Preliminary Individual Comments
Biogenic Carbon Emissions Panelists
Updated 3-19-15

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Comments from Dr. Marilyn Buford

Comments on Framework for Assessing Biogenic CO2 Emissions from Stationary Sources

Charge Questions:

1. a. The temporal scale should vary by feedstocks and landscape conditions.
   i. Actually looking at what the atmosphere sees over the long term relative to shifting among energy sources would support longer temporal scales.
   ii. Actually looking at what the atmosphere sees over the long term relative to shifting among energy sources would support longer temporal scales.
   iii. Not if the temporal scale is tied to the feedstock and landscape conditions – the actual production machinery.

1. b. Examination of the effects of a policy should include consequences attributable to the policy regardless of the date of policy ending. The ramifications of the policy can continue on far past the policy's end date.

1.c. A cumulative approach could be used to effectively blend the spatial and temporal considerations of a variety of feedstocks across diverse landscapes.

1.d. Can the models actually mimic/predict what happens/has happened across the landscape relative to such factors as feedstock supply, price, location, and landowner responses to market changes

2. a. Shocks should reflect what is likely to be the most realistic path of implementation.

2.b. Tons.

2.c. BAU baseline.

2.d. It might be most useful to implement shocks from different feedstocks in a dynamic fashion that allows each feedstock to be included based on supply-price point.

2.e. It might be most useful to implement based on the relative supply/prices.

2.f. Default factors might best be considered on regional bases.

2.g. Not at this time.

2.h. Can the models actually mimic/predict what happens/has happened across the landscape relative to such factors as feedstock supply, price, location, and landowner responses to market changes.

Specific comments:

Sec. 1.3, p.11: carbon that is not returned to the atmosphere is also stored in products in use and in landfills. That point needs to be made and recognized.

Fig. 2. Wildfires are sometimes/often caused by humans. They are not all lightening caused.

Sec. 2.4.4, p. 18: it is technically incorrect to assume that the emissions from unmanaged forest will be less than a managed forest given the impact of fire, insects, and disease on unmanaged forests and the products coming from managed forests.

Appendix J, sec. 3.7, p. J-13: why would agriculture not be addressed in this section? Aren't the issues similar relative to forestry and agriculture for energy sector representation?

Comments from Dr. Mark Harmon

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:

Although I have been asked to answer this question I have decided at this time to not answer it. There are two reasons. First, I am not I am sure I understand this convoluted question. Second, and far more importantly, I believe that answering this question is of minor importance compared to more general issues with the framework released November 2014. I have reviewed that document, the EPA's response to the September 2012 SAB report, the SAB report itself, and a number of Appendices (B, J, L, K). While I appreciated the EPA's changes in terms of including an anticipated baseline as well as the consideration of time, I found a number of other significant issues that were mentioned in the SAB report were not addressed. These include lack of transparency in the terms, problems involving spatial scale, terms that potentially could lead to a violation in the conservation of mass, important terms that were completely lacking, terms that seem to have multiple meanings, inappropriate spatial scales for considering default BAF's, and any number of other issues that would make the framework unworkable and misleading. I will not elaborate examples of all these issues here, but would certainly be happy to do so in a separate report. While some of these problems seem to stem from EPA not following the SAB's advice, I will be the first to admit that the SAB report did not include some information that should have been provided. In particular, important issues related to spatial scale were discussed by the SAB, but not included in the final SAB report. These could have potentially helped EPA to avoid some of the issues that are found in the November 2014 framework.

One example of the significant issues remaining.

The basic formula is

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

Is still a model of non-transparency. GROW is actually net growth which is a term most foresters would understand. So why wouldn't it be called NetGrowth? When something grows it increases. But this function could increase or decrease. This can only be confusing because it is non-intuitive. The AVOIDEMIT is unnecessary and under some circumstances lead to conservation of mass not being obeyed. SITETNC seems to vary depending on whether something is a fuel stock or not, but in some cases fuels stocks could be dead material and so what is SITETNC could potentially vary.

A transparent, intuitive system would be clear to most everyone. If framework was transparent and intuitive it would be based on EPA's own words: "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?"

That would mean that the terms would be things like: the net change in live stores, the net change in dead stores, the net change in soil stores, the net change in product stores, and the net change in waste stores. I don't see how those terms would be ambiguous or non-intuitive.

The framework seems to be a leftover from the static reference point idea. It will not work well in an anticipated baseline setting because no biophysical model would include these terms because they have almost no process basis. In contrast, the net change of the pools mentioned is something produced by most process models and therefore could directly be moved into the framework. Why wasn't the framework be set up this way if the plan is to make it workable?

Comments from Dr. John 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.

I've copied in the charge questions and offer some ideas, although I think the entire method has no foundation.

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—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 so 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 that my hypothesis is that the model 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. Although, I don't think there is any reason to believe this relationship over time would be monotonic or smooth.

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.

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)?

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.

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 don't know what a policy neutral default factor is.

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.

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Winchester, N., S. Paltsev and J.M. Reilly, 2011. Will Border Carbon Adjustments Work? The B.E. Journal of Economic Analysis & Policy, 11(1): Article 7, 2011 (http://dx.doi.org/10.2202/1935-1682.2696]

Comments from Dr. Charles Rice

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

The BAF should not include all future fluxes into one number applied at the time of combustion. The uncertainty of future emissions and land management could increase or decrease future net emissions. Calculations could be revised for regional BAFs on an annual basis or in short time blocks (such as 5 year increments to provide stability and certainty to the stationary source) depending on the temporal scale. Revisions based on regional analyses of net emissions would for encourage sustainable resource (both soil and plant) management.

Comments from Dr. Daniel Schrag

Initial Response to Charge Question 1a.

CHARGE QUESTION:

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?*

> a. Should the temporal scale for computing biogenic assessment factors vary by policy (e.g., near-term policies with a 10-15 year policy horizon8 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.
>
> > 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?
> >
> > 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?
> >
> > 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?

* Though discussion of temporal scale issues appears through the revised framework, targeted discussions on temporal scales can be found in the following framework components: revised report Section 4.2 (pages 33-38); an appendix dedicated to temporal scale issues (Appendix B); an appendix describing the background of and modeling considerations for constructing an anticipated baseline approach has a section on temporal dynamics (Appendix J, Section 3.1.2), and the appendices that construct baselines and alternative scenarios for illustrative future anticipated baseline approach case studies (Appendices K and L). 8 In some cases, it may be useful to distinguish between the emissions horizon and the policy horizon. The emissions horizon is the period of time during which the carbon fluxes resulting from actions taking place today actually occur, which may span a year to several decades, depending on the feedstock and production site conditions. The policy horizon is the established or expected period of time for policy implementation and analysis of related estimated effects. In effect, these time horizons can differ significantly.

The revised Framework and associated Appendixes (especially Appendix B) are a great improvement over the previous Framework with respect to the treatment of temporal issues, as the previous one mostly overlooked the issues entirely. In this revised version, there are several places where temporal issues are discussed, including a long and complicated discussion of what timescales to use with different choices of baselines, and also a complicated discussion of whether it is appropriate to discount future CO2 emissions savings (or carbon uptake), ultimately never reaching any firm conclusion or policy decision. I greatly appreciate the effort that was made to consider these issues in a serious manner, but I feel that the current framework is so complex and unclear about what to do with temporal issues that it is almost a step backwards. We had hoped that the EPA would not only conduct a detailed analysis of the temporal issues (as they have), but also would make a policy recommendation for a reasonable way to handle time scales in a manner that is simple enough to be effective when applied in a regulatory context. This last, crucial part is simply lacking from the discussion provided here, either in the Framework itself or in the Appendix.

I fear that the EPA has gotten stuck in the weeds in this Framework, and – excuse the phrase – is having trouble seeing the forest for the trees. The current version is incredibly difficult to follow – and looks even more difficult to actually implement in any kind of regulatory framework.

In response to the questions above, after dealing with a few specific issues, including carbon discounting, let me try and step back and suggest some possible ways of treating time in a simple way that is consistent with what we know about the carbon cycle, the climate system, and the biological systems used for biogenic feedstocks. I will do this in the context of some simple examples of biogenic feedstocks to highlight the issues that are likely to emerge in different contexts. I think this will be more effective than providing feedback on the complicated discussion in the Framwork and the Appendix, as there is really no specific proposal to evaluate.

Before I start with this discussion, let me point out that the EPA already has made a choice for what timescale to use for climate change accounting. For short-lived pollutants, in particular for methane emissions, the convention is to use the GWP100 – which is the integral over 100 years of radiative forcing from the release of 1 kg of methane relative to the radiative forcing from release of 1 kg of CO2. Some have criticized this for ignoring discounting of the future (more on this later), and some have criticized it (rightly in my opinion) for valuing everything beyond 100 years as worth zero. Regardless of these criticisms, I believe 100 years is roughly the right timescale to consider climate forcings because it is the timescale over which the climate system responds. But more importantly, it seems absurd to have a different time frame to consider biomass than what is already used by the EPA to consider methane. Both should be the same, and if you have to choose a time frame, 100 years seems reasonable.

Now – let me explain why discounting future carbon uptake by regrowing forest is problematic. I know this is a complicated issue, and there are many people who are confused by it. To many economists, the answer is simply – "of course we should discount future emissions reductions!" But emissions are not money, and the relationship between emissions and climate damages is far from straightforward. Consider the example of clear cutting a forest and burning the trees (not to replace fossil fuels – just a big bonfire), but the trees grow back so that in 100 years there is exactly as much carbon stored in the ecosystem as there was before. If one discounted the future carbon uptake using the range of discount rates usually suggested by the economics community (2 to 4% or so), the value of the carbon taken up in the latter half of the century would be extremely low, so the net value of the action would be counted as a huge release of carbon to the atmosphere. But what is the actual effect on the climate system? Repeated studies have shown that peak warming depends on cumulative emissions, and is not sensitive to small changes in trajectory of emissions. And other impacts on society – such as sea level rise, ultimate fate of Greenland and Antarctic ice sheets, food production, etc. - probably also track cumulative emissions more than the exact trajectory in radiative forcing. And in this case I have given, cumulative emissions are exactly the same, and peak warming is exactly the same. There is certainly some value to avoiding the interim radiative forcing from the CO2 that is in the air between clearcutting and regrowing the forest, but it is relatively small, and would be grossly overvalued if one discounted future emissions using discount rates that are appropriate for economic decisions about investments, etc.

I think the discussion of temporal scale with respect to the baselines misses the big picture. My sense is that there are some simple approaches to largely avoid the issue of timescale, or at least let the timescale come from the specific context. Let me explain.

There are really two issues to deal with in this framework, and the current version muddles them both. One is the question of carbon debt – the amount of carbon released from cutting a forest and converting it to a bioenergy plantation, whether as agricultural land or as a forest that is harvested regularly. Those concerned with forest conservation advocates are justifiably concerned with the adoption of regulations that would encourage additional deforestation without consideration of the carbon debt. On the other side, climate concerns drive us to properly value the carbon benefits of an emerging biomass industry, if carbon debts are actually repaid, as it may be essential in replacing fossil fuels, particularly petroleum. The second issue is the question of the fossil fuel emissions associated with biomass production, whether from transportation, fertilizers, or other inputs. In theory, if there were a simple way to measure both of these factors (carbon debt and fossil inputs), I believe the regulation of biomass feedstocks would be relatively straightforward.

Let us consider two cases – one involving annual or perennial crops with rapid growth and harvest intervals, and one involving forest feedstocks (i.e., roundwood) with longer timescales and harvesting times.

First, consider a biomass feedstock crop. It doesn't matter whether it is grown as a perennial (e.g., switchgrass), or as an annual crop (corn). In both cases, the question of biomass growth timescales, and decay of residues and waste are essentially moot because they are fast enough over a climate-relevant timescale (we will come back to exactly what that is later…). The main issue for a stationary source using such feedstocks (whether an ethanol refinery, a biomass-fired power plant, or a paper mill) is the question of carbon debt involved in the original appropriation of the land for agricultural use (presumably deforestation on most cases), and the question of CO2 produced from fossil fuel used to grow the crops (fuel for tractors and trucks, natural gas for fertilizer production, etc.). The Framework should value such CO2 emissions (which should include carbon in short-lived products that are also going to be released to the atmosphere as CO2 – such as ethanol or paper) to make sure that any carbon debt is ultimately repaid, and the inputs (fuel, fertilizer) are properly valued. Such an approach would properly credit those uses of biomass feedstocks that result in real carbon savings.

As an example, let's assume that the clearing of land for a particular biomass supplier resulted in loss of 100 tons per hectare of carbon of total standing biomass (the exact number doesn't matter – and let's assume that this assumes soils too), that the production of the crop yields 15 tons per hectare, and that the fossil inputs (fertilizer, fuel) result in a fossil carbon footprint of one third of the total carbon (or 5 tons per hectare). In this situation, the biomass feedstock used at some factory should be counted as 1/3 of the carbon intensity of fossil fuel CO2 (5 tons per hectare of carbon out of 15). But this facility must repay the carbon debt of 100 tons per hectare (or 360 tons of CO2).

Repaying the carbon debt for a paper mill is more complicated – let's come back to this issue later. But for a biomass energy facility, it is relatively straightforward. The simplest approach is to require that the carbon debt is paid down before carbon benefits are counted. This means that the CO2 emissions from a stationary source using biomass in this example would be treated exactly like a fossil fuel plant until the displaced fossil emissions exceed the carbon debt. After this point, the factory would be treated as a lower-emissions source, with an annual emission of 1/3 of a fossil fuel source (the part due to fertilizer, transportation, etc.).

How fast does the facility pay off the carbon debt? This requires an analysis of what fossil emissions are actually being displaced. For ethanol, one would calculate the reduced emissions from petroleum; for wood pellets added to a coal-fired power plant, it would be the reduced use of coal based, including the changes in heat rate for the plant. In any case, this is relatively straightforward, and could be done quite simply. It would make the carbon debt explicit in the obligation of a factory using biomass feedstock, and that would deal with the environmental concerns most directly.

The important thing for this discussion is that one really doesn't have to select a particular timescale here except that given by the rate of production of fossil-free biomass relative to the timescale to repay the carbon debt through displacing fossil fuels.

In reality, one might not want to require that all the carbon debt is paid down up front. Amortizing the debt over a reasonable time (e.g., 30 years) has very little negative consequence for climate change, as numerous studies have shown that peak warming is mostly dependent on cumulative emissions, with minimal path dependence. There are very complicated questions about the value of delay, but in general, the climate system only responds to emissions on timescales of several decades, given the complexity of decadal variability in ocean heat uptake, natural cycles, etc. So in this example, instead of treating the emissions as equivalent to those from a fossil fuel plant until the carbon debt is repaid, there would be some schedule for repaying the carbon debt, but the facility would still receive some discount in its carbon accounting relative to fossil fuel. Again, once the carbon debt is repaid, the emissions from the factory are counted only as that fraction that represents the contribution from fossil fuels in creating the feedstock.

The Framework discusses the possibility of crediting biomass growers for increases in carbon storage in soil – for example, from switching from an annual crop such as corn to a perennial crop such as switchgrass. I see this as an added complexity with very little benefit. There is no doubt that there is some additional carbon storage in terms of soil carbon, but giving essentially a credit for that carbon assumes that the carbon will persist there in perpetuity. Once a credit is given, that carbon has been monetized, and if the farmer ever tills the land or sells the land to someone who will use it differently, that carbon credit must be repaid – but there is no enforcement mechanism to make that happen. Thus, it is probably easier to simply neglect such details in the accounting.

Now consider the case of a forest that is harvested for wood pellets for power plants. In this case, the situation is almost exactly the same. There is a carbon debt that must be repaid, and also fossil inputs from fertilizers and transportation and harvesting of the wood. But there is also a carbon benefit from displacing fossil fuels. Again, the timescale here is internally determined - i.e., it is simply the timescale for repaying the carbon debt. It will be longer in hardwood forests in New England, and shorter in pulpwood forests in Georgia (see the Manoment studies that discuss both of these cases…). If timber company that was producing wood chips for power plants could show that their planting practices and their rate of harvesting of biomass was essentially resulting in no net loss of biomass, and their was no recent carbon debt to repay, then their feedstock should be counted as just that due to the fossil contributions from fertilizer, etc.

The forestry issue can get a little more complicated than the crops case in thinking about a situation in which the rate of harvesting is exceeding the rate at which that forest is regrowing the carbon. In this case, the carbon debt is actively growing, and an estimate must be made of how fast it will grow, and whether it will stabilize. But the same basic consideration applies – the carbon debt must be repaid over some reasonable timescale (e.g., 30 years) before the biomass can be considered “debt-free”.

There are two aspects of the above discussion that do require some consideration of timescale – and these issues are also touched on in the Framework. One is the lengthy discussion of baseline in the Framework – again that doesn’t seem to make any specific policy recommendation about what to do. For forestry, the baseline must be an “anticipated” baseline to deal with the additionality question. The critical issue is simply that question of how far into the future can we accurately project carbon uptake rates for forests. For example, the Manomet study of Massachusetts forests did a 100 year analysis, but failed to consider how climate change will affect the forest over that timescale. Certainly it is important to account for growth in immature forests to make sure that the carbon savings from using the wood is additional – which simply means that the carbon debt must be calculated based not only on what was there when the harvest began, but what would have been there in the future if the harvest had not occurred. The appropriate timescale is probably the time it takes in that forest to repay the carbon debt (and would be different for south versus north, and hardwood versus softwood, etc.).

The other question of timescale comes in for long-lived products, and the question of whether these count as emissions or not. In my view, claims that “long-lived” products should count as permanent sequestration is problematic, given that the climate system cares about cumulative emissions, and even 100 years is not long enough. Imagine a geologic carbon reservoir that would only last 100 years and then release all of the carbon; it seems unlikely that the public would be satisfied with this counting as permanent sequestration. So then why is the situation with timber products any different? That being said, it seems reasonable to count products with lifetimes longer than 100 years as outside of the scope of the analysis for the purpose of regulations, and products with much shorter lifetimes should be counted as carbon emissions – because from the global climate perspective, they are.

So overall, my feeling is that the Framework has made this exercise more complicated than it need be. The EPA can ask biomass suppliers for two calculations. First, an estimate of the carbon debt – which in many cases will be zero, simply because the land was cleared a long, long time ago, or the forest is being managed in a steady-state manner (i.e., harvesting is balanced by new growth). Second, the supplier needs to estimate the fossil contribution to the biomass – and this is pretty easy to estimate as well. For smaller producers, there could be some standards for different types of biomass activities, and these could be assumed, unless the producer wanted to go through an independently audited analysis. It seems like this would be infinitely simpler than the framework as it stands now.

Comments from Dr. Roger Sedjo

Roger Sedjo's Comments on the SAB questions 2 a), b) and c):

EPA seeks guidance on technical considerations concerning how to select model perturbations ('shocks')for future anticipated baseline simulations estimating the net atmospheric contribution of biogenic CO2 emissions from the production, processing, and use of biogenic material at stationary sources, using the above referenced components of the revised framework report as the starting point for the SAB Panel's discussion.

As the SAB Panel recommended developing default assessment factors by feedstockcategory and region that may need to be developed outside of a specific policy context, and as the framework could be also be used in specific policy contexts, the questions below relate to the choice of model shocks both within and outside of a specific policy context.

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

**

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

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 from Dr. Kenneth 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?*
   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.
      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?
      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?
      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?

*Though discussion of temporal scale issues appears through the revised framework, targeted discussions on temporal scales can be found in the following framework components: revised report Section 4.2 (pages 33-38); an appendix dedicated to temporal scale issues (Appendix B); an appendix describing the background of and modeling considerations for constructing an anticipated baseline approach has a section on temporal dynamics (Appendix J, Section 3.1.2), and the appendices that construct baselines and alternative scenarios for illustrative future anticipated baseline approach case studies (Appendices K and L).

8 In some cases, it may be useful to distinguish between the emissions horizon and the policy horizon. The emissions horizon is the period of time during which the carbon fluxes resulting from actions taking place today actually occur, which may span a year to several decades, depending on the feedstock and production site conditions. The policy horizon is the established or expected period of time for policy implementation and analysis of related estimated effects. In effect, these time horizons can differ significantly.

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:

The Biogenic Assessment Factor, and the time period considered in its calculation, should have the same meaning as the GHG emissions metric that are reported in the U.S. annual inventory of GHG emissions and sinks. Without a BAF metric that has the same meaning 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 technologies 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 CO_2, 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 (IPCC 2007, p 210).

If the BAF metric for a biogenic emission were to time-weight net emissions or radiative forcing associated land carbon change this would be at variance with the meaning (GWP-100) for a single CO2 emission.

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.

For a biogenic emission in a given year there is a the time path of radiative forcing that is associated with 1) the initial emission – over 100 years and 2) changes in land carbon net emissions compared to a reference case with no emission. We should be seeking a metric (BAF) which represents, <u>in the year of the initial biogenic emissions</u>, the radiative forcing – over 100 years - associated with the initial emission plus or minus all the net changes in radiative forcing associated with net land carbon change.

However there is a key difference between computation of the GWP-100 for a single emission and for a biogenic emission. The difference is that the follow-on net land carbon changes and associated change in 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 conceptually as a distinct kind of greenhouse gas with its own radiative forcing profile which has uncertainty as a feature.**

I suggest that we initially consider the idea that a BAF value should estimate the certainty equivalent of the GWP-100 of a one ton emission of CO2. Consider the following decision to understand why we should seek a certainty equivalent. Consider a choice between two policies – one which would weatherize buildings and result in 100,000 tons less CO2 emissions per year and one which would support use of wood energy in place of fossil based energy and result after applying our BAFrf factor of 100,000 tons less CO2 emissions per year. The GWP-100 effect of the weatherization policy is virtually certain, while the BAFrf generated estimate of 100,000 less units of CO2 when using wood energy is less certain because of the uncertainty associated with future net C fluxes on the land.

The decision maker needs to consider how risk averse they should be in comparing policies in order to construct an estimate of BAF that would be somewhat pessimistic in its assignment of the biogenic CO2 equivalent given the uncertainties of future land carbon changes. If the decision maker is not risk averse the BAF value would be the probability weighted average of possible BAFrf values. Note that while in principle we need to consider radiative forcing from land carbon changes out 100 years the uncertainties would be high and the probabilities could be low.

This raises two questions- how should we handle possible risk aversion and how should we consider the uncertainty of future land carbon changes.

Note that the metric we are seeking would be the GWP-100 value but in constructing that estimate we will need to make judgments about the probability of difference between scenarios in future land carbon changes.

First consider an example estimate of BAFrf for a simple case.

A Simple case where net land fluxes are "certain" - Estimating BAFrf(100) for logging residues

For the case of estimating the BAFrf for logging residue, the time path of avoided emissions is relatively certain based on empirical studies of wood decay in forests by location. If the use of logging residue is not large relative to production from existing harvest then logging residue decay curves may be used to estimate the time path of avoided CO2 emissions AND the associated avoided radiative forcing out to 100 years after the initial biomass emission.

BAFrf(100) for a 1 ton CO2 emission from logging residue is the net radiative forcing for a time 0 emission and 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 BAFrf 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) and Cherubini's equations the BAFrf(100) values for burning logging residue that decays at rates in Table L-15 (of Appendix L) are shown in Table 2.

Note that the use of radiative forcing to estimate BAFrf(100) for logging residue, will always result in a BAF > 0 to account for the time lag – and the net positive radiative forcing – that occurs between the initial emission and the avoided emissions. <u>Unlike the BAF that uses CO2 fluxes there is no need to decide the</u>

time horizon to use in computing net CO2 fluxes in BAF in order to account for the near term effects on the atmosphere. BAFrf(100) includes near term radiative forcing effects explicitly.

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

Forest Type	FASOM-GHG Region								
	CB	LS	NE	RM	PSW	PNWW	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 – BAFrf(100) values for logging residue (assuming no effects other than Avoided Emissions)

Forest Type	FASOM-GHG Region								
	CB	LS	NE	RM	PSW	PNWW	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 Complex Case – where future land carbon fluxes can be less certain – Estimating BAFrf(100) for roundwood use in the Southeast

Consider the estimates of BAF values in Table L-4 for cumulative additional emissions for an average user. The BAF values are for an average user and average emission over the 2010 to 2060 time period. Unlike the BAFrf(100) estimate for logging residue where there is a single initial emission and it is clear we track radiative forcing effects of land fluxes to year 100. For the cumulative average BAF values in Table L-4 it is not clear how far out to include radiative forcing from land fluxes since emissions in the denominator for BAF span 50 years. A simple approximation could be to assume the average emission occurs in year 25 of the 50 year projection, year 2035, and therefore we would (in principle) track radiative forcing of ALL land C fluxes to year 2135. A recomputation to give cumulative average BAFrf(100) values at various years may not be much different than the current BAF values, but the meaning would be consistent with the meaning of CO2 emissions in the EPA GHG Inventory.

Unlike the estimation of BAFrf(100) for logging residue where land fluxes are fairly certain there may a difference in certainty of attaining early cumulative BAFrf(100) values (2015-2029), versus later years (2015-2060). A key assumption in estimating some probability weighted Cummulative BAF values is that they are decreasing over time.

Note that even though <u>we are tracking radiative forcing</u> of land C fluxes out to 125 years <u>we may not expect to track all the Land C fluxes</u> out beyond say 50 -60 years. This is because of decreasing certainty of difference in fluxes many decades out.

One general rational for giving at least a 50% probability weight to cumulative BAFrf(100) out to 50 years is the practice supported by the Forest Service and others to make 50 year national forest projections. Implicitly this supports the view that the direction and magnitude of projected changes out to 50 years are relevant for current policy making. This reasoning suggests we have the expectation that such projections are relevant. This view is consistent with the thought that forest policy we set today (and would need to continue) will have effects out at least 50 years.

For the sake of discussion assume the BAF values in Table L-4 approximate the cumulative average BAFrf(100)values and we can conclude that there is a 60% chance of reaching the -0.03 cumulative average BAF value by 2060. If we had lower BAF projections beyond 2060 we are implicitly assigning them 0 probability. Assume it is reasonable to think we have a 75% chance of reaching the 2044BAF value of 0.05 and a 100% chance of reaching the 2029 BAF value of 0.28.

If decision makers are not risk averse the assigned BAF value would be the probability weighted average of these BAF values or 0.06. We could assign this as an estimate of the average BAF for all facilities using roundwood from 2010 to 2060. We would make adjustments to improve this average BAFrf(100) value over time as information is available. See my response for Question 1 d.

If decision makers are risk averse then we would need a utility function for various levels of BAFrf(100) outcomes in order to compute a BAFrf(100) value that decision makers would accept with certainty (for the outcome of using biogenic biomass) in comparing that choice to other potentially more certain choices to limit CO2 emissions.

But in response to question 1d. I propose a periodic retrospective assessment and revision method for BAF values where their could be high confidence in cumulative BAF (average or marginal) over a 50 year period. With a high confidence level in our 50 year cumulative BAF then we could use a probability weighted average BAF without correction for risk aversion.

b. 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: *I suggest the BAF values should be computed using projected difference between scenarios in radiative forcing of emissions and land C fluxes out to 100 years after the initial emission. However this does not mean that we consider all differences in land carbon out to 100 years. We include land carbon changes if we think they have a notable probability of being attained.*

It may be possible that certain policies could express risk aversion by assigning a lower probability, or assigning no probability of attaining land C gains after a certain time. Net emissions calculated with such a BAF value could not be compared to net emissions using BAFs computed differently for other policies.

Generally, in order to maintain comparability of net emission effects of policies, it seems the decisions about probabilities of attaining certain land C fluxes should not vary by the type of policy under consideration. Therefore the BAF values would not change for short term versus long term policies. Just as conservation measures would be assigned a CO2 savings values for each year (and represents100 year radiative forcing), any biogenic biomass *energy policy should be judged by a comparable measure which would be an estimated certainty equivalent BAFrf(100) value for each years emissions.*

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 probability of attaining land C fluxes, 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: *No. It is possible to estimate policy neutral BAFs. In fact it would be required to estimate policy neutral BAFs if they are to match the meaning of CO2 emissions values reported in the EPA Inventory of GHG emissions and sinks. They are also required to be able to compare the effect of policies that may differ in extent over time and space.* ***If we cannot compare across policies using a common metric how do we know if our choice will give a better outcome than alternatives in terms of effect on the atmosphere?***

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Response to Biogenic carbon framework Charge Question 1 d.

Ken Skog, March 18, 2015

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:

For the purpose of this discussion, I will initially interpret "What considerations could be useful…" to be asking "What data and analyses would be useful in evaluating the performance of a future anticipated baseline BAF estimated on a retrospective basis?" Also I will focus on BAF estimates using anticipated baseline that require projections of uncertain land carbon change for both a with bioenergy use case and a lower or now bioenergy use case.

It seems we need to consider retrospective assessments for cases where BAFs are estimated separately for various wood and agricultural biomass sources/ regions and cases were BAFs are estimated and used for aggregates, such as all wood, all agricultural feedstooks, or all wood and ag feedstocks combined.

Appendix K provides projections for estimating net emissions per unit biomass over time for an aggregate of all wood and agricultural biomass by region and for the US as a whole – the aggregate estimation case. The mix of sources changes over time within regions and these changes influence the cumulative net emissions per unit of biomass over time. Appendix L provides projections for estimating net emissions per unit biomass over time for individual biomass sources in individual regions – the unique source and region estimation case.

Basic data needs

To evaluate performance of either aggregate BAF estimates or unique source BAF estimates there is a need for a retrospective assessment to have data on how much of each biomass source is used by stationary sources (roundwood, logging residue, forest products mill residue, agricultural residue, perennial crops). Much of this data might be obtained by region using exiting survey methods in order to substantially (but not entirely) avoid reporting burden for stationary sources. For wood sources we need estimates of wood from main stem of trees (roundwood) and from logging residue (tops branches, cull material).

Forest Service FIA plot surveys for harvested plots could tag them for example as logging residue removed, logging residue not removed, or partial/uncertain. This data along with plot data on main stem wood removed and separate data from surveys on usual logging residue generation per unit of main stem removal could be one source of data on logging residue production.

Stationary sources could be asked to obtain information on the wood source – e.g. forest products mill residue, wood chipped outside the forest, wood chipped in the forest, roundwood delivered to stationary source. It may or may not be realistic for them to determine if forest source chipped wood is logging residue. The logging residue would be some or all of the wood chipped in the forest.

Receipt data from stationary sources in a region could be compared to forest based estimates of logging residue that is removed for the region. Data on wood transport into and out of the region may help refine the comparison to a degree. If wood chips are recorded as coming from whole trees an estimate of the fraction that would have been logging residue (tops, branches, cull) is provided by region in the periodic Forest Resources of the United States (Oswalt et al. 2014, table 40).

Would it be of value for Forest Service FIA in partnership with state governments to survey wood delivered to stationary sources as an addition to their Timber Product Output surveys in each state? Information on feedstock to electric power plants is already collected by US DOE EIA. If agricultural biomass is also being delivered surveys may require coordination with USDA agencies and DOE EIA.

Similar data would be needed on the basic features of agricultural materials used by stationary sources. Data on agricultural production of residues and perennial crops could be compared to data on features of biomass receipts reported by stationary sources.

I will assume that whatever model is being used to make projections will, periodically, be independently peer reviewed and revised in response (e.g. for FASOM-GHG, see USEPA 2011)

Validation of model projections used to estimate of BAF – Unique biomass sources by region

BAF estimates are responses to specific questions about the net emissions effect of a change in use of biogenic material. The questions come with side assumptions about how change in use will be met.

To do a retrospective test of projections and a BAF estimate we are initially bound to incorporate the same side assumptions whether or not the assumptions are met in the real world. 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, a specific question asked and answered in Appendix L is

> Question 1 - Compared to a case of no use of wood biomass use by stationary sources in the U.S. over a period 2010 to 2080, if we had roundwood use for energy in the Southeast U.S. at a levels based on AEO projections, what would be the difference in net CO2 emissions per unit biomass CO2 emitted (year by year or cumulatively) from 2015 to 2060?
>
> [There are alternate questions in Appendix L about BAF values for marginal increases in biomass use. Suggested procedures for Assessment below would be the same with the except there would be lower biomass case instead of a no wood case.]

Question 1 has the side assumption that there is not an increase in biomass use for energy from any other source. If we use the resulting BAF to characterize the effect of higher roundwood we are assuming either 1) there will be no other material used by stationary sources over the projection period (an assumption to be checked over time) or 2) the BAF estimate is independent from (and the same as) any case where, for example, ag biomass use is allowed to meet demand in competition with roundwood in the same region (a calculation that could be made now).

In response to Question 1 Appendix L gives several estimates of the "Landscape factor" part of BAF. I will refer to it as the estimate of BAF.

For this discussion we <u>consider an assessment and revision of the estimated cumulative BAF for the average user over 50 years</u> (table L-4). The cumulative BAF value in 2060 gives the net emissions, over the 50 year period, for an average 1 ton CO2 emission from roundwood burning.

An assessment and correction of an estimate of BAF for marginal change would be much the same except the counterfactual (BAU) case would have marginally less biomass than the With (extra) Wood use case.

One perspective for the cumulative average BAF is to think that collectively all the roundwood produced and emitted is collectively resulting in market response and forest growth over the entire time period. Collective stimulus, collective response. There would be interest in a BAF from a zero baseline if we are interested in a BAF to assign to all stationary sources over the projection period. A marginal BAF would be needed if we are interested in the net carbon emissions effect (over the projection period) of the most recently added stationary sources.

For the purpose of this assessment discussion I will assume a decision has been made to estimate and assign BAF values, over a 50 year period, that over 50 years will match the cumulative average 50 year BAF. This is based on the finding from the analyses for Appendices K and L that the scenario to scenario difference in land C fluxes can vary notably year to year and it is not realistic to use fluctuating year to year BAF values for stationary sources. Assessments done approximately each decade would modify 50 year average BAF values as discussed below.

Assessment/ validation step 1

Of the two projections made for Question 1 to estimate BAF for higher roundwood use in the Southeast – A No Wood Case and a With Wood Case - it is intended that the With 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 (e.g. FASOM-GHG) 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 With 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 FASOM-GHG so the revised projections of the With 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 FASOM-GHG, starting 15 years ago, for the No Wood Case.
E. Fifth, use the projections (starting 15 years ago) of the With Wood Case to the No 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.

Table L-1. FASOM-GHG Emissions Components Matched with *BAF* Equation Terms.

FASOM-GHG Emissions Component	Southeast Roundwood
Agricultural LUC and Soil Management Carbon Flux	*SITETNC*
Logging Residue Decay Flux	*AVOIDEMIT*
Afforestation Harvest Flux	*GROW*
Afforestation Tree Carbon Flux	*GROW*
Existing Forest Harvest Flux	*GROW*
Existing Forest Tree Carbon Flux	*GROW*
Afforestation Litter and Understory Harvest Flux	*SITETNC*
Afforestation Soil Carbon Flux	*SITETNC*
Afforestation Litter and Understory Carbon Flux	*SITETNC*
Deforestation Soil Carbon Flux	*SITETNC*

Existing Forest Litter and Understory Carbon Flux	*SITETNC*
Existing Forest Litter and Understory Harvest Flux	*SITETNC*

Assessment/ validation step 2 - Consistency check with assumptions

For the 15 year historic time period is there consistency with the assumptions? :

1) There will be (essentially) no other material used by stationary sources over the projection period or 2) the BAF estimate is independent from (and the same as) any case where, for example, ag biomass use is allowed to meet demand in competition with roundwood in the same region also.

If the side assumptions do not hold, for example if there has been a significant increase in use of agricultural biomass, or logging residue for energy in the region the With Wood Case would need to be further adjusted to track non roundwood use over the last 15 years and include an assumption about projected non roundwood use. The BAF would need to be further revised.

Correcting the BAF after each assessment

It seems 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 compare a With Wood Case that matches history and a No Wood Case with all the same historic drivers except roundwood use for energy. Prior to 2060, in 2025 and each decade thereafter, 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 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, maybe for BAF(2035), there could be new projections for 50 years to 2085. These projections would estimate average BAF for the period 2035 to 2085.

It seems 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.

Validation of model projection based estimates of BAF – Multiple biomass sources for one region

Consider an additional two questions asking for BAF estimates

Question 2- Compared to a no biomass use case for energy, if we assume market demand for biomass by stationary sources can be met by roundwood, logging residue, mill residue or urban wood residue(?) (according to AEO projections) what would be the net CO2 emissions per unit of biomass CO2 emitted (year by year or cumulatively) from 2015 to 2060?

> Question 3 - Compared to a no biomass use case for energy, if we have market demand for biomass by stationary sources (following AEO projections) that can be met by roundwood, logging residue, wood mill residue, perennial crops or agricultural residue what would be the change in net CO2 emissions (year by year or cumulatively) from 2015 to 2060?

Question 2 asks for a BAF that would cover all forest-based wood biomass received at stationary sources in a region (excluding short rotation woody crops). Question 3 asks for a BAF that would cover all biomass received at stationary sources in a region.

For the first question the With Wood Case projections would estimate, over time, – based on market forces – the fractions of wood coming from each wood source. There would still be an assumption that agricultural residues or perennial crops would not be used or such use would not influence the wood BAF estimate.

For the second question, the With Wood Case projections would estimate the fractions of all biomass coming from each of the forest-based and agricultural sources.

Assessment/ validation steps 1 (A-E) above could be done in 2025 for BAF generated in response to questions 1 and 2. But for steps B, C, and D there would be additional comparison and revision to better track the shares of forest-based wood (Q1) or shares of forest-based wood and agricultural biomass (Q2).

The Assessment/ validation step 2 – check with consistency of assumptions – would be needed for Question 2 – Is it true that no ag biomass was used at stationary sources (or such use has no effect on estimated BAF)? A further revision to FASOM-GHG runs may be needed to include track historic and project non-forest-based wood use.

For Question 2 – where there is one BAF for all biomass there no assumption that some sources do not change and this consistency check is not needed.

Correcting the BAF after each assessment

The correction process for BAFs of aggregates would be the same as for single biomass sources. But the sources of error in projections and the complexity of getting aggregate projections to track history may be greater.

Assessment and correction by using more than one model to project with and without biomass scenarios.

Another technique that literature suggests could improve projections needed to estimate BAF values would be ensemble projections. The ensemble projections are combinations (e.g. averages) of projections from 2 or more models. Each model would be well grounded in theory and data but the errors for predictions over time are believed to have low correlation and thus would be compensating. A conclusion of literature across a range of types of forecasts is that forecast accuracy can be improved by combining multiple forecasts (Clemen 1989). If the proposition that ensemble forecasts are more accurate is accepted then ensemble projections could be used to estimate BAF values in place of projections from a single model. At each assessment point the proposition that ensemble forecasts are more accurate could be tested to a limited degree - Does an ensemble "projection" over the historic period, using actual drivers over the historic period, give better tracking of history than the individual model projections (using historic drivers)? This is only a partial check because what is being forecast to estimate BAFs is the difference between a possible actual case and a counterfactual case. It is not possible to directly check the forecasted difference estimate.

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

The retrospective assessments of BAF values for individual types of biomass are may require corrections at each assessment 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 (from stationary source or resource surveys)	Weight of each type of biomass used by stationary sources in a region	Weight of each type of biomass used by stationary sources in a region	Weight of each type of biomass 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	Highest?	Medium	Lowest?

adjustments – potential magnitude			

An alternate approach to monitor validity of BAF values for forest-based biomass use (Question 2)

In addition to periodic assessments of previously estimated BAF values, we could monitor for land C changes that would substantially decrease the BAF estimate for forest-based wood biomass and monitor for projected characteristics of forest carbon change that support estimated BAF values.

For example, simulations have shown (Latta et al 2013, White et al 20xx) that harvest of wood for energy that converts forest to agricultural use, even perennial biomass energy crops, will substantially reduce or eliminate the net emission reductions from use of wood biomass for energy. So, it could be of value to monitor the absolute level of conversion of forest to agricultural land and specifically forest conversion to agricultural perennial energy crops.

Afforestation of agricultural land would be consistent with (but not proof of) wood biomass markets supporting investment in afforestation.

Forest conversion to plantation or afforestation to plantation would be consistent with (but not proof of) wood biomass markets supporting investments in plantations (which may in part provide biomass for energy).

References

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Comments from Dr. Tris West

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

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 by DOE EIA and by EPA annually.

A default schedule of emissions (i.e., initial release of carbon and subsequent annual increments of carbon uptake) may help track annual net emissions from stationary sources. However, this appears to be outside the scope and purpose of the EPA Biogenic Framework, which is to estimate total net emissions associated per unit of biomass. Additionally, the ability to track net emissions is already inherently included in the US GHG Inventory, albeit not attributable specifically to stationary sources.

In summary, it is not recommended that the Framework be used to estimate or record net emissions into the future associated with stationary sources. Instead, such calculations can occur annually during the process of revising regional BAFs. Annual revisions based on regional analyses of net emissions will also enable changes in the temporal policy horizon as needed to ensure long-term sustainable management of land resources.

Comments from Dr. Peter Woodbury

Overall Comments

1. **(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.

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

3. **(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.

4. **(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.

5. **(Topic: Time scales**). The social cost of carbon discount rates analyzed should include zero to account for temporal (for example intergenerational) equity. Note that this does not mean that there are not greater benefits to reducing GHG emissions sooner rather than later.

6. **(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. Note that for current users of biomass for energy, accounting should be retrospective, not prospective. This retrospective analysis should account for effects such as anticipatory planting that may have occurred in the past.

Question 2b.

Preliminary Individual Comments from the Biogenic Carbon Emissions Panel

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 general comments, particularly #1, 2, and 6. 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. Because forest feedstocks may have long rotation periods, it may make sense to focus on them for modeling.

Question 2e. See general comments, particularly #1, 2, and 6. 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. Because forest feedstocks may have long rotation periods, it may make sense to focus on them for modeling. Also, if one of the co-products has higher market value, such as roundwood compared to wood residue, it may be likely that harvest rates are driven primarily by the higher valued co-product.

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 Framework (main document)

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 #6.

<u>Appendix L.</u>

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 #6.

<u>Appendix M.</u>

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