Comments of Anne Smith on the 8-29-18 SAB Draft Report: "SAB review of Framework for Assessing Biogenic CO2 Emissions from Stationary Sources (2014)"

September 25, 2018

1. Were the charge questions adequately addressed?

In many of its sections, I do not find this review draft to provide useful responses. If the objective of this SAB review draft is to provide guidance that can help the agency improve either its framework, or its explanation of that framework, I think this draft review is unlikely to meet that objective. If it is to provide approbation or disapprobation of certain technical aspects of the agency's framework, it does not do so clearly. I see little or no actionable material in the recommendations, other than what I interpret as a refrain that the charge questions cannot be answered until a specific policy context is provided. Even that refrain, however, seems to me to be overstated.

For example, on p. 12, in response to Question 1(a) regarding whether the temporal scale should vary by policy, the draft review states that "The time horizon, T, for consideration of carbon stock changes should be chosen based on the policy objective"(12:34-35). However, in the next paragraph, apparently in response to Question 1(a)(i) regarding "what goals/criteria might support choices between longer and shorter time scales" if the time scale should vary by policy yet multiple policies cover the emissions in question, the draft review states that "In the service of simplicity, a single time horizon may be selected to serve multiple objectives, in which case the tradeoffs need to be explored to ensure the most parsimonious temporal framework is selected and thus dis-benefits to the environment and public health are minimized." (p12:44-46). These two statements, in consecutive paragraphs, are effectively contradictory. If a single time horizon can be acceptable for a hypothetical case where multiple policies are in question, there would seem to be no reason that a single horizon could not be identified that would suffice for all potential policies.

I have deeper concerns with the second statement, however, which is that it is a non-answer to Question 1(a)(i). I would expect that the only appropriate time horizon would be the longest one necessary to properly address any one of the multiple policies in question -- because anything shorter would not be appropriate for at least one those policies. But the draft review does not note this logical point (or refute it, if that is what the Panel believe). It instead combines three undefined notions to provide an opaque response: (1) that some time horizon choices might be more "parsimonious;" (2) that identifying such a parsimonious framework requires consideration of "tradeoffs;" and (3) that parsimony will result in minimization of dis-benefits to public health. What "parsimonious" means, what the "tradeoffs" consist of, and why the objective of estimating a BAF for GHGs should encompass the minimization of public health impacts are all unexplained. As I result, the statement does not answer the charge question with any actionable guidance, while also creating new sources of ambiguity and confusion for the reader.

There are other parts of the draft review that do not seem to be responding to the charge questions at all. Two examples are the responses to 2a & b (combined), and to 2c. The draft review combines its responses to Questions 2a & b because "both questions relate to the size of the simulated change in demand for biomass feedstocks" (p. 16); however, the one-paragraph response that follows never mentions magnitude of change, and seems to address a different topic, which is regionality of a BAF estimate. Question 2c asks what baseline the demand shock should be

simulated against, but the response appears to address size of the demand shock rather than the baseline against which the demand shock is to be simulated.

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

I have not identified any outright technical errors. However, on p. 1, the draft review notes that the issues that must be considered in developing a BAF estimate are complex, and the framework may become very complex as a result. It then concludes by stating that "Often, simple models are best." (p.1:39). The justification for this conclusion seems to be an observation that results of complex models are more dependent on their input assumptions(p. 1:37-38). I consider both the observation and the conclusion to be technically misleading, and thus inappropriate as guidance to EPA. Results of simple models are also dependent on their input assumptions, with no reason to believe that they are "less" dependent. The only difference is that there may be fewer explicit assumptions to adjust in a simpler model, however, embedded in simpler models are far more hidden assumptions that cannot be adjusted or even observed -- the very assumptions that were made in order to achieve its formulaic simplicity. I agree that complex models should be subjected to sensitivity analyses to better understand or interpret their results, and it seems a missed opportunity that the draft review has not recommended doing more of this sort of sensitivity analysis for the various feedstock and region BAF estimates, and also to better understand how important the choice of biomass demand shock sizes might be to the resulting BAF estimates. Such sensitivity analyses can even be used to identify ways to reduce a complex model to a simpler set of relationships, or meta-model, in a manner that does not create error or biases. However, it is technically misleading to suggest that the challenges in developing a sound BAF estimate can be reduced or managed by choosing to use a simpler model. This is only a reasonable course of action if the simpler model has been shown to provide reliable approximations of the results of more complete (aka "complex") representations of the phenomena being addressed.

3. Is the draft report clear and logical?

I find some of the responses unresponsive to the question, and many to be unhelpful as guidance. But in addition, the writing is unclear and the points do not always seem to flow logically. In many places, it seems as if the document was created via cut-and-paste from some other report, stringing together paragraphs that were not originally written as a unified sequence of thoughts. I also find the section on pp. 14-15 that contains two graphs very difficult to follow. The figures are not explained in the text, and generally the additional cumulative concept if not clearly enough explained to support the recommendation that follows in p. 16.

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

Given the concerns I've expressed above, I find it difficult to answer this in the affirmative.