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REPLY

Author's response to commentary on "Carbon balance effects of U.S. biofuel production and use"

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Abstract The impact of substituting biofuels for fossil fuels on carbon dioxide (CO_2) emissions has been debated for many years. A reason for the lack of resolution is that the method widely used to address the question, lifecycle analysis (LCA), is subjective. Its results irreducibly depend on untestable assumptions, notably those pertaining to system boundaries but also those for representing market effects. The best one can do is empirically constrain estimates of net CO_2 impact using data that characterize important aspects of the overall system. Our 2016 paper, "Carbon balance effects of U.S. biofuel production and use," took such an approach, using field data to estimate the direct CO_2 exchanges for a circumscribed vehicle-fuel system over the 2005–2013 period of expanding US biofuel use. De Kleine and colleagues criticize our work because it does not follow LCA conventions, arguing in particular for the primacy of the assumption that biofuels are inherently carbon neutral. This response refutes their critique; it reminds readers why the lifecycle paradigm fails for a dynamic system involving the terrestrial carbon cycle, stresses the need to bound an analysis of key carbon exchanges, and explains why the circular logic of LCA can be so beguiling.

Our recent paper, DeCicco et al. (2016), evaluated carbon exchanges between the atmosphere and the US energy and agricultural sectors over 2005–2013, a period of large growth in biofuel production and use. The method involved a straightforward estimation of annual CO_2 flows directly to and from a deliberately circumscribed vehicle-fuel system. The analysis emphasized material carbon flows, referring to the carbon chemically bound in the fuels and feedstocks, both fossil (crude oil) and biomass (corn, soybeans). This focus distinguishes carbon exchanges that can be estimated using available data from those that can only be projected through economic modeling. The results constrain (place a lower bound on) the net emissions of CO_2 and other greenhouse gas (GHG) emissions associated with US biofuel use over the period examined.

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We found that the increase in carbon uptake on cropland was enough to offset only 37% of the biogenic CO_2 emissions associated with biofuels over the 2005–2013 period. This result contradicts the assumption of 100% offset (inherent carbon neutrality) for biofuels that is built into the lifecycle analysis (LCA) methods commonly used to evaluate fuel GHG impacts. Our findings challenge the conclusions of the LCA-based literature that justifies promoting biofuels for climate mitigation. Once estimates for processing and economically induced emissions (including those from land-use change) are considered, our paper concludes that the US biofuel expansion has been associated with an increase in GHG emissions, rather than a decrease as found by many LCA studies.

Our results are criticized by De Kleine, Wallington, Anderson, and Kim (2017a, "DWAK," this issue) and in another article, De Kleine et al. (2017b) that critiqued an earlier report (DeCicco and Krishnan 2015). Related objections to work leading up to our paper were given by Wang et al. (2015). DWAK's concerns are that our analysis:

- Evaluates the offset of biofuel combustion CO_2 emissions by applying a test, based on the additionality of net ecosystem production (NEP), that is not meaningful
- Relies on estimates of NEP from agricultural statistics, an approach that is not robust
- Uses inconsistent spatial definitions (system boundaries)
- Chooses a selective time frame for evaluation
- Omits heterotrophic respiration (R_h) in food and feed systems
- Fails to include key consequential and economic effects, and
- Neglects improvements in agronomy and agricultural processing efficiency

In general, DWAK fault our analysis for failing to conform to established LCA guidelines. They claim that their arguments re-affirm that biofuel use is inherently carbon neutral, as assumed in traditional lifecycle assessments, and that LCA remains the best approach for evaluating the GHG emissions impacts of biofuels.

1 Why lifecycle analysis fails for biofuels

LCA traces material and energy flows, presumed to be part of a steady-flow process, through product production and consumption chains. Our decision to eschew LCA and its conventions is deliberate and follows from the recent literature that highlights the uncertainties, limitations, and risks of applying the method to biofuels. Key papers include: McCarl (2008); Cherubini et al. (2009); Johnson (2009); Melillo et al. (2009); DeCicco (2012, 2013, 2015); Delucchi (2010, 2011, 2013); Mullins et al. (2011); Plevin et al. (2014, 2017); and McManus and Taylor (2015). The error due to the bioenergy carbon neutrality assumption was highlighted by Searchinger et al. (2009) and Haberl et al. (2012). Searchinger (2010) identified the need to assess additionality for biofuel feedstocks, a test absent in LCA modeling.

LCA's major limitation is that it is a static method of analysis that by construction treats biogenic carbon flows as if they are in balance over the defined "lifecycle." However, that is an incorrect depiction of the situation because biofuel production engages the dynamic flows of the terrestrial carbon cycle (DeCicco 2013; Haberl 2013). Correct analysis must capture the system dynamics and reflect both the time-varying nature of key flows and the initial conditions of the system. Bruce McCarl put it well when he described how the LCA framework is ill-suited for addressing major carbon-related effects of biofuel production and concluded that “Perhaps we should leave the lifecycle behind and do more systems analysis” (McCarl 2008: 54).

DWAK fail to cite much of this literature and do not address system dynamics in any coherent way. They make definitional arguments, relying on relational diagraming not backed by mathematical rigor and disconnected from the tenets of terrestrial ecology, even though they invoke some of its concepts such as NEP. However, one need not use ecological terminology to contest the assumption that biofuel use is inherently carbon neutral. It is simply a matter of being attentive to real-world carbon flows and conservation of mass.

2 Focusing on the key carbon flows

Changes in the atmospheric CO_2 concentration result from a difference between the inflow (CO_2 emissions from combustion) and outflow (net CO_2 uptake on land or water). As a matter of chemistry, substituting a biofuel for a fossil fuel does not reduce CO_2 emissions; the rate at which carbon flows into the atmosphere does not appreciably change. Any mitigation effect requires an increase in the net rate at which carbon flows out of the atmosphere. For an increase in CO_2 uptake to be physically tied to a biofuel (as opposed to being caused by an indirect effect), it must occur during production of the biofuel’s feedstock. Conservation of mass (carbon) dictates that the increase in net uptake must be large enough to match the rate of CO_2 emissions from biofuel combustion in order to achieve carbon neutrality (i.e., a 100% offset of the biogenic CO_2 emissions).

A way to visualize the situation is to take the perspective of the atmosphere, gazing down on activities on the Earth’s surface and observing CO_2 flows both upward and downward. Focus on the flows to and from the vehicle-fuel system at US continental scale; that is the system for which a claim is made that substituting biofuels for petroleum fuels neutralizes the CO_2 emitted from motor vehicles. The atmospheric observer sees no change in the rate as which CO_2 flows from vehicle tailpipes when the biofuel is burned instead of the fossil fuel. So the question is to what extent the atmosphere observes an increase in the net downward flow of CO_2 into the fuel supply system, of which cropland is a part when biofuels are in play. To first order, there is little change in the downward CO_2 flow because the cropland has already been copiously absorbing CO_2. Nevertheless, a close look over the 2005–13 period reveals a somewhat more rapid CO_2 down flow, largely because more corn was planted and corn is among the most productive of crops. However, when measured, that increase in downward CO_2 flow is not enough to fully balance out the portion of CO_2 emissions from biofuel use. Although in DeCicco et al. (2016) we also examined the various complicating factors related to processing emissions and land-use change, this thought experiment crystallizes the essence of our analysis.

3 The need for a circumscribed spatial boundary

Various interpretations of the definition of NEP and how to apply it do not change these basics of carbon flow. But confusion can ensue when discussing NEP without sufficient care. DWAK say that we use “inconsistent spatial definitions” because we do not include the heterotrophic respiration (R_h) that occurs in the food (including feed) ecosystems to which harvested carbon is exported from cropland. They say that if this R_h is included, then increased NEP on cropland is “exactly matched” by decreased NEP (due to decreased R_h) in food systems. Then, in reference to biofuels produced from biomass diverted from food use, they argue that this decreased food system R_h leads to an increase in NEP, acknowledging that the effect is “perhaps not optimal from economic or moral perspectives.” Although they do not cite it, DWAK therefore seem to agree with the Searchinger et al. (2015) conclusion that a significant portion of any net global CO_2 emission reduction tied to biofuels is due to deprivation, i.e., reduced caloric consumption in the global food and feed system.

In any case, a decrease of R_h in the non-cropland system does not imply that the cropland-to-biofuel carbon flow is carbon neutral; indeed, it suggests that the cropland-to-biofuel pathway itself involves less than a 100% offset. DWAK assert biofuel carbon neutrality by leaving the spatial boundary of cropland and entering the global food consumption system, confusing the meaning of NEP on cropland with one (small) part of global NEP. They also include biofuel combustion CO_2 emissions in the component of NEP that gets oxidized non-biologically (Ox_{nb}), again leaving the spatial boundary of the cropland ecosystem. DWAK thereby confuse local and global scopes of analysis and invoke the global accounting truism that all biogenic carbon flows balance out to argue that “the production of biofuel *by definition* leads to an exactly equal increase” in NEP.

If there is an increase in NEP, exactly where, when, and by how much does it occur? That is an empirical question and the one that our work sought to answer. NEP is a flow (commonly measured on an annual time scale, netting out the seasonality of plant growth) and so an increase in NEP is the time derivative of a flow. DeCicco (2013) formally demonstrated the need for a gain in NEP as a necessary (but not sufficient) condition for biofuel production to have a CO_2 mitigation benefit, i.e., the requirement that

$$\mathrm{d(NEP)/dt} > 0 \tag{1}$$

This additionality test is the reason why we need to measure changes in NEP.

DWAK’s statement that the NEP increase (or part of it) occurs somewhere in the global food system is nebulous. Careful analysis requires a clearly circumscribed temporal and spatial scope for evaluating carbon flows. If the biofuel combustion emissions are to be offset by carbon uptake in biofuel feedstock, then annual cropland data address the “when” and “where” parts of the question and enable estimation of “how much” increase in NEP is associated with biofuel production itself. That is why we use US cropland as the spatial area over which we evaluate NEP and do so one year at a time, a system boundary definition consistent with the need to evaluate the additionality of carbon uptake in biofuel feedstocks.

As discussed in the method section of DeCicco et al. (2016), a good estimate of NEP on annual cropland is provided by amount of carbon in the biomass harvested each year. Estimates so obtained are as reliable as the harvest data, for which we used the standard crop production database (USDA 2015). DWAK say reliance on such data is not robust because the results change from year to year; their related criticism is that our results depend on the time period of analysis. The results do indeed depend on the period analyzed, because bioenergy systems are dynamic, not static. As our paper pointed out, its quantitative result—an estimated biogenic carbon offset of 37% over 2005–13—“is for a specific period of time and so makes no claim to offer a general characterization of corn ethanol” (DeCicco et al. 2016: 676). Table 1 of that paper details the annual estimates; these time-varying flows underscore McCarl’s recommendation to use systems analysis methods rather than LCA.

Using our tabulated values, DWAK say that a different choice of base year would greatly change the estimated offset, making it look more favorable; yes, that is true, and it is also true a different choice of end year would make the offset look worse. The point is that there is no clear-cut numerical answer to the question of "what is the carbon intensity of a biofuel?" As popular as that line of inquiry may be, it is scientifically ill-posed. It is a highly stylized question based on the idea that a fuel lifecycle can be defined objectively, a belief that is incorrect for fuels obtained from the biosphere. It also disregards the International Standards Organization's admonition that "there is no scientific basis for reducing LCA results to a single overall score or number, since weighting requires value choices" (ISO 2006: 9).

DWAK's criticism of our decision to not analyze the consequential effects of biofuel production is without merit. The very large and irreducible uncertainties that arise from such modeling are what led us to parse out the parts of the problem that can be constrained using field data. As we stated, our work is a bounding analysis; although we did not perform new consequential analysis, we considered the relevant literature, which shows that such effects increase the net GHG impact of biofuel production because they are dominated by carbon stock releases from land-use change. Similarly, we do not model prospective improvements in agronomy and agricultural processing efficiency; our study is retrospective and relies on EPA (2010) projections of agricultural system characteristics for 2012 (a reasonable choice for our 2005–2013 analysis period given the quintennial series of parameter estimates published by the agency). It therefore captures whatever efficiency gains may have transpired during that time, at least within the certainty levels of the largely unaudited process data on which EPA relied.

4 Explaining the appeal of a flawed paradigm

A reason why some analysts may find it difficult to accept our findings is a paradigm that took root following the 1970s energy crisis and concerns about resource depletion. A dedicated community of researchers embraced the vision of replacing finite fossil resources with boundless energy from the sun, captured directly through solar power and indirectly through biomass, wind, and other renewable energy technologies. Such options are called sustainable energy systems based on the belief that they will provide a secure supply of energy for future generations, in contrast to the irreversible and conflict-prone consumption of fossil fuels. A linchpin of the paradigm is contrasting a linear material flow ("cradle-to-grave") with a circular material flow ("cradle-to-cradle"), modeled using the notion of a product lifecycle. As global warming rose in importance, these concepts were applied to the problem of rising CO_2 concentrations. The atmosphere is seen as the "grave" for the CO_2 waste product of fossil fuel use. The hope is that the atmosphere can become a "cradle," a source of renewable carbon for producing fuels or other carbon-based materials within a bioeconomy that is a key part of the circular economy vision. LCA codifies these contrasting but stylized notions of material and energy flows, and by extension, carbon flows. Nevertheless, these views are notional rather than experiential, and so modeling based on them is subject to test.

When carbon is the material in question, one must not forget the circulation of carbon that is always underway between the biosphere and atmosphere. Carbon is the fuel of life, and the use of bioenergy is not just the substitution of an idealized circular flow for the linear flow of fossil carbon from underground into the atmosphere. Rather, it is a perturbation of the existing circulation. That large-scale biogenic flow is not in a "sustainable" equilibrium and has not been since humans began extensively appropriating net primary production ages ago. Even if the biospheric carbon cycle were in equilibrium, impacting it at the scale necessary to replace any significant part of the fossil carbon flow is itself a dynamic process and cannot be correctly analyzed by comparing two steady-flow systems as done by LCA methods.

In a paper that helped establish the now-conventional LCA approach for analyzing biofuels, Marland and Turhollow (1991) wrote that "in a sustainable agricultural system, there is no net CO_2 flux to the atmosphere" from biomass combustion. This statement is telling in retrospect because of its premise of a *sustainable* system and the widespread view that certain systems are sustainable by definition, as managed cropland systems are often considered to be at least in terms of biogenic carbon (sustainability complaints about agriculture largely focus on other impacts). Bioenergy cannot be made climatically sustainable by fiat or belief. Our evaluation of the US 2005–2013 biofuel expansion not only shows the lack of balance in the direct, fuel-related biogenic carbon flows but also shows just how misleading LCA can be when inappropriately applied to activities that engage the terrestrial carbon cycle.

Attempting to refute our work by recourse to the faulty lifecycle logic that our method was designed to avoid is itself but a form of circular reasoning. Our critics seem unable to let go of the assumption of a stylized circular carbon flow for biofuels, as embodied by construction in the LCA methods they espouse and in their recourse to a tautology of global carbon accounting. De Kleine et al. fail to raise any scientifically substantive objections to our paper, displaying the power of a false paradigm to blind researchers to seeing the world for what it really is.

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