Updated Individual Comments from Biogenic Carbon Emissions Panel

April 23, 2015

Contents

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Comments from Dr. Abt

Submitted 4-20-15

Overall I think the EPA has been responsive to the comments of the first SAB report. They have vigorously examined the application of an anticipated baseline approach. While I agree that science supports a dynamic "with" and "without" analysis to determine biogenic carbon, it is also true that adding complexity and uncertainty in pursuit of marginal changes won't necessarily lead to a better basis for policy. Because the entire document is policy neutral, our charge is to focus on the science. I think that simplifies our discussion, but solutions that improve policy can't be developed independent of implementation realities. The tradeoffs between simplicity, scientific rigor, and policy effectiveness are not the charge of the SAB, but it is where the ultimate success of the policy will be determined.

I've been asked to specifically comment on three issues with regard to the appropriate scale to use in an anticipated baseline approach, a) should we look at marginal or average effects, b) should it be measured in absolute or percentage terms, and c) should the baseline reflect the impact of the initial departure from the baseline or the marginal effect of the last unit added.

The questions of temporal and spatial scale are not independent. In the case of trees, looking at a management change for one stand in isolation yields a different dynamic carbon response than regional analyses that account for market, landuse and ecosystem feedbacks. The focus below is on spatial considerations.

(c) The limiting factor in capturing meaningful biological responses at a small scale is the ability of the available data to provide a basis to estimate a statistically significant signal. The revised framework report provides a good discussion on this topic. I think the homogeneity of the feedstock merits more emphasis. For example, it may take relatively few data points to capture a statistically significant signal from homogenous pine plantations managed using even-aged silviculture. In the same regional scale, it may be much more difficult to capture a change signal for mixed stands managed in a variety of ways. In the first report of the SAB, we suggested that regional/feedstock factors would be appropriate. I think there is ample literature on feedstock characteristics and regional commodity market integration to build defendable categories.

Analyzing small homogenous feedstock in small regional markets is statistically convenient, but it makes leakage and indirect effects more of an issue. In an attempt to capture all of the leakage, indirect, and economic interactions, global CGE models with endogenous resources and landuse would seem to be a more scientifically-based solution. Unlike the case above where the region may be too small to detect a signal (a relatively straightforward statistical determination), in a global simulation any signal below a major structural change is likely to get lost in the layers of assumptions required to capture the ecological and economic feedbacks of the global bio-economy. The approach analyzed by EPA is one useful middle ground along this spectrum.

Adding feedbacks necessary for an anticipated baseline approach affects the ability to detect marginal change. For example, forest biological dynamics over the next decade are largely pre-determined by starting age class structure. But the simple fact that the economic dynamics in these markets could be driven by housing starts adds enough variance to a baseline to alter the impact of bioenergy demand. Since it also affects residue availability, land rents, planting, etc. it is unlikely that the difference between "with" and "without" is robust across macroeconomic assumptions.

The sensitivity of BAFs to energy and economic context is important to understand. Similar to the modeling exercises mentioned by Dr. Skog, I think empirically testing BAF sensitivity is a modeling question that needs to be addressed. I would include reference point and moving average approaches in the suite of approaches to examine. Depending on the temporal/spatial scale being considered and the importance of markets across regions and feedstocks, a single superior modeling approach is unlikely.

(a) Regarding the question of marginal versus average impact measurement, marginal (per firm) changes might be interesting, but impractical both logistically (*which is beyond our charge*) and empirically. The market and resource impact of a small marginal change would likely be statistically insignificant. Further, the timing of entrants would affect each BAF and there would be incentives to be the first to permit, which could lead to perverse results. On the other hand, simple averaging would, by definition, limit the ability to capture marginal impacts. One logical next step might be to use the modeling exercise described above to define BAF thresholds (scales of consumption that shift the BAF). Cumulative regional/feedstock consumption would be tracked and marginal shifts would be defined as crossing a BAF threshold. Complications of the permitting process and possible perverse outcomes are important but beyond the scope of this discussion.

(b) Determining whether to measure change in absolute or percentage terms doesn't seem complicated. The market and statistical thresholds are likely to be measured in percentage terms, but if the carbon accounting requires absolutes, then we do it in absolute terms.

Overall, I think the revised framework document provides a good base discussion of the temporal and spatial tradeoff questions. I think the case studies provide useful examples. Key remaining questions go beyond the **potential** of the framework to capture carbon effects to include whether the uncertainty from adding dynamic interactions enhances or diminishes our ability to detect the carbon signal.

Addendum 04/20/15

2. What is/are the appropriate scale(s) of biogenic feedstock demand changes for evaluation of the extent to which the production, processing, and use of biogenic material at stationary sources results in a net atmospheric contribution of biogenic CO2 emissions using a future anticipated baseline approach? In the absence of a specific policy to model/emulate, are there general recommendations for what a representative scale of demand shock could be?**
a. Should the shock reflect a small incremental increase in use of the feedstock to reflect the marginal impact, or a large increase to reflect the average effect of all users?

As mentioned above, I think that marginal should be defined to mean a change large enough to significantly affect the calculated BAF. So for example, the model could be run to determine how a 1 mill ton vs a 10 mil ton increment in wood use. Given the limitations of the data, the uncertainty of the macroeconomic environment, etc. I would expect the marginal significant increment might be large. If for example a 5 mil ton increment is determined to be 'significant' for this model, then a "marginal" change is the movement to the next 5 mil ton demand increment.

b. What should the general increment of the shock be? Should it be specified in tons, or as a percentage increase?
c. Should the shock be from a business as usual baseline, or from a baseline that includes increased usage of the feedstock (i.e., for a marginal shock, should it be the marginal impact of the first ton, or the marginal impact of something approximating the last ton)?

I think the response to a) above implies that the baseline should include the increased use of the feedstock. Using DOE energy consumption estimates that are updated to reflect actual consumption is important. In other words, there is a distinction between a baseline that assumes a long-term trend based on its own view of future policy, and one that recognizes that marginal changes should reflect that the "base" demand is evolving.

Other notes from our phone conversation.

While I like the idea of a 5 year updated baseline, I think using a long term dynamic equilibrium in that context is problematic. By definition, these models reflect an optimal non-stochastic adjustment to a long- term equilibrium. It is common for these models to exclude short term business cycles and often their internal data is not current. While this is fine for long-term trend analysis and a better understanding of sector interaction, it seems particularly not well-suited for forecasting the next 5 years.

I worry that a 5 year update would essentially yield a sequence of short term effects which reflect the initial adjustment to long term uncertain phenomena. I don't see any reason that this will capture any of the benefits of updating the baseline, nor will it ever converge to match reality in the short run. The fact that the policy is exogenous to the forecast and unforeseen macro-economic shocks will likely drive the market also implies that there will be very little advantage, in terms of accuracy, to updating the first 5 years of a dynamic equilibrium forecast. It will likely just gives us an idea of what needs to happen in the short run to get us to the terminal conditions.

On the other hand, I'm not sure I understand the "bad actor" argument for making sure we punish folks who change land use and cause a drop in standing carbon. Land use change, and its carbon impacts, are captured in the model. So everyone's BAF should be penalized for that action. If the landuse change is not captured by the model, then the model is the problem. If we *agree that we are not doing firm level BAFs, then we are dependent on the model to tell an approximately correct carbon story.*

But I do favor an updated baseline, not to catch the "bad actors" but to implement a procedure that converges to the reality we observe as this market develops. As noted above, I don't think the first five years of a dynamic equilibrium will necessarily get us there.

Comments from Dr. Barlaz
Submitted 4-19-15

Introduction

In the first paragraph of the Introduction, the document suggests that landfill emissions are focused in the point source only. Indirect emissions are mentioned in footnote 3 on page N-4, but the rest of the Introduction suggests that only direct emissions are considered. As presented in the Introduction, this approach would provide an incentive to do a poor job of methane collection as fugitive methane would not be considered as an emission since it was not treated in a flare or energy generation unit.

The text also assumes that a flare or energy generation unit is an option and credits methane destruction relative to the alternative of direct venting. Under the Clean Air Act New Source Performance Standards (CAA-NSPS), landfills above a certain size (2.5 million metric tons) and landfills that generate more than 50 metric tons a year of non-methane organic carbon, are required to collect and control landfill gas. I think that EPA estimated that about 70% of all landfill gas is generated in landfills that are subject to the CAA –NSPS. As such, the assumption that direct venting is even an option is generous and gives credit for a flare when in fact it may be required by regulation.

The document neglects yard waste composting which is also a source of biogenic CO_2 emissions. This justified by pointing out that composting is not a point source which is correct.

On pages N-6 to N-7, the document correctly points out that waste is generated and that the question that requires evaluation is to quantify the CO_2-e (including methane) from alternative waste management strategies. I agree with this statement but this justifies the need to do a life-cycle analysis (LCA) (this document is clear that it is not doing a LCA). This leads to my biggest concern. Despite all the caveats in the text, I am concerned that readers will simply compare BAFs for various waste management alternatives and use them to rank management alternatives. The approach used in Appendix N neglects other critical aspects of a waste management LCA including offsets for energy generation and carbon storage. As such, a comparison of BAFs amongst MSW management alternatives is misleading.

I think that the definition of the Avoidmit term is inconsistent with the fact that energy and C storage offsets are excluded from its calculation. Specifically, I am worried about this text of page N-8.

> "In practice, as applied here, the AVOIDEMIT term is a proportion expressed as tCO2e avoided (i.e., the emissions reduced, in CO2e, resulting from an alternate waste management strategy to the combustion method) per tCO2e emitted using the combustion method (i.e., the emissions, in CO2e, resulting from the combustion waste management strategy)."

In Table N-2: MSW combustion is -0.02 relative to a landfill with a flare. This is close to zero yet MSW combustion with energy recovery generates so much more energy.

Section 1.2

See paper by Staley on waste composition

Staley, B. F. and M. A. Barlaz, 2009, “Composition of Municipal Solid Waste in the U.S. and Implications for Carbon Sequestration and Methane Yield,” J. Environ. Eng. 135, 10, p. 901-909.

There is some food waste being treated anaerobically at WWTPs but I would hardly call it common. It is just possible and interest is growing.

Section 2

First paragraph: delete “and compaction of the waste”

With respect to methane oxidation, another branch of EPA has looked at this and come up with the following in the Federal Register V 78, No. 230, 11/29/2013, p. 71971. These values are given on age N-14. In work we did for the WARM model we used methane oxidation values of 10% for landfills with no gas control, 20% for intermediate cover and 35% for final cover.

TABLE HH–4 TO SUBPART HH OF PART 98—LANDFILL METHANE OXIDATION FRACTIONS

Under these conditions:	Use this landfill methane oxidation fraction:
I. For all reporting years prior to the 2013 reporting year	
C1: For all landfills regardless of cover type or methane flux	0.10
II. For the 2013 reporting year and all subsequent years	
C2: For landfills that have a geomembrane (synthetic) cover with less than 12 inches of cover soil for the majority of the landfill area containing waste	0.0
C3: For landfills that do not meet the conditions in C2 above, and for which you elect not to determine methane flux	0.10
C4: For landfills that do not meet the conditions in C2 above and that do not have a soil cover of at least 24 inches for a majority of the landfill area containing waste	0.10
C5: For landfills that have a soil cover of at least 24 inches for a majority of the landfill area containing waste and for which the methane flux rate is less than 10 grams per square meter per day (g/m²/d)	0.35
C6: For landfills that have a soil cover of at least 24 inches for a majority of the landfill area containing waste and for which the methane flux rate is 10 to 70 g/m²/d	0.25
C7: For landfills that have a soil cover of at least 24 inches for a majority of the landfill area containing waste and for which the methane flux rate is greater than 70 g/m²/d	0.10

[a] Methane flux rate (in grams per square meter per day; $g/m^2/d$) is the mass flow rate of methane per unit area at the bottom of the surface soil prior to any oxidation and is calculated as follows:

This sentence is not accurate

"Controlled landfills also include a topsoil cover to passively treat the remaining landfill gas that is not collected via CH4 oxidation."

Page N-12 – the paragraph beginning "When organic materials are landfilled…" is not a good description of what happens. I will rewrite this text if useful, or provide references.

In the next paragraph, the chemical composition given is applicable to residential MSW, not all MSW.

The Staley paper referenced above provides updated C storage factors. For lumber, use the factors in

Wang, X., Padgett, J. M., De la Cruz, F. B. and M. A. Barlaz, 2011, "Wood Biodegradation in Laboratory-Scale Landfills," Environ. Sci. and Tech., 45, 16, p. 6864 – 71

Section 2.2 Direct Emissions from MSW Landfills

The EPA Office of Resource Conservation and Recovery developed the WARM (Waste Reduction Model). I worked with EPA and ICF closely to revise the figures for methane collection efficiency. A full explanation of this work is available at

http://www4.ncsu.edu/~jwlevis/Landfill_WARM-2014.pdf

These values are the best estimate of the best people in the industry, EPA and academia. They are an improvement over simply using 75% throughout.

Also – I can provide a summary of data on collection efficiency – you have missed many studies.

Section 2.3

Again, a comparison between a landfill with and without a flare seems very generous as the baseline of no gas collection is atypical.

Section 2.3.1

The text is clear that this is not a LCA, I am nonetheless concerned that people will simply compare BAFs and use the results like an LCA.

Offsets – The offset benefit is very different when comparing a landfill to combustion. As such, comparing BAFs across technologies is inappropriate yet facilitated by the results presented.

Carbon storage – it is true that carbon storage is not a function of how the gas is collected but the Appendix compares BAFs across technologies and there is more C storage in a landfill than in a combustion facility so its exclusion appears problematic for purposes of MSW treatment technology comparison.

Section 2.3.2

Page N-18, 4th bullet at top: Do you mean Direct CO_2 emissions ….?

Table N-4: The ranges are not based on a thorough literature review and should not be represented as such. For example, up to 100% methane oxidation has been reported.

Page N-26: The BAF for the flare is slightly lower than the BAF for energy recovery because no offsets are included. This again is a very confusing result despite the caveats.

Table N-5: Can you define a base case?

Section 3.1

It is reasonable to assume that MSW that is not incinerated will be landfilled. Nonetheless, the calculated BAF is not an accurate comparison between these 2 alternatives as this is not a full LCA.

Comments from Dr. Harmon

Submitted 4-14-15

Alternative Framework

An alternative framework that would be transparent and intuitive to most everyone would be based on EPA's own words describing the basic question involved in the use of biogenic fuel stocks:

"Is more or less carbon stored in the system over time compared to what would have been stored in the absence of changes in biogenic feedstock use?"

The alternative framework could be expressed in annual (for time t) or in cumulative terms (for time period T). In addition, the framework would specify the boundary condition used to calculate the BAF, for example whether it included direct biophysical or indirect market effects or included atmospheric effects. The reason to specify the boundary condition is to avoid confusion and talking at cross purposes.

The basic formula to calculate the BAF would remain the same with the exception that any adjustments to the PGE related to losses in transport be separated from the NEB equation. The generic formula for BAF time interval T and boundary condition B would be:

$$BAF_{BT}=NEB_{BT}/PGE_{T}$$

Where the NEB_{BT} and PGE_{T} terms represent the sum of the two terms over the period T. For a specific year t and boundary condition B the BAF equation would be:

$$BAF_{Bt}=NEB_{Bt}/PGE_{t}$$

Where NEB_{Bt} and PGE_{t} are the annual changes in NEB and the annual PGE. Hence the only functional difference between the two formulae would be whether a period or an annual temporal resolution is used.

There would also be two versions of the NEB, but all would be based on difference between a reference scenario (r) and a policy scenario (p) in terms of carbon stores and hence would directly answer the basic question raised by EPA. At the most aggregated level the NEB formula for time period T and boundary condition B would be:

$$NEB_{BT}=TC_{rT}-TC_{pT}$$

Where TC stands for terrestrial carbon and NEB_{BT} represents the difference in carbon stores between reference scenario (r) and the policy scenario (p) at the end of time period T. This and all subsequent versions of the NEB equation are presented as a finite difference framework given that it unlikely the time step is likely to shorter than 1 year; however a calculus version similar to that presented by Dr. Reilly could also be used. The reason the policy scenario is subtracted from reference scenario is to provide the correct sign: a loss of carbon stores caused by the policy scenario would lead to a positive sign in NEB. Conversely a gain in carbon stores caused by the policy scenario would lead to a negative sign in the NEB.

If NEB is considered at an annual time step then the rate of change (Δ) in the difference in carbon stores between the reference scenario (r) and the policy scenario (p) at time t can be computed as:

$NEB_{Bt} = \Delta(TC_{rt} - TC_{pt})$

Expanded out this would be:

$\Delta(TC_{rt} - TC_{pt}) = (TC_{rt} - TC_{pt}) - (TC_{rt-1} - TC_{pt-1})$

which is the change in the difference scenarios between time t and t-1. This means that when the difference between the two scenarios ceases to expand or contract NEB_{Bt} equals zero.

If a time step other than one year, for example 5 years, is used then it would be the rate of change over that period (e.g., Δ/5 years) instead.

The annual equation can be converted to a cumulative period T for boundary condition B as follows:

$NEB_{BT} = \sum_{t=0}^{T} \Delta(TC_{rt} - TC_{pt})$

Which sum of the annual change in difference in the stores each year t in terrestrial carbon over a total period of time T between the reference scenario (r) and the policy scenario (p) for boundary B. The advantage of this formulation over the version described above is that it can more adequately address situations in which the difference in stores is not described over time as a straight line.

This formula could be subdivided to represent different sectors (i.e., agricultural, forest, waste, etc) or divided into major pools involving differ processes or controls. For example if one was calculating the cumulative effect on individual carbon stores over time period T for boundary condition B, the framework would consider the net change in carbon stores of live (CL), dead (CD), soil (CS), products (CP), waste pools (CW), and transportation loss (TL) pools.

$$NEB_{BT} = \sum_{t=0}^{T} \Delta(\mathrm{CLrt} - \mathrm{LCpt}) + \sum_{t=0}^{T} \Delta(\mathrm{CDrt} - \mathrm{CDpt}) + \sum_{t=0}^{T} \Delta(\mathrm{CSrt} - \mathrm{CSpt}) + \sum_{t=0}^{T} \Delta(\mathrm{CPrt} - \mathrm{CPpt}) + \sum_{t=0}^{T} \Delta(\mathrm{CWrt} - \mathrm{CWpt}) + \sum_{t=0}^{T} \Delta(\mathrm{TLrt} - \mathrm{TLpt})$$

While this expanded formulation includes 6 terms, it still contains one less than the current NEB formula and they are essentially analogous input-output subsystems. If the framework boundary is expanded to include fossil carbon substitutions then those could be added as well as an additional pool subject to inputs and outputs. Although these pools would have to be defined, the terms would be based on what the pools are and not where the pools came from or where they are going. These pools can be subdivided as needed, but the key feature is that all the terms can be readily aggregated or disaggregated and still follow conservation of mass and be subject to mass balance. In addition all the terms would be analogous input-output systems despite the fact the actual processes leading to input and output would change. The new formula would be scale and process invariant as it could be used for a stand or plot, a fuel shed, and a region. It would apply to the system whether direct or indirect effects such as market signals are considered but would explicitly specify when the boundary conditions have been changed. In sum this formula would reduce unneeded complexity, be more direct, be transparent, and would be easily scalable.

Analytical solutions to NEB equations.

While simulation models could be used to estimate the temporal changes in NEB_{BT}, the fact that the formulation is based on pools that have inputs and outputs has major advantages and would allow one to intuitively check the sign and magnitude of NEB_{BT} without elaborate modeling. For example, under steady-state conditions the input (I) and output (O) of carbon is equal:

$I=O$

Where both I and O have units of mass per area per time. The output is determined by the proportion being lost per unit time (*k*) and the amount stored when the system is in steady-state (Css):

$O= k\ Css$

Where Css has units of mass per area. Therefore the steady-state can be predicted as:

$Css= I/k$

This simple formulation applies to all the pools storing carbon (and the virtual stores related to substitutions if that is added) and can be used to test whether the reference scenario or the policy scenario will store more carbon. In the case of increased harvest intensity or frequency k must increase by n and since:

$I/k > I/(k(1+n))$

then there must be a loss of carbon in the system if the policy scenario involves an increase in harvest. Conversely, if the policy scenario also includes an increase in I equal to n then it is possible for there to be no loss in carbon because:

$I/k = I(1+n)/(k(1+n))$

In the case in which I and k do not change, for example when the losses in two cases are equivalent (e.g., burning in a power plant versus burning in the field), then there is also no new net loss of carbon. Finally, when there is just an increase in I then there is a gain of carbon in the system since:

$I(1+n)/k > I/k$

This might reflect the case of negative leakage in which new forest area is increased and effectively increases I.

Boundary Conditions

The alternative framework equations could be used for several boundary conditions:

Direct biophysical effects (DB) which would consider the effects of harvest on the area equivalent of the fuel sheds within a region.

Indirect effects mediated through market signals (IM) which considers responses outside the fuel sheds. Using this boundary condition would essentially deal with the leakage question without confounding pools or emissions with boundary conditions.

Atmospheric responses (AR) in which the temporal effects on greenhouse gas warming of the atmosphere of net carbon added or removed by biofuels activity would be considered.

Full life cycle (LC) in which the effects of substitution for fossil fuels would be considered. While this might be handled by including a substitution pool, it would be specified in the NEB and BAF terms as a change in the boundary conditions.

Subpools

Although one could consider all terrestrial carbon pools in aggregation, the different controls and timing of subpools suggests that it may be better to treat each separately. To address the pools in the original framework the following carbon pools would be needed: live (CL), dead (CD), soil (CS), products (CP), waste pools (CW), and transportation loss (TL) pools. The leakage term would not be needed because it is addressed by changing the boundary condition. This would avoid the current confounding of pools and boundary conditions (i.e., the LEAK term influences the live, dead, soil, products, waste, and loss stores; it not a separate kind of store or flux as now indicated).

The inclusion of product stores is necessary because the current framework treats all products as having the same infinite life-span, a scientifically unjustifiable assumption. The policy decision to not include product life-spans appears to be related to a concern that power plants using biogenic carbon should not be responsible for the actions of those creating products because this is an indirect effect. However, leakage is also an indirect effect and is being considered; if indirect effects are considered, then all indirect effects should be considered: the boundary conditions should be consistent once specified. It is not clear that the use of fate of products is beyond the control of the power plant in that the power plant can select to what product the carbon is sent. By not discriminating among products, a long lasting product (e.g., biochar) will have same consequences as a short lasting product. The current framework also ignores the potential effects of biogenic carbon harvest on past accumulations of product stores. to begin with. Neither seems likely. If harvest is diverted into biofuel feedstocks, then the size of the products carbon store accumulated from past harvests would have to decrease, leading to a net flow of carbon to the atmosphere. However, the current framework cannot detect such a flow.

The inclusion of transportation losses as a pool would address another problem with the current framework which assumes that all losses are instantaneous. This simplifying assumption has no basis in science and inflates the PGE term, but does not address the stores. By tracking the changes in this pool, the NEB equation would be more consistent.

While most of the pools can be dealt with on a carbon dioxide basis, the waste pool (i.e., carbon that is disposed of and not deliberately used) involves the release of methane. This is problematical in that methane has a higher greenhouse gas warming potential than carbon dioxide. This could be dealt with in several ways. Waste carbon that is subject to loss via methane could be tracked separately from waste carbon that is lost as carbon dioxide. For example, wood waste carbon is likely not subject to loss via methane, whereas non-woody waste is likely to produce methane. The stores of the two pools could be adjusted to reflect difference in stores. An alternative would be solve the waste carbon contribution not as a change in stores, but as a change in fluxes. However, this would also require separating waste into the portion generating carbon dioxide versus methane and would introduce non-analogous terms into the NEB formula.

Comments from Dr. Rose

Submitted 4-20-15

Additional General Comments (S Rose)

It is unclear what feedback EPA is seeking from the Panel. This is because it is unclear how exactly the framework will be used and what BAF calculations will be used. Also, the charge questions to the Panel are very narrow, but there are broader issues. If, ultimately, an endorsement of the framework is sought by EPA, then the Panel should take time to address the broader scientific issues (see below). The Panel could consider providing scientific methodological guidance for creating BAFs for specific policy applications. However, it is unclear whether that would imply another round of review to evaluate specific BAFs for a specific application(s).

There are a number of scientific questions related to the Framework that the Panel could consider addressing:

1. Equation concept
2. Equation components/terms
3. Component/term calculations
    a. Approaches – representative, customized, hybrid. The choice is a science and implementation issue, for both BAFs and equation terms.
    b. Methodology and data
        i. Feedstock(s)
        ii. Region(s)
        iii. Stationary source process
        iv. Transport & storage
        v. Emissions and feedstock quantity
        vi. Methodology issues
            1. Temp scale
            2. Spatial scale
            3. Baseline
            4. Feedstock
            5. Process
            6. Modeling and calculations
            7. BAU
            8. Bio demand over time (policy)
            9. Markets
            10. Non-CO2 GHGs
            11. Leakage
4. Consistent treatment of emissions accounting for other fuels (e.g., coal, natural gas, oil)
5. Implementation approach – could have science consequences (e.g., Who will be doing BAF calculations? How often? Would it vary by user within policy and across policies?)

A variety of general issues were identified by the SAB Biogenic Carbon Emissions Panel during our in-person meeting (March 25-26 2015). We were asked to provide preliminary thoughts on these topics as well:

- Framework equation – panelists suggested re-formulating the equation in terms of carbon cycle pools. This is an excellent suggestion for a number of reasons: it would yield a more intuitive and understandable equation, it would directly align with carbon cycle and observational sciences, it would respect mass-balance, and it would be more amenable to the utilization of updated inputs as scientific understanding evolves. All these things would appeal to the scientific community and improve understanding and peer review potential.

- Spatial scale – A spatial scale beyond the biomass feedstock source is important for capturing market and land management changes that might result beyond the source location. Also, additional analyses would be helpful to estimate the bias of different spatial scales

- Reference point v anticipatory baseline – The reference point approach appears to be offered by EPA as an equally legitimate option to users of the framework. The Panel had a strong opinion in its initial feedback about the reference point approach being problematic. The Panel appears to still have this opinion. The main problem with the reference point approach is that there is no counterfactual and thus it is impossible to know how much of the observed carbon changes can be attributable to increased biomass demand.

- Policy neutrality – The framework is extremely difficult to evaluate without specific applications. It needs specific policy context(s) and associated proposed BAF calculations for proper evaluation.

- BAF practicality – As noted by the Panel previously, calculating specific BAFs for individual compliance entities is impractical from an implementation point of view and scientifically challenging given broader market and land management effects.

    - One option would be to create default BAFs that policies/states/others could choose to use, or they could opt to develop a custom approach. Custom approaches however could create inconsistencies in methods and results across states, and evaluation of the implications would therefore be important. This sort of default BAF approach would be a substantial improvement in terms of consistency over what is offered now in the revised framework with its vast number of different types of BAF calculations available to users.
    - Another pragmatic option for the Panel to discuss is the possibility of EPA doing analysis to make waiver decisions regarding the counting of bioenergy emissions.

Some sort of peer review of the analysis would still be needed, but this approach would reduce the implementation issues and challenges substantially.

- Comments on some other proposals
    - Panelists proposed that fossil displacement be included in BAF calculations. This, however, is unnecessary and impractical. The climate policies driving biomass use will implicitly create the fossil displacement trade-off as compliance entities look for lower carbon strategies.
    - Panelists also proposed a calculator computing change in radiative forcing over time from forest logging residues. The necessity of such an approach is unclear, and BAFs over time should be sufficient for capturing the implications of the timing of fluxes on the climate system.

- Users/uses of framework – users and uses were not specified in the revised framework, and the revised framework is not user friendly. EPA needs to clarify the framework's users. It was surprising that EPA noted that states were not users. While states may not be the intended users, they will likely be users nonetheless since there is limited analytical information available to them.

- Numerous variants of BAF calculations – it is hard to judge the many BAF variants with the limited illustrative results available in the appendices and no policy/implementation context. Also, all the variants are presented as equally legitimate. However, that is not the case. The Panel could provide some guidance or principles helpful to narrowing down the alternatives to scientifically defensible variants.

- Implementation – little attention is given to BAF implementation in the framework. However, these details are needed, and they need to be weighed in EPA's and the Panel's evaluation of BAF alternatives.

- Model direct and indirect effects separately – the Panel briefly discussed the possibility of separately modeling direct and indirect emissions/sequestration effects for agricultural biomass feedstocks, where "direct" was defined as the specific location of origin of the feedstock and "indirect" was defined as everything beyond that (and would be classified as leakage). This would potentially be a different approach than that used for forest-derived biomass feedstocks. The idea proposed was to use different estimation methods for the two effects with the argument being that we could be fairly precise in estimating the direct effect with agronomic modeling. This strategy has some appeal but should be fleshed out and evaluated before being pursued. Consistency could be an issue – with the potential for meaningful inconsistency between direct and indirect effect estimation, as well as across feedstock BAFs. Among other things direct effects are already embedded in the indirect effects modeling, which raises both inconsistency and double counting issues. Implementation realities of pursuing separate direct and indirect effect modeling should also be considered.

- The illustrative examples in appendices – It is unclear whether the Panel should be reviewing the appendices examples, and if so, how. The Panel should make a clear decision on how it will proceed with respect to the illustrative examples. Note that the results shown are insufficient for the Panel to evaluate the many BAF variants and feedstocks laid out in the framework.

- Non-co2 – EPA's position on non-CO2 greenhouse gas accounting is unclear. The framework clearly includes methane associated with MSW feedstocks, but nothing more. However, the illustrative agricultural waste example includes a sensitivity result with N2O emissions accounting. What is sought from the Panel in this regard? Furthermore, it is scientifically asymmetric to account for non-CO2 emissions with biomass feedstocks, but not account for non-CO2 associated with fossil fuel feedstocks, e.g., coal mine fugitive methane emissions, natural gas production and distribution fugitive methane emissions.

- Ranges of BAFs – the idea of BAF ranges was mentioned during the Panel meeting. This is practical given uncertainty; however, ranges will create implementation challenges for users. Guidance would be required on how the ranges are to be used in rulemakings so as to have scientifically defensible results and consistency across applications.

- Policy coordination – the Panel raised the issue of policy coordination, where there will be jurisdictional overlap across rules and agencies with respect to emissions and sequestration activities. For instance, it was posited that policies regarding land carbon could account for land-based emissions and carbon stock changes, which raises the question of whether we need BAFs at all. However, this is far from straightforward to operationalize. For example, would land owners be able to transfer carbon rights or abandon them at a price? Furthermore, EPA would still need to make a decision about how to count bioenergy emissions from stationary sources when carbon values are internalized within delivered biomass.

- Principles – Panelists suggested that the Panel could help EPA by providing principles. This seems like a pragmatic and useful strategy that we should consider. For instance, the following is a potential initial set of principles that arose during the Panel meeting and appeared to have general support:
  - Climate change is a long-run phenomenon
  - Important to take into account temporal dynamics of emissions implications
  - BAF equation should be structured in terms of carbon pools
  - Incremental ("marginal") BAF calculation appropriate
  - Regular model updating should be planned
  - Etc.

- Size of shock – the size of the shock used in the illustrative BAF calculations should be driven by economics. Impractical to simulate unrealistically large supplies to estimate BAFs.

Comments from Dr. Sedjo

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Comments on the SAB meetings March 25-26, 2015

The discussion of the meeting provided further evident the weaknesses of the "Framework" approach to addressing the question of how to regulate carbon and GHGs from stationary sources fueled by biogenetics. The basic approach of the framework relies on the use of a BAF as an accounting system to determine the extent to which a particular biogenetic fuel source is neutral or a source or sink. At the conceptual level the framework may be useful in identifying important components that determine neutrality or lack thereof. However, at the empirical level, as would be necessary if the BAF were to be used for regulatory purposes, estimating the magnitude of the various components is daunting at best. Unresolved issues abound. This is particularly true of forests, which by their nature have substantial time dimensions. These unresolved issues relate to the appropriate spatial scale and boundaries as well as to the time scale. Also, the costs of an empirical application of the framework approach to each facility are likely huge, while generating little of regulatory value.

There are additional unresolved issues. What are the appropriate boundaries: a stand, an ownership, a county, a state, region or country? Theory tells us nothing about boundaries and scale. A stationary source can draw from a host of areas. Even if the range is limited by cost features, the entire system will readjust as the old sources are abandoned and new ones drawn in. Thus any empirical BAF developed for a particular source is likely to change over time as some forest stocks are drawn down while new stocks in new locations are accessed, grown or regenerated.

Perhaps even more fundamentally, the question of leakage becomes critical. Leakage relates to emissions or sequestration that takes place outside of the designated geographic and boundary areas. Undetected leakages make correct estimates of the various BAFs impossible. In the case of bio-wood exports, such as with the increasing flows of wood pellets, monitoring the domestic stationary sources will not allow either an estimate of the emissions or an estimate of the land use and forest impacts, as these will be drawn from other sources. Thus, where leakage is an issue the framework approach is fundamentally flawed. In the US, of course, international leakage of bio-wood has grown substantially, as wood pellets for energy are increasingly being exported to Europe and elsewhere thereby making the monitoring of stationary source emissions in the US increasingly irrelevant. Where exports are significant, simply monitoring the areas sourcing the stationary facilities will be of little use as large volumes of wood will be exported independently of any US plant related facilities. Additionally, the locations for these exported pellets will also likely change though time. Finally, logs for pellets are now competing with pulp logs in many markets. Thus market volumes could be misleading.

Furthermore, the ideal of monitoring part of forest withdrawals for US bioenergy without looking at the total withdrawals including bioenergy offshore is of minimal use whatever policy is selected. Thus, as suggested in the SAB meetings, a broader comprehensive forest monitoring system is necessary if a system is to be of any credibility or use at all. Should a broader system be utilized, any activity focused solely on US stationary sources would be redundant and of little use.

A useful approach was suggested in Acting Assistant Administer Janet McCabe in her memo of November 2014, which suggested a broad forest sustainability approach. Such an approach would capture the effects of the use of wood for bio-energy in the broader context of capturing all of the effects upon the forest stock. I note here that while it is generally agreed that the optimal approach to addressing emissions from forest is to tax the emission and subsidize the regrowth, in the absence of the ability to subsidize effectively the second best approach is simply not to tax wood emissions (e.g., Tian et al. 2014). Note that taxing biogenic emissions encourages the substitution of fossil fuels for renewable biogenic feedstocks. If desired or required by law, the emissions associate with US bioenergy could be monitored at the smoke stacks of the various stationary facilities and the effects on the forest of the use of wood by simply monitoring the natural wood inputs into each facility.

Tian, X., B. Sohngen, R. Sands. 2014. “The Greenhouse Gas Effects of Wood Bioenergy—A Dynamic General Equilibrium Model,” presented that the meeting of the American Economic Association, Boston 2014.