Comments to the California Air Resources Board

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THE INTERNATIONAL COUNCIL ON CLEAN TRANSPORTATION

ICCT comments on the preliminary revised CARB iLUC modelling exercise

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Following the public workshop of 14 September, the ICCT would like to make the following comments regarding the draft revisions to CARB indirect land use change modelling. As a general point, we would note that while the report provided by Professor Tyner has been very helpful, clearly there are many details of the preliminary results that have not yet been made public. We have therefore commented based on the information available, in some cases making assumptions about the underlying results. We suggest that CARB should bear this in mind when considering these comments.

Revised GTAP modelling report

The revised GTAP modelling is based on both a set of changes discussed by the Expert Workgroup, and further changes adopted at the discretion of CARB/Purdue not considered by the Expert Workgroup. We support the consideration of modelling changes beyond those considered in 2010, but at the same time recommend additional caution when considering the adoption of changes that have not been given external scrutiny. Our comments on the various modelling revisions, and the preliminary results for the different feedstocks, are presented below:

Updated energy elasticities

In principle, validating the GTAP model against historical data seems to be appropriate and commendable. We see no reason to dispute the revisions to the energy elasticities.

Improved treatment of DDGS and oilseed meals and oils

Improved modelling of the DDGS sector is commendable. We are not able within the time frame for these comments to provide any assessment of the Teheripour et al. and Taheripour, Hertel and Tyner papers upon which improved byproduct modelling has been based. We would encourage CARB to assess this evidentiary base, but presuming that the conclusions of these papers are reasonable we see no reason that these improvements should not be captured. We would sounds a note of caution on the particular issue of the quantities of feed displaced by bioethanol byproducts. Some feeding trials have identified substitution rates of more than one kg of feed displaced by every kg of DDGS. However, analysis we have undertaken with Professor Kirk Klasing suggests that these results are misleading for the market as a whole. We therefore recommend that total displacement of approximately one kg of other feed products for every one kg of DDGS should be modelled. We recognise that this may be consistent with the recent modelling, in which case all the better.

It is unclear to us without further analysis what the implications are of splitting soy meal and oil away from other oilseeds. While in principle this seems like a commendable innovation, we are slightly concerned, especially given the slightly surprising soy biodiesel results, that separating the oil sectors may have resulted in inadequate substitutability between vegetable oils. We would recommend this question for further examination.

Separation of soybean biodiesel from other types of biodiesel

This seems entirely appropriate, insofar as splitting the soy oil from other vegetable oil markets has not generated unlooked for side-effects (see above).

Modified model structure for the livestock sector

The revised livestock nesting structure seems to be an appropriate improvement. On a more long term basis, we would suggest that it would be interesting to consider constraining the livestock feed market more strongly to the nutritive content of different feed ingredients. We recognise that this might be non-trivial within the GTAP framework.

Revised land conversion factor for new cropland

We note that the Expert Workgroup expressed a position in support of the TEM system for new land productivity,

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but that this support was rather cautious based on the discussion we listened to. We believe that it would be appropriate to subject the TEM system of marginal productivity allocation to further testing, ideally comparing predicted productivities with observed productivities for real case of expansion. We have not had access to adequate TEM data to attempt such comparison in any detail ourselves. Such example data as we have looked at did not seem to be a good indicator of observed yields on new land, but we have no strong conclusion without further data to analyse. We would be interested in discussing potential approaches to validate the TEM results further.

Incorporate cropland pasture for US and Brazil and CRP for US

The addition of cropland pasture, especially if it is not treated as having carbon consequences on conversion, seems to be very important to these results. The way that cropland pasture is nested in the model seems worthy of additional examination. Notably, we suggest that additional work may be useful to directly assess the substitution elasticity of cropland pasture with other cropland types, in particular to explore whether it is appropriate to have the same elasticity for e.g. CP to soy conversion as e.g. corn to soy conversion. It is crucial to the modelling results that the comparative ease of conversion of CP to crops against forest to crops is not substantially overestimated or underestimated.

Endogenous yield adjustment for cropland pasture

The response of yield to price for cropland has been the subject of extensive discussion in the LCFS process and elsewhere. We are concerned that a relatively strong response to price of cropland pasture yields should be introduced without more extensive consultation and evidentiary basis than we have seen to date. While it seems potentially reasonable to assume an elasticity > 0, it is not clear to us whether the appropriate value is closer to 0 or 0.2 (Brazil)/0.4 (US). In this context, and given that this is an entirely new innovation not considered by the Expert Workgroup, we would suggest that it would be appropriate to delay including any positive elasticity pending further work.

Greater flexibility in cropland switching in US

We have already shared with Professor Tyner some basic analysis we have undertaken considering this adjustment to the model. While the narrative underlying the change is reasonable, we believe that the statistical support for increased flexibility in crop acreage shifts remains weak. We believe that the value of 0.5 was probably reasonable, but that the evidence is not compelling that it should be raised (we are not aware of clear evidence that it was not potentially set high already). We therefore do not see compelling reason to adopt a higher value. Having said this, it is not clear that the basis for using 0.5 is substantially stronger than that for using 0.75, and hence while as a point of process for CARB we would like to see further evidence before a revision such as this was adopted for the regulation, we would consider adopting the value of 0.75 acceptable pending further consideration.

Update to GTAP version 7

Updating to the more up to date database seems commendable and appropriate.

Separate soybeans from other oilseeds; Separate soybean biodiesel from other types of biodiesel

The data processing for these splits seems entirely reasonable.

Revised emission factors to be provided by CARB

Discussed below.

Land use impacts of US corn ethanol

We have no specific comments on the corn modelling beyond our commentary on the model updates in general.

Land use impacts of US soybean biodiesel

The soy biodiesel land-use results seem very surprising, in particular that GTAP has mapped out a scenario in which soy biodiesel demand actually drives reforestation compared to the baseline. This result seems somewhat counter-intuitive, and apparently relies on soy expanding substantially on low carbon (or at least unforested) land in the US to meet additional vegetable oil demand, resulting in a substantial additional soy meal production.

Especially given that the soy sector has been newly broken out from other oil sectors, we believe that it would be appropriate to undertake significant additional analysis and perhaps further consultation to explore the plausibility of this result before it would be applied in the LCFS regulation. It is our understanding that the changes breaking apart the oilseeds sector were not considered by the Expert Workgroup, and in that context such substantial changes to the modelling outcomes seem to warrant this further consideration.

We would also like to make a few specific comments. We note that Professor Tyner has identified sensitivity around the Armington assumptions as a priority for future work. One counter scenario to the apparently very strong increase in soy production to meet additional soy oil demand would be a shift of vegetable oil demand to other oils such as palm oil. Given that the meal market has been widely treated as the dominant driver of soy production, we wonder whether the Armington assumptions in GTAP are set to unduly limit the substitution of oils such as palm for soy.

Professor Tyner's report notes that reforestation occurs in regions of the world other than Brazil and the US, apparently because oil meal exports reduce demand for oil meals in other regions. This reforestation is greater than deforestation due to expansion in the US and Brazil. This implies that oil meal production outside of the US and

Brazil is more strongly connected with deforestation than soy expansion in the US and Brazil. For Brazil in particular, this is potentially a surprising result (we recognise that an alternate explanation would be that very weak deforestation in the US masks strong deforestation in Brazil). We would suggest that CARB examines whether the comparative oilseed-deforestation linkages in the US, Brazil and elsewhere are plausible. In particular, we doubt that such a significant increase in world vegetable oil demand could be consistent with an actual increase in forestation (compared to baseline) in Malaysia and Indonesia, given that palm oil is well connected to other vegetable oil markets and strongly associated with deforestation.

Finally, we are interested in the possibility that running the corn and soy scenarios simultaneously might alter the results and be more representative of the 'real' iLUc impact of expanded soy biodiesel. This might occur if land competition between corn and soy restricted soy expansion in the US more than would otherwise be the case. As a sensitivity to non-linearity driven by land competition, we would suggest running one or both of the following additional scenarios:

- A scenario with an increase of 0.812 billion gallons of soy and 11.59 billion gallons of corn, compared to a baseline that includes an increase of 11.59 billion gallons of corn.
- A scenario with an increase of 0.912 billion gallons of soy and 11.59 billion gallons of corn compared to a baseline with increases of 0.812 billion gallons of soy and 11.59 billion gallons of corn.

Land use impacts of Brazilian sugarcane ethanol

We have no specific comments on the corn modelling beyond our commentary on the model updates in general., except to note that the difficulty of dealing with unmanaged forest in GTAP remains a particular concern for countries like Brazil with substantial annual unmanaged forest loss.

Sensitivity analysis on assumption regarding food consumption changes

The apparently greater sensitivity of changes in forest cover than pasture cover to changes in food demand is a little surprising, and seems worthy of further exploration, as it may suggest unrealistic behaviours elsewhere in the modelling, and the possibility that forest conversion is underestimated in the 'central' scenarios.

We are somewhat cautious of arguments about Government action to offset food consumption reductions as a justification for holding food consumption constant – we anticipate that biofuel programs will drive real reductions in food consumption. We do, however, recognise the potential value of setting food consumption constant in the modelling as a way to relatively incentivise feedstocks expected to cause less consumption reduction, which might be considered a legitimate aim.

Sensitivity analysis on the yield-to-price elasticity

We have carried out extensive work on yield-to-price elasticity, including commissioning Steve Berry and Wolfram Schlenker to undertake improved econometric analysis of historical data. The report of this work has been submitted to CARB.

Steve Berry in his previous report to CARB during the Expert Workgroup process has detailed, in our opinion compellingly, that the value of 0.25 for yield-to-price elasticity currently used by GTAP and based on Keeney and Hertel (2008) is at best poorly supported by existing analysis. He also notes that the existing studies (even given that they do not support a value of 0.25) are relatively poor.

Berry and Schlenker's analysis aims to be the highest quality econometric analysis of historical short run yield-to-price elasticity available. For almost all cases examined, in both the US and elsewhere, they find no statistically strong evidence of a positive net yield-to-price elasticity. A value of 0.05 would be consistent with the higher end of the statistically non-significant outcomes they find. They also note that the historical data shows no strong evidence of a longer run price link, with the rate of yield increase showing no systematic variation between protracted periods of high price and protracted periods of low price.

Given the lack of evidence to support a value of 0.25, and availability of studies suggesting that the link between price and yield is extremely weak, we believe that the elasticities of 0.1 or 0.05 used for sensitivity analysis by CARB would be more appropriate than 0.25. The choice might be informed by the implied yield-to-price elasticity component resulting from reduced marginal yield (i.e. we contend that the net effect is apparently near zero, not that the 'pure' response should necessarily be modelled near zero if the extensive contribution is significant).

Professor Tyner comments that, 'We do not in reality know if the appropriate value for the yield-to-price elasticity is 0.25 or higher or lower.' While we agree that there is only limited certainty, we would argue that:

- There is sufficient evidence to believe that a value of 0.25 is unduly optimistic; and
- There is a strong message from the econometric analysis of Berry and Schlenker and other work that the effective area-to-price elasticity is much greater than the net yield-to-price elasticity. We would hope that the GTAP results will be consistent with this hierarchy.

Sensitivity analysis on the cropland transformation elasticity

This parameter seems to have mixed impacts on the results, and as mentioned above we do not have a strong basis for favouring one value over another, except a general preference that CARB should in general apply a relatively high burden of evidence before making model adjustments.

Sensitivity analysis on endogenous productivity increase for cropland pasture

We note above that we see little evidence of the magnitude of this elasticity, even granted that there is a narrative that there should be some elasticity here. The introduction of the cropland-pasture category in and of itself seems to reduce the modelled iLUC impacts of biofuels, and we would urge CARB to be extremely cautious of adopting a further change that compounds this effect without a high level of confidence that the introduction of cropland pasture has been realistically captured, and substantial evidence to support the introduction of that further change.

In the absence of solid evidence that the suggested values of 0.4 and 0.2 are more appropriate than a value of 0, evidence of which we are unaware, and given that this was not considered by the Expert Workgroup, we would recommend that CARB should maintain a value of 0 for endogenous cropland pasture yield response for the time being.

Geographically-Explicit Estimates of Soil and Biomass Carbon Stocks report

We welcome the effort to enhance the estimation of carbon stocks for calculating the emissions implied by the GTAP scenarios. Clearly, to the extent that it is possible to match the resolution of carbon stock assumptions to the resolution with which GTAP models agricultural expansion, it is appealing to do so.

We have not considered Dr. Gibbs' report and the underlying work to an adequate extent to make detailed comments. We have the following general comments:

- Dr. Gibbs' analysis seems in general to be a reasonable basis to move forward and to offer a potential improvement on the previous system.
- Dr. Gibbs mentioned the aspiration to include unmanaged land in GTAP. *If* the biomass estimates for unmanaged forest are systematically higher than for managed forest, and given the reality that one might expect to see unmanaged forest conversion, this might introduce a tendency to underestimate carbon loss when applying these carbon stocks to the GTAP outcomes.
- Given the importance of cropland pasture as a land category in the new GTAP work, giving due consideration to the carbon cost of converting cropland pasture should be a priority.

Emissions factor model report

Again, this work seems like a constructive and appropriate addition to the modelling framework. We have not considered Dr. Plevin's report to an adequate extent to make detailed comments, but have the following general comments:

- In GTAP outputs, 'avoided reforestation' can look the same as 'deforestation', and similarly 'avoided deforestation' can look the same as 'reforestation', when scenarios are compared to the baseline. Dr. Plevin notes that 'Using net changes underestimates emissions since carbon loss is faster than regrowth'. We would encourage careful consideration of these issues.
- As noted above, emissions from cropland pasture to cropland conversion would be a key concern.
- It might be appropriate to include the variable underlying biomass content of different crop types, notably perennials like oil palms for which a relatively high persistent biomass content develops compared to some other crops.