Interactive comment from Astrid Kerkweg (Executive editor): please note that the interactive comment is in italics, with our response given in a regular font.

Dear authors,

In my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.1:

http://www.geosci-model-dev.net/8/3487/2015/gmd-8-3487-2015.html

This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section:

http://www.geoscientific-model-development.net/submission/manuscript_types.html

In particular, please note that for your paper, the following requirement has not been met in the Discussions paper:

> *• Inclusion of Code and/or data availability sections is mandatory for all papers and should be located at the end of the article, after the conclusions, and before any appendices or acknowledgments. For more details refer to the code and data policy.*

Please move the Code Availability section to the correct place in your revised submission to GMD.

Yours,

Astrid Kerkweg

>> E.1 We thank you for helping us better meet the editorial requirements of GMD. We moved the "Code availability" section before (instead of after) the appendices. <<

Comments from Reviewer #1: please note that the review of our manuscript is in italics, with our responses given in a regular font.

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.

>> 1.1 We thank you for this positive assessment of our manuscript, as well as for the suggestions you provided below on how to improve it. Please note that we plan to submit other manuscripts based on a "*more comprehensive application of the implemented features*", as the current study was indeed focussed on presenting IBIS-MIM and a simple application of this new model. This decision was based on the realization that it was too much for one paper to cover all the material in sufficient depth. <<

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).

>> 1.2 We thank you for this suggestion. We added the following sentence right after mentioning insect functional types (IFTs) for the first time (p10369, l14). "The concept of IFTs allows simplification of the huge diversity of insect species by grouping species that cause similar impacts (Cooke et al., 2007; Arneth and Niinemets, 2010), and has recently been applied under the name of "Pathogen and Insect Pathways" in a simple ecophysiological model (Dietze and Matthes, 2014)." We also added this last reference to another sentence (p10387, l27; modifications in italics and strikethrough): "The parameterization of IFTs was based on key outbreaking insects affecting North American forests, but could be modified to represent other insect species*, effects on other vegetation types (e.g., agricultural*

fields) and ~~probably~~, with ~~additional~~*further* adjustments, *effects of* some vegetation pathogens *(e.g., Dietze and Matthes, 2014).*" Note that we also addressed another comment in the previous sentence (please see our response 1.16). <<

(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)?

>> 1.3 Based on our understanding, modifications since version 2.2 were for agricultural soils: "The latest complete coverage of Canada (i.e. including areas outside the agricultural regions of the country) is v2.2." (http://sis.agr.gc.ca/cansis/nsdb/slc/v3.2/index.html). <<

(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.

>> 1.4 This is an insightful question and the answer depends upon the capacity of the host DVLSM. We clarified this by adding the following text after the "5% defoliation" example (p10373, l6). "For host DVLSMs that, like IBIS, do not represent many individuals for the same PFT, a 5% defoliation event translates into 100% of the trees losing 5% of their leaf area; in other DVLSMs, this same 5% defoliation event could be assigned differently, for example by removing 100% of the leaf area from 5% of the trees." <<

(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.

>> 1.5 We agree with the spirit of this comment; however, we consider that this shortcoming is a limitation of IBIS (which does not simulate mortality explicitly) rather than of MIM per se. We therefore added the following sentence (p10373, l11): "In fact, for DVLSMs that, unlike IBIS, simulate PFT mortality explicitly (e.g., as a function of carbohydrate reserves), MIM would not need input data on prescribed mortality in the case of defoliators". <<

(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).

>> 1.6 We thank you for pointing this out. We added the following text at the end of the paragraph on reflush (p10376, l9), which also addresses another comment below (please see our response 1.9): "The value of total$_{reflush}$ for the year can be set to zero to prevent unrealistic reflush when the defoliation level is very low, or when trees have already been weakened by previous defoliation events, or if mortality is also prescribed for the same year." <<

(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.

>> 1.7 We think that the most straightforward way to account for within-species variation would be through the use of spatially-explicit data on parameters (similar to what is being done for soil texture). We added the following sentence at the end of the section presenting MIM (p10377, l22; DST refers to dead standing trees): "Moreover, for large-scale studies, the IFT- and DST-related parameters could vary spatially to reflect within-species variation, instead of having uniform values as we have used here (e.g., needlefall for Case #5 could occur over more than three years)." The last part of the sentence addresses a comment from Reviewer #2 (please see our response 2.15). <<

(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.

>> 1.8 We clarified that MIM's delay parameter refers to the time of attack in text describing the fate of all DST pools (p10377, l10; new text in italics): "The transfer of carbon from DST pools (i.e., fine roots, leaves/needles, and stems, the latter including coarse roots and branches) towards IBIS litter pools starts after a delay period and then occurs at a specific rate*; note that the delay refers to the time of attack, even for Case #5.*" Fig. 2 of Wulder et al. (2006) suggests that all trees are needleless (i.e., gray stage) ~31 months after the attack; in MIM, this actually happens ~40 months after the attack (e.g., on December 31, 2004 for an attack happening in 2001, which would last from August 1st to September 19th in MIM; see our Table 1). This small difference is not an issue, given the variability in the time needed to reach the gray stage (Fig. 3 of Wulder et al., 2006) and that longer periods have been reported by other authors (e.g., Simard et al., 2011). <<

(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.

>> 1.9 We thank you for these suggestions to improve precision. We added the following text at the end of the paragraph on reflush (p10376, l9), which also addresses another comment below (please see our response 1.6): "The value of total$_{reflush}$ for the year can be set to zero to prevent unrealistic reflush when the defoliation level is very low, or when trees have already been weakened by previous defoliation events, or if mortality is also prescribed for the same year." <<

(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).

>> 1.10 We were not aware of snag dynamics in FireBGCv2 or FVS, and directly based the dynamics of DSTs in MIM on empirical data from Canadian forests (please see the references provided for each of the five Cases on p10376-10377). We added a reference to these two models where we mention possible improvements to MIM (p10386, l11; new text in italics): "Moreover, MIM could be *modified by simulating the fall of DSTs probabilistically (e.g., as in FireBGCv2; Keane et al., 2011)* or enhanced by: simulating the fall of DSTs as a function of environmental conditions (Lewis and Hartley, 2005) *or the size of DSTs (e.g., as in FVS; Rebain et al., 2010)*; reducing snow albedo when needles fall from DSTs (Pugh and Small, 2012); and accounting for changes in needle optical properties as they turn from green to red (Wulder et al., 2006)." <<

(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.

>> 1.11 As mentioned in the manuscript (p10381, l13-14), we wanted to include only studies having actual control and effect results for the comparisons presented in Table 4 (the only exception being some studies listed in the Mikkelson et al. (2013) review, which is a highly relevant study in this context). The NPP, Rh, and NEP results for bark beetles presented in Fig. 12 of Harmon et al. (2011) do not come from empirical data, but are rather "[h]ypothetical examples of temporal patterns" (caption of their Fig. 12); hence, we decided to not include this study in Table 4. Moore et al. (2013) is the only study of which we are aware that could have been included in Table 4 and was not. The reason we initially excluded this study was that, to be quite frank, we had a hard time fully understanding how they actually derived all their results. Following your comment, we carefully re-read the study and included their GPP results (i.e., a decrease) in Table 4. However, as far as we understand, the GPP results in Fig. 1 of Moore et al. (2013) came from satellite measurements calibrated with field-level eddy covariance data ("locally calibrated satellite estimates of gross primary production (GPP)"; their Introduction), so we added this study under the "satellite" column in Table 4. We did not add to Table 4 the results of Moore et al. (2013) on total ecosystem respiration or soil efflux, because control values were not provided after the disturbance event. <<

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)

>> 1.12 We have no opposition in removing "version 1.0" if the Topical editor instructs us to do so; the current title is the one he suggested when we requested guidance on how to meet the editorial policy of Geoscientific Model Development. <<

(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

>> 1.13 We thank you for this suggestion, which we brought as is to the text. <<

(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

>> 1.14 We added these elements to the text and also addressed your next comment (p10367, l14; modifications in italics and strikethrough): "After describing IBIS-MIM, we illustrate the usefulness of the model by presenting results spanning daily to centennial timescales for vegetation dynamics and cycling of carbon, energy, and water *in a simplified setting and for bark beetles only. More precisely, we simulated 100% mortality events from* ~~following a simulated outbreak of~~ the mountain pine beetle *for three locations in western Canada*". <<

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

>> 1.15 Please see our response 1.14. <<

(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

>> 1.16 We agree that the IFT currently implemented in MIM are based on forest insect species only. However, we believe that the generic design of MIM would allow for the addition of agricultural insect species in appropriate terrestrial models (like Agro-IBIS, which is very similar to IBIS). We therefore decided to avoid deterring readers interested in models of insect impacts on agricultural systems. To clarify this point, we modified the following sentence (as mentioned in our response 1.2): "The parameterization of IFTs was based on key outbreaking insects affecting North American forests, but could be modified to represent other insect species*, effects on other vegetation types (e.g., agricultural fields)* and ~~probably~~, with ~~additional~~*further* adjustments, *effects of* some vegetation pathogens *(e.g., Dietze and Matthes, 2014)*." <<

(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)

>> 1.17 On p10369, l12, we replaced "realistic" with "approximated". On p10373, l12, we deleted "realistically". <<

(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)

>> 1.18 Thank you for your scrutiny; we removed "readily". <<

(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?

>> 1.19 We decided to entirely rephrase the sentence, as it answers a question that some readers might ask themselves: "The duration of leaf onset simulated by IBIS is much shorter than duration$_{IFT}$ for IFT #1, so there is no risk that this defoliator of deciduous trees will consume leaves faster than their simulated onset". <<

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

>> 1.20 We are sorry for this lack of consistency; we used "reflush" throughout the text. <<

(20) add the time period when NPP reduction occurs (~80 yr) after "was reduced..." in 10379, 26, since NPP is balanced out after a certain period

>> 1.21 We thank you for suggesting this improvement. The text now reads (modifications in italics and strikethrough): "In the southern grid cell, on the other hand, total NPP was reduced *for about 75 years and then increased marginally for a few decades, before returning to the control level* ~~except for a marginal increase ~ 100 years after the outbreak~~ (Fig. 2f)." <<

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

>> 1.22 We used the word "generally" to mean that this is the case most of the time, but we agree that the difference is very small. We therefore reworded to: "Although often slightly higher at the beginning of the season [...]". <<

(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)

>> 1.23 We removed the references. <<

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

>> 1.24 Done. <<

(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)

>> 1.25 This is to some extent a matter of interpretation, but we consider that the results shown in Table 4 depend more upon the responses simulated by the IBIS DVLSM than upon the changes prescribed by MIM (only one of the 28 response variables shown in Table 4 was changed directly by MIM, all the other variables being estimated by IBIS based on the new state of the vegetation). The major role of the host DVLSM is also supported by the following outcome: although the change prescribed by MIM was exactly the same in all cases, the IBIS-simulated changes differed qualitatively (i.e., increase versus decrease) across the three grid cells for some variables. <<

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

>> 1.26 Yes, we will update this reference as appropriate (we now have a DOI, but the study is still in press). <<

(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.

>> 1.27 We thank you for the suggestions. For panels (a)-(c), line plots make the data more difficult to see as the different lines often cross each other (particularly for the NEP changes in the northern and central grid cells during the first decades after mortality). Panel (d) indeed differs from the other three panels, not only because it presents the results for a single grid cell, but also because it shows absolute values for the outbreak and control simulations (instead of the difference between the two). Although this breaks the logic across the four panels, we thought it was preferable to show absolute values in this case because we wanted to show that IBIS does a good job at simulating snow accumulation at the daily timescale (such results from DVLSM are not frequently reported). Showing all grid cells would then have required six lines in panel (d), making it less readable. The results in the other grid cells differ quantitatively, but show the same general behaviour: an earlier and faster simulated snowmelt in the outbreaks simulations. <<

References (Reviewer #1)

Arneth and Niinemets (2010). Trends in Plant Science 15, 118-125

Cooke et al. (2007). Chapter 15 in Plant Disturbance Ecology, 487-525

Dietze and Matthes (2014). Ecology Letters 17, 1418-1426

Harmon et al. (2011). Journal of Geophysical Research 116, G00K04

Keane et al. (2011). The FireBGCv2 Landscape Fire Succession Model: A Research Simulation Platform for Exploring Fire and Vegetation Dynamics. USDA General Technical Report RMRS-GTR-255

Mikkelson et al. (2013). Biogeochemistry 115, 1-21

Moore et al. (2013). Ecology Letters 16, 731-737

Rebain et al. (2010). The Fire and Fuels Extension to the Forest Vegetation Simulator: Updated Model Documentation. USDA Internal Report, Fort Collins, CO (revised: June 2015)

Simard et al. (2011). Ecological Monographs 81, 3-24

Wulder et al. (2006). Forest Ecology and Management 221, 27-41

Comments from Reviewer #2: please note that the review of our manuscript is in italics, with our responses given in a regular font.

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 without 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.

>> 2.1 We thank you for your positive review and the comments you provided to help us improve the manuscript. Regarding the "*lack of process-based method for simulating insect activity*", we added the following two elements to the text. First, we added the following sentence (p10369, l25): "Prescribing insect activity is less sophisticated than its prognostic simulation, but nevertheless allows relevant questions to be addressed concerning the climatic and ecological impacts of insect-caused plant damage". Second, we added the following sentence (p10373, l11), which also addresses a comment from Reviewer #1 (please see our response 1.5): "In fact, for DVLSMs that, unlike IBIS, simulate PFT mortality explicitly (e.g., as a function of carbohydrate reserves), MIM would not need input data on prescribed mortality in the case of defoliators" (where PFT stands for plant functional type). We decided to avoid adding simulation results for IFT #1 and #2 to limit the length of the manuscript, choosing instead to provide an in-depth analysis of results for the mountain pine beetle (IFT #3), for which many empirical results exist on a suite of impacts. <<

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.

>> 2.2 We thank you for raising this point. The term "DGVM" is now being used for a wide range of models in terms of the processes included and output generated. Among others, what is often missing (from a climatic point of view) in models labelled as "DGVM" are the land-to-atmosphere fluxes of shortwave and longwave radiation. We modified the sentence starting on p10368, l10 as follows (modifications in italics): "Since the term "DGVM" is often used for interactive vegetation models that estimate only some of the exchanges of carbon, energy, water, and momentum with the atmosphere (Prentice et al., 2007; Quillet et al., 2010), we will refer here to the subset of DGVMs that compute all required land–atmosphere exchanges while accounting for dynamic vegetation as Dynamic Vegetation–Land Surface Models (DVLSMs) to prevent possible confusion *(e.g., many DGVMs do not compute the land-to-atmosphere fluxes of shortwave and longwave radiation)*." <<

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

>> 2.3 We thank you for this suggestion. In response to a similar suggestion and another comment from Reviewer #1 (please see our responses 1.14 and 1.15), we modified the Abstract as follows (p10367, l14; modifications in italics and strikethrough): "After describing IBIS-MIM, we illustrate the usefulness of the model by presenting results spanning daily to centennial timescales for vegetation dynamics and cycling of carbon, energy, and water *in a simplified setting and for bark beetles only. More precisely, we simulated 100% mortality events from* ~~following a simulated outbreak of~~ the mountain pine beetle *for three locations in western Canada*". <<

(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.

>> 2.4 We agree this would provide some value; however, we prefer to avoid citing the studies because this might be not well received by some authors of these studies. In addition, despite their limitations, most of these studies have made other positive contributions to the field. <<

(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."

>> 2.5 We replaced "realistic" with "approximated". We also deleted "realistically" on p10373, l12, which was used with a similar meaning. <<

(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.

>> 2.6 We thank you for raising this apparent contradiction. What we meant is that the host DVLSM simulates the post-mortality competition among the different PFTs present in the grid cell (instead of

MIM also prescribing the resulting impact on vegetation composition). Consequently, we modified the sentence as follows (modifications in italics and strikethrough): "The underlying philosophy of MIM is to prescribe only the direct damage to the vegetation caused by insect activity, letting the host DVLSM quantify the resulting consequences for *the post-mortality competition among the different vegetation types* ~~vegetation coexistence~~ and the exchanges of carbon, energy, water, and momentum, based on the new conditions in the grid cells affected." Please also note that MIM would not actually need to prescribe defoliator-caused mortality for DVLSMs that simulate PFT mortality explicitly (see our response 2.1). <<

(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.

>> 2.7 We thank you for suggesting this relevant addition. We included the following explanation (p10370, l25): "Competition among PFTs accounts for the two-strata structure of vegetation (i.e., trees capture light first, but grasses have preferential access to water as they have a higher proportion of their roots in the upper soil layers) and is based on the annual carbon balance of each PFT." <<

(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?

>> 2.8 We brought these modifications to IBIS before coupling it to MIM in order to improve carbon cycling in IBIS, and reported these changes here as this is the first manuscript we submit with this modified version of IBIS. Please note that we did not explicitly assess the effects of these changes on transpiration; however, these changes are directly related to the changes in net primary productivity (NPP) that we did assess. We performed various simulations for three transects: one in the boreal forest of North America, one in a drier region of North America, and one in the Amazon forest. In a nutshell, we found that the direct effect of these changes was to generally reduce NPP, with this reduction becoming greater as the level of CO_2 increased. Consequently, we concluded that "these changes reduced the strength of CO_2 fertilization in IBIS" (p10372, l5); please note that CO_2 fertilization in version 2 of IBIS was previously reported to be substantially higher than in other models (Cramer et al., 2001; McGuire et al., 2001; Friedlingstein et al., 2006). Our assessment also showed that the indirect consequences of these changes (e.g., changes in vegetation composition resulting from the changes in NPP, which differed among PFTs) could be substantial, with cascading impacts on carbon cycling and others. Although interesting, we believe that these elements are much too detailed to be discussed in the current manuscript and consider it more advisable to only report the main outcome mentioned previously (i.e., the direct effect is to reduce the strength of CO_2 fertilization). <<

(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.

>> 2.9 Done. <<

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

>> 2.10 We are sorry that this sentence was not sufficiently clear. We entirely rephrased it as: "The duration of leaf onset simulated by IBIS is much shorter than duration$_{IFT}$ for IFT #1, so there is no risk that this defoliator of deciduous trees will consume leaves faster than their simulated onset". <<

(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?

>> 2.11 We thank you for raising this point. We mentioned in the Introduction that "previous studies lacked realism by representing insect damage as end-of-year instantaneous events (instead of simulating their unfolding over many weeks during the growing season)" (p10369, l1). There is no major contradiction between this statement and the way we represented defoliators (i.e., IFTs #1 and #2) in MIM, because in the case of defoliators the unfolding damage during the growing season consists primarily of defoliation itself. Please also note that 100% defoliated trees (if no reflush is allowed) actually behave as dead standing trees (DSTs), because they can no longer perform photosynthesis or transpire. We clarified this point by adding the following sentence (p10375, l13): "Note that PFTs entirely defoliated by IFTs #1 or #2 behave exactly as dead trees if no reflush is allowed (see below), even if these killed PFTs are not labelled as "dead" before the end of the year." <<

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

>> 2.12 Done. <<

(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?

>> 2.13 The main reason was that we did not find the required data on the typical amount of stem C consumed by the mountain pine beetle (MPB) and on the partitioning of this consumed carbon among respiration, frass, and biomass. This affects our results by changing the CO_2 fluxes resulting from the activity of IFT #3, because: 1) we neglect the instantaneous release of CO_2 caused by respiration from IFT #3; 2) we do not account for the export of IFT #3 biomass out of the killed trees (which would reduce the amount of stem biomass available for decomposition later on); and 3) we do not account for the possibly different (likely higher?) rate of decomposition for IFT #3 frass versus tree litter. (Note that this third limitation also applies to frass from IFT #1 and #2, as it is treated as litterfall from leaf or needle by IBIS; p10375, l18). Nonetheless, the quantitative impact of these elements on our results is very small and much lower than other sources of uncertainty, because the total biomass of MPB required to kill a tree is orders of magnitude lower than the biomass of this tree. For example, it takes less than 600 individuals of the MPB to kill a tree (Jackson et al., 2008), with the efficiency of the attack strongly saturating when the attack density is ~3 times higher (Raffa and Berryman, 1983); a value of 1800 individuals is therefore an overestimate for a typical successful attack. Combined with a dry biomass of about 3.5 mg per individual (Reid and Purcell, 2011; Graf et al., 2012), this gives a total biomass (dry weight) of 6.3 g for MPB successfully attacking a tree. At such high density, a productivity of 10 pupae per MPB is optimistic (Raffa and Berryman, 1983); assuming that all pupae survive to emerge as adults (optimistic once again), this gives an upper bound of 63 g for the total biomass exported out of a killed tree, leading to a net export of 56.7 g (63 g minus 6.3 g). Now, the

average volume of a tree killed by MPB is around 0.57 m3 (Koot and Hodge, 1992; Jackson et al., 2008); using data for lodgepole pine in Canada, this means a dry biomass for stem (with bark and branches) of about 240 kg (CFS, 2015), i.e., more than 4,000 times higher than the total MPB biomass exported (for an assessment that overestimated on purpose the typical value of this export). Consequently, we consider that this simplification is not consequential in the context of our study and added the following precision to the text (modifications in italics): "At present, MIM does not quantify the stem carbon consumed by IFT #3 and the resulting IFT biomass; *given the difference between the total biomass of bark beetles and the biomass of the trees they killed, this should have very small impacts on the simulated carbon fluxes.*" <<

(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?

>> 2.14 We are sorry about this confusion: Table 1 implicitly provided the response, but we should have included it in the text. Reflush, which can happen following defoliation by IFT #1 only, necessarily starts much after defoliation is completed (i.e., defoliation ends 35 days after leaf onset, whereas reflush starts 56 days after leaf onset); hence, reflush is indeed calculated based on the total amount of leaf biomass lost to defoliation. We therefore modified the text as follows (p10376, l3; modifications in italics and strikethrough): "[...] where total$_{reflush}$ is the total amount of leaf *reflush* ~~re-flush~~ (in % of the *total* leaf biomass lost to defoliation *earlier in the same year*), duration$_{reflush}$ is the duration of the *reflush* ~~re-flush~~ (in days), and start$_{reflush}$ is the specific day of the year when *reflush* ~~re-flush~~ starts (see Table 1*; please note that reflush starts after defoliation is completed*)." <<

(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?

>> 2.15 This discrepancy appears to reflect spatial differences in needlefall following death, as the 4-7 years duration mentioned in the review of Hansen (2014) and the primary research article of Klutsch et al. (2009) are for Colorado, whereas our main reference on this element (Wulder et al., 2006) is for British Columbia, Canada. This last reference actually reported that trees reach the gray stage (i.e., are needleless) ~31 months after the attack, whereas in MIM this happens ~40 months after the attack (e.g., on December 31, 2004 for an attack happening in 2001, which would last from August 1st to September 19th in MIM; see our Table 1). We note that other studies in Wyoming reported that trees were in the gray stage as soon as 3 or 4 years after the outbreak (Griffin et al., 2011; Simard et al., 2011). Regarding the absence of delay in litterfall following mortality, we also note that while Klutsch et al. (2009) did not find statistically significant differences in *total* litter depth between undisturbed stands and stands infested 0-3 years before, Griffin et al. (2011) reported statistically significant differences in total litter depth between undisturbed stands and stands disturbed 2 years before, while Simard et al. (2011) reported statistically significant differences in *needle* litter depth (more relevant than total litter depth) between undisturbed stands and stands disturbed 1-2 years before. Unfortunately, we are not aware of actual measurements of changes in needlefall per se, or at least of changes in needle litter mass (which would be more appropriate than litter depth to parameterize the rate of

needlefall). To address this issue of spatial variability, we added the following sentence at the end of the section presenting MIM (p10377, l22): "Moreover, for large-scale studies, the IFT- and DST-related parameters could vary spatially to reflect within-species variation, instead of having uniform values as we have used here (e.g., needlefall for Case #5 could occur over more than three years)." <<

(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.

>> 2.16 We agree that it is not typical for 100% of pines to be killed by MPB, although this has been reported to happen (Hawkins et al., 2013). Please also note that IBIS, like many DVLSMs, does not have an intrinsic horizontal resolution (i.e., the model specifies the size of the vertical dimension only, for example, the height of trees); hence, IBIS-MIM results do not necessarily correspond to an entire stand, but could apply to a smaller 'patch' containing trees all killed in the same year. Nonetheless, we agree that prescribing a single, 100% mortality event was a simplification; we did it to increase the signal-to-noise ratio of our results, avoid having to consider the specific effect of more complex outbreak patterns, and test the theoretical upper limit to what could occur. In the context of our qualitative comparison of IBIS-MIM results with previous studies, we believe that this simplification was not misleading. For example, a previous study on MPB using a model similar to IBIS found that, for the same level of total mortality, the duration of the outbreak had a noticeable effect on net ecosystem productivity in the short term only (Edburg et al., 2011). We thus added the following sentence to justify this simplification (p10378, l18): "This single, 100% mortality event does not aim to represent actual MPB outbreaks, but was implemented for the sake of simplicity, to increase the signal-to-noise ratio of the results, and to test the theoretical upper limit of impacts." <<

(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.

>> 2.17 We thank you for this insightful observation. Indeed, we have looked at results for the central grid cell over a longer time period and have found a decline in lower canopy NPP around year 1100 (followed by a recovery afterwards). These results, as well as the replicate simulations we performed under different weather conditions (p10385, l1), all suggest that decreases in lower canopy NPP (possibly followed by recoveries) are 'bound to happen' in the climate of the central grid cell. Consequently, we modified the text as follows (modifications in italics and strikethrough): "whereas the MPB outbreak released the lower canopy and *postponed* ~~prevented~~ this decline, *which seems 'bound to happen' in the long term*". <<

(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.

>> 2.18 We thank you for noticing this lack of precision in our previous explanation. We consequently improved the text, which now reads (modifications in italics and strikethrough): "In the southern grid cell, on the other hand, total NPP was reduced *for about 75 years and then increased marginally for a few decades, before returning to the control level* ~~except for a marginal increase ~ 100 years after the outbreak~~ (Fig. 2f)." <<

(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.

>> 2.19 We thank you for sharing this positive thought. We also believe that representing the physical presence of DSTs will likely lead to interesting insights about various land-atmosphere exchanges. <<

(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.

>> 2.20 We deleted this part of the sentence, which now reads (modifications in italics and strikethrough): "Note that the simulated increase in shrub biomass was *marginal* ~~very small~~ in the three grid cells, ~~akin to "not statistically significant" results in empirical studies,~~ but that grass biomass increased substantially." <<

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