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A critique of lifecycle emissions modeling in "The greenhouse gas benefits of corn ethanol—assessing recent evidence"

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Introduction

In 2017, the U.S. Department of Agriculture (USDA) released a report titled, "A Life-Cycle Analysis of the Greenhouse Gas Emissions of Corn-Based Ethanol," authored by the consultancy ICF International (Flugge et al., 2017). This report (hereafter "the ICF report") reviewed the lifecycle analysis (LCA) results from the U.S. Environmental Protection Agency's (EPA) regulatory impact analysis (RIA) of corn ethanol for the Renewable Fuel Standard (RFS; U.S. EPA, 2010). The ICF report aimed to update the EPA's 2010 assessment using additional literature and provide revised estimates of the lifecycle emissions of corn ethanol. The ICF report concluded that the lifecycle GHG intensity of corn ethanol production in the United States was already 30% lower than the value predicted for 2022 by the RIA. On this basis, it was widely quoted in support of corn ethanol by industry supporters (e.g., American Coalition for Ethanol, 2017; Biofuels International, 2017).

Following the release of the ICF report, the consultancy Cerulogy conducted a critical review of its data and analysis, Malins (2017). While the Cerulogy review noted that the ICF report had identified relevant new pieces of evidence regarding the corn ethanol lifecycle, it also concluded that there were several fundamental data errors that invalidated parts of the analysis. These errors included conflating data from the preliminary and final regulatory impact analyses, misquoting data on fertilization rates, misquoting emissions results from the final RIA, and misunderstanding the relationship between the control and feedstock scenarios in the RIA's land use change assessment.

Additionally, the Cerulogy review found that the ICF report was not adequately critical of the evidence it used; it paid more attention to evidence that could suggest lower lifecycle emissions compared to the EPA's analysis than to evidence that could suggest the opposite. Due to these shortcomings, the Cerulogy review stated that the ICF report made unjustified adjustments to the EPA's corn ethanol LCA.

MORE RECENT PAPERS

In 2018, the USDA released an ostensibly updated report from ICF, Rosenfeld et al. (2018), but it contained largely similar analysis to that detailed in the 2017 ICF report. Then, in 2019, an academic paper on the same subject by the same authors as the 2018 paper—though cited in a different order as Lewandrowski et al. (2019)—was published in the Taylor & Francis journal *Biofuels*. Jan Lewandrowski is identified in the article as affiliated with the USDA, and all of the other authors, including Jeffrey Rosenfeld, are identified as affiliated with ICF. The lifecycle results reported by Rosenfeld et al. (2018) and Lewandrowski et al. (2019) appear to be identical. Table 1 thus compares the results for each lifecycle stage reported by Lewandrowski et al. (2019), the ICF report (again, Flugge et al., 2017), and the original results from the EPA's 2010 RFS RIA. Where "no change" is indicated, the lifecycle emissions results are identical to at least three significant figures.

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Table 1. Estimated GHG emissions of corn ethanol production, in grams of carbon dioxide equivalent emissions per million British thermal units of energy supplied (gCO2e/MMBtu)

	EPA RIA (2010)	Flugge et al. (2017)	Lewandrowski et al. (2019)*	Change from Flugge to Lewandrowski	Difference between RIA and Lewandrowski
Domestic farm inputs	10,313	9,065	9,065	**No change**	-1,248
Domestic land use change	-4,000	-2,038	-2,038	**No change**	1,962
Domestic rice methane	-209	-4,034	-1,013	**3,021**	-804
Domestic livestock	-3,746	-2,463	-2,463	**No change**	1,283
International land use change	31,790	9,082	9,094	**12**	-22,696
International farm inputs	6,601	2,217	2,217	**No change**	-4,384
International rice methane	2,089	1,480	2,482	**1,002**	393
International livestock	3,458	3,894	3,894	**No change**	436
Fuel and feedstock transport	4,265	3,432	3,432	**No change**	-833
Fuel production	28,000	34,518	34,518	**No change**	6,518
Tailpipe	880	578	578	**No change**	-302
Total	**79,441**	**55,731**	**59,766**	**4,035**	**-19,675**

Colors in the far right column reflect the difference between the RIA value and the value presented in Lewandrowski et al. (2019). Red signifies lower emissions than the RIA, and green signifies higher emissions than the RIA.
Note that the RIA assessment was of expected emissions in 2022, while the assessments by Flugge et al. (2017) and Lewandrowski et al. (2019) reflect expected emissions in "a composite year representative of the mid-2010s."
*As reported in Appendix Table A2 of Lewandrowski et al. (2019).

It is apparent that between the ICF report and the 2019 academic paper, changes were made to only three lifecycle stages—the domestic and international rice methane emissions, and a small change in the international land use change result. Regarding the latter, there is an inconsistency between the tabulated international land use change result given in Lewandrowski et al. (2019), 9,094 gCO2e/mmBTU, and the number quoted in the text of the paper, 9,082 gCO2e/mmBTU. Given that the described methodology is the same for Lewandrowski et al. (2019) and the ICF report, the small change in reported international land use change emissions appears to be a transcription error in the table.

While the Lewandrowski et al. (2019) analysis addresses a range of issues relating to the corn ethanol lifecycle, the headline result that commentators have paid most attention to is that corn ethanol production has a significantly better emissions profile than the EPA concluded in the RFS RIA. The much lower estimated GHG emissions for corn ethanol in both the ICF report and Lewandrowski et al. (2019) as compared to the EPA RIA is driven by a large reduction in the estimation of indirect land use change (ILUC) emissions outside the United States, as shown in Table 1. Indeed, if the international ILUC emissions are ignored, the ICF report and Lewandrowski et al. (2019) find that corn ethanol's emissions in the mid-2010s are slightly higher than the EPA result.

In addition to a large reduction in international ILUC emissions, the ICF report and Lewandrowski et al. (2019) show significantly lower emissions from international farm inputs, and more modest reductions in emissions (or increased emissions credits) from domestic farm inputs, fuel and feedstock transport, and domestic rice methane. There are significantly higher estimated emissions from fuel production, and modestly higher emissions from domestic land use change and domestic livestock. Below, we consider specific changes made between these two analyses and revisit other aspects of them that appear problematic.

NOTE ON PERFORMING LCA

Before choosing a methodology for LCA, it is important to understand what question the analysis seeks to answer. In the case of an LCA of biofuel production, two different questions can lead to quite different answers:

1. What is the average of carbon dioxide equivalent emissions that are associated with the processes required to produce a given biofuel?
2. How does the total generation of carbon dioxide equivalent emissions across the global economy change when we increase consumption of a given biofuel through the use of policy measures?

The first question is generally answered through the use of attributional LCA. In attributional analysis, we identify each emission source and sink in the system and attribute it to a process. For example, emissions from the fuel required to run agricultural machinery to farm the amount of corn processed at a given ethanol distillery would be attributed to corn ethanol production. The second question is generally answered through consequential analysis. In consequential analysis, inquiry instead considers whether the overall production of corn increases to supply an ethanol plant; whether the overall production of other crops also changes; and what the net emissions changes are that are associated with those changes in the agricultural system. In some cases, attributional and consequential analyses may reach the same conclusions for some emissions sources. For example, as part of a consequential analysis of increased ethanol demand, it is often assumed that all of the ethanol plants required to produce that amount of ethanol are operating specifically because of that ethanol demand. The result for that part of the lifecycle would therefore be the same as in an attributional analysis.

In its 2010 RFS RIA, the EPA, guided by the requirements of the Energy Independence and Security Act, determined that a consequential analysis was appropriate in assessing the GHG intensity of corn ethanol production and the production of other biofuels. In the ICF report and Lewandrowski et al. (2019), as will be discussed in detail below, elements of attributional LCA have been mixed into the reassessment of the corn ethanol lifecycle.

NOTE ON UNITS

Indirect land use change estimates in U.S. literature may be reported using two different sets of units. In the EPA's work for the 2010 RFS RIA, the unit generally used for GHG emissions intensities is gCO_2e/mmBTU. In the California Low Carbon Fuel Standard and in most of the academic literature, SI units are used instead, and GHG emissions intensities are reported in grams of carbon dioxide equivalent emissions per million joules (gCO_2e/MJ). These units are interchangeable with the appropriate conversion factor. There are 1,055 MJ in 1 mmBTU, and so emissions intensities given in gCO_2e/mmBTU may be converted to gCO_2e/MJ by dividing by 1,055.

Review of the Lewandrowski et al. (2019) analysis

As noted above, only two lifecycle stages in Lewandrowski et al. (2019) show results different from those in the ICF report—domestic and international rice methane emissions. For all other categories, the discussion from Malins (2017), which remains applicable, is the starting point for this review.

One exception is the issue of co-product crediting, where Malins (2017) was incorrect. Malins (2017) asserted that, "The ICF report appears to double count the emissions benefits associated with the production of ethanol co-products" (p. 39). This was based on the understanding that the ICF report had taken data on increases in corn acreage from the EPA's RFS RIA, and that the net land requirement reported by the EPA was already adjusted to account for the fact that availability of co-products reduces net land demand. The methodology is explained further by Lewandrowski et al. (2019), and it is now clear that the domestic farm inputs analysis was effectively independent of the EPA's RFS RIA modeling outcomes. Therefore, co-products were not double counted.

INTERNATIONAL LAND USE CHANGE

International ILUC emissions are the main source of difference between Lewandrowski et al. (2019) and the EPA's LCA conclusions from the 2010 RFS RIA. As noted above, there was no change in the approach between the ICF report and Lewandrowski et al. (2019). The 2010 RFS RIA results are based on modeling using the Food and Agriculture Policy Research Institute (FAPRI) partial equilibrium economic model coupled to emissions factors developed by the Winrock International Institute for Agricultural Development. In contrast, the Lewandrowski et al. (2019) results are based on averaging seven cases of results produced with versions of the Global Trade Analysis Project (GTAP) model. The seven cases are:

- Analysis by the California Air Resources Board (CARB, 2014) that was used for regulatory ILUC values in the California Low Carbon Fuel Standard
- Two versions of analysis using the GTAP-BIO model, as included in the Carbon Calculator for Land Use Change from Biofuels Production (CCLUB) module of the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation (GREET) LCA tool (Dunn et al., 2014):
  - Results using Winrock emissions factors
  - Results using Woods Hole Research Center emissions factors
- Two versions of analysis presented in Taheripour and Tyner (2013):
  - Results using the CARB agro-ecological zone emissions factor model (AEZ-EF)
  - Results using Winrock emissions factors
- Two versions of analysis based on applying ex-post adjustments based on Babcock and Iqbal (2014) to Taheripour and Tyner (2013):
  - Results using CARB AEZ-EF emissions factors
  - Results using Winrock emissions factors

In assessing whether the use of these modeling results is justified, we consider the following issues:

- Is adequate justification provided for adopting the GTAP-BIO model in preference to the FAPRI model?
- Is the decision to take an average across the seven cases justifiable?
- Is the decision to apply ex-post adjustments to results from Taheripour and Tyner (2013) based on Babcock and Iqbal (2014) justifiable?

Below, we argue that none of these three choices is well supported, and therefore it is not clear that the international land use change result reported by Lewandrowski et al. (2019) is an improvement over the value estimated in the EPA's 2010 RFS RIA.

FAPRI versus GTAP-BIO

The decision to use GTAP-BIO ILUC modeling results instead of FAPRI ILUC modeling results is justified very briefly in Lewandrowski et al. (2019) as follows:

> Relative to the FAPRI-CARD model used in the RIA and the GTAP model used in CARB [29], the 2013 GTAP-Bio model has several upgrades that make it better suited to analyzing the iLUC impacts related to increases in U.S. corn ethanol production. First, its base period is 2004. Hence, all simulations are relative to the year before implementation of the RFS. Second, the model includes region-specific land transformation elasticities developed from two United Nations Food and Agriculture Organization (FAO) landcover datasets. Finally, the model explicitly accounts for the higher cost of converting forest to cropland relative to the cost of converting grassland. (p. 7–8)

None of the differences between GTAP and FAPRI identified here are clearly advantages. First, FAPRI does not have a single base year because it is not built on a single global database in the same way that GTAP is. The data used in the FAPRI model for the 2010 RFS RIA would have included data more recent than 2004. Its data sources included the FO Lichts Database, FAOstat, and the USDA Production, Supply and Distribution View (CARD, 2009), and these sources all would have had data from more recent years than 2004 available at the time of the modeling.

Second, the region-specific land transformation elasticities between cropland, forest, and pasture for GTAP-BIO, as documented in Taheripour and Tyner (2013), are arbitrary values informed by basic analysis of the FAO data, with no direct empirical basis or detailed justification provided for the specific values adopted. There is an asymmetry in the changes made to the elasticities, with a factor 10 reduction applied for regions characterized as having "very low" land cover change but an increase of only 50% applied for regions described as having "very high" land cover change. The analysis assumes that historical changes in overall harvested area in each region can be used as a proxy for the likelihood that new land will be brought into production in the case of new demand. The analysis does not attempt to control for other factors that may have affected harvested area change in the period, and no additional analysis is provided to justify the use of this proxy measure. Also, land transformation elasticity is reduced in more regions than it is increased, which has the effect of suppressing the overall land use change response. No justification is presented for this decision. Golub and Hertel (2012) provide an alternative basis for regionalizing the land transformation elasticities proposed and present completely different values.

The third point identified in favor of GTAP is "explicit" accounting for the higher cost of converting forest to cropland than grassland to cropland. In the EPA's 2010 RFS RIA, the split between grassland and forest conversion in each region is based on analysis of historical trends. This approach implicitly includes conversion costs, as these costs would have factored into historical decisions to convert different land types. Meanwhile, the revision of GTAP-BIO by Taheripour and Tyner (2013) does not explicitly consider conversion cost (as would be done in some partial equilibrium models, such as the Global Biosphere Management Model [GLOBIOM]), but rather adds an arbitrary assumption that reduces the amount of forest conversion and increases the amount of grassland (pasture) conversion that the model estimates.

These issues, as well as other criticisms of the latest GTAP-BIO modeling approach, are discussed in more detail in Malins (2019a). Further, it is worth noting that a significant shortcoming in the GTAP-BIO modeling system is that it does not model the case of non-commercial land being put into productive use. When GTAP-BIO models forest conversion, it is treated as the conversion of a managed forestry system producing timber. This contrasts with other models, including MIRAGE and GLOBIOM, that explicitly allow conversion of areas of forest that are not being commercially exploited. This is a major shortcoming when considering land use change in countries such as Brazil or Indonesia, where there is significant conversion of unmanaged forest to agricultural use.

The EPA's 2010 RFS RIA included a 111-page peer-review report (ICF International, 2009) that discussed the comparative advantages of and differences between possible land use change modeling frameworks. In contrast, Lewandrowski et al. (2019) do not present a substantive comparison of the advantages of the two modeling frameworks. The peer-review report for the EPA states 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" (p. I-6). These comments are not addressed in Lewandrowski et al. (2019).

Averaging results across seven cases

Lewandrowski et al. (2019) average predicted land use change emissions outcomes across seven cases, all using the GTAP model. One of these cases is the regulatory analysis by CARB, and the other six are variations based on the model version documented in Taheripour and Tyner (2013). While the regulatory analysis by CARB was accompanied by an extensive program of stakeholder consultation, none of the other six cases were similarly accompanied. The CARB result itself is actually an average across 30 scenarios considered. Lewandrowski et al. (2019) take that single average of 30 scenarios and weigh it equally with each one of the other 6 GTAP cases based on land use change results from Taheripour and Tyner (2013). The CARB emissions result is higher than any of the GTAP-BIO cases considered, and so the decision to average only a single CARB estimate with the six based on Taheripour and Tyner (2013) results in a considerably lower emissions result than would have resulted from either treating the two underlying model configurations equally or treating each one of the 30 CARB scenarios equally with the 6 other cases. These alternative approaches would have resulted in ILUC emissions 50% or 100% higher, respectively, than the estimate in Lewandrowski et al. (2019).

Adjustments based on Babcock and Iqbal (2014)

In two of the GTAP cases discussed above, Lewandrowski et al. (2019) applied ex-post adjustments to the Taheripour and Tyner (2013) results. The adjustments were based on results in Babcock and Iqbal (2014), which focuses on the question of increases in harvest area at the intensive margin. When agricultural statistics show an increase in harvested area, this may include an increase in the area of land that is harvested more than once during a year or a reduction in the rate of crop failure (intensive land use increase), as well as an increase in the total area of land under cultivation (extensive land use increase). Babcock and Iqbal (2014) assert that much of the historical increase in harvested area in some regions is due to an increase in harvesting frequency, rather than an increase in total land area under cultivation. For two of the cases considered by Lewandrowski et al. (2019), the land use change emissions estimates from Taheripour and Tyner (2013) are reduced by assuming that some fraction of the international land use change predicted represents intensive land use increases rather than extensive land use increases.

As discussed in more detail in Malins (2017), there are numerous issues with the strong reliance placed on results from Babcock and Iqbal (2014). First, even though Babcock and Iqbal (2014) is not a peer-reviewed paper, results from it are quoted by Lewandrowski et al. (2019) without critique. For instance, it is stated that, "Babcock and Iqbal show most of [increases in commodity production 2004–12] were achieved by farmers using existing cropland more intensely rather than by bringing new land into production" (p. 8). It is, however, not clear that this result is successfully demonstrated.

A second issue is that the basic analytic tool used in Babcock and Iqbal (2014) has fundamental limitations. The main approach is to use FAOstat data to compare the net change in harvested areas to the net change in total reported arable land and permanent crops; this is in order to estimate the area that is cropped more than once a year. The comparison involves differencing two different datasets that may not be comparable. The potential for misleading results is confirmed when Babcock and Iqbal (2014) consider an alternative data source for U.S. planted areas, USDA National Agricultural Statistics Service data, and the result is completely different from that derived only from FAOstat data. Such a large discrepancy between sources for the United States, where one might expect relatively high data quality, suggests that there may also be large data discrepancies for other countries. This introduces a substantial uncertainty into the Babcock and Iqbal (2014) results.

A third problem is that land abandonment is not addressed in the data. In some regions, significant areas of land are abandoned due to degradation at the same time that agriculture is expanding elsewhere. For example, in the Brazilian State of Mato Grosso, more than 1 million hectares of land were abandoned between 2006 and 2011, during which period net total planted area increased by 1.9 million hectares (Spera et al., 2014). The gross expansion in farmed area is therefore at least 50% larger than the net. Ignoring this difference between net and gross extensive change significantly understates the role of extensive expansion in meeting growing demand for agricultural commodities.

A similar issue is created by summing planted area changes across large regions or groups of regions. For example, if area shrinkage in the European Union is offset by expansion elsewhere, then the magnitude of land use changes at the extensive margin is masked in the net results. Reductions in planted area in some regions do not demonstrate that total planted area is not responsive to demand in those regions. In fact, reductions in planted area in regions where the economics for farming are less favorable could be evidence of a strong planted area response to demand changes. Considering only net planted area changes across large regions may therefore give a very misleading view of the true responsiveness of planted areas to price changes.

There are further issues related to the individual regions discussed in the Babcock and Iqbal (2014) analysis. In Brazil, Babcock and Iqbal (2014) only use data for the period 2004–2012 and conclude that 76% of harvested area increase in Brazil was at the intensive margin (i.e., from multiple cropping). Using data for the full period for which Brazil data is available (2003–2015) would suggest instead that only 35% of harvested area increase occurred at the intensive margin. This, in turn, suggests that the period chosen for analysis by the authors may have led to an exaggerated conclusion.

The conclusions for China in Babcock and Iqbal (2014) are based not directly on data but on a narrative argument that economic forces in China were reducing agricultural land area in the mid-2000s and 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" (p. 9). However, the drivers of these reductions in agricultural area were ecological restoration and urban expansion, rather than agricultural economics, and it is not indicated in the study whether losses of agricultural land for these reasons were compensated by agricultural expansion elsewhere.

The adjustment for Sub-Saharan Africa is based on a claim in Babcock and Iqbal (2014) that "higher world prices were not transmitted to growers in many African countries" (p. 16) and therefore that extensive area increase was not likely to be demand driven. This is not well supported by the literature. In fact, one source referenced by Babcock and Iqbal (2014) found that during 2007–08, the average price transmission in Sub-Saharan Africa was 71% of the world price change and that changes in Sub-Saharan African corn prices were actually larger in percentage terms than world price changes (Minot, 2011).

Lastly, Babcock and Iqbal (2014) assume that multiple cropping dominated the harvested rice area increase in Indonesia. This claim is not directly supported by data, and is instead built on a narrative argument that Indonesia is densely populated and thus large increases in planted area are unlikely. This argument is not consistent with the well documented pattern of land expansion on the Indonesian forest frontier (Malins, 2019b).

Beyond data and analytical methodology, it is important to understand that Babcock and Iqbal (2014) only assess historical trends. No analysis is presented that could prove a causal link between crop demand or crop prices and the adoption of multiple cropping practices. Increased multiple cropping may be largely a result of improved techniques and knowledge dissemination, rather than a response to demand for agricultural commodities.

Finally, it is important to note that Babcock, Gurgel, and Stowers (2011) previously argued that the role of increased cropping intensity was already implicitly included in GTAP-BIO through the choice of a higher elasticity of yield to price: "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 [*sic*] 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" (p. 5). Taheripour and Tyner (2013) therefore arguably already implicitly include multiple cropping. The decision in Lewandrowski et al. (2019) to adjust the results from this analysis to further reflect multiple cropping may thus double count the effect of this phenomenon and underestimate international ILUC.

DOMESTIC LAND USE CHANGE

As with the international land use change analysis, Lewandrowski et al. (2019) choose to use results from the GTAP-BIO model for domestic land use change rather than follow the EPA's model choice, in this case the Forest and Agricultural Sector Optimization Model (FASOM). Also, there was no change in the approach between the ICF report and Lewandrowski et al. (2019), and again the discussion builds on the issues already noted in Malins (2017).

With regard to the domestic modeling, no detailed justification is provided for favoring the GTAP-BIO results. The most interesting aspect of the GTAP-BIO results that Lewandrowski et al. (2019) use is that there is "net sequestration associated with all ethanol-related ILUC" (p. 4)—i.e., the modeling shows that conversion of "cropland pasture" to corn production results in an increase, rather than a decrease, in carbon sequestration. This result is surprising, as almost all direct studies of carbon stock changes from grassland conversion to cropland, and other emissions factor models such as CARB's AEZ-EF, expect carbon losses, not increases, with this land conversion (Plevin et al., 2014; Searle & Malins, 2016). Lewandrowski et al. (2019) attribute this result in the CCLUB emissions factor model to "root growth deeper in the soil profile that more than offsets CO2 emissions due to oxidation of carbon near the surface" (p. 4).

The CCLUB documentation (Dunn et al., 2014) suggests a different reason for this result. The CCLUB emissions factors are based on modeling of soil carbon changes using the Century/Cole model. By definition, cropland pasture is a type of land that could be cropped relatively easily and may have been cropped in the past, but has been in a grassland state for several years (USDA Economic Research Service, 2019). However, the modeling for CCLUB considers soil carbon change on a piece of land that was in a grassland state *only until 1976*, has been row-cropped since, and which has been turned over to a low- or no-till corn crop. What the modeling actually shows is an increase in soil carbon sequestration due to a management change on a piece of land that has been continuously farmed for several decades. This method does not reflect the emissions that would be expected in the conversion of true cropland to pasture. The impact of this modeling choice is magnified by another choice to calibrate the GTAP-BIO model so that cropland-to-pasture conversion is the dominant domestic land response.

Had Lewandrowski et al. (2019) applied the less problematic CARB AEZ-EF model to the land use changes considered, domestic land use change would have been identified as a net emissions source, rather than a sink.

DOMESTIC FARM INPUTS

The Lewandrowski et al. (2019) result for domestic farm inputs is unchanged from the ICF report. Recall from above that it has been clarified that an apparent double counting issue identified by Malins (2017) was instead a misunderstanding of the documentation. Still, other concerns identified by Malins (2017) are unresolved by Lewandrowski et al. (2019).

In reviewing domestic farm inputs, it is important to understand how the methodology adopted differs from the EPA's approach in the 2010 RFS RIA. In the RFS RIA, the assessment of changes in domestic farm input emissions is integrated into the FASOM modeling. Changes in assumed emissions reflect increases and reductions in area devoted to each cropping system given typical input use for those crops and any assumed change in intensity of input use in order to increase yields. This is a consequential approach to farm input emissions, and reflects modeled net changes in the production of all crops. In contrast, an attributional approach, as used in the GREET model for the California Low Carbon Fuel Standard, identifies the gross demand for the feedstock crop and calculates the average emissions associated with meeting that demand.

The ICF report documents the following steps in calculating domestic farm inputs:

1. The net change in corn production associated with the RFS is taken from the FASOM modeling for the 2010 RFS RIA.
2. The area required to produce this amount of corn at average current yields is calculated.

3. It is assumed that this corn production is geographically distributed in the United States proportionately to current production statistics, and assumed area of corn for ethanol is identified in each of eight agricultural regions. Just under half is assumed to occur in the Corn Belt, a quarter in the Northern Plains, and a sixth in the Lake States.
4. Using USDA data on average input rates and emissions factors from GREET, SimaPro, and, for nitrous oxide emissions, the Intergovernmental Panel on Climate Change, the total emissions associated with this amount of corn production are calculated.
5. This emissions value is then divided by the total assumed net area change to give the per-hectare average emissions for additional corn production.
6. This per-hectare value is combined with average corn farming and ethanol production yields to give a result for emissions per liter of produced ethanol.

This is an attributional approach, as the calculated emissions relate to the gross increase in corn demand due to ethanol production. It is therefore methodologically distinct from the EPA approach. While the analysis in the ICF report is documented as starting from the net corn demand change reported by the 2010 RFS RIA, the averaging step—the fifth step in the list above—means that this value does not affect the result, because it cancels out between the numerator and denominator of the equation. After calculating emissions for the 773,956,000 bushels specified by the 2010 RFS RIA, equivalently referenced as 19.66 million tonnes by Lewandrowski et al. (2019), the ICF report averages these emissions across the same 773,956,000 bushels. The same result would be returned regardless of the assumed net corn demand change in step 1—emissions for a one bushel net change would be divided across 1 bushel, emissions for a 2 billion bushel net change would be averaged across 2 billion bushels, etc.

This feature of the analysis was clarified by Rosenfeld et al. (2018) and Lewandrowski et al. (2019) and it was not understood by Malins (2017) when drawing the incorrect conclusion that the co-product credit appeared to have been double counted. The net corn demand change value already takes account of yield effects, consumption effects, and the return of co-products to the marketplace. Therefore, if domestic farm input emissions were calculated based on this net corn demand change, it would be unnecessary to also include a co-product emissions credit, and would represent double counting. However, when using the attributional approach based on gross demand change, it is indeed appropriate to apply a co-product credit. While the approach in the ICF report is methodologically consistent (albeit that the reference to the net corn demand change from the 2010 RFS RIA is redundant), there is no justification given for moving from the consequential approach adopted in the RFS RIA to an attributional approach.

One further issue, which was identified by Malins (2017), is that the ICF report referred to the 2010 RFS RIA "control" and "reference" scenarios as the basis for the net corn demand change value used in the first step of the calculation. This would be wrong. The correct comparison would involve the "control" and "corn only" scenarios from the RFS RIA, because the reference scenario includes area changes associated with other feedstocks. However, Rosenfeld et al. (2018) revise the description, and specify that, "ICF used the RIA's projected number of additional bushels of corn in the Control case compared to the Corn Only (773,956,000 bushels in 2017) to determine the additional number of corn acres that can be attributed to the RFS2" (p. 18). But referring back to the published results of the FASOM analysis (Beach & McCarl, 2010), it is apparent that the ICF report number reflects the difference in corn demand between the "control" and "reference" scenarios. The incorrect use of the data is therefore now compounded by an inaccurate description of its use. Still, as noted above, the value taken for net corn demand does not affect the result.

Finally, and as noted by Malins (2017), the ICF report appears to confuse statistics on total fertilizer application rates with nitrogen fertilizer application rates, potentially resulting in incorrect conclusions relating to nitrogen fertilizer use specifically. More broadly, the ICF report and Lewandrowski et. al (2019) appear to overstate the role of techniques such as precision fertilization in reducing per-bushel fertilizer application.

INTERNATIONAL FARM INPUTS

The result for international farm inputs in Lewandrowski et al. (2019) is unchanged from the ICF report. The EPA's 2010 RFS RIA includes assessment of changes in international farm inputs based on the FAPRI model results for changes in farmed area. Lewandrowski et al. (2019) recalculate emissions from international farm inputs based on instead considering land use changes estimated for one of the GTAP-BIO scenarios included in CCLUB. By doing this, Lewandrowski et al. (2019) find a significant reduction compared to the RFS RIA, largely associated with the reduction in international land use change due to changing models. But the inclusion of emissions from international farm inputs reflects a consequential LCA approach. In attributional LCA, such as used in the California Low Carbon

Fuel Standard, such changes would normally be ignored. Given that the assessment of domestic farm inputs was moved to a fully attributional basis in Lewandrowski et al. (2019), the inclusion of this term in the calculation is inconsistent.

RICE METHANE

Rice production results in methane emissions. If rice area shrinks and is replaced by cultivation of corn or other crops, there may be a reduction in net methane emissions. Conversely, if rice area increases, net methane emissions may increase. The ICF report's rice methane assessment is based on the rice area change numbers from the EPA's 2010 RFS RIA and updated emissions factors. Although the ICF report showed a large net credit to corn ethanol from rice methane reductions, Malins (2017) showed that the rice methane analysis included methodological errors and used the wrong spreadsheets from the RFS RIA.

Lewandrowski et al. (2019) updated this analyis and it resulted in a net deficit rather than a net credit. This had the effect of increasing the emissions estimated for corn ethanol. A small credit for reduced domestic rice methane emissions is more than offset by an increase in predicted rice methane emissions outside the United States, and this result is similar to the EPA's 2010 RFS RIA. Note that Lewandrowski et al.'s (2019) calculation was based on the land use change results documented in the RIA, rather than on the land use changes predicted by GTAP-BIO, as was done for international farm inputs. This introduces a further methodological inconsistency.

Conclusions

The corn ethanol lifecycle results in Lewandrowski et al. (2019) are presented as an evolution from the results in the EPA's 2010 RFS RIA, based on newer and better data and analysis. However, the above analysis showed that the significant reduction in the overall estimated GHG intensity for corn ethanol in Lewandrowski et al. (2019) is based almost entirely on changes to the international land use change methodology that are poorly supported and implemented. This and other issues discussed are summarized in Table 2.

Table 2. Changes from the EPA's 2010 RFS RIA to Lewandrowski et al. (2019) and the issues discussed in this study

Emissions source	Emissions gCO2e/MMBtu			Comments
	EPA RIA (2010)	Lewandrowski et al. (2019)	Difference	
International land use change	31,790	9,094	-22,696	The decision to replace FAPRI results with GTAP-BIO results is not well justified. Results from Taheripour and Tyner (2013) are given six times more weight than the regulatory assessment for the California Low Carbon Fuel Standard. Adjustments that result in lower emissions estimates are both weakly justified and made based on inappropriate use of results from a single study, Babcock and Iqbal (2014).
Domestic land use change	-4,000	-2,038	1,962	The replacement of FASOM results with GTAP-BIO results is not well justified. The calculation of an emission credit is based largely on a negative emissions factor from CCLUB for cropland to pasture conversion that is not credible.
Domestic farm inputs	10,313	9,065	-1,248	The fully consequential assessment of the EPA's RIA is replaced with an attributional assessment without clear justification. Elements of the methodology are mis-documented, and the discussion around emissions due to nitrogen fertilizer use lacks balance.
International farm inputs	6,601	2,217	-4,384	Unlike domestic farm inputs, the consequential approach of the RIA is kept for international farm inputs. This creates a methodological inconsistency.
Global rice methane	1,880	1,469	-411	Unlike other lifecycle stages, methane emissions continue to be calculated based on predicted land use changes from the RIA, rather than from GTAP-BIO. This introduces another inconsistency in the methodology.

In particular:

1. The arguments in favor of adopting GTAP-BIO instead of the EPA's FAPRI modeling are brief and not well supported. The lack of a detailed case in favor of GTAP-BIO over FAPRI stands in marked contrast to the considerable attention paid to this question by the EPA in the 2010 RFS RIA. Moreover, the reasons given by the EPA for favoring the FAPRI modeling system over GTAP are ignored in Lewandrowski et al. (2019).

2. The result used for international land use change is an average across seven scenarios modeled with GTAP-BIO. Only one of these scenarios is taken from the extensive modeling work undertaken as part of the consultative regulatory process for the California Low Carbon Fuel Standard, itself an average across 30 modeled scenarios. The California result is the highest of the seven. The other six are based on a single paper. Averaging in this way dilutes consideration of the California analysis while giving six times greater weight to the other paper.

3. Two of the cases considered in the international land use change average reflect downwards adjustment based on results from a paper that discusses the role of cropping intensity in delivering increases in reported harvested area (Babcock & Iqbal, 2014). It is not clear that the data used in that paper are appropriate for the analysis presented, and Lewandrowski et al. (2019) pay inadequate attention to caveats given in Babcock and Iqbal (2014). Neither Lewandrowski et al. (2019) nor any analysis by ICF (Flugge et al., 2017, Rosenfeld et al., 2018) convincingly shows that cropping intensity increases are strongly related to commodity demand, rather than driven by independent technical developments.

Outside of the international land use change analysis, the differences between the results in Lewandrowski et al. (2019) and the 2010 RFS RIA are smaller. There are, however, other issues with the analysis that were identified by Malins (2017). In some cases, these were reiterated above, including incorrect documentation of some details, a lack of balance in the treatment of evidence, and a series of methodological inconsistencies.

The LCA by the EPA for the 2010 RFS RIA was wide-ranging and innovative. It involved a large team of researchers and extensive public consultation. That analysis is detailed over hundreds of pages in the RIA itself, and is complemented by a large number of supporting documents, model descriptions, peer reviews, public comments, and spreadsheets of results that remain available on the regulations.gov website. In due course, this analysis will be superseded by new analysis that integrates new information and better-developed models with the same level of rigor and ambition that the EPA brought originally. The work documented in Lewandrowski et al. (2019) does not meet that standard. While purporting to bring new information to bear on the analysis of the corn ethanol LCA, in truth Lewandrowski et al. (2019) simply brings different information and tools. In some cases, it relies on models and methodological choices that were explicitly rejected for the 2010 RFS RIA. It is not convincingly argued in Lewandrowski et al. (2019) that its adjusted approach is an improvement over the EPA approach. This paper and the earlier critique by Malins (2017) highlight many problems that undermine the validity of the results.

Despite the resolution of some specific analytical errors relating to rice methane emissions, the conclusion from Malins (2017) that, "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," (p. 3) is as true of the academic paper Lewandrowski et al. (2019) as it was of the ICF report that preceded it.

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