A peer review as an input to the

EXTERNAL PEER REVIEW OF STAFF'S METHODOLOGY IN CALCULATING CARBON INTENSITY VALUES AND USE OF GREENHOUSE GAS EMISSIONS MODELS

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Preface

As I understand it the peer review is intended to develop external review opinions on whether the CI methodology used by the ARB staff and supporting parties in calculating carbon intensity values and use of greenhouse gas emission models yields a valid scientific basis for the conclusions in the air resources Board staff reports.

I also believe that while I was sent three reports and a plain English version that I am only supposed to review those within my field of expertise which limits me to comment on

Calculating Lifecycle Carbon Intensity Values of Transportation Fuels in California, March 2015 (Staff Report 1)

Calculating Carbon Intensity Values from Indirect Land Change of Crop-Based Biofuels (Staff Report 3)

Additionally I will comment on the attachment entitled *Plain English summary of staff's methodology in calculating fuel carbon intensities*.

Basic findings

In attachment 2 of the request for external peer review originating from Mr. Jim Aguilia I note that I am supposed to express opinions on the conclusions from the staff reports. I will do this for each report separately.

Staff report 1 - lifecycle fuel carbon intensities

The conclusion stated is "based on staff's assessment of available lifecycle inventory sources, emissions, and efficiency data, ARB staff concludes that the assumptions and inputs used in CA-GREET 2.0 to calculate direct lifecycle fuel Cis are reasonable and the model was applied appropriately under the LCFS."

In my reading of the document I developed a number of notes commenting on presentation, assumptions and scientific basis. These appear below. My final opinion after that reading is that I agree with the staff and believe that the sources used, models used, emissions estimates and procedures within CA-GREET 2.0 provide a sound basis for subsequent use of the estimates that

arise from its use and that in general the procedure is based on sound scientific knowledge, methods and practices.

Staff report 3 - calculating carbon intensity values from indirect land use change of crop-based biofuels

The conclusion stated is “based on the iLUC analysis, ARB staff concludes that the assumptions and input parameters used in the GTAP-BIO and AEZ– EF models to estimate indirect land use change for biofuels are reasonable and the models were applied appropriately under the LCFS.”

In my reading of the document I developed a number of notes commenting on presentation, assumptions and scientific basis. These appear below. My final opinion after that reading is that I agree with the staff and believe that the assumptions and input parameters used in GTAP-BIO and AEZ– EF plus the way those models were used provides a sound basis for development of results for subsequent use under the LCFS and that in general the whole procedure from assumptions through use is based on sound scientific knowledge, methods and practices.

Specific Comments

The comments below arise from a page by page reading of the staff reports. In places suggestions are made for document improvement. Also given this is a rapidly developing and advancing field some suggestions are made for future analyses with the model as the California rule and staff analysis moves into the future.

Comments arising during a reading of the document staff report 1: calculating lifecycle carbon intensity values of transportation fuels in California, March 2015

On page 3 of the staff report under section C in figure 1 it shows a picture of the life cycle analysis but in this it does not show emissions associated with the inputs to the feedstock production such as fertilizer and pesticides. GREET includes this and inclusion of such items in the Figure might lead to a more accurate portrayal of what's going on in GREET.

On page 5 a 1996 survey of sorghum producers is referred to as a source of some of the data although I am unclear to what extent this is relied on as substantially newer EPA study is also referenced. I believe in either case newer data could be obtained from the ongoing USDA ERS ARMS survey and the Sorghum Growers Association. There may be some reason to improve assumptions from survey results that are almost 20 years old. In particular the last 20 years in corn production has seen a big increase in yields with little increase in fertilizer. This may also be true for sorghum. Also sorghum yields have increased and with a long the increase in yields probably comes an increase in costs in terms of seed and harvesting effort. .

On page 6 A particular treatment process for cellulosic biofuels is covered. Today a few companies are just finalizing construction of or are initially operating commercial scale cellulosic biofuel facilities. It would probably be more accurate going into the future to use what can be obtained about those processes as opposed to a lab process using this particular method. I personally am not aware of exactly what methods are being used in those emerging commercial cellulosic plants but the companies may well have created lifecycle estimates for consideration of their fuels under the advanced biofuel category.

In general use of the GREET assumptions and methodology is scientifically sound as the ANL GREET group is the world leader in life cycle assessment and widely accepted in the government and profession.

On page 9 where tier 1 fuels are listed that perhaps the list should be expanded. In particular given that earlier in the briefing paper text that there is discussion about sugarcane ethanol I would probably have said starch and sugar-based ethanol including that from corn and sugar as those are the two largest sources currently. Under the biodiesel sources I might have listed soybean oil corn oil, canola, and other plant oils.

On page 10 when the paper mentions carbon capture and sequestration the terminology might be improved. Normally this is called carbon capture and storage. Also I might put in some wording regarding incorporation of carbon capture and storage into processing facilities.

In figure 2 under tier two generation I might call it ethanol from cellulosic sources. Restricting it to Stover is a pretty narrow set with dedicated bioenergy crops like switchgrass or miscanthus plus use of wood and other things are possible. At some point soon we may also need to list some sources of jet fuel.

Eventually I might worry some about the assumptions of spatial homogeneity. In particular, I know that for corn in the US there are regions where yields are close to hundred bushels an acre but that in other regions there are yields in excess of 200 bushels per acre. I also know that the fertilizer, seed, pesticides, and tillage requirements plus likely planting density and hauling requirements to get to a processing facility vary widely across regions. This would then lead me to wonder whether the GREET assumptions are appropriately differentiated on a spatial basis to reflect varying greenhouse gas intensity of various operations in various places. I do not think this is the currently the case. I would worry about this and might require people using the default values to justify that those default values would apply to their region in terms of the major ones in production quantities, fossil fuel, fertilizer use and hauling distances.

I agree with the conclusion the staff states on page 16 that the GREET uses appropriate methods. I believe it is a representation of the state-of-the-art of scientific knowledge and available data. However I must recognize that this is modeling and there almost always are ways models can be manipulated and slightly improved. In the future I might worry some about the sorghum and potential spatial homogeneity assumptions used. Also given the fact that the cellulosic industry is making its first commercial steps this means that the GREET assumptions will likely need to be updated going into the future.

Comments on staff report three calculating carbon intensity values from indirect land use change of crop-based biofuels

On page 2 I am not totally happy with the chosen wording. In particular the comment is made that the ARB staff has "identified an indirect effect that has a measurable impact on greenhouse gas emissions: land-use change". It's certainly fair to say that scientists and policymakers identified this well before the ARB so I would include some wording to indicate this is not an item uniquely identified by the ARB but rather is identified based on the scientific and policy dialogue.

In terms of documents scoping I see in the title the word land-use change. I think this is a rather narrow perspective and believe one should not strictly limit consideration of that indirect stimulated greenhouse gas emissions to only land-use change. In particular I believe consideration should involve both land-use change and other sources of emissions leakage. I feel when demand for biofuels increases that it either directly reduces the amount of crops in a region that enter the marketplace or causes a diversion of land away from conventional crop production to bioenergy feedstocks production. Both of these forces reduce the amount of conventional crops in the market place and raises market prices. In turn this would stimulate producers elsewhere to either bring nonagricultural lands into production (ILUC) or to adopt more intensive forms of agricultural production. Both of these actions increase greenhouse gas emissions outside of the target area.

Thus I would also not limit the discussion and the model GHG accounting to ILUC carbon emissions but would attempt to cover the fact that the excess or leaking GHG emissions include both those from indirect land use change and those from more intense production practices (heavier fertilizer use, more tillage etc). I believe within the GTAP framework that both of these are considered although I am unsure whether the other effects were included in the GHG accounting that ARB used.

On page 2 I agree with the ARB staff conclusion that the land-use impacts are significant and should be included in the fuel carbon intensities.

On page 2 I agree that the staff selected an appropriate global economic model in the form of GTAP.

On page 2 I again have some wording issues. In particular the report states supply equals demand in GTAP. I do not believe this is uniformly true. In general I believe supply is greater than or equal to demand and that in most sectors supply equals demand but cases like corn stalks have more supply than demand.

I agree with the staff that it's appropriate to shock the model by increasing biofuel production to a higher level of requirement.

On page 2 I do believe it's appropriate to combine GTAP with a more regionally specific emissions model (AEZ) and emissions assumption as was done in the analysts. I do believe in the future that the staff might consider broadening from just ILUC consideration to one that more broadly considers greenhouse gas emissions from any stimulated intensity expansion as discussed above plus, perhaps diminished livestock production (as has been found in my US studies due to increased feed prices). Just to clarify if we reduce corn in the market and Argentina responds by increasing heavily fertilized corn on lands that previously grew a less emitting crop then emissions go up from that source (an intensification response) along with the possibility of expanding cropped land use onto lands that were not previously used for crops. Simultaneously the increased cost of corn may stimulate less livestock production.

On page 2 I agree with the staff that it's appropriate to use a scenario approach with different combinations of input values to estimate the net greenhouse gas implications.

I believe it is appropriate across these assumptions that the staff average the results and not consider the results from one single scenario. I would note I might use a weighted average if I I had prior beliefs that some situations are closer to reality than others. In this case I would agree that a simple average is appropriate if there are no priors.

In the current analysis it appears that the staff has appropriately examined the current major liquid fuel sources including ethanol from conventional crops and biodiesel from conventional sources which are our only agricultural sources as of now. I do believe it will be worthwhile in the future to add ethanol from cellulosic sources, jet fuel may also come into the picture.

On page 5 I again have raised a wording issue. I do not totally agree with the statement that any demand that is not met locally is transmitted to the global marketplace and met by production of the agricultural commodity in other countries. In particular this could be met elsewhere in California, the rest of the US or globally. Also it is possible that this demand is not ever met when the cost in the other countries is more expensive than the result in market price. I might use wording more like where it could be met by production in other countries.

Elaborating I think some of the published findings with GTAP find the demand is not being completely replaced. I also recall a study by Murray and Wear that is references in the Murray, McCarl and Lee leakage piece where 86% of the reduced public timber harvest in the Pacific Northwest is replaced from sources in Canada, the US south and private lands in the Pacific Northwest. This means 14% of the market place reduction was never replaced.

On page 5 I believe one could elaborate a little bit upon the domino effect that is referred to here to illustrate a little more of the complex cities of the issue. What seems to happen in Brazil is that corn expanded in the far south displacing soybeans, then soybeans moved further north displacing grass and the livestock that were eating that grass. Then the livestock moved into the rain forest areas and land-use change occurred. The point is there may be more than one domino falling in the total process.

I also again would not solely limit my attention to indirect land use but talk about indirect land use and emissions changes in other emission categories as this ignores a possible intensification and livestock production reduction response.

On page 4 I again believe it was appropriate for ARB staff to select the GTAP model. I agree it is mature. I believe the model scope description is appropriate. I believe you could strengthen your wording a little and say GTAP is widely used around the world and profession in various forms.

One page 4 I believe the statements about the AEZ model are appropriate and that this was an appropriate model to use and that it has a strong scientific basis.

I believe the modifications made to the GTAP and AEZ models were appropriate and needed. I believe this is a quite satisfactory modeling platform for the ARB analysis with a strong science and databases and that it has been appropriately modified to meet the needs of the ARB LCFS program requirements.

I believe doing the scenario runs that an average for each biofuel is appropriate.

I do believe that in the future it would be desirable to analyze a slightly wider variety of liquid fuels then appears within the list from corn ethanol to sorghum ethanol that is appears on page 6. In particular I think the staff might begin to address cellulosic ethanol since were just beginning to see commercial production and from what I hear jet fuel is emerging.

I do believe that the wording could be improved here in this discussion of scenario runs it would be nice to add another sentence or two on what the nature of those scenarios were i.e. alternative yield responses or the like.

Finally on page 6 I do agree that ARB staff has reached the right conclusions relative to the assumptions and input parameters in the GTAP and the AEZ models. I also believe those models were sound scientifically and data wise and thus were appropriately used to estimate indirect land use. I am unsure whether the analysis is actually broader than a ILUC analysis incorporating use of other inputs and possible livestock reductions. I believe G tab by its very nature would do that analysis but I'm not sure whether or not the ARB GHG accounting picked that up.

All things considered I agree that the models were applied appropriately to develop estimates relative to indirect land use change that can be used under the LCFS.

Comments based on attachment one plain English summary of staff's methodologies in calculating fuel carbon intensities

On page number one I'm a little confused by the referencing to the GREET model as in the technical memorandum it is referred to as GREET 2013 but here we see GREET 2014. Which one is being used? Or are these two names for the same thing?

On page number 2 under the bullet for feedstock production I might talk about feedstock production and production of major fossil fuel bearing inputs to include fertilizer, pesticides fossil fuels consumed etc.

Between page 2 and page 5 there is redundancy in the discussion of the California version of the GREET model. In particular there are two different discussions of what revisions were done and I would think including a single list of them all in one place would be valuable. Also I noticed that in staff report 1 that the shorter list is used.

On page 9 of the document there's a statement that I think should be more nuanced. In particular you say the diversion of crops from the food or feed markets to biofuel production creates an additional demand to produce the biofuel feedstock. I don't think that diversion create new demand. Rather it competes with existing demand. I would say it creates or it leaves unfilled demands in the food and fuel markets and therefore creates a demand to replace that food and feed from somewhere else.

Also in the next sentence rather than limiting discussion to the global marketplace I would say to the marketplace outside the region whether it be other areas in the United States, or the globe. Indirect land use does not only occur internationally it can also occur if California reduces production of some goods in favor of bioenergy and production is increased somewhere else in the US potentially on previously unused lands. While this section refers to indirect land use there is also use the possibility of more intense land-use in other regions for example with increased use of double cropping or less abandoned acres, both of which may well increase emissions from additional inputs. All of these would be present in the GTAP model in some form or fashion although it does not potentially do a very good double cropping.

In the total LCFS analysis in the future I would not dwell solely upon iLUC emissions as the only indirectly stimulated emissions. Rather I would also attempt to account for indirect stimulated emissions coming from other increases and decreases in emissions elsewhere in the world that may come from intensification and livestock use responses.

I do not believe that GTAP uses a baseline where supply equals demand in all sectors. I believe it is possible in the GTAP structure to have more supply than demand. For example demand for agricultural land in Brazil may not have total supply = total demand rather there may be other lands it can be drawn into agricultural land if the price is high enough and at current prices there may be more land available than is used. This is also true in terms of say corn Stover where the

current market price is basically just the cost of collecting it in at the farm level the price is zero as there's a greater available supply than there is a demand.

In GTAP I believe that there also are increases in emissions from intensification (more irrigation or fertilization) so that the characterization of it only in terms of indirect land use change is not accurate.

In improving the indirect land use analysis when you're looking at corn ethanol byproducts there are also newer developments in terms of extracting corn oil from the DDGs.

In recent work Bruce Babcock has been looking at how intensity measures such as double cropping and less acreage abandonment have been stimulated by bioenergy prices and this may be something that analysts may want to look into in the future.

On page 11 I don't like the wording about the economy moving away from equilibrium. Rather I would say save moving the economy away from the current equilibrium to a new equilibrium.

On page 11 you indicate that irrigation was added to the model and I think this is a good move. I do think it's very important to have the water constraints on maximum use as for example that is a big factor here in the United States in many regions. I also think it may be important to have a maximum irrigable land constraint so that irrigation cannot move on to marginal lands. Generally such lands are distant from water sources and highly unlikely to ever be irrigated.

On page 11 you specify your fuel production increase and call this a shock. I think it is possible given the energy and corn prices that we may see fuel production move beyond say the limits imposed by the renewable fuel standard. As a consequence I think you might also need a market structure regarding the demand for bioenergy with it substituting in terms of heat content for petroleum-based gasoline.

On Page 12 there's a discussion of how yields respond to prices which is a good addition. However there might also be a discussion about how input usage and related emissions respond to yield increases. In particular in work I have done the elasticity of input usage response to yield increases is about 0.5 meaning that if you increase yields by 10% that you have a 5% increase in inputs including pesticides, harvest and probably fossil fuel inputs. Note You wouldn't, given recent US history, have much of an increase in US fertilizer use say for corn, but you might well for other crops. There also is likely to be an increase in double cropping and a reduction in idle acres particularly in international settings as shown in the recent work by Babcock.

In terms of the expansion on to marginal lands I believe that it would be good to have in the future a more rapid diminishing yield productivity as the marginal lands expand. The lands that I see around where I live that are marginal would clearly have diminishing productivity as you used more and more of them. Also I believe that it may well be necessary to restrict marginal

land production to only certain crops like energy crops like switchgrass rather than prime agricultural crops like rice, wheat and corn.

On page 14 I think it's highly appropriate to have the localized AEZ emission factor data that was developed.

On page 15 I find myself in concurrence that the ARB staff concluded that the assumptions are reasonable and that the models were applied appropriately. Naturally in a modeling exercise it's also was possible to spend more money and improve some of the assumptions and I've entered a few suggestions above. I do believe at this point of the model is appropriate, scientifically sound and well grounded in the data and that this means it is scientifically valid for use.