this data are now considered in the revised section 6.4.

c) About the growth being complex even without temperature variations, this is true and was in no way contradicted by the original manuscript. Though I had tried to convey this point in sections 4, 5, and Fig. 2, I now further emphasize this point: the reference crystals may be complex, while deviations only cause further changes to the reference crystals.

About references to support the explanation on the role of temperature in the caption, a reference was already given in the figure caption. This reference gives the relevant explanations, explanations based on many observations.

d) Finally, the discussion about the 'diffusion-limited effects' is confusing and doesn't seem to have any bearing on the study. There are two reasons why I say this. 1) It is well known that the snow-crystal primary habit changes abruptly between some temperatures (e.g., -8 and -10 °C), and neither the diffusion field nor the equilibrium vapor pressure change abruptly over this temperature range. If growth was essentially controlled by vapor diffusion (diffusion-controlled), as the referee suggests, the habit-change observation would be hard to explain. It is true that the mass uptake is largely predicted by vapor-diffusion theory, but only if crystal shape is known, and crystal shape is determined by surface processes. This appears to be true under all atmospheric conditions. 2) At any rate, much of this comment seems beside the point - I use measured growth data, not theory, whether or not the data is 'diffusion-limited' is immaterial.

Further remarks (numbering below is my own)

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1) Abstract. The abstract was rewritten and now does not contain the claim that the 'result is independent of the viewing resolution'. But, contrary to the referee's statement, the claim was justified in the original manuscript and explained in section 6.1. This part was not changed.

2) (4410-2.2) I rewrote this section such that the concept of a feature and resolution should be clearer. I agree, a mathematical definition of feature should be possible, but it is not necessary here. We only need to be able to estimate when a feature change can be distinguished during growth. This should be clearer in the revised text. About the viewing angle, the angle does not matter for the model. This is now clearly stated in the same section (2.2).

3) (4410-21) The treatment does not include basal growth, so it is not possible to include this effect. But I now address the question in the last paragraph of section 6.4. In particular, the growth-rate sensitivity of the basal face is much lower than that of the prism face. As the diversity is very sensitive to this parameter, the effect of neglecting basal-face growth should be small.

Note, however, that lateral growth of the prism faces does induce features that protrude partly in the basal-axis direction, and this is included in the present treatment. This is shown in Fig. 2a-c.

4) (4413-11) In the revised manuscript, I no longer make the assumption of independence (among the three sources of diversity). About the total diversity, I added some discussion of this in the new section 6.6.

5) (4414-18,19) I now explain these numbers and add further explanation to this section.

6) (4416-9) Though the referee did not mention the specific problem in this sentence, I realized that 'frequency' should be changed to 'number of times', and made this change.

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7) (4416 -5.2) I am not sure what the referee means by 'behave'. The distribution functions' large-p tails change when the data points are averaged over longer spacings, but this change is insignificant when the spacings are doubled or quadrupled, indicating that the existing F functions are reasonably accurate small-scale limits. As to whether or not the scale-dependence is important when the crystal is 'relatively stationary', the answer is no. Even when the crystal is stationary at constant z, the crystal is still falling through the updraft.

8) (4417-9) Although I don't know why the referee mentions 'space and time', I agree that I should consider how this symbol depends on the crystal size, shape, and growth rate. I think the symbol must depend on these things, and gave a reason. But it remains an unknown to be determined by later experiments. Is this problematic? No because once a realistic growth-history-averaged value is known by experiment, one can easily input the value into the theory to get the diversity - the theory remains useful as it is. This is mentioned at the end of section 6.3.

9) (4421 -6.5) I rewrote section 6.5, though I did not see any need to add references.

10) (4423 - eq 9) About the probabilities not being equal, I already considered this - it was written on line 20 of the original. I changed the wording to make this clearer.

11) (4425-11) I removed 'habit' and simply state the method of calculation

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4407, 2008.

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