

3-28-12

Dear Dr. Stallworth:

I read through the 13 March draft of the SAB report from the Biogenic Carbon panel. From my perspective as one of the authors of the Manomet forest biomass study, I think the report does a very nice job of addressing the broad scope and complexity of the biomass policy problem. The concept of default BAFs for different categories of materials is something that makes a lot of practical sense--maybe with an option for a rebuttable presumption if a source thinks they can show they should not be excluded or merit some alternative BAF.

I'm not sure where the SAB is in its process of finalizing the report but I have a few comments that I would like to offer up for the panel's consideration.

- The report makes frequent reference to carbon neutrality, implying this is the metric by which bioenergy should be judged. Wouldn't it make sense to broaden the evaluation metric by recognizing that bioenergy could be 'carbon better' than fossil fuels while still not achieving full carbon neutrality? In the Manomet framework, bioenergy yields net benefits as soon as the debt (excess emissions from burning wood relative to fossil) is recovered through increased forest growth relative to the BAU. Even if we never get to carbon neutrality, we could still be better off with wood than with the fossil alternative. My point is that carbon neutrality sets the bar at an unnecessarily high position for bioenergy and it's time this got recognized.
- I don't believe the report adequately explains or draws out the implications of timescale with respect to the Allen and Cherubini analyses. If Allen is correct, don't we really only care about conversion of old growth forests? In all likelihood wouldn't most other sustainably produced biomass make sense given that any debt (note this is not the full emissions recovery required for carbon neutrality) is almost always recovered within a less than 100-year time period? So it would be helpful if the report included more explanation about the underlying mechanisms that explain Allen's conclusions. I looked at his paper but couldn't understand the underlying physical/chemical processes driving his conclusions. And I don't see how his results are reconciled with the Cherubini modeling that seems to show relative short-term impacts on temperature. An appendix that deals with these issues would be very helpful since the implications of this for forest-derived biomass are potentially huge and could dramatically simplify EPA's policy problem.
- The discussion of anticipatory expectations for the prospective biomass scenario gets a lot of air time. While I'm not an expert on this, I do wonder whether the discussion is too quick to suggest that markets will seamlessly equilibrate to the benefit of carbon accumulation? Our experience in the northeast suggests that landowner response can be very insensitive to prices and consequently I'd expect (1) the percentage change in new land put into forestry will differ a lot by region and (2) that the reallocation of land use based on expected prices is potentially much stickier than the text of the report implies.
- Finally, it's interesting that there's no mention of how BAFs might differ depending on the type of BAU fossil fuel (or other energy source) replaced by bioenergy. For any policy analysis, someone will have to identify the marginal energy source that gets removed from the BAU when constructing the bioenergy scenario. What this is could differ by region and this would likely affect the time required before society starts accruing bioenergy benefits. While the methodology conceptually accounts for this, I'm surprised the report remains silent on the topic as it is not necessarily a straightforward issue--are we replacing gas, coal, wind or solar at the margin? The timing of reaching the benefits phase could change significantly--although again if Allen is correct, maybe this doesn't matter?

Thank you for the opportunity to comment on the draft report.

Best regards,  
Thomas Walker  
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