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Geoscientific Model Development Discussions

Interactive comment on "Implementation of a Marauding Insect Module (MIM, version 1.0) in the Integrated BIosphere Simulator (IBIS, version 2.6b4) Dynamic Vegetation–Land Surface Model" *by* J.-S. Landry et al.

Anonymous Referee #2

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GENERAL

Overall, the research is important and novel, and the manuscript is well written. The goal of the manuscript is to design and test an insect module that could be incorporated into Dynamic Vegetation Land Surface Models. This insect module, MIM, is designed to simulate the direct effects of defoliating insects and bark beetles (i.e. reduced biomass, mortality, and transfers of leaf litter), allowing the host DVLSM to calculate the indirect effects (i.e. reduced canopy conductance, changes in NPP, etc.). These procedures are an improvement from simply prescribing the indirect effects of insect activity with-

out first considering vegetation dynamics. The model also simulates the lag in effective tree death from insect activity (i.e. no photosynthesis or transpiration) and actual tree fall, which is a significant improvement in modeling the various forms of tree death. The study implements three major insect functional types (IFTs): broadleaf and needleleaf defoliators, and bark beetles, and their effects on broadleaved deciduous and needleleaf evergreen trees. This use of insect functional types is novel and will open up the possibility for the effects of insect activity to be modeled regionally and globally without extensive calibration. However, MIM requires user-defined input of percent defoliation (in the case of IFTs #1 and #2) and percent mortality (all IFTs) for each year of each grid cell, rather than calculating the probability for defoliation or mortality based on the vegetation, climate, or site characteristics. This lack of a process-based method for simulating insect activity is discussed briefly in the Discussion section, but could use some more justification in the Introduction/Methods. The authors present a case study using MIM and the Integrated BIosphere Simulator (IBIS) as the host DVLSM in three grid cells in British Columbia, Canada of a control and a 100% mortality event from a mountain pine beetle outbreak (bark beetle, IFT #3). They compare changes in NPP, NEP, litter, albedo, and snow amount between outbreak and non-outbreak conditions. They also compare IBIS-MIM results to field, satellite, and model studies on the effects of mountain pine beetle outbreaks. They found that in most cases, IBIS-MIM compared favorably to what was found in previous studies. It is possible that this work could have been improved with another case study using a defoliator insect (i.e. IFT #1 or #2), as the simulation of these IFTs is different from IFT #3.

SPECIFIC

Some more specific comments that may improve the manuscript:

(1) On page 10368, line 13 the authors state that DVLSMs contain "all required" land-atmosphere exchanges, in contrast to DGVMs. What exchanges does IBIS include that other DGVMs do not? Please explain this a bit further.

(2) On page 10368, line 21, introduce the mountain pine beetle as a bark beetle.

(3) At the top of page 10369, you explain how other studies on insect activity were lacking in various ways. I would suggest citing the studies that conducted each of the pitfalls you discuss.

(4) On page 10369, line 13, I would change "realistic" to something else as it is simply an even distribution of defoliation/mortality over the duration of insect activity. It is arguably better than having it all occur at the end of the year, but is still not "real."

(5) Page 10369, lines 22 through 25, the authors state that the host DVLSM is in charge of the "resulting consequences for vegetation coexistence…", however MIM requires user-input of % mortality. This seems to be contradictory.

(6) Could you please explain how IBIS simulates vegetation competition in your section on IBIS? You bring it up later in the manuscript so it may be good to explain it here.

(7) Page 10372 and Appendix B2: You state that the updates to the leaf-to-canopy scaling integral and the removal of the "extpar" simplification affect canopy transpiration. How and in what direction?

(8) Page 10373, line 1, delete "per se" as MIM does not model insect population dynamics nor does it include process-based methods of simulating the effects of them.

(9) Page 10374, line 15 "Note that for…": this sentence is confusing.

(10) Page 10375, line 10. Why do you kill defoliated trees "suddenly" at the end of the year rather than throughout the year? You spent a good deal of time in the Introduction discussing "sudden" deaths as unrealistic, could you provide a justification here for your decision to use it for IFTs #1 and #2?

(11) Page 10375, line 20. Change "meanwhile" to "currently"

(12) Page 10375, lines 23 and 24: Could you provide a justification for not quantifying the stem C consumed by IFT #3 and IFT biomass, and whether/how this may affect

your results?

(13) I am in general confused about how your model calculates % defoliation and % reflush? Is the % reflush for a day calculated based on the % of total leaf biomass lost up to that point, or something else? And does defoliation and reflush occur concurrently on a single day, and if so, which comes first in your simulation? Does the forest lose leaves to insects and then grow some back in the same day?

(14) Your snagfall dynamics for Case #5 seem incorrect. It is my understanding that lodgepole pine trees infested with MPB retain their leaves a year or more, and then they gradually fall over 4-7 years (Hansen 2013, Forest Science 60(3); Klutsch et al. 2009, Forest Ecology and Management 258). In Table 2 it shows there being no delay in litterfall for Case #5. Could you justify or explain this difference?

(15) Could you justify your decision to prescribe a single, 100% mortality event occurring in one year? It does not seem realistic for 1) an entire lodgepole pine stand to be killed by MPB, especially the small stems, which are rarely infested by bark beetles (Pfieffer et al. 2010, Global Change Biology 17; Veblen et al. 1994, Journal of Ecology 82(1)), and 2) that if this did occur, it would occur all in one year.

(16) Page 10379, line 19. It seems more likely that the MPB outbreak delayed the decline in lower canopy NPP rather than prevented it. As the decline occurred 600 to 750 years into the control/non-outbreak simulation, it may occur between years 1000 and 1150 of your outbreak simulation, which you do not show. Because the NE PFT retained its pre-outbreak levels of NPP by the end of your simulation, it seems that this may result in a decline in lower canopy NPP.

(17) Page 10379, line 26: Based on Figure 2, it seems that total NPP for the southern grid cell only declined initially (i.e. before year 100). At year 1000 it looks like the change in NPP between the outbreak and control is 0 or very close to it.

(18) I found the consequences of the standing dead trees very interesting. Your results

show that it is important to include these dynamics in land surface models.

(19) Page 10382, line 14. Delete the phrase about the increase in shrub biomass being "akin to 'not statistically significant' " as you do not include statistical tests in your study.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10365, 2015.