Review of "Economic Gain Spatial Analysis-Timber for the Central Coast Region"
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by
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General Comments:

My expertise is in forest-level modelling and that is the focus of my review. I have no expertise in employment/productivity coefficients and therefore do not comment on their correctness. I am reasonably competent in forest economics and do offer a few comments in this area, echoing those given by the other reviewer, Mr. Jim Johnson.

My overall impression is that the data, model choice, model formulations and analysis are appropriate for a project of this scope, and that the work done is correct given the uncertainty of inputs, the range of scenarios and outputs demanded, plus resources available. Data collection and preparation is an enormous task for such a large geographic region and broadly defined project, and I think the authors have done a commendable job. For the most part, I make recommendations that will help clarify methodology and offer suggestions to help interpret the results. In some instances there are, I believe, errors in presentation (as opposed to errors in the model or the analysis) that need to be corrected.

Specific Comments:

Appendix A

1. You should specify that this is a Model II linear programming model. The reader can be referred to Davis et al. (2001) for an overview of LP formulations (including Model II) for forest-level planning.

Davis, L.S., Johnson, K.N., Bettinger, P.S. and Howard, T.E. 2001. Forest Management: To sustain ecological, economic and social values. 4th Edition. McGraw-Hill, New York. Chapters 11.

2. Woodstock is a well known and respected model and the reader can be referred to the Remsoft web site if they wish to learn more about it.

http://www.remsoft.com/

3. Table A.2.
    a. The Table makes reference to Table 2, Table 3 and Figure 4, which I can't find.
    b. I think Table A.2 should include Riparian and Terrain classifications that are referred to in Appendix D.

   c. Looks like a typo under the purpose column of Management Unit/Woodshed/Landscape Unit "TSA or TFL"?
   d. Under Harvesting Method: Are such constraints "to harvest a profile" used in your models? I think not, so just say it is way to track the harvest method.
4. Table A.3. Define ITG and SI as a footnote.
5. Section A.3. We should know how many analysis units are used in the central coast part of the study. For each of the 13 AU's, I am assuming that you have created AU's for each tenure type and management status (4 TSA's X 4 TFL's X 13 AU's X 2 mgmt status Existing/Managed) according to SI and presence. It gets a bit confusing in this section, especially with the comments about extending these AU's to the other coastal regions by adjusting the MU WSI. Some additional text making the links between Table A.4 and the yield curve figures would help.
6. Table A.7 should have footnotes defining OAF's.
7. Table A.4. Make it clear that the total for the CIT Central Coast Area is the sum of the TSA's and the TFL's. It currently looks like this total is only for the TSA's.

Appendix B

1. p. B-2. Please specify what costs are included DVC_w and $THC_{w,u,m}$. This will help with some of my next questions. Also, remind us that management status (m) refers to natural v. existing managed.
2. I generally follow the treatment of development costs, but similar to Mr. Johnson, I am confused about whether roads and bridges are expected to last 100-200 years or if maintenance costs include some form of capital replacement. The other area I find confusing is whether cost trends have been applied in the analysis. I think not, but there are comments that refer to declining costs in second-growth, and Footnote 8 (p B-6) that confuse the issue. Perhaps the trends are for development cost adjustments related to lower volumes. If this is the case, I suggest using "adjustments" rather than "trends". The end result is that it becomes difficult to identify the "base case cost model " that is referred to in Appendix D tables describing the scenarios.
3. Fig B.4. Supporting text should also state cost assumptions. The caption should include the price trend (0.3%). I was surprised that the lowest conversion return (without price trend) was $44. Is there no overlap between conversion return area with and without the price trend (e.g. some stacked bars)?

Appendix D

1. P D-2. First paragraph about the other regions should be in a footnote. Keep this report focused on the central coast.
2. Table D.2.
   a. See previous note about the cost model in Appendix B. Costs are not reported in the main report, so was the analysis not done or just not reported? If the former, put a N/A in the table.

   b. Price model: same as the cost model above (N/A if not done). If the analysis was done, state that only 100% price was used. Leave the 4 price options as a footnote.
   c. Operability >350m3/ha. This is part of how the THLB is determined and specifically if a stand within the THLB will be eligible for harvest. But is it also used to determine the minimum harvest age of stands? Or is some other method (e.g. max mai) used to determine minimum harvest age? We need a sense of how minimum rotation ages are determined and the range that results (true for all scenarios).
   d. Riparian and terrain constraints should be added to be consistent with other tables (e.g. D.4).
3. Table D.3
   a. Cost model – make sure we can find the base case cost model in Appendix B
   b. Price model – state that it was only run at 100%
   c. Add riparian and terrain constraints to be consistent with other tables (if not applied, put in N/A)
4. Table D.4
   a. Management Objective. I am sure the objective is to maximize NPV, not volume. I'd be surprised if we got such high conversion returns by maximizing volume.
   b. Clean up cost and price model parts as in the previous 2 tables.
   c. Avoid acronyms in this table (e.g. CR@100%, RONV OG)or add footnotes defining them.
   d. I am curious as to why the EBMPH scenarios have a non-declining even-flow constraint while the FE scenario is a strict even-flow constraint? A little text on the impacts of non-declining even-flow versus even-flow constraints might add to the report.
   e. Are the region/landscape level constraints (old) actually applied as constraints or are they simply tracking these indicators? When I look at Fig 3.8 and 3.9 in the main report, I see the amount of violation in the old forest occasionally increasing. If the harvest is truly constrained, and there is no natural disturbance modeled, this shouldn't happen – violations can only stay constant for a while and ultimately they must decline. Somewhere, there appears to be an old-growth "leak". It might be caused by the definition of these constraints (p. D-7 - …account for 95% of the area…)? I am guessing that the old-growth violations in Fig 3.8 and 3.9 never reach zero because the model has only been run for 200 years, and the definition of old-growth is 250 years (e.g. stands aged 0 at year 0 are only 200 years old at the end of the simulation).
   f. This refers to Fig 3.10 (mid-seral) but it has its source here – you should explain the increase and subsequent decrease. I assume this is caused by very young stands aging into the 40-120 year group in decades 3-4 (in areas that are not being harvested), followed by other stands aging out of this group in decades 5-8? Also, an explanation of the intent of this constraint wouldn't hurt, since such a broad age-class constraint can

become problematic if there is any variation in the stands recruiting into it (e.g. early seral from disturbance).

5. Sections D.3.4-D.3.6 – a sample calculation of the percentage used in a BEC/AU constraint would be useful. This could be added at the bottom of Table D.9.

Main Report

1. I agree with Mr. Johnson that the conversion return is not properly split between the land owner (stumpage) and the industry (profit to enterprise). As he points out, beyond a fair return to enterprise, the residual belongs to the land owner.
2. Figure 3.1 is not correct. It is the same as Figure 3.3. I note that the correct Figure 3.1 was in the October 14 version of the report.
3. At the start of Section 3.2 tell us why the TSR base case is not included in Figures for conversion return and state of the residual forest.
4. Figure 3.5. Caption for figure 3.5 should have "(Conversion Return)" in brackets following "Net Revenue". Aside from the greedy behavior of the FE scenario in the early decades, net revenue tends to increase over time. Often we see net revenue declining over time when the objective function is to maximize NPV. Having the condition where net revenue> 0 (operable stands) helps prevent these declines, and having a positive price trend leads to higher net revenues over time. I think some comments to this effect should accompany Fig 3.5, along with a reference to Fig B-4.
5. P. 9, first sentence. Note that total growing stock is defined to include both the THLB and the non-THLB.
6. p12, first paragraph after Fig 3.10. From Fig 3.11, it looks to me that there are more than 59,000 additional ha in old forest (approx. 1.4million-1.3million?). Also define old forest here (250 yrs+?). Reword the last sentence to clarify what areas you are comparing and in terms of the age classes shown in Fig 3.11.
7. Minimize acronyms. I would expect the report must service a wide audience, including international, so the fewer of these the better.

Uncertainty

1. Section 3.4 Sources of Uncertainty:
    a. This section can be expanded to include growth and yield information, natural disturbance and classification of "old-growth" stands. Growth and yield projections are always a source of uncertainty in forest-level models which affect not only volumes but also costs and revenues. Natural disturbance is probably a minor concern in the central coast (small gap dynamics) but over the long-term hurricanes and fires are conceivable, and these would tend to reduce the amount of old-growth that accumulates without limits in the inoperable stands. Given the amount of inoperable forest, these rare events would probably have little effect the harvest projections, but readers should be aware of the assumption of non-stand

replacing disturbance and how it contributes to unconstrained accumulation of old-growth.

Using a “crisp” 250+ age definition for old-growth stands certainly will influence harvest projections and residual forest conditions. The initial forest cover inventory often lumps vast areas of forest into a common age (e.g. 251 years or 235 years) and this can immediately affect the seral constraints within the model, and/or lead to periods when the seral constraints are binding. The authors should comment on whether this was the case in the scenarios they modeled. In future work, you might consider formulating “fuzzy” seral definitions that allow for a gradual transition from on seral stage to the next. This will reduce sudden jumps in the amount of old forest as stands age from 249 years to 250 years.