Reply to Taheripour et al.: Comments on "Environmental Outcomes of the US Renewable Fuel Standard"

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Taheripour et al. recently posted comments on their websites about our peer-reviewed study published in the Proceedings of the National Academy of Sciences (Lark et al. 2022). In their commentary, the authors question several components of our work, which they conclude "resulted in overestimation of the GHG emissions of corn ethanol." We find Taheripour et al.'s conclusions to be unsupported and based upon several misunderstandings and misinterpretations of our methods and results. To help clarify, we offer the following replies which correspond to the 9 major points of Taheripour et al.:

1. **The land use changes we identify specifically represent the conversion of pasture and Conservation Reserve Program (CRP) lands to crops and thus are likely to cause a large carbon debt upon conversion.** In our study, we used USDA National Resources Inventory (NRI) data–not the Cropland Data Layer (CDL)–to estimate the types, amount, and regional locations of land converted to crop production due to corn ethanol and the RFS (see Lark et al. 2022, SI Appendix, ln 375-393). The USDA NRI dataset specifically identifies land as pasture or CRP, and such areas are known to result in a substantial carbon debt upon their conversion to cropland (Guo and Gifford 2002; Gelfand et al. 2011; Sanderman et al. 2017; Spawn-Lee et al. 2021).

   Taheripour et al. quote and appear to conflate our methods for estimating water quality impacts (Lark et al. SI ln 696) with our methods for identifying land transitions (SI ln 375) and also seem to misinterpret our use of satellite-based data, which we use only to infer at a higher resolution the biophysical characteristics of converted lands for our water quality and greenhouse gas modeling (SI ln 555). While the USDA NRI data allow us to identify the type, amount, and regional locations of land use changes, we do not know the exact location of those land use changes. Therefore, we used satellite-based data on land conversions only to help parameterize our water quality and greenhouse gas models at sub-regional levels.

2. **The Carbon Response Function (CRF) used by Spawn et al. (2019) which we apply to our study is appropriate for the types of land (e.g. CRP) converted.** We used the CRFs of Poeplau et al. (2011) as described in Spawn et al. (2019) to estimate the average soil organic carbon emissions per hectare associated with conversion of pasture and CRP. Taheripour et al. claim that the nature of this approach is discordant with our application since we find most conversion to be recultivation of lands leaving the CRP. However, further review of the underlying model of Poeplau et al. reveals that many of the grasslands in the training data upon which it is based have a history of disturbance–either cultivation, haying, or intensive grazing–and also a range of tillage intensities upon conversion, such that the model is highly consistent with the land to which we apply it. ***See Note A at the end of this reply for further details about the CRF, its validation, and its appropriateness for our application.***

3. **We do not double-count N20 emissions from fertilizer use changes due to land use change (LUC).** We designed our carbon intensity estimates to be compatible with the EPA's Regulatory Impact Analysis (RIA), which enumerates N20 emissions from fertilizer use in their estimates of baseline emissions from farming inputs (EPA RIA pg 321) and in their estimates of changes in emissions due to LUC (EPA RIA pg 361). Thus, we do not double count N20 emissions anywhere in the system when we replace the EPA's estimates of N2O emissions changes due to LUC with our estimates of N2O emissions changes due to LUC. Failure to account for the change in N20 from domestic LUC would omit an important source of greenhouse gas impacts. ***See Note B for description of the EPA RIA's accounting of N20 emissions.***

4. **Our results are fully consistent and reasonable.** Taheripour et al. reinterpret our results as county-level ratios to point out several supposed inconsistencies, but the results are completely sensible and should be expected. In our spatial estimates of carbon emissions due to LUC, we find negative emissions in locations where cropland area declines, which is consistent with ecological theory, our accounting method, and the example given by Taheripour et al. In our cropland transition results, we find that large changes in cropland area (i.e., the extensive margin) often occur in counties where there is substantial land area available for expansion (e.g., Great Plains states), regardless of which crop is grown or how much of it is corn. In our crop rotation results–which accommodate for crop substitution and geographically variable responses–we find that large increases in corn area (i.e., the intensive margin) often occur in counties that are already predominantly cropped and have limited natural lands available for expansion (e.g. Corn Belt states and the Mississippi Alluvial Valley region). The extensive and intensive responses often occur in different geographies, and thus the ratios between them may be large, particularly if one of the responses is close to zero. ***See Note C for further explanation and economic rationale.***

5. **Our attribution of ethanol volumes to the RFS2 considers other drivers of ethanol production.** As detailed in Carter, Rausser, and Smith (2017), the original RFS essentially mandated ethanol use at levels that would have been required anyway under the Clean Air Act once MTBE was banned. We model the incremental effect of the RFS2 over the original RFS. Thus, the volumes we attribute to RFS2 are additional to those created by the MTBE ban. Using the model in Babcock (2013), 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. Nevertheless, our results reflect the impacts of increased corn ethanol demand in general, regardless of the source of such increases.

6. **We carefully consider and account for both yield increases and DDG offsets.** 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.

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). We assume that each bushel of corn produces 2.8 gallons of ethanol and 18.67 pounds of DDGS (one third of a bushel), which is why we estimate that 5.5b gallons of ethanol displaces 1.3b bushels of corn. Our assumptions imply a net loss to the food and feed system of ⅔ of an acre for every acre of corn grown for ethanol. 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.

7. **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. Table S4 summarizes the data used to estimate the model.

   The observed and BAU prices for all 2006-16 crop years are presented in Fig. 1 of the main text, with the detailed outputs for our soybean and wheat models included in Tables S10-S13 and Figs. S12-S17. For the corresponding output for the corn model, see Tables 2-3 and Figures 5-7 of Carter, Rausser and Smith (2017). The AICC and BIC indicate that the corn, soybean, and wheat models fit best when using a single lag (Tables S10 and S12), and the impulse response functions conform to predictions of economic theory (Figures S13 and S16).

   Taheripour et al. report average annual price changes during 2008-2015, which are not the relevant statistics. We focus on the average price in the post-RFS2 period relative to our projection of that price under business-as-usual. Our Table S1 shows that corn, soybeans and wheat were 77%, 62%, and 62% higher on average in the 2006-2010 crop years than in the 2001-2005 crop years. Our model attributes less than half this price increment to the RFS2. The remainder is due to other market shocks. Thus, we agree with the conclusion of Filip et al. (2019) that "price series data do not support strong statements about biofuels uniformly serving as the main leading source of high food prices and consequently the food shortages." 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.

8. **Our modeling of land transitions purposely avoids the separate problematic category of Cropland-Pasture.** 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). Furthermore, the source of cropland-pasture data in the United States is the 5-year interval Census of Agriculture, where the category is a subjectively interpreted aggregate variable that has undergone significant definition changes (Bigelow and Borchers 2017) and measurement inconsistencies (USDA 2019; 2002) across time, further rendering it inappropriate for LUC assessment.

   The USDA National Resources Inventory, which we use to define land transitions, does not have a separate land use category for cropland-pasture since it is not explicitly identifiable within landscapes. The NRI instead classifies land into observable (and thus verifiable) land cover/use classes including cropland, pasture, rangeland, and CRP lands.

   Lastly, our methods do not impose additional demand for CRP to enter cropland by excluding a separate category for cropland-pasture. Instead, we estimate how transitions between CRP and cropland respond to changes in cropland returns based on historical data. To the extent that cropland-pasture is captured in our pasture and cropland transitions, it is unlikely to substantially affect our results since most of the estimated cropland conversions due to an increase in cropland returns are with CRP.

9. **Taheripour et al. significantly misunderstood the methods and results of our study.** We appreciate and welcome the input from Taheripour et al. and the opportunity to clarify several misinterpretations and inaccuracies in their comments, which appear to reflect a misunderstanding of our methodologies and results. We find that each of the major sections and associated concerns of Taheripour et al. is predicated upon a misconception or factually incorrect claim and that their resulting conclusions are thus also invalid and unfounded.

ADDITIONAL DETAILS:

Note A: Our Carbon Response Function (CRF), validation, and appropriateness for our application

Taheripour et al. claim that the nature of the carbon response function (CRF) we used to model soil organic carbon (SOC) emissions from grassland conversions is discordant with our application since we find most conversion to be recultivation of lands leaving the CRP. They argue that CRP lands have "been under [perennial] vegetation cover for only a few years" such that they are "likely to be less rich in soil carbon stocks than native or undisturbed grassland." However, the 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).

Taheripour et al. also highlight a caveat that we transparently noted in a previous paper (Spawn et al. 2019) describing the emissions model used in Lark et al. (2022). In it, we stated that "When training the CRFs used in this analysis, Poeplau et al. (2011) were careful to only consider data from sites where the natural landcover had not been previously disturbed by human activity. By applying these CRFs to soils that may have been previously cultivated, our approach may overestimate the conversion of some soils to conversion" (Spawn et al. 2019). At the time, we were restating Poeplau et al.'s own characterization of the suite of CRFs they derived, of which only those pertaining to grasslands were employed in Lark et al. (2022). Since making that statement about Poeplau et al.'s model more than 3 years ago, we have come to realize that such terms (e.g."natural" and "disturbed") are neither used consistently among studies nor are they an appropriate characterization of Poeplau's model, particularly in the context of grassland systems. Upon closer examination, numerous studies factored into the Poeplau et al. 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.

Finally, Taheripour et al. highlighted the validation that we previously conducted of our emissions model (Spawn et al. 2019). We seem to both agree that rigorous validation is imperative for assessing a model's predictions both individually and in relation to those of other models. Taheripour et al. extracted a subset of the data presented in Figure 4 of Spawn et al. (2019) to replicate the more detailed validation exercise that we already reported in section 3.4 of that paper. They conclude that our model's fit is "remarkably poor"; and, while we seem to agree that there is always room for improvement, it's unclear to what benchmark they feel our estimates are comparatively poor. Most prior US corn-ethanol LCAs have variously used one of just a handful of SOC emission factor (EFs) sources: the so-called "Woods Hole" (from Searchinger et al. 2008), "Winrock" (from Harris et al. 2008), or "AEZ" (from Plevin et al. 2014) databases or those derived using a modified and highly aggregated implementation of the CENTURY process-based model by researchers at the Argonne National Laboratory and included as the default option in the GREET model (Kwon et al. 2021). To our knowledge, none of these emissions factors have ever been rigorously validated against direct observations.

As they pertain to grassland conversions, the Woods Hole, Winrock, and AEZ EFs are based on coarsely aggregated tabulations of soil carbon inventories or maps and apply either the IPCC 2006 Tier 1 (most rudimentary IPCC method, and now outdated: see IPCC 2019) stock change factors (Harris et al. 2008, Plevin et al. 2014) or simply assume that 25% of the existing SOC stock is lost upon conversion of grassland to cropland (Searchinger et al. 2008). Because certain economic models used to estimate LUC areas include the 'cropland-pasture' land class—a class not commonly recognized in carbon inventories but defined by economists as land that variously cycles between cultivation and perreniality—these three sets of EFs simply assign conversion of cropland-pasture half the value assumed for 'grassland/pasture' conversions. Each of these rather rudimentary emissions factors are thus modeled predictions, and their representativeness could and should be verified against field observations at discrete points; yet, they have not been.

Likewise, the CENTURY-based emissions factors developed to represent forest, pasture (grassland), and cropland-pasture conversions in the default version of GREET have never to our knowledge been rigorously validated. In the case of 'cropland-pasture' conversions—the largest projected source of new cropland in GREET and presumably meant to include lands in the CRP given that CRP conversions are not accounted for elsewhere in GREET—they predict SOC *sequestration* when nearly all field observations (see discussion in Spawn-Lee et al. 2021), other emission factor options (IPCC 2006, Harris et al. 2008, Searchinger et al. 2008, Plevin 2014), and ecological theory (e.g. Janzen et al. 2022) suggest otherwise. In our review of the sequence of literature describing the development of GREET's default EFs (Kwon and Hudson 2010; Kwon et al. 2013; Qin et al. 2016; Kwon et al. 2017; 2021), we have not found any quantitative validation or a discussion of the anomalous nature of the cropland-pasture predictions.

Rather than a demerit, we feel that our transparent and quantitative validation exercise should instead be considered a standard of assessment. Unfortunately, there has too rarely been forthcoming discussion of the likely-high uncertainty associated with SOC in biofuel LCA because it is rarely reported. We encourage all modeling to assess and report uncertainty as a way of collectively advancing our understanding of biofuels' environmental posture. We believe that our treatment of SOC and our reporting of its uncertainty, while somewhat novel to certain LCAs of U.S. ethanol, is entirely defensible and appropriate. As such, rather than viewing any divergence from select other studies as a shortcoming, we encourage interpreting this discrepancy as emblematic of the uncertainty that persists regarding SOC's response to LUC and the potential implications that that may have for assessing the carbon balance of biofuels. Soil carbon dynamics remain a research frontier (Janzen et al. 2022; Zhang et al. 2021; Lehmann and Kleber 2015; Stockmann et al. 2013) and one should be skeptical of suggestions that it is somehow settled, certain, and well-captured by only a single modeling approach.

Note B: Accounting of N2O emissions in the EPA Regulatory Impact Analysis (RIA)

N20 emissions from LUC are not included in the EPA RIA's baseline estimates of emissions from farming inputs. As stated in the RIA, "The crop budgets represent an average for each region, and do not specifically calculate input or yield changes that could result from the use of marginal croplands or altered crop rotation patterns (e.g., continuous corn production)." (RIA pg 321, U.S. EPA 2010)

Instead, N2O emissions from LUC are singly accounted for in the RIA's estimate of GHG emissions from LUC. According to the RIA, "To calculate the annualized cumulative GHG emissions due to land use change for a specific fuel, we first summed all emissions associated with agricultural land (CO2 and N2O from cropland, pastureland, CRP land)…between the years 2000 and 2022 for the control and fuel-specific scenarios….We [then] included in the cumulative GHG emissions from land use change all emission streams due to changes that occurred between 2000 and 2022 for the thirty year time horizon (See Section 2.4.5) after 2022." (EPA RIA pp 361)

Note C: Explanation of geographic differences in intensive and extensive responses:

From an economic perspective, there is no particular reason that cropland expansion needs to occur in the same region as increases in corn acreage expansion. For example, farmers may switch from other crops to corn on existing cropland on the fringe of the Corn Belt and this leads to an increase in other crop prices and an expansion of cropland in areas where less corn is grown but more marginal land is available for expansion. Our reduced form models of changes in corn, soybean, and wheat prices implicitly capture these equilibrium effects across crops. Then these changes in prices drive the predicted change in cropland area. We do not need to prescriptively allocate or "reshuffle" crops across regions; instead, our econometric models empirically estimate the responses of cropland area and crop rotations to prices across geographies based on historical data on observed responses to prices.

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