

Mr. Thomas Carpenter

Designated Federal Officer

US Environmental Protection Agency

Science Advisory Board

Washington, DC

Thank you for sharing the draft SAB review of the framework to assess biogenic CO₂ emissions from stationary sources. As you know I was part of the earlier panel that wrote previous drafts and therefore am very aware of the conversations and analyzes that went into those efforts. I was not part of the current panel, nor was I consulted. I point these facts out to just make it clear that I may have had some indirect influence on the current draft.

My comments are at two levels: general ones versus specific ones referencing particular pages and lines. For some comments the topics are related.

My general comments are as follows:

I agree that it is more appropriate to use a policy-based assessment period than the time at which all (to most) terrestrial carbon stores effects are completed. Despite being in agreement I did not find that the arguments being offered in the report were particularly convincing. At best they seem incomplete and given that this is probably the most contentious issue at this point I believe this is a critical mistake. I have several reasons that I don't believe there is a single, scientifically based time that can be used to assess the BAF's. I believe that mathematically there is a good rationale to assess the BAF when the difference between the reference and policy scenario ceases to change. This indicates when the direct carbon effects have largely ceased and it also is a time at which the BAF starts to act as a hyperbolic function. However, there are other mathematically important and objective times that could have been selected such as the time of the peak BAF value. This means there is no unique time that can be defined by science or mathematics which undercuts one of the rationales offered in the previous panel's report. Another factor is whether the time of assessment is strictly a scientific decision. I do not believe that it is, in part because to understand the full impact one must weigh not only the carbon science, but also the science of climate, ocean acidification, etc. Unless these all act the same as the carbon response, then one must make some sort of subjective judgement as to how to combine scientifically determined assessment times that differ. That is really a job best handled in the policy arena.

The report seems to have either misunderstood or redefined the concept of T. In the old system T is a particular time when particular things happen regarding the carbon dynamics. However, the current report seems to have redefined that to be the policy assessment time but does not describe this conceptual change and only seems to define the new term many pages after it is used. Unfortunately the report uses figures that use the old concept of T and refers to reports that use the old concept. This has created a very confusing situation. I would encourage the panel to keep T as its original concept as it has value as a concept even if it is not used to determine the assessment time. That way old the old

material does not have to be revised. The panel can then come up with a new subscript to designate the policy time (e.g., A for assessment, At for assessment time, P for policy or Pt for policy time, Pat for policy assessment time, etc).

I believe that the report is correct that this problem is most practically solved by examining multiple areas contributing to the plant(s) over time. To make this concept less awkward this has been described as the landscape scale or level. Unfortunately there are two problems when this is done. One can also look at the problem at the level of a fuel shed or at the level of a region, or the globe. So it is not a strictly landscape level that is being recommended. And because of this language it is not clear why a regional approach is being recommended in other places in the document if the landscape level is the only level to examine the problem. This brings up another point that is not clear in the draft report: there is a difference between moving beyond the stand level to do the analysis in terms of a concept and a method. The problem is that the report seems to suggest one method when it probably means to say one concept. In terms of methods one can certainly model an idealized stand and scale it up to the landscape/regional level, or one could model multiple stands in a broader area or use a distributional model, or use a spatially explicit model and even model the broader area as one entity. The point is that for the same concept there are numerous ways to methodologically address the problem. None of them is correct or wrong; they have different strengths and weaknesses.

In numerous places the report refers to updating the baseline as new data becomes available. As described in the report this is quite problematic especially at the regional level. What the report seems to suggest is to use the response of the region being impacted by biogenic fuel harvests to update the reference scenario. This would clearly conflate the two scenarios. Since the difference between a scenario and itself is zero, this would lead to underestimates of the BAF's over time. I don't see a problem with using new information to test assumptions, strengths of interactions, provide better parameter estimates. But I do see a major problem with resetting the reference baseline to match the responding region. Either the report has to explain why there would not be a conflation of the two scenarios or more clearly explain how the new information would be used to avoid this very serious problem.

In this and in previous reports the focuses too heavily biased on much on growing stock effects and not on ecosystem effects. The latter is the relevant concern because it is the response ecosystem that is controlling the net carbon flows. While the term feedstocks is used throughout the draft, the distinct impression is given that this is about growing stock. This is unfortunate because the carbon response might come from the growing stock (i.e., plants) or from the detritus from them (i.e., feedstocks related to waste or slash) or from something that is never harvested (i.e., mineral soil C). By using the term feedstocks the report seems to be prejudging whether there will be mineral soil C responses. I do not believe that there is any scientific basis for this assumption, and while I recognize that some have suggested that mineral soil C responses will be minimal, this is not a scientific statement, it is an aspirational one.

Throughout the report there are recommendations to use the simplest, least complex model. While I certainly do not recommend using the most complex model conceivable, it is not a good idea to use the

simplest model to analyze a problem. The model needs to be as complex as it needs to be to capture the different behaviors of the parts and to represent the interactions between them. If the system has key feedbacks it is always easier to ignore these in a simpler model and they often are. But that will mean the behavior is not correct because you actually aren't modeling the system you are interested in examining. I urge that the panel revise this language because it is quite misleading in terms of how one actually decides on which model to develop or use.

I agree with the recommendation that any models that are used be tested, subjected to sensitivity analyses in terms of assumptions, parameters, and general structure. However, these recommendations are overly generic and seem to skirt the idea of reporting the uncertainty or even what to do with that information. They also seem to miss the point that models need data to drive them and that uncertainty in that data/model can then impact the model predicting carbon responses. The report also seems to suggest by omission that the assumptions, parameters, and general structure of ecological models are about as uncertain as economic ones. The impression given is that if there is a longer list for one kind of model than other, that the former has more uncertainty. I think this is very misleading because it is not the length of the list one can craft; it is the degree of uncertainty about each item on the list. While there is certainly plenty of uncertainty about ecological models, many aspects about the kinds of entities, the processes, and interactions are quite well known. And while there is uncertainty about ecological parameter values, these are often well bounded by data and theory. The same cannot be said of economic models, which in some cases are still trying to understand the form and strength of various connections. There are good reasons for this state of affairs, the problems involved are quite challenging, but that does not change the current state of economic models.

The report is quite inconsistent about its recommendation regarding the two cumulative BAF's. In some places the BAFt is recommended but in other places it suggests that both be studied further. It also seems to suggest that little is known about how the BAFsigmat behaves. I find that to be a very misleading statement. The past panel did a lot of work on analyzing how these acted relative to each other for a wide range of cases and it provided and explained the mathematical relationship between the two cumulative indices. While I am sure that more needs to be learned, I think that is true of both cumulative BAF's. Therefore the report needs to be revised to recommend that both cumulative BAF's be further studies and to state that without the bias the current report contains.

Because the current report is lacking in many details the only location one can currently find them is in past reports. However, the current report provides the reader no effective connection to the past report. This is a major issue that needs to be addressed. Many of the details concerning the definition of terms and concepts, formulation of the BAF's, the analysis of their behavior, and scaling issues are found in appendices of the previous report. While it is good to provide a web link, the current report needs to provide more detailed guidance as to the general location to key sections. As it is the reader is not even alerted as to what they should be looking for. This does a disservice to the reader, but also devalues all the previous work that was done that is quite independent of the contentious issues.

While I believe that the current document makes some important improvements there are numerous problems in terms of concepts, terms, definitions, and overstatements (see below). Before this report is fully considered I believe these have to be addressed.

Sincerely,

Mark E. Harmon

Professor emeritus, Oregon State University

Specific comment

I have numerous specific comments which are referenced to the page of PDF document/line on that page.

2-29 While it is important to refer to the past materials, it is even more important to refer the specific sections and appendices that define terms, present equations, and provide examples. These need to be referenced in later text to make the connections clearer. To do otherwise will mean that access to critical material will be largely obscured.

12-13 It is not clear what soil represents in this context. Is this just the mineral soil? The non-living plant part of ecosystems? Does this exclude dead organic matter? This needs to be clarified because it could be taken to exclude from consideration logging slash, salvage of dead woody parts, even removal of the forest floor. This sentence is also not consistent with how this term seems to be used later which is the more restrictive use.

12-31 I don't think one can state categorically that the boundaries are always absent. That would depend on the power plant and the source. It would be better to qualify this in some way by stating the boundaries can be difficult to define in some cases. Possible absence would probably be a better way to state this.

12-37 I do not think that "and/or" makes any sense in this context. It would not be possible to assess the BAF's if the ecological portion of the system is neglected. That is because it is the part of the system that actually stores the carbon. One must view this hierarchically, with the ecological system nested within the larger ecological-economic system. This needs to be revised to reflect that hierarchical arrangement.

12-39 I do not believe that the statement "simple models are often best" belongs in this document. The best models are the ones that achieve the required objectives and include all the essential parts and interactions. They are as complex as they need to be, because being simpler means that the system being examined is not being examined. Best also depends on the question being asked, the level of resolution needed, the trade-off between generality and realism. Since none of this has been specified it is largely a meaningless statement that seems to reflect someone's particular bias.

12-41 In this context it is not clear what the “last increment” means. How is the last one defined? Is it the last one before the assessment time? Or the last one possible?

13-1 While I agree that much of this information is important to consider and use in models, as written this seems to suggest the future response can be predicted from the past response. Given that much of the proposed expanded biogenic carbon harvest is novel and in the future, it is not clear how these data could be used directly. That is, how does one use past empirical data to predict a novel response? You can’t. This logical problem in the document needs to be resolved.

13-12 The approach of adjusting the baseline does not really make any sense at all as written. I believe that it is important for future information to be used to test model responses, assumptions, parameter estimates. However, periodically adjusting the reference baseline would potentially create a situation in which the reference and policy scenarios (to use the terminology of the previous panel’s work) become conflated. The only way this would not occur is if the areas in a region influenced by the biogenic carbon harvest can be separated from the ones that are not. Moreover, if there is any preference in terms of site productivity, species, management, etc between the two subregions representing the two scenarios then one would risk mistaking the actual cause of differences. I believe that this proposed approach needs to be reconsidered or described in a way that would actually work.

13-14 On what scientific basis can it be stated that carbon stocks always increase? That is what is implied by this statement. I suppose one could say changes in carbon stocks, but this raises the question of how one avoids conflating the two scenarios needed to make the BAF calculations.

13-16 While I have a very broad use of the term model, I do not see how the choice of the kind of reference system selected is a model in any sense. It is certainly part of the assessment framework, but it is not a model because it does not predict anything.

13-24 I do not see how the data underlie the models. I can see how some data might be critical to assessing models or re-parameterizing them, but this is written as if the model will be solely empirical. Limitations on the type of model to be used should not be part of this report.

14-20 It should be stated that the changes are made relative to a reference scenario. That is an essential element of the system that needs to be repeated throughout the document.

14-41 I strongly disagree that one of the cumulative BAF’s should be preferred at this time over another. “more needs to be learned about how they differ” is a very dubious statement. The previous report had numerous examples to illustrate the differences and described how they relate to each other mathematically. This statement introduces a bias that is not based on science and essentially ignores all the work the previous panel did. I believe EPA should do more work to examine the two cumulative BAF’s but this statement basically has the effect of discouraging further work by EPA. This statement also does not accurately reflect the detailed material below.

14-42 There is a contradiction between using BAF_T and the policy horizon approach being recommended. The use of a capital T signifies the time at which the difference between the policy and

reference scenarios stabilizes. That approach is not what is being recommended. To be consistent with what is being recommended the lower case t should be used because that refers to any value of time that one selects. Or the panel can create a new subscript that is not in conflict with the other sections and past work.

15-1 Again the lower case t should be used in this context.

15-7 Again, what does the last effect mean exactly? How is this relevant to the ecological response?

15-10 What does reasonable mean and how can that be scientifically quantified? Reasonable will vary from person to person and from time to time.

16-19 They also could reflect changes in stores associated with other types harvest if bioenergy is using some of that carbon. Bioenergy generation has already started to compete with particle board production and that is reducing the stores in wood products in use. So this needs to be generalized to include all the carbon related to forests, not just what is in forests.

16-37 Again, it is good to have a general link to past material, but in the later sections one needs to refer to specific parts of the previous reports, particularly the appendices that more fully explain the calculations, the concepts, define terms, etc.

18-5 See comments above about conflating the policy and reference scenarios. This is an extremely problematic approach which sounds reasonable, but would actually create problems.

18-14 See comment for 16-19. The statement at 18-14 makes more sense, but it is in conflict with the one on 16-19.

18-21 True, but probably irrelevant as EPA will be doing the modeling for the most part. Given this why is this statement needed in the report?

18-36 This would also require use of models and not just use of data. This should be generalized to “additional information” or expand it to “additional data and modeling”.

19-3 I think it is very confusing to refer to these as influencing the BAF's. The BAF's are just formulae that make the estimate. The calculations are not impacted by the source of information. What seems to be referred to are BAF estimates or values. That makes sense, but influencing the BAF calculation itself does not. Perhaps this is just a case of using jargon, but it needs to be addressed.

21-8 Why does carbon dioxide only have a climate impact? What is the scientific basis of that statement? Are we seriously to believe that there are no ocean acidification impacts of releasing carbon dioxide into the atmosphere? This should be generalized to environmental impacts and not be so centered on climate effects.

22-4 It would be better to state that the harvest is likely to be regulated. There is no science that can predict that it will always be regulated as implied in the current version of this statement. However, there are other issues with this section. The idea that the harvest is regulated only applies to a forest

system. In an agricultural setting there is not really a rotation as this seems to imply. It is entirely possible that the fuel would come from the same land area year after year. In an agricultural system the losses and gains might be fairly synchronous from place to place. So the concept of asynchrony does not work particularly well outside the forest sector. This would be solved if it was made clear that this section is referring to a long-term perennial system such as a forest.

22-7 I think it would be better to state that stand level are not necessarily relevant. That is because one can certainly scale up stand-level results to the landscape and in that case they are certainly relevant.

22- 8-9 It is important to mention the comparison to the reference scenario all possible reactions. Right now it is only mentioned in the case in which outputs exceed inputs. For the BAF's the key point is a difference between scenarios, it is not valid to compare a scenario to itself because scenario cannot be referenced to itself.

23-29. The modeling that was done by the previous panel indicates this is not the case. What is being mixed up here is the BAF delta t which would probably be zero for most by several hundred years, with the cumulative BAF's which are not going to be zero even after hundreds of years. One only has to look at Figure 1 to see that. If the T is 100 years, then the BAFt is not going to reach zero for a very, very long time. Even at 5 X T or 500 years it would not be zero as is stated here. This represents a failure to understand how the cumulative BAF's act over time.

23-34 I clearly missed where T was defined as the time horizon earlier. It was not defined this way in the previous reports and it does not match what is being used in Figure 1. I think it is a mistake to use T in a new way without defining it or considering how it conflicts with other concepts of time that have been used.

24-9 This is not how T is used in the previous documents that you refer to. This is just going to add confusion.

24-38 As stated above this makes no sense whatsoever. Clearly a model can be oversimplified and not be particularly useful to analyzing a certain problem. The simplest model of biogenic carbon effects is that stores do not change. That overly simplistic model has been offered repeatedly in past conversations about BAF's. It is clearly the simplest model, but is also clearly wrong. I urge the panel to be far more careful about statements of model simplicity.

25-41 This is a place where specific reference to the previous materials is needed. None of the terms used in the figures is defined and some of the definitions conflict with what is presented in the current document. This is bound to confuse anyone actually trying to use the current report.

26-28 As mentioned above this approach of a shifting baseline would potentially confound the reference with the policy scenario or conflate various causes. I think this is very problematic as presented.

26-41 While FIA measurements have been made for decades some of them are very recent, and some such as soil have not really be made in an appropriate way and some such as products are not inventoried at all. So while I agree that FIA data is important and useful, it has serious limitations that

need to be recognized. This seems to gloss over all the shortcomings and presents a very inaccurate image. Also one has to be aware that until the last decade or so the FIA database did not include US Forest Service lands, Park Service lands, wilderness areas etc. So to say it has been robust for five decades is not accurate at all and seems to border on a PR pitch.

27-1 This recommendation does not match the more specific one presented in the summary section. I think this is the correct recommendation: consider both at this point and do more analysis.

27-11 I don't think it makes any sense in revising the BAF's in the sense of the calculations. The BAF is always going to be a ratio of carbon changes divided by carbon emitted by combustion. What may change is the estimated BAF. There is a vast conceptual difference that is being missed here.

29-9 What is meant by biogenic factors?

29-30 Again, the BAF equation is not going to be influenced by the amount or novelty of the data. The BAF estimate might. There is a profound difference between the two. To quote Twain "being hit by lightning is not the same as being hit by a lightning bug."