**Geoscientific
Model Development
Discussions**

Interactive comment on "GAPPARD: a computationally efficient method of approximating gap-scale disturbance in vegetation models" *by* M. Scherstjanoi et al.

Anonymous Referee #1

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General

This is a quite extensive article about a new computational methodology that can be used together with gap-models to cover a high number of sites, particularly for screening a large area for sensitive regions that then can be explored in more detail. One of the reasons why it is so extensive is that at the same time new parameterizations of a couple of tree species for the LPJ-Guess model are presented and evaluated with Swiss forest inventory data. I would have preferred if the parameterization had been done separately, although this is not a mandatory request.

For the evaluation, the new method has been implemented into two gap models and

results are compared with runs not using the method. While it is a good proof of applicability, this further complicates the article. In fact, using the TreeM-LPJ model together with the GAPPARD methodology makes only limited sense (since a classical gap approach is not used), which is also apparent from the rather bad results (although I know that it is relative what 'good' or 'bad' means in a world of uncertainty and deficits). Therefore, it should be considered to skip this part of the model comparison (also, the description of the model refers to Scherstjanoi et al. 2013 which is only submitted yet).

Having this mentioned, the papers topic is highly interesting and probably relevant to a number of gap-modellers who are running their models, i.e. LPJ, at increasingly larger regions. So it is principally also in the scope of GMD. I also couldn't find major inconsistencies or errors and apart from a few sections (mentioned below), it is relatively well written and outlined. As in many such cases, I got the feeling that the results are interpreted over-enthusiastically and some more care should be payed to a number of statements (also in the abstract, which is otherwise okay). Overall I got the feeling that some work had been put into it already.

Specific

P1023, L5: add 'are' after 'surface' P1023, L24: there is a broad use of the term 'hybrid'. Being first use to combine yield models with community models (Dale et al. 1985), it has later on being mostly used for combinations of process-based and gap-type models (Friend et al. 1993) or (mostly) process-based and yield models (e.g. Battaglia et al. 1999). So please define what you mean or (better) don't use the term here (and elsewhere). P1025, L25: delete 'therefore' P1025, L2ff: paragraph could be shortened. I am also curious how mortality is treated in undisturbed runs. P1027, L6ff: The timestep of the model is one year, correct? So it doesn't matter at which season the disturbance occurs? P1028, L20: There is probably one 'distribution' too many. P1029, L6: delete the second 'starting'. P1030, L11ff: Reconsider wording at several places (e.g. 'bottom' instead of 'floor', input data for simulations but not 'stands' are derived from climate data, 'Modified' not 'changing' climate is applied). Some shortening is

also possible. P1032, L17: What does RID stand for? P1035, L14: Generally, the statements are a bit over-optimistic giving this statement (and looking at the figures) P1036, L7: 'pubescens' not 'pubescence' P1037, L10: 'The comparison ... shows' or similar P1038, L9: 'speed' instead of 'times'? P1039, L4: Does 'using existing parameters' mean that the same set of parameters is used for all pine species? Or do you mean that the available processes in LPJ are sufficient to represent the behaviour of p. mugo and p. cembra – I suspect the latter. Also, given the scope of the paper, I would avoid the term 'main achievement' here. P1040, L12ff: Here is a bit of a contradiction. If modelling fire is really important (as stated) you shouldn't be able to exclude it and still claim that the forest dynamics could be represented. P1040, L17-18: Very strange wording. P1040, L23ff: Does that mean you think spruces are currently suppressed by management? This seems very unlikely to me. P1041, L1: delete 'here' P1041, L25: Either the model underestimates drought events or the climate input has been wrongly scaled to the landscape. The underlying data are probably correct. P1043, L4: Why does the paragraph start with a summary? P1044, L8: avoid expressions such as 'acceptable' or provide an objective means of what is acceptable. P1044, L13: 'specific' instead 'certain' P1044, L20: what does this mean 'were treated as if they were'? Also the following sentence is quite unclear. P1047, L20: Equation A1: If md (the number of days with full leaf cover) is a species specific parameter and the function is only used for Larch, then you will either use the above or below equation and don't need both – correct? If md is somehow calculated please indicate how. P1050, L18: I don't get it. 'Leaves of all species are equally distributed vertically' – do you mean within a size class? Certainly different size classes have different access to light? P1060, Table 2: units of latitude and longitude? P1061, Table 3: Does that mean newly parameterized species perform particularly bad? Why's that? P1067ff, Table D4ff: Indicate 'site number' or similar above a-h. P1063, Fig. 4, 5, D1, D2: Difficult to read and I don't see the usefulness for the article here.

Noted references

Dale,V.H., Doyle,T.W., Shugart,H.H., 1985. A comparison of tree growth models. Ecol. Modell. 29, 145-169 Friend,A.D., Shugart,H.H., Running,S.W., 1993. A physiology-based gap model of forest dynamics. Ecology 74, 792-797 Battaglia,M., Sands,P.J., Candy,S.G., 1999. Hybrid growth model to predict height and volume growth in young Eucalyptus globulus plantations. For. Ecol. Manage. 120, 193-201