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Geoscientific Model Development Discussions

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

Anonymous Referee #1

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GENERAL

Overall, the research done is substantial and the manuscript is well written. The manuscript can be clearly divided into two major parts: (1) a methodological part, describing the implementation of a new insect module (MIM) into an existing land surface model (IBIS); and (2) a simple application of a part of the new IBIS-MIM, illustrating the simulation of a stand-replacing bark beetle disturbance and its impacts on ecosystem cycles at three locations in western Canada. Implementing insect disturbances into large-scale ecosystem models, such as IBIS, is challenging when going beyond a

single species or region. The concept of insect functional types (IFTs) is applied for the first time in this study, allowing the assessment of different insect disturbances at different regions within a single framework. This step means a significant contribution towards an appropriate representation of insect disturbances in ecosystem models in general. Moreover, the study provides incentives for coupling MIM with other models than IBIS, and for the future implementation of additional IFTs. The only aspect a reader will probably miss is a more comprehensive application of the implemented features. For instance, defoliator-induced damage, different levels of bark beetle-induced mortality and large-scale, quantitative effects on ecosystems (all of which can potentially be done with IBIS-MIM) have not been simulated in the context of this study.

SPECIFIC

In the following, some specific comments (made loosely in the order of reading) may help the authors to further improve their manuscript on certain aspects:

(1) The authors apply the concept of IFTs but they miss to give a short introduction on it (e.g., was it applied previously, and why using such types instead of species or a generic approach?). Surely the modeling community may already know the concept from plant functional types (PFTs), however, it would be useful also for a broader audience to add a short phrase to the introduction (probably useful references: Dietze & Matthes, Ecology Letters 2014, 17: 1418–1426, or Cooke et al., 2007, chapter 15 in: Plant Disturbance Ecology, eds.: Johnson & Miyanishi, p. 489).

(2) Is there a reason why you didn't use the most current version of soil data (version 3.2 instead of 2.1/2.2 (page 10371, line 24)?

(3) The required input for defoliation IFTs need to be clarified, as it is not obvious from reading. What does e.g. "5% defoliation" mean: 5% trees from a grid with 100% defoliation each, or 100% trees with 5% defoliation each? Please indicate whether or not MIM can simulate partial defoliation.

(4) In additional to the prescribed defoliation damage MIM also requires prescribed defoliator-induced mortality as input, instead of simulating emergent mortality as a result of (repeated) defoliation. You may explain why emergent mortality was not simulated, or add a phrase on that issue to the discussion of the shortcomings of MIM.

(5) Defoliator-induced mortality typically doesn′t occur after reflush, i.e., a tree dies as a result of losing ability to reflush due to a lack of carbon resources (e.g., Cooke et al., 2007 see above). MIM doesn′t consider any interaction between (repeated) defoliation and mortality, and thus in MIM a tree can reflush and die in the same season (which is not realistic).

(6) MIM uses fixed parameters for IFTs, e.g. start_IFT (Table 1). Since your IFT #3 (MPB) cover a large geographical range from northern BC to south-western US, how would within-species variation in parameters be attributed in MIM, by using a separate IFT for the same species? This information would be particularly relevant when applying MIM to other regions than Canada.

(7) While for defoliation the time of attack is equal to the time of visible damage, there is a delay of one year from MPB attack until the damage is visible (red stage). Since MIM uses prescribed mortality data, I assume this refers to visibility / detection of damage, not to the time of attack. This point should be clarified within the paragraph where you described daily mortality for bark beetles (e.g., page 10374). Furthermore, MIM uses three years as leaf-falling rate for case #5 (Table 2), but there are two years indicated in the literature (Wulder et al., Forest Ecology and Management 2006, 221: 27–41); maybe this difference is a result of the one-year lag.

(8) MIM defines a reflush parameter for defoliated broadleaf deciduous trees, which corresponds to 50% of the lost leaf biomass (reference for 100% defoliation in oak). In contrast to that generic assumption based on a single-species case study, the ability to an immediate reflush is a function of the portion of defoliation (% defoliated, typically no reflush occurs when the portion is <50%) and the vitality of the tree (number of repeated

defoliation events). I would suggest to mention this simplification when describing the approach (p. 10376) or in the discussion.

(9) The implementation of snag dynamics is well done in MIM, yet it is not discussed sufficiently in terms of how the MIM approach differs from / or is based on previous approaches. A short phrase could be added to the discussion (e.g., 10386, 6) (beside Edburg et al., Journal of Geophysical Research 2011, 116 you have already cited, also other models consider snag dynamics, such as FireBGCv2 and FVS among others).

(10) You haven′t provided field results for GPP, Ra, Rh, and NEP (Table 4), but I presume (without doing a comprehensive review of the related literature) there are some field studies existent. In order to further complete the table, could you please check if you can fill these open fields? For instance, Moore et al., Ecology Letters 2013, 16: 731–737, or Harmon et al., Journal of Geophysical Research 2011, 116 could probably be useful as references.

TECHNICAL

In addition, some minor, more technical issues:

(11) Although the authors have already changed the title according to the editors suggestion, I still think that “version 1.0” can be skipped from the title, so that it reads more fluently without too much specifications (but that might be rather a matter of the authors taste)

(12) replace “. . .damage from broadleaf defoliators, needleleaf defoliators, and bark beetles. . .” with “. . .damage from three different insect functional types: (1) defoliators on broadleaf deciduous trees, (2) defoliators on needleleaf evergreen trees, and (3) bark beetles on needleleaf evergreen trees. . .” (10367, 8-9) in order to better emphasize the three IFTs, and to clarify that you don′t include bark beetles on broadleaf trees

(13) add to the abstract that the application focuses only on one of the implemented IFTs, and that only a simplistic setting is used, i.e. 100% mortality in three grid cells in

western Canada

(14) introduce MPB as a bark beetle species (e.g., 10367, 17 or 10368, 21)

(15) use the keyword “forest disturbance” somewhere in your abstract/introduction, since it is not said at all until the section 2.2 that your model is about forest

(16) skip “realistic” or replace it by e.g. “approximated”, since an equal distribution of damage to the entire attack period is rather approximated than realistic (as you said in 10374, 20)

(17) replace “readily implemented”, it could give the wrong impression that MIM can just be taken and used with other models without any adaption (10369, 26)

(18) the sentence in parenthesis on 10374, 15-16 is not clear to me, could you rephrase it, or skip it if not really needed?

(19) use spelling “reflush” (instead of “re-flush”) consistently

(20) add the time period when NPP reduction occurs ($\sim$80 yr) after “was reduced. . .” in 10379, 26, since NPP is balanced out after a certain period

(21) replace “generally” with e.g. “slightly”, since the difference is obviously very small (10384, 22)

(22) the long list of references is not necessary to be repeated in the conclusions, most of them are mentioned before; I suggest to skip the references from here (10387, 14-17)

(23) replace “over 30” with “37” to be more precise (10388, 13)

(24) rephrase (or skip) the sentence in 10388, 15-16, since to my understanding the good agreement shown in Table 4 doesn′t actually support “the idea that DVLSMs are valuable tools. . .”, yet it rather supports the use of MIM as valuable tool (but that is then said in the following paragraph)

(25) check publication year of Landry & Parrott, probably it will be 2016 and not 2015 (10397, 9 and citation in the text)

(26) Figure 3: line plots (similar to Fig. 2) would probably be the better choice for (a)-(c) with regards to readability; in (d) you don′t compare the three grid cells, aren′t there any differences? Though being a minor issue, a consistent logic among all panels (a)-(d) (i.e., comparison of grid cells, using the same plot type and colors) may improve readability.

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