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Discussions

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Interactive comment on "Carbon sequestration in managed temperate coniferous forests under climate change" *by* C. C. Dymond et al.

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Anonymous Referee #1 Received and published: 3 February 2016 Title: Carbon Sequestration in Managed Temperate Coniferous Forests under Climate Change Author(s): C.C. Dymond et al. MS No.: bg-2015-586 Date: February 2, 2016 This paper presented by Dymond and others represents a solid step forward in our efforts to simulate the effects of climate on landscape carbon dynamics in a manner that explicitly considers both disturbance and plant demographics. I applaud the authors for taking on such an ambitious suite of factors (as is ultimately necessary to make realistic predictions) while also resisting temptation to over-interpret every last one of their results (which could have easily led to an indigestible paper). The objectives are clear, text is well written, and the paper overall is an easy read. My only major concerns regards

the cryptic nature of some of the methods. I deeply appreciate that a comprehensive description of every modeling detail is unrealistic and works such as these must rely on citing earlier-published methods, however there remain a few things that require some clarification before a reader can really understand the results.

Response: Thank you for your kind and positive words!

Regarding the climate inputs that underpin the growth responses: I remain a little confused as to the spatial and temporal nature of the climate data and wonder how these alleged weather stations play in, but most importantly it is unclear exactly what underlies variability among the 144 different scenarios. Do they include alternate Fossil emission scenarios? Do they include alternate GCM models? Are they temporally stochastic expressions of a single change scenario (that would be cool)? And how did you end up with 144 of them. I don't see one answer being any better than another, but this is the source of variation that ultimately defines the "high" and "low" productivity scenarios, so I need to know what it is.

Response: For each ecoregion, historical daily weather data was collected from corresponding meteorological stations and analyzed using a rank and percentile test. Based on the rank and percentile test, 10 historical years of climate data were selected for each ecoregion and used as the historical climate scenarios in the analysis. The 10 years of data represent the 90th, 75th, 50th, 25th and 10th percentiles for both observed annual precipitation and mean annual temperature (Nitschke et al. 2012). A direct adjustment approach was used to create climate change scenarios from the selected historical climate data and global climate model (GCM) predictions for the study region (Nitschke et al. 2012). Monthly outputs from five GCMs were obtained from the Pacific Climate Impacts Consortium (PCIC, 2012). The GCMs and emission scenarios selected were: Hadley GEM-A1B; Hadley CM3-A1B; MIROC HIRES-A1B, GISS AOM- A1B; and, Canadian GCM3-A2. Climate change is projected to increase the study area's mean annual temperature by 1 to 3.5 degree C by the 2041-70 period, depending on the global climate models (PCIC, 2012). Mean annual precipitation pro-

jections are more variable with models showing increasing, decreasing or unchanging precipitation.

The 144 model simulations mentioned in the original text were 10 annual years of daily data per ecoregion (n = 5) multiplied by 4 time periods (Historical, 20s, 50s, 80s) multiplied by 5 GCMS. However, this was not clear and we will remove mention of it in the text.

Regarding construction of climate-specific growth parameters: A great deal of the study results depend on the relationships established between LANDIS growth parameters (i.e. max NPP, max biomass, and the growth factor r) and mapable climate metrics. The methods state that this was done using TACA-GAP, ZELIG, and BRIND. I and other readers unfamiliar with these tools don't need to know exactly how they work, but we do need to know the identity and source of the input variables. Are they based on some sort of empirical site index (i.e. max height and or biomass at some specified location)? Are there other growth limiting or facilitating process built in to the model. What is the source of the climate variables? Table 3 has a lot of information in it, but in no way tells me how growth became described as a function of climate (in TACA-GAP) and eventually time-space (in LANDIS).

Response: The TACA-GAP is a mechanistic gap model to estimate individual species growth potential (biomass) over a range of soil and climate conditions. The model does not simulate stand dynamics and interspecific competition rather the impacts of climate variability on growth over time. Species growth is a function of the maximum height, age, and diameter that a species can empirically achieve modified annually by temperature (sum of growing degree days); drought/ soil moisture (proportion of the year underwater deficit); and, frost damage (number of growing season frosts). The estimates of maximum potential biomass and maximum potential aboveground net primary production (ANPP) from TACA-GAP were linearly interpolated between climate periods and used as annual input to LANDIS-II. ForCSv2 calculated the actual ANPP for each species-age cohort on a grid cell as a function of the maximum ANPP for a

species, the amount of living biomass existing at a site for that species, and competition (the biomass of all existing species and the potential growing space available as provided by the maximum biomass) (Scheller and Mladenoff 2004). Cohort mortality is a function of age, competition or disturbance impacts.

Regarding the simulation of climate change: Did LANDIS dynamically update growth parameters to accommodate incremental climate change between 2010 and 2050, as graphically suggested in Figure 5, or did it simply run from 2010 to 2050 with fixed growth parameters representing the following scenarios: 1) average growth among 144 alternate climates between 1961 and 1990. 2) average growth among 144 alternate climates between 2040 and 2069. 3) average growth plus one SD among 144 alternate climates between 2040 and 2069. 4) average growth minus one SD among 144 alternate climates between 2040 and 2069. If growth parameters were indeed fixed throughout the 40 year simulations, it should be made clear in Figure 5.

Response: The growth parameters changed annually from 2012 to 2050. This was a simple linear interpolation between the parameters for the climate period 1971-2000 and the 2041-70 period. We will be happy to clarify in the text.

How exactly are the landscape-wide "high" and "low" productivity scenarios arrived upon?: In the Methods it seems like the "average", "high", and "low" growth scenarios (from among 144 alternate future climates) were species-specific. I see how these scenarios can scale across the landscape for a single species (e.g. Figure 9), but it is unclear to me how this works collectively across species (e.g. Figure 6). If this was explained somewhere, I must have missed it.

Response: The growth parameters were "high" for all species in all ecoregions for the high scenarios, or all average, or all low. While it is unlikely that productivity of all species in all ecoregions will go in a single direction, this does give us the bounding-box of productivity rates and plausible futures. Further research work will refine these scenarios. We will be happy to clarify in the text.

Regarding the t-tests for significance: I know there is pressure employ some sort of quantitative statistics to evaluate "treatment" effects in simulation models such as these, but rarely is it appropriate or necessary. The way I read it, the t-test in this paper compares two populations: 1) a population of 20 LANDIS runs sharing common set of "high" growth parameters, but differing randomly in the number, size, and intensity of disturbance imposed, and 2) a population of 20 LANDIS runs sharing common set of "low" growth parameters, also differing randomly in the number, size, and intensity of disturbance imposed. This was a great approach to comparing the relative influence of deterministic climate-driven growth in the context of probabilistic disturbance, but all you need to evaluate the results is the variation between and among groups. The strength of the P-value is irrelevant. After all, in the real world, if someone was testing for a significant climate effect they would factor out disturbance. However, if you were to do that here, one would reveal what is built into the model (i.e. a climate effect on growth). In other words, you can't really make discoveries about your own model, you can only evaluate sensitivity, which does not require P-statistics. If the authors choose to retain their t-test (which despite my diatribe, I suspect they will), they should explain in Figure 7, Figure 5, and Table 6 exactly what the sources of between and within group variance are.

Response: To be clear, the t-tests were comparing: the no climate change against the average scenario rather than low vs high. However, you are correct that the t-tests are simply evaluating if the impact of climate change on carbon indicators is greater than the inter-annual variability in fire impacts, as represented by the 20 Monte Carlo runs. Given the large impact fires can have on carbon dynamic, we felt it a sensitivity worth addressing. While perhaps meaningless in the results pertaining to carbon stocks, the annual Net Ecosystem Productivity and Net Biome Productivity flux results are certainly more interesting. We will be happy to clarify in the text.

Closing comment: In closing, let me again say that this is a well-designed study and well written paper which should serve as a good anchor publication to which future

modeling papers can be tied, including but not limited to those more deeply exploring the effects of climate and disturbance on the heterotrophic components of NEP, and Net Sector Productivity. I hope the questions I raised above help the authors clarify their methods.

Response: Thanks for your positive and constructive review!

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