Cerulogy

Navigating the maize

A critical review of the report 'A Life-Cycle Analysis of the Greenhouse Gas Emissions of Corn-Based Ethanol'

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Executive summary

The U.S. corn ethanol program is the largest biofuel program in the world, delivering about 15 billion gallons of fuel per year. The environmental performance of this corn ethanol has long been a subject of controversy, with various studies having found that corn ethanol production from some technology pathways may be worse for the climate than the gasoline use it displaces, while others have claimed that concerns about energy use and land use change have been overstated.

In 2010, the final regulatory impact assessment for the second federal Renewable Fuel Standard was published by the Environmental Protection Agency (EPA). The regulatory impact assessment concluded that by 2022 the lifecycle greenhouse gas intensity of corn ethanol from typical new corn ethanol production facilities would just meet the 20% greenhouse gas emissions reduction threshold required. The original RFS was created in 2005 and greatly expanded in the 2007 Energy Independence and Security Act (EISA), requiring 36 billion gallons of biofuels to be blended into the U.S. transportation fuel supply by 2022. EPA's corn ethanol lifecycle GHG emissions determination, completed three years after EISA's passage, was based on an extensive program of new scientific studies, and was informed by thousands of documents, including work commissioned by EPA and other U.S. government agencies, and comments and evidence submitted by stakeholders. The finding enabled continued growth of the corn ethanol sector under the expanded RFS, and to date has not been reevaluated by EPA.

In this context, at the start of 2017 a report was published, authored by the consultancy ICF International and commissioned by the U.S. Department of Agriculture, which attempted a reassessment of the corn ethanol lifecycle. The report concluded that the average greenhouse gas emissions intensity of the industry in 2014 was already below the 2022 assessment by the EPA, and is likely to decline further over time. The report was welcomed by the ethanol industry, but do these results truly represent an improvement over the original EPA analysis?

In this review, we consider the evidence referenced by ICF in its report, and the conclusions drawn from it. We also consider some of the evidence that was not discussed by ICF. While ICF's report has many useful observations and draws attention to a great deal of pertinent information, we find that overall there are too many problems in it for the numerical lifecycle results to be considered informative. We find that ICF's report is much more comprehensive in considering studies that could suggest that emissions from lifecycle stage of corn ethanol production are lower than estimated by EPA than it is in considering studies that suggest the opposite. In places, the report is not adequately careful nor adequately critical in the way it uses results from others studies. By disregarding caveats in source material there are places where the report over-interprets published results. By failing to critically appraise source results, it makes adjustments to lifecycle calculations that are not justified. Perhaps most problematically of all, the report contains serious errors of methodology and data, rendering several of the calculated emissions intensities inaccurate or meaningless.

The basic nature of some of the errors we have identified (including quoting results from the 2010 RFS Regulatory Impact Analysis (RIA) preliminary analysis instead of final analysis, misquoting fertilization rates, misquoting the emissions given by the RFS RIA for various emissions stages, confusing the control and reference scenarios of the RFS RIA analysis, double counting the co-product credit) begs the question of how many errors we might not yet have found, and undermines any confidence one might have had in the outcomes of the assessment.

Beyond the problems in specific calculations and statements, the ICF report fails at any stage to make an adequate case that the approaches it adopts for lifecycle assessment are an improvement over those used by the EPA, even if they had been correctly applied. While there is no denying that new data has been collected in eight years since RFS2 was adopted, the work presented by ICF does not approach in scope, depth, quality or documentation the work undertaken for the regulatory impact assessment of the RFS2. The authors of the ICF report themselves state that, "Much of the EPA RIA analysis still reflects our best understanding of the relationships between some emission categories, the key emissions drivers within them, and corn ethanol's GHG profile." The problems with the ICF report's reassessment serve eloquently to reiterate the truth in this observation.

In the report in general, but especially in the sections relating to land use change, there is a persistent tendency to undervalue or ignore evidence that might lead to a higher estimated emissions value for corn ethanol, while overstating and adopting without due diligence the results of studies that lead to a lower emissions value. Taken together, this tendency goes beyond healthy optimism, and suggests a systematic imbalance in the report's weighing of evidence.

There is much in ICF's report that will be of interest to researchers and stakeholders in and around the corn ethanol industry, but the work presented is wholly inadequate to justify any firm conclusion on whether the corn ethanol emissions estimates made by EPA could or should be revised down. While the report considered herein fails to draw convincing conclusions on the corn ethanol lifecycle, in due course it will indeed be necessary for a comprehensive update to be undertaken of the lifecycle analyses for corn ethanol and all the alternative fuel pathways considered under RFS2. At that time, we look forward to a reassessment that matches or exceeds the scope and rigor of the work done by EPA, while resolving some of the unresolved questions that remain from that work.

Contents

Glossary of terms

AEZ – agro-ecological zone

CARB – California Air Resources Board

CARD – Center for Agricultural and Rural Development

CCLUB – Carbon Calculator for Land Use Change from Biofuels Production

DGS – Distillers' grain and solubles

EIA – Energy Information Administration

EISA – Energy Independence and Security Act

EPA – Environmental Protection Agency

EU – European Union

FAO – Food and Agricultre Organisation

FAPRI – the Food and Agricultural Policy Research Institute, and the associated partial equilibrium economic model used in the RFS EIA.

FASOM – the Forest and Agricultural Sector Optimization Model, used in the RFS EIA

GHG – Greenhouse gas

GREET – Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation Model

GTAP – Global Trade Analysis Project

ICCT – International Council on Clean Transportation

ILUC – Indirect Land Use Change

LCA – lifecycle analysis

LCFS – Low Carbon Fuel Standard

LUC – Land Use Change

MJ – megajoule

MMBtu – million British thermal units

RFA – Renewable Fuels Association

RFS – Renewable Fuel Standard

RIA – Regulatory Impact Assessment

USDA – United States Department of Agriculture

Note on units:

The work that was undertaken for the RFS RIA includes results in both U.S. imperial and metric units, and ICF's report similarly liberally mixes its acres with its hectares. We have not tried to adopt a single unit convention in this report, and have instead generally followed the units used by ICF (and to some extent the underlying studies) in discussing each topic of interest. Please accept our apologies, and be cautious about units and conversions when referencing any of these documents!

1. About this report

In terms of sheer volume of fuel produced, the US corn ethanol industry is a government-backed industrial success story. Supported by the first and second federal Renewable Fuel Standard (RFS1 and RFS2), U.S. annual corn ethanol production has risen from around 3 billion gallons in 2004 to around 15 billion gallons today. Most of the U.S. vehicle fleet now runs on E10, gasoline with 10% ethanol blended into it (by volume).

While the formidable growth of the industry is undeniable, the environmental credentials have been more controversial. Some early lifecycle analyses reported that once the energy and other inputs to corn cultivation and ethanol processing were taken into account, it had a worse greenhouse gas emissions footprint than the fossil gasoline it replaces. In 2008, researchers led by Tim Searchinger of Princeton University published results that suggested that land use change emissions associated with expanding corn ethanol consumption would eliminate any potential climate benefit from gasoline displacement.

Responding to concerns that corn ethanol production should deliver climate benefits, the Energy Independence and Security Act of 2007 (U.S. Congress, 2007) requires that all newly built renewable fuel facilities (existing facilities are protected from the minimum emissions saving threshold by grandfathering) should deliver at least a 20% reduction in lifecycle greenhouse gas emissions when compared to baseline emissions. Tasked with implementing this requirement (and related greenhouse gas intensity thresholds for other fuel categories) the U.S. Environmental Protection Agency (EPA) undertook a major program of analysis to develop a methodology for regulatory lifecycle assessment of renewable fuels, and to produce default greenhouse gas intensity estimates for various fuel pathways. This analysis is detailed in the Renewable Fuel Standard Program (RFS2) Regulatory Impact Analysis (U.S. Environmental Protection Agency, 2010), henceforth referred to as the RFS RIA.

The analysis of corn ethanol in the RIA combined emissions modeling of domestic and international agriculture, of ethanol production at the refinery, or related transportation requirements, and of associated domestic and international changes in land use and energy consumption. When all the terms are added together, the EPA concluded that a typical newly built natural gas fired dry mill corn ethanol facility in 2022 would deliver a greenhouse gas emission reduction of 21% compared to the baseline – just one percentage point over the required 20% threshold.[1] Note that the analysis for 2012 and 2017 suggested that typical new facilities built earlier would not achieve the 20% threshold.

The 2010 final RFS RIA has been the basis for regulatory treatment of corn ethanol in RFS2 since then, and the corn ethanol industry has expanded under RFS2 to saturate the 'blend wall', the amount of ethanol that can be supplied in the U.S. without systematic adoption of new higher ethanol blends or the risk of damage to older vehicles and small engines.

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[1] The best performing corn ethanol pathway considered by EPA, a biomass powered dry mill plant with combined heat and power, was estimated to deliver a 59% greenhouse gas emissions improvement. The worst performing plant considered, a coal powered wet mill plant, was estimated to have 33% higher greenhouse gas emissions than the baseline.

However, during this period there has also been a robust ongoing debate about the 'real' lifecycle impacts of corn ethanol consumption, with some stakeholders (notably the ethanol industry itself) consistently claiming that the environmental performance is better than suggested by the EPA's assessment, and an almost equally strident group including some NGOs and competitor industries regularly claiming that the environmental performance may in fact be worse than assessed. A variety of research papers and statistics are used to support each case.

Into this febrile environment, in January 2017 the consultancy ICF International published a report undertaken for the U.S. Department of Agriculture (USDA) in which it reassessed the lifecycle greenhouse gas emissions intensity of corn ethanol, reporting a finding that as of 2014 it was already 30% below the estimated 2022 intensity from the RFS RIA (Flugge et al., 2017, henceforth "the ICF report"). The positive reception for this study from the corn ethanol industry[2] was unsurprising, with the President of the Renewable Fuels Association endorsing confidently "a comprehensive and thorough analysis, using real world data and peer-reviewed assumptions."

A demonstrated reduction in the greenhouse gas emissions intensity of corn ethanol compared to the expectations of the RFS RIA would indeed be good news, but before leaping onto the bandwagon it is worth taking the time to assess the evidence, data and methodological choices that have gone into the new result. In this paper, we review with a critical eye some of the sections of the ICF report that are most important to the reassessed emissions intensity, with a view to identifying whether this new work can be considered likely to be more accurate than the RFS RIA assessment. In the chapters below, we detail a variety of data issues, methodological problems and imbalances in the choice of sources that cast serious doubt on that proposition.

For ease of reference, many of the sections and subsections below open with a brief overview of the main points made.

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[2] See for instance http://biofuels-news.com/display_news/11674/usda_unveils_new_lifecycle_emissions_of_corn_based_ethanol_analysis/, https://ethanol.org/GHG%20chart.pdf, http://investigatemidwest.org/2017/01/18/new-usda-report-touts-corn-based-ethanols-benefits-despite-recent-doubts/

2. Introduction – analyzing the corn ethanol lifecycle

One of the goals of the Renewable Fuel Standard is to reduce the climate impact of U.S. road transport, by substituting fossil fuels with renewable biofuels. While the idea of renewability is commonly associated with an assumption of better environmental performance, this is not automatically guaranteed. Producing renewable fuels entails producing additional agricultural outputs (here, we are interested in corn), and using industrial processes to produce liquid fuels from these feedstocks. Modern corn agriculture is not a carbon-neutral business. Producing corn requires the application of chemical inputs and the expenditure of energy. It also requires the use of valuable agricultural land that could otherwise be used to supply other markets or provide other ecosystem services. It results in emissions of nitrous oxide due to nitrogen fertilization. Indeed, globally, the agricuture, forestry and land use sector is responsible for about 24% of anthropogenic greenhouse gas emissions (compared to 14% from transport[3]). Turning corn into ethanol and distributing it for use as transport fuel requires further energy expenditures, often provided by fossil fuels. Of course, producing fossil gasoline also involves more emissions than only those from combusting the fuel in cars. Oil production and refining similarly require energy.

Comparing the supply chains for biofuels and fossil fuels, it is by no means obvious which production pathway will have the lower overall climate impact. Analysts have therefore developed analytical tools to undertake 'lifecycle analysis' to assess the emissions associated with fuel production from start to finish. Different lifecycle analysis tools are designed to answer different questions, and therefore set different system boundaries (they make different decisions about which emissions sources should and should not be counted). For policy makers, the fundamental question is whether introducing policies to force the supply of certain types of fuels will increase or decrease net greenhouse gas emissions, and by how much. This is the question that the U.S. EPA sought to answer for corn ethanol, and for other biofuels, in the greenhouse gas emissions assessment included in the RFS RIA.

The EISA directed the EPA to assess the lifecycle greenhouse gas intensity of different fuel production pathways, and to include not only the 'direct' emissions associated with fuel production (for instance from fuel combustion to power an ethanol refinery) but also the 'indirect' emissions (for instance changes in land use due to increasing demand for agricultural commodities). To make this assessment, the EPA utilized three main models, combined with an extensive program to identify and develop the best available data sources to use with them. The main models used for the analysis were the Greenhouse Gases, Regulated Emissions and Energy Use in Transportation model (GREET) developed by the Argonne National Laboratory, the global partial equilibrium economic model of the agricultural economy developed by the Food and Agricultural Policy Research Institute (FAPRI[4]) and the Forest and

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[3] https://www.epa.gov/ghgemissions/global-greenhouse-gas-emissions-data

[4] In this report, we follow the convention of referring to this model by the abbreviated name of the institute, 'FAPRI'. It should be clear from context whether we are referring to the institute or the specific model.

Agricultural Sector Optimization Model (FASOM). The GREET model is used to assess the greenhouse gas emissions intensity of the fuel production process. The FASOM model is used to assess expected emissions from agriculture and land use change within the United States. The FAPRI model is used to assess expected emissions from agriculture and land use change in the rest of the world.

The FAPRI-FASOM analysis of land use changes and agricultural emissions is based on comparing a set of ‘fuel-specific’ scenarios to a ‘control case’ scenario. The control case scenario includes increased demand for three biofuels, which are corn ethanol, soy biodiesel and sugarcane ethanol. The fuel-specific cases subtract demand for each of these biofuel types in turn, and the difference represents the market response to an increase in demand for that particular fuel. This approach (subtracting demand for each biofuel one at a time) is different from the approach adopted in analysis using the GTAP model (the general equilibrium economic model of the Global Trade Analysis Project) undertaken by the California Air Resources Board (CARB) for the California Low Carbon Fuel Standard (LCFS). In that modeling, as well as in modeling by the European Commission, incremental demand for each biofuel is added one at a time and compared to a baseline. Understanding this difference in methodological approach is vital when interpreting the results of the modeling.

The RFS RIA analysis also includes results for a ‘reference case’ in which there is no additional biofuel demand due to the RFS, but this reference case does not form part of the analysis of the greenhouse gas emissions intensity of the individual biofuels. Comparing the reference case to a feedstock specific case would provide an indication of the combined impact of the other two biofuel types, which is not generally useful. For instance, comparing the ‘corn-only’ case to the reference case would provide an indication of the impact of increasing demand for sugarcane ethanol and soy biodiesel, but *neither scenario would include any additional corn ethanol production*. This is important because (as will be discussed below) it seems that in places the ICF report incorrectly derives results by comparing the corn-only case to the reference case, instead of comparing it to the control case.

The analysis is extensively documented within the RFS RIA, and the background information and modeling results are publicly available on the U.S. government docket system, within docket EPA-HQ-OAR-2005-0161.

3. Agricultural emissions

Emissions at the farm are a significant contributor to the lifecycle GHG emissions of corn ethanol in the RFS RIA. The RIA divides agricultural emissions into six categories (rice methane; farm inputs and fertilizer, all divided into domestic and international emissions), and the ICF report considers these in turn.

3.1. Domestic rice methane

The ICF report appears to take rice area data from the preliminary rather than final RFS analysis, and to use the wrong scenario to compare to the corn only scenario, invalidating the results of the section. A methodology is developed that inappropriately derives percentage area changes from absolute area changes. Features in the original modeling that could suggest that rice methane emissions were underestimated are not discussed. The result is to inappropriately multiply the rice methane reduction credit by a factor of 20.

The production of rice in waterlogged paddies is associated with the release of methane. If increased corn production results in reduced area of rice production, we might therefore expect an overall emissions credit. If it results in increased area of rice production, we might expect an emissions increase. In the RFS RIA for corn ethanol, there is a net increase in rice methane emissions of 3,500 gCO_2e/MMBtu, associated with an increase internationally and decrease domestically.

Unfortunately, there seems to be an error in the use by ICF of docket data from FASOM. ICF reference docket ID EPA-HQ-OAR-2005-0161-0950 for lifecycle greenhouse gas emissions data (for the methane assessment and at other points in the report). This document is from the preliminary LCA analysis for RFS2, not the final analysis. The documents that reflect the final analysis (as documented in the RFS RIA) have the docket IDs EPA-HQ-OAR-2005-0161-3173, EPA-HQ-OAR-2005-0161-3174 and EPA-HQ-OAR-2005-0161-3179. In context, the accidental use of preliminary data is a serious mistake, and further invalidates the conclusions drawn (on this and other points). This apparent confusion between output files may go a considerable distance to explaining why the ICF report found the emissions documented in the RFS RIA 'inexplicably high' compared to the area data. The rice land use areas and methane emissions documented in the data from the final analysis are significantly different to the data referenced by the ICF report. For instance, for 2017 the preliminary data uses in ICF report shows a difference of 0.11 million acres in rice area between the control and corn-only scenarios, where the final analysis shows a difference in area of only 0.08 million acres. The acreage ratios given by the ICF report for 2014[5] are therefore not applicable to the final analysis.

In its lifecycle reassessment, the ICF report interpolates the rice area data from the RFS RIA, and combines it with EPA rice emissions factors and actual observed rice areas to produce

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[5] The 2014 numbers are calculated by interpolating between 2012 and 2014. The final FASOM results do not include 2012 areas, and so it is not possible to redo this calculation with the correct data.

an alternative estimate of rice emissions. Specifically, the ICF authors calculate that in the RFS RIA rice area would have been 4% higher in 2014 without corn ethanol demand. They then apply this 4% increase to the actual rice area in the U.S. recorded for 2014, and assess the emissions that would be associated with that area change. Referring to the final data, a smaller rice area difference of 3% is reported for 2017. Beyond the reliance on the wrong set of results, the methodology developed by ICF for this assessment is questionable on several levels. Firstly, it is not appropriate to apply a percentage area change value derived from the RFS RIA to real observed area. The area change projected in the RFS RIA corresponds directly in absolute value to the area that is required to accommodate expanding corn production. It is not appropriate to derive a percentage area change from this modeled absolute area change and apply it as a multiplication factor. For instance, in the event that overall US rice area had doubled in the intervening period for reasons unrelated to the corn ethanol program, this would not imply that twice as much land would be needed to produce the corn needed for ethanol. By deriving a percentage change from the modeling and applying it to observed rice area, the ICF report implicitly includes in its reassessment any number of drivers of agricultural area change that are unrelated to corn ethanol. Given that total rice areas in 2014 are similar to total rice areas in the RFS RIA, the error term introduced here by moving from absolute change to fractional change may not be large, but this does not change the fact that it is methodologically problematic.

It is also unclear how the ICF report moves from its recalculated area data to its overall emissions calculation. Rather than comparing emissions calculated for its revised control case to emissions for the corn only case, the report states that it compares the corn only case to the reference case.[6] This is entirely inappropriate: the RFS RIA methodology requires that fuel-specific scenarios should be compared against the control case; the meaning of the scenarios is explained in more detail in section 2. There is no difference in corn ethanol production between the corn-only and reference scenarios, and thus one cannot learn anything about corn ethanol emissions by comparing these scenarios. As it is described, the revised domestic rice methane assessment is therefore essentially meaningless. Errors in this section should have been caught, given that there is an inexplicable difference by a factor of twenty between the reassessed result and the result in the RFS RIA.

There is also a notable inconsistency between the way that the ICF report approaches the rice methane emissions, and the way it approaches other land use change related emissions. For other land use changes, the report adopts area change results from modeling using GTAP. Here, it uses area change results from the original RFS RIA FASOM modeling. This inconsistency is not adequately explained by ICF.

Finally, it should be noted that there is a significant difference in rice methane results in the original RFS RIA analysis between FASOM and FAPRI. The FAPRI results include U.S. area changes, but for the final assessment the U.S. predictions are netted off and replaced by the FASOM area changes. In the FAPRI analysis, however, there is no area reduction predicted for

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[6] The report states that, "To estimate a final life-cycle emission factor, ICF calculated the difference in total GHG emissions (all regions included) between the "reference case" and "corn only case" scenarios to quantify the incremental GHG emissions from the reference to the corn only case."

U.S. domestic rice cultivation. This means that there is a rice production discontinuity in the combined modelling, and that the reduced rice area in the U.S. predicted by FASOM is not compensated by increased rice production (and methane emissions) elsewhere in the world. This is a feature of the combined modeling that could well result in an underestimation of overall rice methane emissions in the corn only case. The ICF report does not note or discuss any of these issues.

The ICF reassessment increases the rice methane credit by 3,800 gCO2e/MMBtu, but given the methodological issues described above this adjustment should not be given any weight.

3.2. International rice methane

As with domestic methane, it appears that the ICF report has used data from the preliminary rather than final RFS RIA analysis, and compares scenarios incorrectly. The reassessment therefore appears to be essentially meaningless.

The ICF report notes that in all years except 2009, global rice area as reported by FAO and the USDA Rice Yearbook was higher than that modeled by FAPRI in the RFS RIA. It suggests in this basis that, "the RFS2 RIA most likely underestimates global methane emissions from rice production for both cases." This statement is reasonable in itself, but has limited relevance to understanding the RFS RIA results. The RFS RIA analysis is focused on estimating the difference in rice areas due to increased biofuel demand, not on providing accurate predictions of future global rice area. By differencing two scenarios, the RFS RIA analysis is designed so that uncertainty in the baseline rice area is canceled out (i.e. changes unrelated to biofuel demand that are missed in both the control case and the corn-only case are cancelled out in the differencing). It is therefore misleading to imply that inconsistencies between the FAPRI-FASOM modeling and observed global agricultural areas suggest a systematic under- or over-estimation of emissions in the model results.

In the explanation of the international rice methane reassessment, the ICF report confuses hectares with acres in the area results presented in table 3-50, but this labelling error does not seem to be repeated. More problematically, the rice areas (converted now to acres) quoted in table 3-53 of the ICF report do not match the published data.[7] Indeed, it seems that (as in the domestic rice analysis) the ICF report has quoted values from the preliminary analysis[8] rather than the final analysis. The report also uses incorrect data labels (the column header 'corn only control case' appears to refer to the corn only case, not to the control case), and then (as with domestic rice methane) differences the corn only[9] and reference case results. This differencing of scenarios is completely inappropriate, as noted above, because there is no change in corn ethanol demand between the reference and corn only scenarios. This series of analytic mistakes results in the use of values that are sometimes dramatically different to the results

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[7] Docket IDs EPA-HQ-OAR-2005-0161-3153 and EPA-HQ-OAR-2005-0161-3167.

[8] Docket ID EPA-HQ-OAR-2005-0161-0945.

[9] The ICF report text states that table 3-53 details the "difference between control and reference cases acreage," but this statement also appears to be incorrect.

from the final analysis. For instance, in the case of India the ICF report records an increase in rice area of 101 thousand acres attributed to corn ethanol, associated with a significant increase in methane emissions. The actual result for the change in rice area associated with increased corn ethanol demand in 2017 is a 57 thousand acre *reduction*, with a correspondingly different emissions implication.

The reassessment in the ICF report of international rice methane emissions builds on these completely incorrect area difference numbers, and therefore is essentially meaningless. The land use change results quoted are completely inconsistent with the documented final analysis from the RFS RIA. The ICF report also misquotes in table 3-52 the emissions impacts from international rice methane given in the RFS RIA. The overall result of all this is to inappropriately reduce international rice methane emissions by 1,500 gCO2e/MMBtu in the reassessment.

Beyond the several errors in the analysis presented, there is also again the inconsistency that the report's authors have chosen to use FASOM results for changes in harvested area for rice methane assessment, while adopting GTAP results for the land use change emissions.

We therefore ascribe no weight to the reassessment of this lifecycle phase in the ICF report, which reduces international rice methane emissions by 600 gCO2e/MMBtu.

3.3. Domestic nitrogen related emissions

The ICF report builds a narrative of rapidly reducing nitrogen fertilization rates that is based partly on misquoted industry data and that is not supported by available USDA statistics. Similarly, the report builds a narrative of rapid adoption of precision agriculture techniques for nitrogen fertilization that is based on selective use of data (from 2005-2010) when the full dataset available (1996-2010) does not support this interpretation. The numerical reassessment is based on inappropriate use of the scenario results from the RFS RIA.

ICF report fertilizer statistics from The Fertilizer Institute, which is a representative body for the U.S. fertilizer industry. Unfortunately, at the time of writing this statistical data had been removed from the TFI website and we have not been able to get access to the TFI statistics. Using these data ICF state that nitrogen (N) application rates per bushel of corn in the U.S. fell from 2005 to 2010, and further from 2010 to 2014. They also report per acre fertilization rates from the USDA ERS Agricultural Resource Management Survey (ARMS) (USDA ERS, 2017a), which have risen.

While the TFI statistics are not available, it is possible to derive per bushel fertilizer application rates from USDA NASS crop production data (USDA NASS, 2017) and USDA ERS fertilizer application data (USDA ERS, 2017b)[10].

Firstly, we note that the values reported by ICF as N fertilizer application rates per bushel (1.56 pounds per bushel in 2014 and 1.63 in 2010) are in fact numbers for total fertilizer application per bushel (including phosphorus and potassium fertilizer). This is a related statistic, but

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[10] Noting that these data are slightly different from the data extrapolated from the ARMS.

given the particular role of N fertilization in driving emissions of the greenhouse gas nitrous oxide, the distinction is important. The numbers quoted by ICF show a modest 4% reduction in total fertilization rate from 2014 to 2010. USDA fertilization data are not available through 2014, but for the period 2005 to 2010 this pattern of reduced fertilization rate is not matched when looking at the N fertilization data alone. When reading the data correctly, the reported N-fertilization rate dropped only by 0.1%, from 0.904 pounds per bushel to 0.903 pounds per bushel, a negligible change, and indeed the 2010 N-fertilization rate was higher than either 2004 or 2006. While it is true that per bushel N-fertilization rates have fallen in the medium term, Figure 1 shows that there was not a strong overall trend in the decade from 2001 to 2010.

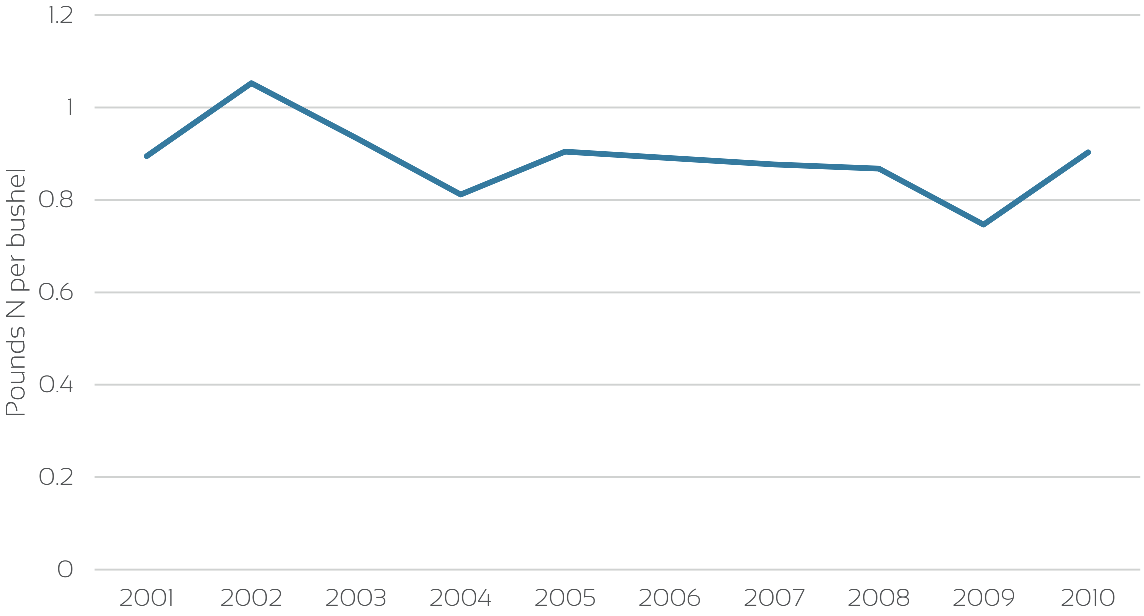

Figure 1. **U.S. average N-fertilization rates per bushel, 2001-2010**

Author's calculation from USDA data.

The USDA ERS data for total application of nitrogen + phosphate + potash decline by only 1% from 2005 to 2010.

Given that the ICF report's interpretation is inconsistent with USDA's fertilization data, its comment that, "This decrease in fertilizer application, combined with the direct change in acres, could reduce the impact of domestic nitrogen application," should be disregarded. Further, the statement in the report that, "USDA statistics already reflect the effects of precision agriculture through the reduced fertilizer use per bushel of corn harvest," should be reappraised, at least for the case of N-fertilizer. Given that the available USDA data show a rate of change in overall fertilization for 2005-2010 rather smaller than that reported by ICF from the unavailable industry source for 2010-2014, the evidence available at best weakly supports the assertion that precision agriculture has had a noticeable impact on overall fertilization rates. In particular, from 2005 to 2010, when the ICF report asserts that the "use of many nitrogen management strategies did increase," there was no significant drop in nitrogen fertilizer use per bushel.

USDA ARMS data on adoption of nitrogen inhibition suggest a rather slower path to general adoption than is implied by the ICF report. According to ARMS data, in 1996 9.5% of farms used nitrogen inhibition. By 2010, this had grown to 12.5%. This growth is not negligible, but the ICF report's choice to quote only the results for 2005 and 2010 (8.5% and 12.5% use of nitrogen inhibition respectively) could give a misleading impression of a much more rapid adoption rate than is seen in the longer term data. The ICF report also fails to consider USDA resources that emphasize the relatively slow rate of adoption of precision agriculture practices. In one such brief (Ribaudo, James, & Livingston, 2012), the USDA ERS notes that the adoption of better practice on nitrogen application in the period up to 2008 was likely related to high fertilizer prices, driven by high energy prices. The ERS authors note that there was a reduction in the number of farmers indicating that they were more carefully managing nitrogen in the period 2008-10 compared to the period 2005-08, and comment that given low nitrogen prices this trend may continue. This report also shows, based on ARMS survey data, that there has been no improvement in the percentage of farms meeting best practice criteria for either timing or method of nitrogen application, similarly contradicting the ICF report's narrative of a rapid adoption of precision techniques. Figure 2 shows the ARMS data on changes in nitrogen application method from 1996-2010 – it is difficult to discern a clear trend of improving practice.

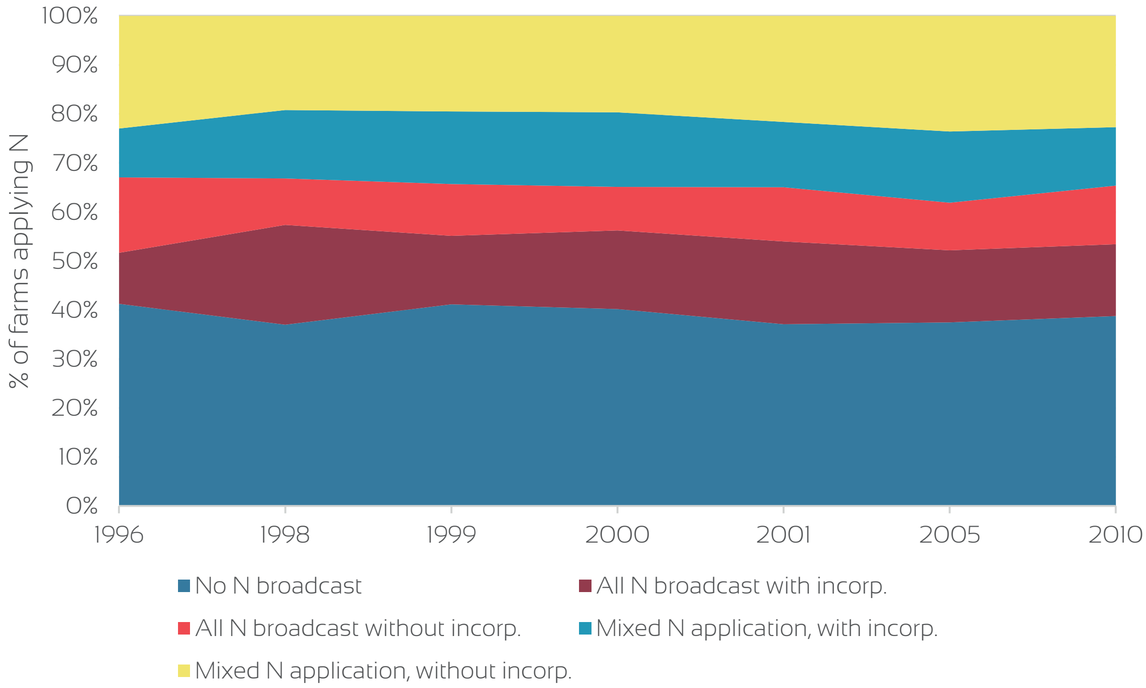

Figure 2. **USDA ARMS data on nitrogen application method, 1996-2010**

Note that not all years are included in the ARMS data, and hence the x-axis scale is non-linear.

In the ICF report's 'building-blocks' scenario for corn ethanol emissions, the adoption of the CPS 590 standard for nutrient application is assumed. The report states that, "CPS 590 assumes the adoption of new nitrogen fertilizer management techniques including reduced application rates from targeted nitrogen fertilizer application management and the use of nitrification inhibitors." This is an overstatement of what is required by the CPS 590 standard, which says only that nitrification inhibitors "must be considered." In any case, from the ICF report's description we understand that it assumes a 100% adoption of nitrogen inhibition in their building blocks scenario – this would represent a dramatic and unrealistic increase in the rate of adoption of the practice, as illustrated in Figure 3.

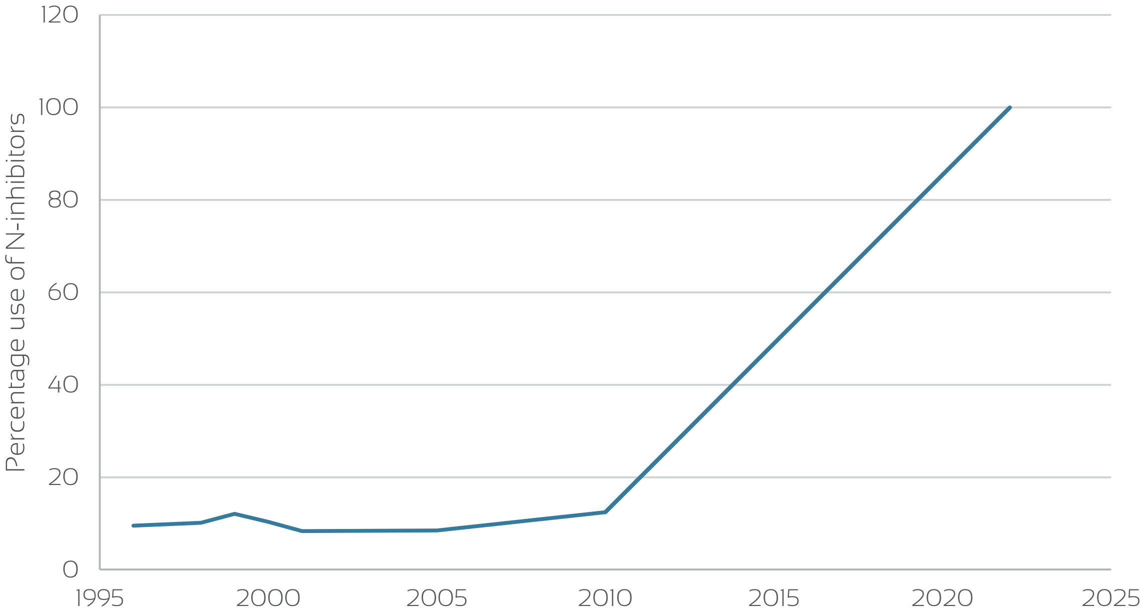

Figure 3. Required increase in nitrogen inhibition adoption to meet 100% utilization by 2020, compared to historical utilization (USDA ERS, 2017a)

While the ICF report emphasizes the opportunity to improve nitrogen efficiency, it does not consider studies that highlight the potential for corn ethanol demand to increase application and nitrogen emissions. It also does not assess whether the dramatic reduction in natural gas prices since 2010 could result in increased N-fertilizer use. The response of corn yield to additional nitrogen fertilizer reduces with increasing fertilization rate (the response function is concave) (Qin, Zhuang, Zhu, Cai, & Zhang, 2011). Increased corn demand for ethanol is expected to increase the optimal expected rate of nitrogen application for corn, leading to marginal increases in yields. However, the return in bushels of corn per pound of fertilizer applied will be much lower than the average, as shown in Figure 4. Zhu, Yan, Smeets, & Berkum (2017) find that because of the lower response of yield to marginal increases in fertilizer application, the marginal carbon intensity of delivering additional corn ethanol through increased fertilization is between 96 and 236 gCO_2e/MJ.

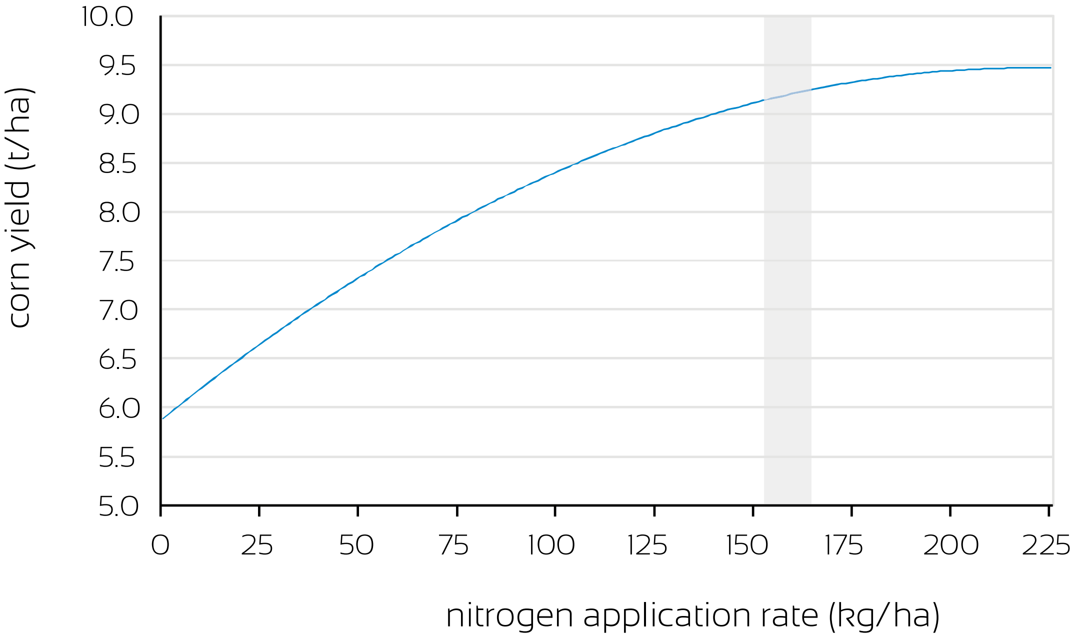

Figure 4. Correlation between N-fertilizer use and corn yield (Qin et al., 2011)

Note: An N application rate of 143 pounds per acre is about 160 kg per hectare.

The RFS RIA identifies an average nitrogen application rate for corn in 2022 of 105 pounds per acre in FASOM[11]. This contrasts to an average N-fertilizer application rate of 138 pounds per acre in the ICF assessment for 2014. The ICF report does not discuss that the FASOM modelling uses a lower N-fertilization rate than might be expected given current trends (cf. Figure 5), and that this may tend to contribute to underestimation by EPA of this element of emissions.

The FASOM analysis for the RFS RIA also has a correspondingly low per-bushel N-fertilizer application rate for 2022 of 0.57 pounds. This is illustrated in Figure 6. Again, the ICF report does not discuss the possibility that the RFS RIA may in fact have overstated potential for reduction in nitrogen fertilization.

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[11] Cf. page 325 of the RIA.

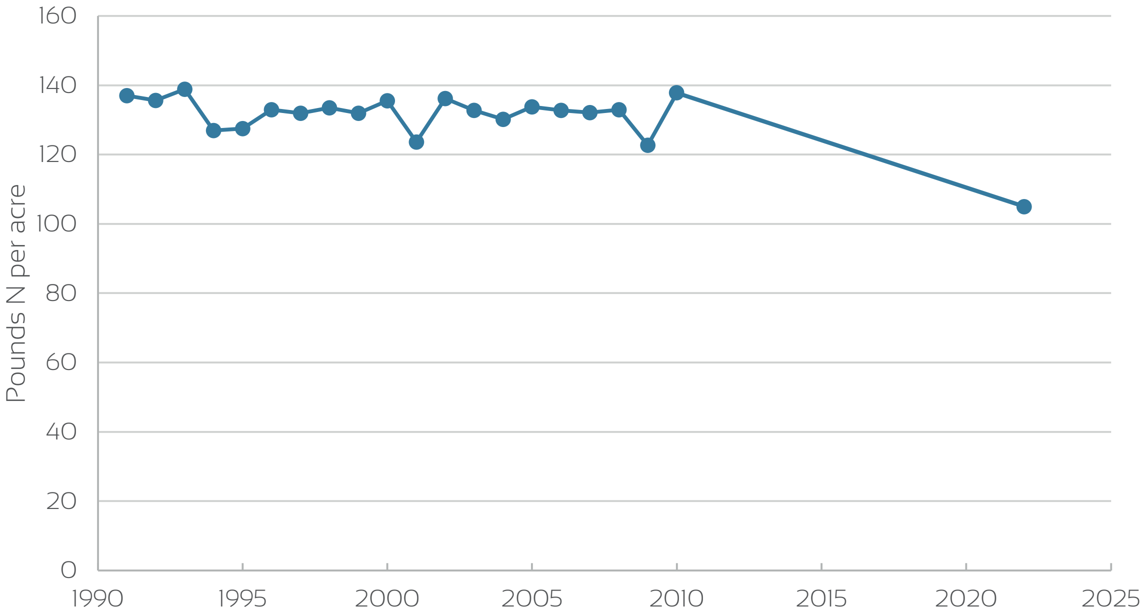

Figure 5. Average N-fertilizer application from USDA NASS data 1990-2010, and in the FASOM modelling for 2022

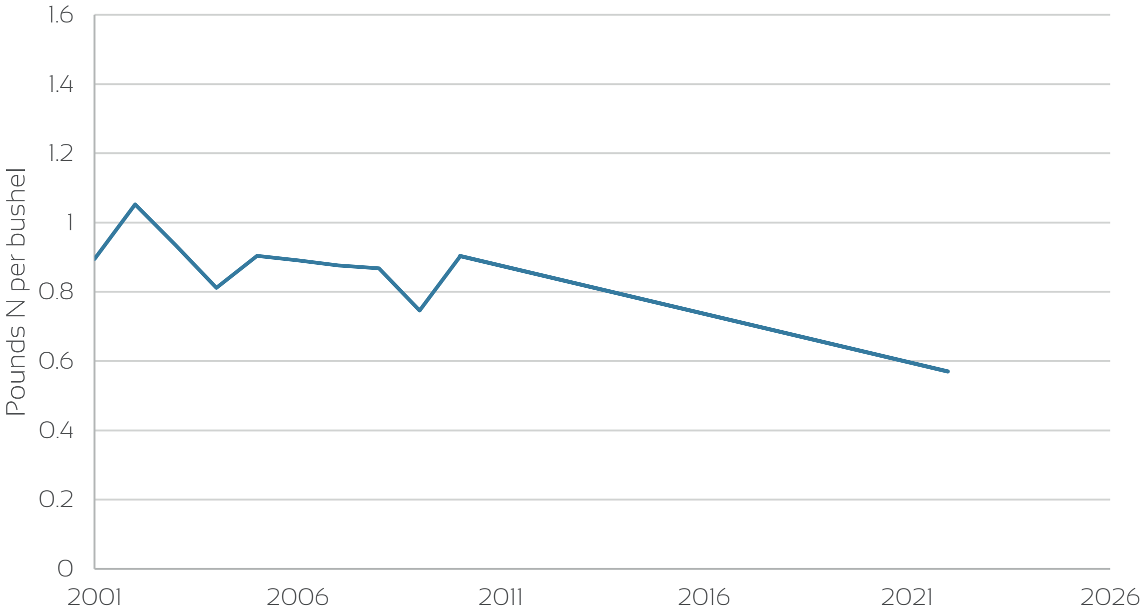

Figure 6. Average N-fertilizer application from USDA NASS data 2001-2010, and in the FASOM modelling for 2022

The reassessment of these emissions in the ICF report is based on "the RIA's projected number of additional bushels of corn in the control scenario (i.e., compliance with the RFS2 regulation), compared to the reference scenario (i.e., no RFS2 is enacted)." It is unclear why ICF's authors have chosen to compare the control scenario (including soy biodiesel and sugarcane ethanol as well as corn ethanol) to the reference scenario (no increase in biofuel use) rather than to the corn-only scenario (with soy and sugarcane biofuel demand but not corn) as is intended for the FASOM analysis. The use of scenario comparisons other than control against corn-only is a methodologically questionable choice that occurs several times in the ICF report.

4. Land use change model

In the RFS RIA, the EPA set out detailed reasons to choose to use the FAPRI-FASOM partial equilibrium modeling framework rather than the GTAP general-equilibrium modeling framework. The ICF report reverses this decision, but provides no adequate justification. The results from the GTAP runs referenced by the report show much less land use change than the RFS RIA analysis, but the report fails to make any convincing argument that these results are more accurate than the RFS RIA assessment. The narrative in the ICF report claims that "a number of studies" support choosing land use change results that show less deforestation, but in fact the report seems to rely on only one study for this contention. In effect, the authors of the ICF report conclude (based on limited and unconvincing evidence) that rates of deforestation associated with corn ethanol should be low, and have therefore chosen a model and specific set of model results that conform to that expectation.

In its work for the RFS RIA, the EPA used a combination of two models to calculate predicted land use changes in response to the RFS2 – FAPRI and FASOM (U.S. Environmental Protection Agency, 2010). In its reassessment, the ICF report largely replaces outputs from FAPRI-FASOM with results from GTAP modeling incorporated in the CCLUB model (Carbon Calculator for Land Use Change from Biofuels Production, Dunn et al., 2014), henceforth referred to as 'GTAP-CCLUB'.

The ICF report does not directly compare the FAPRI-FASOM and GTAP models, and certainly provides no detailed justification of the choice to favor the GTAP-CCLUB results over the RFS RIA results. It does, however, provide some discussion of features of the newer GTAP modeling (Farzad Taheripour & Tyner, 2013), noting that,

> *"This modelling scenario includes updated land transformation data to develop region-specific elasticities using two United Nations Food and Agriculture Organization (FAO) land-cover datasets. It also reflects evidence from recent studies that the costs of converting forest to cropland are higher than had been assumed in prior land-use change studies."*

The ICF report also indirectly criticizes the FAPRI-FASOM modeling by noting that, "a number of studies have evaluated the land-use change outcomes projected in the RIA (see Chapter 2)... [and] generally conclude that there is not a strong link between corn ethanol production in the United States and deforestation in other countries, particularly in Brazil where much of the RIA's emissions related to indirect land use change were projected to occur." The claim that "a number of studies" that have evaluated land use change outcomes in the RFA RIA are discussed in Chapter 2 of ICF's report seems to be a serious exaggeration. The only study referenced in ICF's chapter 2 that fits this description is Babcock and Iqbal (2014). The limitations of this study, and the inadequacies in the way that the ICF report uses it, are discussed in detail in section 7.1. Chapter 2 of the ICF report also mentions Boland & Unnasch (2014), which does at least mention the RFS RIA, and introduces the idea (which ICF lean on heavily) that ILUC estimates have shown a tendency to be lower in newer studies than older ones. However, the Boland & Unnasch paper actually makes no direct comment on deforestation links. Note that the RFS RIA finds that results from GTAP modeling confirm the importance of ILUC impacts, stating that “GTAP confirms that there are significant impacts on international land use due to biofuel production from food and feed crops.”

The lack of depth in the ICF report’s consideration of the comparative merits of the FAPRI-FASOM and GTAP modeling frameworks is thrown into stark relief by comparing the ICF report to the discussion of model frameworks undertaken in the EPA and CARB’s regulatory decision making. For instance, the peer review report on model linkages for the RFS2 (which was managed by ICF itself) concludes that, “The peer reviewers generally agreed that EPA’s approach of linking partial equilibrium models was preferable to using a general equilibrium model such as the GTAP (Global Trade Analysis Project) model,” and that most reviewers, “believed the existing approach to be more reasonable than relying wholly on the GTAP model.” It should be understood that the GTAP model has been developed considerably for biofuel analysis since the RFS RIA was undertaken. Nevertheless, it is disappointing that the decision to replace FAPRI-FASOM results with GTAP-CCLUB results in the ICF report’s reassessment is taken without referring back to the RFS2 model linkages peer review report and discussing whether any of the concerns expressed have been resolved, or whether the reasons to favor the FAPRI-FASOM framework no longer apply.

As well as the discussion of comparative model advantages in the ICF-authored model linkages peer review report, the RFS RIA provides several reasons to explain the choice of FAPRI-FASOM over GTAP. The RFS RIA notes that GTAP predicts land use changes based on land rental rates, and that, “One of the major limitations of this methodology is that unmanaged land[12], which represents approximately 34% of the land cover in the GTAP model, is not allowed to be brought into productive use (e.g., as pasture).” A related feature of the GTAP model is that all forest is treated as productive (in proportion to its modeled land rent), and therefore it is assumed that forest loss results in reduced timber production. The consequent timber shortage is resolved in the model by predicting a certain amount of *reforestation* in other areas. In regions where conversion of unmanaged forest to agriculture is likely, these characteristics of the GTAP model are likely to result in an unrealistic prediction of a ‘forest rebound’ in other regions. The RFS RIA also notes as drawbacks that GTAP is static rather than dynamic[13], that at the time GTAP contained only aggregated crops (coarse grains, oilseeds)[14], and that there is not an adequate literature of empirical econometric analyses of elasticities of transformation between crop types for the relationships in the various model regions to be properly set.

While some of these issues may have been at least partly resolved in the intervening years, the ICF report makes no direct discussion of them. Again, it is disappointing that the report should have adopted an alternative preferred modeling system without at least discussing the validity of the reasons stated for EPA to adopt the FAPRI-FASOM model in the first place.

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[12] The characterization of GTAP as distinguishing ‘managed’ from ‘unmanaged’ land is not strictly correct. The categorization is actually into ‘accessible’ and ‘inaccessible’ land. All inaccessible land is unmanaged, but not all unmanaged land is inaccessible.

[13] I.e. GTAP solves for an ‘instantaneous’ resolution to a shock rather than predicting changes over time.

[14] The number of disaggregated crops has been increased since.

5. Land use change emissions factors

The reassessment in the ICF report uses emissions factors from the CCLUB model, which are described as 'more accurate' than alternative emissions factors that tend to predict higher land use change emissions. The ICF report however ignores detailed concerns that have been documented regarding soil carbon assumptions in the CCLUB model. By adopting the CCLUB emissions factors, the reassessment in the ICF report implicitly includes a very large cumulative carbon credit from expansion of corn at the expense of other cropland and of cropland pasture. This credit is not supported by the available evidence. The report also ignores evidence that could suggest that the RFS RIA may have underestimated some aspects of land use change emissions.

The ICF report provides a brief overview in their section 2.2 of three sets of land use change emission factors – Winrock, Woods Hole, and CARB AEZ-EF. In its reanalysis, however, it adopts emissions factors and land use change fractions from the "Carbon Calculator for Land Use Change from Biofuels Production" (CCLUB, Dunn et al., 2014). The CCLUB analysis has been integrated into the GREET lifecycle analysis model of the Argonne National Laboratory. The California Air Resources Board, in its 2015 regulatory reassessment of ILUC, chose to use emissions factors in its AEZ-EF model rather than adopting the CCLUB model (California Air Resources Board, 2014). The CCLUB model has not (to the best of our knowledge) ever been the subject of a formal public comment period, which differentiates it from the CARB AEZ-EF model and from the work by Winrock for the original RFS RIA.

The ICF report characterizes the CCLUB model as reflecting "more accurate carbon stock factors"[15] than factors used in the original ILUC modeling for the Low Carbon Fuel Standard, but in fact the CCLUB has been criticized for methodological problems and questionable use of data. A report by the International Council on Clean Transportation (Searle & Malins, 2016) assessed the basis of soil carbon emissions factors within the CCLUB model, in particular with regard to the result built into the CCLUB that conversion of cropland to corn cultivation will result in increased soil carbon stocks. The review concludes of the underlying data (Qin, Dunn, Kwon, Mueller, & Wander, 2016) that, "we do not believe that this meta-analysis adequately supports an assumption that the conversion of generic cropland to corn will increase soil carbon." It further concludes that, "The scientific literature points towards a consensus that continuous corn cultivation does not significantly affect soil carbon stocks over time, and there is not sufficient evidence to compare soil carbon under corn with that under other annual food crops." In short, while there is a great deal of modeling complexity input to the generation of emissions factors in CCLUB, it is not at all clear that they are more accurate than other sources, and it seems likely that they contain fundamental flaws.

The soil carbon assumptions for corn in CCLUB are particularly important, because they allow the model to calculate an increase in soil carbon when other categories of cropland, or cropland pasture, are converted to corn production. This is particularly surprising for the case of cropland pasture, as other models (such as AEZ-EF) assume a reduction in soil carbon

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[15] This statement is made in regard to Boland & Unnasch (2014), which used the CCLUB emissions factors.

following cropland pasture conversion to corn, not an increase. This assumption of additional carbon accumulation on corn fields makes a major contribution to the ICF reanalysis of domestic land use change emissions.

The ICF report does not enter into any discussion of the land use change emissions assumptions made in the RFS RIA analysis that complement the emissions factors themselves. The land use change assumptions used in the RFS RIA are based on analysis by Winrock International of historical land use trends, undertaken using mapping from the MODIS system. ICF's report does not discuss that the use of MODIS data in the RIA analysis was subject to discussion and criticism at the time that the analysis was done. The error rate in land-type classification using the MODIS data was high, and this high error rate was compounded by the differencing undertaken between years. For instance, if one in ten pixels was mischaracterized in the first year, and one in ten mischaracterized in the final year, this could result in false positives for land use change in up to one in five pixels overall. If the number of false positives is high compared to the number of pixels really experiencing land use change (say only 10% of pixels experience a real land use change, but 20% show a false land use change), then the land use change patterns detected would be almost meaningless (ICF International, 2009; Marelli, Mulligan, & Edwards, 2011). Holly Gibbs has noted (Marelli et al., 2011) that the MODIS analysis reported highly unrealistic rates of change for certain land types over the period of analysis. For instance, the MODIS analysis showed in one case that 94% of 'shrub' type land and 80% of grassland had changed land use over the period in question, a dramatically higher rate of change than shown by analysis of Landsat data. Erroneous identification of land use changes is important, because if the number of errors is large compared to the number of real changes, the results become meaningless when applied as a guide to likely future outcomes. In particular, if the error rate is higher between harder to distinguish categories (cropland, shrubland, grassland) than between forest and cropland, the relative contribution of forest conversion to cropland will end up being understated, and this would lead to underestimation of indirect land use change emissions.

The ICF-managed peer review report on this issue noted that experts had expressed concern about, "The 3-year time period of the two MODIS data sets chosen and the error associated with each of those data sets." The initial MODIS results were updated following the peer review, resulting in a six year time period (2001-2007) and significant changes to the outcomes. Nevertheless, the concerns expressed in Marelli et al. (2011) refer to the final rather than preliminary results, showing that at least some experts continued to have concerns over errors associated with the MODIS datasets. These issues go without comment in the ICF report.

6. Domestic land use changes

While the ICF report devotes considerable attention to attempting to compare the RFS RIA international land use change predictions to observed data (cf. 9), the discussion of domestic land use change makes no reference to the large body of work (including from USDA's own Economic Research Service and in Farm Service Agency data) that has been produced since implementation of the RFS2 investigating the impact of RFS2-driven biofuel demand on U.S. land use, and which shows links between corn ethanol production and the loss of carbon-rich grasslands and wetlands. The ICF report finds that domestic land use change results in a carbon credit for corn ethanol, but this is based on the adoption of soil carbon assumptions that do not stand up to scrutiny. It should be recognized though that ICF actually calculate a **smaller** ***emissions credit for domestic land use changes than is given in the RFS RIA. This is because the RFS RIA includes a difficult to explain burst of afforestation in 2022 that was predicted by the RFS RIA FASOM modeling.***

Given that the ICF report extensively discusses the relationship between RFS RIA international land use change predictions and the findings of Babcock and Iqbal (2014) regarding observed international land use changes, it is inconsistent that no attention is paid to reports that directly consider domestic land use changes relating to corn ethanol. Data from the USDA cropland data layer (CDL) have been used by researchers to explore land use changes in the United States in the period (2008-2012) following adoption of the RFS2 (Lark, Salmon, & Gibbs, 2015). They found about three million acres of net land conversion, and a gross change of 7.34 million acres. Corn was the crop recording the largest net area expansion, and was associated with the largest fraction of new land conversion. Most of the 4.36 million acres of land identified as abandoned[16] in this period was newly enrolled into the Conservation Reserve Program, contributing to wildlife habitat and soil conservation. For land newly converted to cropland, grassland (including pastures) was the primary source (77%). The contributions of forest land and wetlands to new cropland were more modest (200,000 acres and 140,000 acres respectively). This amount of forest conversion is moderately larger than the total forest conversion estimates in the corn scenarios from GTAP-CCLUB (161,000 acres) or FASOM (50,000 acres of forestland reduction are shown in the RFS RIA). Of the converted grasslands, over a quarter were identified as 'long-term' unimproved grasslands, which are likely to have higher carbon stocks and biodiversity value. This compares to GTAP-CCLUB results in which at most 5% of converted grasslands could have fallen into the long-term unimproved category[17]. These data could suggest that GTAP-CCLUB overestimates the role of cropland-pasture conversion, and thus underestimates potential ILUC emissions. Lark, Salmon, & Gibbs (2015) suggest that the emissions from land conversion to corn and soy that they identified could fall in the range 94 to 186 million metric tons of carbon dioxide. This contrasts with the presumed *carbon sequestration increase* of 54 million metric tons in the ICF report's preferred results.

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[16] Wright, Larson, Lark, & Gibbs (2017) find that Lark, Salmon, & Gibbs (2015) over-identified cropland reversion to natural land by over 100%, which would imply that net cropland increase in the period considered was about 2.5 million acres larger than originally reported.

[17] Of a total 1.9 million acres of conversion of cropland pasture plus other grassland in GTAP-CCLUB, 95% was from cropland pasture.

A similar dataset was analyzed by Wright, Larson, Lark, & Gibbs (2017) with a particular focus on geographical correspondences between ethanol refineries and land use changes. This study found with high statistical significance that proximity to refineries was a predictor of likelihood of land conversion to cropland, and inversely that the likelihood of cropland reversion was greater as distance from refineries increased. Conversion of forests, wetlands and shrublands were all more likely in the proximity of ethanol refineries. Motamed, McPhail, & Williams (2016) similarly observed a strong connection between proximity to ethanol refineries and expansion into previously uncultivated areas. The ICF report makes no mention of any of these studies linking recent grassland and wetland losses to greater ethanol production, which have resulted in greater GHG emissions and losses of biodiversity.

6.1. Results of domestic land use change reassessment

By combining land use change results from GTAP-CCLUB with emissions factors from CCLUB, the ICF report calculates negative domestic land use change emissions of -2,038 gCO_2e/MMBtu. To put it another way, the ICF reassessment assumes that U.S. corn ethanol production has resulted in 65,000 hectares of forest loss, 14,000 hectares of forest-shrub loss, 90,000 hectares of grassland loss and nearly two million hectares of cropland-pasture conversion, and estimates that overall carbon sequestration in the U.S. has increased *because of these land use changes.* As noted above (section 5), it is standard throughout almost all of the land use change emissions factor literature to assume that all of these land use changes would result in net emissions, not in net sequestration (Searle & Malins, 2016). This result is therefore extremely contentious.

It is important at this juncture, however, to recognize that (perhaps surprisingly) the RFS RIA *also assumes a net carbon credit for domestic land use change.* The RFS RIA is based on the Winrock emissions factors (as discussed in the ICF report), and these emissions factors are uniformly positive for conversion to cropland – i.e. in all cases, unlike CCLUB, the Winrock factors assume that land conversion to cropland results in carbon losses. Why then does the RFS RIA document that significantly increased corn area in 2022 goes hand in hand with a net emission credit? The answer is that FASOM predicts that in 2022 there would be a burst of afforestation that would not occur without corn ethanol demand. Specifically, in 2022 there are 0.2 million acres of additional forest-pasture associated with corn ethanol demand, alongside a reduction of 0.03 million hectares in the forestland category – a net increase of 0.17 million hectares in aggregate coverage of forested land uses. This net increase is only seen in 2022, the FASOM modeling has net reductions in overall forest cover associated with corn ethanol in both 2017 and 2027. The transitory increase in net forest area that FASOM models in 2022 is an interesting feature of the RFS RIA modeling, and worthy of further investigation. It is certainly an area in which it seems plausible that the RFS RIA modeling has underestimated corn ethanol emissions (by attributing a carbon sequestration credit that may not be realistic). None of this is discussed in the ICF report.

Both the RFS RIA and the ICF report assign emissions credits to corn ethanol for domestic land use change. It is our considered opinion that in neither case is the emissions credit credible. In the RFS RIA, the credit is associated with a burst of afforestation that is completely inconsistent with the observed U.S. land use change patterns discussed above, and that seems illogical within the modeling framework. In the ICF report reanalysis, the credit is associated with soil carbon sequestration assumptions that do not bear up to scrutiny. We therefore have no confidence in the ICF report's adjusted results, although in this case by reducing the credit term the ICF may nevertheless have come closer than the RFS RIA to a realistic result.

7. International land use changes

The narrative around international land use changes cherry picks statistics that could be consistent with reduced land use change emissions, such as reductions in the last decade in Brazilian Amazon deforestation rates, and conflates correlation with causation. As with the analysis of domestic land use change emissions, the reassessment is based on an inadequately justified substitution of results from the RFS RIA with results from GTAP-CCLUB. The ICF report makes no compelling case that this substitution is warranted. Based on results from a single non-peer reviewed study, the ICF report makes further exogenous assumptions reducing land use change emissions. This use of these results ignores multiple caveats made by the authors, is not adequately supported by the referenced data, ignores flaws in the underlying analysis, and in one case (Sub-Saharan Africa) shows a failure to understand the work being referenced. The overall result of these changes is a dramatic reduction in international land use change emissions in the reassessment. This conclusion is not warranted.

The ICF report’s section on international land use change emissions makes much of the observed reduction in rates of Brazilian Amazon deforestation from 2004 onwards. There is a graph included showing a negative correlation between total Brazilian deforestation and US corn ethanol production, and ICF comment that, “Despite the increase in corn ethanol production (from 3.4 billion gallons in 2004 to 14.8 billion gallons in 2015), deforested land in Brazil decreased over the same period.” Drawing a simple comparison in this way between overall trends and specific market drivers conflates correlation with causation, and ignores the possibility that deforestation reductions would have been even larger without the pressure on land expansion resulting from the RFS.

The RFS RIA assumed an expansion in farmed crop area in Brazil of 316,000 hectares, and a total agricultural expansion including pasture of 343,000 hectares. Of this, only a fraction is anticipated by the RFS RIA to be Amazon deforestation. The RFS RIA anticipates 24,000 hectares of additional Amazon deforestation by 2022, an average of a bit under 2,000 hectares per annum over the course of the RFS2 program. It is pertinent to compare this predicted deforestation to observed Amazon deforestation, for instance about 600,000 hectares in 2015. The RFS RIA predicts that ILUC due to corn ethanol demand could be responsible for a fraction of 1% of current annual Amazon deforestation, even at its much reduced rate compared to ten years ago. It is implicit in the RFS RIA analysis that there are other causes, unrelated to corn ethanol demand, for the vast majority of Amazon deforestation. Given this very marginal impact that the RFS is anticipated to have on Amazon deforestation rates, it is entirely impossible to draw any direct conclusion from observed deforestation about whether the rates predicted by the RFS RIA are reasonable. Clearly, the RFS RIA predictions are completely compatible with the observed deforestation rates, in the sense that of the 600,000 hectares of observed deforestation in 2015, it is possible that 2,000 would not have occurred in the absence of corn ethanol demand.

The ICF report notes that the RFS RIA was not able to consider more recent Brazilian policy against deforestation, which is true, but ignores the fact that the land use change data underpinning the FAPRI modeling are based on the period 2001-2007 – *after* the start of the reduction in Amazon deforestation and after the introduction of important policies such as the soy deforestation moratorium. While the FAPRI land use change assumptions might indeed have been further adjusted given additional data from later years, the ICF report makes no serious attempt to undertake a serious assessment of whether the original modeling assumptions are in fact compatible with observed land use patterns. It also takes no note of the reported increase in Amazon deforestation rates in 2016 compared to 2015[18], or of related concerns from environmentalists that the Brazilian Government has relaxed some of its anti-deforestation policy. There is also no apparent attempt to consider deforestation rates elsewhere (i.e. whether there are regions in which deforestation is now occurring more quickly than in the period assessed for the RFS RIA) or to review the applicability of the RFS RIA MODIS assessment (see section 5).

Following the brief introduction of the Brazilian data, this section of the ICT report includes a brief (and slightly out of context) mention of methodological issues raised by a 2012 Biomass and Bioenergy paper (Kim, Dale, & Ong, 2012) that proposes an alternative methodological approach to ILUC analysis. This reference is an odd inclusion at this point, as it calls into question the entire framework that is used in both the RFS RIA and by the ICF report reassessment. Given that this reference is made without any broader attempt to provide a systematic review of the discussion of the merits of different ILUC assessment approaches, the main purpose of including a reference to this study seems to have been to be able to note that, “By applying their proposed approach, they lowered the estimate of GHG emissions by up to 73 percent when compared to the GTAP model output.” The methodological innovation introduced by Kim, Dale, & Ong (2012) is to assert that because a significant part of indirect land use change is associated with moving the production of animal feed, the responsibility for the emissions should be shared between fuel ethanol (which created the market shock that caused the production shift) and people who eat meat (who eat meat rather than switching to vegetable protein with a smaller land footprint). On this basis, only the fraction of ILUC consistent with replacing displaced meat production with vegetable protein production is attributed to biofuels. While this provides an eloquent reminder of the relative environmental benefit of the vegetarian lifestyle, it certainly does not provide a more realistic assessment of the net marginal environmental impact of the corn ethanol industry – it simply reallocates some of the responsibility for ILUC to meat consumers. Even ignoring the fact that it is not at all clear that this paper adds much value to the analytical literature on the subject, the inclusion of this result at this point in the ICF report is characteristic of an undue willingness to note the results of studies that suggest ILUC may be lower than estimated, and to ignore studies that suggest the opposite.

7.1. Use of results from Babcock and Iqbal (2014)

Results from Babcock and Iqbal (2014) are used in the ICF reassessment without adequate attention to caveats in the original paper. Serious questions about the validity of the conclusions in Babcock and Iqbal (2014) are ignored in the ICF report. Data from Brazil is used

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[18] https://news.mongabay.com/2016/11/brazil-deforestation-in-the-amazon-increased-29-over-last-year/

unduly selectively, data from China and Indonesia are over interpreted, and data from Sub-Saharan Africa is misused in the reassessment. The significant limitations of results presented in Babcock and Iqbal (2014) on a potential link between biofuel demand and incraesed double cropping are systematically ignored.

More than any other source, the ICF report leans heavily in their reassessment of the RFS RIA ILUC values on a single paper by Babcock and Iqbal (2014), published as a (non-peer reviewed) staff report by the Center for Agricultural and Rural Development at Iowa State University. The work was supported by the Renewable Fuels Foundation ("dedicated to meeting the education, research and strategic planning needs of the U.S. fuel ethanol industry"), and the Bioindustry Industry Center. Henceforth, for brevity this paper will be referred to as 'B&I'. Indeed, we counted 40 separate mentions of B&I in the ICF report, with the first on the very first page of the introduction. The results of B&I are consistently treated by ICF as established facts. While the land use change assessments undertaken by EPA for the RFS RIA are repeatedly challenged, the ICF report does not include any critical appraisal of the quality of the B&I data or validity of their conclusions before using these results in the reassessment. Given that the RFS RIA is the end result of a lengthy government process that involved dozens of researchers, generated literally thousands of supporting documents and public comments and was subjected to extensive peer review, while B&I is a corn ethanol industry funded working paper from two researchers, this difference in levels of critical assessment is striking.

As it happens, there are many grounds to question the conclusions drawn by B&I. A further discussion of some of the claims made in the B&I paper is provided in Annex A. Here we focus on the specific claims drawn from B&I by the ICF report.

The central claim of B&I, relayed by the ICF report, is that from the period 2004-2006 to the period 2010-2012 the 'intensive' land response (increasing the number of hectares that are cropped and harvested twice or more in a single year) plays a much larger role in increasing reported harvest area and agricultural output than the extensive land response (bringing new hectares of land into production). This conclusion is based on analysis of data from FAOstat and various other sources. To calculate the total amount of extensive land use change in this period, B&I assess the change in the sum of the FAOstat categories 'arable land' and 'permanent crops'. The premise is that by comparing statistics for total area under agricultural cropping, rather than for area harvested, it is possible to come to a conclusion about the total change in the extent of the agricultural system that is independent of rates of multiple cropping.

In principle, this premise is reasonable, but there are several problems with the use of data. First off, there is always a concern when using FAOstat data about whether changes over time are representative of real world agricultural change, or are simply artefacts of data reporting limitations, errors or changes in the application of data categories. One does not need to look far in the examples chosen by B&I in order to find an example of this. B&I note that, "Reliable country-specific data, such as in the United States, that can measure the change in net planted area should be used when available," and they use USDA NASS data to estimate change in US planted area. This gives a very small increase in total area in the United States over the period considered. If, however, they had used the FAOstat data for the United States, they would have reported[19] a ten million hectare reduction in planted crop area[20]. This would have been the largest result reported except for the result for 'Rest of Africa'. This discrepancy seems to demonstrate that the FAOstat differencing method proposed by B&I could be highly unreliable – for the only datapoint that was reported as being checked against an independent source, the error margin is comparable in magnitude to the largest result reported. Neither B&I nor ICF pass further comment on this demonstrated potential data problem. Given that the basis for the harvested area data and land area data are different, there is every risk that the B&I results are dominated by data discrepancies rather than real findings in several, if not all, cases.

Another potential data weakness in the B&I approach is the role of land abandonment. In the FAO area data, if a million hectares of land is abandoned due to soil degradation, and a million new hectares brought into production, this would register as a zero net change in land area. It would, however, actually demonstrate that there was a strong extensive response to land demand in that region. Data from Mato Grosso in Brazil (Spera et al., 2014) that includes land abandonment and double cropping statistics provides a useful example of this effect in real life. Spera et al. (2014) report that from 2001 to 2011, total cropland in Mato Grosso expanded from 3.9 to 5.8 million hectares. At the same time, the double cropped area increased from 1.1 million hectares to 2.9 million hectares (i.e. the harvested double cropped area increased from 2.2 million to 5.8 million hectares). Based on the B&I approach, we would conclude that the intensive double cropping response was responsible for 56% of harvested land expansion (3.7 million harvested hectares) and that the extensive response was responsible for only 44%. However, this ignores the rate of land abandonment in Mato Grosso. About 2.2 million hectares of single cropland[21] are reported abandoned from 2006 to 2011, and the study reports that about 50% of abandoned land is still abandoned after five years. There is therefore a 'hidden' extensive expansion in the data of about 1.1 million hectares. This would shift the balance of responses from 56:44 in favor of intensive changes to 48:52 in favor of extensive changes. By ignoring this 'shifting' of agricultural area, B&I systematically understate the role of extensive land use change in meeting land demand.

The problem of netting also applies when B&I sum planted area changes across all regions when comparing total extensive response to total intensive response. Several regions, notably the EU, have had a significant reduction in farmed area over the period considered. This area shrinkage is subtracted by B&I from the area increase in other regions to give the net area increase over this period – implicitly, area shrinkage is therefore being interpreted as evidence that total planted area is less responsive to demand. It would, however, be possible to interpret these shrinkages in an entirely different way. A large reduction in some areas and increase in others could be totally consistent with the hypothesis that farmed area is responsive to crop economics, and therefore to demand. If the market competitiveness of crop production in Europe had declined over this period, then a reduction in area is exactly what would be

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[19] By our calculation.

[20] B&I report this as nine million, it is unclear why we get a slightly different result.

[21] In this study, double cropland is reported 'abandoned' if it reverts to single cropping, so it is not considered for this calculation.

predicted by a model with a strong response of planted area to profitability. In that case, it is analytically inappropriate to sum and compare the regional area changes as B&I do.

More generally, B&I fail to make any convincing case that intensive area change is price/demand/profit led. Indirect land use change modeling is not about assessing generalized changes in the agricultural system over time, rather it is about assessing likely responses to a specific marginal demand shock (biofuel demand) in the agricultural system. If double cropping is primarily led by technology change and/or by government policy, then it may not be expected that double cropping rates would be responsive to marginal demand changes, even though double cropping has played a significant role in increasing overall agricultural productivity. B&I do propose a calculation for intensive and extensive elasticities to price, by comparing their calculated extensive and intensive percentage area changes between 2004-06 and 2010-12 to reported corn price increases in the same period. Unfortunately, in economic terms this calculation is essentially useless for assessing causal relationships. As discussed at length elsewhere (Berry & Schlenker, 2011), it is meaningless to calculate purported elasticity values without controlling for external factors (i.e. without showing that a correlation is actually evidence of causation).

Another way to assess the likely validity of the B&I conclusions is to compare them to other commentators on the subject. The OECD-FAO Agricultural Outlook 2009 (OECD & FAO, 2009), referenced by B&I, provides estimates of the role of intensive land use change in delivering increased harvested area in the longer period from 1961/63 to 2006/07. They estimate a 50% contribution of intensive change to harvested area increase in Oceania and Africa, and a 25% contribution in Asia. OECD-FAO also comment that the rate of adoption of multiple cropping is in fact slowing, commenting that,

> *"The trends of [multiple cropping index] and harvested area in general are expected to continue but at a slower pace… Ever more intensive use of land in production in some regions through multiple cropping is perceived as a leading factor for land degradation and the undermining of its longer term productive potential."*

While these results support the general contention that multiple cropping is an important feature of the agricultural system, they are hardly consistent with the findings in B&I that intensive land use change is fifteen times more important than extensive land use change for the world except Africa, and that in Africa (where OECD-FAO found such a strong response) intensive and extensive change are actually counter-correlated.

As noted above, none of these data issues or interpretive challenges are recognized by the ICF report, which simply takes the B&I results without discussion and starts applying them to ILUC calculations.

Beyond a failure to recognize the limitations of the B&I analysis, the ICF report has also neglected to pay adequate attention to the caveats that B&I place on their own results. B&I comment that, "it simply is not possible to conclude with certainty that the [RFS RIA] model predictions have been proven wrong and should be disregarded." B&I provide an example of why this is so:

> *"The Hertel et al. (2009) prediction that large land use changes from output price increases resulting from US corn ethanol production would occur in the United States, Europe, and Canada seems inconsistent with the fact that cultivated land decreased in the EU and Canada and stayed constant in the United States despite price changes that were many times larger than those predicted by the model. However, it could be that the amount of actual land reduction that would have occurred in the EU and Canada would have been much larger without the commodity price boom and that if actual land use changes were calculated relative to what would have happened without the price impact then the GTAP model predictions would be consistent with what we observe."(F Taheripour, Hertel, & Tyner, 2009)*

The ICF report's discussion of B&I entirely fails to capture these caveats, which are clearly stated in the B&I report, and the decision to exogenously apply B&I national-level conclusions to the outcomes of GTAP modeling goes explicitly beyond what B&I describe as appropriate uses of their data. For instance, in the ICF report it is stated categorically that, "[B&I] highlights two regions where the FAPRI-CARD land-use predictions did not come to realization." This contention is not supported by the reported results of B&I – a more appropriate statement might have been that, "Research by B&I has suggested that the FAPRI-CARD land-use predictions may not be consistent with observed trends." The pattern of over stating the strength of referenced conclusions (but only where they support a lower carbon intensity value for corn ethanol) is repeated adequately often in the ICF report as to suggest a systematic lack of balance.

In addition to substituting out the results of the FAPRI-FASOM analysis for modeling results from GTAP-CCLUB, the ICF report makes five further ex-post adjustments to the GTAP land use change results before computing the reassessed international land use change emissions. These adjustments are all based entirely on results from B&I. They are:

1. Treat 76% of increased crop acreage in Brazil as double cropping (i.e. assume it has zero associated land use change emissions);
2. Treat all acreage changes predicted in India as double cropping (i.e. assume they have zero associated land use change emissions);
3. Treat 29% of increased crop acreage in China as double cropping (i.e. assume it has zero associated land use change emissions);
4. Adjust sub-Saharan African results to allocate 1.35% of the changes to corn ethanol;
5. Attribute 50% of increased crop acreage in Indonesia as double cropping (i.e. assume it has zero associated land use change emissions).

The B&I analysis underlying these adjustments for Brazil, China, Sub-Saharan Africa and Indonesia is discussed further in the following sub-sections.

Notes on B&I analysis of land use change in Brazil

There appear to be serious errors in the Brazilian data used in the B&I report. B&I include a chart (Figure 4 in their paper) purportedly showing total harvested area and double crop harvested area from 2004 to 2012, based on data from the Brazilian Government (cf. IBGE, 2017)[22]. The B&I chart is reproduced in Figure 7.

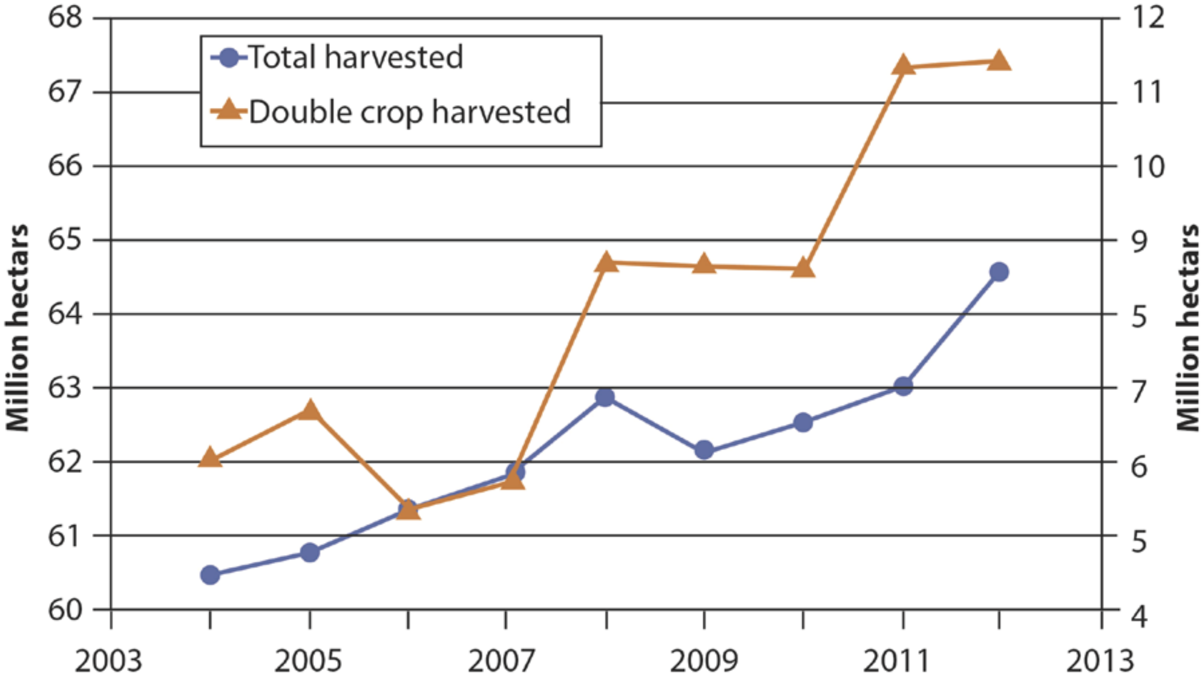

Figure 7. Brazilian harvested land data as included in B&I (their figure 4)

Based on our reanalysis of the underlying Brazilian data, the information presented in B&I appears to be incorrect on a number of points. Firstly, the lines on the B&I chart are labelled the wrong way round. The orange line, labeled as double crop harvested area, is actually the total harvested area. The blue line, labeled as total harvested area, is actually the area double harvested. A correctly labeled chart derived from the Brazilian data, extended to show the full period currently available (2003-2015) is shown in Figure 8. Beyond the labeling error, the values for total harvested area on the chart and quoted in the text appear to be incorrect (this may reflect an update to the published data rather than a transcription error by B&I). B&I report the total harvested area change 2003-2012 as 5.4 Mha, and the change in multiple cropped area as 4.1 Mha. We calculate 6.2 Mha and 4.1 Mha in that period, reducing the proportion of harvested area expansion delivered through multiple cropping to 66%, not 76%. Beyond this, it is noteworthy that the years of data on either side of the period discussed by B&I showed an area response heavily skewed towards extensification – 84% of harvested area increase in these additional years was extensive. That makes a large difference when compared to

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[22] https://sidra.ibge.gov.br/pesquisa/pam/tabelas

the result from the period 2003-2015 (the full period for which data is now available), during which 65% of the overall harvested area change was extensive, and only 35% intensive.

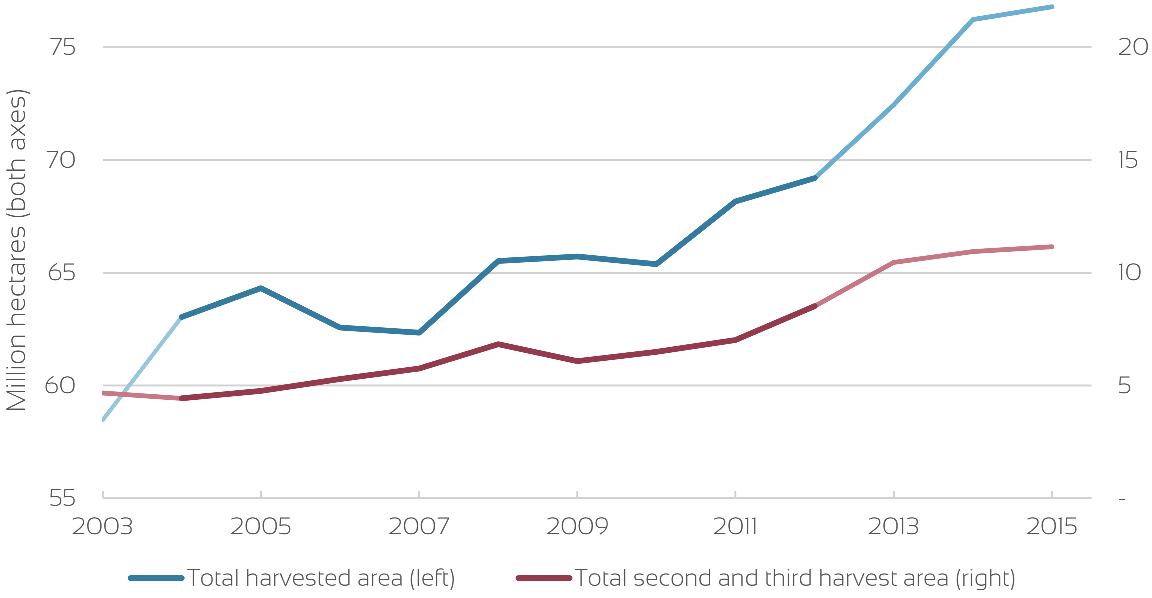

Figure 8. Correctly labeled Brazilian harvest data, 2004 to 2012

Note: the dashed parts of the lines denote years that were not included in the B&I paper

Given the strong reliance on this data in the ICF report, and that the source was clearly labeled by B&I, it would have been appropriate for ICF to check the values given, and to check whether data had been updated or expanded. Had the ICF report considered the larger dataset, then instead of assuming that only 24% of land use change in Brazil should be considered extensive, it would have used 65%.

Overall, the adjustment made to the Brazilian land use change emissions assumptions was based on methodologically questionable use of incorrectly reported data that gave a skewed result because it did not represent the full period available for ICF to consider. Or more simply, "garbage in, garbage out."

Notes on B&I analysis of land use change in China

B&I do not have good data sources for multiple cropping in China, relying on differencing between FAOstat data and data reported in a research paper (Cui & Kattumuri, 2010). Their conclusions on the contribution of extensive land use change to harvested area increase are largely narrative, claiming that in the mid-2000s economic forces tended to reduce land use and that therefore it is "unlikely that a significant portion of the increase in harvested area was caused by an increase in the amount of land cultivated." The source for this deduction (Cui &

Kattumuri, 2010) attributes loss of arable land primarily to ecological restoration and construction occupancy, noting that loss of arable land has been halted by land protection policies. It does not, however, follow that just because land has been lost to construction and other policy priorities there is no scope for marginal increases in land area in response to increased demand. Cui & Kattumuri (2010) report that China's remaining 'usable' grassland area in 310 Mha, and its forested area 170 Mha, compared to 120 Mha of cultivated land, although the agricultural potential of much of this land may be low. Still, there is plenty of scope in principle for extensive land use change.

Notes on B&I analysis of land use change in Sub-Saharan Africa

The ICF report states that it "adjusted the GTAP Sub-Saharan Africa region to allocate 1.35 percent of the GTAP 2013 change in acres to corn ethanol." In doing this, they seem to have misunderstood the meaning of the '1.35%' (and related '20.7 Mha') value given in B&I. The value 20.7 Mha represents the total global extensive land use increase (as calculated by B&I over the period considered) if one discounts all land use changes in Africa, and 1.35% represents this area as a fraction of total land in production. It is meaningless to apply the quoted '1.35 %' to the sub-Saharan African land use change in the way that the ICF report describes.

It is also worth noting that the narrative argument given by B&I to justify treating all extensive land use changes in Africa as unrelated to global market demand is highly questionable. B&I state that, "The extent to which extensive expansion in African countries was caused by high world prices is likely small for the simple reason that higher world prices were not transmitted to growers in many African countries." B&I reference an IFPRI discussion paper (Minot, 2011) in support of this conclusion. This paper notes that in a simple price comparison, the average price increase of food commodities in SSA was 71% of the price increase on the global market, which is very different from claiming that there is no price transmission at all. Indeed, changes in domestic prices were much higher for grains used to make biofuel (112% for corn, 111% for wheat) than for other crops (like beans and plantains, which are not traded as much internationally). Minot (2011) further notes that: "highly tradable commodities are more closely linked to international markets, so domestic prices of these commodities tracked the spike in world prices." The sources referenced do not support the assertion by B&I that sub-Saharan African extensive land uses changes cannot have been linked to world market demand.

Notes on B&I analysis of land use change in Indonesia

B&I argue that a significant fraction of harvested rice area expansion in Indonesia from 2004-06 to 2010-12 has likely come from increased multiple cropping. However, this is not data supported ("The extent to which intensification explains the 1.4 million hectare increase in rice harvested area shown in Indonesia cannot be determined by harvested area data alone"). The narrative case given is that Indonesia is densely populated, and thus a large increase in harvested area through new land is unlikely. This is highly unconvincing, given the well documented ongoing encroachment of agriculture into the rainforest. Similarly, a narrative argument is presented that recent corn harvested area expansion is through multiple cropping, "Given land constraints in Indonesia and the significant expansion of palm oil production." The B&I discussion of Indonesia is highly hypothetical, and it is somewhat surprising that ICF's authors felt this was a solid basis for numerical adjustments to GTAP land use change data.

7.2. Results of international land use change reassessment

As is discussed in Annex A, the conclusions in B&I about rates of double cropping are generally unduly strong given the evidence available, and give too little consideration to alternative explanations. As noted iabove, the analysis in B&I ignores land abandonment, considering only net rather than gross extensive land use change, thus systematically understating the importance of extensive land use change compared to intensive land use change.

There are five specific adjustments of this sort made in the ICF report. We have detailed (above, section 7.1) serious issues of data use or interpretation related to the B&I analysis supporting four of these five adjustments.

B&I also fail to provide convincing evidence that there is a direct causal link between demand for commodities and rates of double cropping, and cannot provide any quantitative comparison between the strength of the extensive and intensive land use change response to demand (as opposed to measuring general changes over time). This is crucial, as in ILUC analysis the question is not what changes are happening in agriculture as a whole, but rather what *additional* responses in the agricultural system may be expected in order to accommodate increased biofuel demand. Even if there were no issues in the B&I analysis itself, it would therefore not be methodologically justified for ICF to use B&I statistics on the overall fraction of new land in a region coming from the intensive response as a proxy for the expected contribution of increased double cropping to respond to a demand shock.

Finally, there is an implied conclusion in the ICF report's use of the B&I work that the results of GTAP ILUC analysis explicitly exclude the potential for multiple cropping. In fact, it can be argued that multiple cropping is one of the productivity increase options implicitly represented within the GTAP price-yield elasticity function. Indeed, one of the B&I authors has previously used potential for multiple cropping to justify the price-yield elasticity values utilized in GTAP:

> *"If the long-run price-yield elasticity not accounting for double cropping is set at 0.175, and if South America and the United States are the countries that contribute the most incremental commodity production in response to higher prices, then a mid-point value of 0.25 for the price yield elasticity seems reasonable" (Babcock, Gurgel, & Stowers, 2011).*

Beyond these general concerns, there are several issues related to the specific regional results, which are discussed briefly below. Overall, we have no confidence in the accuracy or appropriateness of the adjustments made in the ICF report to the international land use change emissions data, which result in a 22,708 gCO_2e/MMBtu reduction in assessed lifecycle emissions.

8. Treatment of co-products

The ICF report appears to double count the emissions benefits associated with the production of ethanol co-products (distillers' grains that are used as livestock feed), due to a failure to understand that the co-product credit is already implicitly included in the land use data used in the reassessment of domestic agricultural emissions.

In attributional lifecycle analysis, it is typical to either allocate part of the emissions from a fuel production pathway to any co-products produced in the course of that pathway (thus lowering overall emissions), or else to use the displacement method to calculate the emissions that might be saved elsewhere in the economy by reducing the need to produce alternative materials. In the RFS RIA, co-products are integrated into the FASOM and FAPRI land use and agricultural production analysis directly. This means that the displacement impacts of co-product generation are implicitly accounted for in both the land use change emissions analysis (domestic and international) and the farming emissions analysis (domestic and international). The increased availability of co-products in the FASOM and FAPRI modeling results in reduced need to produce other crops, and therefore less fertilizer and cultivation emissions associated with producing those other crops. For this reason, "no further allocation was needed at the ethanol plant" in the RFS RIA analysis.

In contrast, the ICF report explicitly considers co-products in its reassessment of domestic agricultural emissions. In section 3.1.4, the ICF report lists displaced animal feed assumptions per gallon of corn ethanol, and reports a calculated co-product emissions credit of -12,749 $gCO_2e/MMBtu$. Calculating a separate result for co-product credit could only be justified if co-products had also been decoupled from the domestic agricultural emissions result. Instead, however, the ICF reassessment methodology for domestic agricultural emissions calculates additional corn bushels required in the control scenario[23], uses USDA corn yield data and corn acreage data to allocate the additional corn acres required across the United States, and then multiplies the area changes by emissions factors for corn production in these regions. By using the RFS RIA results to assess increased production requirements, the ICF report already implicitly includes a co-product credit *before* it is exogenously calculated a second time in section 3.1.4.

The apparent double counting of co-product emissions credits is a major methodological error, and results in a substantial additional reduction in the calculated corn ethanol greenhouse gas emissions intensity.

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[23] As noted in section 3, the ICF report appears to have compared the 2017 control scenario to the reference scenario, when they ought to compare it to the corn-only scenario (comparing to the reference scenario results in some of the impacts of soy biodiesel expansion being rolled into the corn result). We believe that this is a separate methodological error, and this use of scenarios does not justify the double counting of co-product credits discussed here.

9. Process emissions

The reassessment of process emissions in the ICF report is undermined by a confusion between wet and dry milling of corn, and wet and dry distillers' grains as co-products. The ratio of wet to dry distillers' grains in ethanol refinery outputs is used as a ratio for wet to dry milling – in fact, the two ratios are unrelated (only dry milling produces distillers' grains). The ICF report therefore applies the wrong weightings to different process categories. It also misquotes process emissions results from the RFS RIA. By our calculation, these errors result in the ICF report slightly overstating the emissions from this part of the corn ethanol lifecycle.

The ICF literature review on fuel production emissions starts with a statement that "Recent LCA literature has shown that corn ethanol production accounts for over 40 percent of life-cycle GHG emissions." This is an unfortunate statement to include categorically in the introduction of a lifecycle analysis reassessment study – it would be one thing to note that the referenced paper (Wang, Han, Dunn, Cai, & Elgowainy, 2012) reaches this conclusion, but the language used clearly preempts the outcomes of ICF's own LCA.

The ICF report correctly notes that EIA data shows a gradual improvement over time in corn ethanol facility conversion efficiency. The corn ethanol conversion efficiency used in the RFS RIA is 2.71 gallons per bushel for dry mill plants and 2.5 gallons per bushel for wet mill plants, while EIA's monthly energy review (U.S. Energy Information Administration, 2017) takes an assumed efficiency of 2.82 gallons per bushel for 2012. Note that the data in the EIA monthly review are based on only a few observed data points,[24] most recently a 2013 report using industry survey data (Mueller & Kwik, 2013) that considered only the natural gas powered dry mill corn ethanol production pathway. As the dry mill pathway has a higher conversion efficiency than the wet mill pathway, the value used by EIA is not fully representative of the corn ethanol industry as a whole. It is conceivable that there may also be a degree of selection bias towards newer corn ethanol facilities in the survey data by excluding any ethanol plants still using coal as process fuel, but identifying the extent of ongoing coal usage in the industry and confirming whether this may indeed be a factor would require additional research. The update to GREET referenced by ICF for its reassessment used the Mueller & Kwik (2013) data as a basis for dry mill yield assumptions only. ICF's authors separately modeled wet and dry mill plants using GREET, with appropriate process yields for the respective cases.

The ICF analysis states that it gives a higher result than the RFS RIA for this lifecycle stage – 34,518 gCO_2e/MMBtu as against 30,000 gCO_2e/MMBtu. We note that there appear to be some discrepancies in the ICF presentation of the EPA RFS RIA outcomes. For one, the ICF report states that EPA analyzed a technology mix of 63% dry mill and 37% wet mill ethanol facilities. In fact, this 63:37 split in the RFS RIA relates to the production of dry vs. wet distillers grains and solubles from dry mill plants, not to the division between dry and wet milling itself. Moreover, the ICF report's description of EPA's assumptions is incorrect. While the EPA

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[24] "Observed ethanol yields (gallons undenatured ethanol per bushel of corn) are 2.5 in 1980, 2.666 in 1998, 2.68 in 2002, 2.78 in 2008, and 2.82 in 2012; yields in other years are estimated."

undertook modeling of wet mill corn ethanol production, these plants did not feature in the calculation of the 21% value for greenhouse gas emissions savings most often quoted from the EPA work as the lifecycle carbon savings of corn ethanol (for instance, the introduction to the ICF report states, "the EPA RIA's estimated GHG mitigation value for corn ethanol, 21 percent lower emissions than … gasoline, has dominated academic, industry, and policy discussions of GHG issues related to renewable transportation fuels"). The 21% greenhouse gas saving estimate in fact reflects only dry mill ethanol fueled by natural gas (see RFS final rule Table V.C-1, U.S. EPA, 2010). The RFS final rule notes that, "We do not believe new wet mill corn ethanol plants will be built through 2022," but does include wet milled corn ethanol from plants with biomass or biogas power as an eligible fuel for D6 RINs.

It is also unclear to us what the source or basis for derivation is for ICF's reported 30,000 gCO_2e/MMBtu greenhouse gas intensity value for corn ethanol processing from the RFS RIA. The value given for 2022 for the average new dry mill plant with natural gas power is reported as 28,000 gCO_2e/MMBtu in the RFS2 final rule (more precisely 27,851 gCO_2e/MMBtu by our calculation from the underlying results). We were not readily able to find a combination of processing greenhouse gas intensities for specific corn ethanol production pathways with either dry and wet milling or dry and wet DGS production in the ratio 63:37 that would give a weighted average emissions intensity of 30,000 gCO_2e/MMBtu. Similarly, the 50,000 gCO_2e/MMBtu quoted for corn ethanol with power from coal is apparently inconsistent with reported pathway results for processing emissions from coal powered plants. It seems likely that the quoted value of 30,000 gCO_2e/MMBtu is a transcription error, or else a simple case of rounding to too few significant figures, and that the correct value should be 28,000. This is supported by the observation that summing the RFS RIA emissions values given in the ICF report results in a total of 81,140 gCO_2e/MMBtu – almost exactly 2,000 more than the overall emissions value quoted from the RFS RIA of 79,180 gCO_2e/MMBtu.

Given that the ICF report incorrectly reports both the value and the basis of the processing emissions figure from the RFS RIA, we should also reconsider the comparison between its recalculation and the RFS RIA. Adopting the ICF weighting[25] for comparable pathways from the RFS RIA analysis, and the RFS RIA split of wet vs. dry DGS, we calculate an 'average' emissions value of 31,021 gCO_2e/MMBtu. We also note that ICF may have overstated the results of their own analysis. We were unable to reproduce the reported 34,518 gCO_2e/MMBtu ICF result from their documented estimates for the three corn ethanol production cases modeled given the quoted weightings. Instead, we calculated 34,045 gCO_2e/MMBtu. We therefore find that while the ICF reanalysis is still higher than the RFS RIA values, when making a more appropriate comparison the difference is about 3,000 gCO_2e/MMBtu, rather than 6,500 gCO_2e/MMBtu[26]. While the discrepancies in this section only result in a relatively small error term in the overall result, the apparent misunderstanding and misquoting of the documented EPA results and inconsistency within the ICF report's own quoted numbers raises further questions about the overall quality of the reassessment in the ICF report.

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[25] Between pathways for dry mill with and without corn oil extraction, and wet mill, and between natural gas, coal and biomass power.

[26] Calculated as 34,518 gCO2e/MMBtu minus 28,000 gCO2e/MMBtu.

10. Summary of adjustments in ICF 2014 'current' assessment

Figure 9 shows a comparison between the lifecycle emissions for corn ethanol in 2022 from the RFS RIA, as opposed to the reassessed values reported by ICF. On face value, the numbers suggest that the EPA originally overestimated the greenhouse gas impacts, but underneath the figures is a catalogue of data errors and questionable assumptions, as detailed above, that cast serious doubt on the value of the ICF reassessment.

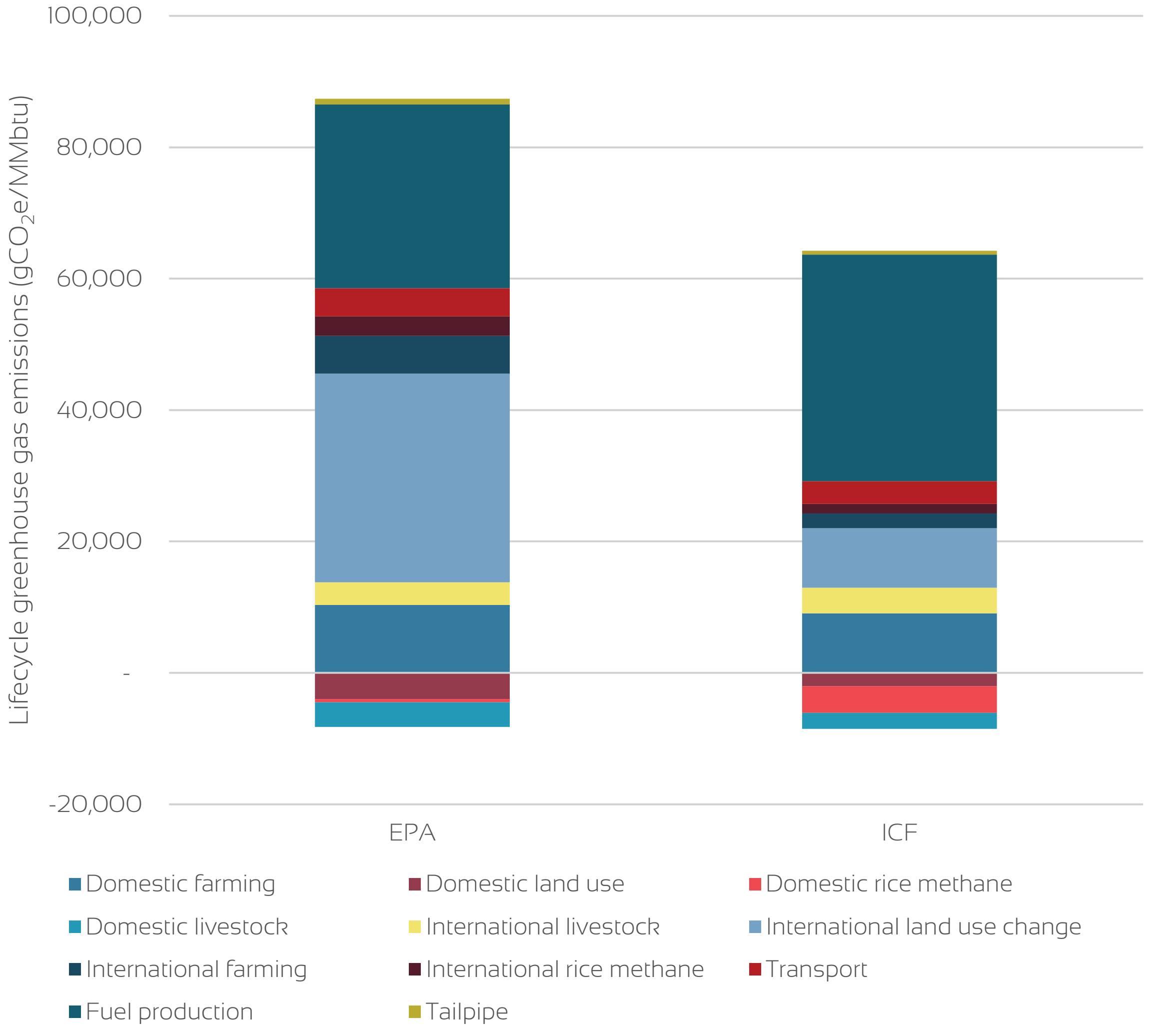

Figure 9. **Comparison of ICF 2014 result and result for 2022 from RFS RIA***

**As noted in Table 1, there appear to be some discrepancies between the numbers quoted by ICF and the values given in the RFS RIA.*

Table 1 provides a brief overview of the revised results from the ICF report for several key lifecycle stages, and comments on the quality of the assumptions that have gone into generating those revised results. In each case considered, there are fundamental problems in the analysis that make it impossible to conclude that the reassessed values represent an improvement over the assessment made for the RFS RIA. Overall, the quality of the analysis in the ICF report is inadequate to draw any conclusion about current corn ethanol emissions, or to conclude that they are different (in either direction) from the values assessed by the EPA for the RFS RIA.

Table 1. Review of key adjustments made by ICF to the RFS RIA LCA result

Emissions source	Emissions gCO$_2$e/MMBtu			Comments	Confidence in ICF result
	RFS RIA	ICF	Difference		
Domestic farming	10,313	9,065	-1,248	The ICF documentation suggests that they have inappropriately compared the control case to the reference case, instead of the control case to the corn only case. ICF also appeared to have double counted ethanol co-products.	**Low**
Domestic land use	-4,000	-2,038	1,962	The CCLUB emissions factors are highly problematic, and ICF do not make a compelling case that GTAP is a preferable model to FASOM.	**Low**
International land use change	31,790	9,082	-22,708	The same issues apply in replacing international FAPRI LUC results with GTAP as when replacing domestic FASOM results with GTAP. Additional adjustments based on B&I are contentious, and in some cases based on errors of data and interpretation.	**Low**
Fuel processing	28,000[A]	34,518	6,518	The reassessed value from ICF is not strictly comparable to the calculation in the RFS RIA. With a more appropriate comparison, the difference is smaller. The ICF weighted average result seems not to match reported underlying pathways.	Low-medium
Domestic rice methane	-209[B]	-4,034	-3,825	The methodology adopted is deeply flawed, the data referenced appears to be from the preliminary not final RFS2 analysis, and a corn only case is inappropriately compared to a reference case, giving a meaningless result.	Low
International rice methane	2,089[C]	1,480	-609-	The methodology applied appears to be deeply flawed, similar to domestic rice methane.	Low
Overall	**79,180**	**55,731**	**-23,449**		

Notes:

We have reviewed only some of the lifecycle stages reassessed by ICF in this report. The prevalence of errors in the stages reviewed, suggests that the remainder of the analysis should be thoroughly reviewed as well.

A Quoted by ICF as 30,000 gCO2e/MMBtu in their Table 3-63.

B This is quoted by ICF as "less than -500", but the exact value is given in Table 2.4-13 of the RFS RIA.

C Quoted by ICF as 3,000 gCO2e/MMBtu in their Table 3-52.

11. Conclusions

11.1. Choice of data

Throughout the ICF report, there is a tendency to reference and emphasize data sources that reflect positively on corn ethanol, while downplaying or ignoring entirely other data sources. In the land use change section in particular (notably the reliance in the international land use change reassessment on Babcock & Iqbal, 2014), this includes a reliance on studies funded by the corn ethanol industry to the exclusion of other perspectives.

The lifecycle analysis of biofuels is a wide field that has generated an immense number of documents over the past ten years, and we do not intend to assert that ICF ought to have attempted a full systematic literature review. We do feel, however, that it was incumbent upon the ICF authors to make a good faith effort to seek balance in the evidence they considered, and this seems not to have happened. In a report commissioned by a private stakeholder, this would be regrettable. In a report for a department of the U.S. Government, it raises significant concerns. It is not particularly difficult to find alternative data and perspectives on these issues; the absence of such perspectives from much of the report severely undermines the conclusions it reaches.

11.2. Use of data

Beyond the selectivity in the choice of references to inform the ICF report's analysis, the way sources are used is often open to criticism. For example, we identified a simple mistake in the use of nitrogen fertilization data (confusing overall fertilization rates with nitrogen fertilization rates), a basic misinterpretation of results regarding intensive land use change in sub-Saharan Africa, and double counting of the emissions benefit of co-products. The use of the control, reference and corn-only scenarios from the RFS RIA modeling is muddled throughout the document. Methodological choices have been made in several places that are simply inappropriate, and apparently show a failure to appreciate the details of the analysis undertaken for the RFS RIA and of several of the other sources referenced. There were cases in which data was presented for a particular period when considering a longer period may have resulted in different conclusions being drawn. Narrative conclusions are in many cases stronger than supported by a balanced reading of the underlying evidence; results from preferred studies are presented as accepted facts ignoring caveats in the studies themselves; there is a constant emphasis on the potential for further improvement but no acknowledgement of the possibility of stagnation or regression.

In the land use change analysis, results from the RFS RIA obtained with the FAPRI and FASOM models are replaced with results obtained with the GTAP model, but the justification for these changes goes little further than the observation that the RFS RIA was published in 2010 and other results are more recent. The ICF report neglects to discuss directly the reasons given explicitly in the RFS RIA and supporting documentation for the EPA to prefer FAPRI-FASOM over GTAP. The casual abandonment of the EPA analysis stands in marked contrast to the detailed explanations and peer review given to justify EPA's original adoption of that analytical framework.

Some of the issues highlighted in this report reflect a degree of subjective judgement (placing more weight on some studies than others, preferring one model to another and so on), but it should be emphasized that many of the problems we have documented in this review reflect analysis that is simply wrong, and the results reported in the ICF report must be understood in that light.

Taken as a whole (the choice of which studies to reference and which studies to ignore, the way that narratives are constructed around referenced results, the errors and misinterpretation, the willingness to rely too heavily on a small number of industry supported sources in making their own lifecycle assessment), the imbalances in the report add up to a systematic tendency to favor a positive view of corn ethanol. This is most true for international land use changes, where a cascade of questionable adjustments give a very large emissions saving compared to the RFS RIA results. It must be acknowledged that there are at least some lifecycle stages for which the ICF reports estimated higher emissions values for 2014 than the RFS RIA gives for 2022, but these are heavily outweighed by the stages where lower emissions are calculated. The result is that the report is very likely to understate the lifecycle emissions of corn ethanol, and exaggerate the extent to which circumstances have changed since the EPA's final assessment.

11.3. Recommendations

The work undertaken for the regulatory impact assessment of the RFS2 is an impressive resource, far less well understood in much of the alternative fuels community than it probably should be. There are elements of the RFS RIA that could be disputed, and that should be done differently if and when the analysis is updated; we have identified several in this review. Nevertheless, the reassessment by ICF is inconsistent, selective and fatally plagued by methodological errors. There are legitimate points raised by the ICF report that should not be dismissed, but their numerical results cannot be taken seriously given the many errors and imbalances described here. The reassessed lifecycle intensity for corn ethanol given in the ICF report should therefore be disregarded.

There is useful insight that can be taken from the building blocks scenario in the report, provided that that scenario is understood as a shopping list for improvements in the corn ethanol industry that should actively be pursued, rather than a prediction for what will be achieved in the absence of concerted effort. There is no real possibility of the full set of practices described in the building blocks scenario becoming standard by 2022. Given this and the analytical flaws, the quoted 76% potential greenhouse gas reduction must be understood as at best aspirational, and perhaps unachievable, for the foreseeable future. At the current time there is no convincing reason presented by the ICF reassessment to believe that the average greenhouse gas emission reduction delivered by U.S. corn ethanol is any better than indicated by the assessment in the RFS RIA.

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Annex A. Notes on 'Using Recent Land Use Changes to Validate Land Use Change Models' (Babcock and Iqbal, 2014)

We are grateful to the International Council on Clean Transportation (ICCT) for permission to include content originally written for the ICCT.

A.1. Overview

Babcock and Iqbal (2014) (henceforth 'B&I') sets out to assess actual land use changes that have occurred globally since the mid-2000s and compare these observed changes to model predictions generated in regulatory analyses of indirect land use change by the US EP and California ARB. The work was supported by the Renewable Fuels Foundation and the Bioindustry Industry Center. The study presents three main findings:

Harvested area as reported by FAOstat and other agricultural statistics may be a poor indicator of total agricultural area in many countries.

For countries outside Africa, the study finds that more land use change has occurred at the 'intensive' than the 'extensive' margin.

It is difficult to draw any categorical conclusion about whether ILUC model results reflect actual outcomes because of the lack of a counterfactual when considering real data.

The authors argue that the importance of responses at the intensive margin (beyond the 'pure' yield response) has not been adequately considered by regulators assessing likely indirect land use change emissions due to increased biofuel production. The conclusion by the authors is that existing estimates of ILUC factors are too large "because they are based on models that do not allow for increases in non-yield intensification of land use."

Overall, the case made is convincing that there are responses at the intensive margin, notably double cropping, that are not explicitly considered in ILUC studies. However, the paper shows a readiness throughout to interpret ambiguous data as supporting the claim that most land use change occurs at the intensive margin when other interpretations might be available. Having correctly emphasized the limitations of agricultural land use data, the paper does not give adequate discussion to the possibility that some of the data differences B&I attribute to intensification may in fact represent differences of quality or methodology in the underlying statistics. Without a more systematic assessment of the extent to which reliable conclusions can be drawn by differencing the datasets used, the conclusions are stated too strongly.

Similarly, in several cases B&I present narrative justifications for assuming that changes in harvested area reflect intensification and not extensification. These justifications are not always compelling. For instance, for Indonesia where statistics show 1.4 million hectares of increased rice harvest B&I claim that "given that Indonesia is one of the world's most densely populated countries, and 1.4 million hectares represents a 12% increase in harvested production, it is unlikely that a significant portion of this 1.4 million hectares is new land." Given, however, that this increase in reported rice area coincides with a larger 2.5 million hectare increase in palm area (of which several hundred thousand hectares represented expansion on peatland with very large ILUC implications, as shown by Miettinen at al., 2012), it is difficult to accept this hypothesis without supporting evidence. Conversely, in Africa where considerable extensification is found by B&I, there is an excessive readiness to conclude that this extensification cannot be linked to international markets. While elements of the case presented are reasonable and correct (many parts of Africa are indeed relatively insulated from world market commodity prices) this does not apply to all of Africa, and thus the conclusion given is much too broad.

A key element missing from the analysis, that would support stronger conclusions on the validity of existing ILUC analyses, is a systematic assessment of the extent to which land use changes at the intensive margin are responsive to commodity prices (and hence increased agricultural demand) and to what extent intensification may have been driven by factors other than demand (such as knowledge transfer or government action). As B&I recognize, ILUC modeling requires comparing a scenario with biofuel demand to a counter-factual scenario without it. The paper does not provide any quantified assessment of the extent to which additional demand due to biofuels may have been driving land-use intensification in the markets discussed. For instance, in Argentina, B&I conclude that the area of double cropped soy has been determined largely by changes to Argentinian government policy, not by international demand.

B&I also do not consider the possibility that intensive land use change has been implicitly characterized in existing ILUC models within the traditional price induced yield parameter. Discussing iLUC modeling by CARB, Babcock et al. (2011) concluded that "if the long-run price-yield elasticity not accounting for double cropping is set at 0.175, and if South America and the United States are the countries that contribute the most incremental commodity production in response to higher prices, then a mid-point value of 0.25 for the price-yield elasticity seems reasonable." This was the price-yield elasticity included in the original GTAP ILUC modeling by CARB for the LCFS. Indeed, the possibility of a cropping intensity response to biofuel demand has been invoked on several occasions in the California discussion in defense of the overall price-yield elasticity used in the models.

In conclusion, the paper places legitimate emphasis on crop intensity as a factor in determining land use responses to expanded biofuel demand, but without further analysis to confirm various assumptions presented in the paper, and further assessment of the extent to which crop intensity responds to price for different cropping systems, it is premature to make any firm assertion about whether land use change data since the mid-2000s suggests that existing ILUC estimates are too high, or for that matter too low.

A.2. Detailed comments

Biofuel and food prices: The paper notes that "In the mid-2000s prices … increased dramatically due to growth in demand for food and biofuel producers, underinvestment in [agriculture] … and poor growing conditions in major producing regions." The paper takes it as read that biofuel demand, as a contributor to agricultural commodity demand, has been one driver of increased prices in recent years. They quote estimates that 36% of corn price increases from 2006 to 2009 and 34% of corn price increases from 2006 to 2012 respectively were attributable to the corn ethanol mandate.

China and double cropping: In the section on China, the paper notes a claim that cultivated land area was reducing in China in the early 2000s and that this reduction was only halted by government action. On this basis, it is argued that underlying economic forces must have been driving reduced land area, and therefore that increases in harvested area to 2012 could only be attributable to increased harvest intensity. This argument is plausible but not compelling, not least because the data referenced only runs up to 2008. Estimates of absolute double cropped area are further derived by comparing FAOstat harvested area values to Chinese Ministry of Land Resources data for 'cultivated area.' This comparison is problematic. Firstly, it is normal for there to be fairly significant inconsistencies between FAOstat and other land data sources. These differences could be due to systematic methodological differences in reporting or categorization of land uses, or other data issues. One should therefore be cautious about the interpretation of differences between data sources. More troubling than this, the paper simply assumes no increase in cultivated area from 2008 to 2012. This is not adequately robust in the context of the inference being made in this paper about total double-cropped area change in this precise period.

Africa and double cropping: In the section on Sub-Saharan Africa, B&I note that "a lack of access to technology and capital is one defining characteristic of traditional agriculture in sub-Saharan Africa" and asserts that hence "there is no evidence that double cropping is widely adopted." While it is certainly plausible that the authors are correct that double cropping is less common in Africa than in the other regions considered, this reasoning is inadequate and is not supported by any reference associating levels of double cropping with access to capital, nor with an adequate recognition that not all agriculture in Africa is traditional. OECD-FAO (2009) note that from 1961-1963 to 2006-2007 about half of the increase in harvested area in Africa was actually attributable to increased multiple cropping index, which contradicts the above claim from B&I.

Data differencing: On page 12, the paper notes that for the U.S. they have chosen to use a different data source in the calculation of total land use than is used for other reasons. The justification is that the sum of FAO arable land plus permanent crops shows a marked shrinkage over the period of interest, related to changes in temporary pasture (it is unclear from the paper whether these are 'real' reductions or a shift in classification). This is likely a reasonable data choice, but it emphasizes the considerable risk of misleading results when conducting differencing on this type of land use data.

Indonesian rice multiple cropping: The paper claims that the major source of increases in harvested rice area is an increase in irrigation that has allowed increased multiple cropping of rice. The case presented against interpreting harvested area increases as extensification is that Indonesia is a densely populated country, and that the areas required were large – this is unconvincing on its own. It is unquestionably true that rice is often double or triple cropped in Indonesia. However, it is not clear whether the interpretation of the data given by this paper is correct. In 2012, USDA in a 'commodity intelligence report' noted[27] that about 70% of lowland rice is double cropped. On this basis, one would expect to see reported harvested area be very considerably higher than reported cultivated area. However, USDA data on cultivated area (USDA FAS World Agricultural Production reports) has cultivated area only marginally below FAOstat reported harvested area. This could imply that USDA is reporting harvested area rather than total cultivated area in the World Agricultural Production Report, which would be consistent with data from the aforementioned commodity intelligence report purporting to show Indonesian data on the area of rice at each harvest in 2011, which add to about 11 Mha, with only 6 Mha of rice in total. However, we were not able to confirm this data in the Statistics Indonesia dataset,[28] and other sources (including satellite mapping) support the conclusion that total cultivated area is of the order of 10 million hectares.[29] This implies that it is in fact incorrect to assume that the FAOstat harvested area data counts each harvest separately. If this pattern of data reporting may be repeated in other regions, it would undermine the conclusions of the paper, as they are predicated on the assumption that all double-harvested areas are double counted in the FAO stat data.

China and the soy trade: On page 13, the paper asserts that Chinese soy demand has been a major driver of land use decisions in the U.S., Argentina and Brazil over the past decades. This is consistent with trade data from FAOstat, as shown below in Figure A, and with the expectation that soy area is largely driven by protein demand.

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[27] http://www.pecad.fas.usda.gov/highlights/2012/03/Indonesia_rice_Mar2012/

[28] http://www.bps.go.id/

[29] E.g. , Redfern, Azzu, & Binamira, (2012); Lee, Moniac and Daratista (2012); Frederik and Worden (2011)

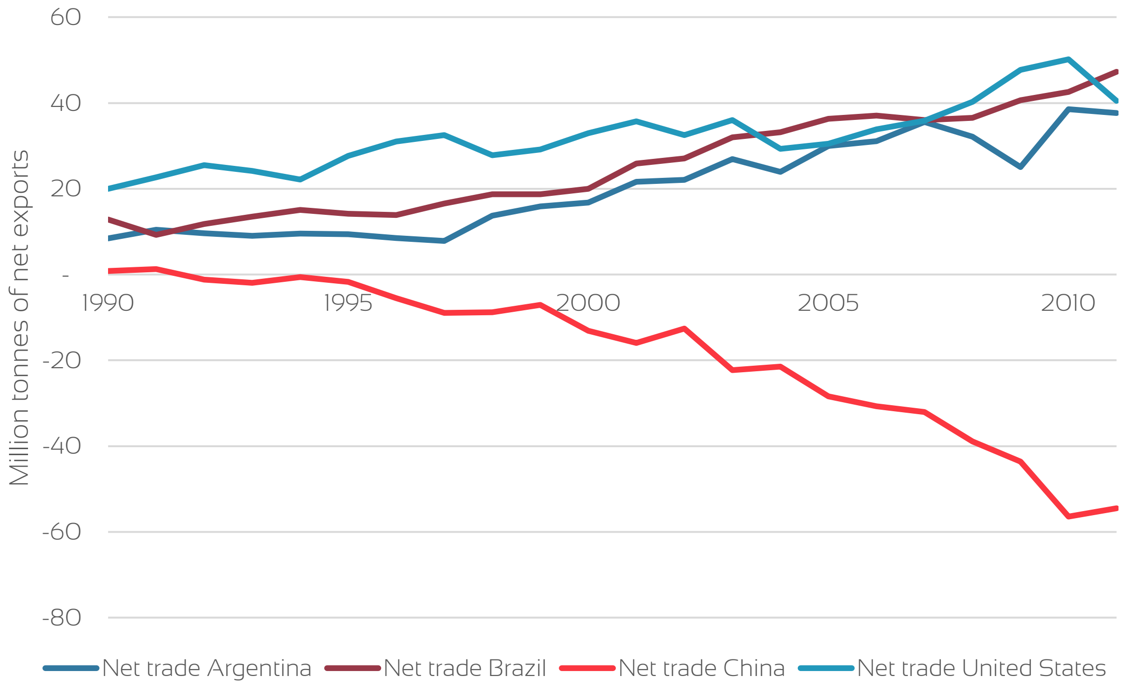

Figure A. Net soy exports (meal plus beans) since 1990 for U.S., Argentina, Brazil and China (FAOstat)

Argentinian double-cropped wheat: The paper notes that in Argentina, increased soy demand was not accompanied by increased double cropping (unlike in Brazil). The explanation is that 'if soybean area needs to increase, less wheat land means less land available for double cropping, thus soybean first area by definition must increase.' This explanation is rather unconvincing – if double cropping (intensification) is indeed a strongly driven response to demand and would have been viable in this case, then it would have been equally possible for Argentina to add considerable acreage of second crop soy after wheat as to replace wheat entirely. The paper argues that changes in government policy to favor soy export over wheat through taxes and subsidies are responsible for preventing economically rational increases in double cropping. However, the source cited (Nogués, 2011) finds that wheat export taxes are still *lower* than those on soy (by 12%), and in fact in 2008 the peak rate of soybean export tax reached 41% during the food price spike. Similarly, the subsidy regime has provided subsidy to wheat mills, not to soybeans, which would generally be expected to encourage more rather than less wheat production. The wheat subsidy was actually paid for through increased soybean export tax. Aside from taxes and subsidies, Nogués does note that Argentina implemented quantitative export restrictions on wheat from 2006, whereas soybean exports have not be regulated in that way. Nogués estimates that the impact of export controls on producer prices has been equivalent to adding 17% to export taxation. In that case, the combined impact of export tariffs plus taxes on wheat producer prices should indeed be greater than that on soy producers. This could have a role in explaining reductions in wheat area (although a perverse one, as the intended purpose of the export restrictions is to guarantee domestic supply, not to reduce production). Overall, the contention by B&I that policy drivers may have contributed to a failure to increase double cropping in Argentina supported by Nogués, but not through the taxes and subsidies B&I point to. More generally, it should be understood that agricultural markets are distorted by policy in many regions. Nogués notes that the possibility to double crop has reduced as single crop soy area has increased, and this increase is also driven by raised soy prices. This provides an example in which increased price (soy prices rose much more than wheat prices coming into 2006) not only has failed to increase double cropping, *but has actively reduced it.* B&I choose not to draw any more general conclusion from this example.

African connectivity to world prices: The one area in which B&I find a clear case of a large extensive land use change but little intensive land use change is Sub-Saharan Africa (SSA). However, they argue that it is unlikely that the extensive change in SSA has been price driven, on the basis that "higher world prices were not transmitted to growers in many African countries." The main reference for this low price transmission to SSA is from Minot (2011). However, in a simple price comparison, Minot found that the average price increase of food commodities in SSA was 71% of the price increase on the global market. While this clearly suggests a reduced connectivity, it is far from showing that there is no price transmission at all. Indeed, changes in SSA domestic prices were much higher for grains that are used to make biofuel (112% for corn, 111% for wheat) than for other crops (like beans and plantains, which are not traded as much internationally). Minot writes: "highly tradable commodities are more closely linked to international markets, so domestic prices of these commodities tracked the spike in world prices." While the simple price comparison is consistent with good transmission of grain prices from world to SSA markets, more detailed econometric analysis on tradable grains in 62 local markets across 9 SSA countries found only 13 with a statistically significant long-run relationship with international prices. It is important to note that the lack of a statistically significant correlation is not the same as proving that there is no correlation. Minot concludes "that international prices of food grains do have some effect on African markets for rice and (to a lesser degree) maize, but the effect is usually swamped by the dominant effect of weather-related domestic supply shocks." B&I interpret these results as finding "some evidence of a linkage in large urban centers and in coastal markets, which is consistent with markets in cities and in coastal ports being more integrated with world markets. However, ... these limited linkages to world prices did not find their way through to rural areas where most crops are grown. ... One can conclude that the main driver of land expansion in many African countries was not higher world prices." This strong conclusion about rural vs. urban prices is not well supported by Minot's discussion. Indeed, Minot does not draw any conclusions about urban vs. rural price linkages and in fact does not discuss this at all. The assertion that world food commodity prices are not transmitted to African farmers *at all* is clearly very much overstated, and not adequately supported by evidence in the Minot paper. There is a considerable evidence base on increasing foreign investment in African land (delineated as 'land-grabbing' in some studies), and it seems reasonably clear that deals of this type, which affect considerable areas, are informed by international prices. On the other hand, as noted by many other authors, it is reasonable to conclude that SSA food production is not as responsive to world markets as production in many other regions.

Comparison of intensive and extensive land use changes: Based on their assessment of changes in cropping intensity, B&I conclude that from 2004-2006 through to 2010-2012 the global intensive land use change was double the global extensive land use change. They note that if one ignores African extensive land use change, then intensive land use changes are 15 times extensive land use changes. This second result is contentious, for one because (as noted above) there is no adequate case to fully disregard all extensive land use change in Africa, and also because there is no comparable scrutiny on the link to price of intensive land use changes in other areas. Given the considerable uncertainty in the way that the comparative values have been calculated and the lack of rigorous demonstration of a causal link between prices and either the extensive or intensive outcomes detailed in the paper, caution is appropriate in any interpretation of the comparison of the values. For instance, while B&I reference the OECD-FAO agricultural outlook 2009 for the claim that "intensive land use change has been the driving force behind higher production levels," this seems to be an overstatement of the results in that report, and is not reflected in the text of the report. Over the long term (from 1961-1963 to 2006-2007), OECD-FAO conclude that about 50% of additional harvested area has come at the intensive margin, and the other 50% at the extensive margin, i.e. at best equal contributions to higher production levels.

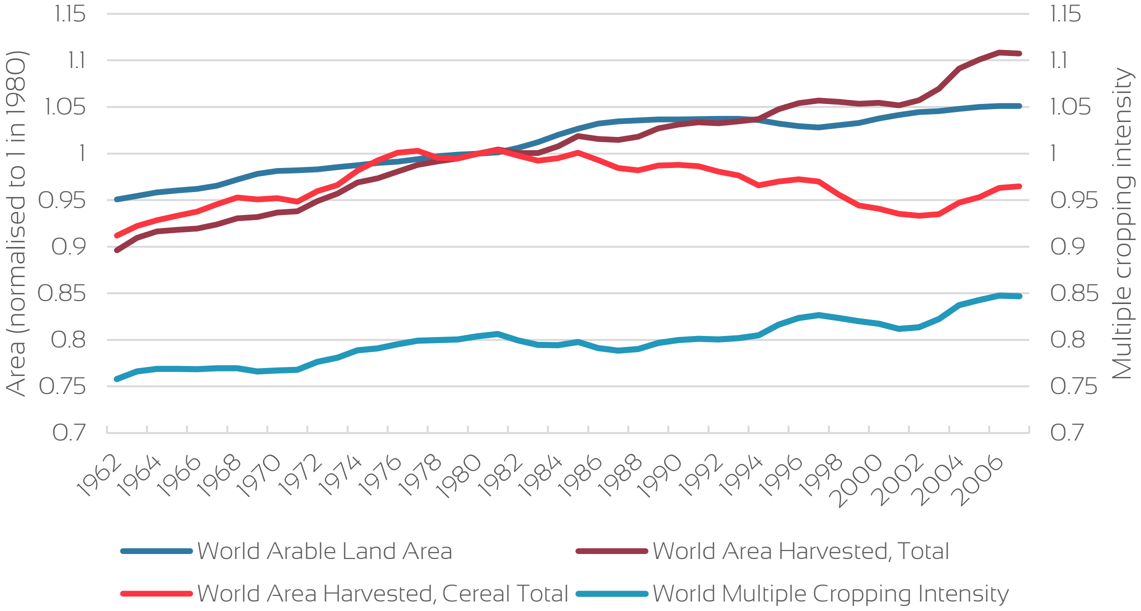

Figure B. **World trend of arable, cropped and harvested land areas and cropping intensity (OECD-FAO)**

OECD-FAO also show a global increase in multiple cropping index from 2002 to 2006, before the recent higher prices kicked in (Figure B). This suggests that there are underlying trends driving cropping intensity beyond prices alone. Finally, the OECD-FAO signal a note of caution about future cropping intensity increase. The report states that 'ever more intensive use of land ... through multiple cropping is perceived as a leading factor for land degradation and its longer term productive potential.' It also finds that 'the trends of MCI [multiple cropping intensity] and harvested area … are expected to continue, but at a slower pace.' This report therefore does not provide an adequate basis for a conclusion that in the future intensive area change will be the dominant response to price change.

Comparison of model predictions with actual land use changes: On page 19, B&I discuss what can be learned by comparing their analysis of real world land use changes with model results. One question raised by B&I is the extent to which land use changes are triggered by price changes, rather than other factors. The claim is that if all land use changes are triggered by prices, then model results could fairly be compared directly to real world outcomes, whereas if land use change is completely price independent then no comparison would be possible. This statement is too categorical on both counts, even for these hypothetical cases. Even if it were accepted that price was the sole driver of decision making, the source of demand changes is an important factor in model results. For instance, in modeling of the US corn mandate, all additional demand comes necessarily from the US corn mandate. This leads us to expect the strongest responses within the US system etc., and in the corn crop more than any other crop. In contrast, real world price increase relates to a sum of supply and demand changes through the global system, making it very difficult to decompose the US corn mandate as a price driver from the Australian wheat harvest etc. etc.

On the other hand, even if it were accepted that price is not a direct driver of land use choices, it would not be clear that demand was not a driver of change through other transmission vectors. For instance, expectations of future demand (e.g. for biofuels) may affect land use choices directly, even if not reflected in prices. The conclusion is that for this and other reasons it is very difficult to take results from a model in which a specific baseline and policy scenario are modeled with a single policy difference, and compare those results to real world land use changes driven by innumerable pressures. In the case of model predictions of US land use change, B&I argue that, "The only way that the US land use prediction is consistent with the historical record is if cropland in the United States would have dropped by a large amount in the absence of the large price increase." This is in fact precisely what many analysts would have expected – that in the absence of demand for biofuels, crop area would have shrunk further in the US, with ILUC emissions resulting to a large extent from 'foregone sequestration' in areas that would otherwise have been allowed to naturally recover vegetation. In another example, B&I state that "the CARD-FAPRI model dramatically over-predicted land use change in Brazil relative to Argentina and other South American countries." In truth, without being able to more accurately decompose the causal links between world prices and land use change in each South American country, it is not possible to draw such strong conclusions. Perhaps US biofuel demand really did drive land use change preferentially in Brazil, while other markets were more strongly affected by Chinese soy demand. B&I have not by any means precluded such an explanation, and many more could be posited. A more legitimate conclusion would have been that the apparent inconsistency between model predictions and real outcomes warrants further examination and should be considered in future model calibration.

B&I do in fact recognize elsewhere in the text that categorical conclusions from this type of exercise are difficult or impossible, observing that "it is not possible to say this prediction is inconsistent with the recent historical data given that we cannot observe what land use would have been without the price increase," and that "it simply is not possible to conclude with certainty that the model predictions have been proven wrong and should be disregarded." The truth is that the thing that any model assesses (a simple change between a policy scenario and a counterfactual) is fundamentally not equivalent to the complex set of drivers of real world land use. The distribution of real world land use changes after a generalized price increase (such as 2007/08) could not and should not be expected to match precisely the land use changes that would be predicted by a model when increasing only corn ethanol demand. Indeed, if it were possible to construct some even a hypothetical 'perfect model' of global agriculture and land use, *we would expect that the model results for ILUC modeling would not be consistent with any particular period of real world land use changes*. It is therefore potentially vexatious and misleading to focus too hard on specific outcomes that differ between model results and real world outcomes, as some such differences would be expected in all cases.

Implications of cropping intensity for ILUC estimates: B&I undertake a comparison of land use changes predicted in some of the GTAP corn ethanol scenarios for the Air Resources Board. Using a decomposition supplied by staff of the US Renewable Fuels Association, they detail predicted changes in total crop area in response to corn ethanol demand, and discuss the fraction GTAP assigns to forest vs. the fraction assigned to pasture. Again, B&I recognize correctly the difficulty of making a direct comparison to what actually happened, "it is not possible to conclude whether the GTAP model prediction that US cropland would be 1.6 million hectares higher due to higher prices is inconsistent with what actually happened." They then, however, speculate on what the implications would be if emissions due to foregone sequestration were different than emissions due to active deforestation, concluding that, "The magnitude of the change in estimated CO_2 emissions from cropland that is prevented from going out of production relative to forest that is converted to cropland is potentially large." This observation is somewhat trivial – certainly, if the emissions assigned per hectare of land use change are inappropriate, this would change the results. Equally, if the emissions from pastureland conversion are lower than the emissions from foregone sequestration, the results would be put out the other way, and indeed foregone sequestration emissions for regrowing forest would be rather higher than typical emissions from grassland conversion, which are higher than emissions from pasture conversion.

The main result related to the B&I analysis that is relevant to our understanding of ILUC emissions is that they have correctly identified that double cropping occurs in many world regions, and that this is not reflected in detail in either the datasets used by ILUC models or in the ILUC models themselves. Two questions arise from this: is it necessary for double cropping to be implicitly included in ILUC models in order to make the results meaningful; and does this analysis clearly suggest that existing ILUC estimates have been overestimates.

To the first question, one way that cropping intensity could be argued to be implicitly reflected is that models allow price based increase in yields, and one could argue that this yield increase can be considered to include cropping intensity. Indeed, Babcock and Carriquiry (2010) recognize that double cropping could be implicitly included in the yield elasticity parameter in GTAP, arguing that "the incentive to double crop soybeans with corn and cotton in Brazil justifies use of a yield elasticity of 0.24 by itself." Based on an argument that double cropping can be reflected in GTAP through the yield elasticity, Babcock et al. (2011) conclude that "if the long-run price-yield elasticity not accounting for double cropping is set at 0.175, and if South America and the United States are the countries that contribute the most incremental commodity production in response to higher prices, then a mid-point value of 0.25 for the price yield elasticity seems reasonable." One could therefore reasonably argue that even though cropping intensity is not explicitly modeled in GTAP etc., that it may be handled adequately through the general yield term. That said, there is a reasonable case from this paper to consider adding double cropping more explicitly to future modeling.

As to whether the results presented by B&I provide conclusive evidence that ILUC has been systematically overestimated in the past, the question is not whether double cropping occurs, but whether it is more or less responsive to demand than land area and crop yields. This paper provides a basis to believe that cropping intensity may not have been adequately considered in the past, but it does not provide any numerically robust insights in to the causal link between increased demand and changed cropping intensity. Without further evidence that the overall magnitude of intensive effects has been underestimated compared to the overall magnitude of extensive effects, it is not possible to draw a strong conclusion.

A.3. Annex references

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