Response to comments from Lark et al. regarding Taheripour et al. March 2022 comments on Lark et. al. original PNAS paper

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1. Introduction

We recently reviewed the article published by Lark et al. (2022) in *PNAS*, detected various problematic assumptions, approaches, data, and results in that study. Based on our findings, we concluded that these authors overestimated GHG emissions of corn ethanol consumption due to the RFS. In response to our comments, Lark et al. have stated that they believe *"Taheripour et al.'s conclusions to be unsupported and based upon several misunderstandings and misinterpretations of [our] methods and results."* In what follows, we review the responses provided by Lark et al. and show that our comments did not misinterpret Lark et al.'s findings, rather that our comments are supported by the literature and actual observation and are based on the statements provided by Lark et al. in their original publication.

In this document, we provide responses to the Lark et al. comments one by one. To avoid confusion, throughout we refer to Lark et al.'s 2022 original publication as Lark et al.(a) and the response of those authors to our original comments as Lark et al.(b).

Our detailed review of the original paper and the responses by those authors reveals various major deficiencies, problematic assessments, and misinterpretation of the existing literature in Lark et al.(a)&(b). In summary, our major findings are:

- Lark et al.(b) admitted that their "*results reflect the impacts of corn ethanol demand in general, regardless of the source of such increases*". Hence, the title of Lark et al. (a) should not be *"Environmental outcomes of the US Renewable Fuel Standard"*, as many factors affected the expansion in demand for corn ethanol.

- Table S22 of the Lark et al.(a) SI shows that almost all of the estimated regional transitions between cropland and pasture land are statistically insignificant. Hence, they build their analyses on the statistically insignificant assessments of land transitions. Lark et al.(a) have not revealed their estimated equations and their parameters at the NRI data point level.

- Lark et al.(a)'s treatment of soil organic carbon (SOC) and reporting of its uncertainty appear to be based on a misunderstanding of the information extracted from other studies, including their inaccurate use of the carbon response functions (CRFs) derived from Poeplau et al. (2011) and overestimation of the SOC sequestration potential in CRP lands.

- Our original comment noted that Lark et al.(a) double counted N_2O emissions. In their response they argued otherwise without directly addressing the double counting issue we raised. We explained again in this document why they did double counting.

- We questioned the inconsistency between their price evaluation period (2006-10) and land use change assessment period (8 years from 2008 to 2016). In response Lark et al. (b) reveled that "*our estimated price effects would be somewhat larger if we used all years up to 2016".* We explained that selecting inconsistent time segments to avoid higher price effects is not an acceptable scientific choice**.**

- Lark et al.(b) in various instances selected and interpreted the existing literature in favor of their analyses, while the literature clearly shows otherwise. For example, the authors referred to price analyses developed by Irwin and Good (2013) to support their incorrect assumption that DDGS is only a substitute for corn. However, Irwing and Good (2013) concluded that the price of DDGS reflects the value of corn and soybean meal.

- In our original comments we highlighted that Lark et al.(a) ignored market mediated responses (e.g., yield improvements). In response Lark et al.(b) argued that: "*We carefully consider and account for both yield increases and DDG offsets"*. We showed that they implemented close to zero yield improvements, ignored demand responses, and incorrectly specified DDGS offsets.

- Lark et al.(b) argued they followed the approached used by Carter et al. and hence their results are valid. We explained that relying on Carter et al. (2017) is a problematic choice due to various deficiencies.

- In our original comments we noted that Lark et.(a) missed various important land transitions that frequently occur within cropland (including but not limited to return of cropland pasture to crop production) and hence they overestimated the land use impacts of corn ethanol. In response, Lark et al.(b) admitted they intentionally missed "cropland pasture" and stated that their approach "*purposely avoids the separate problematic category of Cropland-Pasture".* In response, we explained that, unlike the claim made by Lark et al.(b), cropland pasture is a standard land category recognized by the FAO, IPCC, and USDA Agricultural Censuses. We provided detailed information about this land category and its magnitude and changes over time. We then showed that, while the NRI data implicitly includes this type of land, Lark et al. made no effort to capture its changes over time. Instead, they incorrectly assigned changes in the CRP land to ethanol.

- Finally, we would like to note that our original comments included several comments that Lark et al.(b) did not respond to, including our remote sensing reanalysis revealing false land use change classifications in their original study.

- In conclusion, we find that the Lark et al.(a) paper is more problematic than what we initially evaluated to be the case.

2. The use of CDL and NRI data

In our original comment, we highlighted the hazards involved in determining land types using CDL data and explained that the use of this data layer by Lark et al.(a) leads to overestimation of GHG emissions from ethanol.

In response to our original comment on this topic Lark et al.(b) stated that they used NRI, not CDL, to estimate the types, amount, and regional location of land conversion and that conversion of pastureland and CRP land to cropland generates substantial carbon debt. In addition, Lark et al.(b) stated that we conflated their methods for estimating water quality impacts (Lark et al.(a) SI ln 696) with their methods for identifying land transitions.

Our responses follow.

i. Using both CDL and NRI data

In our original response, we did not dispute Lark et al.(a)'s use of NRI data to calculate land conversion at data points but pointed out the issues caused by the use of CDL data to determine the location and characteristics of converted land at a high-resolution scale. Our original comment outlined the consequences of this use.

In addition, using both CDL and NRI data is a questionable practice, as these two data sets follow different definitions, protocols, and approaches. To highlight these differences, we clarify below a few items related to "pastureland" and "CRP land," the two land classes examined by Lark et al.(a).

The NRI data set presents "pastureland" as a class of land under that title with the following definition:

"*A land cover/use category of land managed primarily for the production of introduced forage plants for livestock grazing. Pastureland cover may consist of a single species in a pure stand, a grass mixture, or a grass-legume mixture. Management usually consists of cultural treatments: fertilization, weed control, reseeding, renovation, and control of grazing. For the NRI, [this] includes land that has a vegetative cover of grasses, legumes, and/or forbs, regardless of whether or not it is being grazed by livestock."* (USDA, 2020)

This definition clearly suggests that the land category of "pastureland" in NRI data represents primarily managed land for producing forage plants, while some natural vegetation could be included as well. The NRI area of this land category has been around 120 million acres since 2012.

On the other hand, the CDL data set represents a class of land labeled "**grassland/pasture"** with an area around 380 million acres since 2012, more than three times of pastureland in the NRI data set. This class of land in the CDL data set covers a wide range of land types that are not included in the NRI data set which basically covers managed land used for forage production by definition. Mapping an estimated change in "pastureland" obtained from the NRI data at a data point to the CDL data at the grid cell level relies on problematic method and non-scientific judgments.

In the case of CRP land, the mapping between the NRI and CDL data sets is even more problematic, as the latter data set does not identify CRP land, and the mapping process is more arbitrary.

ii. Issues with CDL data

The comments above should make it clear that **we have not conflated** the Lark et al.(a) method in using CDL data to determine the location and characteristics of converted land at a high-resolution scale with their methods for estimating water quality impacts. In our original comment on this topic, we used the following quote from the Lark et al.(a) SI to describe the issues related to use of CDL data:

"*For the period 2008-17, we used the USDA-NASS Cropland Data Layer (CDL) and a look-up table to convert CDL land cover classes to vegetation types simulated by AgroIBIS*."

In their response, Lark et al.(b) argued that the above quote is taken from their SI, in which they explained their method of calculating water quality, and we had misrepresented it as their land transition method. To clarify, we used the above quote simply because it explicitly noted the use of CDL data, although Lark et al.(a) noted the use of CDL data indirectly in a few other places. For example, in their main manuscript under the title of **Cropland Area Change** they noted the following:

"[T]he high-resolution field data (37) were used only to identify the possible locations and characteristics of converted land, whereas the data from the NRI were used to estimate the magnitude of conversion and how much of it could be attributed to the RFS.

Reference 37 in their paper refers to CDL data.

iii. "Pastureland" definition

Although by definition "pastureland" in the NRI data set represents primarily *managed* land producing forage for livestock feed, Lark et al.(a) referred to this class of land as *natural* land and argued that conversion of this type of land to cropland results in "*substantial carbon debt."* Following are some observations that do not support their claim.

Figure 1 shows land transitions to and from the NRI pastureland category over time and indicates a land transition of 28 million acres to pastureland, mainly (71%) from cropland, between 1982 and 1997 at the national level. On the other hand, during the same time period, about 38.6 million acres of land *left* the pastureland category, mainly becoming cropland, CRP land, rangeland, forest or other land types. In the five years that followed (1997-2002), about 19 million acres of land (mainly cropland) moved to pastureland and about the same amount left this category of land.

The same pattern of land exchange can be seen in the next three periods, 2002-2007, 2007-2012, and 2012-2017, with around 10 million acres in and 10 million acres out for each 5-year segment. These large exchanges between pastureland and other types of land, in particular with cropland, confirm that farmers continuously rotate a portion of their managed land between cropland and pastureland to produce either primary crops or animal feed plants. The frequent rotations between cropland and managed pastureland should not be interpreted as conversion of natural land to cropland, as Lark et al.(a) misstated and misused in their analysis. It is also important to note that, at the national level, the total area of pastureland increased by 2.1 million acres between 2007 and 2012 and then decreased by 1.4 million acres in 2012-2017. This means that due to all drivers of land use (including biofuels) pasture land has increased, not decreased, between 2007 and 2017.

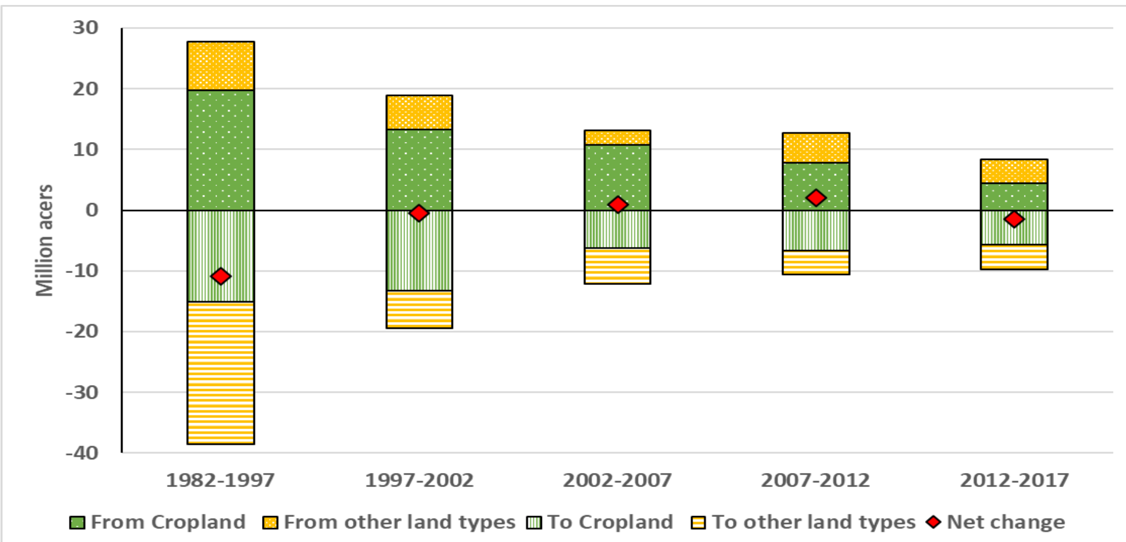

Figure 1. Land transitions to and from pasture land from 1982 to 2017. Based on USDA (2020).

Moreover, the NRI data set, like any other data set, is subject to various potential errors due to the sampling process, data collection, remote sensing, and processing data. Studies have addressed quality of this data set (e.g., Copenhaver et al., 2021); two examples of differences across various versions of this data set are shown in Table 1. As shown in this table, the 2012 and 2015 NRI data releases provide entirely different pictures of the land transition between cropland and pastureland.

Table 1. Land transition between pasture land and cropland in two different releases of NRI data.

Type of land conversion	NRI release 2012	NRI Release 2015	% Difference
Cropland to pasture 2007-2012	4,793.4	7,435.2	55.1
Pasture to cropland 2007-2012	4,585.5	6,368.6	38.9

Sources: USDA, 2012 National Resources Inventory, Summary Report, August 2015. USDA, 2015 National Resources Inventory, Summary Report, September 2018.

iv. Summary

In summary, we understand that Lark et al.(a) used NRI data to determine changes in pastureland and CRP land at data points. We merely point out the issues inherent in using CDL data at high resolution and note two crucial facts. First, the CDL data set determines land types with a large margin of error, and this can lead to overestimation of GHG emissions of ethanol. Second, the mapping between the estimated changes in "pastureland" and CRP land obtained from the NRI data at a given data point and the CDL data at the grid cell level can also lead to overestimation of GHG emissions of ethanol. Finally, we reviewed the NRI land category "pastureland," which mainly represents managed land with frequent exchanges with cropland. In other words, farmers frequently switch back and forth between the two categories of managed land: cropland and pastureland. When the managed land is used for production of forage and grasses for livestock, it goes into the category of pastureland, and when it is used for production of primary crops it goes into the cropland category. These transitions should not be interpreted as the conversion of natural land to cropland, as Lark et al.(a) appear to have done.

v. Remote sensing analysis of parcels asserted to have converted to cropland

Finally, in our original note we commented on **remote sensing analysis of parcels asserted to have converted to cropland.** In their response Lark et al.(b) did not comment on our analysis of their supporting geodatabase, as presented in their SI (titled US_land_conversion_2008-16.gdb.zip). As we described in our response, we accessed the layer "ytc" (described as "areas converted to crop production between 2008 and 2016" with the year of expansion listed in the polygon) in the ArcGIS software and opened the Lark et al.(a) "cropland expansion" layer into Google Earth Engine (GEE). Using the LandTrendr spatio-temporal curves of the Normalized Difference Vegetation Index (NDVI), we found that several of the parcels of land identified by Lark et al.(a) as "expansion to cropland" may often be short-term (less than 10 years) fallow/idle lands. In preparing these comments, we processed more fields that were identified by Lark et al.(a) as having converted to cropland during the study time frame (analysis courtesy of Ken Copenhaver, see Appendix A). We confirmed our original findings that many of the change classifications by Lark et al.(a) appear to be incorrect.

3. The carbon response and CRP land

i. Use of the carbon response functions (CRFs) derived from Poeplau et al. (2011)

In our original response to Lark et al.(a), we pointed out Lark et al.(a)'s potential overestimation of SOC changes upon grassland conversion, which one of the Lark co-authors already noted in his paper (Spawn et al. 2019). Subsequently, Lark et al.(b) provided more detail that had not been included in Lark et al.(a) or elsewhere:

"*Upon closer examination, numerous studies factored into the Poeplau et al.[study]. CRFs represent conversions of previously cultivated grasslands (ranging from just 4 to 100+ years as grassland) and many of these and others have also been intensively grazed or hayed. Furthermore, several of the included studies also represent grassland conversions to no-till cropping. As such, we believe the grassland CRFs of Poeplau et al. 2011 are, in fact, well suited to characterize the types of grassland conversions observed recently throughout the US."*

Even with a "closer examination," the data on SOC and the associated meta-data compiled in Poeplau et al. may still not be fully understood. For example, Poeplau et al. compiled a total of 176 observations from 45 studies related to grassland to cropland conversion (Table 1 of the Poeplau article) but 27% of the observations were extracted from two studies—Newton et al. (1945) and Campbell and Souster (1982). Interestingly, both studies were conducted in Canada, where mean annual temperatures (MAT) of soil are about 4°C and 3°C, respectively. Since the MAT is one of the variables considered in a specific CRF for grassland conversion, the CRF Lark et al.(a) used would be less accurate in higher MAT regions, such as Iowa (9°C), Illinois (11°C), and Nebraska (10°C) in the U.S. Midwest.

Furthermore, 52 observations, including Newton et al. (1945), were obtained from literature published before 1980, when the soil sampling and measuring methods used might be different from recent technologies. Lark et al.(a) would have found it useful to have examined the details behind the Poeplau et al. study before using the CRFs to characterize the grassland conversions observed recently throughout the US.

In addition, the grassland CRFs were applied to a soil depth up to 100 cm, which is much deeper than the 90% of confidence interval of soil depths from 15 to 38 cm covered by the dataset of grassland to cropland conversion (SI Table 2) in Popelau et al.

As a result, Lark et al.(a)'s application of CRFs may not accurately reflect the SOC changes associated with grassland conversion in more recent years and in the U.S. Midwest.

ii. Estimation of the SOC sequestration potential in CRP lands

As noted in our original response, **GTAP LUC modeling does not consider conversion of CRP lands, and thus related emissions factor (EF) models, such AEZ-EF and CCLUB, do not model emissions/sequestrations associated with the conversion**. Nonetheless, Lark et al.(b) continued to stress SOC losses from conversion of CRP lands and stated that "*field studies consistently show that CRP lands recover soil carbon to varying degrees during their contract period that can then be lost upon recultivation,"* citing Spawn-Lee et al. (2021).

In that article, the authors state that *"Field studies assessing SOC changes after recultivation of CRP lands consistently report either net emissions or indeterminant change [19, 27–31], with estimated SOC losses as high as 154 MgCO2e ha^{-1} when CRP land is converted to a corn-soy rotation managed with conventional tillage [29]. Conversion to no-till management results in lower but still substantial GHG costs [19]."*

When we looked into the references cited, we found out that the authors' statement on SOC potentials in CRP lands would have been skewed by the findings of four studies from the Kellogg Biological Station (KBS) Long-term Ecological Research (LTER) site in southwest Michigan (Table 2). Although KBS is an invaluable long-term field experimental site, the site soil is loamy soil with low SOC levels (~1.2%). The CRP-induced SOC accumulation observed from that site would not be representative of many CRPs in the US.

Table 2. Literature cited in Spawn-Lee et al. (2021) related to SOC sequestration potentials in CRP lands.

Literature	Title	Site location
[19] Ruan and Robertson G P 2013	Initial nitrous oxide, carbon dioxide, and methane costs of converting conservation reserve program grassland to row crops under no-till vs. conventional tillage	KBS-LTER
[27] Reeder J D, Schuman G E and Bowman R A 1998	Soil C and N changes on conservation reserve program lands in the central great plains	Wyoming
[28] Piñeiro G, Jobbágy E G, Baker J, Murray B C and Jackson R B 2009	Set-asides can be better climate investment than corn ethanol	Meta-analysis
[29] Gelfand I, Zenone T, Jasrotia P, Chen J, Hamilton S K and Robertson G P 2011	Carbon debt of conservation reserve program (CRP) grasslands converted to bioenergy production	KBS-LTER
[30] Zenone T, Gelfand I, Chen J, Hamilton S K and Robertson G P 2013	From set-aside grassland to annual and perennial cellulosic biofuel crops: effects of land use change on carbon balance	KBS-LTER
[31] Abraha M, Gelfand I, Hamilton S K, Chen J and Robertson G P 2019	Carbon debt of field-scale conservation reserve program grasslands converted to annual and perennial bioenergy crops	KBS-LTER

As we pointed out in our first response, **the literature/data on SOC of CRP lands is very sparse**. The USDA has recognized the need for more observational data from sampling, measuring, and monitoring soil carbon on CRP acres and has recently launched CRP Climate Change Mitigation Assessment Initiative projects where Lark will join as one of collaborators. We believe that Lark et al. will be able to provide more concrete data on SOC of CRP lands once more data is collected from that project.

iii. Cropland-pasture carbon emission factors (EF) in CCLUB

As Lark et al.(b) explained, the three sets of EFs—Woods Hole, Winrock, and AEZ EFs –simply assign half the value assumed for the conversion of “grassland/pasture” to the conversion of cropland-pasture land, which results in net SOC loss. This is different from the approach in CCLUB.

The authors did not mention some key differences between CCLUB and all other sets of EFs described in many of our published articles and reports. For instance, the AEZ-EF model (Plevin 2014) adopts a definition of cropland-pasture different from CCLUB’s, and all other sets of EFs consider only generic croplands instead of different types of croplands. Nor do the other sets consider the effects of land management practices and assumptions for the 20-30 years that follow land use change (LUC).

The SOC EFs calculated in CCLUB are based on the model’s inclusion of different tillage types and the assumption that spatially explicit (U.S. county-level) feedstock yield is either constant or increasing (Taheripour et al., 2021). Significantly, in that study we focused on converted croplands used mostly for annual food crops (e.g., corn, soy, and wheat) and aggregated the EFs from the counties where historical feedstock production data are reported in USDA surveys. To evaluate and calibrate our modeling results corresponding to specific feedstocks of interest, we conducted meta-analyses of published literature (Qin et al., 2016; Xu et al., 2019) and calibrated a CENTURY-based model with long-term experimental data (Kwon et al., 2017).

Lark et al. dismissed the “cropland pasture” category by stating that it is *“defined by economists as land that variously cycles between cultivation and perenniality.”* Cropland-pasture is a standard land category and is defined by USDA, FAO, and IPCC (see Taheripour et al., 2021, for more details). All three define “cropland-pasture” as “temporary pasture and meadows.” It is true that this land type (like CRP) has not been well documented for its carbon inventories. It should be noted, however, that if we want land classes to match carbon inventories, we would need many, more-detailed land classes instead of aggregating land classes currently available from land cover datasets (e.g., NLCD) for economic and emission modeling. Thus, researchers at the Argonne National Laboratory (ANL) have evaluated several scientific approaches (e.g., changing the frequency of switches between cropland and pasture phases that influences SOC levels) to model the current EFs for cropland-pasture conversion.

iv. Additional comments on CRP land and carbon sequestration

In our original comments we noted that:

“Lark et al. applied the CRFs that were based on conversion of “native” or undisturbed grassland to cropland. CRP land is likely to be less rich in soil carbon stocks than native or undisturbed grassland because it has been under vegetation cover for only a limited number of years.”

In response to this comment Lark et al.(b) argued that:

“[T]he minimum is 10-15 years (the length of a CRP contract) and this is often far exceeded if the land was enrolled for more than one contract cycle (e.g., 36% of all CRP land between 2013

and 2016 was enrolled for at least 2 contract cycles; Bigelow et al. 2020). Field studies consistently show that CRP lands recover soil carbon to varying degrees during their contract period that can then be lost upon recultivation, and, while direct measurements of CRP pre- and post-conversion are notably lacking and much needed, when conducted, they have found that emissions can be comparable to those observed following "natural" grassland conversions (see discussion and references in Spawn-Lee et al. 2021).

We question Lark et al. above characterization with the below specific points.

- According to the USDA:

  "The CRP is a Federal program established under the Food Security Act of 1985 to assist private landowners to convert highly erodible cropland to vegetative cover for 10 years. For NRI, only acres that have been enrolled in CRP general sign-up are included in the CRP land cover/use category. It does not include acres enrolled in CRP continuous sign-up." Source: USDA (2020).

  Therefore, the NRI data set provides an incomplete picture from the CRP land and represents only general signups in this program.
- Regarding the claim that "*36% **of all CRP** land between 2013 and 2016 was enrolled for at least 2 contract cycles"* we did not find such a finding or claim in Bigelow et al. (2020). Instead, the authors of this reference reported that *"36 percent (2.76 million acres) of the acreage in **expiring CRP** contracts during 2013-16 was reenrolled in the CRP"*. That is, Bigelow et al. (2020) referred to **the expiring CRP contacts in 2013-2016, not all CRP land in that time period**. Their results shows that 2.76 million acers of the expired CRP lands between 2013-2016 reenrolled again in the program (see Table 2 of the appendix of Bigelow et al., 2020). It seems that Lark et al. (b) misinterpreted the findings of the original authors.
- The extent to which CRP land sequesters carbon in a short time period is unknown and highly uncertain. The fact is that it could take decades, not years, to restore the carbon content of any disturbed land.
- Lark et al.(a) assigned a large emission factor to their over-estimated land conversion attributed to RFS, which resulted in significantly overestimated carbon implications for ethanol production. Figure 2 compares the emission factors implied by Lark et al.(a) with the emission factors provided by other sources. This figure shows that the emission factors assigned by Lark et al.(a) to each hectare of converted land (pastureland/CRP) are larger than those of other sources. For example, they are 80% higher than the emission factors for pastureland imbedded in the AEZ-EF model. Note that the pastureland emission factor of AEZ-EF mainly represents natural grass, while the land in Lark et al.(a) is mainly CRP land and primarily managed pastureland. It is also important to note that the emissions factors of the AEZ-EF model have not been updated with the recent IPCC tables, which provide lower emissions factors. The overestimated emission factors and overestimated land conversion in Lark et al.(a) led to overestimated ILUC emissions for corn ethanol.

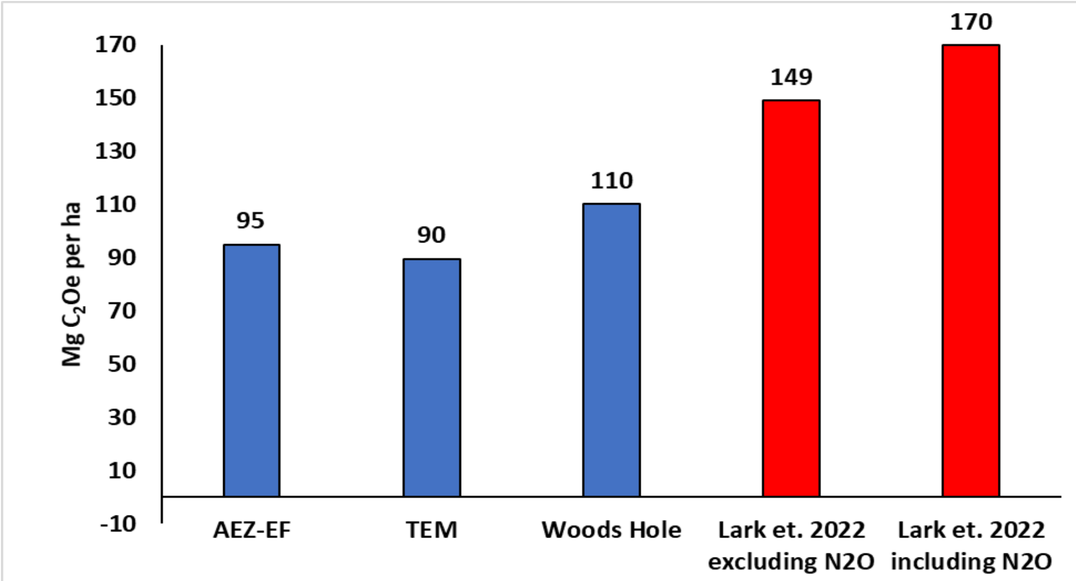

Figure 2. Pastureland emission factors among different data sources at the national level.

4. The issue of double-counting N_2O emissions

Lark et al. did not directly address the double counting issue we raised in our earlier comments. We reiterate the issue here again. As we stated in our original comments, the GREET corn ethanol LCA uses the USDA statistics for a given year for corn yield and N fertilizer application to derive national average N fertilizer use per bushel of corn of that year. That is, by using the USDA annual statistics, we include all corn acreage (existing and new) in a given year. If additional corn acreage results from RFS and uses additional N fertilizer, that is reflected in the USDA annual statistics.

Most corn ethanol LCA studies account for N_2O emissions from nitrogen fertilizers in cornfield by using emission factors, which assume linear/non-linear relationships between N_2O emissions and nitrogen inputs. This is the same approach employed in LCA studies and in Lark et al.(a). However, in their calculation of additional N_2O emissions they did not note that if there is any change in nitrogen applied to corn, the farming emissions would have already included the GHG impact of such a change, which is especially true for those LCA studies on total U.S. ethanol volume.

Thus, when Lark et al.(a) added their N_2O emissions to EPA's results and the GREET results for both CARB and Argonne in Figure 3 of their study, they accounted for the N_2O emissions that GREET and EPA had already accounted for in the remaining bars of the figure 3. In particular, for the EPA RIA such N_2O emissions were included as "domestic farm inputs and fertilizer N_2O" (domestic land use change for other GHG changes). By adding them to the GHG emissions from land use change again, we maintain that N_2O emissions, as they are presented in Figure 3, are indeed double counted in Lark et al.(a).

5. Inconsistencies in Lark et al.(a) results

In our original comments, we referred to various puzzling inconsistencies in Lark et al.(a) results at the county level. Lark et al.(b) responded that their spatial results are correct, expected, and match with ecological theory.

First, it is important to note that we neither generated nor interpreted the county-level results, which were provided by Lark et al.(a) in their supporting materials, but rather used them to make some cross-checks. Second, checking the results of Lark et al.(a) at the county level helps verify consistency in results. When the result of modeling at an aggregated level (e.g., county level) shows unexpected results, that could suggest more issues with using such results for detailed analysis (e.g., spatial resolution). We would be happy to review the results of Lark et al.(a) at the spatial resolution level for annual changes (not the average for the eight years of study period) in cropland, pasture land, CRP land, wheat, corn, and soy and any other crops.

Finally, the Lark et al.(b) response to our original comment suggests that they did not understand our comments, so we will expand on our original comments to help. To take one example, the Lark et al.(a) results at the county level show that in one specific county (fips code 26147), the area of cropland increases by about 130 ha, which generates a carbon savings (not carbon emissions) of 18.65 Gg CO_2e. It is not clear what ecological theory or detailed spatial resolution could justify this and other similar observations. In another specific county (fips code 30085), the area of corn increases by 4 ha, while cropland area increases by 9261 ha. Again, it is not clear what land transformation elasticities and what county characteristics generated this and similar observations at the county level. Note that Lark et al.(a) estimated land transformation functions at the NRI data point level using county-specific characteristics. They did not use spatial data in this estimation process. The general assertion provided by Lark et al.(b) that their results are consistent and match theory should be supported by the release of their estimated land transformation functions, their parameters, and the projected changes in land cover items and crops.

6. Attribution of ethanol volume to the RFS2

In our original comments, we noted that Lark et al.(a) did not isolate the effects of RFS on ethanol growth from other policies and market forces and simply assigned the difference between the targets of RFS1 and RFS2 as the contribution of RFS to ethanol consumption.

In response to this comment, Lark et al.(b) noted that they followed Carter et al. (2017) in determining the effect of RFS on ethanol consumption and then argued the following:

"Nevertheless, our results reflect the impacts of increased corn ethanol demand in general, regardless of the source of such increases."

In response to Lark et al.(b), we provide the following additional comments:

- We believe that, by mistake, the authors misinterpreted the Carter et al. (2017) argument when they said: *"Carter, Rausser, and Smith (2017) argue that ethanol prices were **not** low enough to have incentivized additional ethanol use if the RFS2 had not passed."* Indeed, Carter et al. (2017) did argue that in the absence of RFS2 ethanol production was not profitable.

- However, the Carter et al. (2017) argument that ethanol was not profitable in the absence of RFS2 is simply inaccurate. Indeed, ethanol production was profitable prior to RFS2. Figure 3 indicates the profitability of ethanol prior to RFS2. This figure from Tyner and Taheripour (2008) shows the breakeven line for ethanol production (including 12% return on equity) and the actual profitability of ethanol industry year by year from 2000 to 2008. The figure shows that, unlike the claim made by Carter et al. (2017), ethanol production was profitable prior to the approval of RFS2.

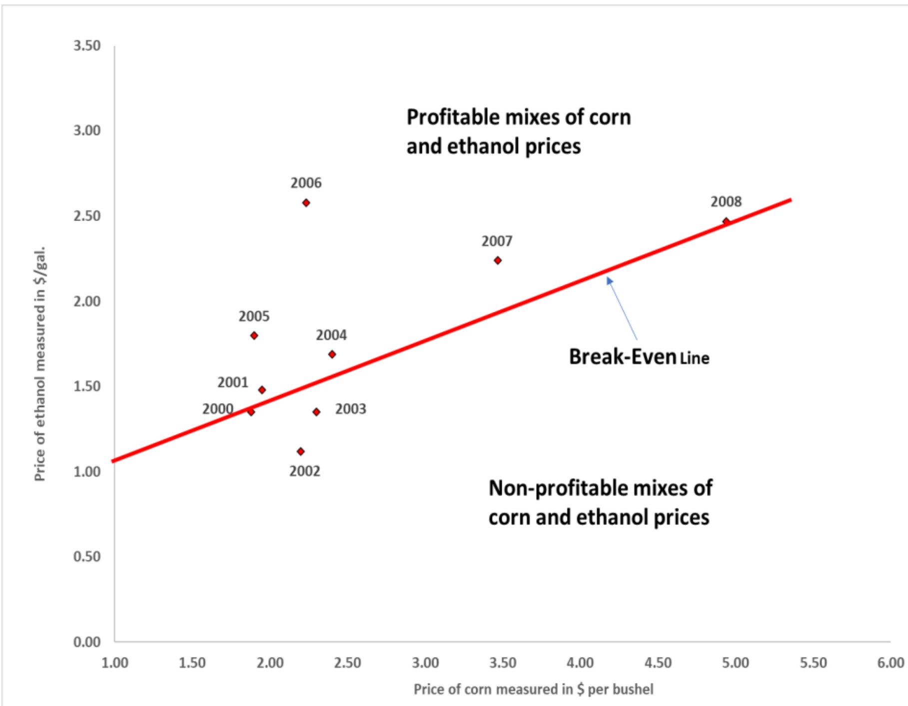

Figure 3. Breakeven corn and ethanol prices compared with actual profitability observations. Red diamonds represent actual observations. The break-even line includes 11% return on equity. Source: Tyner and Taheripour (2008).

- In Lark et al.(b) the authors recognize that "*Nevertheless, our results reflect the impacts of increased corn ethanol demand in general, regardless of the source of such increases.*" With this new confirmation, the Lark et al.(a) results do not represent solely the RFS impacts but the impacts of increased corn ethanol demand from a broader range of effects, including the RFS effects. Hence, a more appropriate title, instead of the original title, should have been used for the PNAS paper

7. Displacement of Distiller's Grains and Yield improvements

i. DDGS displacement

In our original comments, we pointed out that distiller's dried grains with solubles (DDGS) displaces corn and soybean meal in least-cost animal rations, which has important land use implications since some acres producing soybean meal for feed are replaced by DDGS. However, in their response, Lark et al.(b) claimed that DDGS only displaces corn:

"Regarding distiller's grains, Irwin and Good (2013) show that the price of DDGS follows the price of corn very closely, which implies that they are close substitutes (although they have some nutrient differences)."

It appears that Lark e al.(b) misinterpreted the work and findings of Irwin and Good (2013), who analyzed and explained three elements: corn and DDGS prices, soy meal and DDGS prices, and the ratio of DDGS price to corn price. Using these figures and a few simple regression analyses, Irwin and Good (2013) concluded the following:

"We show that dried solubles (DDGS) prices are primarily explained by corn and soybean meal prices, reflecting the value of energy and protein content of the DDGS. Not surprisingly, our simple model still leaves substantial variation in DDGS prices to be explained."

Clearly this statement does not align with the Lark et al.(b) interpretation. Irwin and Good (2013) did not state that corn and DDGS are close substitutes. Detailed animal feed ration formula in fact shows substitution of both corn and soymeal by DGS (see Benavides et al. 2020). Indeed, the findings of Irwin and Good (2013) support the fact that the price of DDGS reflects the value of energy and the protein content of this by-product.

Lark et al.(b), go on to state the following:

"Taheripour et al. use the GREET model to argue that the net loss is about half an acre because DDGS displace some soybean meal which saves land because soybeans are lower yielding than corn. This point is not relevant to our modeling because our LUC modeling estimates how farmers respond to price changes, i.e., rather than making a mechanical adjustment in acreage based on ethanol production, farmers make planting decisions based on prices."

It seems that Lark et al. did not understand our original response, given their assumption that DDGS only replaces corn. In fact, the substitution of DDGS for corn plus soybean meal has been widely documented by Weightman et al. (2011), Buenavista et al. (2021) and very recently by Haque et al. (2022). The later specifically state: "*DDGS substitutes [for] soybean meal (SBM), di-calcium phosphate, and corn in swine diets, providing lysine, phosphorus, and energy. In DDGS, lysine is very restrictive to 0.7%, whereas phosphorus is relatively high (0.71%).*" In fact, any least-cost ration feed formulation program will substitute DDGS for soybean meal as well. Therefore, utilizing soybean meal substitution ratios is not "mechanical" but reflective of livestock use of DDGS. Lark et al.(b)'s statement that DDGS and corn "*have some nutrient differences*" exactly explains why DDGS does not simply substitute for corn only, as assumed in their modeling. Overlooking these more complex substitution effects in their modeling casts doubts on Lark at al.(a)'s analysis. In fact, soybean yield is much lower than corn yield per acre, ignoring of soymeal substitution by DDGS would result in significantly less LUC offsetting effects by DDGS.

ii. Yield improvements

In our original comments, we noted that Lark et al.(a) failed to take yield improvement into account in their modeling approach, and we made some calculations to address the importance of yield improvement. Lark et al.(b) responded to our comment on yield improvement with the following:

"Our model estimates what land use would have been if demand for corn had been 1.3b bushels lower than it was under the RFS2. Taheripour et al. note that corn yield has increased since 2007, but these improvements may have persisted absent the RFS2, so it should not be assumed that all of the change in corn yield before and after 2007 is attributed to RFS2. Moreover, our price analysis accounts for the possibility that price increases cause yield increases. If yields tend to increase when prices increase, then the resulting supply increase would mitigate the price effects. Our goal was not to estimate the amount by which cropland increased after RFS2, but rather the difference between observed cropland use and what would have happened if corn demand were 1.3b bushels lower."

In what follows, we present additional comments regarding the above response.

a) The claim that "*Taheripour et al. note that corn yield has increased since 2007, but these improvements may have persisted absent the RFS2"* represents the views of Lark et al. on yield improvement. While various papers have shown that yield does respond to higher prices (Houck and Gallagher, 1976; Lyons and Thompson, 1981; Choi and Helmberger, 1993; Huang and Khanna, 2010; Weersink et al., 2010; Berry and Schlenker, 2011; Yu et al., 2012; Goodwin et al., 2012; Haile et al., 2016; Miao et al., 2016; Kim et al., 2018; Rosas et al., 2019), Lark et al.(b) apparently ignored this important fact by arguing that yield "*improvements may have persisted absent the RFS2*", especially when Lark et at. asserted significant corn price increase from the RFS.

b) Lark et al.(b) noted that "*it should not be assumed that all of the change in corn yield before and after 2007 is attributed to RFS2."* This is unquestionably the case, and no one has asserted otherwise. However, an economic model that is supposed to represent the effect of a policy (RFS) for 30 years should take into account the impacts of potential yield improvements due to higher crop prices, in particular the sharp price increases projected by Lark et al.(a) (i.e., the 31%, 19%, and 20% price increases for corn, soybeans and wheat, respectively), and one should expect to observe some yield improvements in the Lark et al.(a) results. However, it appears that Lark et al.(a), although stating that "*We carefully consider and account for both yield increases and DDG offsets,"* assigned only very small yield improvements, not significantly different from zero, in their analyses.

c) Third, the Lark et al.(a) modeling approach failed to capture the demand side responses to higher crop prices. They assert that, due to RFS2, the prices of corn, soybeans, and wheat would increase by 31%, 19%, and 20%, respectively. It is logical to question what the impacts of these large price increases would be on demand for these commodities. In fact, the Lark et al.(a) results show nearly no demand response.

iii. An assessment of yield improvement and demand response in Lark et al.(a)

In what follows we provide some analyses to highlight the importance of the missing factors noted above in the Lark et al.(a) work, singly and in combination. Above in (a) we noted the effect of inaccurately specifying the effects of substituting DDGS for corn and soybeans meals, but here we follow Lark et al.'s assumption of 1/3 of corn as the only credit for DDGS to highlight other issues imbedded in the Lark et al.(a) results. Following Lark et al.(a), we also assume 2.8 gallons of ethanol per bushel of corn rather than the average conversion rate of corn to ethanol of about 2.9 gallons per bushel of corn.

Table 3 show the observed yields for corn, soybeans, and wheat in 2007 and 2015. As shown in this table, corn, soybean, and wheat yields increased between 2007 and 2015 by 11.8%, 15.0%, and 8.3% respectively. These are significant yield improvements. Of course, not all of these yield improvements should be assigned to the RFS. The critical questions are:

- What portion of the observed yield increases should be assigned to the RFS?
- What yield impairments are included in the Lark et al.(a) results?

Table 3. Observed yields in 2007 and 2015.*

Description	Corn	Soybeans	Wheat
Observed yields in 2007 (tonnes/ha)	9.46	2.81	2.70
Observed yields in 2015 (tonnes/ha)	10.57	3.23	2.93
Percent change in yields 2007-15	11.8	15.0	8.3

* The observed yields in 2007 and 2015 are approximately on the long-run yield trend lines.

To assess the extent to which Lark et al.(a) have taken into account yield improvements and demand response—the two important market-mediated responses that affect land use (Hertel et al., 2010)—we developed the following analyses.

Table 4 shows potential yield responses to the assumed price increases by Lark et al.(a) for corn, soybeans, and wheat. For example, given the price increases of 31%, 19%, and 20% for corn, soybeans, and wheat claimed by Lark et al.(a), a very small yield to price response of YDEL= 0.05 leads to yield improvements of 1.55%, 0.95% and 1% for corn, soybeans, and wheat. The corresponding yield improvements for YDEL=0.25 are 7.75%, 4.75%, and 5% for corn, soybeans, and wheat, as shown in Table 4. These are significantly lower than the observed yield improvements for 2007-2015. This means that even a YDEL=0.25 in combination with the assumed large price increases by Lark et al.(a) do not correspond to the observed yield improvements in 2007-2015.

Table 4. Potential yield improvements for alternative yield to price responses with assumed increases in crop prices by Lark et al.(a)

Description		Corn	Soybeans	Wheat
Price increases assumed by Lark et al.(a) (%)		31	19	20
Percent change in yield under various YDEL assumptions	Percent change in yield with YDEL=0.00	0.00	0.00	0.00
	Percent change in yield with YDEL=0.05	1.55	0.95	1.00
	Percent change in yield with YDEL=0.10	3.10	1.90	2.00
	Percent change in yield with YDEL=0.15	4.65	2.85	3.00
	Percent change in yield with YDEL=0.20	6.20	3.80	4.00
	Percent change in yield with YDEL=0.25	7.75	4.75	5.00

Table 5 shows the required expansion in corn area to produce 5.5 billion gallons (Bgal) of corn ethanol, targeted by Lark et al.(a), using the following assumptions:

- No yield improvement in corn
- Conversion rate of 2.8 gallons of ethanol per bushel of corn
- A one-to-one displacement between DDGS and corn with 1/3 credit in land use for DDGS
- No change in demand for corn, i.e., zero demand elasticity for corn.

We refer to the required expansion in corn area using these assumptions as the *max-corn* area.

As shown in Table 5, the max-corn area would be about 3.516 million ha (Mha). The expansion in corn area estimated by Lark et al.(a) is 2.8 Mha. The difference between these two values is about 0.716 Mha, as shown in Table 5. In what follows we examine the yield improvements that would close this gap.

Table 5. Max-corn area requirement.

Description	Assumptions/Calculated Results
Increase in corn ethanol production by Lark et al.(a) (Bgal)	5.5
Corn to ethanol conversion rate (gallons/bushel)	2.8
Corn needed (bushels)	1,964,285,714
Pounds per tonne	2204.62
Pounds per bushel corn	56
Corn needed (tonnes)	49,895,220
Corn yield in 2007 (tonnes/ha)	9.46
Yield improvement (% change)	0
Required expansion in corn area (ha)	5,275,181
Required expansion in corn area adjusted for DDGS (ha)	3,516,788
Max-corn area (ha)	3,516,788
Estimated corn area expansion by Lark et al.(a) (ha)	2,800,000

Description	Assumptions/Calculated Results
Difference between max-corn area and Lark et al. results (ha)	716,788

Table 6 shows land saving due to yield improvements and/or demand response in various scenarios. The top section of this table shows data for production, consumption (domestic and net exports), area, and yield for corn, soybeans, and wheat in 2007.

The second section of Table 6, Scenario 1, repeats the calculations of Table 5 with varying yield improvements due to higher crop prices. This scenario considers a tiny yield-to-price-response of YDEL=0. 032. This small value of YDEL in combination with assumed increases in crop prices by Lark et al.(a) generates yield increases of 0.99%, 0.61%, 0.64% for corn, soybeans, and wheat, respectively. In this scenario, the required area for producing 5.5 Bgal corn ethanol drops slightly, from 3.516 Mha to 3.482 Mha. However, these small yield improvements generate lower demands for land for corn, soybeans, and wheat production compared to the status quo in 2007. The total land saving due to the assumed yield improvements in this scenario is about 0.623 Mha, as shown. In this scenario, the net increase in demand for corn area would be 2.8 Mha, as seen in the last line of Scenario 1. This is identical to the projection made by Lark et al.(a) for the expansion in corn area due to RFS. Indeed, the results of Scenario 1 reveal that Lark et al.(a) applied very small yield improvements.

The small yield improvements imbedded in Lark et al.(a) (i.e., 0.99%, 0.61%, 0.64% for corn, soybeans, and wheat, respectively) may be compared with the observed yield improvements of 11.8% for corn, 15% for soybeans and 8.3% for wheat for the period 2007-2015 shown in Table 3. The comparison indicates that Lark et al.(a)’s too-small yield improvements led to an overestimated need for additional land for corn production.

The second scenario presented in Table 6 repeats the first scenario but with a higher yield-to-price response of YDEL=0.175. This is not a high yield response, given the existing estimated value for this parameter. However, it is large enough to compensate for the expansion in corn land by savings in land due to yield improvements. This value of YDEL in combination with assumed increases in crop prices by Lark et al.(a) generates yield increases of 5.43%, 3.33%, 3.5% for corn, soybeans, and wheat, respectively. These yield improvements are significantly lower than the observed yield increases in the period 2007-2015 presented in Table 3. With YDEL=0.175, the required area for producing 5.5 Bgal corn ethanol drops to 3.335 Mha, and the land saving due to yield improvements is about the same, as shown in Table 6. Scenario 2 clearly shows that with a better assessment of yield improvements, even the questionable modeling framework of Lark et al.(a) projects no expansion in demand for cropland due to the RFS.

The third scenario presented in Table 6, unlike the first two, introduces demand response into the picture. In this scenario, it is assumed that the domestic and foreign users of U.S. corn, soybeans, and wheat are responding to higher crop prices with small price elasticities of 0.05 and 0.1 for the domestic and foreign crop users respectively. The demand response, even with the assumed small elasticities, generates land savings of 1.126 Mha.

This land saving result also suggests that Lark et al.(a) overlooked an important market-mediated response and so overestimated the land use implications of ethanol. Note that we limited our analyses of yield and demand responses to corn, soybean, and wheat to match the narrow viewpoint of Lark et al.(a). Extending our analyses to other crops certainly suggests more room for land use savings due to market-mediated responses.

In conclusion, Lark et al.(a) significantly overestimated the land use implications of ethanol production because of the three factors explained above: miscalculated replacement of corn and soymeal by DDGS, an assumed yield improvement close to zero, and overlooked reduction in demand due to higher crop prices.

It is important to also note that Lark et al.(a) dismissed the large share that U.S. agriculture has of commodity markets at the global scale. The next section examines the consequences of this important omission.

Table 6. Effects of yield improvements and demand responses on corn ethanol land use changes.

	Description	Corn	Soybeans	Wheat
Status quo in 2007	Yield (tonnes/ha)	9.46	2.81	2.70
	Harvested area (ha)	35,013,780	25,959,240	20,638,784
	Production (tonnes)	331,177,280	72,859,180	55,820,360
	Net export (tonnes)	56,680,022	29,564,479	30,601,278
	Used for ethanol (tonnes)	77,448,268		
	Domestic use	197,048,990	43,294,701	25,219,082
	Price increases based on Lark et al. (% change)	31	19	20
Scenario 1	Percent change in yield with YDEL=0.032	0.99	0.61	0.64
	Land needed to satisfy crop production of 2017 with higher yield (ha)	34,669,855	25,802,362	20,507,536
	Land saving by crop due to higher yields compared with status quo (ha)	-343,925	-156,878	-131,248
	Total land use saving compared with status quo (ha)	-632,052		
	Corn area requirement for 5.5 Bgal ethanol with higher yield (ha)	3,482,244		
	Net land area needed for 5.5 Bgal ethanol with higher yield (ha)	2,850,192		
Scenario 2	Percent change in yield with YDEL=0.175	5.43	3.33	3.50
	Land needed to satisfy crop production of 2017 with higher yield (ha)	33,211,637	25,123,686	19,940,700
	Land saving by crop due to higher yields compared with status quo (ha)	-1,802,143	-835,554	-698,084
	Total land use saving compared with status quo (ha)	-3,335,781		
	Corn area requirement for 5.5 Bgal ethanol with higher yield (ha)	3,335,780		
	Net land area needed for 5.5 Bgal ethanol with higher yield (ha)	0		
Scenario 3	Price elasticity of domestic demand	0.05	0.05	0.05
	Price elasticity of foreign demand	0.1	0.1	0.1
	Savings in domestic demand due to higher prices (tonnes)	-3,054,259	-411,300	-252,191
	Savings in foreign demand due to higher prices (tonnes)	-1,757,081	-561,725	-612,026
	Total saving in demand (tonnes)	-4,811,340	-973,025	-864,216

	Saving in land due to saving in demand by crop (ha)	-482,498	-335,524	-308,724
	Total saving in demand for land due to demand response (ha)	-1,126,746		
	Land use saving compared with status quo in scenario 2 (ha)	-3,335,781		
	Total land use saving (ha)	-4,462,526		
	Corn area requirement for 5.5 Bgal ethanol in scenario 2 (ha)	3,335,780		
	Net change in land area (ha)	-1,126,746		

8. Estimation of price effects

In our original comments, we questioned the approached used by Lark et al.(a) to assess the price impacts of RFS and its implications for land use change. In our original comments, we first highlighted the work done by Filip et al. (2019) and noted that their findings are in sharp contrast with the results of Lark et al.(a). In our original comments, we noted that Filip et al. (2019) concluded that "*price series data do not support strong statements about biofuels uniformly serving as main leading source of high food prices and consequently the food shortages.*"

Then we noted that "*Lark et al. evaluated the price impact of 5.5 Bgal by using the observed prices for the 2006-2010 period to evaluate the price impacts while their assignment of 5.5 billion gallons to the RFSs is for the first eight years between 2008 and 2016. During 2006-2010 crop prices increased significantly, but these prices dropped in the following years and came back to much lower levels.*" Following this statement, we provided some statistics to highlight the odds with the Lark et al.(a) approach.

In response to our comments, Lark et al.(b) repeated their approach, said that the statistics we provided are irrelevant, and stated that their *"price effects modeling is valid and consistent with existing literature."*

We believe that Lark et al.(b) misunderstood or misinterpreted our critique. We understand their approach. They used the observed prices and their estimated BAU prices for 2006-2010 to assess the price impacts on corn, soybeans, and wheat (see description of Table S1 in SI of Lark et al.(a)), and they selected the eight years between 2008 and 2016 to calculate land use changes due to the RFS. Our critique highlighted the mismatch between the time period of 2006-2010 and the eight years between 2008 and 2016, as clearly noted in our original comments. For the sake of clarity, we restate our critique in what follows.

Lark et al.(a) used the actual observations and their estimated BAU prices for the period 2006-2010 to calculate the price impacts of RFS (5.5 Bgal ethanol) on corn, soybeans, and wheat prices. They then used the results from that analysis to assess the land use impacts of the additional demand for ethanol (5.5 BGal) between 2008 and 2016.

What justifies the selections of these time segments? What is the validity of using calculated price impacts obtained for 2006-2010 to evaluate the induced land use changes by 5.5 Bgal of ethanol during the years between 2008 and 2016? We believe that these are straightforward questions. It is puzzling to use the price impacts of 2006-10 for the eight years between 2008 and 2106, when prices followed different patterns in these two periods.

In our original comments, we provided some statistics to show that the crop prices and their changes are very different between 2006 and 2010 and between 2008 and 2016. At that time, we used the FAO data to show that prices followed different patterns in these two periods. Here, we use the data imbedded in Figure 1 of Lark et al.(a) to show the same issue.

Figure 4 replicates Figure 1 of Lark et al.(a) and Table 7 shows annual percent changes in corn, soybeans, and wheat using the Lark et al. data (perhaps with some insignificant approximation, as we read the data for the Figure 1 of Lark et al.(a)). The last two columns of Table 7 show the average annual percent changes in crop prices for the five years 2006 to 2010 and the eight years 2008 to 2015. As shown in this table, the averages of the two periods are very different. The averages for the first period are quite large: 33.5% for corn, 23.7% for soybeans, and 24.9% for wheat. The averages for the second period are tiny or negative: 0.8% for corn, -1.9% for soybeans, and -6.2% for wheat.

These comparisons clearly show that the prices followed different patterns in 2006-2010 and 2008-2016. That being the case, using estimated price impacts of RFS for the first time period and applying them to the second is problematic.

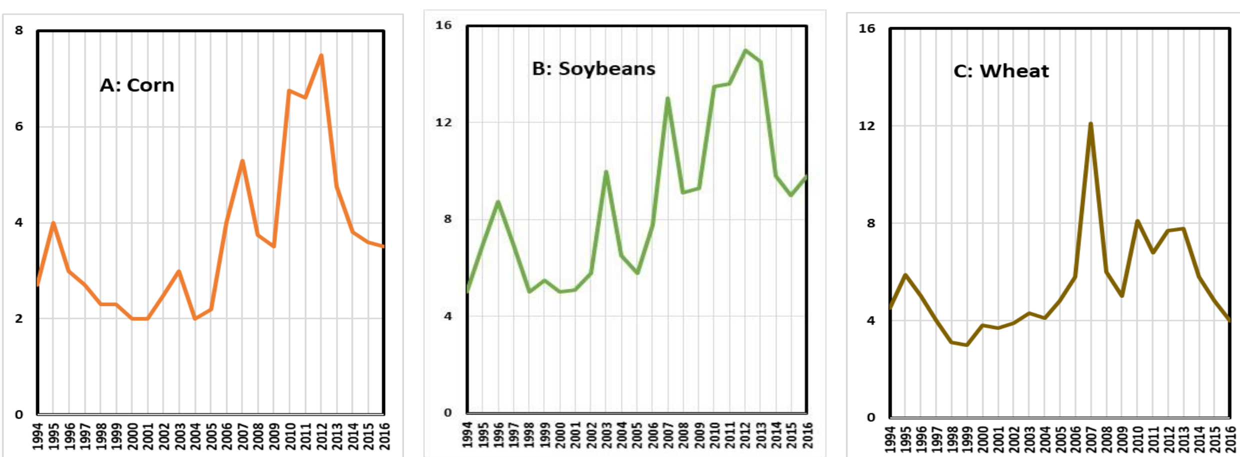

Figure 4. Replication of observed prices presented in Figure 1 of Lark et al.(a).

Table 7. Annual % changes in observed corn, soybeans, and wheat prices from 2006-2016 and their averages for 2006-2010 and 2008-2015. Obtained from Lark et al.(a) Figure 1 with some approximations.

Year	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	Average of annual % changes	
											2006-10	2008-15
Corn	59.1	51.4	-29.2	-6.7	92.9	-2.2	13.6	-36.7	-20.0	-5.3	33.5	0.8
Soybeans	33.6	67.7	-30.0	2.2	45.2	0.7	10.3	-3.3	-32.4	-8.2	23.7	-1.9
Wheat	20.8	108.6	-50.4	-16.7	62.0	-16.0	13.2	1.3	-25.6	-17.2	24.9	-6.2

The last part of the Lark et al.(b)'s response to our critique indeed discloses why Lark et al.(a) selected the time period of 2006-10 and not the second time period for the calculation of price impacts. Lark et al.(b) stated that: "***As we explain in the supplementary text (SI ln 963), our estimated price effects would be somewhat larger if we used all years up to 2016. This fact is visible in Figure 1 in the main text".***

This statement is surprising and merits some attentions for two reasons. The first reason is that the inclusion of a longer time period (up to 2016) led to a larger price impact of ethanol which is inconsistent with the observation that corn prices fell after 2014 even though ethanol production was higher than in the 2006-2010 period. The second reason is the fact that Lark et al.(a) did not use the findings from the study that included the data up to 2016 for their analysis. There is more justification for using estimates based on the updated data set than for applying estimates from an earlier time period. The fact that Lark et al.(a) did not use the updated estimates may suggest that they were not convinced about the validity of their model and its findings when extended to include the later time period.

The statement of Lark et al.(b) mentioned above seems to confirm that the method used by Lark et al.(a) generates somewhat larger price increases if they assess the price impacts for the correct period, from 2008 to 2016, and to avoid estimating larger price impacts, the authors selected the time period of 2006-2010, which is unrelated to their selected time period for the land use assessment. **Choosing an unrelated time period to avoid estimating larger price impacts is not a scientific approach and leads to flawed results.**

At the beginning of their response to our comments on this topic, Lark et al.(b) stated that

"*Our price effects modeling is valid and consistent with existing literature. As explained in our study's main and supplementary texts, our modeling approach is the same as in Carter, Rausser, and Smith (2017), which contains all the details of the model specification and identification strategy."*

We make several comments related to this statement below.

a) Carter et al. (2017) provided an assessment for the price impact of corn ethanol and their modeling approach focused only on corn. There are a large number of papers that have examined the effects of corn ethanol production on crop prices and obtained a wide range of different outcomes. Lark et al.(a) should have justified their reliance on only one study for their price impact and analyzed the implications of using price effects from other studies on their findings.
b) We contend that using the findings on price effects from a commodity-by-commodity reduced form model in Carter et al. (2017) to analyze land use change is not appropriate when multiple crops are competing for land because it does not explicitly consider substitution among crops on the same land. Corn, soybeans, and wheat are produced using a common factor of production: land. Farmers allocate their land to maximize their profits. Using a single commodity inventory model and estimating separate equations for these commodities is not a valid approach when three commodities compete for land. A change in inventory for one of these commodities would affect farmers' land allocation and therefore crop prices.

c) Carter et al. (2017) evaluated the effect of the RFS on corn price in the context of a closed economy, essentially missing the fact that the U.S. share of the global trade of corn is not small. However, a closed economy approach is not a proper method to use for highly traded commodities such as corn, soybeans, and wheat. In particular, the share of U.S. in the global market for soybeans is significantly large. As can easily be calculated from the first section of Table 6, about 17%, 41%, and 53% of U.S. corn, soybeans, and wheat, respectively, are exported to other countries. The size of U.S. soybeans exports in the global commodities market is large (See Taheripour and Tyner (2018 for details). It is problematic to follow Carter et al. (2017) and use a closed economy modeling approach for the three highly traded commodities of corn, soybeans, and wheat.

d) Finally, Carter et al. (2017) compared their estimated BAU prices with actual observations to assess the impact of RFS on corn ethanol. This is a problematic comparison. The Carter et al. (2017) BAU prices are obtained from their estimated model for the goals of RFS1. The same model results for actual observations should be compared with BAU, not the actual observations. Model results with RFS1 and RFS2 should have been compared, ceteris paribus, holding all other modeling assumptions the same, instead of comparing model results in the BAU to actual observations. Figure 5 highlights the correct and problematic comparisons. The problematic comparison of BAU to observed data assigns the model estimation error to the difference between actual observation and BAU projection in each year. Depending on the sign of the error, this comparison could lead to over- or underestimation.

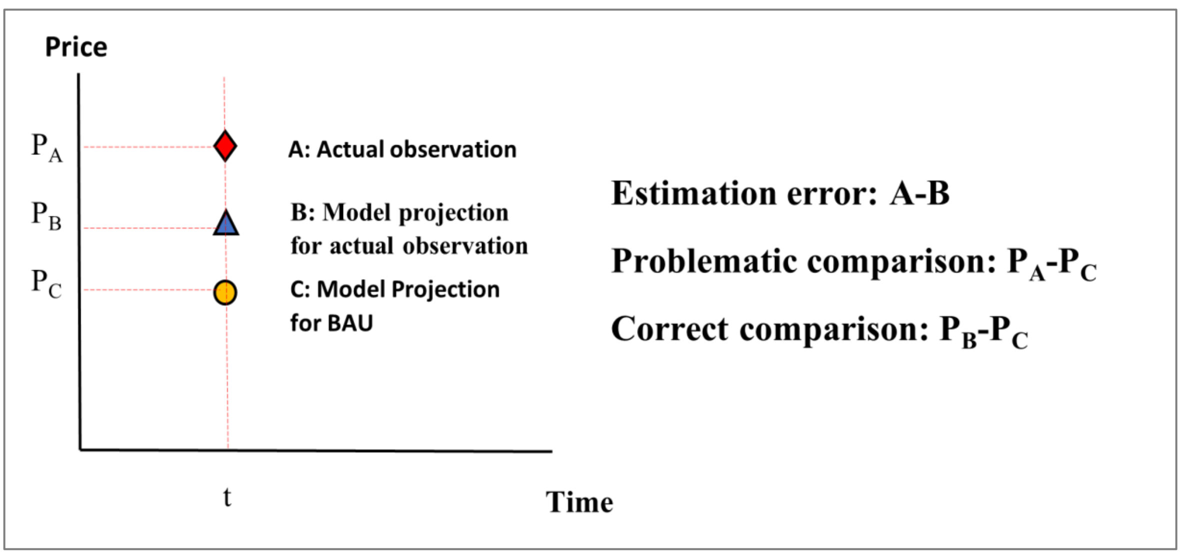

Figure 5. Estimated price impact: Correct and problematic comparison.

9. Ignoring land category of cropland pasture in modeling land transition

In our original comments we noted *"Lark et al. did not recognize cropland pasture as a sub-category of cropland in their analyses and perhaps treated this type of land as pasture land or fallow land. This misidentification and the method used by the authors to assess land return is likely to have artificially led to the additional demand for active cropland being met largely by CRP land and not by cropland pasture."* Then we explained the reasons why ignoring the land category of cropland pasture leads to overestimation of land use emissions.

In response to this comment, Lark et al.(b) admitted that they intentionally overlooked this type of land, stating that "*Our modeling of land transitions purposely avoids the separate problematic category of Cropland-Pasture."* They then provided their reasons for disregarding this important category of land. In what follows we respond to the Lark et al.(b) claims.

i. Unlike the claim made by Lark et al.(b), cropland pasture is not a "*problematic category*" of land. The land category cropland-pasture is a standard category of land defined and recognized by the Food and Agricultural Organization (FAO) of the United Nations and the Intergovernmental Panel on Climate Change (IPCC) as "temporary pasture and meadows." By definition, it is a sub-category of cropland, representing a portion of the existing cropland which has not been harvested and is temporally used as pasture. The U.S. Agricultural Censuses (UACs) have identified this category of land as "cropland pasture", For details, see Taheripour et al. (2021).

ii. Lark et al.(b) claimed that *"Cropland-pasture has been identified as an enigmatic land classification that obfuscates valid estimation of LUC, and its use has contributed to systematically downward-biased estimates of emissions from corn ethanol-induced LUC (Malins et al. 2020; Spawn-Lee et al. 2021)."* We have several responses to this statement.

- Taheripour et al. (2021) have responded to this claim, originally made by Malins et al. (2020). We refer the readers to our 2021 paper and its supporting documents to evaluate the credibility of the claim made by Malins et al. (2020). Here we provide some additional analyses to shed more lights on this claim.
- Cropland pasture is not "*an enigmatic land classification.*" It is a clearly defined and well-known land category, and its area has been recorded and reported in the UACs. Table 5 shows the areas of cropland and cropland pasture since 1959. The area of cropland pasture has fluctuated between 25 and 36 Mha from 1950 to 2002, constituting about 14% to 20% of total cropland. The area of this land category has declined in the last three UACs, to 14.5 Mha in 2007, 5.2 Mha in 2012, and 5.6 Mha in 2017.

  Historically, the area of cropland pasture has decreased when commodity markets were strong and vice versa. The area of this land category has been more than the total area of Illinois and Iowa in many years. The area of cropland pasture has been always larger than the area of CRP land, except in recent years. Lark et al.(b) labeled millions of hectares of land classified under the land category of cropland pasture as "enigmatic" in contradiction with the definitions, identifications, and data provided by the FAO, IPCC, and USDA. In what follows we explain why the area of cropland pasture has declined in the last three UACs.

- The claim that taking into account cropland pasture *"obfuscates valid estimation of LUC, and its use has contributed to systematically downward-biased estimates of emissions from corn ethanol-induced LUC"* needs careful attention. As mentioned above, historically the area of cropland pasture has declined when commodity markets were strong and vice versa. Of course, returning cropland pasture (which has been without crop production for a short period) will not cause significant land use emissions compared with the conversion of forest or natural pasture. While the existing observations indicate that area of pasture has declined after 2002, there is no evidence of deforestation, so it is wise to take into account reduction in this category of land in assessing LUC due to biofuels. When actual observations do not support deforestation in the U.S. but do support return of cropland pasture to crop production why one should ignore actual observations. Taking into account reduction in cropland pasture reduces the estimation of ILUC. Valid estimation of land use changes and their corresponding emissions are a result of account actual observation rather than assumptions.

iii. Lark et al.(b) noted that the UACs do not provide annual data and that the definition of cropland pasture has changed in recent UACs, and quoted a report provided the USDA experts (Bigelow and Borchers, 2017) to confirm that the definition of cropland pasture changed in the 2007 and 2012 UACs. We reply to these claims in what follows.

- The fact that UACs provide data every five years, i.e., we do not have annual data on cropland pasture area, is a limitation but not a reason to ignore this land category and its role in both managing the supply side of the crop markets and buffering unnecessary land transitions. In fact, the UAC results provide the required frameworks for developing many annual surveys conducted by the USDA on land use and land use changes. Providing a good assessment for annual land transition from active cropland to cropland pasture and back again is a valuable and nontrivial task. One cannot ignore the existing 5-year information on this land category provided by the UACs.

- Changes in the definitions of data items happen frequently in censuses and annual surveys, even in the NRI data. Inconsistencies across different versions of a database are also common. In Table 1, we highlighted two inconsistencies between two versions of the NRI data. Here we have another one. The NRI release of 2012 reported that 351.7 million acres of cropland remained in this category of land between 2007 and 2012. The NRI release of 2017 reported 348.1 million acres of cropland remain in the cropland category for the same time period. The difference between these two numbers is 3.6 million acres, which is not a small area. Should we recommend not using the NRI data because of this and many other inconsistencies that may suggest some changes in the definitions, approaches, or data processed? Of course not. For the same reasons, we cannot disregard information on cropland pasture.

- Lark et al.(b) referred to Bigelow and Borchers (2017) to note that the cropland pasture definition has changed in recent censuses. It is true that the definition of cropland pasture in these censuses changed, and Bigelow and Borchers (2017) noted that fact. But that's not the whole story. As described by Bigelow and Borchers (2017), only a portion of the observed reduction in area of cropland pasture was due to the change in definition of this land category. Taheripour et al. (2021) noted that the area of cropland pasture has declined in recent censuses due to the change in the definition, return of cropland pasture to cropland, and conversion of cropland pasture to pasture land. Table 8 shows reductions in cropland pasture and increases in pasture land in the 2007 and 2012 censuses.

Table 8. Historical data on area of cropland, cropland pasture, and pasture land in US Agricultural Censuses since 1959.

Census	Cropland	Cropland pasture	Share of cropland pasture in cropland	Pasture land
1959	181.3	26.5	14.6	188.7
1964	175.7	23.2	13.2	198.4
1969	185.7	35.7	19.2	183.8
1974	178.1	33.5	18.8	181.6
1978	183.7	29.6	16.1	175.4
1982	180.2	26.3	14.6	169.3
1987	179.4	26.3	14.7	166.1
1992	176.2	27.0	15.3	166.3
1997	174.5	26.1	15.0	160.6
2002	175.7	24.4	13.9	160.0
2007	164.5	14.5	8.8	165.4
2012	157.7	5.2	3.3	168.1
2017	160.4	5.6	3.5	162.2

iv. In our original comments, we noted that modeling land transitions due to ethanol are more complicated than the simplified approach taken by Lark et al.(b). We noted that ignoring land transitions among the sub categories of cropland, including cropland pasture, can artificially in a biased way move the land transition to the pasture and CRP land categories, which are the only options in Lark et al.(a). As noted above, Lark et al.(b) said that they intentionally disregarded the land category of cropland pasture because the UACs do not provide annual data for this land category. In what follows we show that Lark et al.(a) failed to properly use the NRI data to identify land transitions within cropland and among the subcategories of this type of land. Proper analyses of land transitions within cropland and between cropland and pastureland with NRI data could help to capture correctly induced land use changes due to ethanol.

- As we noted in our original comments, Lark et al.(a) picked two land transitions. The first one is between CRP land and cropland. As we noted before, the observed transitions from CRP to cropland were not induced directly by the RFS nor by ethanol consumption. The observed reductions in the CRP land are not even limited to the transition from this category of land to cropland. A large portion of the observed reductions in CRP land is due to transitions from this category to pasture land. The observed reductions in CRP land were mainly induced by budget cut approved by Congress, not farmer's decision. In addition to yield improvements, farmers have uncultivated/unused land available to produce more crops in response to more demand for ethanol. As shown by the NRI data, a large portion (37%) of the observed transitions from CRP to other land categories between 2002 to 2017 was transition to pasture land. Even a portion of the CRP land moved to cropland may have remained uncultivated. Conversion of CRP due to RFS or ethanol consumption is a speculative assumption.

- Lark et al.(a) picked land transitions between pasture and cropland as well. They did not specify the transitions between these land categories in a statistically valid manner. Table S22 of the Lark et al.(a) SI shows that almost all of the estimated regional transitions between cropland and pasture land are statistically insignificant.
- Figure 6 represents three land categories of NRI data including cropland, pasture land, and CRP land, with their sub-categories. The NRI data include other land categories that are not presented in this figure. Figure 6 also shows potential land transitions between the main three categories and within each category with red arrows. Blue arrows indicate the two land transitions that Lark et al.(a) chose to study.
- As shown in Figure 6, Lark et al.(a) ignored a number of important transitions, including but not limited to transitions within cultivated cropland, shown on the top and left side of the figure. For accurate results, it is crucial to correctly specify the annual transitions within all row crops and between row crops and other subcategories of cultivated cropland, such as fallow land, not planted land, and "pasture in cropland." Pasture in cropland refers to pasture in rotation with row crops. Disregarding these transitions leads to an overestimation of ILUC for biofuels in general.
- Figure 6 also shows that transitions between subcategories of cropland and pasture land can also be assessed, such as transitions between managed pasture and row crops. A transition between these two types of land might not generate emissions, while a transition between natural pasture and cropland might generate some emissions. Rather than distinguishing between these two types of transitions, Lark et al.(a) bundled the two, which results in larger emissions.
- While the NRI data set do not explicitly represent the land category of "cropland pasture" defined by the UACs, it implicitly covers that type of land either in the main pasture land category or in the sub-category of pasture in cropland. Rather than tracing the annual transitions between and among these lands, Lark et al.(a) estimated overall transitions between cropland and pasture land and produced statistically insignificant estimated land transitions.

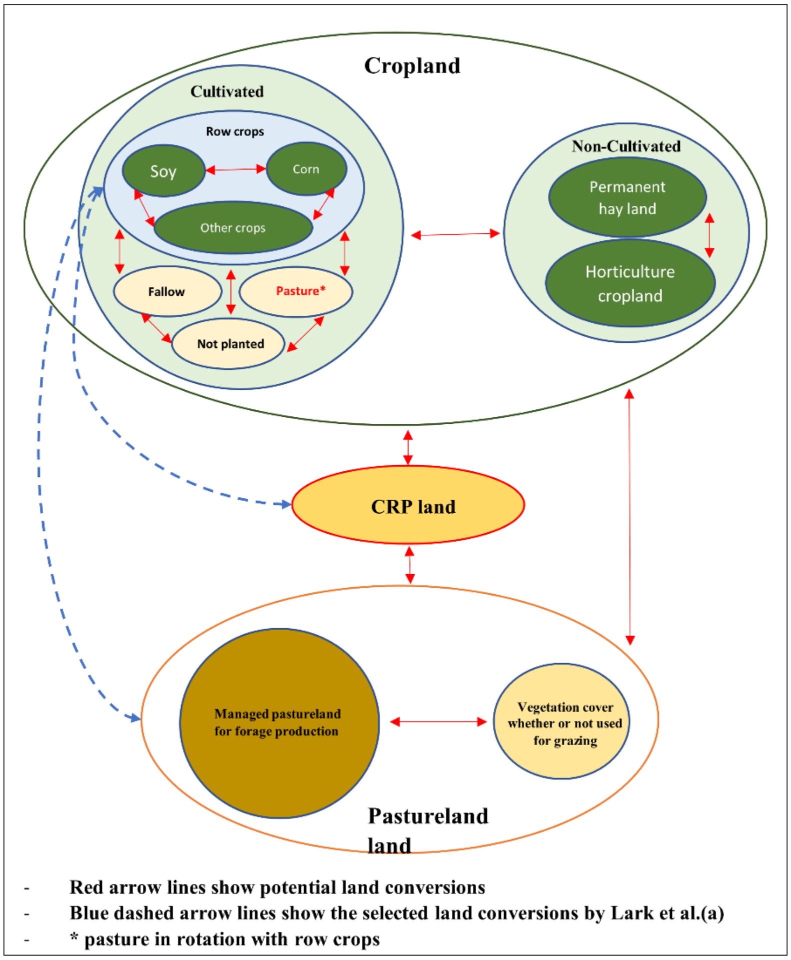

Figure 6. Three NRI land categories, their sub-components, and connections.

10. Conclusion

In a recent publication Lark et al.(a) examined "Environmental Outcomes of the US Renewable Fuel Standard" by assembling a set of loosely connected problematic empirical methods. In a detailed review, we find that this paper suffers from various deficiencies and provided flawed analyses and problematic assessments.

In our comments, we noted that the authors failed to isolate the impact of the RFS on crop prices and land use changes from other drivers of ethanol production and consumption. Lark et al.(b) admitted this important fact and explicitly stated that their "*results reflect the impacts of corn ethanol demand in general, regardless of the source of such increases*". It seems appropriate to revise the title of the original paper to reflect its contents.

The modeling approach used by Lark et al.(a) consists of assessing the price impacts of the RFS on three commodities (corn, soybeans, and wheat) for the period of 2006 to 2010 plus an assessment of land use changes for 8 years from 2008 to 2016. The authors, with no scientific justification, incorrectly used the calculated price effects for 2006-2010 to calculate land use changes from 2008 to 2016, while crop prices followed entirely different pattens in these two time periods of time.

To evaluate the price impacts mentioned above, the authors followed Carter et al. (2017) who assessed the impacts of the RFA on the corn price in a closed economy setup using an econometric approach. In our review we discussed that the Carter et al. (2017) approach suffers from various deficiencies. In addition, we noted that estimating three separate equations for corn, soybeans and wheat prices based on Carter et al. (2017) misses the interactions between the supply sides of these commodities through the market for land and that leads to flawed results.

To evaluate land use changes Lark et al.(a) concentrated on land conversion between pasture and cropland and between CRP land and cropland. Table S22 of the Lark et al.(a) SI shows that almost all of the estimated regional transitions between cropland and pasture land are statistically insignificant. Hence, they build their analyses on the statistically insignificant assessments of land transitions between cropland and pasture land. For land transition between cropland and CRP land we discussed that the CRP lands were returned to crop production at the end of CRP contracts, not induced by RFS. The reduction in area of CRP land were occurred due to budget cut not RFS.

In our review, we showed that the use of CDL data in determining the location of converted land and their characteristics at the grid cell level can lead to overestimation of GHG emissions of ethanol. Furthermore, we discussed that the mapping between the estimated changes in "pastureland" and CRP land obtained from the NRI data at a given data point and the CDL data at the grid cell level can also lead to overestimation of GHG emissions of ethanol. We also used the NRI data and showed that historically land transitions between pastureland and cropland occurred between managed pasture and cropland. However, Lark et al.(a) incorrectly interpreted the land transition between these two land categories as transition between natural pasture to cropland with unjustifiable land use emissions.

We showed that Lark et al. selected the existing literature in various instances in favor of their analyses, while the broad literature clearly shows otherwise.

Our review and analyses confirm that Lark et al. dismissed two important market-mediated effects of biofuel production: Yield improvements and demand responses. We showed that incorporating these effects, even for small fractions of the observed responses in 2007-2016, leads to a small fraction of the estimated land use changes by Lark et al.(a).

Lark et al.(a)'s treatment of soil organic carbon (SOC) and reporting of its uncertainty appear to be based on a misunderstanding of the information extracted from other studies, including their inaccurate use of the carbon response functions (CRFs) derived from Poeplau et al. (2011) and overestimation of the SOC sequestration potential in CRP lands.

In our review we outlined why Lark et al.(a) double counted N2O emissions. We also explained with details why these authors overestimated the carbon content of CRP land.

In our original comments we noted that Lark et. (a) missed various important land transitions that frequently occur within cropland (including but not limited to return of cropland pasture to crop production) and hence they overestimated the land use impacts of corn ethanol. In response, Lark et al.(b) admitted they intentionally ignored “cropland pasture” and stated that their approach “*purposely avoids the separate problematic category of Cropland-Pasture”.* In response, we explained that, unlike the claim made by Larke et al.(b), cropland pasture is a standard land category recognized by the FAO, IPCC, and USDA Agricultural Censuses. We provided detailed information about this land category and its magnitude and changes over time. We then showed that, while the NRI data implicitly includes this type of land, Lark et al. made no effort to capture its changes over time. Instead, they incorrectly assigned changes in the CRP land to ethanol.

In conclusion, we find that the Lark et al.(a) paper is more problematic than what we initially evaluated to be the case.

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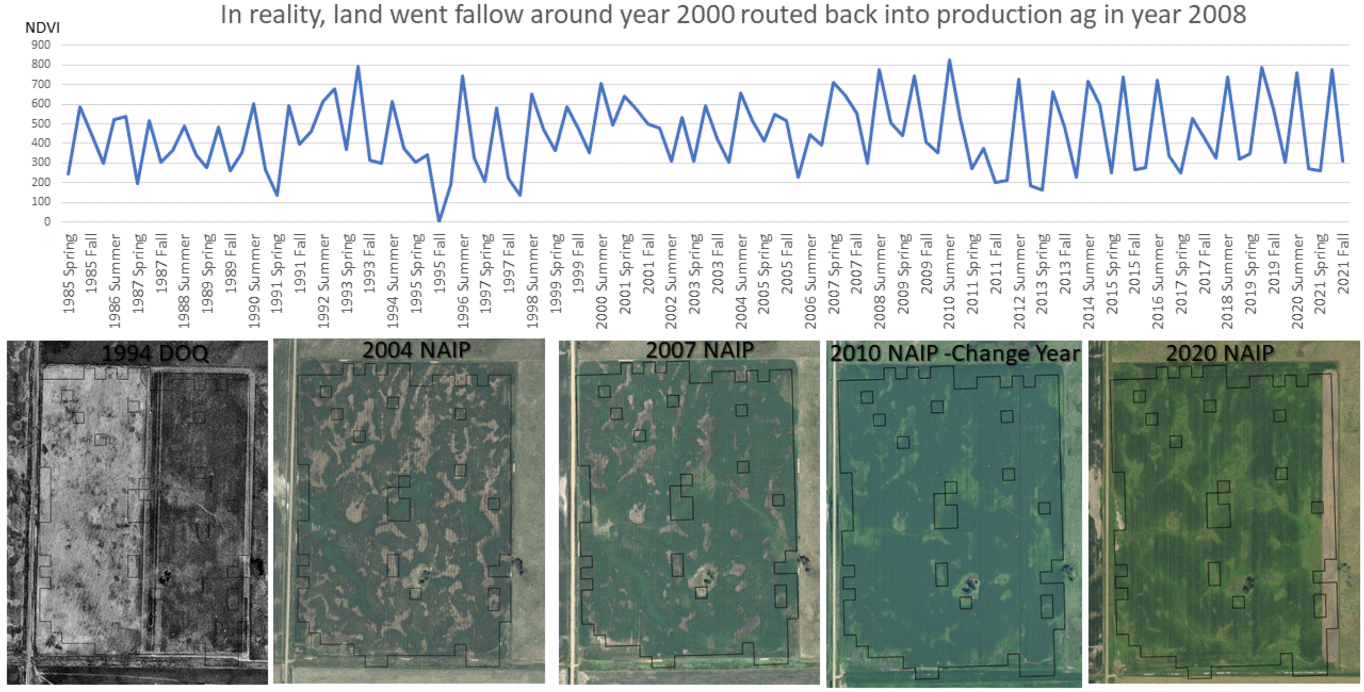
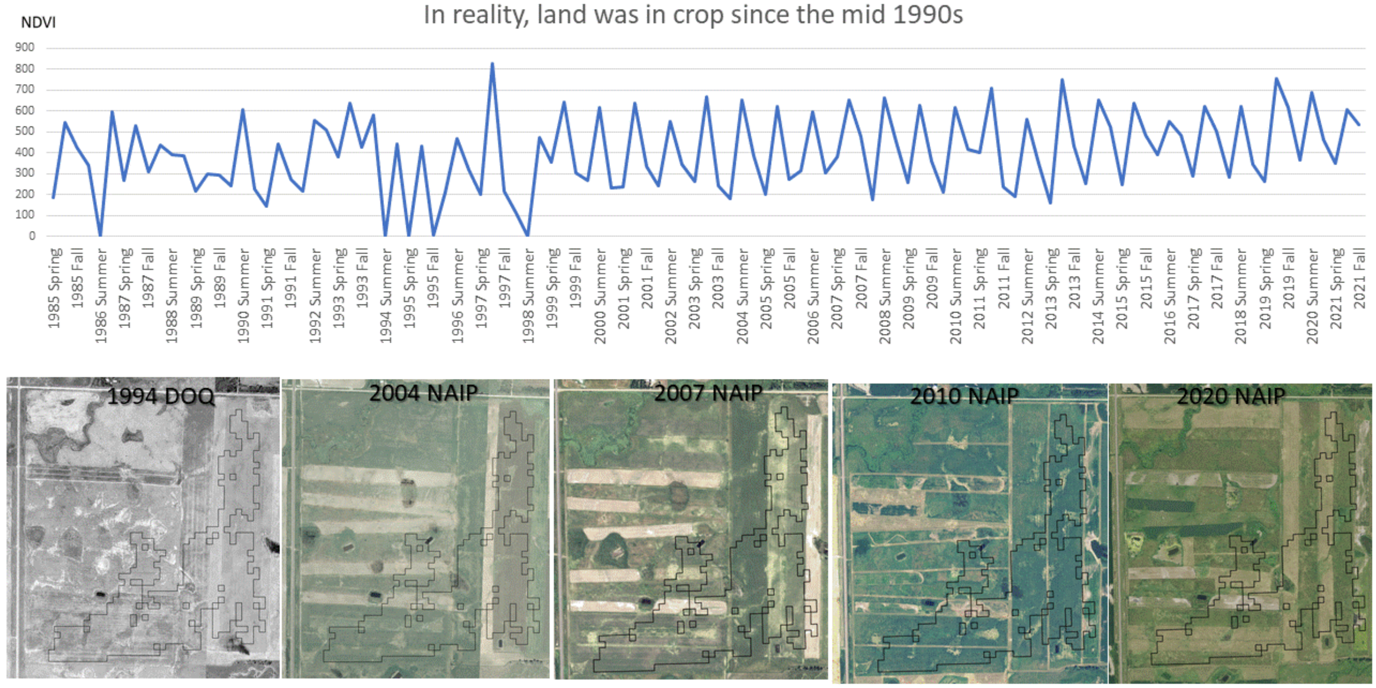
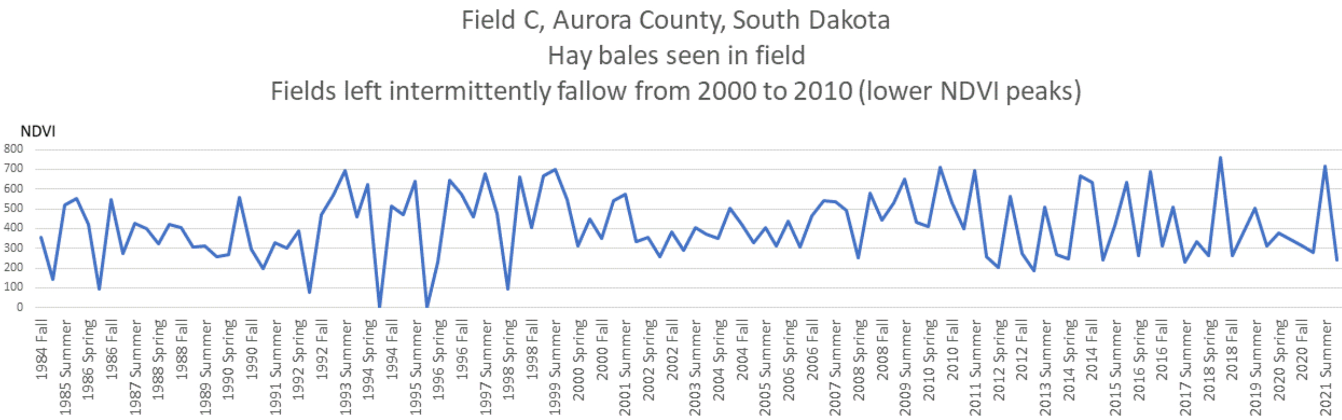
Appendix A: Analysis of the Lark et al.(a) Cropland Expansion Layer

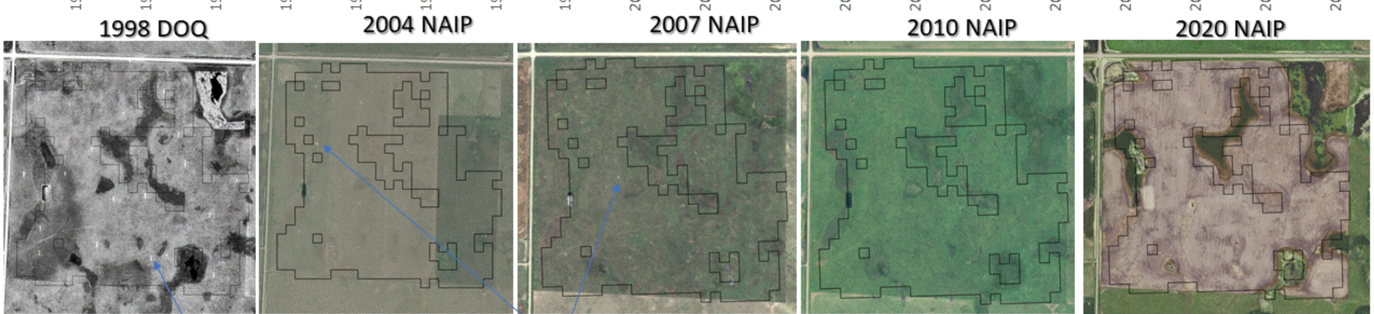
Field A, Aurora County, South Dakota
Lark et al predicted change to cropland in year 2010
In reality, land went fallow around year 2000 routed back into production ag in year 2008



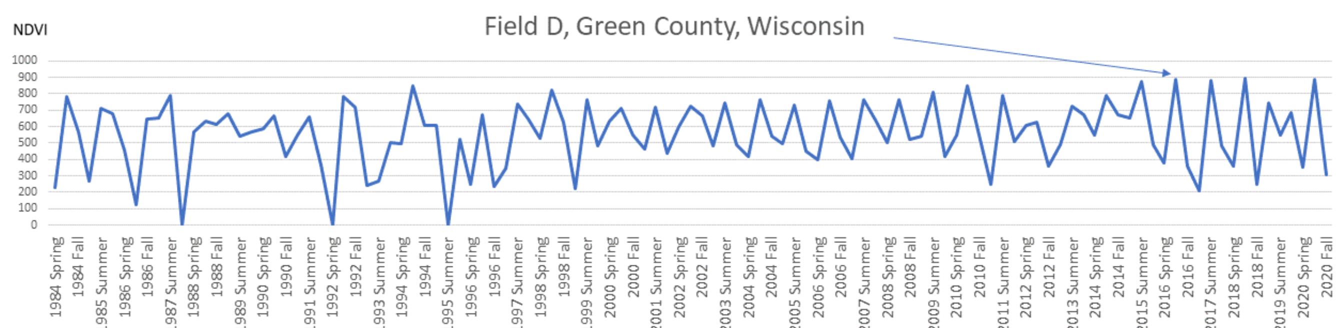
Field B, Aurora County, South Dakota,
Lark et al. predict change to cropland in year 2011
In reality, land was in crop since the mid 1990s



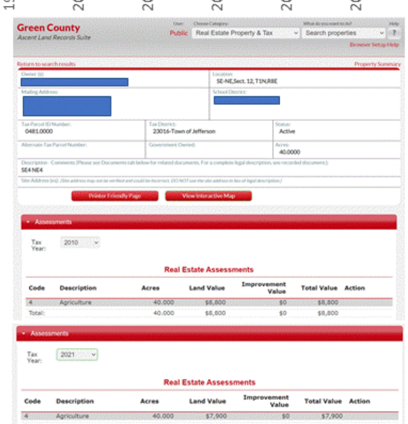




Hay bales can be seen in 1998, 2004 and 2007 images



Green County Tax Records have the parcel coded as Agriculture from 2010 (when digital records became available) to 2021. Change to cropland was predicted by Lark et al in 2016. NDVI indicates continuous ag use consistent with tax records.



Green County
Ascent Land Records Suite

User: Public | Choose Category: Real Estate Property & Tax | What do you want to do? Search properties | Help ?

Browser Setup Help

Return to search results | Property Summary

Owner (s): [redacted]
Location: SE-NE,Sect. 12, T1N,R8E
Mailing Address: [redacted]
School District: [redacted]
Tax Parcel ID Number: 0481.0000
Tax District: 23016-Town of Jefferson
Status: Active
Alternate Tax Parcel Number:
Government Owned:
Acres: 40.0000
Description - Comments (Please see Documents tab below for related documents. For a complete legal description, see recorded document.):
SE4 NE4
Site Address (es): (Site address may not be verified and could be incorrect. DO NOT use the site address in lieu of legal description.)

Printer Friendly Page | View Interactive Map

Assessments

Tax Year: 2010

Real Estate Assessments

Code	Description	Acres	Land Value	Improvement Value	Total Value	Action
4	Agriculture	40.000	$8,800	$0	$8,800	
Total:		40.000	$8,800	$0	$8,800	

Assessments

Tax Year: 2021

Real Estate Assessments

Code	Description	Acres	Land Value	Improvement Value	Total Value	Action
4	Agriculture	40.000	$7,900	$0	$7,900	
Total:		40.000	$7,900	$0	$7,900