THE INTERNATIONAL COUNCIL
ON CLEAN TRANSPORTATION

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Comments on the European Commission Directorate-General for Energy Indirect Land Use Change Literature Review

1225 I Street NW, Suite 900
Washington DC 20005

One Post Street, Suite 2700
San Francisco, CA 94104

48 Rue de Stassart. bte 6
1050 Brussels

www.theicct.org

Summary

One of the four consultation documents[1] published by the European Commission to inform its 2010 consultation on indirect land use change is a literature review by the Commission's Directorate-General for Energy (DG ENERGY), entitled, *'THE IMPACT OF LAND USE CHANGE ON GREENHOUSE GAS EMISSIONS FROM BIOFUELS AND BIOLIQUIDS: Literature review'.* This has some value as a contribution to the consultation insofar as it identifies and quotes from a moderately extensive set of relevant references. The inclusion of references by page to the information in the documents considered is particularly welcome, and helps make it a useful reference to that part of the wider literature that is covered.

More broadly, however, the review falls short of constituting a full literature review in the traditional sense. In particular, in general it does not offer substantial assessment of the quality of different data sources, or make appropriate assessment of the importance of various uncertainties. While this lack of assessment is understandable given the considerable scope of the attempted exercise, it severely limits the usefulness of the review.

In addition, the literature review was not able to fully capture the sheer volume of available evidence, much of it presented in academic journals aimed at particular specialisms. The review therefore leaves many questions hanging that should have been addressed. There is a significant risk that this might lead readers to believe that there are more gaps in the literature than is in fact the case.

A fuller qualitative assessment of the literature in specific areas may well be appropriate and necessary over the coming years, especially if the Commission intends to run further modelling exercises in future to help improve modelling results. There is a clear parallel for such a process in California, where the Air Resources Board has an expert working group on ILUC, and various subtasks in key areas. Just as in California, however, the clear benefits of additional work should not be taken to preclude the adoption of regulatory measures at this stage. The outcomes of the ARB expert group, as well as work currently being done by the ICCT, should provide valuable inputs to the Commission if considering regulatory revisions in future.

The review also contains original calculations and commentary that goes

1 http://ec.europa.eu/energy/renewables/consultations/2010_10_31_iluc_and_biofuels_en.htm

beyond the scope of what a literature review would normally contain. For instance, the review presents apparently new figures (there is no reference given) for the volumes of fuel required to meet the RED/FQD. These numbers are then used to criticise modelling studies, which is beyond the scope of a normal literature review. For instance, the review makes the unequivocal statement, 'In all the modelling exercises where the choice is clear, the policy scenario exaggerates the amount of biofuels from crops … the total share of biofuels from crops will be no more than 8.6%'. Assuming that these additional calculations are necessarily correct and hence criticising any inconsistent estimates from the literature is a flaw in the document especially as there are major discrepancies extant between DG ENERGY's calculated projections for biofuel use and the higher projections in the subsequently published National Renewable Energy Action Plans.

Without a critical evaluation of sources and given the partial coverage of the literature, the document can better be considered as a guide to existing sources than as a full 'literature review' in the conventional sense.

Beyond limitations of scope, we believe that the summary sections of the review tend to understate the likely magnitude of indirect land use change. In general, the review tends to emphasise uncertainties that might reduce the land impacts of biofuels, while playing down similar uncertainties in the other direction. Examples of this are:

- The review treats central yield predictions by the USDA as minimum values, while presenting maximum feasible estimates of overall yield growth by ADAS as if they are a baseline to which price induced effects should be added.
- The review states that no models include price induced technological yield improvements. This is not strictly correct, as innovative yield improvements can be represented through increased capital and labour inputs replacing land inputs in production functions (especially the case for developed economies).
- The review implies that IPCC soil carbon per hectare estimates are too high, because IPCC estimates for total global soil carbon are higher than two other sources. However, the per hectare soil carbon estimates are actually lower than other datasets.
- The review continually refers to 'non-linearity' in the modelling. It asserts that most models overestimate EU biofuel demand. Not only do most

modellers agree that the models are approximately linear (the particular case of IFPRI MIRAGE is addressed in more detail below), but the review neglects to make the reverse point for cases where model baselines are likely to underestimate *global* biofuel demand.

While the review highlights areas of uncertainty where there would indeed be significant value in further work, it also gives undue prominence to other areas. Examples of this from the Executive Summary are:

- ‘Main issue’ 2. The review says that studies are flawed because none of them “evaluates the land use change impact of biofuels *per se,* as opposed to that of particular biofuel promoting policies”. As the current consultation is specifically about a well-defined biofuels policy, it is unclear why this would be problematic rather than desirable.

- ‘Main issue’ 6. As discussed in more detail under ‘Baseline Assumptions’, below, the review implicitly asserts that if cropped areas shrink rotational agriculture will expand. No evidence is presented from the literature in support of this claim.

- ‘Main issue’ 10. The review criticises modelling for not taking account of the sustainability criteria within the RED. As biofuel production is a small minority of overall production, it seems unlikely that sustainability limitations for biofuels alone will have a significant impact on the areas in which land use changes occur *indirectly.*

- ‘Main issue’ 16. The review argues that biofuel should be compared to marginal fossil fuels. This issue does not affect the validity of modelling outcomes that could be used to determine ILUC factors – the choice of fossil fuel comparator is a separate issue.

In general, the review does not quantify the likely magnitude associated with the highlighted uncertainties. Analysis of which aspects of the models, and hence which assumptions, drive the differences in outcomes would allow a more nuanced consideration. The ICCT is engaged in just such an analysis and aims to make the results available by the end of the year. We also note that exercises of this sort have already been undertaken by the JRC in the comparison of ILUC modelling studies that is part of the current consultation, and by Peter Witzke of Bonn University and collaborators (the latter published only as a symposium discussion paper to date).

When discussing models, the review does not clearly identify which assumptions are inappropriate and which are simply poorly documented. Possibly due to shortcomings in the documentation of some studies, the review repeatedly states that the reasons for an assumption are not stated, but is unable to clearly identify whether they are in fact arbitrary. Further background research, and in all likelihood direct contact with modellers, would be needed to properly identify whether or not such assumptions are indeed problematic.

The review will undoubtedly be helpful to some consultees. However, anyone using it, and particularly anyone relying on its summary sections, risks being misled as to the state of some aspects of current science on indirect land use change, and the availability of additional evidence on any given topic beyond that referenced in the review.

Overall, the review unduly stresses areas where ILUC modelling might underestimate rather than overestimate ILUC effects:

- *Neither consultees nor the European Commission should conclude on the basis of the review document that existing models probably overestimate ILUC emissions*.
- *The review document should not be treated as fully comprehensive either by consultees using it to inform their submissions, or by the Commission in finalising its report on ILUC.*

The following comments address in more detail areas where there are issues with the analysis as presented.

Contents

Baseline scenarios

In the *Executive Summary* and *Chapter Summary,* the review notes that none of the modelling addresses the impacts of biofuels *per se.* Given that the primary question for the EC under the consultation is what the indirect impacts are of implementing the biofuels policy defined by RED/FQD, it is unclear why modelling a zero biofuel baseline would be of particular relevance to the Commission's thinking, unless an outright biofuel ban is under consideration.

The possibility of non-linearity in ILUC outcomes is exaggerated in a one-sided fashion, and the extent and source of non-linearity reported in model results is not properly addressed. In particular, the IFPRI-MIRAGE study for DG TRADE in which 'non-linearity' is evident can be well understood as a sum of two relatively linear behaviours – one for bioethanol use and one for biodiesel use. It is the imposition of an exogenous fuel split that drives strong non-linearity in the average outcome, rather than the characteristics of the model itself. The study imposed a fuel split of 45% ethanol to 55% biodiesel, which in the context of low existing market penetration by ethanol leads to ILUC predictions based almost entirely on ethanol expansion – largely in Brazilian sugarcane. The increase in biofuel use to 5.6% is ethanol dominated, but any marginal increased in biofuel use beyond this is 55% biodiesel. This fuel split is based on the split between diesel and petrol use in the EU. It is not clear that this is a good indicator for likely biofuel proportions – especially as the current split is closer to 80% biodiesel, 20% ethanol.

The Member State National Renewable Energy Action Plans (NREAPs, published since the launch of the consultation) provide evidence that this exogenously imposed split is erroneous – Member States in fact predict a 2020 share of 75% for biodiesel and 25% for ethanol. Based on this, the 'non-linearly' higher marginal impact predicted when biodiesel expands is likely to be closer to an accurate prediction.

Other studies are approximately linear in their predictions, largely by design in the models. This conclusion is reflected in the JRC ILUC modelling comparison study. Thus, the appropriate conclusion for the literature review should have been that, based on modelling to date, ILUC impacts can be considered approximately linear. In the IFPRI-MIRAGE study, an estimate for the fuel split more consistent with the NREAPs would be expected to give an average ILUC emissions value that was in fact 'non-linearly' *higher* than the average value in the report. It is hence inconsistent for the review to refer to non-linearity in the IFPRI study while tending to argue that non-linearity is likely to mean that ILUC emissions numbers are overestimates.

Given that uncertainties clearly act in both directions, the review should have stressed at least equally that non-linearity would also increase the likely magnitude of any underestimates. Discussing non-linearity pre-dominantly as likely to exaggerate any over-estimates of ILUC is misleading.

Baseline assumptions

The review questions the assumption in the literature that abandoned agricultural land (if crop area shrinks in the absence of biofuel policy) will revert to grassland or forest, and hence that biofuels should be held accountable for 'foregone sequestration'. It claims that in general it is not obvious that abandoned land will indeed revert to a higher carbon stock state, however it does not provide evidence of cases, regions and/or land types in which low/zero carbon sequestration on abandoned agricultural land would be likely. While examples of low carbon sequestration post-abandonment do exist, such as *imperata* grassland in Indonesia and desertification, these seem likely to be exceptional – in particular as crop shrinkage is most likely to occur in Europe.

The review also claims that some cropland shrinkage will be associated with land entering rotation, rather than being entirely abandoned. This would only be a valid argument if one assumed that as overall cropland shrinks, farmers would use rotation for a higher proportion of their crops. Otherwise, farmers will already be using as much land rotationally as is required. The review does not provide any evidence from the literature for such an increase.

If some abandoned agricultural land does enter rotation, this should also be considered in the analysis of biofuel induced yield change. In general the switch from rotation to longer term monocropping would be expected to reduce yields. The review implies that without biofuel policy rotational use of land would increase, but fails to make the link with yield change.

The review goes on to argue that afforestation will be largely policy driven. If afforestation only occurs due to policy, then any residual land reversion of additional abandoned land would be to grassland – i.e. land abandonment in the absence of biofuels policy would not have any effect on overall afforestation rates. As pointed out elsewhere in the review, the policy environment does seem to be shifting in favour of more afforestation and more nature protection. However, abandoning additional land in the absence of biofuel policy could still affect the rate of afforestation. Indeed, various types of afforestation policies might increase the likelihood that land abandoned in the absence of biofuels policy would become afforested. Only an afforestation policy based on an absolute quota would have the effect suggested by the review. Without further evidence and analysis, it seems plausible that a context of policy driven afforestation could actually increase the level of forgone sequestration attributable to biofuels, rather than reduce it.

Policy scenarios

The review claims that several modelling exercises overestimate the biofuel supply implied by EU policy. It points out that the EC's assessment of likely crop biofuel requirement to meet the 2020 target is 27 Mtoe to supply 8.6% of energy. More optimistic scenarios for cellulosic ethanol and electric cars would significantly reduce the crop biofuel requirements. The review executive summary states that all models are unclear or assume higher biofuel use. However, the review also states that ESIM assumes a 7% penetration, which is lower than the EC maximum estimate. IFPRI MIRAGE assumes a 5.6% penetration, which the review *explicitly identifies* with its middle projection – clearly the assertion in the executive summary that all models are unclear or over-estimate demand is incorrect. It also seems likely that models that consider global supply may well use values consistent with an EU penetration of 27 Mtoe or less.

It is also possible that models that overstate likely EU biofuel consumption in 2020 still understate *global* use (with other regional mandates being ignored). The review itself points out that this is true of both the IFPRI study and ESIM. In these cases, any non-linear effects that scale with overall increase in biofuel consumption would be likely to be understated, not overstated as the review implies.

The review further suggests that if models are non-linear, a lower baseline biofuel use will result in lower average emissions for each extra unit of biofuel. While this might be correct, a logical response to such a finding would be to consider whether, given non-linearity in the system, a biofuel mandate should be reduced, as a reduction in mandate size could deliver disproportionate ILUC mitigation benefits. The review does not draw this link.

The implication from the review is that ILUC should be spread out in a way that minimises the amount attributable to each MJ of biofuel. This is consistent with the assertion in the summary that the impacts of biofuel *per se,* rather than biofuel policies, should be assessed. It makes this association specifically in regard to the IFPRI study (which has a relatively high biofuel baseline compared to Aglink, ESIM). It does not, however, link this comment to that fact that the level of biofuel penetration in the IFPRI policy scenario is actually remarkably low (compared, for instance, to the Member State NREAPS[2] submitted to the EC), nor to the fact that the non-linearity in the IFPRI results arises primarily from exogenous assumptions on the penetration of ethanol in the biofuel mix (which are also inconsistent with the published Member State National Action Plans and which result in an extremely low prediction for increase in biodiesel use, of only 1.9 Mtoe by 2020).

The review also points out that the biofuel feedstocks used are in general determined exogenously rather than endogenously. While this may be so, it is not of particular relevance to the usefulness of the outcomes. Insofar as models attempt to assign differential ILUC factors to different feedstocks, it is clearly necessary that they should model the use of those specific feedstocks. Whether the increases in specific feedstocks used in specific models are the result of exogenous assumptions or endogenous relationships is not a key driver of the outcomes (assuming that outcomes are moderately linear). Exogenously imposing increases in single feedstocks does, however, render the marginal analysis less complex. It therefore seems an inappropriate point to emphasise, especially

2 Based on Member State Action Plans as published at August 1st 2010 http://ec.europa.eu/energy/renewables/transparency_platform/action_plan_en.htm

since the reviewers do not identify points in the existing literature where the same criticism is expressed.

Yields

The review correctly identifies the important role of yield assumptions in the baselines and policy scenarios. However, some of the commentary around yield assumptions is not entirely accurate.

The review considers a small selection of future yield predictions, notably the USDA prediction cited by Trostle, quoted by Searchinger, and the ADAS results produced for the Gallagher Review. The USDA figure is referred to in the commentary on this section as the 'lowest estimate'. This misrepresents the fact that this value, 0.8%, is not a minimum prediction but the USDA's best estimate. A true minimum scenario would be somewhat lower – indeed, such a lower scenario is included in the ADAS report.

The USDA number is relatively consistent with the ADAS 'business as usual' predictions. Indeed, ADAS's maximum figures are based on 'maximum feasible increase'. Thus, the review treats a likely scenario as the 'minimum', and treats a number that is explicitly at the upper bound of what is plausible, and that **already includes possible demand led yield growth** as the other end of the likely range for the baseline. The review ignores the passage within the ADAS report where ADAS comments, 'Most projections of future yields for the period to around 2017 predict lower yield growth rates than historic trends for cereals.'

The review quotes ADAS at some length on historical yield growth, but fails to comment on the assertions *within that quote* that historical rates of yield growth have been broadly linear and arithmetic – i.e. yield growth as a percentage will 'inevitably' decrease with time. Instead, the review quotes ADAS's 'maximum' yield growth rates as a percentage, implicitly using a geometric rather than arithmetic growth trend.

The review also seems to suggest that the RFA's Gallagher Review incorrectly assesses the difference in land requirements associated with different yield scenarios, saying:

> 'RFA (2008) state that, '*High and low yield improvement scenarios result in approximately +/- 10% influence on total land demand for biofuels'*[3] – a

3 Gallagher review p. 29

> figure that is lower than the figure (at least 15%) that would be expected from comparing the "business as usual" and "maximum improvement" forecasts in the work from ADAS UK Ltd.'

It is unclear the basis upon which this statement is made, as a comparison of the quoted median for maximum increase (2.1%) with the median BAU increase (1%) would give an 11% increase in yield 2010 -> 2020, which is consistent with the RFA report. Part of the problem might be the apparent assumption in the literature review that yield increase in the ADAS study should be treated geometrically instead of arithmetically. Even this assumption, however, would give a difference of only 13%. The review does not attempt any similar commentary or analysis on possible lower yield scenarios in ADAS/RFA 2008.

In the chapter on model results, the review claims that none of the models allow for the possibility of increased yields through technological developments in response to demand. This is not strictly correct. Models such as GTAP contain production functions in which capital and labour inputs can be substituted for land. Innovative yield increases can be represented in the models by increased inputs of capital and labour. This is particularly true of developed agricultural economies, where, for instance, capital is unlikely to take the form of additional machinery (but could represent investment in innovative replacements for existing machinery). Based on the literature discussed in this section, it seems that any technology effects from price induced R&D investment are likely to be relatively minor, as the literature generally identifies publically funded R&D as the prime source of agricultural innovation. The lead in time for price induced innovation may also be significant – the JRC ILUC modelling study quotes a figure from IFPRI of a 17 year lag before innovation delivers results. This suggests that even if models do neglect to include price induced R&D investment, this may be reasonable over a ten-year mandate. It is also not guaranteed that increased R&D investment would be yield targeting – for instance, some R&D will target input reduction (although this could still reduce GHG impacts).

Even if, as stated in the review, all price induced yield increase in models should be understood as input led, this could lead to emissions increases associable with higher marginal nitrogen application – that this is not adequately represented in models is not noted by the review. This contrasts with the point later in the review where possible drivers of future fertiliser carbon intensity reduction are highlighted.

The review makes an argument for the inevitability of price induced yield improvements – 'price increases make investments viable'. It claims that it hence makes 'little sense' to deny that demand drives yield improvement. This neglects the (quoted in the body of the report) observation from Ecofys that, 'The adoption of new technologies by farmers has been incentivised by declining producer prices', and again that, 'while some support for the theory of price-induced innovation can generally be found, other factors than price also play an important role', and, 'innovations may also be input saving without increasing yields per ha'. Bouët similarly is quoted, 'This finding cautions against the efficacy of policies based on the premise that price signals alone induce efficient technical change'.

Given that the review itself presents these contrary viewpoints, the firm assertion in the summary that price *will* drive technology induced yield improvement seems unsupported. The sensitivity analysis presented in the summary is also somewhat biased, as only the results of assuming stronger response of yield to demand is analysed – there is no symmetry by presenting results from assuming weaker response.

The strong contention that price will effectively lead yield is also arguably at odds with the point made elsewhere in the review that 'decision making in the agricultural system affecting yields does not, in general, produce results that are anything like yield maximising.' If this observation is correct, it might conflict with optimistic assumptions about the efficacy of price as a driver of technology led yield improvement.

The summary language around price-induced yield also differs markedly from that addressing lower yields in more marginal plots. The summary represents the suggestion that land at the margins of production is likely to have lower yields than land currently in use as 'simply an assumption'. This statement contrasts with the way the review discusses modelling of price induced yield increases. The reviewers make an elementary argument for price induced yield increase, but also explicitly state that 'Econometrists have not yet found a way … to reveal the … positive, causal relationship' between price and yield. Given that the reviewers were unable to identify clear evidence either for price induced yield increase, or for extensification induced yield reductions, characterising the former as common sense and the latter as 'simply an assumption' seems to be arbitrary and to imply a degree of bias.

The review correctly identifies important questions about how marginal yield should be assessed, and whether it is always or generally true that currently used land is indeed the most productive. However, the review goes from a solid exploration of the lack of consensus in the literature on these questions to the assessment that,

> *'IFPRI's results must be considered to be upwardly biased by this assumption'*

and,

> *'That those of CARB, Tyner et al and Searchinger et al … must be considered to be even more upwardly biased'.*

Without any consensus in the literature either way on the 'real' magnitude of marginal yield effects, such strong statements are not warranted. It is also noted that the JRC ILUC modelling comparison study (page 103) refers to evidence from more than one source to support the theory that marginal yields will be lower. The reviewers might have given more credence to reduced yields at the margins of production had they been aware of this evidence.

Determining the type of land converted

The review correctly notes the problems associated with obtaining a confident assessment of future converted land type either by historical analysis (as whichever period is analysed may not be a good indicator for the near future) or by 'suitability analysis'. It then argues that modelling efforts fail to consider any land that is deforested for other reasons and then converted to agricultural use – in the sense that the full 'blame' for this conversion is assigned to agriculture. This is a legitimate point to raise, but the magnitude of this effect is moderated by the likelihood that deforested land would become afforested in the absence of intervention. While agriculture may therefore not properly be held accountable for initial carbon loss, it would be properly accountable for foregone sequestration, which would represent a smaller but comparable emission figure.

The review argues that foregone sequestration should not be accounted for by making a comparison to the treatment of existing mature forest. In short, the review points out that as existing mature forest is not sequestering carbon at a significant rate, the appropriate thing to do to maximise GHG reductions from mature forest would be to chop it down for bioenergy and then regrow it, and repeat. Concluding that 'policy makers and citizens' would not favour such a system, the review hence claims that accounting for foregone carbon sequestration doesn't make any sense.

This analysis ignores the fact that mature forest protection is not based solely on carbon sequestration opportunities. Any decision on whether mature forest should be protected should certainly consider carbon sequestration opportunities and opportunity costs – but the appropriate place for that discussion is in the context of forest protection policy.

To imply that because a consideration of carbon sequestration opportunity costs could support conversion of natural forest to plantation forest these costs should be ignored when accounting for biofuels is a *non sequitur*. There is certainly a policy space in which considering the opportunity costs of policies to reduce GHG emissions could go hand in hand with legislation to protect forest – and to paraphrase the review, it seems entirely plausible that policy-makers and citizens share this view of the appropriate direction land use change policy should take.

Estimating carbon stock changes

The review correctly identifies the variability in estimates of carbon stock of different land types in the literature, and the importance of these differences. However, the review summary emphasises the difference between IPCC estimates for total world soil carbon and two other estimates. Noting that IPCC gives the highest figure, the summary concludes that IPCC 'may tend to overestimate soil carbon'.

This conclusion is not appropriate, firstly because in the absence of further assessment it is possible that IPCC is the best of the three estimates, and secondly because there is no analysis of the cause of these differences, or whether an overestimate by IPCC of overall carbon stock implies overestimates for specific land types. Indeed, when considering specific land type carbon content, the review notes that the IPCC estimates are not the highest. Rather than counting this against the contention that IPCC estimates must be too high, it infers that the even higher estimates '[may] tend to overestimate soil carbon … to a greater degree'.

Results for biofuel land use change impacts

The review opens this chapter by indicating that preference has been given in comparing ILUC models to those scenarios in which the change in biofuel consumption between the policy scenario and baseline is largest. Firstly, this decision seems rather inconsistent with the (incorrect) assertion elsewhere in the review that all models considered overestimate biofuel consumption – if that were the case, it might seem logical to preferentially consider scenarios where the change in consumption was smaller. Secondly, this arbitrarily favours one scenario over another. The review does not justify this choice, and it is unclear whether a compelling justification would be available.

The review goes on to claim that most ILUC modelling exercises to not distinguish between the impacts from different biofuel feedstocks, regions and fuel types. It comments that the chapter is ‘thus based on limited evidence and should be treated accordingly.’ However, the reviewers failed to obtain feedstock specific results that are available. For example, the table of modeling results lists only one ILUC number from the IFPRI-MIRAGE study and did not include the readily available marginal ILUC numbers for various feedstocks that are also contained in the IFPRI-MIRAGE consultation document (and which are higher than the average for all feedstocks except sugars). It is also unsurprising that studies done for analysis of US maize ethanol mandates only give figures for ILUC with US maize as a feedstock.

Any shortage of modeling results for EU relevant feedstocks is suggestive of a failure by the Commission to undertake appropriate studies in preparation for the current consultation. It is particularly surprising that the reviewers did not include the results of scenarios run for the JRC and presented in the accompanying ILUC modeling comparison consultation document.

The reviewers go on to consider a decomposition of increases in production into land use change as opposed to other inputs, from the IFRPI model. It is not clear how the reviewers came to the full set of figures presented on page 191. In particular, the data on *Al Riffai et al* page 61 do not include decomposed numbers by region. The review makes note that additional wheat production is apparently being met through intensification. It is pertinent to note, however, that the review does not mention at this juncture that wheat is assigned a significant ILUC value in marginal analysis for the IFPRI study.

The review comments that the models ‘do not tell us whether an incremental demand for a particular feedstock in the EU is likely to lead to a different result, in terms of land use change, than an incremental demand for the same feedstock elsewhere.’ While it is true that most models do not model demand shocks in different markets, this is because they are modeling the results of policies applied in a particular region (generally either the EU or US). Fundamentally, an incremental increase in demand in Europe will be the result of the RED/FQD. There is scope for additional work to explore the results of shocking different markets, which could inform decisions about whether for instance ILUC factors should be heterogeneous by region. However, the approach taken in IFPRI MIRAGE of applying demand shocks within Europe for different feedstocks seems appropriate for the context of informing European policy.

Considering regional differences in land use change by land type, in different models, the review comments,

> *'They do not uncover, however, the direct and indirect effects at work that lead to the reported land use changes and therefore policy conclusions are difficult to draw except for those that target forest protection legislation and its enforcement especially in the regions where forest conversion is likely.'*

It is unclear what the reviewers mean by this. The data they are quoting is from the IFPRI-MIRAGE modeling exercise, in which the driver of additional land use change is explicit – an increased biofuel mandate. The increase in the biofuel mandate is the cause, thus the assertion that one cannot distinguish the direct and indirect effects is difficult to understand. As the review emphasises earlier on, the modeling is of the overall land use effect of biofuels. While one could certainly conclude that anti-deforestation policies would be desirable, one would equally conclude that biofuels will drive carbon emission through ILUC, and that a possible policy response to this would be to reduce biofuels mandates or impose additional conditions to avoid ILUC.

Biofuel LCA

The review addresses biofuel lifecycle analysis in Chapter 12. It is not clear why an overview of variation between previously assessed LCA values is presented in the context of the consultation, given that the RED/FQD have a well defined lifecycle analysis methodology written into them. Unless DG ENERGY wishes to recommend that this methodology should be revised, or that the Commission should have used different data sources, this section of the review has little relevance to the assessment of indirect land use change.

It is noted that the review accidentally misquotes the data on the 'GHG savings' listed from the ADEME study on review page 201. These are not GHG savings at all – they are emissions as a percentage of fossil fuel emissions.

The review also asserts that there are no figures available from the US EPA for emissions excluding indirect land use change. This is in fact not the case – the emissions of biofuels excluding land use change can in fact be readily calculated from the tables for ethanol and biodiesel respectively on page 255-256 and 259 of the EPA's final RFS2 rulemaking document. For corn ethanol, the number is

49 gCO_2e/MJ. For soy biodiesel it is 8 gCO_2e/MJ. These numbers should be considered somewhat different in character, however, to the other figures quoted in this chapter. This is because they are the results of consequential rather than attributional LCA, and hence the analysis has substantially different system boundaries.

The review presents a selection of LCA values for given feedstocks, and asserts that they cover a wide range. Given that the different LCAs quoted use different assumptions about agricultural practices, this is hardly surprising, and not in and of itself indicative of any uncertainty. Taking the example of wheat, the highest value quoted is from the RFA specifically for Ukrainian production. As the RFA calculations are based on 'worst common practice', it is not surprising that there should be at least one region with a high value. The review does not note that, for instance, within the Renewable Energy Directive itself there is a range of conservative wheat emissions values from 27 to 80 gCO2e/MJ – not a sign of uncertainty, but an indicator that different production pathways have more or less merit from an emissions viewpoint.

It is finally noted that there may be reductions in emissions over the forthcoming years due to the EU ETS. This may well be true, and such emissions reductions are reportable under the existing RED/FQD carbon-reporting framework. None of this affects one way or another the assessment of the magnitude of emissions due to ILUC.

Comparison with fossil fuels

The review makes the argument that biofuels emissions should be compared not to current typical fossil fuel pathways, but to marginal future fossil fuel pathways. This is not only inconsistent with the EU's existing approach to assessing the direct lifecycle carbon savings of biofuels, but it neglects to consider other EU legislation, in this case the Fuel Quality Directive. The FQD calls for a 6% reduction in transport fuel GHG emissions by 2020, which clearly precludes a substantial increase in the use of high carbon unconventional oil resources. In fact, without the use of biofuels this 6% would need to be met largely through reductions in the carbon intensity of fossil fuels, suggesting that a reduction in biofuel mandates would necessitate the use of lower rather than higher carbon intensity crudes. It therefore seems fallacious to conclude that biofuels should be compared to unconventional oil when calculating % savings.

This section also briefly implies that the 20-year amortisation without discounting used by Europe may be over-conservative. While the 20 year figure is lower than that used in the US, the point about discounting is poorly made. Land use change emissions from biofuels will in general occur early in the 20-year period. It is therefore not the land use change emissions that would be discounted, but the benefits accrued from ongoing biofuel use – tending to increase rather than reduce calculated carbon payback times etc.

On a more general note, it is also pertinent to observe that the context of EU biofuels policy is an attempt to prevent runaway climate change. If the best that can be managed from biofuels is a marginal saving compared to a set of fuels worse than the fuels that are already tipping the planet into ecological disaster, this might be considered to be a failure of aspiration.