

COMMENTS TO THE EPA SCIENCE ADVISORY BOARD BIOGENIC EMISSIONS PANEL

Timothy D. Searchinger

Princeton University

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I have had the opportunity to read the draft report. While the report has several good elements, I believe that two of the report's central recommendations will not pass the test of time and judgment by other scholars.

Value of emissions and mitigation over time: The report endorses a view of time accounting that does is not based on a consideration of the core issues related to evaluating the cost of emissions at different times or correlatively the value of their mitigation.

The report's basic approach is to describe how the harvest of trees for bioenergy adds carbon to the air over time, as harvest moves from one tract to another. This analysis takes account of regrowth that occurs on the earlier tracts as harvest occurs in later years on other tracts. Typically the regrowth of forests cut in early years for bioenergy grows over time and becomes in excess of what forest growth would be without the bioenergy harvest on these tracts. When that additional forest growth matches the ongoing forest cuttings and removals for bioenergy additional bioenergy harvests do not add carbon to the air. This analysis assumes that bioenergy continues throughout such a period of years, and that harvests of new tracts continue in the same manner as previous tracts. It is this time that the draft report encourages for analysis of the impacts of bioenergy. The report calls this time T. For clarity, I will call this the equilibrium year because it is the year in which the ongoing harvest of bioenergy (assuming the same harvest regime) no longer adds more carbon to the air.¹ Depending on the harvest regime and the original forest growth rates, the report gives examples that this equilibrium can be reached after a few decades or many decades.

¹ This equilibrium is actually true only in a limited conceptual sense. In any given year, a source could stop using bioenergy. At the "equilibrium" time, such a decision to stop using bioenergy would still typically reduce carbon in the air in that year and for several years. Why? Because the regrowth from earlier harvests would continue, but there would not be additional removals. Viewing the decision to use bioenergy each year for its own incremental affect, bioenergy will never in these typical forest harvest regimes avoid increasing emissions in that year. (The regrowth is paying back the carbon added by the earlier harvests, not the later harvests although the committee's approach views it as compensating for the later bioenergy harvest.) This ongoing use of bioenergy only does not add carbon to the air if someone looks at the decision to harvest wood for bioenergy ex ante as a commitment to harvest bioenergy up to that year. This is a subtle but informative distinction. Notwithstanding the committee's presentation of the decision, every year the decision to harvest more wood for bioenergy will actually add carbon to the air relative to a decision not to harvest that wood for bioenergy in that year.

The first problem with this choice of timing is that by itself it says nothing about how to value the differences between emissions at different times within the period between year one and the “equilibrium” year. Typically, forest harvests for bioenergy will add nearly all the carbon to the air combusted in initial years, and often more than the total combustion because of tree growth destroyed and left behind to decompose. As a result, the decision to produce bioenergy will add more carbon to the air than using fossil fuels in early years, and benefits occur later. The real question is how to value emissions (and mitigation) over time. The report suggests a few possible methods of counting these emissions over this time until equilibrium, but in one way or other, the basic focus is to examine how much carbon has been added to the air because of reduced storage of carbon in the forest at the end of the period. The carbon rating of bioenergy, the BAF, is based on dividing this number by the amount of carbon released by combustion. (Yes, the report distinguishes cumulative emissions from net emissions at the end, but the results are similar). The key point is that emissions in early years count no more than emissions in later years, which means that mitigation counts no more earlier rather than later. There is no discounting. In fact, at least in many scenarios of forest harvest, bioenergy from sources that achieve net emissions reductions sooner will have the same or possibly even a higher carbon score (“BAF”) than bioenergy that increases emissions for much longer periods, which means they are valued essentially the same.

There is no rationale for this approach. I believe it is broadly inconsistent with published thinking about the costs of emissions over time and therefore the value of mitigation over time.

There are many factors that should properly determine the cost we count for emissions at different times and therefore the value we count for their mitigation. These factors include far more than the contributions of emissions toward some future peak warming. Under typical modeling now, peak warming will never be reached until either some period of years after there cease to be net carbon emissions or, in the most catastrophic situation, after carbon emissions have saturated the spectrum at which atmospheric carbon causes radiative forcing. Among the proper timing considerations are damages caused in the interim, and the risk of crossing thresholds – such as those that might trigger a methane pump from permafrost. If we cross critical thresholds, the fact that emissions might be reduced later does not much matter because the damage has been done. Related to these risks are the opportunity values of keeping atmospheric warming lower, as long as possible, which gives mankind to respond to early signals from warming the opportunity to take more vigorous action before those thresholds are crossed. Another set of considerations is the increasing uncertainty that postponed mitigation will occur at all.

And another set of factors are purely economic. Mitigating the same level of emissions is more expensive in the short-run than the long run if only because of the

time value of money. Any entity allowed to postpone the same mitigation can at a minimum put the money otherwise spent on mitigation in the bank and earn a return before undertaking the mitigation. And those who are allowed to wait for mitigation can also take advantage of the progress of technology to lower mitigation costs. Yet, if we want mitigation to occur sooner as well as later, we need to be prepared to pay for it, which means we must value it more highly. Those the costs of mitigation over time, and the ability to choose mitigation options based on technology available in the future rather than to make commitments to a specific technology today, are critical factors that are part of the timing consideration.

Taking account of many of these considerations, there is also an abundant literature on the cost of carbon that will typically call for substantial discounting of future mitigation relative to the present. In fact, the discounting is greater under a “cumulative emissions” physical model of future warming compared to previous models that assumed the earlier emissions had less effect on future warming than later emissions because of the absorption of carbon over time into the oceans and forests.

To appreciate the significance of the report’s recommendation, the valuation of emissions and mitigation over time should apply the same to a power plant regardless of how the plant achieves those emissions and mitigation. A power plants decision to use forest material that increases emissions for many years and decreases them only after many decades has exactly the same effect on the atmosphere as a decision simply to burn more coal for many years and offset that coal with additional mitigation later on. If this committee wishes to defend its report, it must then also be prepared to defend the argument that such a power plant’s decision to burn additional coal in the short-term along with a commitment to achieve the eventual mitigation down the road should receive the same climate credit as the actions of another power plant that reduces emissions immediately.

Precisely for this reason, the considerations affecting the value of emissions timing are independent of the nature of the forest harvest regime. Those considerations relate to damages and risks from climate change, as well as the economics of mitigation. They do not relate to the forest harvest regime. What the forest harvest regime (and BAF curves) do relate to are the quantities of additional carbon that will be in the air at different years. The committee has in effect described one method of estimating the timing of emissions (assuming ongoing bioenergy use of a particular harvest type in a particular forest). That biophysical factors that affect the quantity of net carbon in the air at different times are separate and from the factors that determine the different costs of that carbon at different times.

That is also why I do not understand how the time period chosen is relevant at all to the timing consideration. The theory seems to be that the period at which the decision to use bioenergy adds carbon to the air is the period of relevance for policy. Why? Why isn’t the period relative to policy the year in which the policy is

designed to produce emissions reductions (particularly since that policy may be replaced by a different policy)? Or the year at which a power plant ceases to operate? Or an indefinite future period at which carbon in the atmosphere has been altered in any way? Why should this equilibrium period be used – based on the assumed continued use of bioenergy – even if bioenergy use ceases after five years, or a decade? What if the source of biomass changes? What if the harvest regime changes? Again, while the method can help to estimate the amount of carbon added to the air over time (whose value is limited by the reality that bioenergy use and harvest regimes may actually change), the method does not relate to the key timing question.

Finally regarding timing, the report has a particularly problematic provision. If the committee believes that timing should be judged using the equilibrium year for any particular type of harvest, it should consistently follow that approach. However, the report also suggests using a longer time period if there are other harvests for which this “equilibrium” time period is longer. The effect will be to greatly lower the BAF of wood harvested with a regime with a shorter equilibrium time. For example, if the equilibrium period is reached after 30 years, and another forest harvest regime reaches an equilibrium point after 90 years (in the region? in the country? in the world?), the calculation may be performed for the first harvest regime over 90 years and the BAF will be very, very low. That is logically inconsistent and I can discern no rationale other than to make bioenergy more attractive.

Economic modeling: My other main concern involves the endorsement of economic modeling, and apparently regional economic modeling.

Using a biophysical approach, nearly all analyses find that increased harvest of otherwise standing wood for bioenergy will increase carbon in the atmosphere for decades relative to fossil fuel use for electricity. These analyses typically choose as their counterfactual the assumption that the wood would otherwise remain unharvested. In many contexts, the use of wood for bioenergy will divert wood from other uses. But that does not necessarily or even likely imply a better result. Replacing that wood would lead to additional carbon harvests elsewhere. In fact, if wood from plantations otherwise destined for pulp is replaced by wood harvested from more natural forests, the carbon increases are likely to be larger.

Notwithstanding these results, the approach suggested by EPA’s draft framework is to use economic modeling, particularly using its FASOM model, to estimate whether increased demand for wood products will alter the carbon storage effects of bioenergy. It could do so by leading landowners to (a) plant more forests, (b) in the short-term reduce harvests of wood at least in the analyzed regions in anticipation of higher economic returns from wood harvest later, or (c) manage forests to obtain higher growth rates. All of these effects are possible in part. But at least two of these effects, if they occur, would also have further economic effects with adverse carbon consequences. Planting more forests would most likely

displace agricultural land, likely leading to further agricultural expansion elsewhere to replace at least much of the displaced product. And withholding harvests in a region analyzed will likely lead to further harvests of timber elsewhere to replace the products, or possibly, reduced use of wood products and emissions from substitutes.

The FASOM model results presented, although variable, come out with some rather extraordinary results. In one scenario, increased demand of one ton of wood from southeastern U.S. forests results in so much additional planting or reduced harvesting that it results in a net increase of .4 tons of carbon in the forest. This result would imply that paper recycling in the U.S. harms the environment because every ton of carbon saved by recycling actually reduces forest carbon by four tenths of a ton.

One reason for the result is the purely regional analysis. This approach ignores the carbon costs of replacing wood products no longer generated. But addressing this problem requires a reliable global international model of both forestry and agriculture.

The question is whether use of economic models to generate these results can have a reliable economic foundation. The report properly raises some questions about FASOM and calls for some effort to try to improve and at least somewhat demonstrate its reliability before its use for this purpose. But the report still calls for use of this type of economic modeling.

Yet no model at this time can generate sufficiently reliable results. Doing these kinds of analyses requires an enormous range of elasticities and functions that simply are not known and have not been estimated using proper econometric methods. Consider some basic questions. When demand for bioenergy-quality wood rises, what are the true demand elasticities, and what are the supply elasticities? And of these supply responses, how much is likely to come from additional forest plantings versus more harvests of existing forests anywhere in the world? Of the demand responses, how much involves displacement of other wood products and their replacement by products built with other materials that would generate carbon emissions. If some of those harvests come from existing plantations in some other part of the world, how much of that will be replaced by more plantings versus more wood harvests of existing forests? (For example, the U.S. is importing wood from Brazilian plantations, but Brazil is cutting down vast areas of natural forests to produce charcoal for steel making.) All of these analyses properly require a large number of cross price elasticities, but few cross price elasticities are even estimated. Instead, they are typically generated by the choice of the mathematical forms of the model. We do not even have good data on key harvest practice factors in much of the world, such as the ratio of harvested and removed wood to wood that is killed and left to rot or burned. That harvest information would have great significance for the implications of diverting timber from plantations in the US to bioenergy. The result is that these models are filled with

assumed parameters and functions, elasticities that are not based on modern econometric methods, and parameters that may have some evidentiary basis in one region or context but are applied without evidence or even any reasonable justification to others.

I found refreshing the comments of John Reilly in his initial proposed answers to EPA's questions, which are particularly admirable because he is one of our country's most accomplished researchers at developing these kinds of models himself. As he wrote, "Having constructed models of this type myself, my view is that they are illustrative, give some insight into processes, but unless they can be corroborated in some way would be very poor guides for establishing a factor to apply to different biomass sources. . . . I don't think any model could ever achieve credible reliability at the level needed."

The committee presents no analysis to the contrary showing that the data or underlying economics are adequate for the use of these kinds of models for the purposes suggested by EPA or the report.

Finally, the assumption behind the goal for this kind of model is that increased demand has a large chance of significantly influencing forest plantings and management in a way that offsets at least much of the additional harvest for bioenergy. In fact, the common claim is that increased demand has led to increased forest carbon in the U.S. But I can find no real evidence to support that claim. Yes, forest carbon is growing in the U.S., but there are large numbers of exogenous factors. They include the aging of forests from heavy cuts long ago, both carbon and nitrogen fertilization effects that spur forest growth and reductions in agricultural land in the northern hemisphere in part as agricultural areas switches proportionately toward developing countries. Another major factor is the decline of traditional bioenergy. That has occurred first in the form of draught animals, which as late as the 1930s required vast areas of oats and other small grains to provide feed, as well as unknown large expanses of pasture. There has also been a decline in recent years of traditional firewood harvests. Yes, it is true that forestry has increased plantings and management in place of natural regeneration, and that may generate more wood for harvesting on the same land, and demand probably plays some role in these decisions. But increasing demand also has effects in other regions and internationally – the U.S. imports more wood than it used to. Increasing global demand for wood products also leads to expansion of extensive forestry abroad. The net global effects of increasing wood demand are therefore ambiguous. As far as I can tell, the analyses that claim increased carbon sequestration due to increased forest products demand are modeling analyses of the type described above with all their attendant assumptions and uncertainties, not rigorous econometric analyses. Overall, there is no sound factual basis for going beyond the biophysical analyses of the impacts of wood harvest.