

**Comments from Members of the Chartered SAB on the SAB Draft Report:
Quality review of the Draft (2-8-2016) SAB review of Framework for
Assessing Biogenic CO2 Emissions from Stationary Sources (2014)**

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Comments from Lead Reviewers

Comments from Dr. Steven Hamburg

The SAB subcommittee on biogenic carbon accounting has been asked to take on a challenging topic with a challenging charge. The committee has produced a report that raises some critical issues and brings forward important recommendations for US EPA to consider. Regrettably the draft report is dense and overly policy prescriptive. The report requires significant editing to pare it down to clarify the key concepts as well as strip it of the non-science based judgments. Particularly troubling is the treatment of time.

Revision of the draft report is feasible but will require significant effort. Below I provide comments on each of the major recommendations followed by specific comments on the report.

Recommendation 1 - For proper scientific evaluation of a biogenic carbon accounting approach, the EPA should specify a policy context, propose specific BAF calculations and values, and specify its legal authorities over upstream and downstream emissions as well as the spatial boundaries for assessing emissions associated with a stationary facility. It is also important to have more clarity on underlying expectations about other prevailing land use management, renewable energy and carbon policies that could impact the choice of feedstocks and their production methods and thus the estimates of their BAF.

Comment: While I agree that the policy context is critical to the design of an implementable carbon accounting approach the request for defining the wider set of exogenous variables is inappropriate as they may or may not be related to the policy context to be considered. This conclusion and the underlying text needs to be modified to reflect that some of the ‘desirable’ information and constraints are likely to never be available. This lack of clarity influences the ability to implement some of the details described in the report – particularly as relates to establishing the reference baseline. The report should consider the influence of these likely uncertainties on the ability to project a baseline and the strengths and weaknesses of doing it using a variety of approaches. The report falls short of providing the guidance required and critically examining the possible options.

Recommendation 2) The appropriate time scale for calculating a BAF is the time period over which all terrestrial effects on the stock of carbon on the land occur in response to a policy induced shock in sustained demand for bioenergy. Thus a cumulative BAF metric is appropriate.

Comment: This recommendation is very problematic and muddles two ideas one supported by the science and one unsupported. This recommendation and the underlying text needs to be revised to extract policy prescriptive language and recommendations. As written the treatment of time presumes that the SAB understands the policy outcomes that are desired and the temporal elements of such outcomes. Neither is known and they cannot be scientifically determined – thus not within the purview of the Science Advisory Board. There are many possible policy goals related to addressing climate change and the temporal aspects vary widely, yet the draft report

suggests that the net changes in carbon stocks should be addressed over 100 years for minimization of long-term maximum temperature change. Such a recommendation is well beyond the scope of the charge or what can be determined scientifically.

Recommendation 3) The appropriate cumulative metric for calculating BAF will depend on the scientific assessment of mechanisms by which changes in atmospheric carbon stock affect the climate, with consideration of climate and carbon cycle uncertainties. An alternative cumulative BAF metric is offered in this SAB report that takes into account the changes in terrestrial carbon stocks *over time*, thus incorporating the time course of carbon emissions.

Comment: this recommendation is supported by the science and is an important insight. The key is ensuring that the cumulative metric is not temporally prescriptive, but rather reflects the net radiative forcing impacts for whatever period of time is selected in the policy context. This distinction is not currently clear in the body of the report and thus requires clarification. The selection of a timeframe is a policy decision that involves tradeoffs for which there is no scientific basis that can be deployed. Do we value this generation more or less than five generations from now? The decision of how to tradeoff present damages for future damages or the ability to find alternative mechanisms for mitigating future damages is outside the bounds of this report. The report needs to be explicit on this point. The first sentence of the recommendation is outside the scope of the report and should be eliminated – or significantly reworked.

4) A BAF formulation based on changes in carbon stocks (terrestrial pools such as live, dead, soil, products, material lost in transport and waste) is preferred over an emissions (flux-based) approach because it comports with conventional carbon accounting, has well-defined boundaries and follows conservation of mass as well as mass balance.

Comment: this recommendation provides a critically important insight that needs further development in the text. This is one of the most important findings of the report but the operational difficulties of using flux rates is not fully detailed nor the operational advantages of using stocks explained. The presence of a strong historical record of stock changes is critical to understanding the value of basing biogenic carbon accounting on stock changes, but the value of this observation is not developed.

5) EPA should identify and evaluate its criteria for choosing a model and modeling features that affect BAF outcomes, including both model structure and assumptions about economic and biophysical parameters. EPA should also update and validate the model to incorporate the latest scientific knowledge while ensuring that the model outcomes are consistent with the observed reality.

Comment: this recommendation states the obvious, simply stated ‘use models supported by good data and science’. The challenge is that in the text the discussion drifts far from this recommendation and assumes much about what the best models would be *a priori*. The report needs to critically examine if econometric or other models have and can provide accurate projections of carbon stock changes in the absence of bioenergy demand. The assumption that such models actually work is not born out by the very limited literature in this field (see Buchholz et al. 2014, Nature Climate Change 4:1045-1047). A neutral review of the accuracy of

simple assumptions versus complex econometric models needs to be included for this recommendation to provide anything useful to the underlying challenges faced by EPA.

General comments:

- Overall the report struggled to clearly define the key issues that should be considered – it struggled to describe the forest and rather focused too much on the trees (pun intended) –
- The IPCC in its Fifth assessment report and the broader scientific literature increasingly recognize that the selection of temporal context is not a science question and depends on the policy objectives and context (see J. Shoemaker, D. Schrag, M. Molina, V. Ramanathan, What Role for Short-Lived Climate Pollutants in Mitigation Policy? *Science* **342**, 1323-1324 (2013))
- There is a growing body of literature about the net radiative impacts of fuel switching that is highly germane to the subject of this report that has been totally ignored (see R. A. Alvarez, S. W. Pacala, J. J. Winebrake, W. L. Chameides, S. P. Hamburg, Greater focus needed on methane leakage from natural gas infrastructure. *Proceedings of the National Academy of Sciences*, (2012))
- The draft paper seems to implicitly assume that all or much of the forests/biomass of interest are being actively managed when in fact the vast majority of timberlands and the potential biomass is not actively managed and not held in industrial/TIMO ownership. This point needs to be explicitly addressed as it fundamentally affects management decision making and the ability to predict those decisions *a priori*. This challenge is one of the reasons that decades of trying to predict timber production/availability/forest growth has been so unsuccessful (see Buchholz et al. 2014, *Nature Climate Change* 4:1045-1047).
- The introduction of the ton-year is highly distracting and unnecessary. The concept of cumulative emissions is very important and does not require the ton-year metric and serves the required purpose. If the relative impacts of different energy sources need to be considered the technology warming potential metric can be used to provide the required climate impacts (see R. A. Alvarez, S. W. Pacala, J. J. Winebrake, W. L. Chameides, S. P. Hamburg, Greater focus needed on methane leakage from natural gas infrastructure. *Proceedings of the National Academy of Sciences*, (2012)) This approach is based on well-established GWP factors.
- Need to address the simpler empirical baseline determination approaches that have been proposed, and that many believe are more robust and much less easily manipulated than the modeling approaches put forward by EPA or discussed in the draft report.
- Overall the report needs to more clearly state the complexity of the issues being addressed and explain why they are complex – what the contrasting considerations are. The draft report currently makes the underlying science less available to interested parties though its dense writing, rather than providing the desired increased clarity.

Specific Comments

Page/line

5/4-9 Beyond the scope of the report and inappropriate as the assumption that maximum temperature is the metric of importance is policy prescriptive. In fact there is currently no US policy that focuses directly on this metric and there are many other goals that make policy and scientific sense. While the UNFCCC does use this metric, that policy framework is not the focus of this report.

9/34-44 Need to encourage non-CO₂ ghg to be included, given the varying half-lives and the impacts those would have on the net radiative forcing over time.

11/5-12 While the spatial scale is important and has the potential to introduce greater variability into the BAF of any specific feedstock; the text misses the opportunity to detail how this would affect accuracy versus precision. Again this text is implicitly policy prescriptive. Is the goal accuracy or precision and why. This is a key question that is left unaddressed to the detriment of the report and the goal of advancing the civil society conversation of the underlying set of issues.

13/13-18 The phrase ‘that the model outcomes are consistent with the observed reality’ is key yet the rest of the recommendation and the text does not explore how to ensure this is in fact the case and the degree to which simplicity or complexity will best yield this result.

14/41 ‘influence on peak climate response’ is once again policy prescriptive, it can be included but other potential policy goals/metrics need to be examined as well.

14/20 Discussion of carbon pricing is inappropriate for this report.

16/31-33 “The appropriate time scale for calculating a BAF is the time period over which all terrestrial effects on the stock of carbon on the land occur in response to a policy induced shock in sustained demand for bioenergy. Thus a cumulative BAF metric is appropriate.” The first sentence is policy prescriptive and inconsistent with how accounting for ghg emissions are handled more broadly across gasses and accounting regimes. The second sentence is solid and should be retained.

18/25 Sloppy language – remove reference to decay of carbon molecules in the atmosphere over time. Use uptake of carbon dioxide or reaction of methane etc.

20/30-44 Discussion assumes a temporal context that is not based on scientific first principles and should be reworked to frame the conversation so that it refers to the cumulative impact over the time frame of choice. The assumption that the point at which stocks are stable is the appropriate point from which to calculate the BAF is once again policy prescriptive, implicitly making assumptions about policy objectives. This section needs to be rewritten explaining how the BAF would change over time depending on the time frame selected. In turn calculating the variable impacts of using differing BAFs would be input to the policy conversation. Lines 41-43 raise this point but it is so buried that few will understand the centrality of the policy decision that needs to be made – this needs to be up front and part of the

framing. The temporal decision applies to all BEFs not just that are 0 at some point in time.

21/39-40 The thought that “it is important to continually test this (reference case) assumption against actual data as they becomes available.” Is key and again needs to be central to the conversation – what does post-hoc analyses tell us about how reference modeling has performed to date. This discussion is central, yet completely absent.

22/2-6 This bullet point is not clear – it seems to be making to points one that the impacts have to be looked at on a cumulative basis and a second that the cumulative BAF over a long period of time needs to be deployed. Not clear and the second message is implicitly policy prescriptive.

24/4 Assumes little to no biomass is currently being used in the US, yet it currently accounts for the largest source of renewable energy in the US. Reconciling how existing usage affects baseline assumptions is important and unaddressed.

Comments from Dr. Robert J. Johnston

1) Were the charge questions to the committee adequately addressed?

Yes, the charge questions to the committee were adequately addressed. This is an ambitious review. As noted in the committee report, many of EPA's charge questions addressed narrow technical issues. The relevance of issues such as these are conditional upon a consistent and sound underlying approach to comparing biogenic carbon emissions and deriving relevant Biogenic Assessment Factors (BAFs) across scenarios. This led the committee to recommend broader changes to EPA's BAF calculations than were implied by the charge questions. These recommendations could lead to a more consistent treatment of net biogenic carbon emissions and related BAFs. I agree with the committee that extending the report beyond the original, relatively narrow charge questions is justified.

The committee made two primary recommendations beyond the immediate charge questions posed by EPA. First, the committee recommended calculating BAFs based on changes in carbon stocks rather than changes in carbon fluxes. Second, the Panel recommended a new approach to BAFs, which they denote $BAF_{\Sigma t}$, which is intended to explicitly account for the residence time of carbon in the atmosphere. Intuitively, this may be thought of as a relative difference in carbon ton/years. This is distinct from the EPA's original approach, which models a BAF based on the ratio between net biogenic emissions (NBE) and potential gross emissions (PGE) at a single time t . Although modeling changes in net biogenic carbon emissions is fraught with empirical challenges, the stock-based approach proposed by the SAB appears to have a number of advantages compared to the flux-based approach in the original EPA framework. The motivation of proposed $BAF_{\Sigma t}$ metric is also sound, reflecting the fact that the residence time of carbon in the atmosphere is an important factor (among others) in its climate effects. The current BAFs proposed by the agency do not have a direct mechanism to account for residence time.

The committee's comments—and the proposed BAF models—also support the use of model results to inform such influential factors as temporal scales and national/regional demand, rather than relying on potentially arbitrary judgments. This is an appropriate approach. EPA's charge

questions sometimes imply an intent to specify important components of the model exogenously (the quantity of demand for each feedstock in each region), when some these components may instead be simulated as an endogenous aspect of the model. The committee's comments also highlight the challenges associated with the estimation of a "policy-independent" BAF, as many of the factors that influence BAFs over time are conditional on policy factors.

Finally, I strongly concur with the committee's recommendation that the models used by the EPA for their BAF estimates (e.g., the Forestry and Agricultural Sector Optimization Model, or FASOM) require additional validation, evaluation, justification and sensitivity analysis, including a formal means to characterize uncertainty. The validity and accuracy of BAF calculations depend on the underlying intertemporal optimization model, and forecasts of these complex models are subject to myriad assumptions and uncertainties. For example, FASOM presumes that landowners optimize based on current and expected economic returns—assumed behavior that may or may not match observed behavior. Without careful validation and periodic updating, the performance of such models is unknown.

In summary, the committee report does a good job of responding to the presented charge questions, and also proposing broader improvements to EPA's approach to BAFs. The scope of these recommendations does lead to some additional questions; these are dealt with in #2 below.

2) Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

The SAB Panel's report does a good job of describing a complex set of issues and challenges. However, the scope of the Panel's recommendations—in effect recommending that EPA (1) adjust their fundamental BAF approach to one based on stocks rather than fluxes, and (2) develop BAFs that explicitly account for residence time—also raises a few questions that should be addressed in some way.

First, the treatment and role of carbon stocks in water (within the committee's proposed carbon stocks approach) would benefit from greater clarification. The committee report comments that (p. 18) "the effect on the atmosphere (what the atmosphere sees) from the sequence of biogenic emissions will be the difference in carbon stocks on the land and water," but then explicitly comments that "Neither the EPA's framework nor any modifications we offer take into account ... oceanic uptake of carbon." Hence, stocks of carbon in water do not appear to enter the model. Are there any scenarios in which aquatic carbon stocks are endogenous in a significant way and/or could have non-trivial implications for BAFs? Clearer discussion of the role of aquatic versus terrestrial carbon would improve the report, particularly because a focus on stocks leaves open the question of how much carbon stock is held in aquatic environments (and whether this influences BAF estimation in any significant way). This need not require lengthy text—merely some concise additions to highlight the role (or lack thereof) of aquatic carbon considerations for BAFs, and how these are treated in the committee's proposed approach. This is particularly relevant if the storage of carbon in water has a potentially different impact on the calculation of a flux-based BAF than a stock-based BAF. It is possible that storage of carbon in water has little impact on BAF calculations using either approach. If this is the case, it would be useful if the report could state this.

The committee reports also notes that “there still remain the issues of selecting appropriate temporal or spatial boundaries, considering variability within a class of feedstocks, accounting for non-CO₂ greenhouse gases such as nitrous oxide and methane, and quantifying stocks and fluxes that are difficult to measure or estimate.” The difficulty and relevance of these issues could differ between the EPA’s carbon flux approach and the committee’s stock-based approach. Given this, it would be useful if the report could provide some insight into whether these or other challenges would be increased or decreased by the committee’s proposed stock-based approach. For example, from an empirical perspective, is it easier to quantify stocks or fluxes? Does this difference affect model uncertainty in any significant way? Are challenges with spatial scale increased or decreased by a stock-based approach? Are challenges for non-CO₂ greenhouse gases different across the two approaches? If the challenges related to these issues are essentially unchanged across flux-based and stock-based approaches to BAFs, it would be useful if the report could state this explicitly. Again, this need not be a lengthy addition.

More generally, it would be instructive to include a general statement of the pros and cons of the two competing approaches (stock-based versus flux-based BAFs). The committee report implies (although never states explicitly) that there are few or no advantages of a flux-based approach to BAFs. Is this true? Greater clarity in this area would be helpful, particularly given that the committee is recommending that EPA change its fundamental approach. For example, it would be helpful to know whether there are any significant disadvantages of switching to a stock-based BAF that should be considered.

There are also a few minor typographical errors (or at least seeming errors) that should be addressed. These are listed below.

- Page 13, lines 31-33: “EPA also neglected to quantify carbon storage associated with landfills, and selected a landfill baseline that is inconsistent with regulatory practice. Moreover, the landfill baseline that was selected is inconsistent with regulatory practice.” These two sentences are redundant.
- Page 18, line 1: “We note that the EPA’s cumulative BAF metric is based on changes in carbon **stocks** at any single point in time. There are other approaches to a cumulative BAF metric. One such metric is based on the accumulation of annual differences in carbon stocks...” [bold emphasis added]. Should the bold “stocks” in this sentence be replaced with “fluxes”?
- Page 19, equation (3): Given that the subscript $\sum T$ is used on both NBE and PGE in the equation, should $BAF_{\sum t}$ be restated as $BAF_{\sum T}$ in this equation? So, the equation should be:

$$BAF_{\sum T} = \frac{NBE_{\sum T}}{PGE_{\sum T}}$$

- Page 20, equation (4): Correct subscripts (twice) from “Re *ference*” to “*Reference*”.

3) Is the draft report clear and logical?

Yes, as a whole the report is clear and logical. However, there are a few edits which could improve clarity and readability.

- The section entitled “The Rationale for computing $BAF_{B \sum t}$ Residence Time” (page B-7) is currently relegated to an appendix, but is central to understanding why the committee is proposing the $BAF_{\sum t}$ approach. I recommend that this section be moved into the main body of the report, or at least that some of the primary statements from this section be repeated in the main body. This section describes the issue very well.
- The graphical treatments of NBE and PGE in the appendices were very helpful to communicating the basic concepts of the $\sum t$ approach. Perhaps one or two of these graphs could be moved forward into the main text to help communicate these concepts.
- Regarding the graphs, note that the colors (e.g., that distinguish different lines on the graphs) do not come through well into a printed grayscale document. This makes the graphs difficult to interpret when printed – many of the lines appear identical when printed in greyscale.
- I initially found it difficult to understand the relationships between Δt , t and $\sum t$, and why the $\sum t$ makes intuitive sense. In fact, this relationship is simple. Perhaps a sequence of equations such as the following would help clarify the concept for readers unaccustomed to the notation. I use PGE for illustrations, but identical notation would apply to NBE.

$$PGE_t = \sum_{t=0}^T PGE_{\Delta t}$$

$$PGE_{\sum t} = \sum_{t=0}^T PGE_t$$

Therefore,

$$PGE_{\sum t} = \sum_{t=0}^T \left(\sum_{t=0}^T PGE_{\Delta t} \right)$$

That is, $PGE_{\sum t}$ “counts” each $PGE_{\Delta t}$ multiple (T-t) times, depending on how long each remains in the atmosphere. For example, $PGE_{\sum t}$ counts $PGE_{\Delta 1}$ a total of T-1 times, reflecting its residence time. $PGE_{\Delta 2}$ has a residence time of one fewer years, and is hence included in $PGE_{\sum t}$ only T-2 times. These equations, perhaps combined with a simple graph of PGE over time, could possibly help some readers grasp the concept more quickly and easily. This information is all implied by the information that is currently included in the report (e.g., on pages 18-20). However a more sequential and concrete exposition of relationships might make the concepts more immediately accessible. It would also clarify why $\sum t$ is a more intuitive approach to BAFs.

4) Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes. As a whole the conclusions and recommendations are supported by the body of the draft report. As noted above, the report would be enhanced by a general statement of the pros and

cons of the two competing approaches (stock-based versus flux-based BAFs), given the committee's strong preference for the former.

Comments from Dr. Surabi Menon

Q1: Were the charge questions to the committee adequately addressed?

The framework looks at the extent to which production, processing and use of biogenic material at stationary sources results in a net atmospheric contribution of biogenic CO₂ emissions.

The charge questions to the committee were related to a future anticipated baseline approach and temporal scale (how policy and other factors may impact the the Biogenic Assessment Factor (BAF) for carbon emissions associated with combustion of biogenic feedstocks); scale of biogenic feedstock demand changes and the impact on biogenic CO₂ emissions, without links to any particular policy frameworks. These charge questions were addressed well by the committee.

The committee in particular provided a useful recommendation on accounting for biogenic carbon using mass conservation formulations that is carbon stock based (and not carbon emissions based as considered by the EPA) and provided very specific details on the formulation as well as the use cases in the Appendices of the report. While I did not go into details of the formulation and application, it provides sufficient information to guide the development of an accounting framework.

On time scale, they also recommended the use of cumulative BAF rather than one at the end of the time horizon and one that can capture all effects and be the same across feedstocks and policies. They also provided useful recommendations on value of reducing cumulative emissions versus likelihood of tipping points in the future. Additionally, in the absence of specific policy applications that can be used to capture scale of the demand change for a future anticipated baseline approach, the recommendation of capturing demand changes at incremental levels of demand using BAF from a simulation model is a good suggestion.

In summary, both charge questions (and sub questions thereof) were addressed more than adequately.

Q2: Are there technical errors or omissions or issues that are not adequately dealt with in the draft report?

None that I came across. Lack of a charge question on the alternate fate approach of waste-derived feedstocks was pointed out by the panel as important to consider, especially electrical energy recovery from landfills and combustion and carbon storage from landfills. Though it was recognized that this was outside the boundary of the framework, it still is a useful suggestion for the EPA to consider.

Q3: Is the draft report clear and logical?

The review in particular recommends that the Framework developed include specific policy context and BAF for the application rather than offering a choice of options. It offers suggestions on the calculations of a new approach and guidance regarding use. The new calculations are

expected to be mass-balanced and based on carbon stocks rather than emissions based. This alternate suggestion seems acceptable. The supporting case for using this approach is well presented.

The report is well laid out. It includes a general response to specific charge questions and lists major conclusions and recommendations. It further includes via an appendix more details on recommendations (for the new suggested BAF formulation and its application).

The review report contains references to a previous assessment on the same topic, and recommendations provided for that framework in 2012 were incorporated in the new 2014 framework that was reviewed by the panel/committee. Since I don't have sufficient context for that work, it was sometimes confusing to understand references in this report for the earlier work. However, that does not deter from agreeing that the conclusions/recommendations included in this report are presented well.

A summary/recommendation section is included within the report in various sections and also for the Charge Question 1. One suggestion would be to follow the same formatting and include a similar recommendation section for Charge Question 2.

Q4: Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes, for the outlined charge questions and recommendations, conclusions are well supported within the draft report. The report also outlines recommendations for further advancing biogenic carbon accounting within a policy context that is not the subject of the present review. This is helpful for future consideration.

Comments from other SAB Members

Comments from Dr. Kiros Berhane

1) Were the charge questions to the committee adequately addressed?

Yes, the charge questions were adequately addressed. In fact, the committee went beyond the charge questions to address several overarching issues. The committee's claim that EPA's instructions not to take policy context into account have limited its ability to fully evaluate certain elements of the report is well taken. I also fully agree with the committee's argument about the need for thorough model validation evaluation, justification and sensitivity analysis.

2) Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

No, there are no technical errors or omissions in the draft report. All issues have been addressed adequately, including several additional issues that were identified by the committee.

3) Is the draft report clear and logical?

Yes, the draft report is very well written and logically organized.

4) Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes, the conclusions and recommendation are well laid out and adequately justified.

Comments from Dr. Ingrid Burke

1. Were the charge questions adequately addressed?

The charge questions were addressed adequately. A very large proportion of the report, however, seems to range far outside of the charge questions (6 pages relative to 10 that focus on the charge questions). It seems that the charge questions were focused on specific technical details of the revised Framework. I will appreciate hearing a discussion about the appropriateness of the much larger response.

I found the section beginning on page 14, line 29, about the impacts of greenhouse gases on climate dynamics on earth, and how long it takes greenhouse gases to influence climate and sea level, etc, to be particularly inappropriate. It is outside the scope of the report.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the draft report?

Not that I could see.

3. Is the draft report clear and logical?

I found there to be a good number of places where the text was difficult to read and understand, both because of logic flow and quality of writing. The Executive Summary needs rewriting. Paragraphs are poorly organized and sentences interminable, particularly in that section, but throughout the report.

As I note above, I would like to hear the SAB discussion about the major section of the report that is an “overall summary” rather than focusing on the charge questions.

Under the Summary of Major Conclusions and Recommendations (~ page 6), I have concern about number 3, that the appropriate metric for BAF depends on the mechanisms by which atmospheric carbon dioxide influences the climate, and climate feedbacks. That is beyond the scope of the problem, and requires a whole different set of experts. The Framework focuses on carbon emissions, period. At the end of that summary, there is a statement about how using a longer integral over time will be better, which I agree with, but it does not decrease any uncertainties. Once you start to aggregate uncertainties across landscapes, associated with how direct and indirect land use will change, whether and how carbon will accumulate in the recovering ecosystem, there is plenty of uncertainty. Addressing how one might estimate that uncertainty would really strengthen the Framework, but it was not part of the Charge Questions

to the committee.

In the introduction, there are comments about emissions that are avoided when feedstocks are used for bioenergy, and it mentions uncapped landfills and decomposition of wood mill waste. This section was unclear. Burning those wastes for bioenergy rather than allowing them to decay in situ does not avoid emissions, in fact, it speeds up the process. Perhaps I am missing something, but this paragraph is important so it should be made clear. As the committee likely knows, the lifecycle assessment of many feedstocks does not work out to avoid emissions over some fossil fuels.

I was unsure about why there were so few true citations (essentially none). The National Academy report to EPA about the economic and environmental impacts of RFS seems particularly germane, and yet was never cited at all.

The report does not refer to direct and indirect land use change associated with feedstock use, nor is it very clear about the different turnover rates of feedstock types and why this might necessitate different calculations and time scales.

The term “intertemporal” is used several times in places where its absence would be an improvement in simplicity and straightforward communication.

Model “validation” is just about impossible for this kind of work. I recommend continuing the recommendation on improved model evaluation and sensitivity analyses, and removing the term “validation”.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

I have concern about the recommendations related to the “overview” portion. I would like to hear discussion by the Board about the metric recommendations as well.

Comments from Dr. Michael Dourson

1. Were the charge questions adequately addressed?

This is not my area of expertise but the committee appears to have been very well appointed and appears to have addressed the given charge questions very well.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the draft report?

Please see response to question 4.

3. Is the draft report clear and logical?

Yes, the report was very easy to read and it generally made sense.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Many of the conclusions seemed reasonable. Other lines of thought were not as clear. In particular,

- I have a vague recollection at a prior SAB meeting of mentioning the use of biochar as a way of both amending soil depleted by extensive farming and also carbon capture. Perhaps I missed it, but biochar is not mentioned in this report. Is this not a helpful option, or perhaps this is one option among many that does not need a separate discussion?
- Why is a tipping point of concern in the discussion of time scale if biomass is being substituted for fossil fuel? Or does it take more biomass, and therefore more CO₂ emissions to net the same energy as fossil fuel?

Comments from Dr. Joel Ducoste

1) Were the charge questions to the committee adequately addressed?

Yes overall.

I would like to see more detail or examples in certain parts of the document. For example in Charge question 1 (temporal scale and the future anticipated baseline approach), the report recommends the same temporal scale for all feedstocks to make appropriate comparisons of their BAFs. While I agree with the recommendation, it's not clear to the reader how large an error would occur if comparisons between different feedstocks were not on the same time scale. I think a simple example would help clarify this issue.

Although the charge questions related to shock has been adequately addressed, as in the previous comment above, I think a simple example added to the appendix would help illustrate the points made in 2a-e.

2) Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

There were a couple of instances where BAF (likely due to a typo). These occurred on pages 15 and 19.

T, BAFT, PC

3) Is the draft report clear and logical?

Yes. However, I think that the addition of some figures within the body of the text would help the reader instead of putting it all together in Appendix C and D. I think some key figures (not all, of course) could help with the reader's understanding of the issues expressed by the response to the charge questions.

4) Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes

Comments from Dr. Susan Felter

Overall, the Draft SAB report is well-written, follows a logical flow, and responds directly to the charge questions posed by the EPA.

1. Were the charge questions to the committee adequately addressed?

Yes, the 5 charge questions were adequately addressed.

2. Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

None that I am aware of, but this is not my area of expertise.

3. Is the draft report clear and logical?

Yes, the draft report is very well organized, clear and logical.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes, the conclusions drawn are supported by the body of the report. That said, the language is not always clear as to whether the SAB is offering an alternative (but still accepting EPA's current approach) or truly recommending a change. For example, in the cover letter, p. iii lines 25-26, it states "*In sum, this report offers an alternative cumulative BAF to take into account changes in terrestrial carbon stocks over time, thus incorporating the time course of carbon emissions.*" The report describes the advantages to this approach and the basis for this statement, but I think it would help to be more clear as to whether this is just being offered as an alternative or if it is truly a recommendation to change the approach used by the EPA.

Comments from Dr. Sue Marty

1. Were the charge questions adequately addressed?

Yes, there is considerable discussion on temporal scale and its associated tradeoffs and model perturbations (shocks) for future anticipated baseline simulations. The SAB review also provides guidance on the formulation and use on Biogenic Assessment Factors (BAFs) with the recommendation to use cumulative BAF.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the draft report?

Not to my knowledge. There is a typo on page 20, l. 13-14.

3. Is the draft report clear and logical?

Yes, for such a complex topic, the report is well written.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes.

Comments from Dr. Kristina D. Mena

- 1) i Were the charge questions to the committee adequately addressed?

Yes

- 2) Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

No

- 3) Is the draft report clear and logical?

Yes

- 4) Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes

Comments from Dr. Thomas Parkerton

Quality Review of "SAB review of *Framework for Assessing Biogenic CO₂ Emissions from Stationary 16 Sources* (2014)"

1. Were the charge questions adequately addressed?

The report has adequately addressed the charge questions posed by EPA. The report also includes general comments (e.g. model validation, sensitivity and uncertainty analysis) that should be helpful in transparent application of this framework by the Agency in future policy contexts.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the draft report?

In the description of BAF estimates described in Table on page D-21, lines 34-36 the draft report states:

"In the cases in which the BAF is negative and the policy scenario leads to a decrease in carbon stocks relative to the reference scenario, both the BAF_T and the $BAF_{\Delta t}$ tend to be higher (e.g., less negative) than the proposed $BAF_{\Sigma T}$."

This presumably is a typo as negative BAF values reflect an increase (not decrease) in carbon stocks of the policy scenario relative to the reference scenario. Please correct this error.

3. Is the draft report clear and logical?

The report is clear, logical and well written. Several editorial comments are provided for consideration to improve or clarify draft text.

Pg. 2 lines 3-6

Suggest revising text

The EPA's case studies applied the future anticipated baseline approach on a regional basis to Southeastern roundwood, Corn Belt corn stover and Pacific Northwest logging residues, however comments on the modeling analysis or feedstocks used in these simulations were not within the scope the charge questions posed to the SAB.

Pg 2 line 11

Suggest replacing "alternative" with "different"

Pg 9 lines 38-41

Suggest re-wording

The 2014 Framework mentions that methane emissions from biogenic feedstocks are relatively small compared to those from other sources in the United States and also illustrates the implications of accounting for N₂O emissions in BAF calculations. However, for many feedstocks, the global warming potential attributable to N₂O or CH₄ is greater than from CO₂.

Pg 10 line 14

May want to consider adding statement making the point included in the previous section that for transparency Non-CO₂ emissions that contribute to overall GHG emissions need to be acknowledged.

Pg 11 lines 24-26 and 32

See earlier comments on Pg. 2

Pg 18 line 34

Text is awkward "would yield something like" .. please revise

Pg B-8 lines 32-37

Suggest revising to:

In the case of increased harvest intensity, k must increase by n and since:

$$TC_{reference\ T} = I/k > TC_{policy\ T} = I/(k(1+n)) \text{ (EQ. B-24) 35}$$

then in accordance with Eq. B-4, NBE_T must be positive.

Pg B-9 line 5

Suggest adding after equation B-27

"and NBE_T is negative"

Pg B-9 line 6

Suggest deleting “*negative leakage of which*” as unclear what is meant by this text which is not needed to make the point that is trying to be conveyed

Pg B-9 line 44

Correct typo “*the use of fate of products*”

Pg C-7 line 39

Please clarify equation in Appendix B you are referring to, i.e. B-21 ?

Pg D-1, line 12

Think reference here is to Appendix C not B, please correct.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

The conclusions drawn appear to be adequately supported by the body of the report. The examples provided in Appendix D are particularly helpful in illustrating how the alternative method to compute BAF differs from that proposed by EPA.

Comments from Dr. Tara Sabo-Atwood

1. Were the charge questions to the committee adequately addressed?

Yes. There were several charge questions (with sub-parts) and as a non-expert in this particular area, I felt the panel did address each charge question adequately.

2. Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

No, I did not identify any technical errors or omissions/issues in the report based on my area of expertise.

3. Is the draft report clear and logical?

Yes. The report is highly technical and therefore can be quite dense (perhaps somewhat due to the report being outside my area of expertise) – but does seem to follow each charge question(s).

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes. In general the conclusions and recommendations are supported by the report. It would be helpful to have a comprehensive ‘summary recommendation’ paragraph for charge question 2 – similar to the format for charge question(s) 1.

Comments from Dr. William Schlesinger

I wish to submit the following comments on the *Draft Framework for Assessing Biogenic CO₂ Emissions from Stationary Sources (2014)*, which is on the agenda for discussion by the SAB on 31 March.

This version of the report represents a major effort by the subcommittee to tackle a difficult and multi-faceted issue, covering a large range of spatial and temporal scales. I have no doubt that some forms of biomass, especially grasses and existing short-rotation plantation trees, can offer valuable contributions to reducing our nation's CO₂ emissions by providing "biofuels" to replace fossil fuels. Nevertheless, I believe that the draft report's recommendations concerning woody biomass are much too general and thus dangerous to the environment.

The time-frame is of essence. When one cuts large trees, any trees planted to replace them will require many decades to recover the carbon that was previously stored on the site. During that interval, the CO₂ burden in the atmosphere is higher. Climate scientists say that we have but a decade or so to avoid the impact of the worst scenarios of global warming, so a short-horizon time frame is most relevant. We need to promote activities that will reduce the CO₂ emissions to the atmosphere right now—not mitigated during the rest of the century, in which harmful climate change may be "locked in." Policies that allow the cutting of large, old trees will not help us. The report treats all biomass as equivalent.

Secondly, we need to consider the type and quality of the forested landscape in the United States. If old-growth forests are replaced by short-rotation plantation forests, the value of habitat for wildlife conservation and preservation of biodiversity is much reduced, as reviewed by Root and Betts (2016, *Journal of Forestry* 114: 66-74).

Differing from the subcommittee, I have significant doubts that cutting more trees results in planting more trees, and I find the evidence for that assumption weak in the draft report. The recent paper by Nandts et al. (2016, *Science* 351: 597-600) finds that decades of forest management in Europe reduced forest area and carbon storage. We can rely on hypothetical economic models of how land owners *should* behave, but why should we believe that reality will follow them? And, will we be pleased with the type of forests that will dominant the landscape under this practice?

In sum, I believe the report should be returned for further work that clarifies how the EPA will recognize the type of biomass being delivered as a fuel, and specifies that biomass older than 25-30 years is unacceptable, as it will not provide a carbon-neutral alternative to fossil fuels.

Comments from Dr. Edwin Van Wijngaarden

1. Were the charge questions adequately addressed?

- The charge questions are adequately addressed, though it could be clarified that the response to the overall charge question 1 (on page 16 of the report) comprises in effect responses to charge questions 1a and 1b. Similarly, on pages 4-5 of the report (executive summary) it may be helpful to clarify to which subparts of charge questions 1 and 2 each of the paragraphs is referring.
- In the body of the report there is some inconsistency in chapter 4 of the draft review in

terms of providing recommendations to specific charge questions, with recommendations provided only specifically for charge questions 1 and 1c and not for charge question 2. A recommendation for question 2 is provided in the executive summary however.

- While charge question 1 asks about criteria that could be used, the response does not specifically provide a list of criteria. This may be due to the nature of the charge questions or due to the complexity of the issue.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the draft report?

- None that I am able to identify.

3. Is the draft report clear and logical?

- Yes. The draft report is clearly written. The inclusion of Chapter 3 on matters that lie outside the scope of EPA's charge questions is logical and provides a context within which to interpret the subsequent responses specifically addressing these charge questions. Combing the responses to some of the charge questions is logical. Perhaps the inclusion of question-specific recommendations in Chapter 4 could be done more consistently across all charge questions.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

- Appendix B-D provide the detailed basis for the recommendations in the report.

Comments from Dr. Jeanne VanBriesen

1. Were the charge questions to the committee adequately addressed?

Some were, but some were not. The report structure is unusual in that the early sections are more general and not until section 4 are the charge questions addressed.

The EPA SAB is to be commended for expanding the charge questions when necessary to allow adequate input on critical components, such as the modeling method and limitations (section 3.3). The SAB correctly indicates that since the choice of the model can change the results of the analysis (p 2, lines 10-11; p 11, lines 31-33), EPA should clearly articulate the model details, including how variability in feedstocks and economic decisions are modeled; how the model would be calibrated; how frequently it would be updated with new data; and its sensitivity with respect to various parameters. Additional discussion of this issue is covered under #2 below. The SAB should review the model details when EPA provides them.

The charge questions were very narrow, focused on technical points in the structure of the Framework. However, some of the responses are broad and general and do not approach the level of specificity suggested by the questions. Without specificity in the answers it is difficult to see what EPA will do with the response the SAB provides. The SAB should add clarity or

state explicitly that answers cannot be provided based on current knowledge or without additional input from EPA (e.g., the policy frame or modeling details).

Charge question 1 and 1(a)(i), 1(a)(ii), 1(a)(iii) and 1(b). The response does not provide adequate justification for the support of the ‘emissions horizon’ or for stating that the temporal scale should not vary by policy or other metrics or that the “assessment horizon” should not be used. The report indicates that effects work on different time scales (p. 16 line 12) and yet recommends the same time scale for all feedstocks to ensure comparability. Justification for the need for long-time frame comparability (rather than short time frame comparability) is not provided and should be included. The paragraph ends with a weak statement “could be used” (p. 16 line 15). Is the SAB stating that the longest of the time scales “should” be used? If so, change ‘could’ to ‘should’ and provide adequate justification for this assertion. Similarly the following paragraph ends with a ‘could’ as well (p. 16, line 28). Since the bulk of the answers to the sub charge questions after this rely on this initial answer and refer to the ‘principle’ asserted in this section, the SAB should justify this principle with references to its application in the peer-reviewed literature or analysis showing it to be a valid approach.

The SAB should provide additional support, including adequate references, for the apparent conclusion that the best way to assess BAF of different choices is to look at the impacts out as far as any choice is likely to have impacts. Further, and related to the discussion of modeling below, SAB should provide adequate support for the assertion that BAF values calculated at this long time frame, which are based on models with underlying assumptions that may not hold for long time frames or have comparable effects on base and policy cases over long time frames, are valid and useful for comparison purposes.

EPA has asked some very detailed questions that suggest they are struggling with the issue of the right time frame to calculate and compare the BAF and to estimate the effects of biogenic CO₂, particularly within different policy frames. The SAB should take some more time to provide input on these questions, either by expanding the justification for the principle described on page 16 or by individually answering the related charge questions with more support for the overall principle. Without additional information on the reason for the conclusions, I find these sections unconvincing, and I cannot support the recommendation to EPA to use a long time scale for BAF calculations and comparisons. While climate effects at some point in the future are the correct outcome to analyze, there is insufficient support for an assertion that the BAF model correctly predicts these effects at that time scale or adequately captures the differences among base and policy case at those scales. If such support exists in the literature or can be demonstrated through analysis the SAB should provide that support in the response to this charge question.

Charge question 1(c). The SAB introduces a new methodology that enables BAF to account for differences in carbon stocks each year (p. 18, line 35). However, SAB has already asserted (in earlier charge question responses) that the cumulative BAF at the end of all effects should still be used. In this section SAB adds that a one-time cumulative BAF “may not remain an accurate representation of reality over time.” Such contradictory statements can only be reconciled if one makes unsubstantiated assumptions of the model validity over long time frames or if one assumes periodic updating to the model will occur at intervals that overcome problems with the assumptions (p. 21, lines 26-27). Neither of these assertions is made directly in the report, but the use of a long time frame implies them. If the assertions are correct, they should be supported. If

they are incorrect, then the long time frame analysis must be justified some other way. This is discussed further below.

Charge question 1(d). The response that one must *periodically* revise the modeling and BAF estimates *but not too frequently* is too qualitative to be useful. Further, the idea that updating must be based on new data is clearly correct, but data do not just appear, we collect them. What frequency of data collection would be necessary to ensure the updates to underlying economic and biophysical assumptions are frequent enough to ensure the models are adequate? I'd be particularly concerned about the economic models, which appear to be based on many assumptions about how as yet unspecified policies would affect markets that are currently small (biomass for energy) and that interact with large existing markets (wood products, agriculture). Further, they appear to be based on assumptions of stakeholders as rational beings with long term planning horizons. The SAB suggests that human inputs (change in management and land use, p. 22, lines 28-29) alter the physical model responses, suggesting feedback loops where the model is only as good as its underlying assumptions about how people work, rather than how physics works. If this question of model update frequency cannot be answered *a priori* based on SAB review of the model itself, SAB should recommend EPA do additional model sensitivity analysis and calibration to determine what an adequate time frame for revision of the BAF model would be. This time frame for model recalibration must then be considered when advising EPA on how time frame is relevant for BAF comparisons and for assessing the effects of different policy changes (the responses to questions 1(a)-1(c)).

Charge question 2 deals with shocks in biomass demand. I find the SAB responses adequate; however, I remain concerned that the stability and reliability of the model has not been adequately assessed and so a discussion of the response of predicted BAF to shocks is premature. Shocks could have large uncertainty (p. 23, line 38), and it is not clear to me how the model deals with uncertainty in parameters or input values. Further, the response discusses the need for sizing the analysis to ensure a statistically significant effect can be assessed. However, if the BAF is assessed at long time frame, where differences are muted, this will require a larger marginal unit (geographically) to be used than if BAF is assessed at shorter time frames, where policy affected changes may be larger and easier to see through the noise. Also, since the BAF calculation is dependent on feedstock-specific demand and the BAF calculated and incorporated into a policy will affect this demand (p. 26, lines 15-16), the model will be accurate only for short time frames and then will likely diverge, possibly significantly. Model iterations are suggested by the SAB (p. 26, lines 19), but it is unclear if this should lead to a recommendation that BAF be updated at regular intervals based on such model calibration. And, if so, what interval is appropriate given the joint effect of feedstock demand on BAF and BAF on feedstock demand? These questions suggest that the effect of the time horizon choice in response to charge question 1 on the limitations in answers to charge question 2 should be more transparently addressed.

2. Are there any technical errors or omission or issues that are not adequately dealt with in the draft report?

The question of how biogenic CO₂ estimates will be used in conjunction with other GHG estimates (e.g., N₂O and CH₄) is inadequately addressed in the Framework. The review discusses this limitation (e.g., p. 9, lines 36-38), but I think this should be strengthened in the

report and raised to a level of a specific recommendation or incorporated into the first recommendation. The SAB rightly notes in the letter to the administrator (p ii, lines 33) that accounting for these non-CO2 greenhouse gases remains an issue. The SAB report also notes that EPA did not provide a rationale for not acknowledging the importance of all GHG emissions in the Framework (p.10 lines 3-4).

It is difficult to imagine what purpose biogenic CO2 estimates could serve other than looking at climate effects. That the report and the review look at biogenic CO2 emissions without looking at other related GHG emissions (N2O and CH4) that might be affected seems myopic and misleading. The 2011 and 2014 Framework documents, and the related SAB reviews, highlight the significant complexity in assessing the role of biogenic CO2 estimates, especially in the challenges of incorporating policy and market components into the physical models and adequately addressing time scale issues. It is reasonable to assume that similar models for nitrous oxide and methane (especially associated with alternative fate of biomass wastes and fertilizer inputs on biofuel crops) would suffer from the same complexities, and that integrating such models into overall GHG models would be even more challenging. This is also highlighted in the ES p. 3 lines 17-20 and in the report on pg 9, lines 34-44 and page 13 lines 28-37. These additional GHG emissions could have significant technology forcing and feedstock demand effects that should not be ignored in the analysis (e.g., the avoided CH4 associated with burning waste biomass rather than sending it to a landfill is very different from the N2O emissions associated with large scale fuel biomass production).

My second concern deals with model calibration and routine updating as discussed numerous places in the report. Model validation and uncertainty analysis is mentioned as important (ES p 2, lines 21-22; p. 11, lines 32-33; p. 12 lines 21-42) particularly assessing the ability of the model to replicate observed phenomena. The SAB is correct in suggesting that model evaluation can assist with understanding the sensitivity of models to the input parameters (ES p 2, lines 25-26; p. 12 lines 44-46 and pg 13 lines 1-8), which is crucial to understand how frequently the model should be updated with new data. I think the SAB report should include a stronger recommendation that such analyses be done to understand the model sensitivity to input parameters and assumptions as well as to provide insight into the time frame for BAF updates within different policy frameworks. This could be added to recommendation 2 (p. 13).

The SAB report is surprisingly unspecific on the time frame for model updates and recalibration, including in the Recommendation (“periodically update and validate” p. 13, line 16). “continually testing assumptions” and ‘periodic baseline resets’ are called for (p. 21), but it is uncertain how EPA should interpret these recommendations. How often does the model require recalibration, how much data is needed to recalibrate it, and how would frequent recalibrations affect the need for ‘long term incentives for sustainable management of land resources’ (p. 21)? I would like to see an improved summary of the assumptions necessary to use the BAF for projections into the future and some discussion of how uncertainty in the stability of the assumptions would affect the utility of the BAF calculation (this would have to come from EPA for SAB review). Page 21 lines 22-40 deals with some of the major assumptions and advises caution if the framework assumptions are not stable. But, it is unclear to me how large the problems could be if the assumptions are not stable, and how often the assumptions would have to be tested and/or updated with new data. Page 21 Line 39 says “it is important to continually test this assumption against actual data as they become available” but ‘continually’ is not

feasible, and it is not clear to me how often relevant data become available or if the cycle of data availability is adequate for the potential BAF miscalculations that could result from the assumptions being in error. How far off might the BAF calculations be if assumptions about long term sustainable land and forest management do not hold true and if data to support them is assessed on a decadal vs annual basis? This type of assessment of the effects of assumptions on model outcomes should be done so the impact of uncertainty in the assumptions can be evaluated.

Finally, I am concerned with the treatment of time frames in the report. There are several different ways time is discussed, and it is very difficult to follow the differences among the time frame for model predictions and need for model updating, the horizon for estimating and comparing BAF values, and the climate effect time frame.

Starting with the model and its time dependencies. As noted in discussing the model above, the relevant time frame for model updating, and therefore for BAF recalculation, is unclear in the SAB report (and the EPA Framework). It is not clear that short term changes that will affect the physical and economic components are adequately represented with feedback in the model. While the physical climate changes associated with assuming CO₂ sequestered next decade is equivalent to CO₂ released this decade may be negligible (as suggested by statements on page 5 line 4), this does not mean the economic forces will act on that long a time frame or that policy decisions will not lead to significant changes in the assumptions underpinning the BAF calculation. For example, on page 21 lines 23-28, uncertainty in the assumptions of stable management of forests and land use make the BAF prediction subject to significant uncertainty, probably over short time frames. The need to account for a shifting projection of the reference baseline makes this time frame issue particularly salient (p. 21 line 33). What might be relevant for the comparison of BAFs (a long, all effects considered time frame) is demonstrably not relevant for assessing the predictive capability of the model. Several places the SAB report mentions the need to update the BAF model regularly and to check that it is adequately representing real-world data. However, the sensitivity of the model to its internal dependencies is inadequately described by EPA, and the SAB report does not give clear guidance on the regularity of updates needed or the impact of this regular updating on using a long time horizon for BAF comparisons.

The model uses assumptions related to choices people will make that are unlikely to hold true over long time horizons. For example, the assumption that land use and forest management practices would remain stable in response to market forces around biomass-based energy seems unsubstantiated. Do we have recent history (e.g., the last 100 years) to suggest the duration of maintenance of forest and land management practices globally? Is it reasonable to assume such practices will be maintained or would it be more reasonable to assume that they will NOT be maintained. If they are not maintained, how does the appropriate time frame for comparison of BAF values change? And, would a shorter time frame for the model output alter the BAF values? Would using the equilibrium end point comparison hide short duration changes that make the BAF calculation over the full 100 years unsuitable for particular policy frames? Or, would these changes have similar effects on all the options and thus still enable comparison of results at the long time horizon? Without sensitivity analysis of the model parameters and assumptions, it is impossible to know the frequency of model recalibration necessary to give adequate predictive results for comparisons at any time frame.

The alternative BAF method is very well described but it isn't clear if SAB is recommending its adoption or recommending some other appropriate cumulative metric that depends on intertemporal trade-offs (p. 22 line 4-5). The SAB report contends that the alternative BAF method offered in the report allows consideration of time course effects (page iii, line 26), replacing EPA's method which can account for differences only at the end of the time horizon (pg iii, line 18), but it also says that even after this change, 'there still remain the issues of selecting appropriate temporal or spatial boundaries' (page ii, line 32). Thus, it is not clear that the *capability* to include temporal effects (via the carbon stock method rather than the carbon emission method) and to use shorter time frames in biogenic CO₂ accounting translates into the SAB's *recommendation* that such shorter time frames be considered in order to allow for more regular model updates. Further, it is not clear how the carbon stock based method takes into account the intertemporal trade-offs between short term and long term impacts of carbon emissions on the climate system, if it does, unless it does so by using a shorter time horizon for BAF comparisons (which the response to charge question 1 says is the wrong approach). The SAB report should clarify how the new method accounts for trade-offs without considering a short time frame BAF comparison. If the new method is not accounting for these intertemporal trade-offs and SAB thinks that BAF methods should do this, then SAB should recommend that EPA should develop a BAF metric that adequately addresses these trade-offs.

Since the carbon stock approach enables alternative time scales to be considered and uncertainty exists related to human processes (i.e., markets and land management) and physical processes (i.e., carbon uptake rates by biomass in the future when CO₂ concentrations are higher), one method to deal with the trade-offs would be to let the model results and their comparison with real-world data drive the updating frequency. This is obliquely referenced in the SAB report (p. 26, lines 19-22) where iterations of the model to match data is suggested as a way to deal with the joint relationship between calculated BAF and feedstock demand. This would suggest that the BAF computation would have to look at multiple time frames for possible comparisons rather than the single cumulative point in the distant future, but this should be tractable in a numerical model.

The second way the 'time frame' issue comes up is in the appropriate time frame to assess all changes in carbon relevant for the BAF to allow for comparison of BAF for different choices. In this, the SAB says that looking at the long time frame (until equilibrium is reached) is appropriate in order to allow comparison of BAF results. This is conceptually simple and mathematically tractable although I can't understand why a long time frame is necessary given the simplicity of the structure of the model. One could compare the BAF at any time point just as easily (or at every time point as done in the examples). I don't think it is clearly explained in the report why the cumulative effect at a far distant point in the future is the right time frame for this comparison. As discussed above under the charge questions, the response to charge question 1 contains insufficient justification for this approach and far too little explanation in the body of the report for the reader to understand the basis for this *analysis choice*. Further, since the structure of the BAF calculation makes differences in BAF smaller at these longer time horizons, it is more difficult to use BAF to make policy decisions when this long time frame is selected. That makes it more difficult to see the effect of choices made around biofuels, and generally we model things in order to see the differences, not to obscure them. Other time frames could be justified as relevant to the policy arena, related to time constants in the human rather than climate systems, and other time frames might provide insights into how policy choices might affect

stakeholder actions, which are likely on shorter time scales. Significant revision to this section of the report is needed to avoid the current confusion over the difference between this time frame (which is a theoretical look at the future for different choices holding all other things constant) and the climate impact time frame (which is trying to predict actual effects in the physical system).

As noted above in the charge question response, it is not clear to me from reading the report why the SAB does not support the change in temporal scale to fit policy horizon (p. 4 lines 17-19) or why the SAB report supports the ‘emissions horizon’ approach EPA defines (p. 16 lines 18-19). The report should provide additional justification for this *analysis time frame* by referencing components of how the model works or of the stability of its assumptions over those time frames. Or, it could show that differences that occur in predictions when using shorter time frames represent errors when making comparisons rather than important information about differences in outcomes. In the absence of more detailed justification of this conclusion, it would be necessary to demonstrate that making this comparison and applying it in the policy domain wouldn’t lead to short-term effects that distort the market in ways that change the outcome of the BAF calculation.

Finally, there is the issue of the appropriate time frame for climate effects (discussed in section 3.5). The conclusion that cumulative CO₂ emissions over roughly a 100 year period that are similar lead to similar temperature responses is discussed extensively on pages 14-15. However, the report is lightly referenced in this section (e.g., the values of delaying tipping points is mentioned without any references, pg 15, lines 15-17), and as the EPA Framework notes, there are different perspectives on how to assess future emissions trajectories. In some places the report appears to discuss issues around time frames for assessment of climate impacts (p. 3 lines 40-45 and section 3.5, p. 14-15) and in other places it says considerations of related time scale issues are out of scope (issues around uncertainty in tipping points, feedback effects, etc.; p ii, lines 44-45; p. 5 lines 13-17). The reasons for and implications of choosing a relevant time frame should not be discussed some places in the report and dismissed other places. Unless analysis, including adequate review of applicable literature, was done to address the issues related to time frame that the SAB says is out of scope, no conclusions should be drawn, and broad generalizations related to topics outside of scope should be removed from the report.

However, even if such a review were undertaken, it is difficult to see how this time frame is relevant to the assessment of the BAF model. This BAF model (or any BAF model) seems unsuited to *predict* the changes in emissions over 100 years since it is dependent on many assumptions that are unlikely to remain stable for 100 years (e.g., p. 6 lines 12-13), and methane and N₂O would need to be accounted for within the model to look at climate effects in any case. Thus, I do not think this section adds value to the report as it confuses the issue of what time frame to use for BAF comparisons of CO₂ emissions (see previous section) with what time frame to use for climate impacts of all GHG emissions.

The SAB report specifically states that ‘this report does not address the impacts of the magnitude and timing of those emissions on the climate system,’ (ES 3,lines 37-39), but then goes on to conclude that harvesting of trees ‘does not have to imply potential increased net greenhouse gas emissions at longer time scales.’ I would strongly urge removal of this section as it does not add value to the analysis of the EPA report on BAF, and it is confusing. “Does not have to imply” is

not a very strong conclusion, and the following sentence underscoring caution in interpreting the previous one, because changes in management, market forces and natural causes are all in play, does not inspire confidence. If the report does not address the impacts of the magnitude and timing of emissions on the climate system (p. 3, lines 37-39) then it should remain silent on those impacts. This is in keeping with the SAB's comment (p. 5. Lines 16-17) about the narrow scope of the charge. Similar comments about taking care not to predict the future (p. 21, line 22) suggest that the SAB is **not** recommending predictions of the future using the BAF on the time scale of the climate effects (100 years). However, if the idea **IS** to use BAF to predict impacts 100 years out, then I have to strongly disagree with this recommendation due to the many model uncertainties and inherent feedbacks (discussed above).

Appendix B and ocean carbon. The method described in appendix B is based on mass balance – less mass in the ‘terrestrial system implies, if conservation of mass is to be observed, that there is an increase of carbon flowing to the atmosphere.’ (p. B-1, line 17-18). This ignores the carbon stock in the oceans completely. It seems unlikely that the atmosphere and the terrestrial environment exchange carbon without involving the oceans in some way. Is this model assuming the ocean carbon stock is stable? If so, why?

3. Is the draft report clear and logical?

The report is very well organized. The links between the report, the executive summary and the cover letter are clear and logical. However, the draft report has many areas where the discussion of time and modeling are unclear (see notes above), and there are several places where clarity could be improved.

On page 10, I think the model would have to look at spatial variability in these effects across the world, since that is the relevant geographical extent for the climate impacts and since the market forces are global not US centric.

Page 11, line 12. I'm not sure what factors are being discussed in terms of 'representativeness' as it isn't clear to me how EPA developed the overly broad categories that the SAB is concerned with. Can you include some details of what factors might be involved in determining representativeness of the categories?

Page 11, lines 23-24. The fact that EPA uses a model to predict GHG emissions in order to assess CO₂ emissions related to biomass should be called out as a clear policy 'hint' even though they claim to want SAB input in a policy neutral way. Can the SAB comment on whether EPA's illustrative simulations in FASOM are cases where methane or N₂O emission changes would be relevant?

On page 11, lines 37-38, I'd suggest a mention here that a well designed sensitivity analysis could ALSO give insight into the model sensitivity to time frame assumptions and stability of land use assumptions.

On page 11, line 40. "some have criticized . . ." there should be references to support this assertion. Similarly the following couple of sentences suggest there are alternatives but no citations are included to support these statements.

Page 12, lines 5-8. Is the SAB suggesting that EPA should consider the alternative fate of biomass and/or the co-benefits of biomass harvesting in the model? If so, what quantification methods are suggested for this? Ecosystem services are referenced in the paragraph, but no references to a quantification method for these ‘other’ benefits is discussed. This entire paragraph seems unrelated to the specific charge questions and diverts attention away from the actual assessment of how BAF is estimated in the EPA framework. Unless SAB is suggesting EPA incorporate these other factors, this paragraph is better left out.

On page 18 lines 1-4, it is not clear how the cumulative BAF metric based on changes in carbon stock at a single point in time differs from the accumulation of annual differences in carbon stocks on the land and how these distinctions are related to EPA’s differences in emissions.

On page 18, line 12, it is not clear why the policy scope is mentioned here when the EPA method is supposed to be policy agnostic. Is it the physical or temporal boundaries that are at issue or the policy boundaries? The issue of policy scenarios (again on line 14) does not seem to be responsive to the charge question in this section (although they are clearly relevant to the overall issue at hand).

On page 23, lines 9-13, is this a return to estimating by facility? I thought EPA was trying to determine regionally specific BAF to avoid facility-specific BAFs. Is SAB saying a regional BAF will have to be determined using a representative facility specific BAF? This would be a significant recommendation. This should be clarified. Is SAB suggesting one option (preferred?) for regional BAF calculations or recommending a specific approach to regional BAF calculation based on a ‘representative facility’?

On page 23, lines 40-42, it is not clear to me how the approach would enable assessment of the sensitivity to time-path, but if it did allow assessment of this sensitivity, could the sensitivity to time-path be used to determine an appropriate time frame for BAF comparisons? If the regional feedstock-specific BAFs are sensitive to a decadal time-path, then model updating on a decadal scale and BAF comparisons at the 10 year mark might make sense.

4. Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Several conclusions are not adequately supported by the body of the draft report, as noted in section (1) above, most specifically, those dealing with response to charge question 1 and the relevant time frame for assessing BAF. These sections should be strengthened to provide adequate support for the conclusions.

The first recommendation is particularly important and its nature is telling in terms of the adequacy of the Framework and the review of the framework to advance national and international goals to manage climate effects of energy choices. The SAB correctly notes that a scientific evaluation of the biogenic carbon accounting approach is difficult in the absence of a policy context and spatial boundaries of the assessment (p. ii line 12; ES p. 1, lines 16-32; p.9 lines 16-17). I think that is understating it. I don’t think an adequate scientific evaluation of this method can be made in the absence of policy context and boundaries. Biogenic carbon accounting considers market forces and human choices, and as such, cannot be done in the

absence of the human policy context for the accounting. That EPA specifically asked for an assessment of an approach in the absence of this context is misguided. Further, the SAB concluded that evaluating the framework in the absence of how EPA may use it hampered adequate scientific review. Providing a partial review of the framework has the potential to give the *appearance* that the framework has undergone adequate peer review while the SAB clearly concludes that it is unable to give adequate review without the policy context.

The second recommendation is insufficiently quantitative. Is the EPA SAB stating that the appropriate time scale will vary based on other factors that affect all terrestrial effects on the stock of carbon (if so, which factors?) or is the SAB stating that the appropriate timescale is when equilibrium is reached (ostensibly 100 years) regardless of how changes occur or of feedback and coupled processes inherent in the model? I read the report and the recommendation, and I cannot determine what this recommendation is recommending. Based on my earlier comments, I would recommend more specificity here – the appropriate time frame for assessment cannot be determined in the absence of the policy and spatial specificity required for full review of the relevant science (as noted in the first recommendation). Further, the appropriate time frame may depend on sensitivity of the model input parameters to factors that occur on shorter time frames than the impact of the CO₂ on the climate system, e.g., changes in cumulative BAF forced by economic choices not physical system behavior, which would be difficult for the model to predict accurately over 100 years. And, the relevant time frame may be policy-specific and cannot be determined *a priori* in a policy agnostic manner. A cumulative BAF metric, which is noted to be appropriate in the final line of this recommendation, can be calculated for any time horizon, so it is not clear if the SAB is recommending use of the cumulative calculation at a specific time point or just in general.

The third recommendation claims that changes over time can be accounted for using the new method, but that both methods are dependent upon climate and carbon cycle uncertainties. Is the new method better because it accounts for time effects? Then, is SAB saying accounting for time effects is important and should be done? If so, say so. And, over what time frame should time effects be considered?

The fourth recommendation says the formulation is preferred. This is a weak recommendation. Is the SAB saying EPA should consider the alternative? Should adopt the alternative? Should avoid using the flux based approach? If the carbon stock method is better, it should be strongly recommended by the SAB that EPA use it in place of the proposed flux based approach, right? Why is this recommendation not stronger?

The fifth recommendation is excellent. EPA should indeed provide information on the modeling approach, its assumptions and how it plans to acquire data for validation and subsequent frequent (how frequent?) updating. It is unclear what ‘periodically update’ means and what EPA should do if the model outcomes are not consistent with the observed reality. It would be good to provide some context either in the recommendation or in the section that leads to it (section 3.3 pages 11-13).

Comments from Dr. John Vena

Science Advisory Board (SAB) review of Framework for Assessing Biogenic CO2 Emissions from Stationary Sources (2014).

1. Were the original charge questions to SAB Standing or Ad Hoc Committees adequately addressed?

I extend my compliments to the Panel for the comprehensiveness and thoroughness of their review. The review is exceptional in content and format. Explicit recommendations are made after very well written responses to the questions, thoughtful critique of the document and justification for the recommendations that follow. In my opinion all charge questions were very effectively answered. It is noteworthy that they developed well articulated responses and complemented them with very detailed feedback with superb comments and recommendations.

I liked how the panel articulated how the 2014 framework incorporated the SAB's prior advice but then stated the limitations and faults of the Framework and provided overarching suggestions for moving forward.

2. Are there any technical errors or omissions in the report or issues that are not adequately dealt with in the Committee's report?

None that I can tell based on my expertise.

3. Is the Committee's report clear and logical?

The **cover letter** is concise and very effectively highlights aspects of the review and the major recommendations. The letter in my view is one of the best cover letters I have seen to accompany a review.

The **executive summary** is well done and provides an excellent overview of answers to charge questions and recommendations.

The **review** is exceptional in content and format.

4. Are the conclusions drawn or recommendations provided supported by the body of the Committee's report?

Yes. In my opinion the report is very well written and comprehensive in responses to the charge questions.

Comments from Dr. Charles Werth

1) Were the charge questions to the committee adequately addressed?

Yes, the charge questions were adequately addressed.

2) Are there any technical errors or omissions or issues that are not adequately dealt with in the draft report?

Page B-4, Eqn B-3: Does PGE_Bt depend on the carbon source burned for electricity or heat? For example, different fuels may combust with different efficiencies, and therefore one might get more energy per unit of CO₂ emitted from one carbon source compared to another.

Page B-4: Is the impact of having to build additional energy facilities to combust biogenic carbon being considered. Most facilities burn coal or natural gas, and a shift in carbon stock would require new capital infrastructure. Is the impact of building these facilities being considered, or should it be considered?

3) Is the draft report clear and logical?

Page 4, line 27: Might want to specify scale as spatial scale,

Page 4, line 37: The phrase "the entire time horizon" doesn't make intuitive sense to me in this paragraph. I understand its meaning when I read further in the report. Is there a more explicit way to restate this here?

Page 4, line 41; I'm not sure that assuming "carbon stays in the atmosphere" is a correct way to think about the alternative BAF metric. I think what is being assumed is that net CO₂ emissions are only changed by modifying stocks of carbon on land.

Page 6, line 46: Are conservation of mass and mass balance equivalent?

Page 11, line 32: Consider changing "we think this was an oversight." to something like "we thought it was important to comment on this."

Page 12, line 7: Not sure how to weigh the benefit of preventing forest fires by tree thinning versus CO₂ emissions from burning wood. It isn't clear if report is recommending that this consideration be quantified by the EPA, or if it is just a general consideration.

Page 13, line 30: I don't think I understand the phrase "current use in electrical energy recovery from both landfills and combustion." I am aware of MSW being converted to electricity by capturing landfill gas, and using it to run a combustion engine that generates electricity. Another alternative would be for direct combustion of MSW for electricity generation. In both cases, there is a combustion process, so I'm not sure "energy recover from both landfills and combustion" make sense. Perhaps it just needs to be slightly rewritten.

Page 15, lines 20-25: This seems out of scope.

4) Are the conclusions drawn or recommendations provided supported by the body of the draft report?

Yes, I believe the conclusions drawn and recommendations are supported by the body of the draft report.