Updated Individual Comments from Biogenic Carbon Emissions Panel
April 8, 2015

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Comments from Dr. Barlaz

Preface
My comments are focused on the manner in which the framework would be applied to waste as that is the areas in which I have expertise. In general, I struggled with the idea of a policy neutral approach and think that the framework will be most useful as applied to a specific policy. In its current form, I think that the framework may serve as a catalog of everything that should be considered. Exactly how each term should be evaluated requires a policy to be specified.

f. How should scale of the policy be considered, particularly for default factors? (e.g., can a single set of default factors be applied to policies that lead to substantially different increases in feedstock usage)?

My overall sense is that the factors used may have to be policy-specific or more broadly, evaluation-specific. EPA has tried to develop a "one size fits all" approach and I understand that there are reasons why this may be appropriate or required. I worry however, that this approach may lead to outcomes that are not desirable. With reference to landfills, the EPA has defined a narrow system that encompasses the landfill gas control device (flare, engine) and also recognizes fugitive emissions. However, in practice, I am concerned that Appendix N results will be used to compare waste combustion with waste burial. Such a comparison can only be made after a full life-cycle analysis (the document is clear that EPA did not attempt an LCA). Specifically, without consideration of offsets from generated energy (typically electrical) as well as carbon storage, comparisons between waste management options are not correct.

EPA has defined narrow systems focused on emissions control at the source and compares alternatives for emissions control within a system. For example, comparing a landfill with and without a treatment device to convert methane to carbon dioxide. This may be all that EPA can do within its current regulatory authority. In this case, the agency must include a discussion of how Biological Assessment Factors (BAFs) should and should not be used.

g. Would the answers to any of the above questions differ when generating policy neutral default factors, versus generating factors directly tied to a specific policy?

I understand why default factors should be consistent but given the limitations in defining the various systems for evaluation, I am concerned that one set of factors for all situations may lead to inappropriate comparisons.

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Comments from Dr. Buford

Additional comments

Lack of policy context

- This panel and this effort should refrain from trying to, or appearing to try to, make national (with a little n) forest policy. From the discussion, and from a number of the comments, it appears that some appear to think this is more about national forest policy (or view this as an opportunity to try to make forest policy) than about an appropriate accounting system.
- This group should not be advocating for a "land" policy, or for "any" policy. It should be commenting on the efficiency and efficacy of the BAF approach for accounting for emissions and providing advice on what would be a science-based, practical and affordable way of doing it if BAF is not the way to go.

Comments from Dr. Harmon

Spatial Scale

The EPA 11-14 Framework does not present a well thought out consideration of spatial scale. Unfortunately the initial SAB report did not cover this aspect as thoroughly as was needed. The 11-14 Framework followed the SAB recommendation to not determine BAF's at a stand or plot level but instead use a landscape or regional level. Unfortunately the initial SAB report did not specifically define what a landscape was or what a region was and as a result several conceptual problems regarding spatial scale appeared in the 11-14 Framework.

To help remedy that situation, in the following section I describe the different spatial levels in more detail, their characteristics and other features relevant to estimation of BAF's.

A stand or plot is the smallest level of spatial extent that might be considered in determining a BAF. Although theoretically possible to consider the area covered by individual plants (e.g., trees), this level of detail would be completely impractical and unnecessary. The key feature of a stand or plot is that it is homogenous in terms of environmental factors, the biota occurring in it, and its disturbance and management (including harvest). As noted in the initial SAB report, while the stand or plot level is a logical building block for broader scale estimation of BAF's, it is not practical to track each and every stand or plot over a long period for long-rotation systems. Nonetheless one can track virtual stands[1] or plots to estimate BAF's at broader extents. Another feature of the stand level is that the concepts of a carbon debt versus credit and their order (but we will see this is largely irrelevant at the landscape level) as well as the timing of emissions (e.g., decomposition-related emissions) can important considerations.

Footnote 1: A virtual stand is one that is equivalent in area, environment, biota, disturbance, and management, but its location is not specifically known. However, it acts as if the location is specified. By analogy it is not important to know exactly which dollar bill to be used in a purchase, the only important thing is that each dollar bill is equivalent.

The next level of spatial extent would be a landscape. Unfortunately there are multiple meanings for landscapes: it is always an area larger than the typical stand or plot, but in the BAF context could range between the area containing a single fuel shed to a region with multiple fuel sheds as well as areas not managed to produce biogenic fuel stocks. A more useful way to look at a landscape is to consider how increasing spatial extent alters the apparent behavior with respect to carbon. The key feature of a stand or plot is that it is homogeneous and its disturbance and management events are synchronous. In contrast, a key feature of a landscape is its potential for disturbance and management events to be "partial"[2] and asynchronous. As the spatial extent increases environmental and biotic factors (such as the species) can become heterogeneous in the landscape; however, it is

the potential for the stands and plots to become asynchronous that changes the apparent temporal behavior of landscapes, specifically by dampening the variations observed at the individual stand or plot level. In the most extreme case of asynchrony (e.g., a regulated harvest with a uniform age-distribution), harvest losses in one stand or plot is completely countered by gains in the other stands or plots in the landscape (or vice versa). As a result a landscape can be theoretically in steady-state as long as the environmental, biotic, disturbance, and management factors influencing the carbon flows are constant and have remained so for a long enough period for the steady-state to be reached. It should be noted that while this is a theoretical possibility, in reality it is never achieved because temporal variations in all these factors will cause the carbon stores to "wobble" about a mean. This behavior might be termed a quasi-steady-state as it is not the classical constant steady-state, but it does have a central tendency as well as variation. Even when there is variation in the controlling factors, the variation in carbon is highly dampened and the mean adequately captures the concept of a true steady-state. The opposite theoretical situation occurs when all the stands in a landscape are synchronous in terms of disturbance and management events. In this case, the landscape acts similarly to a very large stand or plot. Real landscape behavior falls between these two theoretical extremes; if large enough relative to an individual harvest or disturbance event a real landscape carbon dynamics should largely exhibit a highly dampened behavior except in the case when a disturbance or management event deviates markedly from the average. In the specific context of biofuels, the rotation required to produce the fuel stock influences the degree of dampening of carbon dynamics. If an annual rotation is adequate to produce the feedstock, then the fuel shed will act like a very large stand or plot. As the rotation interval increases, the less annual variability will occur, in part because in any given year a smaller and smaller fraction of the landscape is involved in the harvest.

Footnote 2: By partial it is meant that only a fraction of the landscape area is impacted by a given disturbance- or management-related event. In the case of a regulated harvest the area influenced is one divided by the harvest interval.

Although the theoretical landscape with complete asynchrony is often used to explain the impact of biofuels harvest, it must be borne in mind that this is a special case and cannot be used to explain the general case of effects of biofuel harvest on landscape carbon stores. Unfortunately this special case has been used to explain all biofuel systems in general. In many cases biofuel harvest represents a new flow of carbon (an additional drain or loss) from the landscape. Therefore there are only a few situations in which this explanation of continued steady-state is valid: 1) the harvest for biofuels has been going on long enough that a new steady-state has already been reached or 2) the harvest of biofuels substitutes exactly for another drain or loss from the system. The first case is not generally the one that generates concern relative to the atmosphere; rather the concern is that increases in harvest intensity (fraction harvested each harvest event) or the frequency of harvests (the inverse of the harvest interval) will lead to a reduction in carbon stores. The steady-state condition therefore has no bearing on this novel situation and cannot be used to explain it. In the second case some substitutions might lead to no change in landscape carbon stores (e.g., materials burned in the field versus burned in a

power plant), but for some the substitution might lead to a decrease in stores outside the landscape, for example in forest product stores. More specifically, if a harvest that has been used to create buildings or other long-lived items is diverted into a biogenic fuel stock, then a decline in the carbon store in buildings and municipal waste related to products will occur.

At the landscape level, the concept that the order to carbon debts and credits is important becomes nonsensical. That is because at the landscape level carbon debts and credits are being created simultaneously, hence the order is irrelevant. This does not mean that carbon debts or credits cannot occur (i.e., landscapes are always in carbon balance). In the case of landscapes, carbon credits or debts are created when changes in environment, biota, disturbances, management influences the landscape stock in a systematic manner. The debit or credit becomes apparent once the change becomes systematic and will remain as long as the new values of the controlling factors remains.

An another point that seems to have been missed in discussions is that while it is theoretically (as long as a quasi-steady-state has been reached) possible for growth in many stands or plots to offset the loss caused by harvest in one stand or plot, the opposite can theoretically occur when considering decomposition losses. That is inputs of dead material caused by harvest can offset the losses occurring in all the other stands or plots in the landscape. That is because when a steady-state is reached the inputs have to equal the outputs. There is no reason that inputs have to be in multiple stands and outputs in a single stand or vice versa. In the live carbon case, the harvest removal is in one stand or plot, and at steady-state the other stands or plots are adding in an equivalent amount. In the dead carbon case, decomposition losses in multiple stands or plots is equal to the amount of material being added by a harvest (or disturbance for that matter). Unfortunately, the 11-14 Framework materials seem to suggest that decomposition-related losses do not act similarly to harvest-related losses at the landscape level; in part because it only considers decomposition-related losses at the stand level; moreover the framework materials ignore the fact that not all material is necessarily decomposed within a harvest cycle. When the time to decompose a cohort of dead input exceeds the harvest interval, accumulations larger than the inputs are possible.

Increasing in terms of spatial extent, the next level to be considered is the region. There is no fixed definition of a region per se, but that does not mean one cannot be specified in the case of biogenic carbon. In the context of biogenic carbon harvests, a region might defined as the area that contains at least one fuel shed as well as multiple land use types. While there is no scientific upper areal limit to a region, it could be defined statistically to represent the area in which environmental, biotic, disturbance, and management factors are relatively homogeneous. An indication of this degree of homogeneity could be expresses in the degree of variation in responses to changes in controlling factors or the minimum within region variation relative to the variation between regions (e.g., a nested analysis of variance).

In addition to these considerations of spatial extent, it should be acknowledged that the biofuels problem is a nested hierarchical spatial system with at least two levels as defined by: their level of openness; the degree effects are primary (or direct) versus secondary (or indirect); and the degree they represent one set of highly related players versus multiple, relatively independent players. One of the complexities of the biofuels system is that it is hierarchical; ignoring this structure leads to ineffective communication, conceptualization, and hence understanding of the system. While these hierarchical levels intergrade, they are important to distinguish.

At one extreme, there is the fuel shed which can be defined as the actual area or its equivalent that is directly impacted by biofuels management. At this level, the effects are largely direct, the players are those related to a single facility including the suppliers of biofuel feedstocks, and the system is relatively closed. While it is true that that carbon can flow in via photosynthesis, and out via respiration, fire combustion and harvest, by envisioning it as a closed system one can determine how much is exchanged between the terrestrial part of the system and the atmospheric part by mass balance. Another feature, at least in theory, is that the flows in and out can be in the form of carbon; flows of information such as market signals do not occur between other fuel sheds or other land uses (this is addressed at another level). Because the system is relatively closed means that conservation of mass has to be strictly obeyed and carbon cannot appear or disappear without a specific biophysical mechanism within the system. Although in some cases the areas involved in this fuel shed may be contiguous, there is no need that this always be the case, nor is it necessary to know exactly where harvests will be. That is because if the plant is operate at a fixed capacity the fuel stock must come from somewhere and the area is likely to fall in a limited range. For any given year, the area involved must be a function of the capacity of the plant and range between the area required using the most carbon dense fuel stock and the area required for using the least carbon dense fuel stock. For a give fuel stock, over the life of the power plant the area involved could be as large as the power plant's life-span times the annual area harvested. If multiple rotations of the feed stock can be produced over the life-span of the power plant, then this maximum extent will decrease as the number of rotations increases. While these areas have uncertainty, they are highly constrained by the nature of the fuel stock and the capacity of the plant. Specifically a power plant cannot burn more biofuel than its capacity, and while it might burn less, it cannot burn far below its capacity and remain economically viable.

At the other extreme one can have a region in which multiple biofuels plants are drawing from and that also contains alternative land-uses that could complete or compensate for what is going on in any particular fuel shed. At this level the system is relatively open with diffuse boundaries, strict conservation of mass may not be obeyed relative to a particular fuel shed (but would have to be obeyed for the larger system), and there are multiple independent players that respond to each other via market signals. In terms of the BAF, this is the level at which leakage begins to be observed. While the spatial limits of this level are not fixed, there are likely spatial limits on its size. At the lower end, it has to be larger than an individual fuel shed; while there can be shifts of what is

harvested or grown that are caused by economic factors within the fuel shed, these can be accounted for directly. Additionally without being large enough to contain alternative land-use types, it is hard to envision, at least in the negative leakage case, how there could be much adjustment. At the upper end this level cannot be literally "global" in that it is hard envision how oceanic areas, zones of environmental extremes (e.g., Antarctica), and highly urbanized environments could possibly be involved. Moreover, it is unlikely that conservation reserves, national parks, and wilderness areas would be participating in market adjustments. Although this leaves a considerable terrestrial area, it is very much smaller than the global surface. From an economic perspective the upper boundary is diffuse in that market effects could make the boundary smaller or larger over time. Nonetheless these are unlikely to be random shifts, and should be dependent on a number of factors that can be examined scientifically (e.g., transportation costs, degree of local competition for resources, degree resources can be substituted, and strength of market signals).

Comments on the Analytical Framework and an Alternative

The analytical framework has been changed to address some of the issues uncovered in the initial SAB review such as spatial scale dependence and redundancy of terms. I believe it is essential the new formula be checked, but few of the SAB panelists seem to have provided written or oral comments on the fundamental formula that describes the 11-14 Framework. I believe the problems I have found are not based on personal misinterpretations or my failing to understand definitions as offered in the 11-14 Framework. Rather I believe they represent fundamental problems that would need to be corrected before this framework could be applied correctly. Below I describe in detail some of the problems I have uncovered. I then describe a reformulation that would solve most if not all the problems that appear in the formula that appeared in the 11-14 Framework.

For reference the basic formulae in the 11-14 Framework included:

BAF= NEB/PGE

NEB = (PGE) (GROW+AVOIDEMIT+ SITETNC+ LEAK)(P)(L)

The first issue is a continued lack of transparency and confounding of terms. The formula for the BAF is clear and intuitive, but the formula for NEB is not. Although it can be argued that as long as the current formula for NEB is solved using fundamental terms such as the net change in live stores etc it remains suitable. While that may be true, the fact that the fundamental terms are not being used means, by definition, that the formulation is not transparent. Having a non-transparent formula means that the framework is less direct and more complicated than it has to be, neither of which has value.

Confounding of terms is also a problem. The most obvious example involves LEAK which accounts for leakage (negative or positive) caused by the harvest of biogenic

feedstocks. The process of economic leakage would influence the terms involved in GROW and SITETNC (and AVOIDEMIT if that was a valid term—see below for why that is not the case). It therefore is not an additive term as indicated, but is instead the same system of terms examined at a different spatial scale or process level. If I recall correctly, in the original framework formula, LEAK was an adjustment, but in the current framework it seems to be expressed as a set of flows which are added to other sets of flows that it could involve. This is formulation is therefore confusing scale or level of examination with different flows. There are also less obvious, but to my mind other confounding of terms. Although the Framework indicates there is no overlap between AVOIDEMIT and SITETNC is not clear why this is not the case for dead material. AVOIDEMIT appears to account for the flows from dead material, but SITETNC accounts for the net change in stores of the same set of pools which is a net flow. The current framework seems to distinguish terms based on where the carbon came from or where it is going to, but in terms of net balances of the fundamental pools neither is relevant. Dead material could come from natural mortality, incomplete harvest, and disturbances. All are inputs as far as a carbon balance is concerned. Dead material could be lost to the atmosphere by decomposition, combustion in the field or combustion in a power plant. These are all outputs as far as the atmosphere is concerned and as long as fossil carbon is not part of the Framework, are not important to distinguish. To elaborate on this point if there are three landscapes of the same size and characteristics except there is one in which slash is left to decompose, one in which slash is burned on-site, and one in which the slash is harvested and combusted in a power plant. If these landscapes have been managed these ways long enough for them to be in steady-state, then the flow of carbon to the atmosphere for each of them is identical and equal to the rate of slash generation. If they are all the same size in terms of inputs to the atmosphere, the how would the atmosphere be able to distinguish them?

The current formulation for determining NEB is not directly subject to mass balance. Given that that the net flow of carbon is solved by most systems using mass balance, this presents a major problem. While the underlying terms might be subject such as change in soils stores via SITETNC might be subject to mass balance, this has to be done indirectly. This therefore leads to another aspect of non-transparency and indirectness that could be avoided.

The current framework equation is based on an analytical model method that was designed to compare a reference point to some point in the future. However, the underlying assumption of the current framework seems to be an anticipated baseline versus a counter factual scenario that includes harvest of biogenic carbon. The current formula therefore seems to be a holdover from the initial approach. The current approach would involve using a process based model to calculate the difference (sometimes called the wedge) between these two cases. Translating the output of a process-based model into an analytical model should therefore be an unnecessary steps, but would be required using the current framework because no process-based model calculates changes using these terms. While some translation will no doubt be needed, these should be trivial in nature (e.g., dead material could be detritus or litter or debris, but all are essentially the

same) and not complex. As an indication of the complexity involved in translating process-based model output into the framework one need look no farther than Appendix L Table L-1.

I believe that the NEB formula contains an inappropriate term. Specifically AvoidEmit which seems to account for the fossil carbon substituted for by use of harvested dead material. This would mean that either the system being described is not obeying conservation of mass or it not consistent with the bounds described in Figure 3 of the 11-14 Framework, which specifically indicates that fossil fuel substitutions is not included in the framework. If Figure 3 is a true representation of the system then it cannot be obeying conservation of mass because it does not include fossil fuel substitutions. The increase in carbon to system caused by AVOIDEMIT, has to be appearing from outside the system, otherwise it cannot be explained. This is most obvious when a dead material is instantly burned in the field versus in a power plant. If this is so, then switching management would, in theory, have no impact on the stores of dead material because in both cases they would be zero. Yet, with AVOIDEMIT, there would be a gain in carbon in the system. The only explanation for this gain is that fossil carbon was not used, but then this exceeds the bounds of the framework. The framework is therefore inconsistent in that it treats dead material one way, but treats products differently in that product substitution is not accounted for. So either the framework has to be expanded to include all fossil carbon-related substitutions or AVOIDEMIT has to be removed. It cannot remain as it currently is formulated.

As mentioned above, if the 11-14 Framework really does consider the levels described in Figure 3, then it exhibits a lack of conservation of mass because of the AVOIDEMIT term which is premised on fossil carbon outside the system. But it does so in at least one other area that involves products. While the impacts of future of products related to biofuel harvest is considered (although incorrectly by assuming no losses), it does not consider the effect of future biofuel feedstock harvests on products that existed before the feed stock harvest has started. It therefore must involve unstated assumptions (another lack of transparency): either the flow of harvested carbon to products always remains unchanged or there were no products to begin with. Neither seems likely. If harvest is diverted into biofuel feedstocks, then the size of the products carbon store would have to decrease, leading to a net flow of carbon to the atmosphere. However, the current framework cannot detect such a flow. Unless the area being examined has had no harvests in the past, then it seems likely that products and related stores of disposed materials would have accumulated from past harvest. The current framework is incapable of accounting for losses in these stores and thus violates conservation of mass. While it has been argued that a reduction in products stores is unlikely, the framework should not be based on what is or is not likely: it should be based on conservation of mass and mass balance.

As mentioned above, the current framework NEB formula confounds spatial scales by including a separate leakage term that involves many of the same processes and pools as the so-called direct or primary effects. The formula is also very difficult to aggregate or

disaggregate. Ideally the formula could be aggregated into one term or disaggregated into many terms all obeying conservation of mass and subject to mass balance.

Finally, the terms used in the NEB formula do not translate directly into those used by inventories, models, and other frameworks such as the IPCC. These systems are all based on well recognized pools such as live, dead, soil, products, and waste. They all involve well recognized processes such as mortality, harvest, combustion, respiration/decomposition, etc. While they do not use identical terms, they are conceptually related and should be readily translated into the EPA biogenic framework and vice versa.

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?"

At the most aggregated level the NEB formula would be:

NEB= $\sum_{t=0}^{T}(\mathrm{TCab} - \mathrm{TCcf})$

Which sum of the difference in stores each year t in terrestrial carbon over a total period of time T between the anticipated baseline (ab) and the counterfactual (cf) involving biogenic carbon harvests. The reason the counterfactual is subtracted from anticipated baseline is to provide the correct sign: a loss of carbon stores caused by the counterfactual would lead to a positive sign in NEB. Conversely a gain in carbon stores caused by the counterfactual would lead to a negative sign in the NEB. I note that a related calculus based formula has been offered by Dr. Reilly, but essentially the concepts presented here are similar.

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 the framework would consider the net change in carbon stores of live (CL), dead (CD), soil (CS), products (CP), and waste pools (CW):

NEB= $\sum_{t=0}^{T}(\mathrm{CLab} - \mathrm{LCcf}) + \sum_{t=0}^{T}(\mathrm{CDab} - \mathrm{CDcf}) + \sum_{t=0}^{T}(\mathrm{CSab} - \mathrm{CScf}) + \sum_{t=0}^{T}(\mathrm{CPab} - \mathrm{CPcf}) + \sum_{t=0}^{T}(\mathrm{CWab} - \mathrm{CWcf})$

If the framework boundary is expanded to include fossil carbon substitutions then those could be added as well. 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 could 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. In sum this formula would reduce unneeded complexity, be more direct, be transparent, and would be easily scalable.

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 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 counterfactual or the anticipated baseline 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 counterfactual involves an increase in harvest. Conversely, if the counterfactual 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.

System Functional Boundaries

During the discussions various system boundaries have been discussed. It would be very helpful to put these in one place and to contrast their benefits and limitations. This differs from the issue of the spatial boundaries of the system discussed above.

According to Figure 3, the current framework does not consider fossil carbon and only involves the net change in carbon stores in the terrestrial system. I have questions as to the degree the formulation for NEB stays within these boundaries, but in concept this is the most restrictive boundary. The disadvantage is that it does not consider the potential effects of timing on the atmospheric system.

Dr. Skog has proposed an alternative in which the time effects on the atmosphere of net carbon added or removed by biofuels activity be considered. While this alternative would likely address the timing issue, it does raise two questions: 1) would adoption of this system mean that fossil carbon emissions have to be treated differently than currently proposed and 2) would expansion outside the terrestrial system only involve the atmosphere? Currently the fossil-based power plants would have their full emissions counted, but if the dynamics of the atmosphere are considered then some fraction is taken up by the oceans. To have symmetrical treatment, both systems would have to treat the atmosphere the same way (as gross emissions or net emissions involving oceanic uptake). If the effects on the atmosphere are to be considered, then why wouldn't the effects on the oceans be considered as well given that acidification is likely to cause environmental damage? I don't have an answer to this question, but if the framework includes atmospheric effects, it would help if the framework explained why atmospheric-climate related damage is of more importance than acidification damage in the oceans.

Dr. Schrag has proposed that the BAF remain 1 until the amount of biogenic carbon substituted for fossil carbon equals any carbon debts caused by harvesting biogenic carbon. After this point the BAF would be set to 0. This system would require that the boundaries of the framework be expanded to include fossil carbon substitution. The advantage is that the carbon debt (or gain) could be estimated with a relatively simple analytical equation based and the payback time could be estimated from the annual harvest of biogenic feedstock and the displacement factor (which is in turn determined by the form of fossil carbon being substituted for and the net amount of energy captured per unit biogenic carbon). However, expanding the boundaries would also introduce complications: 1) there are other forms of substitutions involving products that would have to be considered and 2) displacement related to these substitutions is not infinite in nature. The more difficult to deal with is the fact displacement, while often assumed to be of infinite length is for a discrete period. This is because unused fossil carbon eventually can be used by someone and this is market dependent. Therefore the payback time may be considerably longer than the harvest indicates and if the leakage associated with substitution is high, then it is possible the carbon debt cannot be repaid in full. There will also be complications in including product substitutions in that the current convention ignores leakage and assumes displacement is of infinite duration (instead of tying the life of the displacement to the life of the products).

During the discussion of Dr. Schrag's proposal, there were comments that the payback of carbon debts could be caused by regrowth of the fuel stock. However while this reduces the effective value of the BAF, this is not strictly a payback. The phenomena that the BAF is reduced over time is caused by fact that NEB and PGE behave differently over time. The cumulative NEB is limited and while it can be positive or negative, in time the system will go to a new steady-state and the cumulative NEB (assuming the environment, biota, disturbance and harvest remain about the same) is fixed. In contrast the cumulative PGE has the potential to increase each year and for as long as biogenic feedstocks are used. This means the basic BAF formula:

BAF= NEB/PGE

behaves as a hyperbolic function diminishing over time, but never reaching zero. Hence the debt is never repaid unless substitution is included.

A Practical, Science-based Implementation System

Dr. Skog should have provided some comments related to a practical, science-based implementation system. Here I provide some additional thoughts on this topic.

Estimation of BAF's will involve uncertainty. This does not mean that there is no scientific certainty in estimating BAF's, just that there are scientific bounds in these estimates. To develop a practical system it might make sense to uses this uncertainty to the framework's advantage. That is one could use a class-based system that is defined by the level of uncertainty.

Assuming that the policy objective of this framework is to send the signal to the biogenic carbon sector that it is not acceptable to release more carbon on a net basis than the fossil carbon sector, then there are two important breaks: 1) a BAF of zero which indicates no net release to the atmosphere and 2) a BAF of 1 which means the net emissions are equivalent to fossil carbon. This means that at a minimum there would need to be three classes: 1) worse than fossil carbon: BAF>1, 2) somewhat better than fossil carbon: 1>BAF >0; and 3) much better than fossil carbon: BAF≤0.

Additional classes might be scientifically determined based on the level of scientific uncertainty. Sources of uncertainty might include the degree of: 1) model parameter uncertainty, 2) model structure uncertainty, and 3) sampling or data uncertainty. In the case of model structure uncertainty, there are differences between ecological and econometric models in terms of assumptions, but there is also a difference between whether one examines the direct, closed system effects, or the indirect, more open system effects. In this sense the analysis with and without leakage is a difference in model structure. Before applying this system, an analysis of the level of uncertainty likely present should be undertaken to inform the number of BAF classes used.

Another feature of the BAF estimation system that would make the overall system transparent would be acknowledge that this is a nested, hierarchical system involving a

closed-direct effects subsystem, nested within a more open-indirect system. This would mean that estimates of BAF's should involve examination of the closed-direct system and the open-indirect system. In effect this scientifically brackets the range of expected responses and increases transparency in that one can tell where and how carbon is lost to or gained from the atmosphere. As proposed by Dr. Rose, it should be possible to examine the same areas to determine the two responses as long as an anticipated baseline and counterfactual scenario are run for each and the key variable examined is the difference between these two scenarios.

Ecological Models

Although the aspects of econometric models were discussed extensively in the 11-14 Framework, the same attention to detail was not given to ecological models. I am particularly concerned that certain formulations of ecological models would not be suitable for estimation of BAF's and that many econometric models may not be modeling ecological phenomena adequately. I believe this needs fuller discussion in the SAB. Particularly worrying are models that are highly empirical because they cannot adjust to novel conditions. For example, models that rely on fixed growth curves would be problematical if the factors controlling growth change because of changes in the environment, biota, disturbance, or management. Models that invoke the basic processes of production, mortality, decomposition, etc are far more flexible in this regard than fixed curves of growth, decomposition, etc. I am concerned that models based on assumptions of uniform age-structures and steady-state frameworks could be used to estimate the net change in carbon stores. By definition these will estimate no change in carbon stores in a context in which the stores could be in fact changing. Another concern is the basic kinds of carbon stores included in the models. It is essential to use models that include all the basic pools: live, dead, soil, products, and waste (i.e., disposed materials). It is essential, not because (as has been implied) these are someone's favorite pools, but because use of conservation of mass in mass balance calculations requires it. Excluding them because they are not one's favorite is not more scientifically justified than including them because they are someone's favorite. They are included because the pools exist and must therefore be considered. If, in certain contexts, pools prove to be very small or prove to not change, then excluding these terms might be justified. However, that would create numerous models that change with context and hence lead to a complex system of models. In my opinion a better solution would be to have the same model predict different outcomes in different contexts. Finally, to work correctly one must use ecological models that incorporate initial conditions of all the pools. The behavior of the carbon system with regard to BAF depends on the difference between the initial and the final value of carbon stores. By not specifying the initial conditions, the ecological model tacitly assumes the store is zero and this could bias results dramatically.

Comments from Dr. Reilly

Reflecting on this issue generally, I think there are 3 questions to which one must answer in the positive if the approach is to be defensible:

(1) Does it appropriately deal with the physical aspects of the problem?
(2) Does the method provide a measurement/accounting system that has some scientific basis where we believe that assessed carbon implications are capturing "accurately" the physical aspects of the problem as described to meet (1)?
(3) When implemented is there a science foundation (where I include economics as an important part of the science) to believe that the actual carbon implications of the system will be as assessed in (2)?

The EPA staff members grappling with this problem are doing a heroic job to come up with something. I think, however, that it is an impossible task to develop factors that can be applied to actual biomass sources that will meet these criteria. Certainly we can do (1) and I think the proposed guidelines are there or are close. Answering the second question positively is far more problematic. If the guidelines deal with average emissions factors for any process, even if those processes are defined fairly precisely, there is going to be large variation—every hectare of forest is different, every hectare of corn field is different, every pile of waste material is different. In previous review, the SAB argued for the difference from a future baseline to be the scientifically appropriate approach. In the EPA charge questions, they raise many appropriate and practical questions about how such a baseline would be developed—including time horizon, nature of the perturbation they would use to shock a model, etc. Having constructed models of this type myself, my view is that they are illustrative, give some insight into processes, but unless they can be corroborated in some way would be very poor guides for establishing a factor to apply to different biomass sources.

I realize such factors have been applied to biofuels in this way, but I don't believe these have much foundation either. Different models can come up with much different values, and even different signs of the effect. Biomass material planted on severely degraded land, and managed to be productive, can lead to considerable build up of soil carbon even with the harvesting of the biomass, whereas, of course, harvesting a relatively mature forest and then putting it in a short rotation biomass crop would likely lead to a significant reduction in the carbon stock. In Reilly et al., 2006 we have some examples of how land conversion to cropland, or abandonment can have completed reversed and hugely different signs. As a result, the variation in carbon implications across different parcels with different histories is nearly infinite, or I guess essentially equal to the number of parcels you divide the world up into. I don't know of any model that has this detail, albeit, for example, the TEM model with which I have worked does have "data" on land history that goes back to 1700 or so, and then simulates how the carbon stock of each individual cohort (disturbed at a different point in time) changes through time. A

recent attempt to estimate these changes for the US is described in Lu, et al. (in press). This is heroic, and possibly a big improvement on previous estimates for the US and large regions such as states. But for the approach proposed here one needs to first believe we have the current condition of each parcel correct (because the ability to store carbon depends on the disturbance history going back hundreds of years). Then we need to project which parcels would be used for biomass to supply a particular point source use over the indefinite future (or tag each of these infinitely varying bits of biomass with a unique carbon content) based on an estimate of how each parcel would have evolved into the indefinite future, and predicting how that would change carbon storage, compared to a case without the biomass installation. One could imagine that installations would describe where they would get their biomass-the "wood shed" idea we had in the last go around. But, any sensible company will want to be able to shop around for the lowest cost source of material in the future. So I don't know how one would enforce that initial plan over the life of the point source.

I don't think any model could ever achieve credible reliability at the level needed. My view is thus that any factor used should be corroborated with direct measurement or reporting/estimation based on data for each source of biomass at each installation. Of course, the reason the SAB recommended the future baseline approach is that the carbon implication of a particular use depends on management of the forest (or field) into the indefinite future, and we can only assess that with a model. And, the most difficult aspect of the problem are the indirect emissions associated with land use change elsewhere given that one has displaced a current activity on a parcel to produce biomass energy. Again, that can only be assessed through the construction of a "what if" alternative. I guess potentially one could attempt to statistically isolate an effect if one had enough years of data, but the problem is that land use change is massively dominated by almost every other possible use of land—very little biomass energy is actually being produced, and so the bioenergy signal in land use change is vanishingly tiny.

Finally, then as observed with the biofuel rules, if some of the corn or palm oil is being harvested and grown responsibly and others not, and you set up a criteria that favors the responsibly grown material, and the bioenergy part is a very small component of the total, there is every incentive to make sure the irresponsibly grown product ends up in the uncontrolled market, and the responsibly grown material is dedicated to regulated bioenergy. But, then that regulation simply reshuffles which material is being used, without having any effect on global carbon emissions.

Hence, I think that any variation on the broad method proposed is going to fail on the 2nd and 3rd criteria I propose as necessary.

The far more effective way to do achieve low carbon biofuels is to focus directly on forests and land use. If carbon is effectively managed on all land, then that will necessarily be reflected in biomass energy (and conventional forests, agriculture, etc.) and any addition regulation on the use of bioenergy will be redundant and unnecessary. The memorandum from Janet McCabe, Acting Assistant Administrator, Office of Air and

Radiation indicates that the Administration will pursue conservation and sustainable management practices for land. If that is effective, then no further need for these regulations. And, if those efforts are being intensified, then by definition, any estimates we would derive from historical evidence where these efforts were not in place would be wrong. (Or the simulated future would have to correctly estimate the effect of these efforts, whatever they might be.) Of course this leaves effects abroad via trade uncontrolled unless foreign governments control land use change in their countries themselves. We have limited ability to affect those practices. Obviously international negotiation is a possibility. Border carbon adjustments are another possibility where a tariff on imported goods related to its life cycle carbon implication of producing is applied at some level. We have looked at this broadly (Winchester et al., 2011). Consistent with my conclusion above that even carbon factors are applied on different biomass sources they will have virtually no effect on global carbon emissions, we find that border carbon adjustments have only a very tiny effect on actual emission abroad—because of the reshuffling of trade, etc. Such adjustments could be applied just biomass feedstocks or energy product from abroad—of course that would miss the impact through trade of agriculture production shifting abroad. However, as a general principle, the narrower the application, the more possible ways to avoid them having any effect. In Winchester, et al. 2011 we apply border GHG border adjustments to all goods imported into the US—the broadest possible policy the US could institute, and it still has a vanishingly small impact on emissions outside the US. So focusing on just biomass energy would reduce the tiny effect we see there by an order of 1000. The border carbon adjustment can impose a significant economic penalty on foreign countries, hence it could be a strategy for inducing action abroad to protect forests. In the Winchester et al piece, I believe we found that a carbon in other countries a few cents could be as effective at reducing carbon in these countries as a US border adjustment of $30 or $40, indicative of the near uselessness of this approach to reducing emissions. The border carbon adjustment mechanism is the perfect analogy for the entire approach of trying to tag biomass sources with a carbon coefficient, and hence why I believe the evidence is clear that this entire mechanism would have virtually no effect on carbon emissions in the US or globally. (Except maybe for the volume paper on which the regulations were printed and stored.)

A Specific Comment on the Adequacy of the Science Foundation of the Approach

On Page 2, Figure 1. This simple figure is useful, however, I think it could be much more useful if it could highlight a crucial element: What matters is the change in the carbon stock in vegetation/soils combined with the change in a stock of carbon in a product or processing loss. I wonder if this couldn't be illustrated by having the vegetation/land picture shown as either expanded or shrunk—representing a growing or shrinking stock, and perhaps imagine some supply chain loss stock or product stock expanded or shrunk. If on net those two stocks are growing then there is a carbon benefit. If on net they are shrinking there is a carbon loss. The flows back and forth from vegetation/land to atmosphere, or the emissions from the stationary source are irrelevant if we measure the stocks. The current figure unfortunately doesn't tell us anything about

the ultimate fate of products or processing losses. If those products are long lasting lumber products and this stock is building up over time that is keeping that carbon out of the atmosphere. We had a very confused discussion in the panel last time about "biomass waste" with some concluding it should get pass—z zero carbon coefficient. That is not true. Waste that accumulates and only gradually decomposes, if in a steady state where the waste added, equals the rate of decomposition, will permanently keep that carbon out of the atmosphere. If we disrupt that situation, by harvesting the waste and burning it then that is essentially speeding up the decomposition—replacing it with a very fast combustion process. As a result the equilibrium waste stock will be much lower, and so the lowered stock will mean more carbon in the atmosphere. E.g. take corn stover. If left on the field it will slowly decompose, become soil carbon, and eventually oxidize, a process that will take years to decades. If we harvest, and burn it quickly, there are less additions to the soil, yet the soil decomposition will continue at faster rate, until the stock of soil carbon sinks to a level consistent with the lower input of residue. Unfortunately there is no carbon-free lunch here with waste, unless for some reason the waste itself would oxidize as fast as the combustion process.

The need to focus on the change in stocks of carbon was raised by Professor Mark Harmon in his comments at the meeting and in writing. This led to what I believe is a rather general and straightforward mathematical formulation of the problem, which is theoretically sound from an ecological, earth system, and economic perspective. This formulation here is a little more complex than it may needs to be as I have generalized it so that it can represent different questions raised by EPA. I am calling the Net Atmospheric Carbon Change (NACC), or really it is the present value of the change :

$$NACC = \int_{t=0,i=0}^{t=T,i=T'} PC_t(LC_{iR} - LC_{tA})e^{-rt} + \int_{t=0,i=0}^{t=T,i=T} PC_t(PRC_{iR} - PRC_{tA})e^{-rt} + \int_{t=0,i=0}^{Tt=T,i=T} PC_t(WC_{iR} - WC_{tA})e^{-rt}$$

Where:

PC_t is the Price of Carbon at time t

LC_{iR} is the Stock of Land Carbon at time i in the Reference or base case

LC_{tA} is the Stock of Land Carbon at time t in the Alternative simulation with bioenergy production

PRC_{iR} is the Stock of Carbon in Products or By-Products of the bioenergy production process in the Reference or Base case. This might be 0.

PRC_{tA} is the Stock of Carbon in Products or By-Products of the bioenergy production process in the Alternative simulation with bioenergy production

WC_{iR} is the Stock of Carbon in Waste in the Reference or Base case.

WC_{tA} is the Stock of Carbon in Waste with the bioenergy production process in the Alternative simulation with bioenergy production

r is a discount rate.

Stocks of carbon could be further divided up depending on available measurements, product pools, etc. For example, each of many different wastes could be a separate component, soil and vegetation carbon could be separate, different product and by-product pools could be separately identified, each of these could have a domestic and foreign component, etc..

From this general formulation many of the important issues discussed by EPA and the panel immediately fall out.

(1) The baseline

What should the baseline against which the Alternative scenario is compared? This is capture in the iR subscript. If i=t, then the alternative scenario in each year is compared against the Reference projection for the same year. If T' is equal to zero, then the Alternative scenario is always compared with base year level of carbon emissions rather than a dynamic baseline.

(2) How to deal with the fact that changes in carbon stocks occur over time as a result of use of biomass energy in any given year?

How far into the future should we consider the carbon impact? This is determined by the choice of T. If T=1, then only the carbon change in year 1 is considered, and none of the regrowth over time is credited. T could be 5, 30, 100, or ∞. Economists would prefer to treat time using economic discounting and evaluate the value of carbon in the atmosphere as it varies through time. Thus, I include a time varying price of carbon and discount rate. If this was judged to not be acceptable, then PC_t or r or both would be set to 0 and these terms would fall out of the equation. Dan Schrag argued that discounting was in appropriate because of the nature of the climate problem. From an economic standpoint, I would argue that discounting is appropriate, however, in earlier papers (Herzog, et al., 2003; Reilly et al., 2006; Reilly, J. and M. Asadoorian, 2007) I have pointed out that, similar to Schrag's argument that if CO_2 stabilization is the target, and to the extent a stabilization target can be associate with a well-defined carbon budget (and is largely invariant to when that carbon is emitted, and all damages are associated with exceeding the stabilization target, the following the Hotelling principle of allocation of the fixed

resource through time, a character of this the solution is the the price path of the allocated good should rise at the discount rate. Hence PC_t is replaced with PC_0e^{rt}. Then the e^{rt} is canceled by the e^{-rt}, so the discounting term falls out. Then indeed the carbon changes can be simply summed up over time. On the other hand, given EPA's social cost of carbon (SCC) calculation and application to other policy decisions, it may be appropriate to use the time profile of SCC for PC_t and the same discount rate used in that calculation. (The SCC is the present value of damages of a ton of carbon emitted at t, integrating forward, in principle to infinity.) There remain debates about whether in the SCC calculation it should be based on a reference emissions pathway, or a pathway of abatement (perhaps optimal abatement).

(3) Are some of these terms left out for some reason?

As my note above questions—what about the possible changing stock of carbon in products? As I understand EPA's argument in the case of biofuels, for example, they point to the fact that there is or will be direct consideration of the carbon in the fuel when it is burned, and so it is not a stationary source issue, which this method is designed to address. However, by clearly having this term in the equation, the scientific fact that what happens to carbon in these products is recognized, but it can be directly pointed out that this term is not being considered for reason X.

(4) Is EPA correctly measuring what matters to the climate?

The actual data and measurements that tell us what these changes in stocks are can vary. Stocks could be directly measured each year to assess changes, or changes in stocks can be inferred from flows, or both flows and changes in stocks can be measured and then compared to see how the inferred changes in stocks compare with the measured changes.

(5) Should the horizon be different than the policy horizon?

The formulation I propose turns the problem into one with a value given that we are attaching a price to carbon over time. However, implicitly at least there is a price on carbon associated with the denominator fuel to which bioenergy is being compared. So under the circumstances discussed above, where the PC_t is replaced with PC_0e^{rt}, then the e^{rt} cancels with the e^{-rt} as discussed above, and PC_0 factors outside of the integral sign in both the denominator and numerator, because it is now independent of t, and it cancels as well (assuming the same price is being applied at t=0 to the numerator as applies to the denominator. Then this just becomes a simple sum of the carbon impact over time in physical terms. So this provides an economic rational for not discounting and ultimately not attaching a specific carbon price in this calculation. If the price path cannot be described in this simple fashion or we are thinking that the policy to which this applies has a limited horizon, short of which the land, waste, and product carbon pools have achieved some steady state (which would be a reason to choose a value for T short of infinity, as someone suggested in the meeting), then the value and prices do not drop out neatly. Hence we are then stuck wondering why we are including carbon impacts beyond the policy horizon. Here, following Schrag's intervention, I think it only makes sense to

imagine that (1) what we are trying to ultimately due is to stabilize concentrations, (2) that is approximately associated with a carbon budget, and (3) the specific policy to which this is being applied is our best estimate, given our current understanding of the climate system, of society's willingness to pay for avoiding climate damages associated with exceeding the concentration target. So the PC_0 actually reflects a commitment to an optimal program of emissions control associated with some (not necessarily known) stabilization level. Dan and I discussed the idea that this omits consideration of damages that might occur short of the point of exceeding stabilization. So even though the explicit policy has some defined horizon, it really makes no sense to undertake it unless it is seen as a program that will continue in some form. In fact, if there is a constant marginal damage associated with emissions over time prior to the stabilization target, then this too factors out of the numerator and denominator when comparing the biogenic carbon to its fossil alternative (this case was also shown in Herzog et al., 2003). In fact, as long as the price path can be described as a simple function of P_0 then I believe it should cancel out. And, our ability to know the specific path of the marginal damage over time as we approach stabilization is so poor, that I can't imagine we can do better than assuming it is some simple of function of an initial price.

However, as Reilly and Asadoorian (2007) point out, this only works if there is a liability on land use carbon over time (and the carbon market is complete in space and time.) This concern was expressed by committee members in the terms: What is to assure that once the initial carbon loss that might associated with converting a natural forest or long rotation forest to a short rotation forest, the land would fall into some "sustainable" or steady state condition. In terms of a "complete" market as I mean it, a land owner would face a carbon charge for reducing carbon stocks in the near term, but then sell carbon allowances, or receive a credits if the forest partially recovered. Over the long term, if carbon neutral, the carbon charges/credits would net to zero in net present value terms. And, in a complete cap and trade system, if economic conditions changed, and the land owner further reduced carbon stocks on the land, (s)he would need to acquire allowances in a market, meaning someone else would need to reduce emissions or take up more carbon. So one doesn't need to worry if each parcel of land, or even land in toto is in some sustainable state, if additional carbon losses from land are made up for by further reductions in fossil fuel emissions. So the equation here is consistent with ecological, earth system, and economic reasoning, it is based on the idea that we can predict/project the long-term implications of an increase in bioenergy today. I guess that if we project the system assuming there is no long-term liability, then perhaps we get an accurate forecast. And if we are using a forward-looking model with perfect foresight, that system would go to some long-term steady state solution for a marginal change in biomass energy use at time t=0. The possible solutions seem to be (1) deplete the land carbon stock to zero (or very close to zero) and keep it there (2) Fall into a steady state level of carbon (more or less) than the current carbon stock (3) Biomass energy is worse than the fossil alternative and it the current steady state is maintained. I think it is then key to think of this as a marginal change. Marginal changes on top of the first change, or in

succeeding periods could well have a different carbon implication, and so the need to update this as the biomass energy market expanded.

What might these steady states look like? Suppose you had a high carbon stock forest, and it made economic sense to replace that with an annual crop, where it was possible to harvest every bit of biomass (including the roots of the crop fro energy). This would immediately get rid of the vegetation carbon in the forest, and ultimately with no residue returned to the soil would deplete all soil carbon. It seems unlikely that the carbon coefficient calculation (BAF) in this circumstance would be less than 1, and so this conversion would give no carbon value. Of course this is an extreme case, but conversion of prairie and forests to cropland as the US was settled led to an estimated 100 year or more gradual decline in carbon, that only seems to have been reversed with crop breeding and inorganic fertilizers, led to more grain and more crop residue. However, if we now harvest more of the residue, and portion we harvest would reduce the equilibrium stock of soil carbon. But harvesting some fraction of it would be associated with some new, reduced carbon stock. But assessing the new equilibrium soil carbon stock could be a very long-term process. As noted, the cropping practices adopted in the 1800's showed no sign of coming to a new equilibrium soil carbon level—soil carbon was estimate to be continuously declining into the 1950's and 1960's when practices changed (the advent of inorganic fertilizers and successful crop breeding to generate large yield increases). Of course the importance of nitrogen fertilizer in this process begs the question of why it would not be included as a GHG cost in this process. As Melillo et al. (2009) showed, the carbon debt may be temporary but the nitrogen release associated with repeated fertilization will go on forever. In the simple sum story—no discounting—a release over time is as important in 100, 1000, or 10,000 years. Hence, whatever the size of this annual GHG emission, its simple sum with an infinite time horizon is infinite, and hence the BAF is infinite (and obviously bigger than 1.0).

Other steady states could be natural forest or long rotation forest harvest to short rotation woody bioenergy crops. Most estimates suggest some reduction in equilibrium carbon—a saw-toothed carbon amount on the land, falling with each harvest and then rising as the forest grows back to harvest age. There would of course also be some underlying effect on soil carbon depending on whether more or less residue was left behind.

Still other steady states could involve land in row crops or pasture, going to perennial or woody bioenergy crops. These could be associated with an increase in equilibrium soil carbon stocks. Hence they would have a BAF of less than 0.

These examples are a source of concern for the projection process. Since the right carbon cost assignment will depend on the specific land transition from one steady state to another, and the resulting correct BAF's can range from infinite to negative we are betting heavily that any model gets this right. And, the especially concerning issue is that the BAF value is not associated with a feedstock, primarily, but rather the specific land transition. A perennial grass could be great if it displaces an annual crop, but awful if it displaced natural forest. I'm not confident of models to get this right—even the right

sign or whether the BAF is less than or greater than 1. So in principle we could be encouraging a biomass energy source that is actually worse than coal.

A small issue: Is the method based on a comparison to the fuel—biomass versus coal? Given different efficiencies of conversion to something useful—e.g. electricity the difference in conversion efficiency should also be taken into account.

I've copied in the charge questions and offer some ideas, although I think the entire method is severely flawed in terms of working effectively.

1.What criteria could be used when considering different temporal scales and the tradeoffs in choosing between them in the context of assessing the net atmospheric contribution of biogenic CO2 emissions from the production, processing, and use of biogenic material at stationary sources using a future anticipated baseline?

The correct temporal scale is an infinite horizon. The future should be discounted. The EPA is already recognizing the need for discounting in its estimates of the social cost of carbon, and so there no reason not to do that here, and to not do so would be inconsistent with the SCC, which is governing other aspects of EPA's carbon regulations. An advantage of discounting is that under some conditions the distant future becomes less important, and hence the infinite horizon can be truncated at 50, 100, or 200 years without much affect on the problem—although that depends on the special case that the value of the carbon avoidance in increasing at a slower rate than the discount rate. See Herzog, et al. 2003; Reilly et al., 2006.

a. Should the temporal scale vary by policy?

NO.

b. Should the consideration of the effects of a policy with a certain end date (policy horizon) only include emissions that occur within that specific temporal scale or should it consider emissions that occur due to changes that were made during the policy horizon but continue on past that end date (emissions horizon)?

The horizon of emissions calculation should be independent of the policy horizon. The Social Cost of Carbon calculation the EPA uses does not depend on the policy horizon—as that would be nonsensical. The same here.

c. Should calculation of the biogenic assessment factor include all future fluxes into one number applied at time of combustion (cumulative or apply an emission factor only once), or should there be a default biogenic assessment schedule of emissions to be accounted for in the period in which they occur (marginal apply emission factor each year reflecting current and past biomass usage)?

The stationary source would need to produce a periodic report (e.g. each year, a la in a trading system where they turn allowances for the previous year) that would document

the mix of biomass sources they used and the carbon coefficients of each, to assess whether/how these met whatever regulatory requirement was being enforced.

d. What considerations could be useful when evaluating the performance of a future anticipated baseline application on a retrospective basis (e.g., looking at the future anticipated baseline emissions estimates versus actual emissions ex post), particularly if evaluating potential implications for/revisions of the future anticipated baseline and alternative scenarios going forward?

We will always be dealing with a counterfactual, even in an historical assessment, because we will be forced to create a counterfactual history where the biomass energy was not used to separately identify its impact, from all the other impacts affecting land/vegetation emissions. This is a highly demanding requirement as one would need to believe that one was correctly assessing all these other factors as well-essentially accurately ascribing the historical land use changes we observed to all possible causes. You can look at it as the equivalent of ascribing climate change to all of the various human and natural contributors as well as any natural variability.

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?

If you are lucky (or more likely the model you use is simple) and you find the carbon emissions per unit of biomass is insensitive to the size of the shock then it doesn't matter. However, if you find non-linearity, non-monotonicity, or even dis-continuity in the relationship between expansion of the amount of biomass energy then there is not simple answer. I'm concerned that any model may may fails to capture the actual variability in the system, hence it would be a false result. Then you are stuck with I guess evaluating a carbon coefficient factor for every use of biomass in some small unit, approaching a derivative, in the order that biomass is used, and assigning it to each user. E.g. if I used biomass of type x early in the year, I might have lower coefficient than someone who used the same type later in the year. If you have a generic BAF for all point sources, perhaps different biomass energy types, then maybe high variability and discontinuities will average out, and even though the factor will not apply well to any one point source, it may get it right on average. But the inability of this to pick out actually low BAF from actually high BAF sources is, of course, a measure of the inefficiency of using an average BAF and applying it without checking.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?

See above.

b. What should the general increment of the shock be? Should it be specified in tons, or as a percentage increase?

See above—you are trying to approximate a derivate at every point of system response that is likely not continuous, smooth, or monotonic. If the model is deterministic then you should be able to use a small number, and though the effect is small, one should be able to pick out, unless it is so small that it is below the numeric precision of the computer or, if an optimization routine, the tolerance limit of the optimization procedure. I don't know that it makes much difference in tons or percentage.

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)?

Ideally each feedstock and each unit would be computed given the assumption a model state of the world where all previous units were in the new baseline. So the baseline would change with each new unit. Some tests could be performed to see if this made a much of a difference or not.

d. Should shocks for different feedstocks be implemented in isolation (separate model runs), in aggregate (e.g., across the board increase in biomass usage endogenously allocated by the model across feedstocks), or something in between (e.g., separately model agriculture –derived and forest-derived feedstocks, but endogenously allocate within each category)?

You would need to do it for each increment, in order that they occurred, with the previous increments added to the baseline.

e. For feedstocks that are produced as part of a joint production function, how should the shocks be implemented? (e.g.,a general increase in all jointly produced products; or, a change in the relative prices of the jointly produced products leading to increased use of the feedstock, and decreased production of some other jointly produced products, but not necessarily an overall increase in production).

A change in the demand for the feedstock, with the model presumeably accurately estimating what would happen to the price of joint produced products, their fate, and implications for other products with which these joint products competed.

f. How should scale of the policy be considered, particularly for default factors? (e.g.,can a single set of default factors be applied to policies that lead to substantially different increases in feedstock usage)?

As above, I doubt it, but this can be tested at least in models. Whether it applies in the real world is a deeper question.

g. Would the answers to any of the above questions differ when generating policy neutral default factors, versus generating factors directly tied to a specific policy?

Different policies would bring forth different combinations of feedstocks at different times and in different orders. If it turns outs results are sensitive to the order in which feedstocks are used, or what mix are used in the baseline against which a new shock is evaluated, then pretty clearly the BAF will be policy dependent.

h. What considerations could be useful when evaluating the performance of the demand shock choice ex post, particularly if evaluating potential implications for/revisions of the future anticipated baseline and alternative scenarios going forward?

See my general answer under 2. Essentially the model would need to be tuned to closely describe the state of the world in which the actual shock occurred, and then one could evaluate whether the model predicted correctly the carbon implications of a shock given that all other variables in the model accurately reflected the state in which the shock occurred. This could then tell you something about whether the mechanisms in the model predicting land use change were "realistic". The question is what time frame are you evaluating. If these effects play out over years, decades, or through infinity, then you have a long time to wait to evaluate the full model response against a "real" shock. Further, if the model did not fully predict the reality in all other variables, and they had to be tuned, even if after tuning it predicted perfectly, you can't conclude the BAF it might produce would be accurate, because you have not verified that the model is predicting all other variables correctly that affect how it responds to a shock. You can hope that any affect is so isolated from other variables, that they don't matter. You can test this by calculating BAFs from widely different initial conditions/states of the world.

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Comments from Dr. Sedjo

Roger Sedjo's Comments on the SAB questions 2 a), b) and c): These remain the same as submitted earlier.

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 resources 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?

Responses (Sedjo): The devil is in the detail of the model used and the question being addressed. I don't think any single answer is always correct for any of the above. We tend to use annual increments (shocks) with demand shifting out some amount annually and the supply system responding. In this context the annual is viewed as a proxy for the marginal or incremental impact. However, when a forward looking optimal control approach is used, the response of supply in any one year depends not only that year's change in demand but the entire intertemporal set of demand changes. We tend to use percent but a volume change could work if the model was calibrated that way.

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.

Comments from Dr. Skog

Charge Question 1 a.

1. What criteria could be used when considering different temporal scales and the tradeoffs in choosing between them in the context of assessing the net atmospheric contribution of biogenic CO_2 emissions from the production, processing, and use of biogenic material at stationary sources using a future anticipated baseline?*

Response:

Choosing how many years of land carbon difference to include in computing BAF should be determined only by our uncertainty in being able to estimate more distant land carbon changes associated with biogenic emissions. But to take this focus we need to modify how BAF is computed using initial emissions and year by year land carbon changes (or avoided emissions). In the text that follows land carbon difference refers to the difference between an alternate scenario and a business as usual scenario at specific times in the future. Land carbon change refers the change in this difference year to year in a projection.

The Framework's Biogenic Assessment Factor (BAF) is the ratio B/A where A is the CO2 in the initial emission and B is the net CO2 emission after adjusting for the net difference in land C or avoided emissions at time T. BAF is not influenced by the extra amount of CO2 that is in the atmosphere between time 0 and time T. As a result we can feel pressed to select a time T that is just a relatively few years away to capture the effect of this additional CO2 in the near term.

There is an alternative way to calculate BAF that converts an initial biogenic emission of CO2 into a net CO2 equivalent emission that represents the net radiative forcing over the entire time period to 100 years including the initial emission and the year by year carbon changes on the land. The metric could include land carbon changes out 100 years but inclusion would be limited or modified by our uncertainty in the land carbon changes.

The revised BAF would be the ratio B/A to convert the initial biogenic CO2 emission (A) to the net CO2 equivalent emission (B) considering land C change each year of the projection. The net CO2 equivalent is the radiative forcing out 100 years for the initial emission and the additional or avoided radiative forcing associated with land C change for each year out to 100 years or other limit of based on uncertainty in future land C change.

This net CO2 equivalent is identical to the metric used to report GHG emissions in the EPA Inventory of U.S. greenhouse gas emissions and sinks each year under the UNFCCC.

Without a BAF that converts a biogenic CO2 emission to this net CO2 equivalent (global warming potential to 100 years) it would not be possible to compare and judge BAF adjusted CO2 emissions to the emissions for other sources in the U.S. economy. Nor would it be possible to compare biogenic energy mitigation benefit to mitigation efforts that conserve or otherwise avoid fossils CO2 emissions.

Pages ES-2 and ES-3 of the Draft Inventory of U.S. greenhouse gas emissions and sinks: 1990-2013 (USEPA 2015) indicates:

> "The IPCC developed the Global Warming Potential (GWP) concept to compare the ability of each greenhouse gas to trap heat in the atmosphere relative to another gas."
>
> "The GWP of a greenhouse gas is defined as the ratio of the time-integrated radiative forcing from the instantaneous release of 1 kilogram (kg) of a trace substance relative to that of 1 kg of a reference gas (IPCC 2013)." … "The reference gas used is CO2, and therefore GWP- weighted emissions are measured in million metric tons of CO2 equivalent (MMT CO2 Eq.)."

Table ES-1 indicates global warming potentials with at 100 year time horizon are used in the report.

There is no discounting of the radiative forcing in computing the global warming potentials. Although the issue is discussed in the IPCC 4th Assessment report they settle on a metric without time weighting. The IPCC 2006 Guidelines for National Greenhouse Gas Inventories also calls countries to report GHGs in tons CO2 equivalent that represent CO2 radiative forcing over 100 years - unweighted.

The radiative forcing of an initial emission of a GHG declines over time (e.g. IPCC 2007, 4th Assessment report, Physical Science Basis, pg 213, footnote a). The path of decrease has a degree of certainty based on broad scale global processes.

However there is a key difference between computation of the GWP-100 for a single emission and BAF(GWP100) for a biogenic emission. The difference is that for biogenic emissions the follow-on net land carbon changes and associated net radiative forcing may be notably less certain than the declining radiative forcing path associated with a single CO2 emission.

Just as the EPA GHG Inventory of Emissions and Sinks recognizes that different greenhouse gases have different radiative forcing profiles –we could think of biogenic CO2 emissions (from various feedstocks and regions) conceptually as kinds of greenhouse gases with their own radiative forcing profile which has uncertainty as a feature.

For each biogenic feedstock type – logging residue, roundwood, mill residue, agricultural crops, agricultural residue and municipal solid waste the land carbon changes (or avoided emissions) between two scenarios can occur over a few years or many years. The BAF(GWP100) could include such changes out to 100 years while using some method to account for and mitigate uncertainty.

Some initial ideas for accounting for uncertainty include 1) doing a sensitivity analysis changing projection parameters and drivers to project land carbon changes (or avoided emissions) out as many years as feasible by models (how robust are estimated BAF(GWP100) values?), 2) If sensitivity tests show notable variation for BAF(GWP100) with long time horizons consider shorter horizons that have lower variation in BAF(GWP100), 3) consider the time frame of expected effectiveness of land

management policies that will influence maintenance of land C – e.g. policies for sustainable management, 3) consider using BAF(GWP100) averages (for projected change in emissions over many years) that can by their nature be corrected over time and thereby mitigate uncertainty over time. Note that these are all focused on addressing uncertainty in future land carbon changes (avoided emssions). We do not need to select a short time frame to be sure near term CO2 effects are included.

An example calculation where avoided net land C fluxes are "certain" - Estimating BAF(GWP100) for logging residues

Consider estimating BAF(GWP-100) for logging residue assuming demand for logging residue has little impact on indirect land use change. The time path of avoided emissions is relatively certain based on empirical studies of wood decay in forests. BAF(GWP100) for a 1 ton CO2 emission from residue is the net radiative forcing for a time 0 emission and the avoided emissions out to 100 years after the initial emission divided by the 100 year radiative forcing of a 1 ton emission of CO2 emission alone.

Cherubini (2011) shows equations (1 - 3) for computing a BAF(GWP100) that includes the radiative forcing for a period of biogenic emissions (or a single initial year) minus the radiative forcing from change in forest growth or from avoided logging residue emissions (or other avoided wood residue emissions).

Using a CO2 radiative forcing curve from the IPCC 4th Physical Science Basis assessment report (IPCC 2007) the BAF(GWP100) values for burning logging residue that decays at rates in Table L-15 (of Appendix L) are shown in Table 2.

For example, for softwood logging residue in the South East and South Central U.S. the decay rate is 0.057. For a 1 ton CO2 emission from logging residue in year 0 the net radiative forcing from the initial emission and each year's avoided decay emission is 0.18 ton CO2 equivalent. It is equal to the radiative forcing caused by a 0.18 ton CO2 emission at time 0 alone. BAF(GWP100) = 0.18 (Table 2). Table 2 shows an estimate of an index value for the radiative forcing over 100 years of an initial emission (=48.5) and a the net radiative forcing index after deduction for the avoided radiative forcing of avoided decay (=8.9). The ratio of the net to initial radiative forcing is 0.18.

The BAF(GWP100) will result in value greater than 0 to account the net positive radiative forcing that occurs over time as the avoided decay would occur. Unlike the current BAF that uses beginning CO2 emission and net CO2 emission at a time T there is no need to decide what T to use.

Table L-15. FASOM-GHG Annual Coarse Woody Debris Decomposition Rates.

Forest	FASOM-GHG Region								
	CB	LS	NE	RM	PSW	PNW	PNWE	SC	SE

Softwood	0.048	0.048	0.053	0.02	0.023	0.027	0.027	0.057	0.057
Hardwood	0.084	0.084	0.069	0.082	0.082	0.082	0.082	0.082	0.082

Table 1 – BAF(GWP100) values for logging residue (assuming no effects other than Avoided Emissions)

Forest	FASOM-GHG Region								
	CB	LS	NE	RM	PSW	PNW	PNWE	SC	SE
Softwood	0.22	0.22	0.20	0.46	0.42	0.37	0.37	0.18	0.18
Hardwood	0.12	0.12	0.15	0.12	0.12	0.12	0.12	0.12	0.12

a. Should the temporal scale for computing biogenic assessment factors vary by policy (e.g., near-term policies with a 10-15 year policy horizon[8] vs mid-term policies or goals with a 30-50 year policy horizon vs long-term climate goals with a 100+ year time horizon), feedstocks (e.g., long rotation vs annual/short-rotation feedstocks), landscape conditions, and/or other metrics? It is important to acknowledge that if temporal scales vary by policy, feedstock or landscape conditions, or other factors, it may restrict the ability to compare estimates/results across different policies or different feedstock types, or to evaluate the effects across all feedstock groups simultaneously.

Response: *The BAF values should be computed using radiative forcing of emissions and land C fluxes (or avoided emissions) where radiative forcing for the emission and each year to year flux extends out to 100 years after the initial emission. However this does not mean that we necessarily consider all land carbon changes out to 100 years. We consider the uncertainty in projected land carbon changes (or avoided emissions) for each feedstock to determine how they are included. The decision about how to adjust for uncertainty would include steps to mitigate the risk of over or under estimating BAF(GWP100) (e.g. choice of BAF(GWP100) over a range estimated). It seems the risk assessment and adjustment for uncertainty could logically be the same for all policies so the potential effectiveness of policies could be compared to one another. Alternately there could be a conscious decision for some policies to be more averse to the possibility of underestimating (or overestimating) a BAF(GWP100) which would influence the assignment of BAF(GWP100) from the range of possible values.*

i. If temporal scales for computing biogenic assessment factors vary by policy, how should emissions that are covered by multiple policies be treated (e.g., emissions may be covered both by a short-term policy, and a long-term national emissions goal)? What goals/criteria might support choices between shorter and longer temporal scales?

See response above

ii. Similarly, if temporal scales vary by feedstock or landscape conditions, what goals/criteria might support choices between shorter and longer temporal scales for these metrics?

Response: *Although BAF values should be computed in a consistent way using radiative forcing and judgments about uncertainty in land C changes, there are still choices about computing BAFs for single biomass types and single regions versus computing BAFs for aggregates of biomass types and /or regions. But the choice to aggregate biomass types and regions or not would not influence the time frames used to compute BAFs.*

iii. Would the criteria for considering different temporal scales and the related tradeoffs differ when generating policy neutral default biogenic assessment factors versus crafting policy specific biogenic assessment factors?

Response: *Possibly not. It may not be possible to anticipate all policy situations but it seems the guidance for selecting time frames for BAF(GWP100) could be the same for neutral BAF(GWP100) as for specific policies. In principle it seems all policies would be seeking to modify the amount of CO2 equivalent emissions. The guidelines for BAF(GWP100) serve to quantify the level of CO2 equivalent emissions that would be caused by various feedstocks. If we cannot compare across policies using a common metric how do we know if our choice among policies will give a better outcome than alternatives in terms of effect on the atmosphere?*

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USEPA. 2015. Draft Inventory of greenhouse gas emissions and sinks: 1990-2013. http://www.epa.gov/climatechange/pdfs/usinventoryreport/US-GHG-Inventory-2015-Chapter-Executive-Summary.pdf

Table 2 – Example estimate of BAF(GWP100) for logging residue in the South East and South Central U.S.

This sheet estimates the time zero CO2 emmission equivalent (tons) associated with one ton wood energy CO2 emission taking into account the difference in land carbon due to use of wood from forest thinnings. The

	CO2 radiative forcing response function parameters				
	Source : IPCC 4th Assessment report - Physical Science Basis p 231 footnote a				
ao	0.217				
a1	0.259	T1	172.9	yrs	
a2	0.338	T2	18.51	yrs	
a3	0.186	T3	1.186	yrs	

Forest and Non-Forest Carbon Deficit Function Parameters
Residues and Thinnings

	Decay Rate Half Life, years	k
Biomass from Logging slash	20	**0.057**

For wood biomass from logging slash
Tons Time zero CO2 emission equivalent per ton of biomass CO2 emission
0.18

Table - Radiative forcing (index) of initial emission (col B) and radiative forcing additions (col C) and reductions (to year 100) due to difference in land carbon with use of wood thinnings for energ

	Year	0	1	2	3	4	5	6	7	8	9	10
		1	2	3	4	5	6	7	8	9	10	1
		0.94	0.89	0.84	0.80	0.75	0.71	0.67	0.63	0.60	0.57	0.5
Time (yrs)	C02 (kg)											
0	1.00000	-0.055										
1	0.87477	-0.048	-0.052									
2	0.81085	-0.045	-0.046	-0.049								
97	0.36658	-0.020	-0.019	-0.018	-0.017	-0.016	-0.015	-0.015	-0.014	-0.013	-0.012	-0.012
98	0.36564	-0.020	-0.019	-0.018	-0.017	-0.016	-0.015	-0.015	-0.014	-0.013	-0.012	-0.012
99	0.36470	-0.020	-0.019	-0.018	-0.017	-0.016	-0.015	-0.015	-0.014	-0.013	-0.012	-0.012
100	0.36377	-0.020	-0.019	-0.018	-0.017	-0.016	-0.015	-0.015	-0.014	-0.013	-0.012	-0.012
Radiative forcing index of initial emission	**48.51247**											

Charge Question 1 d.

1 d. What considerations could be useful when evaluating the performance of a future anticipated baseline application on a retrospective basis (e.g., looking at the future anticipated baseline emissions estimates versus actual emissions ex post), particularly if evaluating potential implications for/revisions of the future anticipated baseline and alternative scenarios going forward?

Response:

The procedures to use in evaluating the performance of anticipated baselines and alternate cases to estimate BAF – ex anti and ex post – could vary by feedstock type.

The procedures may also vary depending on the type of BAF metric estimated. It may not be realistic or appropriate to compute certain BAF metrics for certain feedstock types.

Duration of biogenic emission increase	Extent of biogenic emission increase	
	Marginal increase	Increase from zero baseline
Single year	Practical for municipal solid waste and pulp liquor Not practical for forest or agricultural feedstock where time series of increases must be evaluated to get market induced land C change (leakage)	Not certain how zero baseline counter factual would differ from marginal increase or if zero baseline would be highly uncertain
Multiple years or duration of projections Average BAF is ratio: cumulative net emissions / cumulative biogenic emissions	Appropriate for forest and agricultural feedstocks where the time sequence of biogenic emissions collectively determines the time sequence of land C change	Appropriate for forest and agricultural feedstocks where the time sequence of biogenic emissions collectively determines the time sequence of land C change Possibly greater uncertainty than for marginal increase

Biogenic emissions from municipal solid waste and pulp liquor

The estimates of BAF for (A) biogenic material from municipal solid wastes and pulp liquor sources differ from estimates of BAF for (B) forest and agriculture sources in that the former may be computed comparing alternate and baseline cases where market prices do not influence carbon stored on land.

The uncertainties in BAF from category (A) are from the uncertainty in selecting counter factual (BAU) cases that reflect the real world and the emissions from biophysical processes for alternate and counterfactual cases. It is not clear if a zero baseline case would differ from a marginal case.

The marginal BAF estimates for category A could be evaluated periodically, (e.g. each 5 years) to determine if 1) models of biophysical processes for the alternate and counterfactual cases are validated by data from additional study and monitoring of such processes, 2) need adjustment so the counterfactual case represents that realistic alternatives to the alternate case. For example, if municipal solid waste is not burned for energy, is the alternative still landfills and is the landfill methane capture rate as in the original counterfactual case?

Biogenic emissions from forest and agricultural biomass

For biogenic emissions from forest and agricultural biomass the uncertainty to be estimated ex anti and mitigated ex anti, and to be evaluated and mitigated ex post is not the uncertainty in net carbon emissions over time for a single projection of biomass use.

It is the uncertainty in the ratio of net emission increase to biogenic emission increase between a baseline (business as usual) projection and an alternate projection.

It is possible that we may be quite uncertain if the projection for a single scenario will predict the future. But it may be that the change in net carbon emissions per unit increase in biogenic emissions may be relatively robust (not vary much) across variation in assumptions in projections of macro-economic drivers and elasticities of supply and demand in projection models. For example the net emissions per unit of biogenic emissions shown in figures K-8 and K-12 (Appendix K) computed for a several alternate and base case combinations show relatively little variation for the longer time periods. If ex anti projections are not robust there must be implicit or explicit measures to account for uncertainty. But even robust ex-anti predictions will need adjustments after actual macro-economic drivers and land carbon trends are known.

For forestry and agricultural biomass sources there will likely be biomass use and market induced land carbon change in early years that will affect land conditions in future years. Also the effect of future biogenic emissions on land carbon can differ depending on the biogenic emissions in prior years. Because of these links over time, an average BAF is needed that uses the ratio of net carbon emissions increase over many years to biogenic emissions increase over many years (as done for examples in Appendix L). It is not possible to discern the effect of a particular year's emission on net emissions over time without including an assumption about future year emissions. This ratio may be for a marginal increase or an increase from a zero baseline. It seems justified to use average cumulative BAFs over some time period as shown in tables L-3 to L-8.

It also seems justified to seek estimates by feedstock and by region. This level of detail is needed in the case where policies would want stationary sources to discriminate between feedstock sources with lower and higher BAF values. It may be possible to make estimates for feedstocks for more local conditions only if it is realistic to separately estimate the direct land effects of biomass use and the indirect land effects including the effects of leakage of product production on land carbon.

Ex anti evaluation and mitigation of uncertainty

The primary ex anti evaluation would be to assess the uncertainty in BAF values due to uncertainty in model drivers, structure and parameters. The most straight forward evaluation would be to see the variation in BAFs from a series of sensitivity tests – medium, high and low values for model drivers and changes in parameters (within the range of uncertainty for those parameters). In principle monti carlo simulations could be done but are likely not practical given the complexity of models. If the variation is limited to moderate, it may be realistic to take a median value of estimated BAF, or the BAF generated by the median set of drivers and parameters.

An addition to this approach could be the use of two or models to make an ensemble forecast. Literature suggests ensemble forecasts can have a lower error of estimate (Clemen 1989).

Another/additional approach to ex anti evaluation and mitigation of uncertainty – for discussion on practicality and effectiveness. Premise 1 – given the uncertainty of modeling results to estimate average BAF over an extended time it may not be realistic to assume we have single digit significance for estimates. It may be more realistic to assume we can place a BAF in a category such as 0±0.25; 0.5±0.25; 1±0.25 (+ other categories if needed). Premise 2 – We may be better able to assess uncertainty and decide how to place the BAF for a feedstock in a category by separately estimating 1) the BAF based on direct land carbon effect of use of a biogenic feedstock and 2) estimating how indirect effects (leakage) would change the initial assignment from step 1.

This would only be practical if these modeling steps – to assess the effect of increased feedstock use in a region - could be separated. Issues associated with this idea are discussed in the Annex below.

Ex post evaluation and mitigation of uncertainty

To do a retrospective test of projections and a BAF estimate we are initially bound to incorporate certain same side assumptions. As an additional step we need to check to see if the side assumptions have been met and further correct the BAF estimate.

For example, specific questions asked and answered in Appendix L are

> For pairs of cases comparing Southeast softwood roundwood use for biomass marginally higher than AEO productions vs AEO projections, or comparing SE softwood roundwood use in the AEO case vs no roundwood use what would be the ratio of the difference in cumulative net emissions to the difference in cumulative biogenic emissions from 2015 to 2060?

These questions require a side assumption about fixed projections of other forest or agricultural biomass use for both the high and low roundwood cases. These BAFs are assuming either 1) there will be no change other roundwood used by stationary sources over the projection period or 2) the BAF estimate is independent from in the changes in use of other biomass feedstocks.

Appendix L gives several estimates of the "Landscape factor" part of BAF.

Proposition – It is best to use a BAF which uses the ratio of net cumulative emissions to cumulative biogenic emissions over a lengthy time such as 50 years because 1) it accounts for the effect of both early emissions and later emissions on overall land carbon change and 2) it can be corrected over time such that it can match the best possible estimate in 50 years.

The analyses for Appendices K and L indicate that the scenario to scenario difference in land C fluxes can vary notably year to year. It seems unrealistic to use fluctuating year to year BAF values for stationary sources. Assessments done approximately each 5 to 10 years could modify the 50 year average BAF values as discussed below.

Assessment/ validation step 1. Of the two projections needed to estimate BAF for higher roundwood use in the Southeast (for either the marginal or zero baseline BAFs)– it is

intended that the high wood case should be an approximation of what may actually occur over time.

A. For a retrospective assessment, in say 2025, first rerun the model for the With Wood Case using actual macro-economic drivers for the 15-year historic period and revised projections of macro-economic drivers out to 2060.

B. Second, compare revised high wood case projections of production and trade of major commodities, and carbon stocks and fluxes for categories in Table L-1 to data available for the 15 year period. Wood demand would still be exogenous.

C. Third, modify the model so the revised projections of the high wood case better track actual data from the last 15 years – for variables under B. Perfect tracking is likely not possible.

D. Fourth, run the revised model, starting 15 years ago, for the low wood case.

E. Fifth, use the projections (starting 15 years ago) of the high wood case to the low wood case and compute a revised value for BAF. Compare the new 50 cumulative average BAF values over time to the BAF values (over time) estimated 15 years prior. Note the old and revised BAF values are for the original 50 yr period.

<u>Assessment/ validation step 2 - Consistency check with assumptions.</u> For the 15 year historic time period is there consistency with the assumptions? Were the assumptions (projections) of other feedstock use (in each region) correct? If the side assumptions did not hold, for example if there difference in projected agricultural biomass use in the region or other regions then the lower and higher wood cases would need to be further adjusted along with adjustments to projections in non roundwood use. The 50 year average BAF would be further revised.

<u>Correcting the BAF after each assessment</u>. The best (most accurate) estimate possible of average BAF for roundwood over the 50-year period would be one that is made after the 50-year period has passed. The estimate would use a higher wood use case that matches history and a low wood case with all the same historic drivers except lower roundwood use for energy. Prior to 2060, every 5-10 years, we can make successively closer estimates, and corrections, where the BAF values assigned collectively over time would eventually average to that best possible 50-year average estimate, made just after 2060.

At the end of, say, 15 years, in 2025, a new 50 year BAF(2025) estimate, still to 2060, could be compared to the original BAF(2015) . BAF(2025) could be modified to correct for over or under predictions of differences in land carbon flux from the first 15 years. The land carbon flux correction could potentially be spread over time to 2060. The revised BAF(2025) would be used through 2035.

At some point, e.g. BAF(2035), there could be new projections for 50 years to 2085. These projections would estimate average BAF for the period 2035 to 2085.

Both the revised BAF(2035) from the initial 50 year projection and the new BAF for the second 50 year period could be considered in assigning a BAF in the period 2035 to 2045.

It seems there could be an emphasis on assigning successive BAFs values that over time would match the final Best BAF computed for the first 50-year projection.

There may be a more rigorous method to define adjustments for successive decadal BAF values that are contributing toward several successive 50-year best estimates simultaneously.

Tradeoffs between individual BAFs by biomass type and region vs BAFs for aggregates of biomass types by region - text for discussion

The retrospective assessments of BAF values for individual types of biomass require corrections in assumptions about historic and projected use of alternate biomass types. If notable assumption corrections are needed at the time of an assessment that could require a notable correction in BAFs for individual biomass sources.

The extent of correction needed by unmet assumptions might be reduced by using BAF values for aggregates of biomass types. However the BAFs for aggregates of biomass types would be based on uncertain projections of the mix of biomass types and corrections of the mix projections could also require notable but possibly less adjustment in the BAF for aggregates than for BAFs of individual biomass sources.

Stationary sources will have more control over their average BAF of biomass with use of individual BAFs.

If regional surveys could provide data on mix of biomass sources used for assessments, the use of BAFs for aggregates may reduce reporting burden on stationary sources.

If violation of the biomass use assumption for individual BAFs is just as likely to cause notable adjustment in BAFs and the uncertainty on projected mix of biomass types then the tradeoff between individual vs aggregate BAFs would seem to come down to another choice. What is the benefit of high flexibility in controlling stationary source average BAF versus possible higher burden in collecting reporting details of types of biomass used?

Table 1 – Features of BAF retrospective assessments - record keeping, assumptions, corrections – by degree of feedstock aggregation

	BAF estimate for each biomass type and region	Two BAF estimates per region, forest-based wood & ag residue/crops	One BAF estimate covering all biomass sources
Record keeping for Stationary Source (SS) to assign BAF	Weight of each type of biomass having a BAF	Weight of 1) forest-based wood & 2) ag residue/crops	Total biomass weight
Data needed for retrospective assessment	Weight of each type of biomass	Weight of each type of biomass used by	Weight of each type of biomass

(from stationary source or resource surveys)	used by stationary sources in a region	stationary sources in a region	used by stationary sources in a region
SS flexibility in changing Average BAF (by changing types of biomass used)	High	Limited	None
BAF Assumptions needing to be checked in Assessment/validation/ revision	Each BAF estimate assumes other biomass uses are zero (average BAF) or does not increase (marginal BAF)	Each of two BAF estimates assumes other biomass use is zero (average BAF) or does not increase (marginal BAF)	None
Key sources of uncertainty in BAF estimates - cause for revisions during assessment	Uncertainty in meeting assumptions about use of alternate biomass sources	Mixed uncertainty – market based mix within aggregates, alternate use levels between aggregates	Uncertainty in prediction of mix of biomass sources that will be used
Post Assessment BAF adjustments – potential magnitude	Highest?	Medium	Lowest?

References

Clemen, R. T. 1989. Combining forecasts: A review and annotated bibliography. International J. of forecasting 5:559-583.

Annex – Another/ additional approach to ex anti evaluation and mitigation of uncertainty – for discussion on practicality and effectiveness.

Estimating direct land carbon change from increased biomass use and initial BAF category – by feedstock and region

Mark and Bob can you offer comments on how this might be done?

Estimating indirect land carbon effects from increase biomass use – by feedstock and region?

Bob can you offer comments on how this might be done?

Using information on indirect land carbon effects to change initial assignment to BAF categories

Bob do you have any rational for doing this?

Comments from Dr. West

The response to assigned charge question 1C has been revised to reflect comments from other panelists on this topic. The revised response is below. Additionally, I have included a section for general framework comments and comments to topics that were outside the scope of assigned charge questions.

Charge Question 1C
Should calculation of the biogenic assessment factor include all future fluxes into one number applied at time of combustion (cumulative – or apply an emissions factor only once), or should there be a default biogenic assessment schedule of emissions to be accounted for in the period in which they occur (marginal – apply emissions factor each year reflecting current and past biomass usage)?

Response to Charge Question 1C
A one-time cumulative emissions factor could reasonably be used to meet the objectives of the Biogenic Framework to estimate net biogenic emissions associated with the combustion per unit of biomass. Annual reassessment of regional trends could be used to update the Biogenic Assessment Factor (BAF). In this way, the BAF or the relative BAF (i.e., relative to an anticipated future baseline) could be updated annually just as emissions factors for fossil fuels are updated annually by DOE EIA. Since the BAF will likely be updated regularly to reflect changing landscape dynamics, it is all the more important that a one-time cumulative emissions factor be used per unit of biomass at the time of combustion.

A default schedule of incremental net emissions (i.e., initial release of carbon and subsequent annual increments of carbon uptake), a suggested option in the draft Framework, may help track annual net emissions from stationary sources over time. Use of an incremental or marginal BAF, in theory, would provide a greater incentive to ensure forest regrowth following harvest. However, this appears to be contrary to the main objective of the EPA Biogenic Framework, which is to estimate total net emissions associated per unit of biomass in a manner that is consistent with fossil fuel emissions. Also, while this
could work at a project level, questions were raised as to whether this could work at a marginal BAF could be implemented at a national level. Any program established that would require plot-level confirmation of regrowth would require more intense land monitoring through high resolution geospatial modeling using remote sensing or third party verifiers on the ground.

In summary, it is not recommended that the Framework be used to incrementally estimate or record net emissions into the future associated with stationary sources. Instead, it is recommended that a one-time cumulative emissions factor or BAF be applied per unit of biomass at the time of use or combustion. The BAF would be feedstock specific and would be updated over time based on landscape level changes in land cover and carbon

stocks. Future revisions to the BAF based on changing growth-to-harvest ratios, along with potential changes in the policy horizon, could be used to promote long-term sustainable management of land resources.

General comments on framework

There are many trade-offs in developing a framework to estimate net emissions from biogenic carbon. The current framework attempts to account for all stock changes and fluxes that influence net biogenic carbon emissions. The framework also identifies three primary issues for discussion: spatial boundary for accounting, temporal boundary for accounting, and use of a reference point versus anticipated reference line for comparing actual land use change to a business-as-usual scenario. Comments are provided below on each of these three issues.

Accounting over space
In theory, accounting for harvested biomass and regrowth over time at the plot-level or within a specified radius around a user facility would be optimum. However, indirect land-use change that occurs outside of the specified land area and that is associated with harvested biomass inside the specified radius can be substantial, particularly at the national scale and driven by national policies. Therefore, the general consensus was that a system needs to be established that considers land-use change and carbon dynamics occurring nationally. While there currently are national systems in place (e.g., US Greenhouse Gas Inventory), these systems do not attribute emissions specifically to the stationary sources. This reasoning is the basis for comparison of harvest and regrowth rates within multi-state regions in the US to develop BAF values. The BAF values represent net emissions at the regional scale, not at the plot- or facility-level. As such, while a region may be managed sustainably, there is no guarantee under this framework that a specific plot will be managed sustainably or that a plot of harvested trees will regrow and reach its initial carbon stock. While there are obvious trade-offs in conducting the accounting in smaller tangible areas versus multi-state regions, the regional approach taken by EPA appears to be the most logical and effective choice. This choice has direct implications on the time horizons and the reference point options, which are discussed below.

Accounting over time
The framework suggests the use of an 'emissions time horizon' and a 'policy time horizon'. The emissions horizon represents the time over which harvest and regrowth occurs. For a mature forest, this could be over 100 years. The policy time horizon represents the time period over which accounting occurs. It is important to base the BAF on the appropriate emissions time horizon, but also enable adjustment of the BAF based on a policy horizon. The ability for policy to manage or account for regrowth or indirect land-use change past about 20 years is very limited. Some might suggest that anything past 10 years is generally unreasonable, given how policy changes based on new

information and new perspectives. Additionally, there is concern regarding the amount of emissions in the first 1-10 years that may occur, and additional concerns regarding non-carbon related environmental impacts. As such, a separate policy mechanism within the accounting framework may be reasonable. In this case, the policy time horizon could be adjusted to reflect uncertainty in future regrowth of carbon or discounting of future carbon.

Reference point versus anticipated baseline
Once a multi-state regional approach is chosen, the use of an anticipated baseline is nearly mandatory. A regional approach results in negative BAF values, because growth in some regions is greater than harvest. However, the application of a negative BAF value to a unit of biomass does not well represent the dynamics of a tree or forest stand. To remedy this issue, use of an anticipated baseline does a better job of representing the marginal change in carbon stocks that are occurring due to changes in policy. The anticipated baseline also introduces increased uncertainty, but it is nonetheless important to get a more realistic assessment of landscape dynamics.

<u>Topical comments</u>

Full GHG accounting
The biogenic carbon framework is intended to assess net emissions associated with the harvested biogenic biomass. The purpose is to obtain an estimate of net emissions associated with the combusted biomass, and do so in a manner that is consistent with fossil fuel emissions. Fossil fuel emissions do not incorporate a life cycle analysis (LCA) that considers the energy and emissions associated with mining, transport, infrastructure, or fuel loss during processing and transport. It is therefore logical that an LCA is not conducted to estimate net emissions associated with biogenic carbon. However, EPA is encouraged to conduct LCA for all fuel types and conduct comparisons between fossil (geogenic) and biogenic fuels to ensure that there are not perverse incentives being developed. This can be done regardless of whether LCA is incorporated into the biogenic framework.

Policy versus science
EPA has chosen to maintain a focus on science-driven accounting of net emissions associated with biogenic carbon. I think this is appropriate and that it is logistically possible for EPA to develop a BAF based on current scientific knowledge. The EPA has been transparent when considering, for example, a policy time horizon in replace of or in addition to an emissions time horizon.

Comments from Drs. Woodbury and Buford

Response to Assigned Charge Questions (Woodbury & Buford)

These responses were written by Peter Woodbury and Marilyn Buford after reviewing draft preliminary responses from all other reviewers and reviewing notes from the meeting held 25-26 March 2015. However, other reviewers have not yet seen these revised responses.

Question 2d [Woodbury, Buford]. See general comments, particularly #1, 2, and 5. Shocks for different feedstocks should probably be analyzed together, but this choice may depend on the policy context, for example, whether there would be mandates or incentives for specific types of bioenergy production and/or feedstocks.

Question 2e [Woodbury, Buford]. See general comments, particularly #1, 2, and 5. The choice of how to handle joint production functions may depend on the specific policy context, for example, whether there would be mandates or incentives for specific types of bioenergy production and/or feedstocks. If one of the co-products has higher market value, such as roundwood compared to wood residue, it is likely that harvest rates are driven primarily by the higher valued co-product.

Comments from Dr. Woodbury

Overall Comments

(Topic: Policy context). The Framework document should acknowledge explicitly that the purpose is to assess global warming potential (GWP). Such acknowledgement is important because it will provide a rationale for the accounting framework. This would not make the document a "policy" document, but instead would provide needed clarity of the purpose for an accounting framework for CO_2 emissions. For example, it would clarify that other greenhouse gases such as nitrous oxide (N_2O) and methane (CH_4) are very important sources of GWP for some bioenergy feedstocks, whether or not they are specifically being regulated. There is some investigation of the importance of N_2O in Appendix M, but the example (corn stover) is not very relevant for N_2O impacts: for example, much larger impacts would occur for corn grain. The EPA responded to this same issue in responding to previous SAB comments (Page 13), but still provided no rationale other than GWP for accounting for net CO_2 emissions.

(Topic: Policy context). The framework should include key aspects of the life cycle greenhouse gas emissions for biogenic feedstocks (see comment #1).

(Topic: Policy context). The Framework document acknowledges in many places that many decisions involved in the framework will depend on the specific purpose and policy context in which it is used. The SAB panel should not endorse or recommend the use of the framework for any policy context at this time. Instead, once a specific policy context is selected, the EPA should ask the SAB panel to review the framework for use in that specific context.

(Topic: Time scales). Analysis and discussion of time scales in the Framework should include at least 20-year, 50-year, and 100-year time scales. The 20 and 100-year analyses correspond to two of the time periods commonly used for assessing global warming potential, and the 50-year time scale represents an intermediate time scale relevant, for example, for long-rotation biomass feedstocks.

(Topic: Overall approach). There is a very large range of BAF values among regions, feedstocks, and using different baselines and other assumptions. The range in BAF values for individual facilities is likely to be even larger due to additional variation in feedstocks,

management, etc. Thus the Framework should account for this wide variation such that unintended consequences for global warming potential (GWP) do not occur if there is preferential implementation of selected feedstocks, locations, types of facilities, regions, etc. For this reason, the charge questions should not focus on choosing one among many options, but instead modeling should be conducted to explore the range of possible outcomes and inform a risk management approach that acknowledges a wide range of possible future outcomes.

Responses to Specific Charge Questions

Question 1a-i. See General Comment #4. Even for shorter-term policies, results should be analyzed and presented for at least 100 year time scale to capture a corresponding GWP of 100 years so that long term impacts are included in the decision making. In other words, because the biophysical impacts of CO_2 emission occur over longer than 100 years, these long term impacts should not be ignored.

Question 1a-ii. See response to Question 1a-i.

Question 1a-iii. Because no specific examples of policy or regulatory actions are provided for evaluation, it should be assumed that the answer could vary by policy (see General Comments #3 and #4).

Question 1b. See response to Question 1a-i.

Question 1c. The answer depends on the specific policy goals and on practicalities of implementation. However, the analysis of impacts should account for the long time scale of emissions and impacts (see response to Question 1a-i). Presuming that the policy goal is to reduce global warming, then actions that reduce GHG emissions today would be more beneficial than actions that reduce GHG emissions in the future, and this difference should be accounted for. For this reason, it probably does not make sense to account for all future emission changes as if they occur today. However, note that current biomass use for energy may have caused changes in emissions due to factors such as anticipatory planting, which can be estimated with retrospective analyses (see response to Question 2a).

Question 1d.

Question 2a. If the goal is to develop a BAF to apply to all users, then the anticipated biomass use by all users should be modeled

Question 2b.

Question 2c. The shock should be selected to represent the anticipated increase in biomass use for energy due to a specific policy being addressed by the framework, and should be in addition to existing policies. See response to Question 2a.

Question 2d [See responses provided above in first section]

Question 2e [See responses provided above in first section]

Question 2f.

Question 2g. Because no specific examples of policy or regulatory actions are provided for evaluation, it should be assumed that the answers to the above questions could vary among specific policies (see General Comment #3).

Question 2h.

Specific Comments on the Document

<u>Framework (main document)</u>

p.7, 10. See general comment #1 regarding GWP and N_2O. To assess GHG emissions, N_2O and CH4 should be included in addition to CO_2. Is there any reason why N_2O cannot be included in calculations of GHG emissions, and a BAF factor, even if the policy focuses on CO_2?

p. 8. Figure 2 should clarify the "natural CO_2 fluxes" are modified by humans, and therefore can be anthropogenic.

p. 9. It is misleading not to clarify that the purpose is to reduce GWP (or radiative forcing, see general comment #1). If not, then what is the purpose of this accounting framework?

p. 13. Equation 1 should include unites (stated for NBE as mass, but mass of what (C? CO_2?) and should be included in each equation.

p. 14, 3rd line below Equation 3. Typographic error, remove "of".

p. 15. May need to account for fossil fuel carbon in ash etc.

p. 16. Clarify that not all stover can be harvested.

p. 21. The unit CO_2e is used here and other places supporting my contention that the purpose of the document is to assess GWP (see general comment #1).

P. 30. Cut the sentence "In certain contexts …" because it is not supported in the document.

p. 34. Discussion of time scales should include 20, 100 y GWP (see general comments #1, #4).

p. 34, (bullets). The document should clarify that some choices are not appropriate for estimating GWP (see general comments #1, #4).

Appendix B.

p. B-14-15. See general comment #5.

Appendix K.

Current biomass use for energy is ~130 million oven-dry tons/yr., mostly black liquor (Figure K-1). For this use, accounting should be retrospective, not prospective. But the retrospective analysis should account for effects such as anticipatory planting that may have occurred in the past.

K-3. FASOM-GHG doesn't include black liquor, which is a major source of bioenergy, so it should be included if feasible in the model, and at a minimum there should be discussion of the effect of leaving it out of the model analyses.

K-4. Commercial scale use is not included; could this be a major source, or potential for reduced emissions? Some states for example have implemented bioenergy programs targeting commercial scale entities such as schools.

K-4. Correct the text that industrial consumption is greater than that in the electric sector (Figure K-1).

K-5. Figure K-1, is current ethanol production included in "Industrial Other"?

K-5 and K-9. The total in Table K-2 (22 million tons) is not 37% of the total in Figure K- (the 37% appears on page K-5).

K-10. Does "low renewable" include biopower? Ethanol? Other bioenergy?

K-10 and 11. Do you assume no industrial fuel switching to biomass?

K-12. Use the same vertical scale on all figure panels.

K-14, bullet 4. Clarify agricultural versus forest producing and demand.

K-16. Should show effects including wood products and CH_4 and N_2O, because they may have a large impact on total GHG emissions and thus GWP. Also should show effect on fossil fuel CO_2 emissions, which would presumably increase with a zero-biomass scenario.

K-20. The comparison per ton is more relevant than per acre. Note that an analysis of corn grain (but likely not corn stover) would show very different results if N_2O is included, which it should be. It is important not to get the wrong answer for total GHG emissions by excluding N_2O, even if the regulation addresses only CO_2.

K-26. See general comment #5.

Appendix L.

L-13. The marginal calculation seems more appropriate than the average because it includes the full response to the demand shock (including anticipatory planting etc.). The average seems not to include such impacts but it should include them even if they happened in the past for current biomass use.

L-15. Is it realistic that increased demand for forest residues would cause increased harvest of roundwood? See response to Charge Question 2e.

L-15. Is increased emission due to decreased stocks? What is the fate of increased roundwood harvest? If it replaces concrete and steel, then it could cause a net decrease in GHG emissions (although not at the stack).

See General Comment #5.

Appendix M.

M-11 and 12. Table M-6. What should "with N_2O" be compared to?