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**EXTERNAL PANEL PEER REVIEW OF EPA’S DRAFT
“BIOFUELS AND THE ENVIRONMENT: THIRD
TRIENNIAL REPORT TO CONGRESS”**

FINAL PEER REVIEW SUMMARY REPORT

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1.0 INTRODUCTION

This report documents the results of an external independent peer review of the U.S. Environmental Protection Agency's (EPA's) draft "Biofuels and the Environment: Third Triennial Report to Congress." ERG, a contractor to EPA, organized this review and developed this report. The peer review included a virtual meeting that was open to the public as observers and included an opportunity for oral public comment.

Section 1.0 provides background about the review. Section 2.0 provides a high-level summary of reviewer comments, including areas of agreement and disagreement. Section 3.0 presents reviewer final individual written post-meeting comments exactly as submitted, without editing or correction of typographical errors (if any). Appendices A, B, C, and D respectively, provide the list of reviewers, EPA's charge to reviewers, the peer review meeting agenda, and the oral public comments made at the meeting.

1.1 Background

Under Section 204 of the 2007 Energy Independence and Security Act, EPA is required to submit, on a triennial basis, a report to Congress on the impacts to date and likely future impacts of the Renewable Fuel Standard Program on:

- Environmental issues, including air quality, effects on hypoxia, pesticides, sediment, nutrient and pathogen levels in waters, acreage and function of waters, and soil environmental quality.
- Resource conservation issues, including soil conservation, water availability, and ecosystem health and biodiversity, including impacts on forests, grasslands, and wetlands.
- The growth and use of cultivated invasive or noxious plants and their impacts on the environment and agriculture.
- The annual volume of imported renewable fuels and feedstocks for renewable fuels, and the environmental impacts outside the United States of producing such fuels and feedstocks.
- Recommendations for actions to address any adverse impacts found.

EPA published *Biofuels and the Environment: First Triennial Report to Congress*,¹ the first report in the Section 204 series, in December 2011, and *Biofuels and the Environment: Second Triennial Report to Congress*,² the second report, in June 2018. The draft of the third report in this series, *Biofuels and the Environment: Third Triennial Report to Congress* (RtC3), was the subject of this peer review.

1.2 Peer Review Process

During the first stage of the peer review process, ERG assembled a list of experts interested in serving as reviewers, who collectively spanned nine areas of expertise relevant to this peer review: (1) land use change and remote sensing; (2) agronomy (including tillage practices, water use, and chemical applications [including nutrients, pesticides, and herbicides related to biofuel feedstock production]); (3) agricultural economics, including partial and general equilibrium economic modeling; (4) hypoxia and other impairment of water quality related to feedstock and biofuel production; (5) soil science; (6) air quality; (7) ecology,

¹ U.S. EPA. *Biofuels and the Environment: First Triennial Report to Congress* (Final Report, 2011). U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-10/183F, 2011.

² U.S. EPA. *Biofuels and the Environment: Second Triennial Report to Congress* (Final Report, 2018). U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-18/195, 2018.

ecosystems, and biodiversity; (8) biofuel conversion processes; and (9) international biofuel trade and international impacts of biofuel production.

To identify candidates, ERG used standard search processes and considered experts nominated by the public in response to a Federal Register Notice (FRN) requesting nominations. After considering public comments on these candidates submitted in response to a second FRN, ERG narrowed the list of experts to a final pool of 19 experts from which to select reviewers for this review. ERG selected nine experts who (1) collectively, had the strongest breadth of expertise across the nine areas listed above, and (2) confirmed they had no conflict of interest for this review:

- Sarah C. Davis, Ph.D.
- Harry de Gorter, Ph.D.
- Bernard A. Engel, Ph.D., P.E.
- Jason D. Hill, Ph.D.
- S. Kent Hoekman, Ph.D. (Panel Chair)
- Stephen R. Kaffka, Ph.D.
- Lyubov A. Kurkalova, Ph.D.
- Douglas A. Landis, Ph.D.
- Aaron Smith, Ph.D.

See Appendix A for a more detailed list of reviewers.

ERG provided reviewers with the draft RtC3 review document and with EPA's charge to reviewers (Appendix B). Working individually, each reviewer prepared written pre-meeting comments in response to the charge questions as relevant to their areas of expertise, and ERG compiled and distributed these preliminary comments to all reviewers a few days prior to the peer review meeting to help them prepare for discussions at the meeting.

ERG organized and facilitated a virtual peer review meeting, which took place via Zoom.gov on February 24, 27, and 28, 2023. Appendix C provides the meeting agenda. The meeting was open to members of the public to attend as observers and provided an opportunity for the public to make oral comments. The commenters also submitted written versions of the oral public comments they made at the meeting, which are provided in Appendix D. During the meeting, reviewers discussed and commented on EPA's draft RtC3, with discussion structured by EPA's charge questions and by document chapter. After the meeting, reviewers provided their individual final comments (Section 3.0) to ERG and ERG prepared a high-level summary of reviewer comments by charge question and document chapter (provided in Section 2.0 below).

2.0 SUMMARY OF REVIEWER COMMENTS BY CHARGE QUESTION AND CHAPTER

This section provides a high-level summary of reviewer comments, including areas of agreement and disagreement. Comments are summarized by charge question (with EPA charge questions shown in *italic* font, for reference). Note that questions were assigned to reviewers based on their relevant expertise, thus for many questions a subset of reviewers provided responses. For the full text of all reviewer post-meeting comments and for additional details on the comments referenced here, see Section 3.0.

2.1 Charge Question 1

Charge question 1 (CQ1) pertained to “Part 2: Attribution to the Renewable Fuel Standards (RFS) Program” of the report, comprised of Chapters 6 and 7. This charge question included a background section (see textbox below) plus five specific questions (a, b, c, d, and e) for reviewers to respond to. Reviewer CQ1 responses are summarized below by chapter.

Charge Question 1 Background. *In the First and Second Triennial Reports to Congress on Biofuels (RtC1 and RtC2, respectively), the Agency could not separate the effects of the RFS Program from the effects of other factors (e.g., market or other policy effects). Many factors simultaneously influenced the production and use of domestic corn ethanol in the U.S., including the need for fuel oxygenates to replace methyl-tert-butyl-ether (MTBE) in gasoline, the Volumetric Ethanol Excise Tax Credit (VEETC), high oil prices, and dozens of individual state biofuel programs and MTBE bans over this period. Because attribution was identified as a major knowledge gap in previous reports, this report includes a new emphasis on attribution, referred to in this report as an “attribution analysis.” Part 2 in this report (Chapters 6 and 7) covers the attribution analysis of the RtC3.*

2.1.1 Chapter 6 (Attribution: Corn Ethanol and Corn)

All nine reviewers commented on Chapter 6 in response to CQ1.

a) Please describe your overall impressions of the clarity and technical accuracy of material presented in the chapter.

- Eight of the nine reviewers found the overall chapter to be generally clear and technically accurate, and the ninth reviewer (Hill) commented that the “material presented in this chapter is neither clear nor technically accurate.” Several reviewers provided additional comments and/or suggestions:
 - Davis noted the absence of advanced biofuels in the discussion.
 - de Gorter disagreed with two of the report’s reasons why most of the ethanol consumed in the U.S. was not due to the RFS Program, stating that he did not agree with the conclusion on the impact of free market production, and that “D6 RIN prices do not reflect the ethanol price premium above the tax credit that free market production would have occurred on its own.” He also questioned the report’s conclusion that the RFS Program caused up to 2 billion gallons of extra ethanol in 2016 and provided his reasoning in his comments.
 - Smith commented that, “the chapter as a whole presents a skewed view of the evidence. By leaving out or dismissing some highly relevant literature, it underestimates the effects of the RFS on prices and land use.”
- Hill stated that the “modeling choices are incorrect and conflict with the methods recommended in a recent National Academies of Sciences, Engineering, and Medicine (NASEM) report. Furthermore, the conclusions drawn in this chapter are unsupported by the data.”

b) Are the most important data and recent relevant literature cited and appropriately interpreted?

- Three reviewers (Engel, Hoekman, Kurkalova) agreed that the most important data and recent relevant literature had been cited and appropriately interpreted.
- Five reviewers (Davis, de Gorter, Landis, Smith) mentioned studies or topics missing from the analyses that should be included.
- Hill commented that this chapter “should be heavily revised and rewritten to align with the conclusions of the NASEM’s recent report “Current Methods for Life Cycle Analyses of Low-Carbon Transportation Fuels in the United States” further stating that the attributional approach should not be used to understand the consequences of a proposed decision or action such as RFS2.
- Kaffka did not respond to CQ1(b).

c) Please comment on the selection and use of models (e.g., econometric models, general and partial equilibrium models), the accuracy of analyses, and accuracy of inferences.

- Four reviewers (Davis, Engel, Kaffka, Kurkalova) agreed with the study’s model selection and analyses.
- Hill described “four fundamental problems with the modeling performed in this chapter” including: an incremental approach should not be used; important factors such as reduction of risk are excluded; the subtractive approach is inappropriate; and important consequential effects such as on fuel and feed markets are neglected.
- de Gorter expressed concern that the situation is too complex for the approach, while acknowledging that “a whole new research agenda is required to untangle what would have happened.”
- Smith suggested that more emphasis be placed on econometric studies over simulation models.
- Hoekman and Landis did not comment on CQ1(c), indicating modeling was outside their area of expertise.

d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement.

- Eight reviewers generally agreed that, for the most part, the conclusions and recommendations described in the “Chapter Synthesis” are supported by the material presented.
 - Most of these reviewers qualified their agreement and/or provided suggestions for improvements.
- Hill disagreed that the conclusions and recommendations are supported.

e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.

- All reviewers noted that, to some extent, the chapter identified the uncertainties and limitations associated with its conclusions.
- Four reviewers (Davis, Hill, Landis, Smith) further commented on issues they identified with uncertainties or limitations.

2.1.2 Chapter 7 (Attribution: Biodiesel and Renewable Diesel)

Six reviewers (de Gorter, Hill, Hoekman, Kurkalova, Landis, Smith) provided comments on Chapter 7 in response to CQ1. One reviewer (Kaffka) did not respond to the CQ1 subparts but commented extensively on Chapter 7's text and figures (see detailed comments in Section 3.0 of this report). Two reviewers (Davis and Engel) did not comment on Chapter 7.

a) Please describe your overall impressions of the clarity and technical accuracy of material presented in the chapter. Five of the six (Hill, Hoekman, Kurkalova, Landis, Smith) reviewers that provided responses to this question found the chapter to be clear and technically accurate. One reviewer (de Gorter) commented that the chapter is “somewhat ambiguous” and provided specific suggestions for improving the presentation of data in the chapter.

b) Are the most important data and recent relevant literature cited and appropriately interpreted? Five of the six reviewers (Hill, Hoekman, Kurkalova, Landis, Smith) that responded to this question agreed that the most important data and literature had been cited. In his written comments, the other reviewer (de Gorter) noted that the chapter omits important literature and provided the reference in his written comments (see Section 3.0), noting that it “must be integrated into the analysis.”

c) Please comment on the selection and use of models (e.g., econometric models, general and partial equilibrium models), the accuracy of analyses, and accuracy of inferences.

- Hill and Kurkalova agreed with the approaches selected.
- de Gorter commented that while the chapter does not address how bio-based diesel impacts prices of soybeans and soybean oil, studies on this topic are analyzed in more detail in Chapter 16 and Chapter 4.
- In Smith's response, he questioned why Chapter 4 includes sections on impacts from the RFS program when the point of Chapters 6 and 7 is to explicate these impacts.
- Hoekman and Landis did not comment in response to CQ1(c), indicating modeling was outside their area of expertise.

d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement. All six of the reviewers that provided responses agreed that the conclusions and recommendations described in the “Chapter Synthesis” are supported by the material presented. Three of these reviewers (de Gorter, Kurkalova, Landis) qualified their agreement with a comment or exception.

e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.

- Four of the six reviewers that provided responses (Hill, Hoekman, Kurkalova, Landis) agreed that the chapter identified the uncertainties and limitations associated with its conclusions.
- Kurkalova and Landis also included a caveat or suggestion.
- de Gorter noted that a “big shortcoming” of the chapter is a robust discussion on the future of renewable diesel, and its implications for the RFS and the biofuel market.
- Smith commented that likely future effects should address renewable diesel.

2.2 Charge Question 2

Charge question 2 (CQ2) pertained to “Part 3: Environmental and Resource Conservation Issues” of the report, comprised of Chapters 8 to 16. This charge question included a background section plus six specific questions (a, b, c, d, e, and f) for reviewers to respond to (see textbox below). Reviewer CQ2 responses are summarized below by chapter. To enhance readability, an abbreviated title is used for each CQ2 subpart.

Charge Question 2. *In Part 3, Chapters 8 through 16 characterize scientific evidence on potential impacts of biofuel production on air quality, soil quality, water quality, water use and availability, terrestrial ecosystem health and biodiversity, aquatic ecosystem health and biodiversity, wetland ecosystem health and biodiversity, invasive or noxious plant species, and international effects, respectively. For each chapter you review:*

- a) Please describe your overall impressions of the clarity and technical accuracy of the material presented in the chapter.*
- b) Are the most important data and recent relevant literature cited and appropriately interpreted? Have any important studies that would affect the conclusions been omitted? Please identify any data, literature, or studies you think should be added and describe why you recommend they be included.*
- c) Please comment on the selection and use of models where applicable to a given chapter (e.g., GREET, BEIOM, EPIC, SWAT), the accuracy of analyses, and accuracy of inferences.*
- d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement.*
- e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.*
- f) Are the graphics, glossary, and other supporting material useful in clarifying and supporting the discussion and conclusions in the chapter (and supporting Appendices, if any)? Please describe any suggestions for improvement.*

2.2.1 Chapter 8 (Air Quality)

Three reviewers (Davis, Hill, Hoekman) commented on Chapter 8 in response to CQ2.

- a) Overall impressions of clarity and technical accuracy.* Davis and Hoekman commented that the chapter is accurate, however, they both provided suggestions to improve the chapter’s clarity and/or accuracy. Hill found the technical accuracy to be deficient.
- b) Data and literature citation and interpretation.*
 - Davis commented that advanced biofuels, including biogas and cellulosic biofuels, are underrepresented in the chapter even though “they are very relevant to the RFS and potential future scenarios for air quality.”
 - Hill provided comments on unsupported statements in the chapter and on the omission of a 2021 study that shows 1,200 deaths from corn ethanol.

- Hoekman agreed that the most important data and relevant literature had been cited and interpreted appropriately in Chapter 8, but he suggested that “any data that distinguishes between biodiesel and renewable diesel should be noted.”

c) Selection and use of models.

- Davis found the models to be reasonable selections and added a suggestion for improvement.
- Hill did not agree with the model selection, commenting that the GREET model does not provide geographic resolution and does not track ammonia, which he noted is a major contributor to atmospheric PM_{2.5}.
- Hoekman commented that the models used in EPA’s “anti-backsliding study” are not identified in the text and suggested adding this detail to the chapter.

d) Conclusions and recommendations.

- Davis commented that the summary “leads the reader to perceive a large increase in PM_{2.5} from ethanol relative to conventional fuels,” which “seems overstated.”
- Hill agreed with the conclusions and recommendations, with the exception of those related to his above comments that the model selected does not provide geographic resolution and does not track ammonia; that the report includes the unsupported statements; and it omits a 2021 study.
- Hoekman agreed that the chapter’s conclusions and recommendations are supported by the material presented but also suggested that EPA develop more quantitative estimates of biofuels’ impacts on specific air pollutants.

e) Uncertainties and limitations.

- Davis commented that the uncertainty around the impact of cellulosic sources on emissions is stated as though the emissions are expected to be “very large,” but this was not clearly addressed in the chapter and evidence for the opposite was suggested.
- Hill largely agreed that the uncertainties and limitations are identified but commented that the limitations he described related to the literature and models used should be noted.
- Hoekman agreed that the most important uncertainties and limitations are identified and provided a suggestion for clarification.

f) Usefulness of the supporting materials.

- Davis made several suggestions to improve the presentation of the information.
- Hill agreed that the supporting materials are useful in clarifying and supporting the discussion and conclusions in the chapter.
- Hoekman agreed that most of the supporting materials are useful but provided several suggestions for improvements.

2.2.2 Chapter 9 (Soil Quality)

Four reviewers (Davis, Engel, Kurkalova, Landis) commented on Chapter 9 in response to CQ2.

- a) Overall impressions of clarity and technical accuracy. Three reviewers (Engel, Kurkalova, Landis) found the material in the chapter to be clear and technically accurate. One reviewer (Davis) commented that the chapter is mostly accurate, but certain topics, such as corn stover and biogas, deserve greater detail. Her written comments describe the information that she suggests EPA add to the report.

b) Data and literature citation and interpretation.

- Two reviewers (Engel, Kurkalova) agreed that the most important and relevant data are presented, but each provided additional considerations for EPA.
- Engel commented that the plant growth data in the models are out of date.
- Davis noted areas where she suggested including more information.
- Landis provided numerous additional references for EPA's consideration.

c) Selection and use of models.

- All four reviewers agreed with the models selected and most reviewers also provided additional considerations related to the modelling in the report, including:
 - Davis questioned why DayCent model results were not considered in the analysis.
 - Engel suggested that EPA include improved crop growth parameters.
 - Kurkalova suggested adding references to the model's history and capabilities.

d) Conclusions and recommendations. Three of the four reviewers (Engel, Kurkalova, Landis) agreed that the conclusions and recommendations are supported by the material presented and they also provided suggestions for improvement. Davis commented that more information on biofuels is needed.

e) Uncertainties and limitations.

- Three reviewers (Engel, Kurkalova, Landis) agreed that the uncertainties and limitations are clear and appropriate. Engel and Kurkalova also both provided suggestions for improvements.
- Davis commented that uncertainty around the comparison with petroleum-related impacts to soil should be addressed.

f) Usefulness of the supporting materials. Three reviewers (Davis, Kurkalova, Landis) agreed that the supporting material is useful in clarifying and supporting the discussion and conclusions in the chapter. Davis and Engel also provided suggestions for improvement.

2.2.3 Chapter 10 (Water Quality)

Four reviewers (Engel, Hill, Hoekman, Kaffka) commented on Chapter 10 in response to CQ2.

a) Overall impressions of clarity and technical accuracy.

- Three reviewers (Engel, Hill, Hoekman) found the material in the chapter to be clear and technically accurate. Additionally:
 - Hoekman added that, "the issue of indirect water quality impacts should at least be mentioned."
 - Kaffka suggested that EPA clearly state in "Key Findings" at the beginning of the chapter why the report must be tentative about some conclusions, based on the limited number of long term studies.

b) Data and literature citation and interpretation.

- Three reviewers (Engel, Hoekman, Kaffka) agreed that the most relevant citations and data are largely included. Kaffka commented that some studies were overlooked, and he provided additional citations.
- Hill commented that "Section 10.3.3, as it relies on Chapter 6, needs to be revised."

c) Selection and use of models.

- The three reviewers (Engel, Hill, Kaffka) that responded to CQ2(c) agreed that the conclusions generally follow the data presented in the chapter, however, two of these reviewers provided additional comments:
 - Engel commented that the models selected were appropriate but reiterated his Chapter 9 comment that the models use outdated crop growth data.
 - Kaffka provided extensive written comments on the modeling and the subset of modeling analyses used. He suggested that EPA make clear that there are critiques of some of the studies used in the chapter.
- Hoekman did not comment on CQ2(c) for this chapter, indicating that modeling was outside his area of expertise.

d) Conclusions and recommendations.

- All four reviewers agreed that the conclusions generally follow the data presented in the chapter, however, two reviewers provided additional detail:
 - Engel commented that “additional clarity is needed in the conclusions that an increase in pesticide loss or nutrient loss due to lands converted to corn or soybeans that are used in biofuels does not necessarily create a significant water quality issue unless thresholds of concern were reached. The data presented in the chapter do not suggest such thresholds were crossed nor even approached.” Engel also found some of the research recommendations to be broad and unsupported.
 - Hill qualified his response commenting that the conclusions and recommendations in the chapter that are based upon Chapter 6 are not supported.

e) Uncertainties and limitations.

- Three reviewers (Engel, Hill, Hoekman) agreed that the most important uncertainties and limitations are identified in the chapter. Hoekman suggested an additional area of uncertainty involving the indirect water quality effects resulting from production of biofuel feedstocks in other regions of the world.
- In Kaffka’s extensive written comments, he provided input on the uncertainties and limitations related to the modeling studies used.

f) Usefulness of the supporting materials. All four reviewers agreed that the figures are helpful in supporting the chapter discussion and conclusions. Hill, Hoekman, and Kaffka provided suggestions to improve the readability or clarity of numerous figures.

2.2.4 Chapter 11 (Water Use and Availability)

Two reviewers (Engel, Kurkalova) commented on Chapter 11 in response to CQ2.

- a) Overall impressions of clarity and technical accuracy. Both reviewers found the chapter to be a clear and technically supported analysis of water use impacts of the RFS.
- b) Data and literature citation and interpretation. Both reviewers agreed that the most important literature is cited and correctly interpreted. Kurkalova suggested adding a discussion on how differing data collection approaches may result in different estimates.

- c) Selection and use of models. Both reviewers agreed that the selection of models for the analyses in this chapter was appropriate.
- d) Conclusions and recommendations. Both reviewers agreed that the conclusions and recommendations described in the chapter synthesis section are supported by the material presented. Engel noted that several of the recommendations, while interesting and having impacts in other areas, do not seem to be significant enough to support future analyses needed for this report. He suggested that EPA review the recommendations with this lens.
- e) Uncertainties and limitations. Both reviewers agreed that the major uncertainties and limitations associated with the chapter's conclusions are identified and reasonable.
- f) Usefulness of the supporting materials. Both reviewers found the graphics in the chapter to be useful. Kurkalova provided suggestions to improve the clarity of numerous figures.

2.2.5 Chapter 12 (Terrestrial Ecosystem Health and Biodiversity)

Five reviewers (Davis, Hill, Hoekman, Kaffka, Landis) commented on Chapter 12 in response to CQ2.

- a) Overall impressions of clarity and technical accuracy.
 - Three reviewers (Davis, Hill, Hoekman) found the chapter to be clear and accurate. In addition:
 - Davis commented that the chapter provides very little review of the potential impacts from other advanced biofuels.
 - Hoekman suggested areas where the chapter could be improved by adding discussions of indirect effects and the impacts of future increases in biofuel and corn ethanol production.
 - Kaffka provided a summary of the chapter but did not respond directly to CQ2(a) for this chapter.
 - Landis noted this chapter is his primary area of expertise and found “major gaps” in the chapter, as described in his responses to the other subparts of CQ2 below.
- b) Data and literature citation and interpretation.
 - Three reviewers (Davis, Hoekman, Kaffka) found the literature review to be generally complete.
 - Davis commented that the review was complete except for the lack of information cited to describe biogas and other advanced biofuels.
 - Kaffka suggested that, to improve readability, EPA add a table summarizing the reports cited. He also suggested that when using a paper whose approaches have been criticized, EPA should critically evaluate the reliability of both the original paper's assertions and the criticisms of the methods and results.
 - Hill pointed out a specific misinterpretation that needs correction.
 - Landis noted several gaps in the chapter, including international effects, beneficial insects, ecological (aka, evolutionary) traps, and impacts of bioenergy cropping systems on taxa and ecosystems services (for which he included many references). He also provided line-by-line suggestions for additional or preferred references.
- c) Selection and use of models. The reviewers noted that modeling is not applicable to this chapter. While Kaffka did not respond specifically to the question of models, he provided several suggestions for clarifications.

d) Conclusions and recommendations.

- Four reviewers (Davis, Hill, Hoekman, Landis) agreed that the conclusions and recommendations are supported by the material presented, and most of these reviewers also provided caveats or additional suggestions as follows:
 - Hill agreed that the conclusions and recommendations are supported by the material presented, with the exception of the conclusions and recommendations based on Chapter 6.
 - Hoekman suggested that in addition to the chapter's qualitative conclusions, some semi-quantitative conclusions be provided regarding the extent to which renewable fuels impact terrestrial ecosystem health and biodiversity.
 - Landis recommended that, at a minimum, EPA include review of the literature on the effects of changing agricultural landscape complexity on terrestrial ecosystem health and biodiversity.
- Kaffka provided numerous line-by-line suggestions and commented that the absence of a local-scale approach in the RtC3 overall is a "significant oversight."

e) Uncertainties and limitations. All five reviewers agreed that the chapter identifies uncertainties and limitations associated with its conclusions.

f) Usefulness of the supporting materials. All five reviewers agreed that the supporting materials are useful. Davis suggested adding locations with stover removal for bioenergy to the land use analysis, and Hoekman suggested improvements to one figure he found confusing.

2.2.6 Chapter 13 (Aquatic Ecosystem Health and Biodiversity)

Three reviewers (Engel, Hoekman, Landis) commented on Chapter 13 in response to CQ2.

a) Overall impressions of clarity and technical accuracy.

- All three reviewers generally found this chapter to be clear and technically accurate, with the following caveats:
 - Engel commented that the statement, "Water demand for feedstock production reduces stream flow and changes flow patterns that are important for supporting fish diversity," is not supported by material presented in the chapter. If this statement is included in the report, he suggested that EPA conduct additional analyses to support the statement.
 - Landis commented that landscape-scale effects of agriculture on aquatic ecosystems require increased coverage in the chapter.

b) Data and literature citation and interpretation.

- Engel commented that if there is interest or concern about changes in flow, much more data and discussion of literature are needed.
- Hoekman found that the most important literature had been cited and interpreted appropriately.
- Landis referenced 11 recent studies of the impacts of agriculture on aquatic biodiversity and function.

c) Selection and use of models. Engel and Landis found the model selection and use in the chapter to be appropriate. Hoekman did not comment on CQ2(c).

d) Conclusions and recommendations.

- All three reviewers found that the conclusions and recommendations are generally supported by the material presented, however, they all also qualified their responses as follows:
 - Engel commented that the conclusion on the impacts on stream flow is not supported in the chapter and many nuances exist around flow that were not explored.
 - Landis recommended including a review of the literature on the effects of changing agricultural landscape complexity on aquatic ecosystem health and biodiversity.
 - Hoekman noted that since many of the conclusions are qualitative, the impacts attributable to the RFS Program cannot be determined, making the recommendations for more research to quantify impacts very appropriate.

e) Uncertainties and limitations. Two reviewers (Hoekman, Landis) agreed that the chapter identifies the most important uncertainties and limitations. Engel commented that the report does not appropriately acknowledge the extensive assessments on endangered species that EPA has conducted related to the registration process for pesticides.

f) Usefulness of the supporting materials. All three reviewers agreed that the supporting materials are useful.

2.2.7 Chapter 14 (Wetland Ecosystem Health and Biodiversity)

Three reviewers (Hill, Kaffka, Landis) commented on Chapter 14 in response to CQ2.

a) Overall impressions of clarity and technical accuracy.

- Hill found the chapter to be clear and accurate.
- Kaffka noted his concern that the broad-scale approach used to estimate wetland losses and gains across this large region does not adequately account for the actual land management that is occurring and the heterogeneity of the landscape.
- Landis found that the topics covered in the chapter are clear and technically accurate, however, he recommended that landscape-scale effects of agriculture on wetland ecosystems be included.

b) Data and literature citation and interpretation.

- Hill agreed that the most important literature had been cited and appropriately interpreted.
- Kaffka suggested that a better description of the prairie pothole region and its diversity would enhance the discussion in this chapter. He provided several suggestions and references to do this. He also commented that this chapter repeats a problematic characterization of the RtC3 that underestimates the role of soybean production on crop choice, attributing most change to corn production for ethanol.
- Landis provided references for many recent studies of the impacts of agricultural landscape structure on wetland biodiversity and function.

c) Selection and use of models. Reviewers noted that CQ2(c) is not relevant to this chapter.

d) Conclusions and recommendations.

- Hill agreed that the material presented supports the conclusions and recommendations.
- Kaffka found the chapter unclear as to whether losses in one area within the prairie pothole region are made up for by gains in others due to increasing precipitation across the region.

- Landis recommended including a review of the literature on the effects of changing agricultural landscape complexity on wetland ecosystem health and biodiversity.
- e) Uncertainties and limitations. Hill and Landis agreed that the chapter identifies uncertainties and limitations associated with its conclusions. Kaffka noted that this chapter may cover one of the most difficult and uncertain topics in the report.
- f) Usefulness of the supporting materials. All three reviewers agreed that the supporting materials are useful.

2.2.8 Chapter 15 (Invasive and Noxious Plant Species)

Two reviewers (Davis, Landis) commented on Chapter 15 in response to CQ2.

- a) Overall impressions of clarity and technical accuracy.
 - Both reviewers found the chapter to be clear, but Davis also included line-specific suggested edits and several general comments for EPA's consideration, stating:
 - No new analysis of biofuel impacts on invasive plants was included, which seems inconsistent with the priority recommendations from RtC2.
 - The discussion about *Miscanthus x giganteus* and *Panicum virgatum* leaves the impression that these could be classified as invasive, but there is a body of literature suggesting some varieties are not invasive.
 - The list of advanced biofuels warrants more detailed discussion.
- b) Data and literature citation and interpretation. Both reviewers provided additional suggestions for references to include in this chapter.
- c) Selection and use of models. Reviewers noted that CQ2(c) is not relevant to this chapter.
- d) Conclusions and recommendations. Landis found that the conclusions and recommendations are supported by the material presented. Davis provided a suggestion related to the research recommendations on the need for a new weed risk assessment tool.
- e) Uncertainties and limitations.
 - Davis suggested adding the need for research on feedstocks that are expected to be *less* invasive to the need for research on novel feedstocks that pose an invasion risk.
 - Landis suggested emphasizing the mounting evidence of the potential invasiveness of *Miscanthus* and the potential costs to control it.
- f) Usefulness of the supporting materials. Both reviewers agreed that the supporting materials are useful.

2.2.9 Chapter 16 (International Effects)

Three reviewers (de Gorter, Hill, Smith) commented on Chapter 16 in response to CQ2.

a) Overall impressions of clarity and technical accuracy.

- de Gorter's comments focused on his concerns with the report's emphasis on the international environmental effects of U.S. biofuel consumption in changing U.S. trade in biofuels, when that is only one way that U.S. biofuel use affects the environment internationally. He also provided detailed comments on the text and figures in the chapter.
- Hill commented that the chapter is unclear and in part, technically inaccurate, including the text dependent on the Chapter 6 analyses.
- Smith found the chapter was clearly written and appeared to be accurate in what it presents.

b) Data and literature citation and interpretation.

- de Gorter suggested that the report analyze the effects of U.S. biofuel consumption on biofuel and feedstock prices and how they impact world feedstock prices. The environmental effects would then have to be derived.
- Hill pointed out several conclusions and interpretations in the chapter that are unsupported or ambiguous, and commented that, "the most glaring omission in this chapter is the lack of detailed treatment of potential market-mediated effects."

c) Selection and use of models.

- de Gorter included extensive line-by-line comments and suggestions on the approach used in this chapter.
- Hill commented that the GTAP-BIO model does not allow for natural forest or grasslands to be converted to agricultural production.
- In response to the report's statement that "international market-mediated effects are not substantively addressed in this report due to time and resource constraints, and this remains a priority for future study," Smith commented that the chapter needs to at least summarize the large literature on indirect land use change.

d) Conclusions and recommendations.

- de Gorter disagreed with some conclusions and provided comments describing his reasoning.
- Hill commented that he does not agree that the conclusions and recommendations based on Chapter 6, or in relation to the points he made above, are supported by the material presented.
- Smith found that the conclusions and recommendations are supported by the material presented.

e) Uncertainties and limitations.

- de Gorter and Smith agreed that the chapter identifies uncertainties and limitations associated with its conclusions.
- Hill commented that the uncertainties and limitations receive only a cursory treatment in this chapter and suggested they could be improved.

f) Usefulness of the supporting materials.

- Hill and Smith agreed that the supporting materials are useful.
- de Gorter did not provide a response specific to CQ2(e) but made detailed suggestions for improvements on specific figures and boxes in the chapter.

2.3 All Chapters

In their responses to Charge Question 3, reviewers were asked to consider all chapters they had reviewed, as well as the background chapters in Part 1 (Chapters 1 – 5) of the document.

Charge Question 3. *Considering all chapters you reviewed, as well as the background chapters in Part 1, does the report address the requirements of Section 204 as set forth in Chapter 1 and as reflected in the scope set forth in Chapter 2? If not, please describe what should be added, removed, or changed.*

Seven reviewers (Davis, Engel, Hill, Hoekman, Kurkalova, Landis, Smith) responded to CQ3.

- Four reviewers (Engel, Kurkalova, Landis, Smith) agreed that the report addresses the requirements of Section 204.
- Two reviewers (Davis, Hoekman) generally agreed that the report addresses the requirements of Section 204 but provided qualifying comments:
 - Davis commented that the analysis in the report is weakened by the lack of information describing advanced biofuels.
 - Hoekman commented that the report misses opportunities related to the future impacts of biofuels and the quantitative impacts of indirect land use change today and in the future.
- Hill disagreed that the report addresses the requirements of Section 204, citing missing information and a misinterpretation of “environmental impacts outside the United States” in Section 204.

2.4 Executive Summary and Integrated Synthesis

Charge Question 4. *The Executive Summary provides a high-level overview of the report, and the Integrated Synthesis provides a more detailed overview, including RtC3 scope and content, the report’s conclusions and recommendations, a comparison of RtC3 conclusions with those of RtC2, and a discussion of future reports under EISA Section 204.*

- a) *Is the presentation of information in the Executive Summary clear? Is the Executive Summary appropriately distilled down from the Integrated Synthesis and the broader RtC3? Please describe any suggestions for improvement.*
- b) *Is the presentation of information in the Integrated Synthesis clear? Is the Integrated Synthesis appropriately distilled down from the broader RtC3? Please describe any suggestions for improvement.*

All nine reviewers responded to CQ4(a) on the Executive Summary:

- Davis suggested that EPA provide a summary of recommendations in the Executive Summary such as by highlighting one recommendation from each chapter.
- de Gorter raised questions on specific parts of the Executive Summary in his comments.
- Engel commented that the Executive Summary appropriately recognizes challenges of the analysis, but also noted the missed opportunity to acknowledge the pending commitments to significant increased spending to adopt climate-smart practices such as conservation practices.

- Hill commented that while the Executive Summary is clearly written, it contains many of the errors and deficiencies that he noted throughout his comments, especially those on Chapter 6. He also noted that air quality, a major finding of the report, is not covered in the Executive Summary, but should be.
- Hoekman agreed that the Executive Summary presents a clear distillation but also commented on areas that should be emphasized more strongly such as indirect environmental effects and future impact projections.
- Kaffka and Landis found the Executive Summary to be clearly written and they both provided suggestions for improvement.
- Kurkalova commented that the Executive Summary was easy to follow and clear.
- Smith commented that the Executive Summary is clear, but reiterated his earlier comments that the authors unreasonably eliminated the possibility that the RFS had larger effects.

All nine reviewers responded to CQ4(b) on the Integrated Synthesis:

- Davis provided numerous specific suggestions to improve the Integrated Synthesis such as including a comparison between the impacts of biofuels and fossil fuels, and a comparison of land use change impacts (from Chapter 12).
- de Gorter provided comments or questions on several specific parts of the Integrated Synthesis.
- Engel commented that the Integrated Synthesis (and the chapters describing land use changes) “fails to acknowledge the great advances in corn and soybean genetics that have occurred over the past decade. These advances have made production of corn and soybeans more profitable than alternatives in many locations and especially in the historically more marginal areas that are the focus of many chapters of this report. The lack of acknowledgement that such advances in genetics are driving these changes potentially leads to a misinterpretation of RFS and biofuel impacts.”
- Similar to his comments on the Executive Summary, Hill noted that while the Integrated Synthesis is clearly written, it contains many of the errors and deficiencies he noted throughout his comments.
- Hoekman commented that the Integrated Summary should summarize the greenhouse gas (GHG) impacts of the RFS Program. While this was not identified as an explicit requirement under EISA Section 204, he noted that this information is highly relevant when judging the overall impacts of biofuels in general, and the RFS Program in particular. He stated that, “without considering GHG impacts, the reader is led to conclude that on balance, the RFS Program has had a negative effect on the environment.”
- Kaffka’s comments on the Integrated Synthesis questioned the accuracy of the statements on the quantification of increased corn and soybean cropland, and he suggested that EPA clearly define some of the terms used in the Synthesis.
- Kurkalova commented that the Integrated Synthesis was easy to follow and clear.
- Landis found the Integrated Synthesis to be very well written and clear. He added some suggestions for improvement.
- Smith commented that the Integrated Synthesis is clear, but he reiterated his earlier comments that the authors unreasonably eliminated the possibility that the RFS had larger effects.

2.5 General

- Davis, Kaffka, and Kurkalova all began their comments by complimenting EPA on the report.

Charge Question 5. Please provide any additional comments you may have on the draft RtC3.

- Davis commented that the discussion of advanced biofuel feedstocks and cellulosic ethanol is “conspicuously minimized throughout the report.”
- de Gorter and Hill referenced their chapter-specific comments in response to CQ5.
- Engel’s comments on CQ5 covered the report’s lack of discussion of soybean meal and he provided references to additional resources for glyphosate use data. He also emphasized that the report does not acknowledge the assessments of pesticide impacts on people and the environment that EPA routinely conducts through efforts as part of Federal Insecticide, Fungicide, and Rodenticide Act; Food Quality Protection Act; and Endangered Species Act.
- In response to CQ5, Hoekman provided an overview, some line-specific comments, and three recommendations, which are described in detail in his final comments:
 - De-emphasize the focus on attribution of environmental effects of the RFS Program.
 - Include more information about the GHG effects of biofuels.
 - Extend the time horizon of the report.
- Kaffka provided numerous comments, which are summarized here and described in more detail in his final comments:
 - While it is difficult to identify the role of policy certainty on investments, it is a critical issue that should be addressed in the report.
 - Place the RFS era in the much larger historical context of agricultural land use in the U.S. and federal policy. He included references to papers on the longer term trends on acres in production.
 - Consider the broader benefits of economic prosperity in the farm community, partly sustained by the RFS Program, that allows for technology adoption and investments in conservation practices that would not happen under unprofitable conditions.
 - Some attention should be given to higher ethanol blends and adapted engine technology in the report’s forecasting exercise.
 - The report acknowledges that environmental effects are local and difficult to quantify at the local scale, but it makes little use of local knowledge at the state land grant university level. This is an important omission in the report’s approach.
 - EPA used several papers whose methods and/or data have been criticized. EPA could provide an independent review of those papers and the criticisms to give more clarity on the use of those papers’ findings.
 - Kaffka noted that contrary to some other reviewers, his view was that adding indirect land use change and GHG emissions to the report would add complexity and increase uncertainty further; he agreed with EPA’s decision to remain within the statutory language cited that constrains the analysis.
- Kurkalova suggested that clearer explanations of how uncertainties are defined would facilitate the clarity of the uncertainties and limitations sections throughout the report. She also provided numerous citations for references mentioned in her chapter-specific comments.
- Landis had two overarching concerns, discussed elsewhere in his comments, and further described in detail in his response to CQ5:

- Dismissal of potential market certainty effects.
- Omission of agricultural landscape complexity effects.

He also identified two topics as needing more attention in the RtC3. These topics are listed below and described in detail in his response to CQ5:

- Expanded emphasis on precision conservation.
- Indirect effects of neonicotinoid insecticides.
- Smith made three recommendations to improve the report:
 - Incorporate the economics literature on investment under uncertainty, which predicted that the RFS reduced the uncertainty inherent in building an ethanol plant and thereby encouraged construction of more plants. The modeling in the chapter addresses the amount of ethanol used under an assumption that the ethanol plants have been built.
 - Report all outcomes in per gallon (or dose-response) terms.
 - The main use of this report will be future policymakers. Write every sentence with an eye to how they can make good policy decisions.

2.6 Individual Reviewers' Key Comments

At the end of the peer review meeting, each reviewer was given the opportunity to highlight their key comments. Those comments are summarized below.

- Davis:
 - If Section 204 can be interpreted to include GHG emissions as an RFS impact, then GHG emissions should be included.
 - The historical content for land use change is important to consider as a factor in why so much corn and soybean are used for biofuel.
 - Advanced biofuel feedstocks and cellulosic ethanol should be better incorporated in the report.
- de Gorter:
 - Address the shortcomings of the international effects and integrate those effects in Chapters 6 and 7.
 - Include more on future effects where there's more renewable biodiesel and less corn ethanol.
 - Improve the figures in several chapters so they follow the model in [Report with Radich](#).
- Engel:
 - Acknowledge that genetics in corn and soybeans have changed and that it may be a future research focus to incorporate into the models.
 - Given the significant work that EPA already does on pesticide impacts for other EPA program, those assessments should be leveraged in the RtC.
 - The report acknowledges that it is difficult, if not impossible, to assess the localized impacts on many of the areas examined. Because this is not likely to get much easier in the near- or longer-term future, the report's recommendation on quantifying localized impacts should be qualified.

- Hill
 - Attribution in Chapter 6 is wrong – it uses the wrong approach and is inconsistent with the National Academy recommended approach. Additionally, it is missing factors such as fuel market effects and reduction of risk that are important and necessary. The chapter's authors have published work that estimates the effect of reduction of risk for RFS and if that work is included in the RtC, the production volumes and land areas attributable to RFS would be much larger. A complete revision is needed.
 - The exclusion of international market-mediated effects is a glaring omission and is inconsistent with EPA's approach elsewhere, such as in GHG accounting.
 - Many of the chapters looking at the specific environmental effects are very good. They paint a clear picture that in almost all cases, the RFS has made things worse. This doesn't come through in the Executive Summary and Integrated Synthesis.
 - Air quality is the largest contributor to reduced human health. The chapter on air quality shows RFS has reduced our ability to meet air quality targets, however, using GREET is not sufficient as it does not address ammonia and is not spatially explicit. EPA should use the existing tools to address these factors, if not for this report, then in the future assessments.
- Hoekman:
 - Reduce the emphasis on attribution of environmental effects due to the RFS Program.
 - Increase the focus on the environmental impacts of biofuels regardless of what is driving their use.
 - Include a summary of existing work on biofuels' GHG impacts.
 - Extend the horizon of the report by 10 to 15 years which allows for some assessment of advanced alternative fuels.
- Kaffka:
 - The agricultural landscape is evolving due to many different factors. People involved in the land grant system are very familiar with these changes and how they're impacting the landscape. EPA should include reports generated from the land grant system.
 - With respect to the future, there is little discussion of carbon capture and storage, such as related to the capture of CO₂ from dry mill and other facilities.
 - EPA should consider, especially when projecting for the future, that there will be unexpected needs for almost all U.S. capacity as the world population grows and unexpected events (e.g., loss of acreage in Ukraine due to the war) occur.
- Kurkalova:
 - She agreed with the other reviewers regarding the need to address policy certainty effects.
 - It would be desirable if the forward-looking component of the report extended past 2025, which would also provide the opportunity to better address advanced biofuels.
 - Differentiate between the different types of uncertainties, as noted in her earlier comments.
- Landis:
 - The chapters on environmental impacts are well done.
 - Market and policy certainty effects should be considered.
 - Consider the impact of landscape effects magnifying many of the environmental impacts.

- While the direct effects of pesticides are covered in other EPA assessments, the indirect effects are not covered in the existing assessments and should be considered in the report.
- There is an opportunity to add more to the report on precision conservation, especially in future projections.
- Smith:
 - For attribution in Chapters 6 and 7, do not omit the role of RFS causing the build out of renewable fuel plants.
 - Report all outcomes in per gallon terms. It would be much clearer to readers to show the estimated environmental effects of each additional gallon of biofuel.

3.0 REVIEWER FINAL COMMENTS BY CHARGE QUESTION

3.1 Part 1: Background and Drivers (Chapters 1 to 5)

For background at the start of the review, all reviewers should read Chapters 1 and 2. While there are no charge questions associated with Chapters 1-5 reviewers should incorporate any comments they may have on any of these chapters into their response to the chapter-specific charge questions below, as appropriate, or into their response to Question 5 (additional comments).

3.1.1 Chapter 1: Introduction

Reviewer	Comments
de Gorter	On p. ES-1, it states the EPA does another study on GHGEs. The 1st question arises: do you use the same impact of the RFS Program on biofuel consumption? International effects? If not, how do you justify? On p. IS-12, footnote 13 should be brought up into the text. It is key. More explanation is also needed. For example, the EIA reports consumption of Conventional Blendstock for Oxygenate Blending (CBOB) and RFG separately – are these both part of the irreversible investments to contain 10% ethanol for octane? Have RFG and conventional finished motor gasoline reported separately – same BOB? Line 100: what are the results of the 2 previous reports? Clarification: I must have been referring to line 100 of Chapter 2. I was just wondering what the conclusions were of RCT1 And RCT2 but no need – you discuss elsewhere. Figure IS.1 is better to be replaced by a comprehensive summary of what happens if policies are combined. Consider the RFS and LCFS. Each program reinforces the effect of each other as RIN prices are additive to the carbon credit prices for renewable diesel for example. But the RIN price is lower for a given carbon credit price, and vice-versa. So, they are offsetting each other. The mandate for BBD in California cannot impact the overall BBD mandate in the RFS.

	The federal BBD tax credit subsidizes diesel plus BBD consumption because the mandate is binding. See these papers: de Gorter, Harry, and David R. Just. (2008). "The Forgotten Flaw in Biofuels Policy: How Tax Credits in the Presence of Mandates Subsidize Oil Consumption." <i>Resources for the Future Policy Commentary</i>, June 9, 2008. • de Gorter, Harry, and David R. Just. (2009). "The Economics of a Blend Mandate for Biofuels." <i>American Journal of Agricultural Economics</i> Vol. 91: No. 3, August, pages 738-750. • de Gorter, Harry. (2010). "The one tragic mistake of biofuel policy that everyone can agree on: how tax exemptions with mandates subsidize oil consumption." <i>Policy Update in Biofuels</i> 1(4), 531–533. • Drabik, Dusan. (2011). "The Theory of Biofuel Policy and Food Grain Prices". Working Paper 2011-20. Charles H. Dyson School of Applied Economics and Management, Cornell University. December • de Gorter, Harry, Dusan Drabik and David R. Just. (2013). "The Perverse Effects of Biofuel Public-Sector Policies." <i>The Annual Review of Resource Economics</i>. Volume 5. If the tax credit is binding, then the mandate is redundant (but as described above, not vice-versa).
Hoekman	Chapter 1 In discussing the four categories of biofuel defined under the RFS Program (Chapter 1, Section 1.1), it is briefly mentioned that each category has a GHG reduction requirement. This point should be expanded by explaining that life cycle assessment (LCA) is used to determine whether the GHG reduction target is met for each biofuel pathway, and that this LCA evaluation must consider direct emissions and significant indirect emissions, such as emissions from land use changes (LUC). The renewable volume obligation (RVO) requirements under the RFS Program are shown in Table 1.1 up to the year 2022. It is mentioned in several places throughout the report that RVOs have not been established beyond 2022, and that this creates uncertainty about future requirements and impacts of the RFS Program. Recently, however, RVO targets have been formally proposed by EPA for the years 2023, 2024, and 2025. These proposed values should be included in Table 1.1, along with a statement that they are not yet final values. It might also be noted that in the past, final RVO values did not vary much from proposed values. It is pointed out in Chapter 1 (and elsewhere), that as opposed to the RtC1 and RtC2 reports, this draft RtC3 report focuses on the issue of attribution of land use change and environmental effects due to the RFS Program itself, as opposed to biofuels and agricultural markets generally. This is a major improvement in RtC3, which is important in addressing the Section 204 requirements that EPA assess the impact of the RFS Program. At the same time, it is of great interest to understand the broader impacts of

	biofuels in general, whether or not these impacts can be attributed to the RFS Program. While it may be true that the RFS Program is a minor driver of ethanol in U.S. gasoline, the overall impacts from production and use of massive volumes of ethanol are of considerable interest – whatever the causes.
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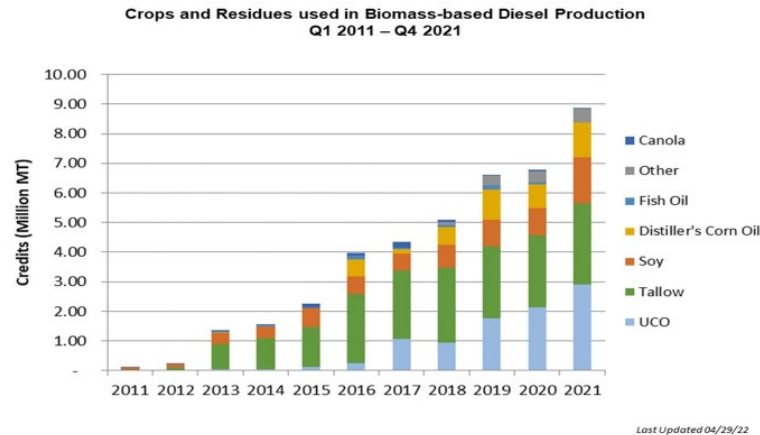
3.1.2 Chapter 2: Scope of the Report

Reviewer	Comments
Davis	See Section 3.6.1
de Gorter	See Section 3.6.1
Hill	Chapter 2 Lines 307–312: The authors should consider avoiding use of the terms “direct” and “indirect.” See Chapter 4 “Direct and Indirect Effects” of the NASEM report “Current Methods for Life Cycle Analyses of Low-Carbon Transportation Fuels in the United States.” The report concludes, “Dividing emissions into direct and indirect can be used when identifying and classifying sources of emissions, but it can cause confusion, even if carefully defined and transparently presented in an LCA,” and recommends, “Because the terms ‘direct’ and ‘indirect’ are used differently in different contexts, these terms should be carefully defined and transparently presented when used in LCA studies or policy. Another option is to avoid using the terms ‘direct’ and ‘indirect’ altogether, as they are not considered necessary elements of LCA and may lead to greater confusion.” Fig. B.2.2 in Box 2.2 should be deleted if the authors’ interpretation of EISA Section 204 is that GHG emissions are out of scope for this report. The NASEM report described in Lines 394–408 of Box 2.2 has been released, and RtC3 should be updated accordingly to include that report’s findings, especially as relate to RtC3’s failure (especially Ch. 6) to use methods consistent with the NASEM’s committee’s conclusions and recommendations. Box 2.3 does not provide a balanced description of the reality of CCS and its impacts related to biofuels. Potential drawbacks should be described, and overly positive language such as the last sentence should be deleted or tempered.
Hoekman	Chapter 2 In my view, a critical gap in the RtC3 is failure to include more detailed information about the GHG impacts of biofuels generally, and the RFS Program in particular. The RtC3 report argues that EISA Section 204 does not require examination of GHG effects, that GHG impacts are covered in other EPA documents, and that RtC1 and RtC2 did not discuss GHG issues. While these points are all true, it seems obvious that any comprehensive report on “Biofuels and the Environment” would contain a substantive discussion of GHG impacts. Inclusion of Box 2.2 (page 2-17) is helpful, but not sufficient in covering GHG issues. Because reduction of GHG emissions is the principal

	environmental driver for the RFS Program, not discussing this issue more thoroughly in RtC3 is probably mystifying and disappointing to most readers. In describing the biofuels and feedstocks considered in RtC3 (Section 2.3), it is pointed out that biodiesel from soybean oil and from FOGs dominated during the historical period of 2005-2020. However, during the latter portion of this period, renewable diesel (RD) became very prominent. RD volumes are already larger than biodiesel (BD) volumes in California, and it is likely that RD will surpass BD nationwide within a few years. In Table 2.1, which shows year-by-year volumes of renewable fuels, BD and RD are combined into single values. If possible, these should be separated to show individual volumes of BD and RD. In Section 2.4 (page 2-14), it is explained that EPA interprets “impacts” to include both direct and indirect effects of the RFS Program. While touched upon in several of the subsequent chapters, issues related to indirect effects do not receive very much attention. This is surprising, given that in the recent past, the topic of effects resulting from indirect land use change (ILUC) was very prominent and quite controversial. In this RtC3, the term ILUC is not used, and does not even appear in the list of “Acronyms and Abbreviations” shown near the beginning of the report. In reviewing Chapter 2, a couple of typographical errors were detected. These are identified below: • Page 2-13, legend for dashed blue line in Figure 2.2: “Projected Total Biodiesel and Renewable Diesel Production.” • Page 2-21, y-axis label (left side) in Figure 2.3: “Gasoline Vehicles.”
Kaffka	Chapter 2 (Line 25)...evaluate impacts from 2005 to about 2020 and, Interpret the likely future ... to approximately 2025. (Line 56-58): Section 204 does not require assessing the larger effects of the US biofuel industry as a whole, or crop and biomass cultivation in the US as a whole, but these and other factors are critical in assessing impacts of the RFS so are considered in this report, and attributed proportionally to the RFS. Clarification: In my overall general comments, I have urged EPA to take into account the success of the RFS in meeting its goals to generate alternative fuels and reduce fossil fuel use, but also to account for beneficial effects on the farm economy of improved crop prices on improving efficiency and adoption of conservation practices. The RtC3 was focused on mostly adverse environmental effects, but where these occur and are attributable to the RFS, are the side effects of the program’s primary objectives. Cost benefit analysis is not part of the RtC3, but recognition of its benefits seemed under-emphasized. Changes in land use in agriculture are long-term processes. The RtC3 has a limited time horizon, which varies somewhat throughout the report. But in general,

	recognition of the long-term nature of change in land use on farms and across regions needs further emphasis. (Line 78, ftnt 2): Different data sets are mentioned and comparability is mentioned but not yet identified or discussed. Careful discussion of methods is essential for the quality of the RtC3. Clarification: I was uncertain at this point in my review how different data sets were used or would be used in the report. It occurred to me that some specific clarification of the relationship among these agricultural land use data sets and their use in the report would be useful. (Lines 209-210; Table 2.1): Biodiesel from corn oil is not included in the overall evaluation (which only focuses on soy oil BD). But high yielding corn fields can provide almost as much oil as average yielding soybean fields, in addition to the starch (energy) and protein produced. (Lines 211-212, Table 2.2): EtOH from Brazil is accounted but is a lower total % of biofuel in the US than corn oil biodiesel. (Lines 231-233; lines 284-290, and lines 299-300): CNG/LNG from dairy and other livestock manure biogas is not included, but is a rapidly growing source and likely to continue to increase with supportive policies. Most credits are marketed in California. CARB data and CDFA data may be useful. This is an oversight. (Lines 240-251): Discussion here of E15 expansion seems reasonable. There is literature on higher alcohol blends, that is only first mentioned on lines 294-295, but without any detail or references. This is an important omission. The use of higher alcohol blends in fuels (20 to 30% EtOH) is a path not considered nor proposed by EPA or CARB. Obstacles include fuel distribution and logistics, and shifts in manufacturing. But these obstacles are not more challenging and arguably less challenging than a shift to battery electric vehicles and the radical disruption required. Increased ethanol demand based on higher blend rates would be partially met by increased corn yields (an ongoing trend), and also allow for new cellulosic feedstocks to be accommodated. This could have larger beneficial effects for landscape management in the US. The use of perennial grasses, and other cellulosic feedstocks like residues from fuel load reduction in forests, (especially important in the western US) would help achieve many public goods that currently are not being met. (Lines 266-270): Projections for future EtOH are larger than USDA's estimate and closer to EIA's estimate. Clarification: Here is EIA data: https://www.eia.gov/todayinenergy/detail.php?id=53539 ; Here is USDA's data: https://www.ers.usda.gov/data-products/u-s-bioenergy-statistics/ ; If I remember correctly, It appeared to me that EPA's projections differed from USDA's, so I wondered why? Any discrepancies between different projects should be noted and discussed.
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(Page 111, Figure 2.2): Renewable diesel production is projected to decline despite new domestic and international capacity coming into production in the near term. EPA should explain why this is the case. California alone has increased RD use in the US in a significant manner.



<https://ww2.arb.ca.gov/resources/documents/lcfs-data-dashboard>

The Low Carbon Fuel Standard (above) incentivizes growth in fuels derived from non-land-based sources. In 2021, 77% of total credits generated by biodiesel and renewable diesel fuels were derived from wastes or residues rather than conventional crop-based fuel credit generation. One LCFS credit is equal to 1 metric ton CO₂-equivalent (MTCO₂e), as determined on a life-cycle basis which accounts for the emissions during raw material extraction or recovery, feedstock cultivation, fuel production, transport, processing and use of the fuel. The feedstocks used to produce California's low-CI biomass-based diesel fuels include industrial by-products such as used cooking oil (UCO), tallow, corn oil extracted from distiller grains and solubles (DDGS), and oils from fish processing. Soy oil use is minor source. All these other sources ultimately derive from crops.

2.4 Spatial extent

(lines 307 to 309). EPA's definition: "EPA interprets "impacts" to include both effects directly influenced by the RFS Program (e.g., by the cultivation of feedstocks for use as a biofuel as a result of the RFS) as well as those indirectly influenced by the RFS Program (e.g., the displacement of other crops by corn, leading to greater cultivation of those crops elsewhere to meet the market gap."

Only the impacts caused by the RFS itself are considered, not the consequences of biofuel production and use generally. This is an important distinction since critics of crop-based biofuels generally consider effects or impacts from the entire sector.

2.6 Emerging issues not addressed

	(Lines 494-495; Horizon Scanning): <i>“The trends identified in the “horizon scanning” sections are likely to have modest impact, at most, on likely future consequences within the timeframe of the RtC3 out to 2025.”</i> Agreed.
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3.1.3 Chapter 3: Biofuel Supply Chain

Reviewer	Comments
Hill	Chapter 3 Line 188: This is not always the order of events. Chapter 3 Footnotes 12 and 13. The descriptions are incorrect. Dimethenamid and Metolachlor are racemic mixtures. See Section 9.4 of https://pubchem.ncbi.nlm.nih.gov/compound/Dimethenamid-P and https://en.wikipedia.org/wiki/Metolachlor
Hoekman	Chapter 3 Non-crop feedstocks (FOGs) for biofuels are discussed in Section 3.2.2 (page 3-32). It would be helpful to include a graphic showing the time trend of FOG usage, as well as the distribution of the individual FOG constituents (i.e., animal fats, brown grease, yellow grease, etc.) year-by-year. A stacked bar graph similar to those used in Figure 3.15 would be suitable. Box 3.2 (page 3-33) describes the production of biogas from anaerobic digestion and the growing use of this feedstock to produce CNG/LNG as transportation fuels. [These final products are commonly referred to as renewable natural gas (RNG).] The current growth rate of RNG is extremely high. As stated in Box 3.2, RNG use as a transportation fuel increased “... from nearly none in 2011 to just over 400 million gallons in 2019.” EPA should indicate the current (2022) amount of RNG being used and the estimated usage in 2025. It is also stated in Box 3.2 that currently, most of the cellulosic biofuel volumes under the RFS Program are being met by RNG. This is certainly a surprising development that was unforeseen at the time the RFS Program was initiated. EPA should discuss the type of environmental and natural resource impacts that might be anticipated if this growth of RNG continues. In reviewing Chapter 3, a typographical error was found on Page 3-38, line 489: “(see Box 3.3)”
Kaffka	Most of my answers to these questions are made as comments by page and line number below. It was easier and made greater sense to comment where questions or issues arose directly in the text. Overall this is a well written and informative chapter.

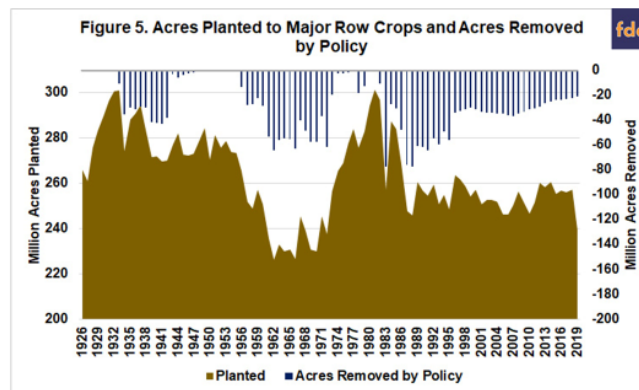
	Key Findings: These are clearly written and summarize well the content of Chapter 3, but I suggest some word changes or additions to the language used for greater clarity. These are documented by page and line below. Introduction: The organization of this section is logical, clear and helpful for non-agronomists. Some confusion about prevent acres. Specific issues that stand out for reconsideration are EPA’s comments on livestock feeding (Box 3.3). Cropland acre discussions lack sufficient context, and risk over attribution of LCLM to the RFS program. There is a long history of land use change related to government policies, markets, climate change, crop and technology adaptation, etc., largely resulting in ever fewer acres being used over the course of time. The period analyzed is a short-term snapshot of longer-term processes. The RtC3 would benefit from placing the period analyzed in a longer historical context. There is some under emphasis given to the unique historical circumstances affecting trade and planted acres associated with the unprecedented growth in demand for whole soybeans in China and elsewhere in Asia. Better, (longer) historical context would improve this analysis and that found in other chapters as well. See comments starting with lines 99ff. The role of increased demand in Asia for US soybeans is under-attributed in the report in this chapter and elsewhere. Lines128ff. Key findings (Page 3-2): Line 34: drop the word chemical. Line 35: Insert the word “typically” before corn...”corn <i>typically</i> uses” . There is a great deal of variance in fertilization recommendations and practices across the region. Lines 34-36: Soybeans have a higher P content than corn, but lower yields. The result is that removal and fertilization rates for typical corn and soybean crops differ by approximately 20% to 30%, but at high soybean yield levels and low P soil test values, comparable or larger amounts of P fertilizer may be applied to soybeans than to corn. Good sources of comparisons are: https://ohioline.osu.edu/factsheet/anr-74 ; https://extension.umn.edu/crop-specific-needs/soybean-fertilizer-guidelines#phosphate-and-potash-1078561 ; https://farmdoc.illinois.edu/field-crop-production/uncategorized/new-grain-phosphorus-and-potassium-numbers.html ; https://extension.sdstate.edu/sites/default/files/2020-03/S-0004-25-Soybean.pdf Line 41: Suggest adding: “...but are slowly increasing” ... to the end of the sentence. Lines 45-46: To the extent that DDGS is fed in total mixed rations for cows (dairy and beef), it reduces protein required from other sources, including soybean meal. Line 50: Animal fats, tallow and waste grease from frying (in soy oil) that constitute most FOG, are in a sense also by-products of corn and soybean production. Corn and soy are fed
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to livestock (the source of tallow and animal fats) and soy is commonly used for frying (yellow and brown grease).

3.1 Introduction

Line 83: material should be plural - “materials”

Lines 93 ff: The choice of the years 2000-2001 as the basis for comparison of corn acres starts with the lowest recent year for corn planting. Data for plantings further back in time include years when corn planting was closer to current levels or even much larger. The effects of government programs on acres might also be considered when discussing corn (and soybean) acre plantings and trends in planted acres. From the 1960s to the 1990s, there were payments for land use diversions to reduce corn planting³, market surpluses, and federal deficiency payments. Acres withheld from corn planting in the 1980s are approximately similar to recent increases in corn plantings. Current acres in principal crops are lower than at other times during the last 50 years⁴.



Coppess, J., N. Paulson, G. Schnitkey, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink](https://farmdocdaily.illinois.edu/2020/04/acres-crisis-prospective-plantings-and-perspectives-from-the-past.html); <https://farmdocdaily.illinois.edu/2020/04/acres-crisis-prospective-plantings-and-perspectives-from-the-past.html>

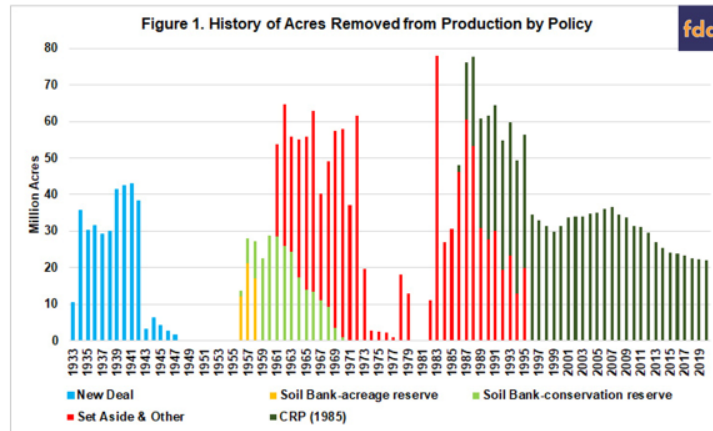
Figure 5 illustrates the total acres planted to the major row crops and the acres removed from production or planting by policy. Acres removed by policy are indicated by the navy bars and work down from zero on the right-hand side. The brief history of acres removed by policy can be divided into the parity system of the 1930s, which used allotments to restrict planted acres of surplus crops. Next was the Soil Bank (1956 to 1958) which coincided with allotments for cotton and wheat but also removed additional acres in an acreage reserve and a conservation reserve program (*farmdoc daily*, February 20, 2020). It was followed by specific feed grains legislation in the 1960s that used Payment-in-Kind policy to encourage farmers to reduce acres planted to corn and the other feed grains (barley, oats, rye and sorghum).

The Conservation Reserve Program (<https://www.fsa.usda.gov/programs-and-services/conservation-programs/conservation-reserve-program/>) removed some acres that might also have been planted to corn. The amount of land in the CRP is a decision of

³ Coppess, J., G. Schnitkey, N. Paulson, K. Swanson and C. Zulauf. "Production Controls & Set Aside Acres, Part 1: Reviewing History." *farmdoc daily* (10):117, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, June 26, 2020. [Permalink](#); Coppess, J., N. Paulson, G. Schnitkey, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink](#); "The term 'Acreage diversion programs' as it applies to the area of agriculture can be defined as 'Historically, commodity programs included provisions to reduce commodity supplies by diverting acreage to non-crop uses. Examples include paid diversion, unpaid diversion, set-aside, and acreage reduction programs. The FAIR Act of 1996 eliminated authority for the USDA to implement annual acreage reduction programs. The Conservation Reserve Program pays farmers for the long-term conversion of fragile cropland land to conserving uses and is not considered to be an acreage diversion program.'" http://teachmefinance.com/Scientific_Terms/Acreage%20diversion%20programs%20.html;

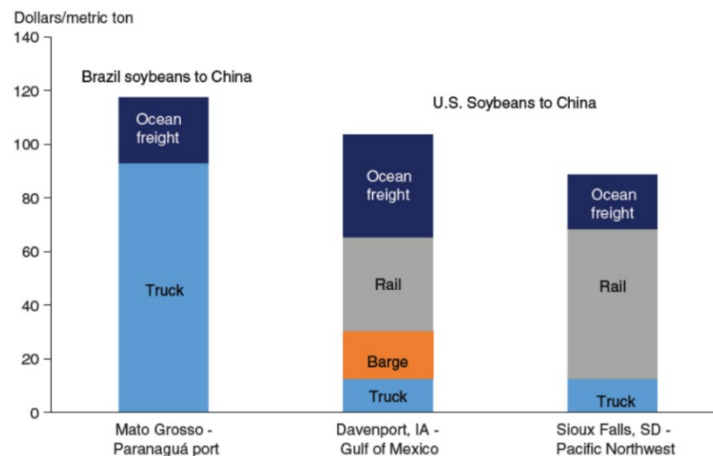
⁴ "... total acres of cropland used for crops peaked at 387 million acres in 1981. By comparison, the average of these acres from 2007 to 2019 has been 335 million acres." Coppess et al, 2020.

congress, does not result from market forces, and varies over time. The long-term effect of the CRP program on planted acres is illustrated in the figure below from Coppess et al., (2020) and is contrasted with other federal set aside programs aimed at controlling acres planted to principal crops. Some land reversion to planted acres would be expected on the part of land owners once CRP subsidies were removed.



Coppess, J., G. Schnitkey, N. Paulson, K. Swanson and C. Zulauf. "Production Controls & Set Aside Acres, Part 1: Reviewing History." *farmdoc daily* (10):117, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, June 26, 2020. [Permalink](#).

Figure 18
Soybean transportation costs to China, selected locations, 2017



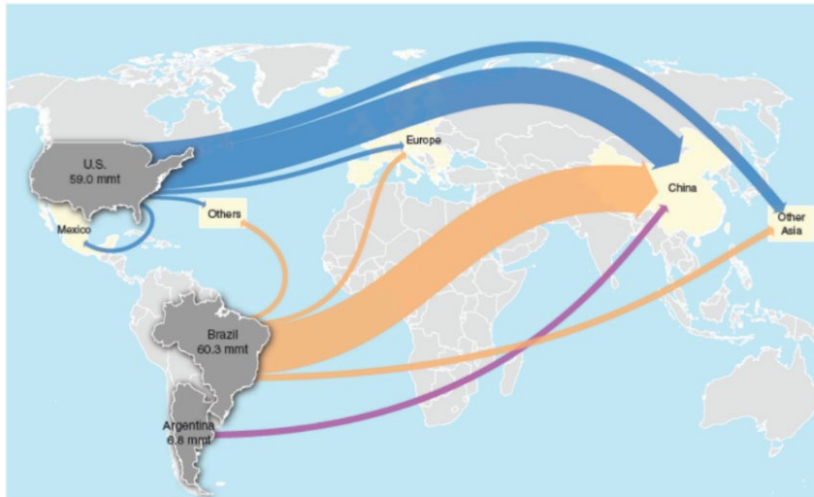
Note: Transportation costs to Shanghai, China, are shown.
Source: Data taken from Salin (2018).

(Gale et al. 2019⁵, page 31)

⁵ Gale, F., Valdes, C., and Ash, M., 2019. Interdependence of China, United States and Brazil in Soybean Trade. USDA-ERS. See also: Gale, 2015. Development of China's feed industry and demand for imported commodities. FDS-15K-01; USDA-ERS https://www.ers.usda.gov/webdocs/outlooks/36929/55490_fds-15k-01.pdf

Line 128: Exports to China are mentioned. A large portion of increased soybean acreage correlated with increased whole bean exports to China as that country rapidly increased meat production and food oil use during the same period. This was largely unpredicted and unprecedented at the scale at which it occurred. Increased export markets for soybeans influenced planting decisions on existing cropland and may have increased land conversion from hay or cropland-pasture in areas with freight cost advantages for Asian export. These include areas in the Dakotas and parts of MN and IA.

Figure 1
Leading soybean exporters and destinations during 2016/17



Note: Chart shows production of soybeans by United States, Brazil, and Argentina during 2016/17. Width of arrows represents volume of exports. mmt = million metric tons.

Source: ERS analysis of customs data from IHS Global Insight, Global Trade Atlas.

Gale et al., 2019⁶.

Since corn is commonly rotated with soybeans, attributing land use conversions to the RFS and to corn ethanol in particular may be in error in these regions of the US. Once soybeans are planted, corn commonly follows in rotation, though wheat may also be rotated in western areas of the corn belt.

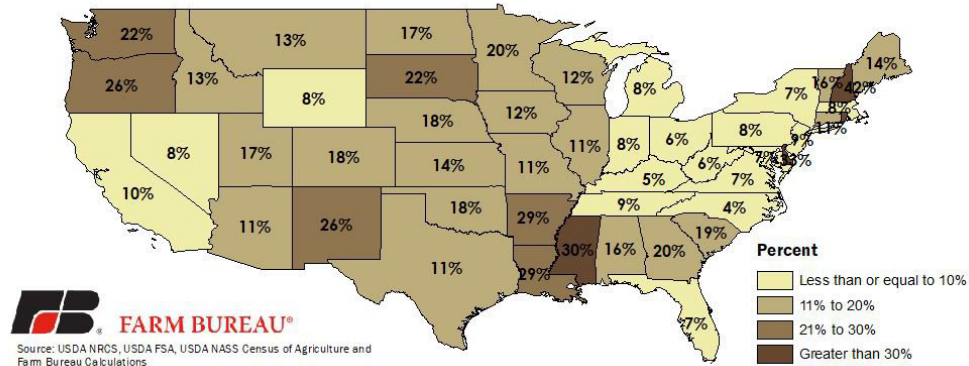
Increased soybean yields due to improved varieties adapted to shorter growing regions in the prairie regions, reduced costs of production due to herbicide tolerance, improved agronomic methods and perhaps a more favorable climate supported soybean expansion in these areas (Gale, 2015, page 22).

Lines 131-136 and figure Fig. 3.4 (page 3-6) and lines 172-186 and Fig. 3.5 (Page 3-6):

Since soybeans are not grown in the same fields year after year, rotation is common, most often with corn. Increase in export of whole beans to China and elsewhere in Asia and the rest of the world influenced land use in the US by increasing soy acres and also subsequently influenced corn acreage both through common crop rotation practice and agronomic necessity.

⁶ Gale, F., Valdes, C., and Ash, M., 2019. Interdependence of China, United States and Brazil in Soybean Trade. USDA-ERS.

	N fixation by soybeans is minimal and most fixed N is exported in the beans. Soybean residues are small and mineralize more readily than corn residues. Reduced N levels in corn following soybean are likely due to increased soil organic matter mineralization, rather than N contributed by soybeans. (see for example: : https://extensionpublications.unl.edu/assets/pdf/ec117.pdf ; https://extensionpublications.unl.edu/assets/pdf/g859.pdf; there are many other sources). Soybeans have a penalty for soil organic matter accumulation and maintenance compared to corn. ARMS data are used to arrive at characteristics for corn-soybean rotations frequency, but ARMS data are variable and less reliable than other sources. (ftnt 4, page 3-5). How does EPA account for differences in data quality when estimating acreage attributions associated with the RFS? Page 3-5, lines 127 to 129, and ftnt 3: Prevent acres are discussed for the year 2019. A large number of acres of both corn and soybean were reported to the FSA as prevented from being planted, to keep those acres enrolled in FSA programs. These acres are accounted differently by NASS and acreage totals in NASS data are consistently larger than in FSA data. Page 3-5, Fig. 3.4. Does “other” include fallow? In areas of the western corn belt, fallow was historically an option in rainfall limited years and water-limited cropping systems and likely still plays a role. Line 167: The term “soil health” is used but not defined. Soil health is difficult to define well and not generally useful in soil science. It may be correlated with one or more measurable characteristics, but outside of such specific correlation, is not meaningful, (Powlson, D. 2021_Is ‘soil health’ meaningful as a scientific concept or as terminology? https://bsssjournals.onlinelibrary.wiley.com/doi/epdf/10.1111/sum.12721?saml_referrer). EPA should be careful when using language with policy implications so as not to confuse a qualitative term like health with varying and vague definitions when referring to quantitative effects with meaning for crop production and soil management generally. Lines 217 ff: NRCS reports (CEAP 1 and 2) are discussed. Effects include increased adoption of reduced and no-till methods, which can be protective against soil erosion. But also importantly, for adoption of landscape scale conservation practices (ftnt 7, page 3-8, and Table 3.1, page 3-8; Box3.1, page 3-28). To what extent is increased adoption of expensive landscape conservation practices attributable to the economic effects of feed grain prices associated with the RFS? Should these changes be accounted as positive environmental consequences of the RFS for the larger agricultural landscape? The figure below is from the Farm Bureau, but the amount of land in conservation-related programs could be independently verified by the appropriate federal agencies.
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Figure 5. Conservation Acreage as a Percent of Total Agricultural Land*Includes Acreage on Active and Completed CRP, CSP and EQIP Contracts*

Nowhere does EPA discuss increases in the efficiency of principle crop production, especially corn and soybeans. Why? How was increased efficiency and increased adoption of conservation practices related to the RFS, particularly to positive price effects?

Pest Management

Page 3-11 (ftnt 9): Why does EPA rely on a proprietary research firm for estimates of pesticide use on major crops in the US?

When soybeans or corn substitute for cotton, there is little change in herbicide use but less pesticide use. Cotton also uses herbicides and other materials as growth regulators to encourage crop maturation (Table 3.6; page 3-14). These are absent in corn and soybean.

Wheat may require greater use of fungicides, but less insecticide application. Applied acres are less than for corn and soybean, so wheat results overall in less pesticide application than corn and soybean (Page 3-15, Table 3.7).

Page 3-16, (Lines 352-365): Neonicotinids are mentioned, particularly their use as seed treatments. They are increasingly adopted as substitutes for organophosphate and carbamate pesticides, with known significant human, mammalian and aquatic risks. This substitution effect could be mentioned.

EPA does not comment on relative risks or provide any quantitative assessment of risk between crops or within crops for differing pesticide usages or management practices. There are such tools available such as the Pesticide Risk Tool from the IPM Institute (<https://pesticiderisk.org/>). Since EPA uses proprietary data and private sources for some of this report, it seems reasonable to also evaluate useful risk assessment tools.

Fig 3.7 is a useful presentation of trends in usage. Additional discussion of the literature cited would be useful.

Page 3-22, lines 65-8: EPA suggests that adoption of HT in soybeans at faster rates than in corn was due to ALS inhibitor resistance, but soybean is also a less competitive crop with weeds than corn.

Page 3-24, lines 117-121: EPA mentions risk briefly and for the first time.

	Page 3-24, lines 126: ECOTOXnet is cited. The actual citations mention that it may be out of date. EPA should improve on this aspect of its report. See comments above about IPMI. Fertilizer Use (page 3-25) Increases in N fertilizer to both corn and soybean are presented in Fig. 3.12 (Page 3-26). These increases are cited without mention of corresponding increases in yield and correspondingly larger rates of nutrient uptake. Increases in use do not automatically correspond to increases in emissions of nutrients from farm fields. Technologies like precision agriculture and the use of cover crops can reduce the risk of loss. Crop use Page 3-31 (Lines 286-288): Numbers for total corn use for feed are used, but then discounted due to a lack of accounting for distillers dry grains and solubles (DDGS). But Fig. 3.14 does account for DDGS. Why the difference? (lines 288-289): Corn exports have been stable over the entire period. This is relevant to subsequent use of economic models that propose indirect market effects on foreign land use from the domestic use of corn for ethanol in the US. Page 3-31, (lines 308-311): EPA states that the use of soybeans increased in most sectors from 1999/2000 to 2020/21. But data in Fig. 3.15 (page 3.1) suggest that the increase is almost entirely in exports of whole beans and secondarily in soy oil. During this time, the use of whole beans increased massively in China to support rapid increases in meat consumption⁷. The growth in wealth in China over a short period of time and its consequences for land use and trade in commodities worldwide was unprecedented in world history and largely unanticipated at such a scale. It influenced land use all over the world at a much more significant scale than did diversion of crop surpluses in the United States towards industrial use. Page 3-32, (lines 316-323): Uses of soybeans are discussed using weight, but exports in terms of bushels. Why the difference? Include both measures for clarity. My calculation is that exports increased from 30 M tons to 61 M tons over this period. This is larger than domestic uses. 3.2. FOG Box 3.2: Biogas. This source of renewable energy is projected to increase substantially. See the data sources highlighted in this footnote.⁸
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⁷ Gale, 2015. Development of China's feed industry and demand for imported commodities. FDS-15K-01; USDA-ERS; https://www.ers.usda.gov/webdocs/outlooks/36929/55490_fds-15k-01.pdf

⁸ From the Renewable Natural Gas Coalition website: <https://www.rngcoalition.com/infographic>; Argonne has a dataset of all facilities active or under construction: <https://www.anl.gov/esia/reference/renewable-natural-gas-database>; Total potential estimates across the US we usually point to this work by ICF: <https://gasfoundation.org/2019/12/18/renewable-sources-of-natural-gas/>; Gladstein and Associates: <https://www.gladstein.org/research/an-assessment-californias-in-state-rng-supply-for-transportation-2020-2024/>

	Page 3-32, Lines 344 ff: FOG is largely animal fats resulting from large-scale livestock agriculture (poultry, swine, beef and dairy). Corn and soybean feeds dominate this sector. FOG from livestock residues thus derives more or less directly from the growth and production of feed grains and can be considered a beneficial side effect of this use. This should be noted by EPA. See also Box 4.1, Page 4-18; lines 498-502. 3.3 Crop Logistics No comments. 3.4. Ethanol production Good description of wet and dry processes. Corn oil extraction in dry mills is not mentioned in the description of the process. The relatively large amount of corn oil produced per acre of corn when corn yields are large is not mentioned except in a footnote, nor is oil yield from corn compared to oil yield from soybeans on a per acre basis. This would be useful. For example, a 200 bu yield of corn with 3.8 % of oil in the grain contains nearly 500 lbs of corn oil per acre. A 55 bu per acre soy crop at 11 lb of oil per bushel yields approximately 600 lbs of oil. Page 3-36: Box 3.4. EPA states that the N-P-K-S contents in DDGS are non-optimal from a feeding perspective, and result in excess shedding of these nutrients by cattle. This misunderstands animal feeding, which is largely based on optimization of different combinations of feed ingredients to meet desired targets for protein, energy and fiber and other constituents, minimizing price. There are no perfect feeds, so all feed substitutions in general result in some imbalance in nutrients consumed and nutrients shed. This is not unique to the use of DDGS and should not be specifically attributed to the use of DDGS. Inefficient use of nutrients in livestock diets varies due to many factors, including protein quality in corn, the skill of nutritionists and the availability and price of diverse ingredients in formulating livestock total mixed rations (TMRs). Page 3-38: Co-products. Page 3-39, lines 516-517: 2.9 gal/bushel is used, but on page 3-37, line 459, 2.8 lbs/bu is used. Page 3-39: Biodiesel and renewable diesel: Good discussion. Transport and logistics: clear discussion. But no mention was made of higher alcohol blends, perhaps because of potential equipment limitations. Page 3-41: Biofuel logistics. Helpful discussion. Page 3-43: Dispensing. No comments are made about the reduction of BTX in gasoline by the use of ethanol as a fuel oxygenate, and the potential public health benefits from this reduction. 3.6 Biofuel End Use E85. No comments Biodiesel. No comments.
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	Renewable diesel. No comments.
Kurkalova	I recommend adding a greater emphasis on the year-to-year dynamics of no-till and cover crops in both Chapter 3 and in relevant chapters of Part 3 (Environmental outcomes). Some specific suggestions are listed below: • Chapter 3, Key Findings bullet 3 (line 38): Add a clarification that the extent to which the conservation practices are used continuously from one year to another is largely unknown. • Chapter 3, section 3.2.1: Just like with conservation tillage, farmers do not necessarily use cover crops continuously. The few estimates that have been developed suggest that, on average, the discontinuity of the use of cover crops is not as wide-spread as the discontinuity of no-till (Wallander et al. (2021); suggested references: Dunn et al., 2016; Sawadgo and Plastina, 2022). • Chapter 3, section 3.2.1.3: An emerging area of research utilizes satellite-based data to provide information on spatial and temporal variation of tillage types and cover crop use (suggested references: Seifert et al., 2018; Hively et al., 2019; Beeson et al., 2020; Sharma et al., 2020). This area of research could be specifically mentioned as potentially useful for identification of over-time use of farming practices on fine geographic scale. Another recommendation concerns the need for additional research on socioeconomic barriers to greater use of cover crops and conservation practices in general. A currently active area of research focuses on the co-use of such practices, and particularly the complementarity between conservation practices. Here complementarity is defined as an increase in the probability of using one practice given the use of the other practice. Lee and McCann (2019), Canales et al. (2020), and Gong et al. (2021) (all additional references) specifically discuss the potential complementarity between cover crops and no-till.

3.1.4 Chapter 4: Biofuels and Agricultural Markets

Reviewer	Comments
de Gorter	Clarity and Technical Accuracy I disagree D6 RIN prices reflect the incentives of the mandate prior to January 2013 – explain why elsewhere. I disagree the RFS could have increased corn ethanol production by up to 5 billion gallons after 2012. I explain elsewhere. Ethanol prices were never high enough relative to gasoline to generate a counterfactual of no RFS having a reversal in the production of BOB with match blending at 10% or RFG gasoline below 10% as need octane enhancer. Otherwise, it is a good synthesis.

Literature

The literature review on the overall effects of ethanol on corn prices (pages 4-11 to 4-13) omits an important literature I identify above.

Use of Models

See my comments below.

There is no synthesis section.

It does not identify uncertainties and limitations associated with its conclusions, nor gives suggestions for improvement.

Comments on Chapter 4 - Biofuels and Agricultural Markets**Lines 16-18**

Renewable Identification Number (RIN) prices for renewable (D6) fuels provide evidence that the Renewable Fuel Standard (RFS) Program increased U.S. consumption of renewable biofuels in 2009 (and late 2008) and from 2013 to 2019.

Unlike for D4 RIN prices (see Figure 2), D6 RIN prices were nowhere near related to the ethanol mandate price premium (see Figure 1 and compare to D6 RIN prices – no relationship). This is because the ethanol market is so different than the BBD market, with sharp changes in policies and engineering and logistics challenges. Watering down the scotch was the order of the day as the industry by late 2007 saw the handwriting on the wall and quickly went to 10% ethanol blends by embarking on BOB. E10 blend ratio almost reached 10% in 2010 (see Figure 2 in [Report with Radich](#)). It went fast. States like Alaska that were exempt but had E10 anyways as did SREs. Everybody watered down the scotch even if they did not have to. That's the point.

It is true in Figure 10.3 in [The Book](#) that when ethanol prices were almost \$1 above gasoline, D6 RIN prices peaked at 16 cents! But that is peanuts, and yes, it looks like some blenders broke away from watering down the scotch and the path to BOB by trying to take a (slight) advantage in increasing their fuel sales. But it is a mere small blip that surely does not overturn my assessment of what happened in this period?

The high D6 RIN price after January 2013 was due to the blend wall and more likely reflects the marginal cost of adding BBD to fill the implied corn-ethanol mandate in the RFS2 than the calculated ethanol price premium due to the mandate as measured by equation [3] and shown in Figure 1 above.

In fact data in Figure 2 of the [Report with Radich](#) shows the blend wall was reached in 2012 and the ethanol mandate blend ratio was above the total ethanol blend ratio. The tax credit expired in 2012 so the relatively high ethanol mandate price premium was not related to the D6 RIN price at all.

Therefore, D6 RIN prices do not reflect the degree to which the mandate is binding. See [deGorter Summary Biofuel Theory](#) for how to calculate.

It is ironic that a high D6 RIN price reflects the blend wall and there is less ethanol being used as a result. For sure the RFS2 is binding but really the blend wall is binding because

BOB means irreversible investments. The blend wall is binding, and it is now, *ex post*, a “market” outcome. The RFS2 only increases ethanol production through extra ethanol consumed in E15 and E85.

Lines 19-22

Advanced (D5), biomass-based diesel (D4), and cellulosic (D3) RIN prices provide evidence that the RFS2 increased U.S. consumption of advanced, biomass-based diesel and cellulosic biofuels in every year of RFS2 for which standards had been set for these fuels (i.e., starting in 2010).

Yes, BBD is totally different.

Lines 23-26

Prospective studies of the expected impact of RFS Program on corn ethanol production, estimated that the RFS Program could increase corn ethanol production between 0 and 5 billion gallons under scenarios with relatively high oil prices (greater than \$60 per barrel in 2018 prices).

For which years? For 2013 to 2019, the blend wall is binding which is equivalent to E10 = 10% (can go as high as 10.4%). We have 100% of the blendstock as BOB and all blenders are doing 10% match blending (there is no price competition between E0, E2, E5, E7 and E10, unlike for BBD) so if crude oil prices are \$68 per barrel, and you removed the RFS2, why would the industry reduce ethanol consumption by up to 5 billion gallons. It seems unreasonable to me.

Lines 30-31

Prospective studies suggests that the RFS2 increased biomass-based diesel consumption 0.9–1 gallons for every gallon in the biomass-based diesel volume obligations.

Why not 1?

Lines 30-31

This is equivalent to an increase in biomass-based diesel consumption of 0.6–0.7 gallons for every gallon in the advanced volume obligations.

Why? It would be useful to provide data on overages (D4 into D5 and D4 and D5 and BBD D6 into the D6 category shown in Tables 3-5 in [Report with Radich.](#))

Lines 34-35

Prospective studies suggest that for every billion-gallon increase in biomass-based diesel production, soybean prices were estimated to increase 1.8–6.5%.

When I ran the Drabik, de Gorter and Timilsina (2014) closed economy model to reproduce 44% increase in soybean oil prices for a billion-gallon increase in BBD (FAOSOM and FAPRI models), I get an 11% increase in the price of soybeans. So, I do not understand how the increase in soybean prices reported here are so low relative to the increase in soybean oil prices. It depends critically on the relative demand

elasticities for the demand for soybean oil versus that for soybean meal. In world markets, I suppose these elasticities can diverge significantly.

Lines 34-35

The RFS2 was estimated to have a limited impact on soybean meal production (decrease of 1.2% per billion gallons of biodiesel) and put downward pressure on soybean meal prices (decrease of 4.1% per billion gallons of biodiesel).

I get a decrease of 8.1%

Lines 86-99

Figure 1 shows mandate premiums when D6 RIN are prices low. The cost of compliance was zero because of watering down the scotch and saw the handwriting on the wall and no price competition among different blends of ethanol (unlike for the case of BBD).

In other words, ethanol was being sold above the point where a consumer would be indifferent between ethanol and gasoline on a miles per gallon basis. Not so for BBD.

Lines 123-126

Whistance and Thompson (2014) examined RIN price behavior between January 2009 and May 2013 and found that 94% of the time they responded in ways consistent with expectations (based on whether a mandate was binding, the hierarchical nesting structure of the different RFS standards, and RIN vintage).

Not true. Compare Figure 1 with actual D6 RIN prices up to January 2013. No relationship. And not even after 2013.

Lines 126-127

*Focusing only on biomass-based diesel, Irwin et al. (2020) found that RIN prices **also** largely adhered to expectations.*

Exactly! Irwin et al. (2020) reproduced my Figure 2. They used my equations to calculate the premiums. See [The Book](#) or all of the papers listed on page 2 and 3 of [deGorter Summary Biofuel Theory](#) on how to do so. But to say “also” is erroneous. D4 RIN prices are “consistent with expectations” but not D6 RIN prices.

Babcock (2020) commenting on Irwin et al. (2020) wrote that their methodology works for BBD but how about D6 RIN prices! Well, it does not work out for the reasons I have been repeating above in my comments here.

Lines 245-250

A comprehensive review of this literature is outside the scope of the present report. Where possible, existing reviews that synthesize the many available discussion papers, reports, and peer-reviewed articles are relied upon. Because many of these studies provide insights about how increasing corn ethanol production is likely to affect corn prices, even when they do not specifically focus on the RFS, a broader

range of studies that examine the effects of changes in ethanol volumes on corn prices is considered.

I agree with everything in this paragraph, and I think the EPA should review “a broader range of studies that examine the effects of changes in ethanol volumes on corn prices is considered.” It is useful as an overview for the reader before one gets into the nitty gritty of how much corn production can be attributed to the RFS specifically.

Please note that one specific yet important and very significant literature has been excluded. It is summarized in [The Book](#) or in all of the papers listed on page 2 and 3 of [deGorter Summary Biofuel Theory](#). This approach comes to very different conclusions as to the total effect of ethanol on corn prices compared to the studies the EPA Report surveys. So please include all of these in your future report.

But these papers also adopt the framework:

- Drabik, Dusan. (2011). “The Theory of Biofuel Policy and Food Grain Prices”. Working Paper 2011-20. Charles H. Dyson School of Applied Economics and Management, Cornell University. December.
- Moschini, G., H. Lapan and H. Kim, “The Renewable Fuel Standard in Competitive Equilibrium: Market and Welfare Effects,” *American Journal of Agricultural Economics*, 99(5) (2017):1117-1142 and 1146-1147
- Lapan, H. and G. Moschini, “Second-best biofuel policies and the welfare effects of quantity mandates and subsidies,” *Journal of Environmental Economics and Management*, 63(2012): 224-241
- Cui, J., Lapan, H., Moschini, G., and Cooper, J., “Welfare Impacts of Alternative Biofuel and Energy Policies,” *American Journal of Agricultural Economics*, 93(5) (2011):1235-1256.
- Moschini, G., Lapan, H., Cui, J., & Cooper, J. (2010). Assessing the welfare effects of US biofuel policies. *AgBioForum*, 13(4)(2010): 370-374
- Mallory, Mindy L., Scott H. Irwin, and Dermot J. Hayes. “How Market Efficiency and the Theory of Storage Link Corn and Ethanol Markets.” *Energy Economics* 34, no. 6 (2012): 2157–2166.
- Irwin, S.H., McCormack, K. and Stock, J.H. (2020), The Price of Biodiesel RINs and Economic Fundamentals. *Amer. J. Agr. Econ.*, 102: 734-752. They use a modified version of equation [2] for biodiesel
- Babcock, BA. (2013). Ethanol without subsidies: An oxymoron or the new reality? *Am J Agric Econ* 95: 1542 1317-1324. Babcock re-derives equation [1] and uses an incorrect version of equation [2] for ethanol

All the studies do not use the results of the omitted literature I outline above. If you read those papers (see [The Book](#) or all of the papers listed on page 2 and 3 of [deGorter Summary Biofuel Theory](#) as examples), there is no way ethanol (in total – we are not talking about one program or another) was to increase corn prices by 17% (Condon et al.) or 13% (Hochman and Zilberman) from 2006 to 2012. Nobody believes that. Read

	page 17 of deGorter Summary Biofuel Theory where Zilberman determines ethanol had little impact on corn prices to July 2008! He uses his study in the meta- analysis but not mine (now just read the analysis of Table 1 in deGorter Summary Biofuel Theory). How could one conclude a small increase in corn prices due to ethanol? Relying on Zilberman, Condon and so many other papers who miss the key literature (also Persson, NRC, Zhang et al., Thompson et al.) needs to be corrected in the Report. Now Section 4.3.4 is more specific on the impact of the RFS impact only and I say very little (or all of it if the RFS is the cause of BOB! But we have no way to tell). The literature on the effects of BBD on soybean markets requires careful analysis of substitution in demand between vegetable oils and the fact that soybean meal prices decline. I would start with non-black box models⁹ like: <i>Soybeans only</i> • Drabik, Dusan, Harry de Gorter, and Govinda R. Timilsina. 2014. "The Effect of Biodiesel Policies on World Biodiesel and Oilseed Prices." <i>Energy Economics</i> 44: 80—88. • Cui, J. and Martin, J.I., 2017. Impacts of US biodiesel mandates on world vegetable oil markets. <i>Energy Economics</i>, 65, pp.148-160 • Jayson L. Lusk Food and Fuel: Modeling Food System Wide Impacts of Increase in Demand for Soybean Oil Purdue University Prepared for United Soybean Board November 10, 2022. <i>Soybeans and Corn</i> • Moschini, G., H. Lapan and H. Kim, "The Renewable Fuel Standard in Competitive Equilibrium: Market and Welfare Effects," <i>American Journal of Agricultural Economics</i>, 99(5) (2017):1117-1142 and 1146-1147 Then when one gets a feel for what determines the relative change in soybean oil, soybean meal and soybean prices with an increase in BBD, then go to black box models but at least you know what you are looking for and why.
Hill	Section 4.6 should be updated to include post-2019 data.
Hoekman	In reviewing Chapter 4, a couple of typographical errors were detected. These are identified below: • Page 4-2, line 30: change "suggests" to "suggest" • Page 4-31, line 801: change "unites" to "units"
Kurkalova	The presentation of the Lark et al. (2022) study in section 4.3.4 is short. I suggest expanding it to explain what specific limitations lead to the conclusions stated in lines

⁹ GTAP, FAOSOM, FAPRI etc.

	407-411. Such as explanation could be provided either in the section 4.3.4 or in section 6.3.3.
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3.1.5 Chapter 5: Domestic Land Cover and Land Management

Reviewer	Comments
Davis	Chapter 5 Lines 125-127 of chapter 5 state the importance of small-scale changes in LCLM, which supports the point that small changes are important to consider, but this is in contrast with the general focus of the report avoiding advanced biofuels that are produced in smaller amounts. Lines 144-145 highlights that farmers producing corn and soybean are not necessarily aware of the end use of each unit sold. This is not the case with advanced feedstocks, which again suggests advanced feedstocks would be a better metric to assess the efficacy of the RFS. The 32% change noted in line 381 is inaccurate. The percentage change based on the numbers in table 5.2 is more like 20%. Both the reduction of agricultural diversity and increase in management intensification are cited several times (e.g. line 423, 429-431, 493-495), but there is no discussion of the potential for advanced biofuels to mitigate this. For example, perennial grasses can diversify the landscape and require less inputs. The direction of change (positive or negative) is not clear in figure 5.5.
Hill	Chapter 5 Line 166: “Pristine natural habitat” is not equivalent to land “never cultivated”.
Hoekman	Section 5.1.2 (page 5-5) is very useful in providing definitions of many terms related to land cover (LC), land management (LM), and other concepts. As pointed out here, use of different definitions and terminology can lead to different conclusions across a range of studies. Apparently, preferred terminology is evolving, as the term “Land Cover and Land Management” (LCLM) is used in RtC3, whereas the term “Land Use and Land Use Change” (LULUC) was used in RtC2. In the broader literature, the terms Land Use Change (LUC) and Indirect Land Use Change (ILUC) are frequently used. ILUC in particular has engendered considerable confusion and controversy, yet this term does not appear in RtC3. (ILUC is not even included in the list of Acronyms and Abbreviations provided at the beginning of RtC3.) A couple sentences should be added to identify the term ILUC, and explain what it means and why EPA prefers other terminology. Figure 5.2 (page 5-12) uses a set of stacked bar graphs to depict trends in U.S. cropland from 1982 to 2017. Because of the uneven time spacing of the bars, a line graph with regular time spacing on the x-axis may be a better way to show this information.

	In the Conclusions of Chapter 5 (and several other places throughout the report), it is stated that “corn likely has larger environmental effects than hay, pasture, and other crop types because corn uses more fertilizer, pesticides, and other inputs than other crops.” Some quantitative sense of these larger effects should be provided, along with references to later chapters where these effects are discussed in greater detail. In reviewing Chapter 5, a few typographical and other minor errors were detected. These are identified below: • Page 5-11, line 271: “... to compliment insights <u>from the</u> NRI ...” • Page 5-16, line 393: change “Figure 5.4” to “Figure 5.4b” • Page 5-16, line 401: change “Figure 5.4” to “Figure 5.4c” • Page 5-26, line 580: change “North Missouri” to “northern Missouri” • Page 5-34, line 737: change “Figure 5.13” to “Figure 5.14” • Page 5-35, line 766: change “Figure 5.14” to “Figure 5.15”
Kaffka	Chapter 5¹⁰ <u>Cited Literature</u> I have added some citations which I think they add clarity or provide alternative viewpoints. For clarity, I have added figures derived from literature reports or other sources not cited in the chapter. Most of my comments are embedded with the page and lines where they occur. <u>Selection and Use of Models</u> EPA seems to favor some modeling approaches over others, especially assessments based on the use of CDL data. This is incomplete or potentially misleading in my view. <u><i>Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented?</i></u> Not entirely in my view. The increase in croplands cited for the 2007 to 2012 period was not linear if NASS data are considered. Rather, land use varied around a long-term average value for major crops during this period. The variation may be greater than the amount of acres attributed to the RFS. The notion of agricultural intensity is associate more with annual production of corn and soybeans compared with wheat, but hay crops like alfalfa and some grass forages can be intensively managed as well.

¹⁰ Most of my comments and answers to these questions are contained in the page and line number comments below. It was easier and made greater sense to comment where questions or issues arose directly in the text.

Uncertainties and Limitations

EPA is tasked here with identifying small changes at the margin of a large amount land use for major principal crop production in the US landscape, and attributing these to the RFS. There are many dynamic, long-term forces acting on land use for crop production over time, so this is a challenging task. RFS effects can only be estimated, especially since they reflect a small time period during which longer-term processes are occurring. Better discussion of uncertainty and disagreements about methods of assessment, land conversion amounts and rates among authors and research groups would be helpful.

Estimates of attribution hinge on highly technical approaches to assessment, which are not universally accepted as appropriate or accurate in the literature. I think EPA's assessment of this issue and the relevant literature is incomplete. EPA should include a better technical assessment and direct comparison of the strengths and weaknesses of the methods used in papers which they rely on for attribution. (see Page 5-10 ff).

Key Findings:

Page 5-2: lines 22-31: Cultivated cropland declined, but corn and soy acres increased, especially soy acres. Shifts in cropping patterns varied across the US in response to many local and market factors. The increase in soy acres compared to corn seems not to be acknowledged.

It would be helpful to depict CRP acreage trends, especially in context with other federal programs to manage cropland acres over time (See comments in Chapter 3). CRP levels are set by congress and are not a market-driven category, nor are enrolment levels determined by the RFS. Any crop could be planted on fields being removed from the CRP, so acreage in this category is difficult to attribute directly to the RFS. CRP grasslands, once de-enrolled, could be grazed by cattle. Beef consumption has declined in recent years, while consumption of meat from non-ruminant monogastric species (poultry and pigs) has increased. These species are larger users of soybean meal than are cattle. In the face of declining beef consumption, hay and pasture become less profitable land uses and a transition to annual cropping makes economic sense for those acres capable of being cropped. Dietary trends influence land use but are not related to the RFS.

Cropland-pasture and non-cultivated cropland are difficult categories to define and measure with accuracy. In some regions and on some farms, these categories are fluid and transitions are part of long-term management. EPA should not rely too strongly on strict definitions of these land uses and categories in identifying transition effects (Page 5-2; lines 39-41). This may be the case especially in regions on the western margins of the traditional corn belt, where weather variability, especially precipitation amounts, are part of long-term risk and land management strategies for farmers.

(Page 5-2; line 51) Increases in soybean and corn acres along the western margin of the corn belt have resulted from breeding improvements in corn and soybeans that led to better adaptation of these crops to local environments, also to technology evolution (no-till and reduced till), to changes (increases) in precipitation and to increased demand for soybeans from Asian markets that are served most efficiently from this

	region by rail to west coast ports (Gale 2015 and Gale et al., 2019)¹¹. Corn acres increase in tandem commonly with soybeans, as natural agronomic complements. The western corn belt region appears to be experiencing a change in annual precipitation that supports a shift in cropping patterns, accompanied by farmer learning¹². There is data and modeling from the Dakotas region and other areas along the edge of the western corn belt to suggest that increased production of corn has led to increases in soil organic matter over time in this area¹³. (Page 5-4; lines 110-113): Reasons for increases in soy acres fail to identify increased export demand as a motivator for increased planting (<i>see comments and citations above</i>). The reference cited is from a time before Asian demand increased to its largest level and predates the timeframe of interest. Pages 5-4 and 5-5; lines 113-114: The decline in wheat acres mentioned predates the RFS by decades (since the 1980s) and cannot be attributed to the RFS. Wheat yields have increased much more slowly than corn yields. This seems to be related to inherent genetic characteristics that affect the rate of improvement to both crops (corn yields have been increasing at a greater rate than wheat), which in turn supports a tendency to adopt corn compared to wheat in many areas of the Midwest. In addition, since wheat production is export focused (nearly half of US production), increasing international competition has reduced demand over time and forced prices downward. An exception occurred recently when Russia invaded Ukraine and disrupted wheat exports from that region. Page 5-5; lines 124-127: EPA's hypothesis: <i>"This longer-term context is important when interpreting the changes from 2005 to current, which are the focus of the RtC3. However, even though LCLM changes from 2005 to current may or may not be small relative to some earlier periods, that does not preclude their potential impacts on the environment as the cultivation of various crop types have varying environmental impacts..."</i> As noted, these changes are small in an historical context. But by providing greater historical context, EPA can allow congress and other readers to conclude for themselves the importance of the changes attributed to the RFS during the short period reviewed. See for example this figure from Coppess et al. (2020)¹⁴ that highlights land planted to
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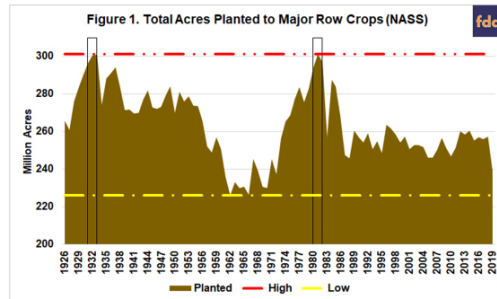
¹¹ Gale, F., Valdes, C., and Ash, M., 2019. Interdependence of China, United States and Brazil in Soybean Trade. USDA-ERS. See also: Gale, 2015. Development of China's feed industry and demand for imported commodities. FDS-15K-01; USDA-ERS https://www.ers.usda.gov/webdocs/outlooks/36929/55490_fds-15k-01.pdf

¹² "... Using maize as a benchmark, we estimate that if warming continues unabated through the first half of the 21st century, the best climatic conditions for rainfed maize and soybean production may shift from Iowa and Illinois to Minnesota and the Dakotas with possible modulation by soil productivity." Hoffman, Kemanian, and Forest. 2020. Env. Research Letters 15(9). The response of maize, sorghum and soybean yield to growing phase climate revealed with machine learning. DOI 10.1088/1748-9326/ab7b22

¹³ Corn Yields and No-Tillage Affects Carbon Sequestration and Carbon Footprints. David E. Clay, Jiyul Chang, Sharon A. Clay, James Stone, Ronald H. Gelderman, Gregg C. Carlson, Kurtis Reitsma, Marcus Jones, Larry Janssen, Thomas Schumacher. May 2012; <https://doi.org/10.2134/agronj2011.0353>

¹⁴ Coppess, J., N. Paulson, G. Schnitkey, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink](#)

major crops over time. Current use is approximately mid-way between historic highs and lows, and year to year variation, even in the last years since the start of the RFS programs, is close to or greater than the amount of LCLM changes mentioned in the text.



Coppess, J., N. Paulson, G. Schnitkey, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink](#)

Page 5-6, lines 168-173: EPA asserts: *"lands and more specifically croplands that are set aside for years can accumulate carbon, become suitable habitat for many species, and thus can begin to provide many ecosystem services over time (Johnson et al., 2016). Agricultural lands also provide ecosystem services (e.g., carbon sequestration), though the magnitude and composition of these services differ from unmanaged lands. Thus, agricultural expansion onto lands that were once cultivated at some point may incur similar kinds of environmental effects as expansion onto pristine habitat, albeit at lower levels."*

This is a difficult assessment and (potentially) a difficult regulatory problem. Much here seems to depend upon the definition of grassland, cropland pasture, and the location and cultivation history of individual fields. Can EPA justify using average values when discussing this heterogeneous, dynamic category? Especially since the amounts of land at stake are small relative to total cropland and effects that are considered important will vary most at the local scale and annually. In addition, in the western corn belt, longer-term crop rotations involving hay, pasture, fallow land and annual cropping have long been important economic and risk management strategies. Too short-term a focus on this process may misattribute the actual importance of the RFS on management decisions¹⁵.

Page 5-8, lines 195-196. EPA uses NASS data for annual information on cropland, but NRI data for total cropland acreage (lines 195-196). It seems hard to escape the conclusion that the choice of the data set also is a choice of the outcome of the analysis.

Page 5-10, lines 241-248: ... *"The LCLM that is most relevant to the EISA Section 204 Report Series are those pertaining to any lands that went into production either directly*

¹⁵ Land-Use Change Impact on Soil Sustainability in a Climate and Vegetation Transition Zone. K. D. Reitsma, B. H. Dunn, U. Mishra, S. A. Clay, T. DeSutter, and D. E. Clay*. "Our assessment suggested that land-use changes from 2006 to 2012 were likely linked to several factors including a desire to increase financial returns, government policies, climate change, a changing ownership structure, improved technology, and an aging workforce"...Page 2369.

	<i>to support the production of feedstocks used for biofuels (i.e., direct changes in LCLM), or indirectly because of cascading effects from the diversion of existing crops to this new market (i.e., indirect changes in LCLM). It is very difficult to isolate the subset of LCLM attributable to the RFS Program using these reports <u>Thus, as noted above, these reports are more useful for describing the broader trends in agricultural land, some of which might be attributable to biofuels and/or the RFS Program.</u></i> A set of references cited by EPA here, (largely from the same authors) rely on the use of CDL data to estimate grassland conversion in the western corn belt and larger Missouri River Agricultural Basin (MOAB). These papers assert that CDL data can be used to assess land conversion from grasslands to annual crops, and these conversions can be attributed to demand for corn and soy beans attributable to the RFS¹⁶. Differences in land use assessment based on differences in the data used create a margin of error for any estimates of LCLM here. EPA should refer always to this margin of error in its attribution analysis. Estimation error may be larger than the actual effects of the program, especially given differences among data sources and assumptions about cause and effect. For example, two references: (1) Copenhagen, K; Hamada, Y; Mueller, S; Dunn, JB. (2021) examine the characteristics of the cropland data layer in the context of estimating land cover change. IJGI 10: 281. https://dx.doi.org/10.3390/ijgi10050281 ... and (2) Dunn, JB; Merz, D; Copenhagen, KL; Mueller, S. (2017, Biofuels, Bioproducts and Biorefining 11: 247-257), Measured extent of agricultural expansion depends on analysis technique. https://dx.doi.org/10.1002/bbb.1750)¹⁷ highlight the difficulties, potential inaccuracies and raise caution about using CDL values to document LCLM. In particular, they note the danger of overestimating land conversion from perennial or cropland pasture uses to cultivated cropland. They note the difficulty of identifying fallow land as a part of a cropping system. There are also other references that are relevant to this discussion as
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¹⁶ Lark, TJ; Schelly, IH; Gibbs, HK. (2021). Accuracy, bias, and improvements in mapping crops and cropland across the United States using the USDA cropland data layer. Remote Sensing 13: 968. <https://dx.doi.org/10.3390/rs13050968> ; Lark, TJ; Spawn, SA; Bougie, M; Gibbs, HK. (2020). Cropland expansion in the United States produces marginal yields at high costs to wildlife. Nat Commun 11: 4295; Lark, TJ; Mueller, RM; Johnson, DM; Gibbs, HK. (2017). Measuring land-use and land-cover change using the US Department of Agriculture's cropland data layer: Cautions and recommendations. International Journal of Applied Earth Observation and Geoinformation 62: 224-235. <https://dx.doi.org/10.1016/j.jag.2017.06.007> . Lark, TJ; Salmon, JM; Gibbs, HK. (2015). Cropland expansion outpaces agricultural and biofuel policies in the United States. Environ Res Lett 10: 044003. <https://dx.doi.org/10.1088/1748-9326/10/4/044003>.

¹⁷ See also: Wang, M., Wander, M., Mueller, S., Martin, N. and Dunn, J.B., 2022. Evaluation of survey and remote sensing data products used to estimate land use change in the United States: Evolving issues and emerging opportunities. Environmental Science & Policy, 129, pp.68-78. <https://reader.elsevier.com/reader/sd/pii/S1462901121003762?token=E82F2EE51DE6774DDB510172A956279AAE9298E3B6C8EEF43B28900DBFB23336ABBB1B8686D856B654EEEA61B223CAA4&originRegion=us-east-1&originCreation=20230217212810> ; Joshi, D.R., J. Ulrich-Schad, T. Wang, B.H. Dunn, S.A. Bruggeman, and D.E. Clay *, 2019. Grassland retention in the North America Midwest following periods of high commodity prices and climate variability. Soil Sci. Soc. Am. J. 83:1290-1298; Reitsma, K.D., D.E. Clay, S.A. Clay, B.H. Dunn, and C.L. Reese. 2016. Does the U.S. cropland data layer provide an accurate benchmark for land-use change estimates? Agron. J. 108:266-272; Reitsma, K.D., B.H. Dunn, U. Mishra, S.A. Clay, T. DeSutter, and D.E. Clay. 2015. Land-use change impact on soil sustainability in a climate and vegetation transition zone. Agron. J. 107:2363-2372.

	well, including estimates provided by scientists working directly within the landscapes discussed (see footnote 9). These differences of opinion revolve around the proper use of very large data sets, especially the CDL, compared to the use of other approaches, like other remotely sensed data. All the studies rely on the use of large data sets requiring sophisticated data interpretation and analysis techniques. I find it hard to resolve these disagreements, even with careful reading of the papers. Methods used in these papers are technically challenging. Since EPA makes substantial use of these papers in its assessment in this and many subsequent chapters, it would be useful if EPA could evaluate or have evaluated at a technical level these differing claims and provide that analysis to congress and other readers of the RtC3. Page 5-12, lines 292-305: EPA's estimates increased cultivated cropland... as just over 1 M acres per year from 2007-2017; but actual the numbers seem to be for less than 1 M ac/yr. 5.3.1.4 Crop rotations: Page 5-29: lines 630-631: The use of more fertilizer is relative to the ability of crops to recover fertilizer, not absolute amounts. Fertilizer use efficiency seems to be increasing in corn production, as in many other crops, and new technologies have developed and are developing to push this process further in the direction of improved efficiency. In general, EPA seems to consistently avoid the issue of resource use efficiency in its analysis. Page 5-29, lines 634: Corn receives the largest amount of "pesticides", here glyphosate, which is an herbicide with relatively low toxicity. Page 5-29, lines 642-647: EPA uses the phrase: "...more rotations of corn" when good usage would be.. "more rotations <u>with</u> corn". Rotation, by definition includes the use of alternating crops. Corn seems always to be the driver of this discussion but rotation with soybeans are common and in some years soybeans are the most profitable crop (and the driver of the rotation). This may especially be the case in the areas of the western corn belt that can more cheaply supply Asian export markets for whole soybeans (See Gale et al., 2019; and Gale, 2015; cited above). Page 5-31, Lines 686-692. Recent projections (2023) are for increases in wheat production, limiting corn and soybean acres for production. This is due to both domestic and world demand and perhaps to "geopolitical factors," among others. These were and always are unpredictable. <i>Ceteris paribus</i> predictions are of limited value in this analysis. For example: "...Two consecutive seasons of wheat production shortfalls in the U.S. (spring wheat in 2021, winter wheat in 2022) sent Chicago winter wheat futures trading nearly 25% higher in Fall 2022 than in the previous winter wheat planting season. The higher prices encouraged a surge in winter wheat planting last fall – a move that will likely take away acreage from corn and soybeans in 2023." https://www.farmprogress.com/crops/wheat-expansion-to-limit-corn-soybean-acreage-growth-in-2023 . If this report is true, then the trends projected in Fig. 5.12 are inaccurate for 2023. In addition, the recent Russian invasion of Ukraine disrupted
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	exports from an emerging grain export hub, resulting in price increases and short-term shortages. Page 5-34, lines 726-730: But EPA thinks the exercise of projection is reasonable: “... <i>It is important to reiterate the wide fluctuations in actual historical plantings compared with the relatively stable plantings in future projections (Figure 5.12). This is not unexpected, as the steady-state assumptions in the modeling will almost certainly not occur in the real world. Nevertheless, these estimates are not expected to be biased high or low and reflect reasonable expectations in the absence of shocks to the agricultural system.</i>” By definition and in recent experience, shocks are unpredictable, but can be expected. For example, this ignores the fact that in 2022, there was major shock to the international system of trade in feed grains and wheat resulting from the Russian invasion of Ukraine. These were made up by surpluses from other export regions including the US (a welcome example of resilience in the global food system) and the eventual restoration of some exports from the region. Page 5-36, line 739: How does EPA define industrial uses? Ethanol is an industrial use of corn.
Kurkalova	Section 5.4.4: add a recommendation on improvements in the skill of satellite- derived data to successfully track implementation of best management practices such as conservation tillage and cover crops.

3.2 Part 2: Chapters 6 and 7: Attribution to the RFS Program

3.2.1 Question 1: In the First and Second Triennial Reports to Congress on Biofuels (RtC118 and RtC2, respectively), the Agency could not separate the effects of the RFS Program from the effects of other factors (e.g., market or other policy effects). Many factors simultaneously influenced the production and use of domestic corn ethanol in the U.S., including the need for fuel oxygenates to replace methyl-tert-butyl-ether (MTBE) in gasoline, the Volumetric Ethanol Excise Tax Credit (VEETC), high oil prices, and dozens of individual state biofuel programs and MTBE bans over this period. Because attribution was identified as a major knowledge gap in previous reports, this report includes a new emphasis on attribution, referred to in this report as an “attribution analysis.” Part 2 in this report (Chapters 6 and 7) covers the attribution analysis of the RtC3. For each chapter that you review:

- a) Please describe your overall impressions of the clarity and technical accuracy of material presented in the chapter.**
- b) Are the most important data and recent relevant literature cited and appropriately interpreted? Have any important studies that would affect the conclusions been**

¹⁸ U.S. EPA. Biofuels and the Environment: First Triennial Report to Congress (Final Report, 2011). U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-10/183F, 2011.

omitted? Please identify any data, literature, or studies you think should be added and describe why you recommend they be included.

- c) Please comment on the selection and use of models (e.g., econometric models, general and partial equilibrium models), the accuracy of analyses, and accuracy of inferences.
- d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement.
- e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.

3.2.1.1 Chapter 6: Attribution – Corn Ethanol and Corn

Reviewer	Comments
Davis	1a. The chapter seems technically accurate. Several approaches were used to probe attribution of RFS to changes in corn ethanol and corn production. While there are many interacting factors to consider, the approach was logical and clearly explained. Some information was a bit repetitive and there are section that could be reduced in length. Blend wall issues could be added to Table 6.2 to complete the factors that are highlighted there as interacting drivers of change. In section 6.3.1, criteria for non-binding RFS are defined. Consumption levels that do not meet the mandate levels and are lower than the mandates could indicate non-binding RFS, but that is not included in the explanation (p. 6-19). 1b. There was a lack of information about corn stover use as a cellulosic feedstock. Generally the background section overlooks the drivers associated with advanced biofuels, despite these being mandated by the RFS. It is true that grain-based bioethanol dominates the biofuel market and there are more data describing that market, but emerging cellulosic technologies are also making use of corn in small amounts. Even though these advanced uses are relatively small, they are potentially more directly responsive to the RFS mandates. There is a lot of literature on this topic. Sources that could be considered include: • Panoutsou C, Bauen A, Duffield J. 2013. Policy regimes and funding schemes to support investment for next-generation biofuels in the USA and EU-27. Biofuels, Biofproducts, & Biorefining, DOI:10.1002/bbb.1428. • Gallagher PW. 2014. The regional effects of a biomass fuel industry on US agriculture. Energy Policy 69:598-609.

	Witcover J, Williams RB. 2020. Comparison of “Advanced” biofuel cost estimates: Trends during rollout of low carbon fuel policies. Transportation Research Part D 79: 102211. 1c. The models selected for review were clearly explained and the rationale seems sound, but econometric models are not my area of expertise. 1d. The chapter synthesis reflected the chapter. Note: The overall synthesis of the report (ES and IS) highlights more of the uncertainty reported than the actual findings that were meaningful. The findings indicate the RFS did have an effect that was accelerated by other drivers in the early years, and moderated by different drivers in the latter years. This section could be restructured some to highlight these findings 1e. Yes, most notably the last one that suggests biofuels other than corn bioethanol should receive more attention in future reports. Why not in this report?
de Gorter	1a. Very clear and very accurate overall. I have very specific suggestions for improvements on the presentation of data, especially Figures 6.2 and 6.10 where ex post mandated volumes should be compared to actual consumption. Figures and Tables like in Report with Radich are highly recommend so the analysis becomes sharper and in better focus to understand exactly what is going on. Hence, Box 6.2 is inaccurate. The EPA Report lists 7 reasons why most of the ethanol consumed in the U.S. was not due to the RFS Program. I agree with 5 of those reasons (free market production would not have occurred on its own with high gasoline prices in 2008 – too short a period) and D6 RIN prices do not reflect the ethanol price premium above the tax credit (or above the price of consumers being indifferent between consuming ethanol or gasoline on a miles per gallon basis when the tax credit was zero). I explain in detail why (ironically, the EPA Report gives ample details on the structure of the ethanol market that helps me come to that conclusion). The Report concludes the RFS Program caused up to 2 billion gallons of extra ethanol in 2016. I give detailed reason in my comments that is highly unlikely. Therefore, I conclude the RFS attribution for ethanol is lower than the Report concludes, but with two important qualifiers: the move to 100% BOB was not influenced by the RFS2 volumes (I cannot prove it did not), and the RFS2 provided some degree of investment certainty that may have influenced ethanol production (very difficult to appropriate). Newes et al. (2022) argue an extra 2 billion gallons. This unpublished paper argues much more: Thome, Karen E. and Lin Lawell, C.-Y. Cynthia, Ethanol Plant Investment and Government Policy: A Dynamic Structural Econometric Model. Available at SSRN: https://ssrn.com/abstract=4207341 or http://dx.doi.org/10.2139/ssrn.4207341 Clarification: Here is the author’s response-

	EPA question: <i>Not peer reviewed, is there a peer reviewed version or do the lead authors intend to publish?</i> Thank you for your interest in our paper, and for the detailed and very helpful questions and comments! We have updated our paper to better address your very helpful questions and comments below. The latest citation for this paper is as follows: Thome, Karen E., and C.-Y. Cynthia Lin Lawell. (2023). Ethanol plant investment and government policy: A dynamic structural econometric model. Working paper, Cornell University. URL: http://clinlawell.dyson.cornell.edu/ethanol_US_dynamic_game_paper.pdf The latest version of this paper is available at: http://clinlawell.dyson.cornell.edu/ethanol_US_dynamic_game_paper.pdf We intend to publish this paper in a peer-reviewed journal, and are in the process of preparing the paper for submission. EPA question: <i>Also, very helpful paper, but any reason why the “MTBE policy” was only modeled to have an effect from 1996-2004? What about 2005 and 2006? The ban on MTBE didn’t actually occur until summer 2006. Any idea what that modification may have on the results?</i> Thank you for your kind words, your very helpful questions and comments! Our MTBE Ban variable is a state-level policy variable, which we define as a dummy variable for whether the state banned MTBE at any point in a given year. The first states in our sample banned MTBE as early as 2000. All the Midwestern states in our sample implemented MTBE bans by 2005, before the nationwide ban took effect in 2006. Thus, in our data set the MTBE Ban variable is equal to 1 for all states in our sample from the year 2005 onwards. Nevertheless, we have observations for our MTBE Ban variable for all years in our time period of analysis, including for years from 2005 onwards; and we use observations from for our MTBE Ban variable for all years in our time period of analysis, including for years from 2005 onwards to identify the effect of the MTBE Ban. For example, the significant positive effect of the MTBE Ban on the payoff from investing in an ethanol plant across multiple specifications in Table 1 of our paper was estimated using observations for our MTBE Ban variable for all years in our time period of analysis, including for years from 2005 onwards, when the MTBE Ban dummy variable is equal to 1 for all states. While we use data on the MTBE Ban for all years (including the years from 2005 onwards) to estimate the effect of the MTBE Ban on the payoff from investing in an ethanol plant, we are only able to run our counterfactual policy simulations for the No MTBE Ban scenario, which analyzes the counterfactual scenario in which the MTBE Ban was removed in all states in all years, for the
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pre-RFS period only (1996-2004). The reason is as follows. All the Midwestern states in our sample implemented MTBE bans by 2005, when the Renewable Fuel Standard was first implemented. Thus, the MTBE ban was implemented in all states in our sample in each of the two years that the federal RFS1 was in place in all states (2005 and 2006); and similarly the MTBE ban was implemented in all states in our sample in each of the years in our data set that the federal RFS2 was in place in all states (2007 onwards). As a consequence, in our data whenever we observe one of the RFS standards in place in any county in any year, we also observe the MTBE Ban in place in that county and year. In contrast, we never have any county-year observations in which one of the RFS standards is in place, but the MTBE ban is not. This means that it would therefore be impossible to identify a counterfactual state of the world in which one of the RFS standards is in place, but the MTBE ban is not, since we never observe this counterfactual state of the world in the data. We therefore conducted the counterfactual simulations for the No MTBE Ban scenario for the pre-RFS period only (1996-2004), and compare their results with those from similarly running the Base, No Policy, and No Tax Credit scenarios through 2004 instead of through 2008.

EPA question: *They also omit VEETC, any idea why?*

We do not include other federal-level policy variables such as the tax credit or the small producer subsidy in the analysis because they do not vary enough in the time period to identify the effects. We hope in future work to quantify the stringency and extent of various state tax credit policies and combine the various state tax credit policies with the federal ethanol tax credit in order to further examine the effects of government policies on ethanol plant investment decisions.

We analyze the ethanol production subsidy in another paper of ours: Yi, Lin Lawell, and Thome (2023). In Yi, Lin Lawell, and Thome (2023), we empirically analyze how government subsidies and the Renewable Fuel Standard affect ethanol production, investment, entry, and exit by estimating a structural econometric model of a dynamic game. We use the estimated parameters to evaluate three different types of subsidy -- a production subsidy, an investment subsidy, and an entry subsidy -- each with and without the Renewable Fuel Standard. While conventional wisdom and some of the previous literature favor production subsidies over investment subsidies, and while historically the federal government has used production subsidies to support ethanol, our results show that, for the ethanol industry, investment subsidies and entry subsidies are more cost-effective than production subsidies for inducing investment that otherwise would not have occurred.

The latest citation for this paper is as follows:

Yi, Fujin, C.-Y. Cynthia Lin Lawell, and Karen E. Thome. (2023). A dynamic model of subsidies: Theory and application to the ethanol industry. Working paper, Cornell University. URL:

http://clinlawell.dyson.cornell.edu/ethanol_subsidy_paper.pdf

	The latest version of this paper is available at: http://clinlawell.dyson.cornell.edu/ethanol_subsidy_paper.pdf We intend to publish this paper in a peer-reviewed journal as well, and are in the process of preparing the paper for submission as well. 1b. This chapter and all other chapters omit a very important literature. I list the papers on pages 2-3 of deGorter Summary Biofuel Theory and above in my comment on Chapter 6 and at the end of my specific comments on Chapter 4 below. These must be integrated into the analysis of this EPA Report. For Chapter 6, “<i>Have any important studies that would affect the conclusions been omitted?</i>” is not critical for the conclusions reached. The reason why I think they should be included is to improve the exposition, to understand why it happened the way it did (e.g., D6 RIN prices do not explain the ethanol price premiums, unlike for BBD) and improve the data presentation that makes the points clearer. 1c. I am concerned that studies like Babcock (2013) and Taheripour et al. (2022) do not have the capacity to really determine how the RFS would have increased ethanol consumption by up to 3 billion gallons. It is complex. We had a race to all blendstocks being BOB with 10% match blending and all the while watering down the scotch, it seems impossible to untangle this by assuming some parameters and simulating it. It seems like a whole new research agenda is required to untangle what would have happened now that we have gone through it all. But given the circumstances, maybe we should not complain too much. 1d. Yes, except for the caveats I identify above and those in my specific comments above. But regardless, I concur with the general conclusion the RFS Program attribution can be viewed to be small (even smaller than what the Report concludes). 1e. The limitations cited on page 6-39 is very good. It emphasizes the analyses may underestimate the impact of the RFS. Then on p 6-40 emphasizes how it may overestimate the effect of the RFS Program. This page summarizes my arguments in the specific comments on Chapter 6 above. Lines 1481-1487: I do not think ethanol prices ever got so high that there would be any change to revert back to non-BOB. A more important study is to test my theory the handwriting on the wall and watering down the scotch pretty much happened so fast the RFS was redundant (not to say the RFS2 prompted to race to BOB – that would be analyzed in this same study I am proposing). Lines 1488-1490: it is clear RFS1 was redundant – no need for a study here. I would emphasize that future research should include the framework in the literature outlined above that was not used by this EPA Report.
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	Specific Comments on Chapter 6 - Attribution: Corn Ethanol and Corn The goal of this chapter is to determine how much ethanol consumption is due to the RFS. The Report concludes the RFS did not have a very large impact on the level of biofuel consumption in the United States, and therefore on corn production (another key variable impacting the environment The Report is required to evaluate). The RFS had little impact because: 1. Ethanol was used to replace MTBE as an oxygenate. 2. Air emission standards (presumably refers to the RFG that required ethanol because of the <i>de facto</i> ban on MTBE) 3. MTBE was the dominant additive nationally as an oxygenate and octane enhancer because of its lower cost and compatible blending properties. Because MTBE was <i>de facto</i> banned, ethanol was also used as an octane-enhancer. By late 2007, refiners saw the writing on the wall¹⁹ and so made irreversible investments in the production of suboctane blendstock for oxygenate blending (BOB). This results in match blending where 10 percent blend of ethanol with gasoline occurs,²⁰ regardless of the relative price of ethanol to gasoline.²¹ 4. The VEETC increased the ethanol price by almost the full amount.²² Clarification: by almost the full amount of the credit. 5. High gasoline prices resulted in significant free market generated ethanol production, especially from mid-2007 to mid-2008 (high gasoline prices in 2010-11 probably too late to impact decisions to invest) 6. State programs would have generated a significant amount of ethanol production, regardless of all the above and of the RFS (many states had both production and consumption subsidies for ethanol, and the California's LCFS would have generated a significant amount of ethanol regardless of the RFS)
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¹⁹ "Saw the writing on the wall" refers to the stagnation of gasoline consumption in 2007 (and dropping all the way through 2012) after the EIA 2006 forecast had gasoline consumption to continue straight up far into the future. With the Great Recession, and with volume requirements continuously increasing in the RFS2, refiners decided to make irreversible investments in producing and selling BOB so there would be 10% ethanol in all ethanol sold at the blend wall (except for small amounts of E0).

²⁰ Apparently one can blend up to 10.4 percent ethanol with BOB.

²¹ Some (Irwin; Babcock) write refiners will reverse to a substitute for ethanol if ethanol is 20 percent higher than gasoline, but I suspect this is highly uncertain (plus ethanol prices rarely above gasoline prices by 20% for extended periods of time).

²² Increased ethanol consumption lowered gasoline prices to the extent that U.S. ethanol production averaged 0.68 percent of total implied average world gasoline consumption from 2007-08 to 2014-15 (on a miles-equivalent basis). Meanwhile, the U.S. became an active exporter of gasoline and diesel in 2006 so the impact of ethanol on the world crude oil and gasoline markets must be minimal at best.

7. D6 RIN prices were low and so means the RFS was not binding until mid-January 2013

Furthermore, ethanol consumption was higher than mandated volumes in RFS1 and through 2011 in RFS2 (see Figure 2 of [Report with Radich](#)).

The EPA Report omits the importance of farm subsidies generating ethanol production before and during the periods of the RFS1 and RFS2 (de Gorter and Just 2009).

The Report does note:

1. The RFS volumes provided a backup to any investments in ethanol production should gasoline prices decline, and the tax credit not be renewed. It did so by guaranteeing a minimum demand by establishing market certainty.
2. The RFS could have generated much of the observed ethanol consumption if the 7 factors listed above did not exist.

I add one more important question: would refiners make irreversible investments in BOB without the RFS2? I thought “saw the writing on the wall” referred to lower forecast gasoline consumption in the face of ever-increasing mandated ethanol volumes. But perhaps the tax credit, even with lower gasoline prices, would have generated the same expected ethanol volumes and would have made the industry race to have all blendstock being BOB and downstream firms would have done 10% ethanol match blending, with RFS2 not even existing. I do not know.

Before giving my assessment of factors 1-7 above, let me first give some specific comments on Chapter 6.

Lines 38-40:

Studies that include other factors in their examination of the RFS Program tend to estimate smaller effects from the Program, while studies that only include the RFS Program estimate larger effects.

This seems impossible. If you model the RFS and the tax credit simultaneously, and the mandate is binding, then the tax credit is subsidizing fuel (gasoline plus ethanol) consumption (see references below). The same applies to corn production subsidies and biofuel production subsidies (see Drabik 2011 and Ch. 12 in [The Book](#)). No studies cited in The Report use equation [2] of [deGorter Summary Biofuel Theory](#) to determine which is binding: the mandate or the tax credit. These studies fail to recognize that if the tax credit is binding, then the mandate is redundant (but not vice-versa).

Lines 41-42:

the overproduction of ethanol domestically compared to the RFS standards

The calculated ethanol mandate premium was quite significant in 2012 as the tax credit expired so would the RFS not have a more significant impact? The mandate premium was significant in 2008-09 too. Not sure your method of what the ethanol price, and hence corn price would have been in 2008-09 is correct. If one uses the method in

[deGorter Summary Biofuel Theory](#), the ethanol price is higher, and using equation [1] would suggest higher amounts. Ditto for 2012.

Lines 49-50:

From 2013 to 2019 annual estimates of the impact of the RFS Program vary from zero to up to 2.1 billion gallons in 2016

As I will explain in detail below, BOB for all motor gasoline was achieved around 2010 – see Figure 2 in [Report with Radich](#) -- where the E10 blend ratio is almost 10%.²³ There were no price competition between E0, E5 and E10 – it was BOB and E15/E85 with a rapidly declining sales of E0 on the side). How much of the blendstock was BOB with match blending versus splash blending is unknown but E10 sales were close o 10% in 2010. The blend wall was hit in 2012 – see Figure 2 in [Report with Radich](#) – the total ethanol blend ratio in 2012 is for the first time below the mandated ratio. So how can the RFS increase ethanol consumption from 2013 to 2019? The blend wall is E10. BOB is E10. So, you get E10 regardless of the mandated volumes in 2013 onwards because BOBing was a done deal by then and no going back. So, the only way the RFS2 could increase ethanol consumption is any increase in ethanol consumed by E15 and flex cars in various levels of ethanol E10 all the way to E74. That is the impact of the RFS – it must be small and not 2.1 billion gallons Taheripour and Tyner predict, or 3.7 billion gallons predicted by Babcock (2013). I am not sure these estimates are realistic.

The only other way the RFS2 could be increasing ethanol consumption is by forcing a reduction in E0 but that must be low to begin with by 2013. Or by having non-ethanol fill the implied ethanol mandate, freeing domestic ethanol production for export but that resulting change in corn production must be small. The cost of overcoming the blend wall with high D6 RIN prices is a cost to ethanol producers (compared to no blend wall) so the ethanol price is lower than otherwise, and so by equation [1], corn prices are lower than otherwise too. Some studies argue high D6 RIN prices means a boon to the ethanol industry unlike prior to January 2013. For the most part, that is not true.

It would be great if “The Ethanol RIN Gap” could be presented and then show it is overcome (as shown in the [Report with Radich](#) and the figure on next page.

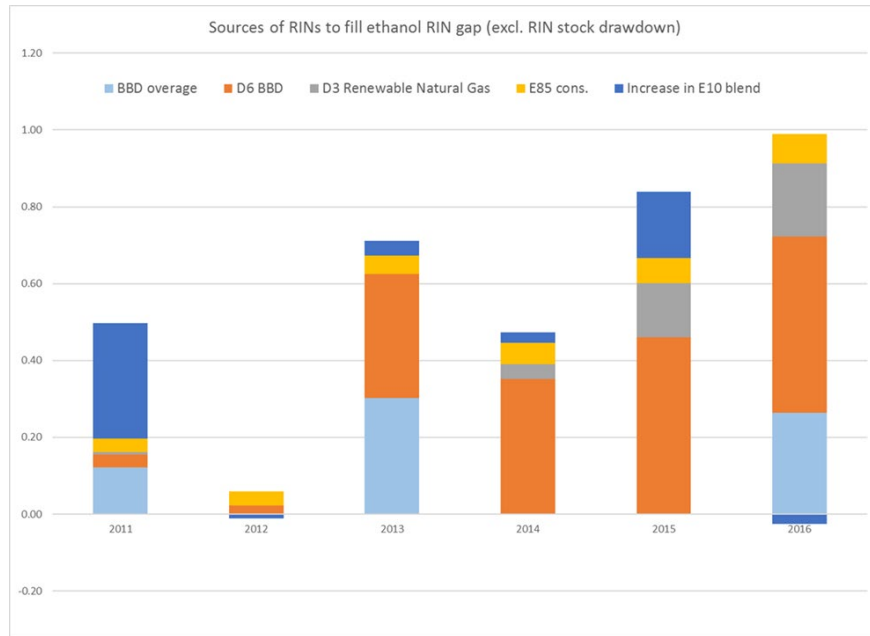
Figure 6.1: I prefer a graph showing *ex ante* mandated volumes, *ex post* mandated volumes (because the RFS is implemented as a blend ratio based on forecast gasoline and diesel consumption) so *ex ante* and *ex post* mandated volumes will never be the same; it is the *ex post* mandated volume that matters and compare to actual ethanol consumption. See Tables 3-4 in [Report with Radich](#). Such a graph would really inform. Comparing mandated volumes and *ex post* consumption is misleading and really does not show anything important as in your Figure 6.1.

Need a separate figure for ethanol production and have changes in ethanol inventories plus net exports on same graph.

²³ This is consistent with the EPA Report footnote 13 on page IS-13 “Between 2005 and 2010 refineries invested in switching to “match blending,””

Figure 6.2: I prefer three percentages: consumed as E10 (versus E15 and flex cars in E85), mandated percentages (implied after 2010 because of joint compliance base so need to re-figure $E^*/(E^*+G^*)$; and total ethanol blend ratio.

See figures in the [Report with Radich](#)



Lines 201-203: I do not understand. Ethanol prices were higher than gasoline all through that time period, and the free market price of ethanol as per using equation [2] was extremely high.

Lines 204-208:

...these changes provided revenue to the ethanol producers...

Switching from an excise tax exemption to a tax credit has the same effect on ethanol prices (and hence on “revenues to ethanol producers”) in a closed economy. Both are ethanol consumption subsidies. It can make a difference with international trade (see Splash and Dash paper).

Figure 6.4a:

I prefer showing Figures 3 and 7 as in [deGorter Summary Biofuel Theory](#)

Figure 6.4b:

I prefer showing Figure 6 as in [deGorter Summary Biofuel Theory](#) because it shows ethanol mandate premium and when blending constraints cause ethanol prices to be lower than their per gallon miles equivalent with gasoline.

Figure 6.4c:

From page 10 of [deGorter Summary Biofuel Theory](#), plot data generated from equation [3] and [5]. In this way, you can get an understanding of how the ethanol price premium (with *versus* without the tax credit) relative to gasoline where consumers would be indifferent between gasoline and ethanol to achieve miles per gallon.

The dashed line in Figure 6.4a fails to adequately measure if ethanol is price competitive to gasoline or not (at any price of crude oil or corn price). Box 1 here does.

Lines 210-211:

As discussed earlier, an ethanol tax credit of one form or another makes gasoline with ethanol more competitive than it would be without the credit (Figure 6.4c).

If the mandate is binding in that there is a mandate premium above the tax credit (equation [3]), then the tax credit is subsidizing fuel (ethanol plus gasoline consumption) – see references below that explains. Even if this is due to replacing MTBE (a *de facto* mandate as MTBE was banned) or if BOBing (seeing the writing on the wall is a *de facto* mandate, especially there was no way the industry could price compete between E0, E5 and E10 so watering down the scotch was possible and at the blend wall – the latter acts like a mandate or is infra-marginal to the binding effects of the mandate, namely ethanol in E15 and E85).

Even so, it was not until after 2000 when oil prices began to increase that ethanol was cost-competitive for several years in a row, and was even cost-competitive without the credit in 2008 and after 2010 (Figure 6.4c).

Ethanol was not cost competitive! In 2003, the ethanol price was \$1.35 (P_E) and the ethanol price P_E^* to be indifferent on a miles per gallon basis between ethanol and gasoline was \$1.08. It was not necessarily cost competitive after 2010. You need to present the data as I do here to show it. Figure 6.4c cannot show it.

Figure 6.6: ethanol production capacity today is 17 billion gallons according to the RFA (and production substantially below that).

First word on line 332 should be “consumption”, not “production”.

Lines 335-337:

The RFS2 went into effect in 2009, but only for total renewable fuel; thus, it was not until 2010 that the RFS2 with the four volumetric standards went into effect. By then the percentage of ethanol in gasoline was already at 9.3%, near the “E10 blend wall.”

Figure 2 in the [Report with Radich](#) shows the total ethanol blend ratio was 9.3% but the E10 ratio was at 10% in 2010 so the BOB was complete? For sure by 2012.

Figure 2 in the [Report with Radich](#) shows all three measures were above 10% in 2016 yet Taheripur et al. (2022) argue RFS2 had an effect in 2016? Blend wall so no effect of RFS2 and BOB was surely 100% widespread in the industry by then.

Box 1: From p. 10 of [deGorter Summary Biofuel Theory](#)

To do so, if the observed ethanol price is above the predicted price floor for ethanol P_E^* from equation [2], then the observed ethanol mandate price premium (above the tax credit) is:

$$P_E - P_E^* \quad [3]$$

Note that when in State of Nature 2, the tax credit still has its (initial) approximate \$2/bu price increasing effect.

The free-market ethanol price (the lowest ethanol prices can go with no tax credit) is:

$$P_E^{LG} = \lambda P_G - (1 - \lambda)t \quad [4]$$

and the observed total ethanol price premium above free markets would be:

$$P_E - P_E^{LG} \quad [5]$$

Lines 409-413

The Program is binding if consumption would not have occurred at that level without the mandate,

Consumption of ethanol reached the blend wall (E10 with at least 10% ethanol) and 100% of the blendstock being BOB is the same thing as saying the RFS2 is binding, even if watering down the scotch and seeing the handwriting on the wall caused 100% of the blendstock to be BOB with 10% ethanol matched blending. In other words, hitting the blend wall with 100% BOB with 10% ethanol is the same thing as the RFS2 is binding. The only way RFS2 can increase ethanol consumption is through increasing ethanol in E15 and E85 sales, and with high D6 RIN prices, making E0 more expensive so to reduce sales, thereby increasing E10 sales.

and the Program is not binding if consumption would have occurred at those levels under market conditions regardless of the mandate.

After 2012, no mandate means same E10 and BOB with match binding for 100% of all motor gasoline sales. Ethanol prices were never high enough for BOB to revert to another blendstock or that any BOB would be below 10% ethanol blending.

RFS2 is binding because of higher E15 and E85 sales. So, conclude it is binding overall?

When consumption is higher than the mandate, that is evidence that the RFS Program was not binding in that year (Taheripour et al., 2022; Tyner et al., 2010).

This study uses the wrong data as does Figure 6.10. Please read pages 11-18 closely in the [Report with Radich](#). Figure 6.10 is deceptive because it looks like the mandate was not fulfilled in 2020. It was because it is a blend mandate; not a volume mandate. Box

6.2 is the source of confusion for Figure 6.10. Generating data as in the [Report with Radich](#) can explain when the RFS2 is binding or not. But the infra-marginal level of ethanol (up to the blend wall; i.e., up to where 100% of the blendstock is BOB with 10% ethanol match blending) is due to BOB and the handwriting on the wall and watering down the scotch (both may or not be due to the RFS2).

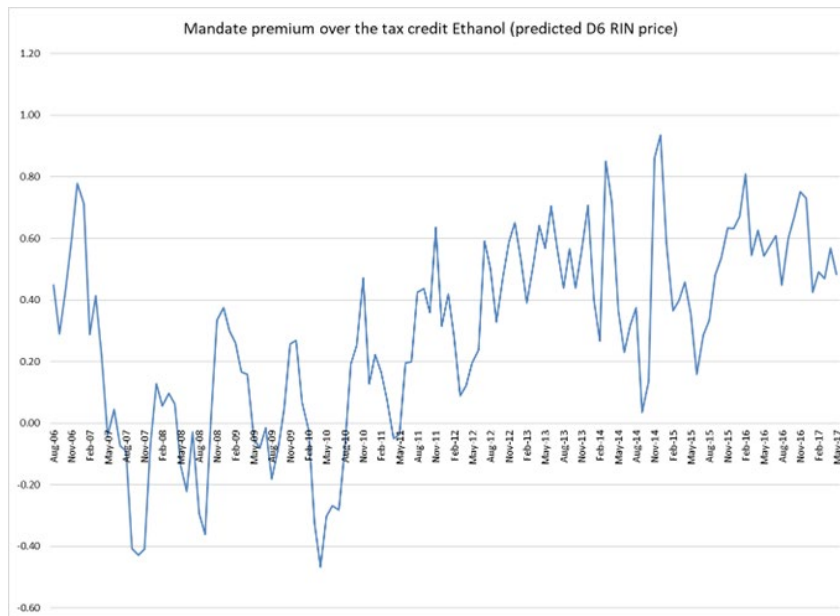
If not, then the only way RFS2 can increase ethanol consumption after 2012 is through.....

Lines 440-441:

When RIN prices are near zero, the RFS is said to be non-binding.

I disagree with this. D6 RIN prices were close to zero until mid-January 2013. But the mandate premium (and hence predicted D6 RIN prices) are often very high as shown in the accompanying figure. The negative values are due to blending constraints at the retail fuel pump for ethanol and excess profits in ethanol production (see [deGorter Summary Biofuel Theory](#) for details). Obviously, the prediction of D6 RIN prices is a way off. Does that mean the mandate premium as calculated using the methodology in in [deGorter Summary Biofuel Theory](#) is wrong? It would predict high D6 RIN prices in some periods, especially in 2012 with no tax credit. No. The theory is correct.

Figure 1



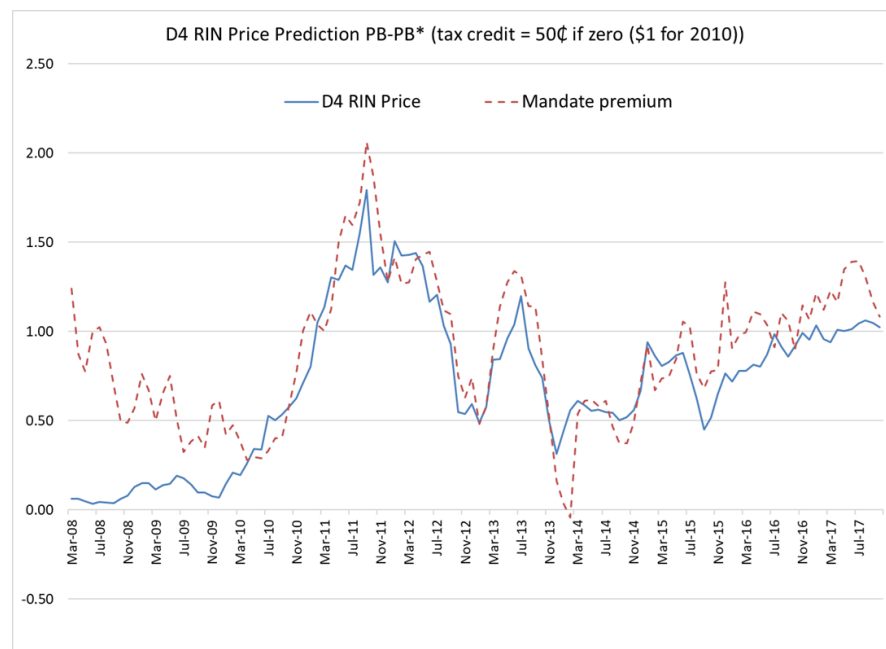
Before explaining, let's look at the case for BBD. The accompanying Figure 2 shows a perfect prediction.²⁴ Here is the same theory applied to BBD D4 RIN prices. Theory and data are perfectly aligned. So, what is going on!?

Let me explain.

²⁴ See Drabik, de Gorter and Timilsina (2014).

For BBD, if you want to opt out, it is easily done and the price in the market is close to the mandate premium. So, the market works. Not the case for ethanol. As the EPA Report correctly indicates, the BBD-diesel complex “is not affected by the same engineering and logistical constraints as ethanol” (page 7-3) nor “characterized by dramatic changes as the ethanol industry in production and consumption” (page 7-4).²⁵ Up to mid-January 2013, D6 RIN prices were low because of the unique market and technological aspects of the ethanol industry, even if calculated mandate premiums were high. First one had to replace MTBE and then seeing the writing on the wall is a *de facto* mandate, especially because there was no way the industry could price compete between E0, E5 and E10 (it was just not practical, unlike for BBD) so watering down the scotch was possible. And then on to the 100% BOB with 10% ethanol as match blending. There was not an in between situation, unlike for BBD.

Figure 2



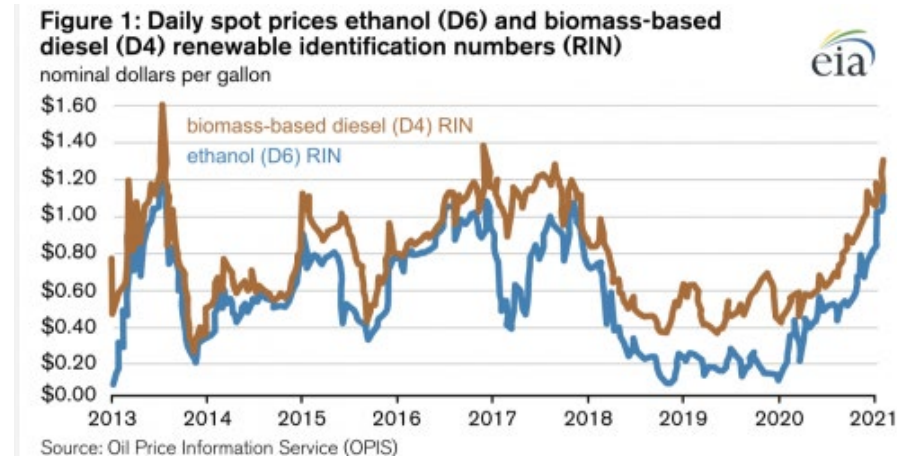
Post mid-January 2013, D6 RIN prices reflect the subsidy to consumers to overcome blend wall – has little impact in raising the ethanol price as costs are high to overcome blend wall – so a high D6 RIN price means the gap between P_E and P_E^{LG} from Jan 2012 through Jan 2013 does not necessarily increase after Jan 2013!! but does it equal D4 RIN prices? Why not? Why do D4 RIN prices represent this same “blend wall” extra cost?

Box 6.2 is wrong. It is a blend mandate; not a volume mandate. Think of 2020 and COVID. The demand curve shifted in but D6 RIN prices went up in 2020 (not to zero as Box 6.2 would predict) because it is a blend mandate.

²⁵ In discussing the adoption of BOB, the EPA Report notes p. IS-13 “But, they rely on changes to refinery operations and the downstream distribution and blending network. As a result of these changes, it would difficult and costly to reverse back to the production of finished gasoline as opposed to BOBs.”

Therefore, I also disagree with the statement on lines 493-494:

Observations of D6 RIN prices suggest that the total renewable fuel standard of the RFS Program may have been binding in 2008/2009 and after 2013, and thus had some effect increasing corn ethanol



D6 RIN prices in 2008/09 were nowhere close to the mandate premium (which sometimes went negative with blending constraints at the retail level and excess profits in ethanol production). Data in Figure 2 of [Report with Radich](#) shows the blend wall was reached in 2012 (and the mandate was binding) but RIN prices were low because of watering down the scotch in 2012 as maximum BOB with 10% ethanol match blending apparently achieved.

Lines 515-518 argues RFS2 impacted industry's expectations to be binding and so may have not only impacted investment in plants but go to 100% BOB with full 10% ethanol for octane and oxygenate so the latter acted as a mandate, even if the RFS2 was not working directly on industry's decisions year to year. Watering down the scotch could not be overcome and 100% BOB with 10% ethanol is the blend wall so now no room for RFS2 to impact ethanol market except to increase ethanol through E15 and E85 sales. SREs lowered D6 RIN prices but after January 2013 and had no impact on ethanol sales – water down the scotch! Plus, must use 100% BOB.

Lines 551-560:

Remember ethanol incentives, whether replacing MTBE or using 10% ethanol in BOB for all sales, or tax credit or whatever, this caused the link between corn and ethanol prices in Period 1 of [deGorter Summary Biofuel Theory](#), a link that did not exist previously.

Lines 561-583:

These studies do not provide insights as to how you can tell if the RFS2 mattered or not. For example, Babcock (2013) argues the RFS would have been needed at lower crude oil prices, but the blend wall was reached by 2012 and we had low crude oil prices after 2012 (\$26 in 2015 and again in 2020) and there was not a twitch in the petroleum industry: BOB at 100% of blendstock with 10% ethanol match blending and the blend

wall. Who needs the RFS2? Once you replace MTBE and use 10% ethanol for octane, there is no going back. There is no such thing as 50% BOB and 50% not BOB and compete in price of E0 with E2 with E6 with E10. Watering down the scotch was the game of the day and no going back.

How could any economic study determine a fine tuning of let's say 80% BOB and 20% not. These models have no insights to determine that. You just impose it, but no justification to do so.

Page 6-27

Look at Figure 2 in [Report with Radich](#): take 2016, I do not understand how the RFS2 was required to ensure the blend wall was met and that refiners would not backslide and begin offering less BOB and another blendstock to lower ethanol blend rates below 10%. D6 RIN prices were high because of the need to overcome the blend wall which is determined by the marginal cost of increasing BBD and other types of biofuels to spill into the residual D6 market. The RFS2 is required to have E15 and E85 sales and high D6 prices, but ethanol in E15 and E85 is so small. That would be the effect of the RFS2 on ethanol consumption in those years. Not increasing ethanol consumption by a billion or 3 billion gallons as Babcock argues. I do not understand. Think of the 10% BOB as a blend mandate. But this occurred due to the RFS2 or despite it. But once accomplished, the RFS2 only accounts for ethanol in E15 and E85 (encouraged only by high D6 RIN prices caused by the blend wall, itself required to be breached only because of the RFS2).

All the papers by Babcock cited in the EPA Report has high and low gasoline prices. If low gasoline prices, Babcock's papers argue the RFS2 increases ethanol consumption up to 3 billion gallons per year. But in mid-2014, crude oil prices skidded all the way down to \$26 per barrel (and again in 2020). But ethanol consumption as a percent of gasoline or of the joint compliance base did not decline at all. So, the speculation presented in those papers were confirmed to be inaccurate by empirical events right after the papers were published. In now way would refiners reverse their investments and the logistics all the way down the supply chain would be a nightmare.

So Taheripour et al. (2022) arguing the RFS2 has bigger effects in 2016 than in earlier years seems like a flawed analysis. Think of blendstock being 100% BOB with 10% ethanol match blending is being a mandate (just not the RFS2 mandate). As I have explained earlier, there is no relationship between D6 RIN prices and the degree to which the RFS2 is binding (up to mid-January 2013). After that, D6 RIN prices would still be low if the RFS2 simply required the ethanol blend rate to be that which the industry would do anyway, given their irreversible investments (this could be up to 10.4 percent as I understand once can blend 10.4% ethanol with BOB and sell as E10). Why does the EPA not do this? All the fuss of high D6 RIN prices just for an extra couple of hundred million gallons of ethanol consumption!?

Lines 648-670

The EPA Report then criticizes the papers by Carter et al. and Lark et al. for assuming the RFS2 increased ethanol consumption by 5 bil. gallons but it could be other factors like the tax credit, replacing MTBE and the race to 100% BOB with 10% matching

ethanol which is irreversible (or has shown to be so, even with super low crude oil prices in 2015 and 2020). The problem with the EPA's criticism is that Carter et al. and Lark et al. could simply argue (a) the RFS2 induced the race to 100% BOB with 10% match blending of ethanol which is irreversible. Think of this as a mandate (which it is). So, it is equivalent to attributing 5 billion gallons to RFS2 (it induced the octane rush); or (b) even if the RFS2 did not induce BOB as 100% of the blendstocks, think of the 100% BOB with 10% match blending of ethanol as a blend mandate (which it is because it is irreversible). So, they will argue their 5-billion-gallon increase in ethanol consumption holds.

Reading [deGorter Summary Biofuel Theory](#): Period 1 was the industry's realization of what corn prices should be with the zero-profit condition in corn-ethanol production (equation [1]). Corn prices rose 100%. There was not supply/demand shock whatsoever. And if there was one, it could not have any effect anyway because the zero-profit condition was going to occur regardless. Like I write: it would have been like shuffling the chairs on the deck of the Titanic. Ditto for the potential effects of a change in corn inventory demand. Besides, corn inventory declined 34% in Period 1.

In Period 2, corn prices were locked onto gasoline and crude oil prices through ethanol prices. No supply/demand shock or change in inventories could affect the corn price. Ditto Period 4. Please read [deGorter Summary Biofuel Theory](#) to understand.

How to attribute an increase in ethanol consumption to the RFS2?

1. The RFS volumes provided a backup to any investments in ethanol production should gasoline prices decline, and the tax credit not be renewed. It did so by guaranteeing a minimum demand by establishing market certainty.
2. The industry saw the "handwriting on the wall" when they looked at specified ethanol volumes in the December 2007 RFS2 after gasoline consumption declined from 2006 levels (and the 2006 forecast was a straight line up) so refiners decided to go 100% BOB with downstream going for 10% ethanol match blending. Logistics were such that "watering down the scotch" was the order of the day, there was no price competition between E0, E4 or E10, and as a result, D6 RIN prices did not match the ethanol mandate price premium (unlike for BBD)

How important is (1)? I do not know how one could calculate it, but I do know nobody in the literature has. As for (2), could one make the case that the industry saw the handwriting on the wall because of increased ethanol volumes due to replacing MTBE as an oxygenate and octane enhancer, and the tax credit with high gasoline prices? Maybe. I interpreted the implication of the phrase "see the handwriting on the wall" only in terms of future RFS2 volumes, but maybe I was wrong. So, I cannot conclude the RFS2 was needed. But I do know that no study in the literature has analyzed this question in a correct way. Studies just simply eyeball things and pick a number.

All ethanol above the blend wall (=10.4% E10?) can be attributed to RFS2 – that is ethanol in E15 and E85 which required high D6 RIN prices which occur because the RFS requires the blend wall to be breached (otherwise, D6 RIN prices would go down to very low levels as before).

	The issue of “attribution” puzzles me. The EPA Report states the RFS2 could have generated the outcome without the 7 factors listed earlier. But that is not enough to attribute the outcome to the RFS2. So maybe one should separate out “could have” versus “what really happened”. What really happened? See (1) to (7) on the first page of my comments on Chapter 6. I disagree with (5) and (7). High gasoline prices from mid-2007 to mid-2008 was too short a period to generate ethanol production capacity by itself. Furthermore, D6 RIN prices did not measure the ethanol price premium above the tax credit because of the race for all blendstock to be BOB with 10% ethanol match blending and because of watering down the scotch. After January 2013, the D6 RIN price reflected the cost of overcoming the blend wall, best represented by the marginal cost of increasing BBD consumption. But factors (1)-(4) and (6) were significant contributors to what really happened. Was that enough to generate the observed outcome without the RFS2? Theory indicates the RFS2 could have made it happen but what really happened (without the RFS2) may have been enough to generate the outcome. I am unable to prove otherwise. My conclusions about attribution of the RFS2 for observed BBD consumption is quite different: the RFS2 in theory could have generated the outcome but also the RFS2 is responsible for what really happened (see my comments on Chapter 7 below). Tax credit subsidizes fuel consumption with a binding mandate • de Gorter, Harry, and David R. Just. (2008). “The Forgotten Flaw in Biofuels Policy: How Tax Credits in the Presence of Mandates Subsidize Oil Consumption.” <i>Resources for the Future Policy Commentary</i>, June 9, 2008. • de Gorter, Harry, and David R. Just. (2009). “The Economics of a Blend Mandate for Biofuels.” <i>American Journal of Agricultural Economics</i> Vol. 91: No. 3, August, pages 738-750. • de Gorter, Harry. (2010). “The one tragic mistake of biofuel policy that everyone can agree on: how tax exemptions with mandates subsidize oil consumption.” Policy Update in <i>Biofuels</i> 1(4), 531–533. Splash and Dash • de Gorter, Harry, Dusan Drabik and David R. Just. (2011). “The Economics of a Blender’s Tax Credit <i>versus</i> a Tax Exemption: The Case of U.S. ‘Splash & Dash’ Biodiesel Exports to the European Union.” <i>Applied Economic Perspectives and Policy</i>. Winter. 33 (4): 510-527. doi: 10.1093/aep/ppr024 Blend Wall • de Gorter, H., and Drabik, D. (2015). The Distinct Economic Effects of the Ethanol Blend Wall, RIN Prices and Ethanol Price Premium due to the RFS. Charles H. Dyson School of Applied Economics and Management, Cornell University. November 2015.
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	• Korting, Christina and David R. Just. (2016). "Demystifying RINs: A partial equilibrium model of U.S. biofuel markets." <i>Energy Economics</i> Volume 64, May 2017, Pages 353–362. • Korting, Christina, Harry de Gorter and David R. Just. (2018). "Who Will Pay for Increasing Biofuel Mandates? Incidence of the Renewable Fuel Standard Given a Binding Blend Wall." <i>Amer. J. of Agric. Econ.</i> Volume 101, Issue 2, March 2019, Pages 492–506, https://doi.org/10.1093/ajae/aay047 Effects of Farm Subsidies on Ethanol Production • de Gorter, Harry, and David R. Just. (2009). "The Welfare Economics of a Biofuel Tax Credit and the Interaction Effects with Price Contingent Farm Subsidies." <i>American Journal of Agricultural Economics</i>. Vol. 91. No. 2, May, pages 477-488. • Drabik, Dusan. (2011). "The Theory of Biofuel Policy and Food Grain Prices". Working Paper 2011-20. Charles H. Dyson School of Applied Economics and Management, Cornell University. December. Section 6.4 Evidence of the Impact to Date of the RFS Program on Corn Production and Cropland It would be great if the final column in Table 6.9 was like Tables 6.7 and 6.8 in that the change in cropland was measure din the same units so we could compare studies (like change in acreage per dollar bushel increase in corn price). Maybe also have the change in billion gallons of ethanol per percentage change in corn price. I am not familiar with any of these studies except that of Lark et al. and I believe that study is not credible as I explained earlier. And besides, one cannot have a 10% increase in corn production and a 20% increase in soybean prices. The soybean/corn price ration in \$/bu is always 2.5 (using Chicago prices) and the reason for that is marginal profits between crops are always equal. Once one crop becomes more profitable, farms switch into that crop causing the price of the other crop to increase later. The relative soybean to corn price is a flat line over time. The EPA Report uses Bento and Klotz study elsewhere: they have an 8% increase in corn prices due to ethanol and a 1% increase in soybean prices. This is impossible.
Engel	1a. The clarity and technical accuracy of material presented in the chapter were reasonable and align with my broader understanding of the agricultural and biofuels industries. The key findings are reasonable based on the information presented in the chapter. The key findings provide a useful and understandable overview of the chapter. Additional detailed review of references and their supporting materials would be required to more fully assess the technical accuracy of the material in the chapter. There was nothing identified that calls into question the technical accuracy. 1b. The most important data and literature cited seem to be interpreted correctly. I would note I am not as familiar with the overall base of literature in this area as I am for other chapters. Key studies that I am familiar with are cited and used in supporting the materials within the chapter. Key authors that work extensively in

	this area are cited. I have no additional recommendations as to other literature or studies that should be considered in this chapter. 1c. I am not an expert on these types of models so it is difficult to comment specifically as to whether these are the best/most appropriate models, as to the accuracy of the analyses and accuracy of inferences. Based on my limited knowledge of such models and analyses and the information provided in the chapter, the models are appropriate and accuracy of analyses and inferences seem to be well supported. The ranges used in reporting results and inferences seem reasonable based on the information presented in the chapter and throughout the report. 1d. The conclusions and recommendations provide in this section follow and are supported by information presented within the chapter. The most important conclusions are captured. The recommendations largely follow from conclusions and materials presented in the chapter. Some of the conclusions are quite broad and far reaching and unclear that they relate very directly to the mandate for the overall assessment report. The recommendation suggesting exploration of the RFS Program on spatially explicit land cover and management changes is quite relevant given the information presented overall in the document given the spatial nature of some of the changes that have occurred and potential for future spatially differentiated responses. The agency should be cautioned in how much spatially explicit analyses are useful given the time and cost of such analyses and limited ability to explicitly connect the RFS program to land use changes and corresponding impacts. 1e. The uncertainties and limitations associated with the conclusions provide a good overview of those that are most important. Addition of an acknowledgement of the issues associated with the spatial variability of RFS impacts should be considered as this arises in this chapter as well as in some others. The ability to assess the spatial impacts of RFS is also almost certain to continue to be a challenge into the future.
Hill	1a. The material presented in this chapter is neither clear nor technically accurate. While I appreciate what is being attempted here, the modeling choices are incorrect and conflict with the methods recommended in a recent National Academies of Sciences, Engineering, and Medicine (NASEM) report. Furthermore, the conclusions drawn in this chapter are unsupported by the data. Lines 74–75: Attribution can, in fact, be assigned in future anticipated/alternative scenarios. This is commonly done in RIAs. 1b. No. This chapter should be heavily revised and rewritten to align with the conclusions of the NASEM’s recent report “Current Methods for Life Cycle Analyses of Low-Carbon Transportation Fuels in the United States.” It describes the appropriate use of a consequential approach for understanding life cycle impacts of decisions made to increase fuel production, such as in the case of RFS/RFS2. The NASEM report explicitly states that the attributional approach currently employed

	in RtC3 should not be used to “understand the consequences of a proposed decision or action,” such as RFS2. See Recommendation 2-2. 1c. There are four fundamental problems with the modeling performed in this chapter. First, as described in the NASEM Report, an appropriate analysis would have been to model observations against counterfactuals that excluded RFS/RFS2 over its entirety (including anticipatory effects), not an incremental approach as is currently employed. Second, the approach taken does not account for fundamentally important factors such as reduction of risk provided by RFS/RFS2 or anticipatory effects of its passage and implementation. Many of these effects are noted in Sections 6.3.7 and 6.4.4, but they are excluded from the analysis. The exclusion of reduction of risk in the quantitative analysis of RtC3 is a particularly glaring omission. The Newes et al. (2022) paper cited in RtC3, and which shares many co-authors with it (e.g., Clark, Newes, Burkholder, and Korotney), estimates the effect of reduction of risk and concludes that “the overall effects of the RFS1 could have accelerated ethanol production growth by one year and increased annual production growth by one year and increased annual production level by up to two billion gallons...” Reduction of risk must be included in this Triennial report, and the effect of doing so, using the analysis of Newes et al., is that the current draft of RtC3 grossly underestimates environmental effects from RFS/RFS2. Third, as there were numerous reinforcing federal and state policies, the subtractive approach taken here is inappropriate. [In other words, in Fig. IS.1, the statement, “conceptually this report focuses on the question of how large is the red circle overall and relative to the blue circle,” is incorrect.] RFS/RFS2 complemented these drivers, and so reinforcing federal and state policies do not bear the full responsibility for increased ethanol production. Fourth, the analyses presented here neglect important consequential effects such as on fuel markets (rebound effects) and feed markets (reinforcement of ruminant production over other protein sources) that, if ignored, tend to underestimate the environmental impacts of biofuel production. For example, Recommendation 6-9 of the NASEM report states, “Beyond research on induced land use change and rebound effects, research should be done to identify and quantify the impacts of other indirect effects of biofuel production, including but not limited to market-mediated effects on livestock markets, land management practices, and dietary change of food type, quantity, and nutritional content.” 1d. No, the conclusions and recommendations in the “Chapter Synthesis” section are not supported for reasons described in my answers to the questions above. 1e. Yes, some are, such as in Sections 6.3.7 and 6.4.4, but they are dismissed as “difficult to capture in traditional modeling frameworks and thus are not quantified in the literature.” Nevertheless, these are exactly the sort of factors that must be included. Also, some key uncertainties and limitations are not identified, such as in the fourth point in my response to part c. The text in Lines 1383–1400 deserves special consideration as it provides the motivation for Chapter 6 and justification for the approach being taken. It is stated that “Thus, even though the estimates here may need to be revisited as additional studies are published, this approach, while not without limitations, provides a
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	credible estimate of the scale of land change effects from RFS-attributable ethanol at the county level and nationally.” I do not share this conclusion; rather, the approach taken is unsound for the reasons I have described above, and to say that it provides a “credible estimate” is fully unwarranted.
Hoekman	1a. The previous triennial reports to Congress (RtC1 and RtC2) provided considerable information regarding corn agriculture within the U.S., production and use of corn ethanol, and environmental impacts from this use. However, these earlier reports were unable to reliably attribute the use and impacts of corn ethanol specifically to the RFS Program, as compared to other factors that also drove the usage of corn ethanol. The 3rd triennial report (RtC3) explains clearly the challenges of conducting such an attribution analysis and presents a set of approaches to accomplish this goal. It is reasonable to expect that if RFS is considered to be the dominant factor influencing decisions about corn and ethanol production, the estimated effects of the RFS Program on environmental impacts would be relatively large. However, by attributing a fraction of these impacts to other factors (such as MTBE phase-out, demand for octane, changes in tax and incentive policies, and other factors), the environmental impacts attributable to the RFS Program become much smaller. This situation is examined in Chapter 6 in a manner that is clear and technically robust. At the same time, other attribution approaches could be used, which likely would produce different results. 1b. To my knowledge, the RtC3 does cite and appropriately interpret the most recent data relevant to attribution of environmental effects to corn and corn ethanol. This includes the different data sources used to assess whether the RFS Program is (or is not) binding during different time periods. The five main sources of information used to assess the effects of the RFS Program are identified at the beginning of Section 6.3 (page 6-18), with further elaboration in the following sections of the report. These information sources are appropriate for the task, and together present a reasonably consistent picture of the changing effects of the RFS Program over the years of its existence. One place where additional data could be helpful is in estimating the importance of “logistical inertia” that is asserted to be a reason why refiners would continue to blend ethanol into gasoline even if this were more expensive and there was no RFS driver (page 6-36). While there probably are no hard data to support this understanding, interviews with knowledgeable parties could provide some insights into the strength of this “logistical inertia” argument, and what conditions would be necessary to overcome this inertia. 1c. I am not qualified to judge the selection and use of models used in Chapter 6 to assess the impacts of the RFS Program. I note, however, that the models referenced [e.g., various econometric models, partial equilibrium (PE) models, computable general equilibrium (CGE) models, the Biomass Science Model (BSM), and others] are commonly used and widely accepted by the broader modeling community. It should also be noted, however, that a consequential modeling

	approach could be used to determine the impacts of the RFS Program, as opposed to the attributional modeling approach used in the RtC3 report. 1d. For the most part, the conclusions and recommendations are supported by the material presented in Chapter 6. One possible exception may be the 3rd bullet point on page 6-61. Here, the potential effects of the RFS Program in providing market certainty is mentioned (which would result in larger RFS effects than determined by the analyses conducted in the RtC3), along with the costs and willingness of refiners to switch back to non-ethanol gasoline at certain times (which would result in smaller RFS effects than determined by the RtC3 analyses). It is mentioned that these factors are difficult to quantify (which is certainly the case) and may offset. Such offsetting is possible, though not at all certain from the available materials presented here. Although the appropriate information is provided in the Conclusions section, it is not explicitly stated that based on the combined set of analyses conducted, it appears that the overall effects of the RFS Program on corn and corn ethanol are quite small. It would be useful to provide quantitative estimates of total, cumulative ethanol values attributable to the RFS Program. For example, something like the following (but with realistic calculated values rather than the illustrative values stated here) could be added: “Over the 4-year period of 2006-2009, the RFS1 Program was estimated to increase ethanol production by about 0.5 bg (out of a total of 30 bg). Over the 11-year period of 2010-2011, the RFS2 Program was estimated to increase ethanol production by about 12 bg (out of a total of 150 bg). 1e. Section 6.4.4 (pages 6-57 to 6-59) provides a good summary of the limitations of the assessments summarized in Chapter 6. Uncertainties in the conclusions resulting from these limitations are identified and described qualitatively. More quantitative determination of uncertainties would be desirable, but is probably not possible, given the nature of the limitations. General Comments In Figure 6.13 (page 6-25), what is meant by the open symbols as compared to the closed symbols? Also, what does it mean that some datapoints are at negative values of ethanol production? In Table 6-10 (page 6-54), notes are included to indicate what is meant by the symbols “ ” and “→”. It would be simpler (and clearer) to use words rather than these symbols in the table. In reviewing Chapter 6, a few typographical and other minor errors were detected. These are identified below: • Page 6-32, line 783: change “math blending” to “match blending” • Page 6-32, line 795: change “Figure 6.16(b)” to “Figure 6.17(b)”
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	• Page 6-36, line 857: “In the “No-RFS case” the use of ethanol, biodiesel, and renewable diesel fuel in each PADD ...” • Page 6-37, line 872: delete the word “over” • Page 6-39, line 919: delete the word “a” • Page 6-44, line 1084: “... in the absence of the RFS Program from <u>2004</u> to 2011.” Clarification: The draft version of RtC3 that I reviewed said “... 004 to 2011.” My comment was to correct this typo by changing “004” to “2004.” • Page 6-63, line 1517: replace “in” with “is”
Kaffka	1a. Chapter 6 is one of the most important chapters in the RTC3. It grapples with estimating the direct consequences of the RFS during the period analyzed and by inference, the amount of crop acres and corn and soybeans produced due to this stimulus. It has a very clear and even-handed treatment of the relevant studies focused on this issue. It is clear about the uncertainty involved in making these estimates and the relationship between methods and outcomes. 1b. [no response provided] 1c. The choice of models is derived from the literature on LCLM and the effects of the RFS. There are a range of models and modeling approaches discussed. The principal models that I am aware of were discussed. There were many others as well. 1d. Yes. 1e. Yes. These are well articulated and discussed. Chapter 6: Attribution: Corn Ethanol and Corn Page 6-3. Infrastructure supporting ethanol production (and crop use) is mentioned. This is a key support for ethanol use and a key constraint on alternative feedstocks. It is often underappreciated. Page 6-4: Fig. 6.1 is a helpful summary and clear graphic Page 6-5: Table 6.1 is a helpful review of earlier supportive legislation for ethanol production and use. It is clear that Congress has long favored some ethanol use in gasoline, even before climate issues became widely known and important legislative priorities. Page 6.2, Fig 6.2: Most ethanol buildout was completed by 2009-10. Corn is 70% starch and oil (energy) so it was inevitable that it would be linked to energy production and use, especially when the US produces chronic surpluses of corn. The figure illustrates the effects of the 10% blend wall on ethanol in gasoline, a key policy constraint. Pages 6-3 to 6-12. Good discussion of legislative history. This long section is very informative. I know something about alternative fuel policy but still learned from the

	narrative and information included here. It's good that CA's powerful role in the alternative fuel market is recognized. Page 6-9, lines 177-184. EtOH should be credited with protecting water quality compared to MTBE. It was also the least costly alternative, so saved American consumers \$. Lines 196 ff: Initially the price of corn and oil remain unlinked, but since corn is 70% energy, they became linked, unavoidably (Fig. 6.4, page 6-10). Gasoline with ethanol became less expensive. Page 6-16, lines 408-409: <i>"The Program is binding if consumption would not have occurred at that level without the mandate, and the Program is not binding if consumption would have occurred at those levels under market conditions regardless of the mandate."</i> Very little volume was required to reach the 10% ethanol blend level once the RFS2 came into effect. This supports the RFS2 having only a limited additional effect on the expansion of corn planting and use. Fig. 6.9. The role and amount of exports is highlighted. This may have an effect on international land use and is important. It is relevant to chapter 16. Table 6.2 is valuable. Page 6-20. Good discussion of RINS. Page 6-21, lines 466-467: The chapter focuses on the RIN effects below the blend wall (10% of motor gasoline). Page 6-22. Policy makers were surprised by the increase in oil prices that occurred, reducing the stimulus effect of the RFS. Page 6-23, lines 529-532: <i>"Not an assessment of the potential effects of the RFS, but the actual effects that occurred historically..."</i>. This is a key distinction and one that is hard to keep in mind since many of the environmental effects discussed later in the report are derived from assessments or studies of overall biofuel production and use. Page 6-23, lines 539-541: No individual study accounts for all possible factors affecting ethanol production and consumption, ...but collectively the studies MAY be informative. It is good that EPA has been cautious in its treatment of the literature. Page 6-24. The importance of match blending is noted. Once adopted, it became difficult to revert for oil refineries. This is an important influence on the use of ethanol in gasoline. Page 6-27. Lines 648 ff A paper by Lark et al, (2022) is mentioned and its limitations noted. Generally, EPA suggests that it overestimated the effects of the RFS on corn production and use and some of its economic assumptions are criticized. This assessment seems reasonable. Elsewhere, this paper is criticized based on its use of CDL data and general classification of grasslands. It is frequently used elsewhere in the RtC3, however.
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	Page 6-28 and following---Biomass Scenario Model (BSM). Used for counterfactual analyses. Results seemed reasonable (Fig. 6.15). Page 6-31 lines 745-Scenario G-F suggest an effect of the RFS only for 2015(6) to 2018. In 2015 = 1.1B gal in most likely scenario. It seems the RFS acts as a backstop occasionally to keep the EtOH industry stable. Policy stability is an important consideration for investors. Page 6-32, lines 774-780: Model estimates for RFS1 = 0 to 1.1 B gal/y and for RFS = 0 to 1.1 B gal/yr. The BSM model has a complex structure that will not be apparent to most readers. It may be useful to have a box and appendix elaborating on this model if EPA chooses to rely on it. Page 6-34, lines 865-866: Logistical inertia is mentioned. this has been under described in the proceeding discussion as a motivating factor. Page 6-32: OTAQ not defined. Page 6-37: AEPE not defined. Fig. 6.20. Taheripour et al (2022) is the high estimate of the studies plotted. Page 6-39, lines 914-916: ... <i>"Thus, the synthesis of evidence suggests a dynamic range of effects from the RFS Program from 2013 to 2019, with the smallest effect in 2014 (0–1.2 billion gallons) and the largest effect in 2016 (0–2.1 billion gallons)".</i> The largest effect is a model estimate from the use of partial equilibrium model, so has some uncertainty associated with those large-scale economic models. Page 6-37: 6.3.7 Limitations Economic models may underestimate (or do not include) human judgement, and minimize the market signal's effect on investments and other decisions supporting the use of ethanol. Page 6-40, lines 965-967. Key conclusion in Chapter 6: <i>"Overall, current evidence suggests the RFS Program appears to have had a relatively modest effect during the period of major growth of the industry and a larger effect more recently as other factors have diminished in influence (e.g., oil price, VEETC."</i> Page 6-41 6.4 Corn ethanol. Page 6-42: Simulation and statistical models described. Good discussion of their relative strengths and weaknesses. Lines 1001-1003: the value of 2.1b gal for 2008 is used as a high point for assessing effects on corn production, but as modeled occurred only one year, and other years were much lower of zero. It is referred to as a midpoint, but a midpoint of that estimate might be 1.05 b gals. This is confusing. Please clarify.
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	Page 6-42, lines 1001-1004: EPA calls the high estimate of 2.1b gals/yr (from 2016) only the midpoint estimate for attributing the use of land to ethanol production in the US. Isn't it rather the high point and a single year's point estimate at that? Page 6-43, lines 1041-1043: Despite limitations, EPA relies on their judgement about results from both simulation and statistical studies to estimate the effects of ethanol production on corn and cropland area. Page 6-43, lines 1056 and ftnt #62: <i>"Using average yields for the country suggests that 0–756 million bushels of corn could be produced on 0–4.8 million acres of corn, or 0-5% of the planted corn acreage in 2016. Ftnt: From 2013 to 2019, an average of 14.3 billion bushels of corn were produced on 91 million acres of corn, for an average yield of 157 bushels per acre. Thus, 0–756 million bushels of corn is 0–4.8 million acres of corn (USDA, NASS). Planted corn acreage in 2016 was 94.0 million acres."</i> EPA's calculation for corn use in 2016-2017 crop year used 94 M acres planted (correct) but then the average yield from 2013-2019 (157 Bu/ acre), but this included the record drought year and was an off-trend year. The reported value from USDA for 2016-17 crop year was 174.6 bu/ac on average. EPA's use of an average value for yield biases the reported land area required upwards and is incorrect. This is problematic in that the effect of the RFS (re EPA) is not distinguishable from zero in any year, and should be corrected. Page 6-47, lines 1131-1132: the increases in US cropland acres reported on this page are small and less than year to year variation on total principal crop acres. How does EPA reconcile increases attributed to the RFS in only two years with production over the entire period estimated? Page 6-55, lines 1298-1299: EPA's best estimate for the effects of the RFS on corn and crop acreage is 0 to 3.5 M ac +/- 1 M ac for corn, and 0 to 1.9 M ac for cropland in total (based on the highest estimate in the literature (Taheripour et al., 2022). This is not large amount of land compared to approximately 325m acres in principal crops. The total current acreage is principal crops is near a multidecade low. The amount of land estimated to be affected is similar to year to year variation in land use across the period discussed, so lower estimates seem prudent. It is not clear that using a single, high year estimate is robust. Page 56, lines 1314 to 1329: They use an estimate from Lark et al (0 to 20 %) for total new cropland. These estimates have been contested and earlier in the Chapter were described as over-estimates. The land amounts are in any case small relative total cropland use. Care will have to be used in attributing environmental effects to the RFS. Page 6-58: EPA relies predominantly on Li et al., 2019 for their estimates. Cropland in this study excludes hay and minor crops and is almost 100M ac lower than major crop acreages. Does EPA analyze the validity of the instrumental values used by Li et al? Are these exogenous estimates based on <i>a priori</i> knowledge? See page 6-59, line 1393. Chapter synthesis: Clear and thorough.
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	Missing: an adequate discussion of export demand and commodity markets. See for example: <i>..."Meanwhile, robust economic growth in major global markets in 2010 and early 2011 (including China, India, Brazil, and other parts of Asia and the Middle East) reinvigorated international consumer demand and, when coupled with a weak U.S. dollar and events that occurred in international feed grain markets—drought in Russia, Kazakhstan, and the Ukraine in 2010, plus strong Chinese demand for corn and feedstuffs—contributed to record U.S. agricultural export values in 2010 and 2011 and helped to push commodity prices, especially corn, upward again."</i> Congressional Research Service (2013), page 15. This reference and its discussion of exports is generally missing or underemphasized in Chapter 6. This is especially important when discussing crop shifting to corn and soybeans in rotation, and especially in locations where new demand for soybean exports to Asia is supported (the western corn belt and the Dakotas). This area is noted in Lark et al (2022) as a region where land use change has occurred and change is attributed to the RFS, when at least some of whatever actual changes occurred can be attributed to soybean exports.
Kurkalova	1a. Chapter 6 material is clearly organized and presented, and appears technically accurate. The authors are to be commended for considering five lines of evidence to separate the effect of the RFS Program from other, multiple factors that simultaneously influenced the production and use of domestic corn ethanol over four distinct time periods. The criteria for the assessment of attribution are clearly explained for each source of information. The comparison of mandates with consumption, the analysis of RIN prices, the review of peer-reviewed literature, and two new analyses jointly justify the reported attribution. The synthesis of evidence in section 6.3.6 is convincing. 1b. The most important data and recent relevant literature is cited and appropriately interpreted. The explanation of the usefulness of both prospective and retrospective studies is clear; the discussion of the importance of accounting for specific factors in addition to the RFS Program is insightful, and so is the analysis of the studies' comparability. The detailed analysis of the Taheripour et al. (2022) and the comparison with Lark et al. (2022) results in section 6.3.3 are clear. For completeness of the arguments put forth in the report, it might be useful to include references to the studies that specifically analyzed which assumptions, approaches, data, and other modeling details of the latter study led to the differing results. Alarcon Farconi et al. (2022) and Taheripour et al. (2022b) (both additional references) provide such analyses. 1c. The reliance on five different lines of evidence contributes to the overall accuracy of inferences.

	The authors are to be commended for the clear explanations of the benefits and limitations of alternative modeling studies, simulation and statistical (section 6.4). The ranges of the final estimates are well justified. 1d. The conclusions and recommendations described in the chapter synthesis section are supported by the material presented. Two suggestions for improvement are detailed below. To supplement bullets one and six of section 6.6.3 (Recommendations), which are also related to bullet five of section 6.6.2 (Uncertainties and Limitations), I suggest recommending the integration of the economic models with good market detail of the biofuels industry (e.g., Taheripour et al., 2022) with the models that capture the geographic variability of crop production at a fine geographic scale (e.g., Lark et al., 2022). The integration might help to identify location-specific attribution of the RFP Program in future RtC reports. The other recommendation for future research is to analyze the increased corn (and soybean) acreage in the areas outside of the Midwest such as the lower portion of the Mississippi river basin and the south-eastern coastal plain. Both areas have experienced significant increase in corn and soybean acreage over the range of the years considered in the RtC3 (e.g., Figure 5.6 of Chapter 5) and are environmentally sensitive (please see the corresponding recommendation for Chapter 9). 1e. The major uncertainties and limitations associated with the chapter's conclusions are clearly identified.
Landis	1a. Overall, I have a very favorable impression of the clarity and technical accuracy of the materials presented in this chapter. As an agroecologist who has worked in the biofuel research arena for more than 15 years, I am familiar with many but not all the terms, concepts, and potentially causal factors addressed in this chapter. The authors have done a very good job of explaining the background behind each concept/factor covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results. 1b. Several additional important studies should be included in the analysis. • Donnison, C., Holland, R. A., Harris, Z. M., Eigenbrod, F., & Taylor, G. (2021). Land-use change from food to energy: Meta-analysis unravels effects of bioenergy on biodiversity and cultural ecosystem services. <i>Environmental Research Letters</i>, 16(11), 113005. https://doi.org/10.1088/1748-9326/ac22be • Kocak, E., Bilgili, F., Bulut, U. and Kuskaya, S., 2022. Is ethanol production responsible for the increase in corn prices?. <i>Renewable Energy</i>, 199, pp.689-696. • Stewart, D.W., Cortés-Peña, Y.R., Li, Y., Stillwell, A.S., Khanna, M. and Guest, J.S., 2023. Implications of Biorefinery Policy Incentives and

	Location-Specific Economic Parameters for the Financial Viability of Biofuels. <i>Environmental Science & Technology</i>. 1c. I have limited knowledge of economic and technoeconomic modeling and virtually no familiarity with partial equilibrium modeling, therefore I am not able to comment on selection or use of the models or their accuracy. I did review the modeling explanation and results and I have no reason to doubt the conclusions or resulting synthesis. 1d. Overall, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material <u>presented</u>. However, I feel that insufficient attention has been paid to the concept of “<i>market certainty</i>” described in my response to Chapters 1-5 above. Accounting for market effects would likely increase the estimates of RFS attributable biofuel production with cascading implications for the LCLM analysis and environmental impacts. 1e. For the most part, the chapter does a good job identifying uncertainties and limitations associated with its conclusions. However, I feel that dismissing potentially critical factors such as the “market effects” argument is done without sufficient justification.
Smith	1a. This chapter is the core of the report because every reported effect of ethanol use that comes later is small because of the small amount of ethanol attributed in this chapter to the RFS. This approach limits the applicability of the findings to future changes in the RFS, which is one important potential use of this report. Rather than basing the entire report on the estimated amount of ethanol attributable to the RFS, the authors should report per gallon effects (or dose-response functions) throughout. This would make clear the estimated environmental effects of each additional gallon of ethanol. Clarification: I don’t think the blendwall and VEETC are relevant here. A billion gallons of ethanol requires 370m bushels of corn, which is about 2.5% of annual corn use. Based on estimated elasticities of corn demand and supply, you can estimate the increase in price from this 2.5% increase in demand and you can estimate the increase corn acres planted resulting from this price increase. Then you can estimate the effects on water quality and other outcomes due to that increase in corn acres planted. If you think ethanol increased by 2 billion gallons in response to the RFS, then the effects are twice as big. This is the approach taken by Lark et al (PNAS, 2022). All estimates in that paper are scalable in that way. I am less familiar with air quality estimates than agricultural effects, but I think you can do something similar there. Maybe it matters whether the marginal gallons are in E10 or E85, but it seems you could deal with that. I agree that increasing the RVO by a billion gallons might have different effects depending on whether we are above or below the blendwall. If we’re below the blend wall, those additional gallons would come from ethanol; if we’re above

the blendwall, then those gallons may come from biodiesel. However, you can report the environmental effects for *each gallon of ethanol*.

The chapter is clearly written and appears to be mostly accurate in what it presents. However, as I discuss below, I believe the chapter as a whole presents a skewed view of the evidence. By leaving out or dismissing some highly relevant literature, it underestimates the effects of the RFS on prices and land use. This comment is also relevant to the background chapters 1, 2, and 4.

- 1b. The core of the entire report is the finding that the Renewable Fuel Standard had only a small incremental effect on ethanol consumption: 0-0.4 billion gallons in 2012 and 0-2.1 billion gallons since 2013. For context, actual ethanol consumption was 12.9 billion gallons in 2012 and 14.4 billion gallons in 2016.

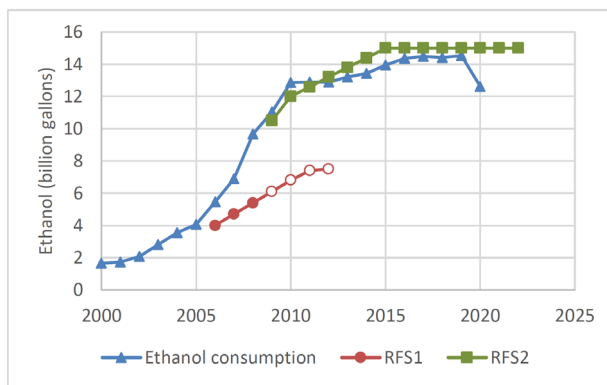


Figure 6.10. Ethanol consumption versus the RFS1 and RFS2 mandates. Annual consumption is from EIA Monthly Energy Review (Table 10.3). RFS1 mandates in the EPA Final Rules and EPAct were equal, and mandates for the RFS2 are from the implied conventional biofuel which is mostly corn ethanol in the United States (see Chapter 1, Table 1.1). Open circles indicate years where there was a standard by statute that was not in effect by rule (e.g., 2009-2012 for the RFS1).

The authors present multiple pieces of evidence.

- i. MTBE and the oxygenate requirement (Section 6.2.2). When MTBE was phased out, refiners replaced it with ethanol to meet the standard for reformulated gasoline (RFG), which accounts for about a third of gasoline in the nation. RFS1 mandated ethanol volumes slightly above what was required to meet RFG standards. Given the RFG requirements and that some ethanol had been used in conventional gasoline in the Midwest prior to RFS1, it seems reasonable to assume that RFS1 was not binding (Figure 6.5).
- ii. RIN prices (Section 6.3.2). RIN prices were low until the end of 2012, which suggests that fuel suppliers did not need to be subsidized to use ethanol. However, RINs only started trading in 2008 after most ethanol plants has been built. The low RIN prices tell us that *existing* ethanol refineries could produce ethanol at a low enough cost that fuel suppliers would use it without the RIN subsidy. However, it does not tell us whether those ethanol refineries would have been built without RFS2. RFS2 removed the risk associated with the capital investment in constructing new ethanol plants.

- iii. Octane value vs energy density (Section 6.3.3). Ethanol contains about 30% less energy than gasoline, which makes it less valuable, but it has higher octane than gasoline, which makes it more valuable. The report uses the model in Babcock (2013), which assumes that these two factors cancel each other out exactly up to 10 billion gallons of ethanol, after which willingness to pay for ethanol drops quickly. Based on this model, the report estimates that RFS2 increased ethanol use by 2.2-3 billion gallons. This estimate depends on the assumed point at which ethanol demand drops (here, it was 10 billion gallons).

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Ethanol without Subsidies 1321

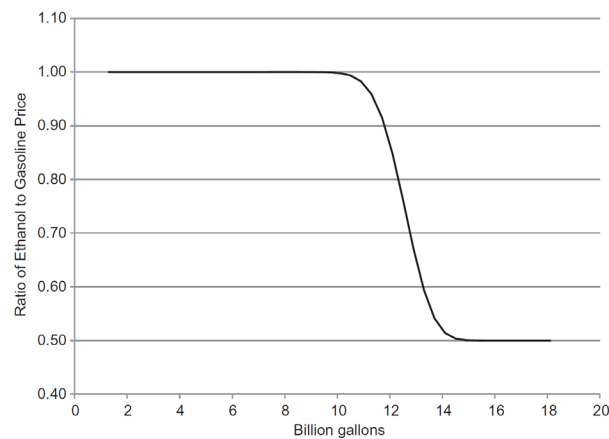


Figure 4. U.S. demand curve with high value for octane and oxygen and blend wall

- iv. Oil prices and VEETC (Section 6.3.3). Based on a model from Taheripour et al. (2022), the report finds that the RFS2 was not binding in 2005–2007 and 2009–2010, and was binding in 2008 and 2011–2016 (Figure 6.14). The standard becomes binding in 2008 because of low oil prices and in 2011 because the VEETC expired. This model simulation assumes that the ethanol plants had been built; the authors are modeling the decision whether to blend ethanol and not the decision to build ethanol plants in the first place. The potential role of the RFS2 in was noted on page 17 of the supplemental material of Taheripour et al. (2022), where they write “prior to this year (2011), the RFS provided an incentive to get capacity”.

The report notes on page 6-39 that one limitation of the analysis is that it excludes the role of the RFS in incentivizing the construction of ethanol plants. I encourage the authors to include scenarios in their main analysis that incorporate this possibility.

On page 6-27, the report misrepresents the econometric analysis of Carter, Rausser and Smith (2017). These are as follows

- i. Claim: it assumes the RFS2-effect began in 2006, which is actually prior to EISA (signed into law December 2007) and well prior to promulgation of the rule (March 2010).

Response: The paper explains at great length that the corn price increase in late 2006 resulted from expectations about the future path of the Renewable Fuel Standard.

Clarification: As with any price change, there is no way to definitively say what caused corn prices to increase in the fall of 2007. Perhaps I overstated the point by pointing to “the future path of the Renewable Fuel Standard” rather than “the future path of ethanol production”. As I noted in other places, we cannot know how many of the plants under construction in that period were spurred by the RFS rather than optimism about the demand for ethanol.

The model in Carter, Rausser, and Smith (2017) identifies shocks to expected future net demand using distant futures prices. Intuitively, if futures price for post-harvest delivery increases and if carryover inventory increases, then, then it indicates that traders are anticipating higher net demand in the following year. The model shows a big increase in this inventory demand in 2006-07. I don’t believe this is due to MTBE for two reasons:

(i) the national MTBE ban was imposed in May 2006 after many states had banned it previously. Corn markets do not usually react with a such a long delay.

(ii) MTBE replacement could have been achieved under RFS1, but the expansion in ethanol production capacity was far above that. The first figure below (from Carter, Rausser, and Smith (2017)) shows that 5.5 billion gallons of capacity was under construction at the beginning of 2007. The second figure below (from the supplementary materials of Lark et al (2022)) shows that USDA dramatically increased its baseline projections for ethanol between Feb 2006 and Feb 2007. The increase was over an above the amount required for MTBE replacement and was similar to the amount of new capacity under construction in early 2007.

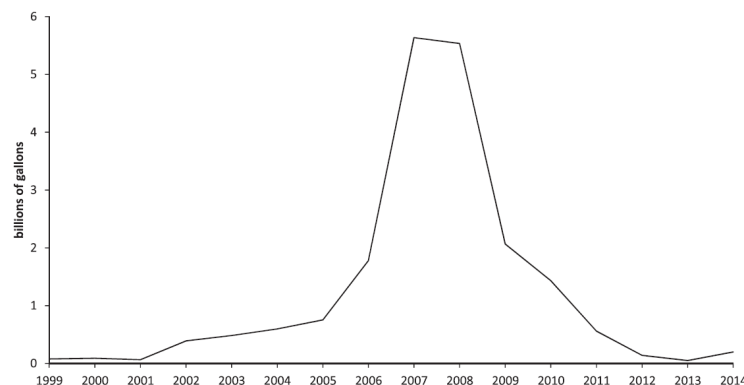


Figure 1. Ethanol production capacity under construction of expansion

Note: Measured at the beginning of the year. Data source is Renewable Fuels Association.

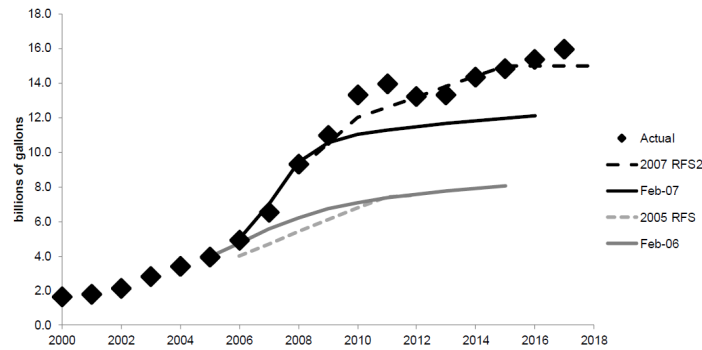


Fig. S7. Projected, mandated, and actual ethanol production. Dashed lines represented the amount of conventional renewable fuels mandated by the 2005 and 2007 versions of the RFS. Solid lines represent the amount of production projected by the USDA in February 2006 and February 2007.

- ii. Claim: it assumes beginning in 2006 a baseline without the RFS Program to be the annual mandates under the RFS1. Given that actual production far exceeded the RFS1 mandates in every year that the RFS1 volume targets applied (Figure 6.10), it is not realistic to assume zero growth above the RFS1 mandates absent RFS2.

Response: Volumes exceeded RFS1 in 2006 because of the oxygenate requirement. In later years, RFS1 requirements would have increased above the amount of ethanol required to meet RFG standards. Ethanol use would only have continued to exceed RFS1 volumes if enough additional ethanol plants were built.

- iii. Claim: two key factors that affect corn ethanol production and consumption were omitted from the underlying economic model: the effects from MTBE replacement on corn price and the effects of a switch from splash blending to match blending.

Response: Corn prices jumped by 25% in early 2004 due to higher than expected ethanol demand from the phaseout of MTBE. However, the 2004 growing season produced record yields, which offset the ethanol demand shock. This year is in the estimation sample, so the varying responses of spot prices, futures prices, and inventory levels to these shocks help determine the coefficient values in the model. I refer the authors of the report to Stock and Watson (Journal of Economic Perspectives, 2001) for discussion of how vector autoregressions are used in macroeconomics to estimate causal effects without specifying large models filled with numerous assumptions. The splash blending to match blending claim is not about the modeling but rather a statement that, after the ethanol plants were built and began operating, the industry re-optimized.

Section 4.3.2 addresses the corn price effects from corn ethanol policies. It omits Carter, Rausser, and Smith (*American Journal of Agricultural Economics*, 2017). This paper reports a 31% price increase from every 5.5 billion gallon increase in corn ethanol production, which corresponds to a $31/5.5 = 5.6\%$ increase in the price of corn per billion gallons. This estimate is slightly above the 3-5% range reported in

	Section 4.3.2 based on Condon et al (2015). It has a wide confidence interval, so plausible estimates include substantially higher and lower effects. The Carter, Rausser, and Smith (2017) incorporates storage, which allows it to separately estimate the effects of short-run shocks (e.g., bad weather in a single crop year) from long-run shocks (e.g., a persistent increase in demand for ethanol). The authors note this distinction on page 4-10, where they write “a weather-induced shock to corn yields had a relatively modest impact on corn prices, as the ability to store corn absorbed much of the impact.” However, they do not follow this point to its logical conclusion that annual variation in prices and quantities is driven largely shocks that have muted price effects because they can be mitigated by adjusting storage. Therefore, econometric studies based on such data may underestimate price effects. Section 4.3.4 dismisses Lark et al. (2022) as “better characterized as an estimate of the effect of the increase in corn ethanol demand above the 2005 RFS1 mandates from many factors, including, but not limited to, the RFS2.” However, the estimates in this study are scalable. The authors write “Our price effects are scalable, however, such that all land use, environmental quality, and GHG emissions that we report would remain the same on a per volume of ethanol basis, independent of the magnitude of demand change (within reasonable limit) or its source.” Lark et al. (2022) study the effects of a 5.5 billion gallon increase in ethanol demand and estimate that corn area increases by 2.8 million hectares (6.9 million acres). They find that total cropland increases by 2.1 million hectares (5.2 million acres). These estimates are based on spatially-granular econometric modeling similar to Pates and Hendricks (<i>American Journal of Agricultural Economics</i>, 2021), which is omitted from the report. Clarification: Pates, N.J. and Hendricks, N.P., 2021. Fields from afar: Evidence of heterogeneity in United States corn rotational response from remote sensing data. <i>American Journal of Agricultural Economics</i>, 103(5), pp.1759-1782. In Table 6.10, the review reports econometric estimates for increasing ethanol demand by 2.1 billion gallons. They report increases of up to 3.52 million corn acres and up to 1.93 cropland acres. The Lark et al. (2022) estimates imply an increase of $6.9 \times 2.1 / 5.5 = 2.3$ million corn acres and an increase of $5.2 \times 2.1 / 5.5 = 2.0$ million cropland acres. Thus, rather than being “roughly double the estimates presented here,” the Lark et al. (2022) are similar on a per gallon basis. Moreover, the statement on page 6-58 that “Li et al. (2019) is the only statistical study the authors identified on the effect of ethanol on land that is national in scope” is false because Pates and Hendricks (2021), and by extension Lark et al (2022), also meets this standard. 1c. I am unsure why Chapter 4 includes sections on impacts from the RFS program when the point of Chapters 6 and 7 is to explicate these impacts. Why not combine them?
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	I am wary of simulation models, especially large general equilibrium models. They are compelling because they promise to account for all relevant factors. Yet, building a model that accounts for all factors necessarily requires numerous assumptions on how all those factors interact. Taheripour et al. (2022) exemplify this challenge when they have to add “shift factors” to their modeling to get it to match the data. Such modeling can be necessary before policies are enacted as we do not yet have data after implementation of the policy. For this reason, I recommend that relatively more emphasis be placed on econometric studies. In Chapter 6, the simulation and econometric studies provide similar estimates of land use change per gallon of ethanol, so this recommendation is less consequential than it otherwise might be. To my knowledge, there are few studies that incorporate intertemporal factors into their modeling (whether simulations or econometrics). Relevant factors include commodity storage and crop rotations. The aforementioned Carter, Rausser, and Smith (2017) and Pates and Hendricks (2021) papers are exceptions. 1d. Yes, but for the caveats I described above. In particular, the conclusion that the RFS2 did little to spur ethanol production relies on an assumption that the RFS played no role in incentivizing ethanol plants to be built. Perhaps this assumption is correct, but the authors should include in their range of potential effects the possibility that the RFS caused an increase in ethanol production capacity. 1e. Yes, notwithstanding the above caveats.
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3.2.1.2 Chapter 7: Attribution – Biodiesel and Renewable Diesel

Reviewer	Comments
de Gorter	1a. The chapter is not so clear as to how much the RFS Program impacted BBD consumption (unlike in Chapter 6 for ethanol). It leaves us hanging and does not take a stand – it is somewhat ambiguous. As you read my specific comments above, it seems like the RFS2 did generate most of BBD consumption as the tax credit was never high enough not to have the mandate binding all the time (see Figure 2). And because the tax credit is not redundant when not binding, I think we must conclude the RFS2 dominated. I have very specific suggestions for improvements on the presentation of data, especially Figures 7.1 and 7.6 where ex post mandated volumes should be compared to actual consumption. Figures and Tables like in Report with Radich are highly recommend so the analysis becomes sharper and in better focus to understand exactly what is going on. Figure 7.10 could be improved on – see my Figure 5. There is too much ambiguity as to the substitutability between vegetable oils – I think the evidence in Figure 5 is strong that they are very highly substitutable. Figure 7.4 can be improved upon. See my Figures 3 and 4 for starters.

Figure 7.5 may make the reader believe the price of biodiesel is below the diesel price. My Figure 2 rules out any doubt.

The effect of state incentives can be viewed in the same way as the BTC when the mandate is binding.

- 1b. This chapter and all other chapters omit a very important literature. I list the papers on pages 2-3 of [deGorter Summary Biofuel Theory](#) and above in my comment on Chapter 6 and at the end of my specific comments on Chapter 4 below. These must be integrated into the analysis of this EPA Report.

For Chapter 7, Section 7.3.3 reviews the literature yet the literature in Chapters 16 and 4 are more extensive as to the effect of BBD on prices. There is a good amount of duplication too. It would be good to have all the papers on the effects of BBD on prices be analyzed in one section.

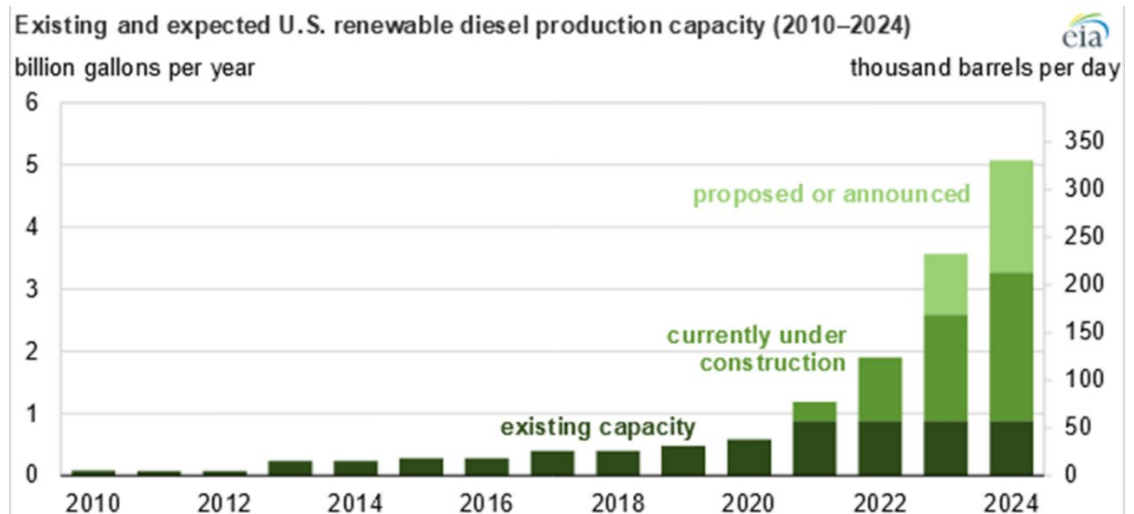
- 1c. On lines 420-423, the EPA Report punts on how BBD impacts prices of soybeans, soybean oil and the like. Yet studies on this topic are analyzed in more detail in Chapter 16 and Chapter 4.
- 1d. Yes, except for the caveats I identify above and those in my specific comments above. But regardless, I would be more direct in arguing the RFS was the major reason for BBD consumption in the U.S.

Lines 625-628: I disagree. I think my research has shown very clearly what a BTC does with a binding mandate and is accepted by the profession at large.

Hence, I disagree with Lines 644-645 too.

- 1e. More research is required. I agree with that.

A big shortcoming is a robust discussion on the future of renewable diesel, and its implications for the RFS and the biofuel market. The EIA predicts production of 5 billion gallons.



The EIA predicts biodiesel production to level off and decline somewhat. What is the EPA going to do with the standards?

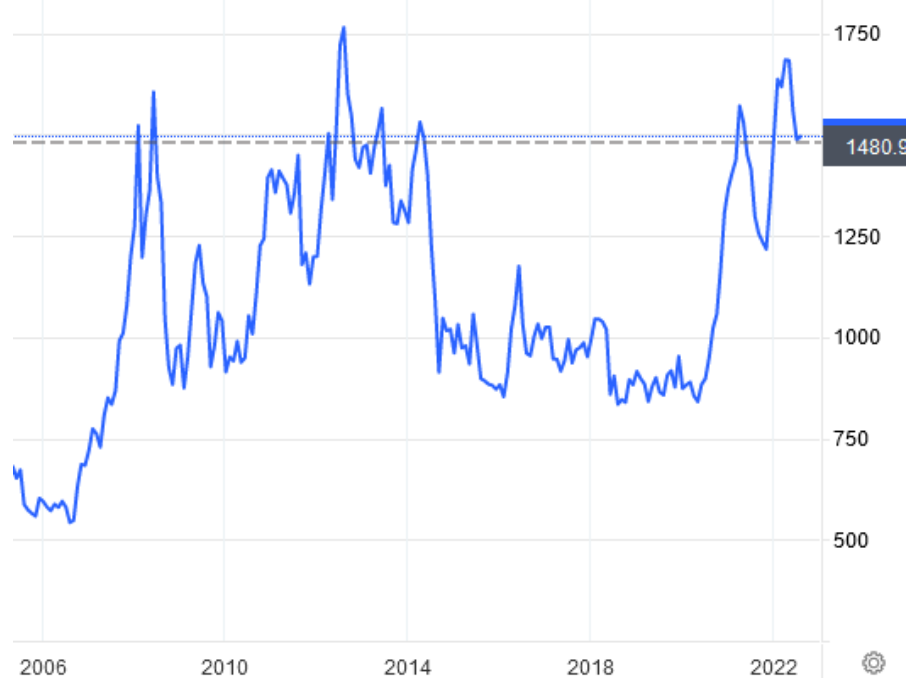
Comments on Chapter 7 -Attribution: Biodiesel and Renewable Diesel

BBD production increased in 2006 because of higher crude oil prices, the BTC and “splash and dash” for both domestic and imported BBD. Imported of BBD at the port could get a splash of diesel and then receive the \$1/gal BTC and sell to a higher BBD price market in Europe. This lasted to late 2008 where domestic production could still get the splash and dash (but Europe put on import duties on U.S. BBD exports).

Figure 7.1 in the EPA Report shows for 2007 to 2009 that BBD imports are greater than domestic consumption of BBD, itself half of domestic production of BBD. The BTC with splash and dash dominated (along with high diesel prices).

It would be useful to show mandated volumes *ex post* (based on actual diesel and gasoline consumption) as the mandate is a blend mandate (see my comments on Chapter 6 for ethanol – see figures and tables in [Report with Radich](#)). See the data in your Figure 7.1 for 2010: why consumption less than 500 million gallons? Not because the mandate was not fulfilled (used stored D4 RINs or need to calculate the volume of BBD consumed given the blend (%tage) mandate).

Because BBD consumption is low in 2010 (the first year of the RFS2 being implemented), the effects of the BTC and all other state incentives should for the most part be ignored because it would not have sustained BBD consumption. Production and imports were distorted for other reasons. Figure 2 shows without a doubt that the mandate price premium for BBD was above that even with the tax credit. The tax credit in that situation is not redundant: it subsidizes fuel (diesel plus BBD) consumption (see references above). So, the RFS2 dominates the reasons why BBD consumption increased.

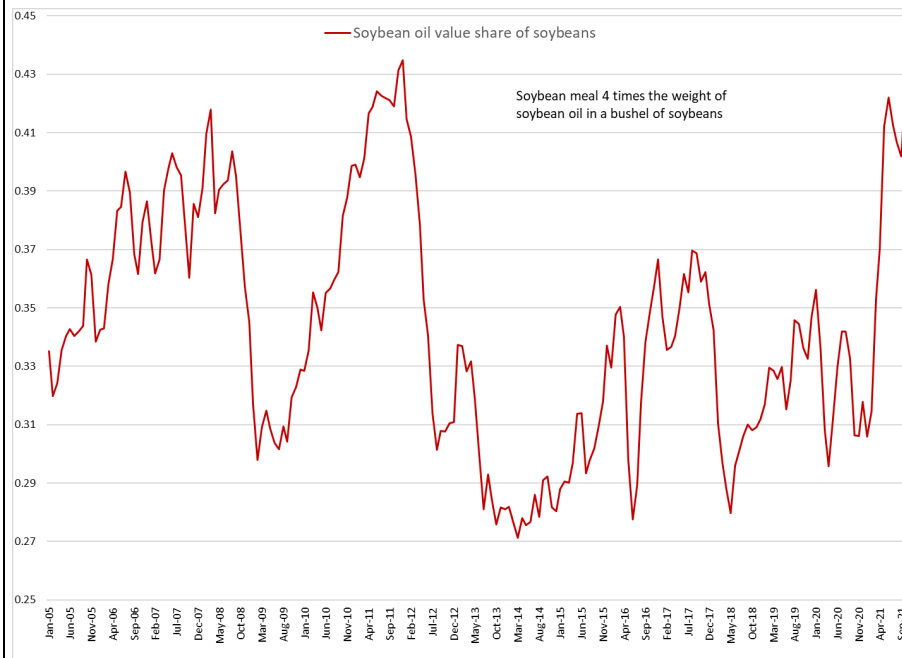
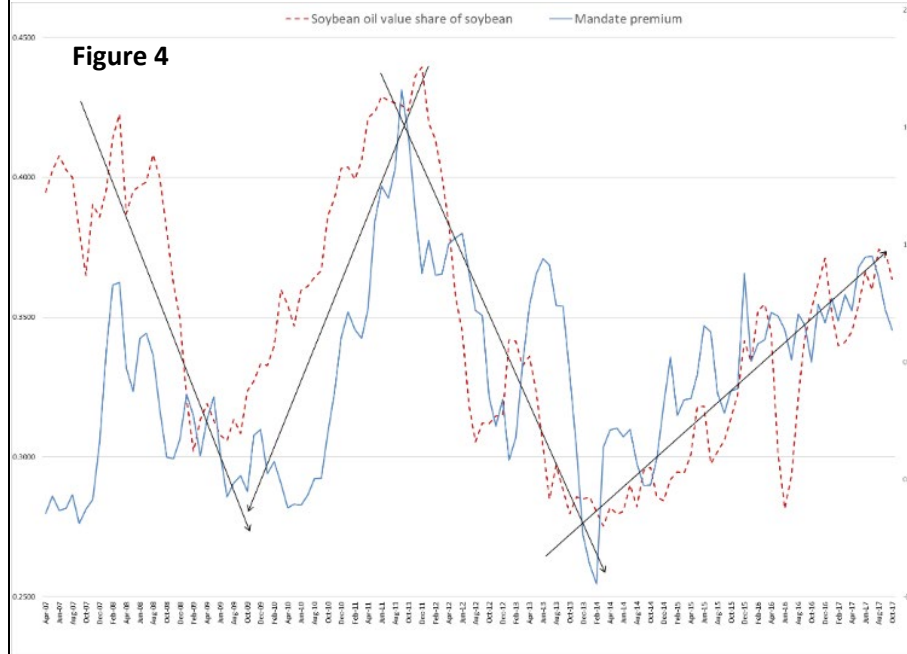


And mandated volumes of BBD increased.

Figure 7.2: need to show consumption volumes as in [Report with Radich](#) and how tax credit impacts that.

Lines 138-147: Figure 2 shows mandate price premium for BBD above the tax credit was positive from March 2008 onwards so the tax credit cannot increase BBD consumption. It subsidizes fuel (diesel plus BBD) consumption, most of which is fossil fuel-based diesel.

Lines 160-169: The accompanying figure above shows soybean oil prices. Figure 3 shows the soybean oil value share of a soybean. It rose in 2006 to 2008 because of “splash and dash”. It rose on 2010-11 because of BBD consumption worldwide with high diesel prices. And again in 2021. The relatively high soybean oil share 2015 to 2017 is informed by Figure 5 – then mandate premium for BBD was high as calculated from equation [2].

Figure 3**Figure 4**

The lessons from **Figure 7.5** are better served by presenting and drawing out the implications of my Figure 2.

Figure 7.6: much better to present the data in Figure 2 and Tables 3-4 in [Report with Radich](#) to see BBD consumption and how much is due to BBD mandate versus overage for ethanol blend wall. The volume requirements in Figure 7.6 are those listed in the RFS2 – not the

volumes resulting from the blend mandate after actual diesel and gasoline consumption is realized (which indicates if the mandate was fulfilled or not).

It would be great to have data on BBD consumption from D6 BBD RINs, renewable diesel, and combine with California consumption in Figure 7.7.

Section 7.2.5 State Mandates

This section better titled *State Incentives*.

A lot of these incentives exist at the federal level too (e.g., fleet cars must use ethanol).

It is impossible to identify how much BBD consumption would have occurred. All these incentives are like the BTC with a binding mandate: they are subsidizing fuel consumption.

One exception is California's LCFS. It takes advantage of D4 RIN prices to encourage their goals but also vice-versa: the cost of compliance for the rest of the country of the RFS2 BBD mandate was lower because of the LCFS. The LCFS carbon credit price and D4 RIN prices are additive. But the LCFS is subsidizing BBD consumption in California and acts just like the national BTC does (in theory, it is a BBD consumption subsidy). But, because the RFS2 mandate is binding, all subsidies are subsidizing fuel consumption, most of which is diesel.

Lines 237-245: same conclusion above holds.

Figure 7.7: all California consumption counts towards the RFS2 (the BBD mandate, D5 and spillage into the D6 category).

Line 256: I thought the stated goals of the RFS2 are three-fold: to reduce greenhouse gas (GHG) emissions and expand the nation's renewable fuels sector while reducing reliance on imported oil (yet the U.S. is exporting a lot of petroleum products while using imported oil and now a net exporter of petroleum).

Lines 246-247

State-level incentives appear to have a significant impact on the consumption of biodiesel and renewable diesel in states where they are active.

Lines 248-250

Some of these incentives may be significant enough to drive the production and use of biodiesel and renewable diesel in the absence of the RFS Program.

BTC not redundant so in theory, it could have generated some BBD consumption (but for sure less than with the RFS2) and "what really happened" is that the BTC was not redundant; it was subsidizing fuel consumption. Same thing for all the state incentives, including the LCFS.

The EPA Report states the RFS2 "could have" generated the observed ethanol production and consumption but in reality, other policies and factors did. The implication being the RFS2 was redundant in affecting ethanol consumption.

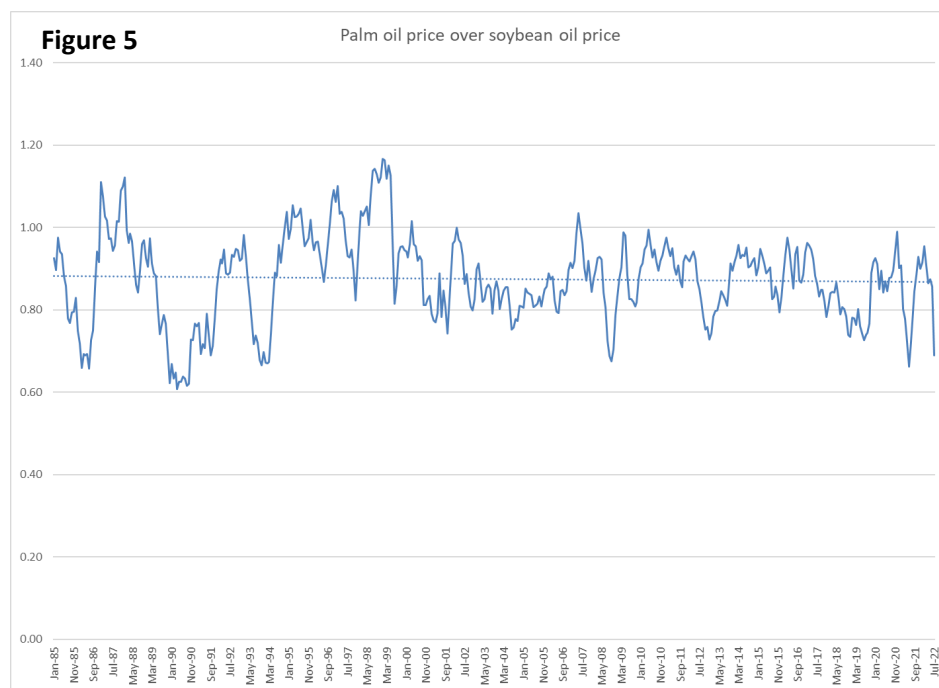
Likewise, if RFS2 did not exist, the BTC and state incentives for BBD "could have" produced part of observed BBD consumption (Figure 2 shows the mandate is always binding). But the

	BTC and state incentives for BBD were not redundant – they subsidized fuel consumption with a binding mandate. So, using the same logic as in Chapter 6 for ethanol, I come to a very different conclusion: the RFS2 dominated in affecting BBD consumption and indirectly the BTC and state incentives for BBD increased BBD consumption a little. So, BBD consumption should mostly be attributed to the RFS2. I therefore disagree with lines 365-366: <i>unlike ethanol, the RFS Program has been binding since 2010 indicating that at least some volume of biodiesel and renewable diesel has been attributable to the RFS Program</i> Most is attributable to the RFS Program. Lines 385-387: <i>In years with a BTC, the credit absorbed a portion of the potential effect from the RFS Program, and thus the D4 and D5 RIN price was lower</i> D4 RIN prices are lower, but the tax credit did not have the RFS mandate be relaxed – that is not how the economics of a tax credit combined with a binding mandate works. The mandate does all the heavy lifting and the BTC subsidizes fuel consumption. Lines 397-399 <i>None of these studies included the cost of oil in their estimates or the BTC, suggesting limited utility for the purposes for this chapter of assessing the effect of the RFS Program specifically.</i> Because the mandate is always binding, the BTC would only slightly increase BBD consumption through subsidizing fuel consumption. If a counterfactual analysis is required (BTC with no RFS2), then need to do and know the market price of diesel. But it would be a hypothetical analysis -- the tax credit was only binding prior to March 2008 (no data to go back further) and with “splash and dash”. Lines 420-423: <i>This section does not include an assessment of the impact of the RFS Program on the price of soybeans or soybean plantings. These relationships are complicated by the fact that historically most of the value of a bushel of soybeans has come from the soybean meal. The relationship between biodiesel production, soybean prices, and soybean planting is an area where further research is needed</i> See this paper: Drabik, Dusan, Harry de Gorter, and Govinda R. Timilsina. 2014. “The Effect of Biodiesel Policies on World Biodiesel and Oilseed Prices.” <i>Energy Economics</i> 44: 80—88. Use of such a model can answer the question of how much an increase in soybean oil demand due to biofuels will impact soybean prices.
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Lines 486-488

Another, perhaps more important, way that the RFS Program may incentivize the production of palm oil is by enabling the biodiesel and renewable diesel industry to outbid other industries for RFS-qualifying feedstocks such as soybean oil and FOGs. These industries, primarily animal feed and oleochemicals, may then turn to lower-cost palm oil (Figure 7.10),

Palm oil and soybean oil are very close substitutes. See Figure 5. The relative price line is flat. In no way will soybean oil prices go up in the past due to BBD and not have palm oil revert to the flat line in Figure 6. It is not chance. It is that either vegetable oil can be used for biodiesel or for food or for industrial purposes.

**Hill**

- 1a. The material presented in this chapter is largely clear and technically accurate.
- 1b. Yes, the most relevant recent research appears to be cited.
- 1c. This chapter's descriptions of general trends and policy environments are well presented overall.
- 1d. Yes, they are.
- 1e. Yes, it does.

Hoekman

- 1a. Chapter 7 clearly lays out several issues related to the impact of the RFS Program on the production and use of biodiesel fuel. Based on my understanding, this material is presented in a technically accurate manner. Unlike the previous chapter in which the attribution of corn ethanol to the RFS Program is defined, there is insufficient data to

	conduct a similar attribution of biodiesel. Nevertheless, it appears certain that the RFS Program (especially during the RFS2 period) had a greater impact on biodiesel production than it did on corn ethanol production. Because other factors such as the Biodiesel Tax Credit (BTC) and various other State and Federal incentives influenced the production of biodiesel, it is not possible to accurately attribute specific annual biodiesel volumes to the RFS Program. Yet, some data exist that allow for semi-quantitative assessment of this attribution. For example, footnote no. 31 (page 7-22) states: “The maximum volume of biodiesel and renewable diesel attributable to the RFS Program from 2010 to 2019 averaged approximately 1,193 million gallons (70%) with a low of 136 million gallons in 2010 (52%) and a high of 1,902 million gallons in 2016 (74%).” Based on this information would it be accurate to say that a best guess estimate is that 70% of biodiesel production and use can be attributed to RFS during the RFS2 time period? This is in sharp contrast with ethanol, where a very small fraction (estimated to be 0-10%) is attributed to the RFS Program. 1b. To my knowledge, Chapter 7 does cite and appropriately interpret the most recent relevant data. One possible exception regards differences between biodiesel (BD) and renewable diesel (RD). For the most part, BD and RD are treated together in this chapter. Yet there are some data that distinguish between the two. For example, Figure 7.6 (page 7-10) distinguishes BD and RD in the U.S. market, and Figure 7.7 (page 7-12) distinguishes these two in California. However, Figure 7.1 (page 7-4) makes no distinction between BD and RD. To the extent that data are available, BD and RD should be dealt with separately. 1c. I am not qualified to judge the selection and use of models used in Chapter 7 to assess the impacts of the RFS Program on biodiesel and renewable diesel. 1d. Yes, for the most part, the conclusions and recommendations are supported by the material presented in Chapter 7. It appears clear that during the entire RFS2 period (2010-2019), the RFS Program was binding on BD/RD. However, because of the important influence of other State and Federal policies, it is not possible to define accurately a specific BD/RD volume attributable to the RFS Program. Nevertheless, it would be useful to provide semi-quantitative “best guess” figures, wherever possible. The recommendation that “Agro-economic modeling and other quantitative analyses ... are needed to determine the extent to which the RFS Program impacted biodiesel production and impacts” is certainly supported by the material presented here. The level of model sophistication and reliability for BD/RD must be improved substantially to approach the quality of current modeling used for ethanol. With more reliable modeling tools, it would be possible to provide better answers to some of the questions posed in this chapter about the RFS Program’s impacts on LCLM, volumes of BD/RD produced, distinctions between BD and RD, and others. 1e. Yes, important uncertainties and limitations associated with the conclusions in Chapter 7 are identified. The most fundamental limitation seems to be lack of reliable data needed to make quantitative conclusions about the impacts of the RFS Program on changes in agricultural practices, substitutability of different food/feed stocks, and interactions between the RFS Program and various other State and Federal incentive programs. The
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	remedy for this situation involves development of additional data and application of additional analyses, as explained in the Recommendations section. General Comments It is noteworthy that in California, RD now dominates over BD. This development is driven by the requirements/incentives within the California Low Carbon Fuel Standard (LCFS). This raises obvious questions about how the LCFS Program has influenced biofuel choices and volumes in the rest of the U.S., and how this situation is likely to change as the LCFS requirements become more stringent and additional jurisdictions adopt similar programs in the future. Some discussion of these issues should be included in the RtC3. Although BD and RD are generally treated together in the RtC3 report, they are different fuels, produced by different processes. For example, glycerin is produced during the manufacture of BD (but not RD), while propane is produced during the manufacture of RD (but not BD). Additionally, H₂ is required in the production of RD, but not BD. At least a brief discussion should be included regarding potential differences (if any) in the environmental impacts of BD and RD. This is important as the relative amounts of BD and RD are changing, with RD volumes increasing rapidly while BD volumes remain quite constant. It is expected that soon (if it hasn't happened already), RD will exceed BD throughout the entire country.
Kaffka	Page 7.2: Key Findings: Lines 21-22: No quantitative estimate of the fraction of biodiesel and land attributable to the RFS program is made. Lines 24-28: The RFS was likely binding for biodiesel and renewable diesel in 2010-2019. Does this include rapid increases in Renewable Diesel (RD) demand in CA? Lines 29-3333: Fed and state policies have been more important for RD and BD production than for corn ethanol. There is a complex policy environment supporting or discouraging biodiesel and RD production and use. See tables 7.1 and 7.2 (pages 7-6, 7-7) and Fig 7.2 (page 7-6). Generally helpful discussion of complex policy issues and international trade (see Page 7-14, Figure 7.8). BD and RD are mentioned together in the chapter, but the effects of policy on RD consumption in particular are strongly affected by the LCFS, but somehow lumped into the federal RFS program. This suggests that the RFS program is given too much credit for stimulating overall RD + BD demand. Page 7-18, lines 406-411: EPA uses awkward units for land conversion. They should be converted to simpler numbers to avoid confusion. Ex: For "forest" land 0.05 M acres = 50K acres. Lines 421-423: <i>..."These relationships are complicated by the fact that historically most of the value of a bushel of soybeans has come from the soybean meal."</i> ... This is true. The demand for protein meal drives most production of soy. The oil can be seen as a byproduct of the

protein market. And soy oil tends to be surplus or trade at low values in the US market. So biodiesel use is a market clearing mechanism.

Whole bean exports are also a factor in land use.

The codependency of soy and corn production is given little emphasis. So too, the rapid increase in whole exports to Asia (China). See notes and references included below.

Page 7-20. Palm oil from legacy facilities is discussed. Indirect effects leading to palm oil substitution in animal feeds and oleochemicals are mentioned.

Page 7-21: Effects of state programs

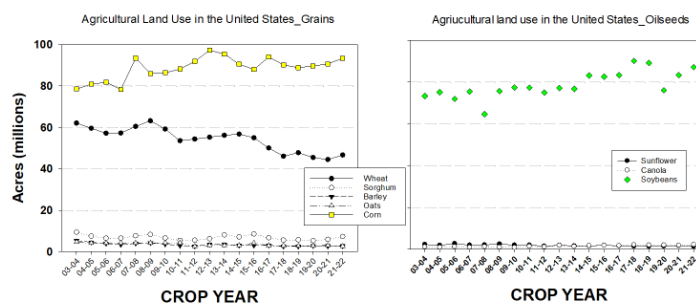
Lines 514-516: RFS increased BD use beyond amounts needed to meet state mandates, but state mandates and the LCFS amounted to more than half the uses by about 2016. (fig. 7.11).

Line 523-525: Because EPA cannot quantify the RFS contribution to total BD/RD production and use, the effects of (crop) production are not included in assessing the environmental effects of the RFS. This could have the effect of attributing to corn ethanol, a larger set of effects than otherwise, given the close association of corn and soybean in crop rotations throughout the US.

See page 7-26, lines 650-652:... *Soybeans are grown for a variety of markets as well as for soil fertility and pesticidal reasons as a rotational crop. Hence, conclusions about potential mechanisms connecting soybean-related land cover and land management changes to the RFS Program are not made in the RtC3.*

Page 7-23, lines 548-551: mentions policy uncertainty and its significant role in affecting investment decisions. This is extremely important and the primary effect of the RFS in all likelihood.

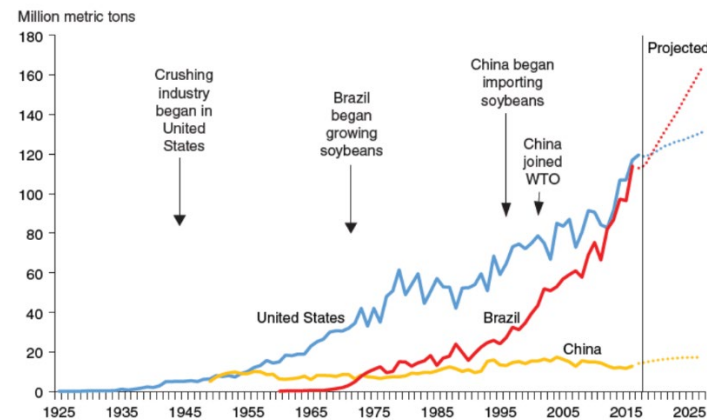
Increased demand and production of soybeans compared to corn



Source: USDA_NASS

Production of soybeans has increased by slightly more acres than the production of corn over the RFS period, from less than 80 M ac early in the period discussed to approximately 90 M ac currently. There are a number of complex and interacting factors contributing to this increase. Increased demand for soybeans has affected land use, perhaps most importantly in the prairie regions where annual crop production has expanded in some areas.

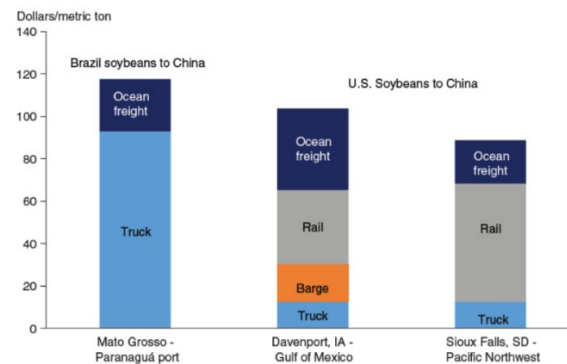
Figure 13
Soybean production, U.S., Brazil, and China, 1925–2028



Source: Data compiled from USDA National Agricultural Statistics Service, China National Bureau of Statistics, UN Food and Agriculture Organization, and USDA (2019).

Gale et al, 2019, state: *...“U.S. soybean production expanded through growth in yield and by shifting land from other crops such as corn and wheat to soybeans. U.S. harvested soybean area rose from under 23 million hectares in 1990 to nearly 30 million hectares in 2000 as changes in U.S. domestic support programs gave U.S. agriculture the flexibility to respond to China’s demand for soybeans. Moreover, new higher yielding soybean cultivars adapted to a short growing season contributed to a northward expansion of soybean planting into traditional wheat-growing regions in the Northern Plains²⁶. Cost savings from the herbicide-tolerant varieties facilitated the expansion of soybean acreage in all regions.”* (Page 22).

Figure 18
Soybean transportation costs to China, selected locations, 2017



Note: Transportation costs to Shanghai, China, are shown.
Source: Data taken from Salin (2018).

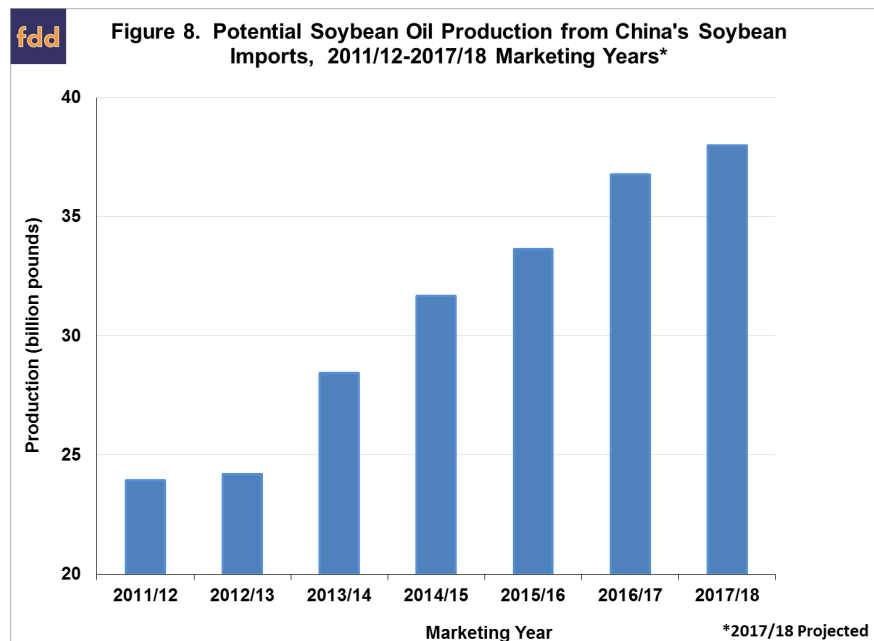
²⁶ Underline added. Gale, F., Valdes, C., and Ash, M., 2019. Interdependence of China, United States and Brazil in Soybean Trade. USDA-ERS.

There were freight rail advantages for soybean producers in the prairie states to markets in China and elsewhere in Asia.

Gale et al (2019) report: *“Figure 18 summarizes transportation costs from production regions to ports in Brazil and the United States. During 2017, hauling soybeans by truck from northern Mato Grosso to the Brazilian port of Paranaguá cost \$93 per metric ton. Transporting soybeans from Davenport, Iowa, to the Gulf of Mexico by truck, rail, and barge cost \$65 per metric ton, \$28 less than the internal transport costs in Brazil. Shipping soybeans by truck and rail from Sioux City, South Dakota, to the U.S. Pacific Northwest cost \$68 per metric ton. That year, ocean freight to China from Pacific Northwest ports was less than freight from the Gulf of Mexico, prompting increases in shipments through Pacific Northwest ports.”* Page 31

The effects of global markets are complex and sometimes counter-intuitive. China’s greatly increased import of soybeans and soybean meal during the period examined for the RtC3 affected US soybean markets and prices. This is discussed by Scott Irwin of the University of Illinois:

“Traditionally, meal used in animal feed rations has been by far and away the most important source of the value of a bushel of soybeans. As almost everyone knows there has been an enormous boom in soybean exports to China to provide soybean meal in feed rations for its burgeoning livestock herds (mainly pork and chickens). What is less appreciated is the supply of soybean oil that has been produced as part of the “soybean meal” boom in China. Figure 8 illustrates the potential scale of the increase in global soybean oil supply from this boom.” Page 7²⁷

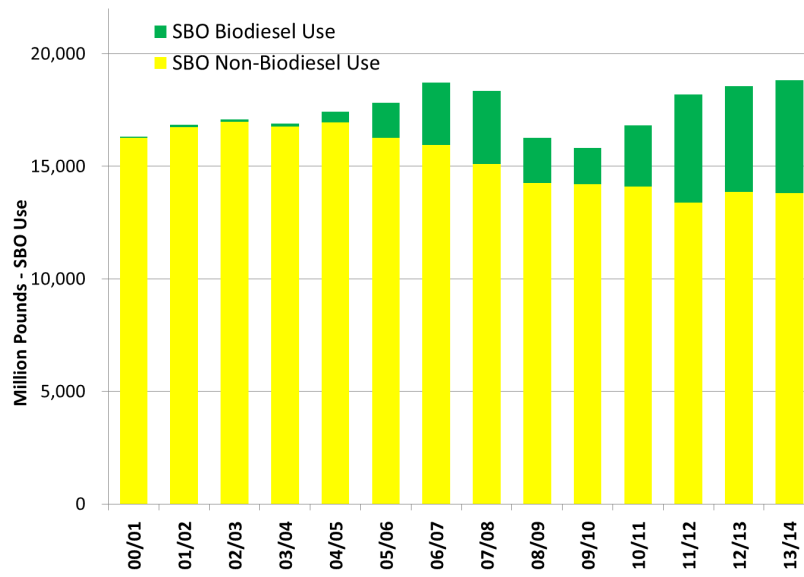


²⁷ Irwin and Good, 2017. <https://farmdocdaily.illinois.edu/2017/09/relationship-between-biodiesel-soybean-oil-prices.html>

	Also: <i><u>“Biodiesel production and consumption in the U.S. boomed in recent years due to rising RFS mandate requirements. In a result that many will find quite surprising, <u>we find no evidence that the boom in biodiesel led to a boom in soybean oil prices, the main biodiesel feedstock.</u> For example, total biomass-based diesel supply in the U.S. (biodiesel + renewable diesel) jumped from 1 billion gallons in 2011 to around 2.5 billion gallons in 2017, a gain of about 150 percent, while at the same time soybean oil prices declined from around \$0.50 to \$0.30 per pound, a drop of almost 40 percent.</u></i> <i><u>The reason for this surprising state of affairs is China’s soybean import boom, which paralleled the U.S. biodiesel boom. Since soybean oil is a joint product that is produced in a fixed proportion when soybeans are crushed, China’s “soybean meal” boom necessarily also produced a huge quantity of soybean oil. In essence, China’s soybean import boom has been so large that the resulting increase in global soybean oil supplies has allowed the U.S. boom in biodiesel production to take place without causing a corresponding boom in soybean oil prices. It is important to recognize this does not necessarily mean that the boom in biodiesel production had zero impact on soybean oil prices. The RFS-driven boom in biodiesel production in all likelihood played a role in preventing the decline in soybean oil prices during recent years from being even more severe.”</u></i> Page 7²⁸ Soybean oil apparently is not the primary driver for soybean production in most circumstances. Oil content is less than 20% of seed biomass. Protein makes up much of the rest and soybean meal is an important protein sources for livestock production worldwide, especially for poultry and swine production. Oil has been a market drag or surplus commodity in the US and elsewhere at times, so use for biodiesel helps clear markets and sustain prices. Soy oil, like cornstarch, became linked to energy markets on an energy basis. Informa Economics (2015), discussed this issue in a report for the National Biodiesel Board²⁹: <i>“In the early 2000’s, soybean oil was viewed as a “drag” to the soybean complex. In the U.S. market, supply chased demand and in fact, soybean crushers and soybean oil distributors stored soybean oil for longer periods until there was sufficient export demand to market soybean oil.... The so called “drag” was significantly reduced by the emergence of biodiesel, particularly after 2006, when soybean oil use for fuel increased to meaningful volumes the market structure for soybean oil shifted from oversupply to potentially undersupplied” ...</i> page 11.
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²⁸ Ibid. Underline added.

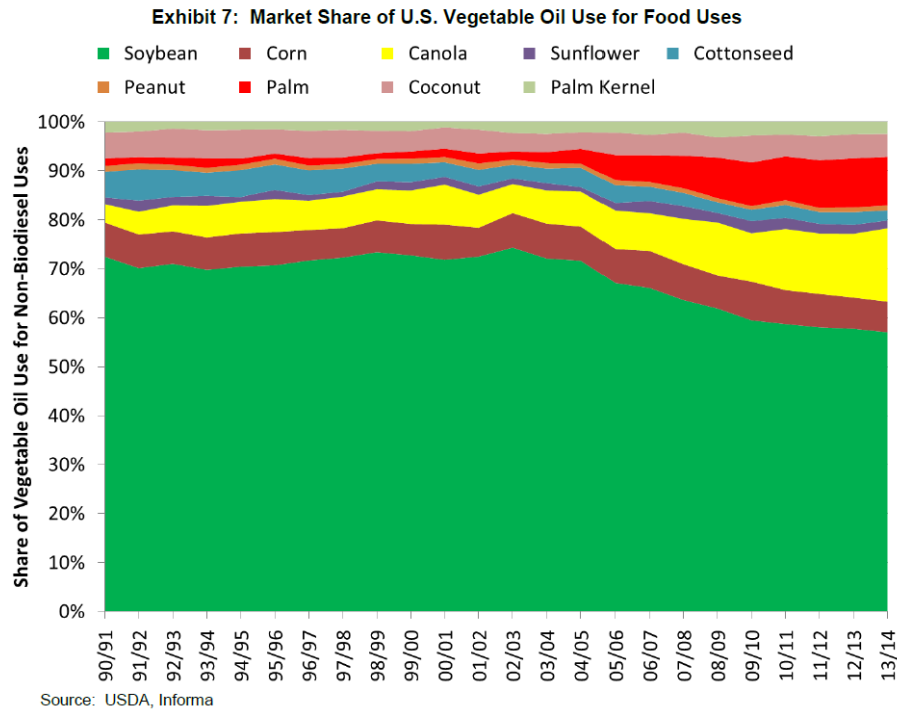
²⁹ Informa economics.-2015. Impact of the U.S. Biodiesel Industry on the U.S. Soybean Complex and Livestock Sector; Informa economics; https://www.biodiesel.org/docs/default-source/fact-sheets/impact-of-the-u-s-biodiesel-industry-on-the-livestock-sector.pdf?sfvrsn=5102c0b6_2



Other factors contributed to soy oil surpluses:

*"In July, 2003, the U.S. Food and Drug Administration (FDA) started to regulate the labeling of trans-fats for foods sold in the U.S. In January of 2008, such labeling became mandatory for all vegetable oil sold at retail outlets. While food service institutions are not required to label trans-fats, many high-profile food service chains such as McDonalds started to shift away from trans-fats as consumers learned about the health risks associated with the consumption of trans-fats oils. The result was a very rapid transition away from trans-fats vegetable oils (mainly hydrogenated soybean oil in North America) and towards more stable (i.e., stable for frying purposes) oils such as palm and towards vegetable oils perceived as healthier than trans-fat such as rapeseed/canola oil or sunflower oil. For perspective, U.S. soybean oil consumption for non-biodiesel uses started to decline after 2005 and since then domestic use for non-biodiesel has declined 4.0 billion pounds from 2004/2005 to 2013/2014. The decline is a consequence of the substitution of soybean oil mainly for palm and canola."*³⁰

³⁰ Ibid. underline added.

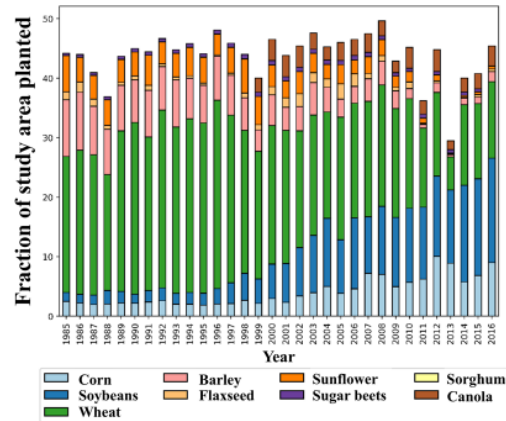


In effect, soybean exports and soy oil use for biodiesel and renewable diesel have sustained demand and to some degree soybean oil prices. An additional effect is that increased soy production has lowered the price of soybean meal domestically, benefitting poultry and swine producers and likely lowering prices to consumers.

With respect to estimates made for environmental effects from annual cropping, corn production is considered the larger source of emissions, compared to soybeans. So the relative proportion of corn to soybeans is important, as well as the potential for local effects on sensitive environments or processes.

The amount of corn grown and the relative importance of corn vs soybean production options in driving planting decisions in the PPR may be overestimated.

For example, in study cited in Chapter 14 by Krapu et al., (2018), the cropped area in the North Dakota PPR was reported from 1985 to 2016. In this figure, cropland acres seem to remain stable if not decline somewhat by 2016. The crop increasing the most is soybeans while wheat and other oilseeds also decline.



Krapu et al., 2018. Fig. 3.. Planted acres. Soybeans increased more than corn while wheat production declined most. particularly. Total acres appear to have declined in the North Dakota prairie pothole region. Less than 50 % of the area studied was planted.

Land use in some of this region was also estimated by the NRCS (2022). In this large transition zone along the corn-soy belt border in ND (and part of SD), soybean production is reported to be dominant. There are also significant areas devoted to Grassland Pasture and to herbaceous wetland and open water, and to fallow cropland. Changes in climate appear to be a strong influence on evolving land use.³¹ This emphasizes the difficulty of accurately attributing environmental effects to land use in this evolving landscape region.

55B—Central Black Glaciated Plains



Figure 55B-1: Location of MLRA 55B, which covers 3,686,700 hectares (9,109,900 acres), within Region F.

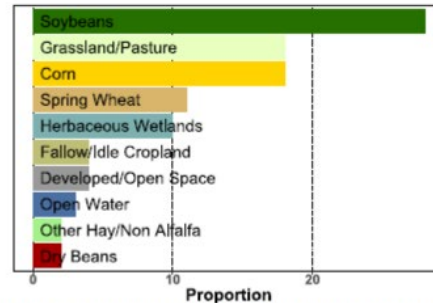


Figure 55B-2: Relative proportions (percentages) of land use in MLRA 55B.

Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin; USDA-NRCS, 2022; Agriculture Handbook no 296

³¹ "... Using maize as a benchmark, we estimate that if warming continues unabated through the first half of the 21st century, the best climatic conditions for rainfed maize and soybean production may shift from Iowa and Illinois to Minnesota and the Dakotas with possible modulation by soil productivity." Hoffman, Kemanian, and Forest. 2020. Env. Research Letters 15(9). The response of maize, sorghum and soybean yield to growing phase climate revealed with machine learning. DOI 10.1088/1748-9326/ab7b22

Kurkalova	1a. Chapter 7 material is clearly organized and presented, and appears technically accurate. 1b. The most important data and relevant literature is cited and appropriately interpreted. The chapter convincingly explains that the RFS2 has been an important driver of the production and consumption of biodiesel and renewable diesel. Yet given current state of knowledge, the effect of the RFS Program on biodiesel and renewable diesel production and consumption could not be quantitatively separated from the effects of multiple other factors. The other factors, including the Bioenergy Program, the BTC, other Federal biodiesel programs, macroeconomic factors, multiple state mandates, and trade policies are clearly described and referenced. 1c. Given the relative scarcity of the studies on the effect of the RFS program on biodiesel and associated feedstocks, the selection of the three approaches (comparison of mandates and consumption, RIN prices, and peer-reviewed literature) to investigate attribution is justified. 1d. The conclusions and recommendations described in the chapter synthesis section are supported by the material presented. For one suggestion to add to section 7.5.3 (Recommendations) please see the comment in section e) below. 1e. The major uncertainties and limitations associated with the chapter's conclusions are clearly identified. One suggestion concerns two interrelated bullets that emphasize the need for studies that examine the changes in soybean acreage: third bullet of section 7.5.2 (Uncertainties and Limitations) and first bullet of section 7.5.3 (Recommendations). I suggest adding an emphasis on location-specific modeling of soybean acreage because in addition to soil and climatic conditions, management practices differ between production regions. For example, report already mentions double cropping (section 3.2.1.2). Nation-wide, the total area of double cropping is larger than that with cover crops (Borchers et al., 2014), the conservation practice that is emphasized throughout the report. In 1999-2012, on average, 83% of double cropped acres included soybeans as a spring crop in the US region that leads the nation in total double-cropping acreage, the Southeastern US (Borchers et al., 2014). Incidentally, during the years considered in the report, some Southeastern areas (e.g., the lower portion of the Mississippi river basin) have experienced a growth in soybean acreage that is similar to that in the Midwest on a percent basis (Figure 5.6 of Chapter 5). An implication of these observations is that economic modeling of soybean acreage in the Southeast might need to include explanatory variables (e.g., winter crop prices) that are not as important for understanding soybean acreage in the Midwest, in the majority of which the growing season is too short to practice double cropping. Similarly, the prevalence of irrigation differs across space, with the practice being more important in NE, AR, MS, and MO than in other soybean production regions (Figure 11.4, panel (d); Figure 11.12, panel (b); and Figure 11.13).
Landis	1a. Overall, I have a very favorable impression of the clarity and technical accuracy of the materials presented in this chapter. I am less familiar with the factors affecting the

	soybean and biodiesel market than I am with corn. The authors have done a very good job of explaining the background behind each concept/factor covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results which are noted to be much more limited than for corn. 1b. I am not aware of any recent data or relevant publications that have not been cited. 1c. I have limited knowledge of economic and technoeconomic modeling and virtually no familiarity with partial equilibrium modeling, therefore I am not able to comment on selection or use of the model or their accuracy. I did review the modeling explanation and results and I do not have reason to doubt the conclusions or resulting synthesis. 1d. Overall, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. However, like Chapter 6, I feel that insufficient attention has been paid to the concept of “<i>market certainty</i>” described in <i>Chapter 6 lines 917- 934 6.3.7</i>. I found no mention of the concept of “<i>market certainty</i>” in Chapter 7 and yet like corn, as biorefineries (and farmers) come to understand the increased governmental commitment to biodiesel represented in RFS1 and RFS2 it is logical to assume that they feel more confident in making investments in the production and refining of soybean which would have implications for expanding soybean acreage. Accounting for market effects would likely increase the estimates of RFS attributable biofuel production with cascading implications for the LCLM analysis and environmental impacts. 1e. For the most part, the chapter does a good job identifying uncertainties and limitations and indeed concludes that insufficient data are available to with its conclusions. <u>I recommend adding a section on the potential effects of “<i>market certainty</i>” as was done in Chapter 6 for corn would be appropriate for this chapter as well.</u>
Smith	1a. The chapter is clearly written and appears to be accurate in what it presents. 1b. I agree with the authors that there is relatively little literature on biodiesel and renewable diesel, especially relative to ethanol. I also agree that the economics are simpler for biodiesel than for ethanol. There has (for the most part) not been a blend wall constraint and RIN prices have been substantial throughout the program. It seems reasonable to assume that most of the biomass-based diesel consumption above state mandates is caused by the RFS. The potential environmental effects of the biomass-based diesel component of the RFS vary widely by feedstock, e.g., waste oils, vs soybean oil vs palm oil. I agree with the authors that more research is needed to model the effects, especially given the likely large increases in renewable diesel in the coming years. 1c. I am unsure why Chapter 4 includes sections on impacts from the RFS program when the point of Chapters 6 and 7 is to explicate these impacts. Why not combine them? 1d. Yes.

	1e. Likely future effects should address renewable diesel, which is poised to grow massively, in part due to state programs such as California's LCFS and in part due to federal incentives through the RFS and the tax credit.
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3.3 Part 3: Chapters 8 TO 16: Environmental and Resource Conservation Issues

3.3.1 Question 2: In part 3, Chapters 8 through 16 characterizes scientific evidence on potential impacts of biofuel production on air quality, soil quality, water quality, water use and availability, terrestrial ecosystem health and biodiversity, aquatic ecosystem health and biodiversity, wetland ecosystem health and biodiversity, invasive or noxious plant species, and international effects, respectively. For each chapter you review:

- a) Please describe your overall impressions of the clarity and technical accuracy of the material presented in the chapter.
- b) Are the most important data and recent relevant literature cited and appropriately interpreted? Have any important studies that would affect the conclusions been omitted? Please identify any data, literature, or studies you think should be added and describe why you recommend they be included.
- c) Please comment on the selection and use of models where applicable to a given chapter (e.g., GREET, BEIOM, EPIC, SWAT), the accuracy of analyses, and accuracy of inferences.
- d) Are the conclusions and recommendations described in the "Chapter Synthesis" section supported by the material presented? Please describe any suggestions for improvement.
- e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.
- f) Are the graphics, glossary, and other supporting material useful in clarifying and supporting the discussion and conclusions in the chapter (and supporting Appendices, if any)? Please describe any suggestions for improvement.

3.3.1.1 Chapter 8: Air Quality

Reviewer	Comments
Davis	2a. Generally speaking, this chapter is thorough and accurate for the information it provides. The comparisons between fossil fuels and biofuels may be a bit misleading because the embedded emissions in some of the biofuel systems are from fossil fuel use. This is briefly mentioned, but it would be most clear if the fossil fuel emissions could be partitioned within the production chain to determine which emissions are actually caused by biofuels. Also, the system boundary that

	was used to define pollutants from gasoline should be identified so readers can evaluate the suitability of the comparison. 2b. Advanced biofuels, including biogas and cellulosic biofuels, are underrepresented. Even though they are “not widely used,” as the report states, they are very relevant to the RFS and potential future scenarios for air quality. Biogas has been growing in use, and would have lower PM_{2.5}, for example. Cellulosic feedstocks can be associated with lower NH₃ emissions. Note: despite some recent analysis that suggests application methods of fertilizer for perennial crops poses a risk, there are also instances of perennial grasses grown WITHOUT fertilizer that pose much less risk than annual crops with high fertilizer requirements. Section 8.3.5 addresses advanced systems very briefly but it is underdeveloped based on the amount of research and early stage commercial development that has been accomplished. The section identifies the potential for maturing the industry, with improvements in air quality impacts, and addresses cellulosic and herbaceous feedstock processing improvements without specifics. This evidence should be analyzed more rigorously. It is mentioned again on page 8-50, indicating the lower emissions from cellulosic sources without in depth analysis. What about corn stover that is being used at commercial scale for cellulosic biofuel? Smaller amounts of others are being used also. 2c. The models are reasonable selections. The analysis would benefit from contextualizing the emissions numbers, making a comparison with thresholds for criteria pollutants. What risk do these emissions values really pose? The system boundary that was used to define pollutants from gasoline should be identified so readers can evaluate the suitability of the comparison. 2d. The summary leads the reader to perceive a large increase in PM_{2.5} from ethanol relative to conventional fuels; after reading the chapter, that seems overstated. Again, partitioning the emissions from coal that are embedded with biofuel emissions and contextualizing with thresholds for criteria pollutants would help. For example, at the top of page 8-51, instead of stating that emissions are a complex issue, the text could indicate that emissions may decline with a declining reliance on fossil fuels in the production process. 2e. The uncertainty around the impact of cellulosic sources on emissions is stated as though the emissions are expected to be “very large,” but this was not clearly addressed in the chapter and evidence for the opposite was suggested. 2f. The left panels in figure 8.15 are unnecessary, and potentially confusing, without further justification/explanation. The figure legend could be expanded to describe this more clearly, and text added to make the distinction between the total emissions and the emissions per unit energy. I suggest adding the threshold for acceptable levels of criteria pollutants to figures that show the difference between fuel types.
Hill	2a. The chapter is mostly clearly written but is deficient in its technical accuracy.

	2b. Lines 45–46 are not supported, nor are lines 56–57. On the first point, air pollution travels, and in the Midwest there are large population centers directly downwind of large corn producing areas (e.g., Chicago), where marginal effects of emissions are high (see Goodkind et al., 2019, https://doi.org/10.1073/pnas.1816102116). On the second point, emissions from biofuel production have been increasing overall and as a result of increased intensification from crop production. Section 8.3.1.1.1 notes that Hill et al. (2019) “did not separate the emissions impact by end use of the maize produced.” In a follow-up study (Domingo et al., 2021, https://doi.org/10.1073/pnas.2013637118), they did just that, concluding that approximately 1,200 deaths per year in the US are attributable to corn production for ethanol (see Fig. 1 “Product” column of that study) from primary and secondary PM_{2.5}. Deaths from soybean production for biofuel are also shown (<100). Section 8.3.4, as it is based on Ch 6, is unsupported. 2c. The GREET model on its own is insufficient for estimating the inventory of air quality-related pollutants relevant to changes in air quality from RFS/RFS2. Notably, GREET does not track ammonia (NH₃), a major contributor to atmospheric PM_{2.5}, especially as relates to corn/ethanol/agricultural production (See Hill et al. and Domingo et al.). Tessum et al. (2012) modified GREET to include NH₃, and this version is available for public use (https://pubs.acs.org/doi/10.1021/es3010514). Also, GREET provides no geographic resolution other than a coarsely-defined (and opaque to the user) urban/rural fraction. Tessum et al. (2012 and 2014, https://www.pnas.org/doi/10.1073/pnas.1406853111) and Hill et al. (2019) modified GREET to include spatial data from US EPA’s National Emissions Inventory, and in the latter paper estimated PM_{2.5} damages from ethanol production and combustion. 2d. Mostly they are, absent those related to the points above. 2e. Yes, largely, but the limitations I described above should be noted. 2f. Yes
Hoekman	2a. My overall impression is that the material presented in Chapter 8 is clear and accurate. For the most part, the additional information obtained since the RtC2 report supports and strengthens the earlier findings. This chapter emphasizes the challenges in reaching firm conclusions regarding the impacts of biofuel production and use upon air quality. Most effects are small and variable, and depend upon the particular metric or outcome being considered. For example, the overall ozone impacts are probably negative, while the overall CO impacts are positive. However, there are exceptions to these, and almost all other conclusions. A factor that is clear to those involved in such issues, but may not be obvious to the general reader, is the fact that motor vehicle pollutant emissions have been largely controlled and are much lower today than in the past. On a fleet-wide basis, most pollutant species are emitted today at 1% or less of their values in the

	1960s. Because these emissions are becoming vanishingly small, any impacts caused by use of biofuels are now mostly lost in the noise. This point should be highlighted in the report. Additionally, this is another reason to include assessments of GHG emissions in the RtC3 report, because GHG effects are much larger and easier to quantify than are the fuel effects on criteria emissions and ambient pollutant levels. The issue of Mobile Source Air Toxics (MSAT) should be discussed in the RtC3 report. MSAT emissions include benzene, 1,3-butadiene, formaldehyde, acetaldehyde, and other designated species. As there has been considerable interest and debate regarding how addition of ethanol under the RFS Program affects MSAT emissions, further analysis of this should be included. Related to this are considerations of oxygenated gasoline production via “splash blending” and “match blending,” and how this could affect the reliability of vehicle emissions models (such as MOVES) that are used to estimate fleet-wide emission. These issues should be at least briefly discussed in this chapter. 2b. To my knowledge, the most important data and relevant literature are cited and interpreted appropriately in Chapter 8. However, as in Chapter 7, I believe that any data that distinguishes between biodiesel and renewable diesel should be noted. 2c. The models used in EPA’s “anti-backsliding study” (described in Section 8.3.2.2; page 8-25ff) are not identified. Although more detail is provided in an appendix, a brief description of these models should also be provided in this chapter. REET is a well-established LCA model that is frequently used to assess the air quality impacts of biofuels – especially GHG impacts, which surprisingly are excluded from this report. Use of REET to estimate VOC, SO_x, PM_{2.5}, PM₁₀, CO and NO_x is appropriate. It is also good to use BEIOM, which is a very different type of LCA model and determines air quality impact results on an entirely different basis. Of the four metrics reported from BEIOM (SFP, ACP, PEP, and ODP), only two can be compared to REET results [smog forming potential (SFP) and PM exposure potential (PEP)]. It is encouraging that for these two metrics, there is some degree of agreement between BEIOM and REET, with both models showing increased environmental impacts from use of biofuels as opposed to conventional fuels. 2d. Yes, the conclusions and recommendations in the Chapter 8 Synthesis section are supported by the material presented. It might also be noted that most of the conclusions regarding biofuels’ impacts on specific air pollutants are qualitative only. A useful recommendation would be to develop more quantitative estimates for these impacts. 2e. Yes, the most important uncertainties and limitations regarding current and near-future impacts of biofuels upon air quality are identified. One point that could be emphasized more strongly is that the overall effects of biofuels on air pollutants (O₃, CO, VOC, PM, etc.) are very small, as indicated by modeling results, meaning that it is difficult (or impossible) to observe any real-world effects. Potential impacts further into the future (10-15 years out) have additional, unaddressed
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	uncertainty resulting from unknown market, economic, policy, and regulatory changes. 2f. Most of the graphics, glossary, and other supporting material is useful, though a couple of improvements could be made: - Figure 8.3 (page 8-9) shows locations of ethanol and biodiesel plants, but not renewable diesel plants. [Similarly, RD is not included in the emissions data of Table 8.1 (page 8-10).] If such information is available, RD plant locations and production emissions should be included. - Figure 8.9 is very useful in displaying the modeling results from EPA's "anti-backsliding study." However, it would be helpful to the reader if the pollutants being modeled were identified in each panel, not only in the table's title. Also, it would be useful if a brief explanation were provided in the text for certain anomalies that appear in this figure, such as the particularly high NO₂ increases and 1,3-butadiene decreases that appear in certain metropolitan areas. General Comments In discussing ethanol end use, it is mentioned that "However, ethanol has a unique effect on permeation emissions, and this effect is accounted for in the MOVES model." (page 8-12, lines 322-324) A brief description of this unique effect should be provided. In the discussion of PMI effect on page 8-13, it would be useful to include the formula used to compute PMI values. It should also be explained why the fuel terms appearing in this formula would be expected to relate to formation of PM emissions. The title of Table 8.4 (page 8-16) mentions that these CRC E94-3 emissions results were obtained using the LA92 chassis dynamometer test cycle. Although a similar statement is not given in the title of Table 8.3 (page 8-15), weren't the CRC E94-2 emissions results also obtained using the LA92 test cycle? In discussing the criteria pollutant emissions associated with the manufacturing of biodiesel (page 8-19), it is mentioned that mechanical expelling results in higher emissions than the other processes used to crush soybeans and obtain the oil. Why is this the case? Is it simply a consequence of higher emissions from the primary energy sources used to drive the expelling process, or are there other factors involved? In reviewing Chapter 8, a few typographical and other minor errors were detected. These are identified below: - Page 8-13, line 353: delete the word "respectively" - Page 8-13, footnote No. 11: "$(R+M)/2$" - Page 8-19, line 434: change "form" to "from" - Page 8-20, the title of Table 8.5: should state that the values shown here are in units of tonnes/year
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	• Page 8-36, line 740: insert period after “wet mills.” • Page 8-43, lines 872-873: change “Table 8.7” to “Table 8.8” • Page 8-47, Figure 8.14: in the bar labels, change “gas” to “gasoline”
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3.3.1.2 Chapter 9: Soil Quality

Reviewer	Comments
Davis	2a. The information in the chapter is mostly accurate, but certain topics deserve greater detail. Corn stover is mentioned for the first time in the summary of the prior triennial report (RtC2), on line 142. This serves as a reminder that this was something missing in the prior chapters, but also is relevant to the discussion about residues left from different cropping systems. Line 180 implies that corn has greater residue that contributes to soil inputs, drawing specific contrast against cotton and wheat. This is an oversimplification because harvest of corn stover would reduce residue inputs, and wheat can be grown in rotation systems that include winter crop cover – a management that would increase residue and soil protection. Corn stover is not addressed again until the “Horizon scanning” section (9.6), but this is something that is already commercially deployed and not merely on the horizon. Line 273-275 states that N losses “can be” greater in soybean crops than corn crops. This statement is technically true, but I do not think there is evidence for this being the predominant observation. It is a potentially misleading statement. Line 368 defines a value without units (3.2). The comparison with soil impacts from petroleum (line 595-598) leaves a lot of questions that shed doubt on the conclusion the petroleum impacts on soil are lower. It is stated as the impacts “may be lower,” but gives the reader an impression that the impacts are lower. If it may be, but is highly uncertain, the uncertainty should be stated. There are a few typos throughout the chapter. A thorough copy edit would be useful. 2b. As mentioned above, greater analysis of corn stover is warranted. Also, missing entirely from this chapter is an analysis of biogas systems that produce a lot of material that is applied to soils. This is a growing sector of biofuels and has consequences for soils in several ways. Sewage sludge from waste water treatment plants (WWTPs) is often land applied, with direct soil impacts, and biogas plants are being constructed at WWTPs to make advanced biofuels (e.g. RNG) – thereby diverting sewage sludge, but producing a different nutrient-rich digestate product that can be applied on soils. 2c. This model selection is justified, but I wonder why DayCent model results that are reported as part of the Inventory of U.S. Greenhouse Gas Emissions and Sinks (EPA

	430-R-21-005) were not considered in this analysis. DayCent is used to estimate changes in soil carbon and nutrient leaching. 2d. Consider adding more detail about the need to discern corn and soybean changes that are specifically resulting from biofuels. This could be done with survey instruments or reviewing contract terms and would be reported in aggregate to maintain privacy. Recommendations should also address biogas. 2e. Uncertainty around the comparison with petroleum-related impacts to soil should be addressed. 2f. Yes. Additional detail could be added to graphics to address the comments in above answers to questions a through e
Engel	2a. The material presented are clear and technically accurate. The results were largely as expected and supporting analyses and data are presented that support the findings. The focus of conservation practices on conservation tillage/no-till and cover crops given focus on soil quality is appropriate – other conservation practices are widely used but these most important for soil health. 2b. The data presented are useful in presenting the findings and seem to be the most important and relevant data. The literature cited seem to be most relevant and important for analyses conducted. In this and other chapters, when modeling and analyses are conducted that include plant growth and related plant characteristics, the models used (e.g. EPIC in this chapter and SWAT in other chapters) are using very old plant growth and plant characteristics that are no longer accurate and will not be accurate in the future given the rapid changes being made in plant genetics. A recent paper by Ren et al. (2022) provides some initial analyses of these potential assumptions when conducting hydrologic/water quality modeling. In future work, consideration of the rapid changes in plant growth parameters that have occurred and are continuing on the analyses for this report should be considered. Ren, D., B. Engel, M.R. Tuinstra. 2022. Crop improvement influences on water quantity and quality processes in an agricultural watershed. Water Research, Volume 217, 2022, 118353, https://doi.org/10.1016/j.watres.2022.118353. Clarification: Many SWAT studies which care about crop growth also calibrated these crop parameters (Guo et al., 2018; Cibir et al., 2016; Wang et al., 2017; Nair et al., 2011). Cibir et al. (2016) indicated that "The SWAT model crop growth simulation is not capable of tracking the genetic improvements of crops in long term simulations". Researchers who care about crop growth often calibrate crop parameters, and thus they don't worry about the accuracy of the original database. Researchers who do not care about crop growth usually will directly use the default database, and they won't question the database.

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The table below has crop growth model parameters from SWAT and corresponding papers in sections organized by parameters that provide details on those parameters. This is likely more detail than desired and above may be sufficient.

Characters	Parameters	Description
Growing degree days (GDD)	PHU	Total number of heat units needed to bring plant to maturity (°C)
	T_BASE	Minimum (base) temperature for plant growth (°C)
	T_OPT	Optimal temperature for plant growth (°C)
Leaf area index (LAI)	BLAI	Maximum potential leaf area index (*)
	FRGRW1	Fraction of total potential heat units corresponding to the 1st point on the optimal leaf area development curve (*)
	LAIMX1	Fraction of the maximum leaf area index corresponding to the 1st point on the optimal leaf area development curve (*)
	FRGRW2	Fraction of total potential heat units corresponding to the 2nd point on the optimal leaf area development curve (*)
	LAIMX2	Fraction of total potential heat units corresponding to the 2nd point on the optimal leaf area development curve.
	DLAI	Fraction of growing season when leaf area begins to decline (*)
	EXT_COEF	Light extinction coefficient (*)

		Light utilization (LU)	BIO_E	Radiation-use efficiency ((kg/ha)/(MJ/m ²))
		Drought tolerance (DT)	EPCO	Plant uptake compensation factor (*)
			RDMX	Maximum root depth (m)
			GSI	Maximum stomatal conductance (m/s)
			FRGMAX	Fraction of maximum stomatal conductance corresponding to the second point on the stomatal conductance curve (*)
			VPDFR	Vapor pressure deficit corresponding to the second point on the stomatal conductance curve (kPa)
			WAVP	Rate of decline in radiation use efficiency per unit increase in vapor pressure deficit (g/MJ/kPa)
		Nutrient content (NC)	CNYLD	Normal fraction of nitrogen in yield (kg N/kg yield)
			BN1-3 (kg N/kg biomass)	Normal fraction of nitrogen in plant biomass at emergency
				Normal fraction of nitrogen in plant biomass at 50% maturity
				Normal fraction of nitrogen in plant biomass at maturity
			CPYLD	Normal fraction of phosphorus in yield (kg P/kg yield)
			BP1-3 (kg P/kg biomass)	Normal fraction of phosphorus in plant biomass at emergency
				Normal fraction of phosphorus in plant biomass at 50% maturity
				Normal fraction of phosphorus in plant biomass at maturity
		Harvest index (HI)	HVSTI	Harvest index for optimal growing conditions (*)
			WSYF	Lower limit of harvest index (*)

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	is unclear that sufficient location specific data will be available anytime in the foreseeable future to conduct location-specific analyses such as those presented in this report and chapter is highly unlikely. Some progress in improving the site specificity of impacts is possible but may be cost prohibitive and will have great uncertainty given the range of potential mitigation/conservation practices and other factors for which location specific data are unavailable or cost prohibitive. Making it clear that one should not expect these significant challenges to be overcome in the near future (e.g. next report) should be clarified. Changes in plant growth characteristics and other plant attributes is important to soil health but not considered or acknowledged. Acknowledging this is important but efforts to more rigorously consider these changes within the models used and rerunning the models should be delayed until the next report. The inflation reduction act will spend nearly \$18.5 billion on climate smart practices (e.g. conservation practices) that have potential to have profound impacts on soil health. A recommendation that acknowledges this and the need for the work in accounting for these practices should be considered. 2f. In table 9 caption and figures 9.4 and 9.5, clarifying the model used in simulating the results presented in the table and figures would improve ability to quickly understand the table. Including model name in caption would improve this.
Kurkalova	2a. Chapter 9 material is clearly organized and presented, and appears technically accurate. 2b. The most important data and recent literature is cited and appropriately interpreted. I commend the authors for bracketing the estimates of soil erosion, SOC loss, and nutrient losses between the estimates derived under the assumptions on tilled and no-tilled corn-soybeans replacing other land uses (section 9.3.2). Although the recent estimates, appropriately cited in Chapter 3, suggest that the majority of corn and soybean fields are in conservation tillage, the exact spatial location of tillage practices and the continuity of the no-till or conventional tillage is not discernable from the currently available data. The paucity of location-specific data on farming practices is rightfully accentuated in many parts of the report, but that of the year-to-year dynamics of practices - not so. For example, consider a region in which 50% of area was in no-till two years in a row. What does it tell us about continuous use of no-till? Unfortunately, not much because the same 50% area could have been in no-till implying that 50% of area is in the practice continuously. Also, such data are consistent with no area being in no-till continuously if going from one year to another, tillage is flipped between no-till and conventional tillage at every field. And any percentage between zero and 50 could also be consistent with the two yearly observations. Yet the dynamics, such as the continuity of practice use is important because the majority of biophysical processes simulation models, including EPIC used to develop the estimates described in Chapter 9 and SWAT model used in Chapter 10, rely on the estimates derived from long-term agronomic/soil science field

	experiments. In these experiments, the practices in question are in place for multiple, usually 10+ years (e.g., West and Post, 2002; 2002 Zhang et al., 2021). And the relationship between SOC accumulation and time in no-till is not generally linear, i.e., there is no indication that the per-year soil quality effects are constant. Therefore, the per-year soil health effects under a long-term continuous no-till (e.g., 10+ years) are generally not the same as the per-year soil health effects under a short-term continuous no-till of only a couple of years. For that reason, if no-till is assumed for a region, the per-year EPIC-derived estimates apply only under the assumption that farmers use the practice continuously for long duration such as for 10+ years. While EPIC and SWAT estimation routines assume a long-term continuous use of practices (e.g., Zhang et al. (2021) estimates rely on 30-year EPIC runs), the limited evidence reported in the literature suggests that farmers often rotate tillage systems (Claassen et al., 2018; USDA NRCS, 2022; additional reference: Sawadgo and Plastina, 2022). As Table 3.1 in Chapter 3 shows, while 67% of the total acreage used conservation tillage in the CEAP-2 survey, only 33% of the total acreage was continuously in no-till during the four years for which the information was recorded (USDA NRCS, 2022; p. 104). The mismatch of the time frames, e.g., 30 years of no-till in models versus a maximum of four years of actually documented continuous use of no-till, creates a problem with applicability of the EPIC-derived results even for the regions in which the share of no-till use is relatively high in any given year. For that reason, the bracketing of estimates between no-till and conventional tillage used in the report is very appropriate. Because of the importance for evaluation of environmental outcomes of expanding corn and soybean acreage, I recommend adding a greater emphasis on the year-to-year dynamics of no-till and cover crops in the report. Some specific suggestions are listed in my Chapter 3 comments. In this chapter, I suggest: • Chapter 9, section 9.3.4: Line 548: Add the word “continuous” before “conservation tillage”. 2c. The choice of the biophysical simulation model, EPIC, is well justified: the model is appropriate to the task of estimating the “edge-of-field” estimates of soil health measures. EPIC model has been calibrated and validated for a variety of climates, soils, LCLM, and farming practices. For completeness, I would only recommend adding references to the model’s history of development, calibration, and wide capabilities (suggested references: Williams et al., 2008; Izauralde et al., 2006). 2d. The conclusions and recommendations described in the chapter synthesis section are supported by the material presented. Three suggestions for improvement are detailed below. Section 9.7.4 (Research Recommendations): An additional research recommendation is to expand future analyses of the soil quality effects of the increased corn and soybean acreage to two areas outside of the 12-state Midwestern region: the lower portion of the Mississippi river basin and the south-eastern coastal plain. Figures 5.5, 5.6, 5.7, and 5.10 of Chapter 5 collectively show that in these two areas, corn and soybean acreage has increased significantly over
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	the range of the years considered in the RtC3. Although the total area under corn and soybeans in eastern AR, eastern LA, western KY, western TN, and western MS is relatively small when compared to the 12-state region considered throughout Chapter 9, the soil quality implications of the crop switching attributable to the RFS could be relatively high because of the proximity to the river, and the specifics of soil, climate and farming practices (e.g., irrigation, double cropping, and inclusion of fallow in rotations). Additionally, the coastal areas of VA, NC, and SC are prone to flooding from hurricanes, which poses soil quality concerns such as erosion, nutrient runoff, and salt water intrusion (additional reference: Sawyer et al., 2016). It could also be noted that the gap between the assumptions behind EPIC simulations and the observed farmer behavior mentioned in part b) of my comments could be shortened by developing EPIC capabilities to simulate rotational tillage systems such as intermittent/seasonal no-till. With that goal in mind, it could be useful to expand the research that synthesizes the long-term agronomic/soil science field experiments on intermittent rather than continuous use of conservation practices. One known study that provides such synthesis is Conant et al. (2007) (additional reference). Additional syntheses would provide the much-needed information for calibration and validation of the biophysical processes' models under the more realistic assumptions about farmers management choices. A corresponding research recommendation could be added in section 9.7.4. The recommendation about the need for additional research on socioeconomic barriers to greater use of cover crops (third bullet in section 9.7.4) could be supported by the references on the studies that analyze the complementarity between conservation practices. Here complementarity is defined as an increase in the probability of using one practice given the use of the other practice. Lee and McCann (2019), Canales et al. (2020), and Gong et al. (2021) (all additional references) specifically discuss the potential complementarity between cover crops and no-till. 2e. The major uncertainties and limitations associated with the chapter's conclusions are clearly identified. In the last sentence of section 9.7.3 (Uncertainties and limitations), I suggest to mention the need for not just spatially resolved, but time-series data on management practices at a smaller geographic scale. 2f. The graphics, glossary, and other supporting material are useful and appropriate.
Landis	2a. Overall, I have a very favorable impression of the clarity and technical accuracy of the materials presented in this chapter. As an agroecologist who has worked in the biofuel research arena for more than 15 years, I am familiar with many but not all the terms, concepts, and potentially causal factors addressed in this chapter. The authors have done a very good job of explaining the background behind each concept/factor covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results.

	2b. In the current section lines 196 -204 consider: • Schipanski, M.E., Barbercheck, M., Douglas, M.R., Finney, D.M., Haider, K., Kaye, J.P., Kemanian, A.R., Mortensen, D.A., Ryan, M.R., Tooker, J. and White, C., 2014. A framework for evaluating ecosystem services provided by cover crops in agroecosystems. <i>Agricultural Systems</i>, 125, pp.12-22. • Hallama, M., Pekrun, C., Lambers, H. and Kandeler, E., 2019. Hidden miners—the roles of cover crops and soil microorganisms in phosphorus cycling through agroecosystems. <i>Plant and Soil</i>, 434, pp.7-45. • Austin, E.E., Wickings, K., McDaniel, M.D., Robertson, G.P. and Grandy, A.S., 2017. Cover crop root contributions to soil carbon in a no-till corn bioenergy cropping system. <i>Gcb Bioenergy</i>, 9(7), pp.1252-1263. In the current section line 276 -317, note new review of bioenergy crops on soil organisms. • Zahorec, A., Reid, M.L., Tiemann, L.K. and Landis, D.A., 2022. Perennial grass bioenergy cropping systems: Impacts on soil fauna and implications for soil carbon accrual. <i>Gcb Bioenergy</i>, 14(1), pp.4-23. In the current section lines 196 -204 consider: • Adetunji, A.T., Ncube, B., Mulidzi, R. and Lewu, F.B., 2020. Management impact and benefit of cover crops on soil quality: A review. <i>Soil and Tillage Research</i>, 204, p.104717. • Schipanski, M.E., Barbercheck, M., Douglas, M.R., Finney, D.M., Haider, K., Kaye, J.P., Kemanian, A.R., Mortensen, D.A., Ryan, M.R., Tooker, J. and White, C., 2014. A framework for evaluating ecosystem services provided by cover crops in agroecosystems. <i>Agricultural Systems</i>, 125, pp.12-22. • Fox, A.F., Kim, T.N., Bahlai, C.A., Woltz, J.M., Gratton, C. and Landis, D.A., 2016. Cover crops have neutral effects on predator communities and biological control services in annual cellulosic bioenergy cropping systems. <i>Agriculture, Ecosystems & Environment</i>, 232, pp.101-109. 2c. I have had some exposure to EPIC modeling. Specifically, I am aware that in the past it was limited in how it could be used to infer landscape-level effects as the individual management unit outcomes were combined across the area of study and therefore could not account for adjacency effects. I am also generally aware of SWAT's and GREETS capabilities as watershed and life cycle analysis models respectively. I accept that EPIC, SWAT and GREET are standards used in analysis of bioenergy cropping system impacts. I have no prior exposure to BEIOM and cannot comment on its appropriateness. 2d. Overall, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. However, I feel that insufficient attention has been paid to the concepts of “market certainty” and cumulative landscape effects described in my response to Chapters 1-5 above. Accounting for
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	these effects would likely increase the estimates of RFS attributable biofuel production (Chapters 6-7) with cascading implications for the environmental impacts in this chapter. 2e. The chapter generally does a good job identifying uncertainties and limitations associated with its conclusions. 2f. The graphics, glossary and supporting materials are helpful in clarifying and supporting the conclusions of the chapter.
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3.3.1.3 Chapter 10: Water Quality

Reviewer	Comments
Engel	2a. The chapter provides an overview of water quality impacts (primarily nutrients, erosion and pesticides) due to increased corn and soybean production and increases in these crops due to the RFS. The analyses rely on both observed trends in water quality data and modeled results. Some analysis of water quality impacts for biofuels versus petroleum-based fuels is provided. The clarity and technical accuracy of information presented meets expectations. Determining location specific impacts of RFS on water quality is not possible given complexity of the biofuel system, RFS interaction with biofuel system, and limitations in spatial data to support analyses of this type. In line 937, the sentence indicates 250 acres which seems incorrect based on sentences around this. 2b. The chapter includes a reasonable group of literature overviews with citations and reasonable amounts of data. There are numerous water quality modeling and data analyses studies that are not included that tend to be local in nature and that would require significant interpretation to fit within the goals of the chapter. Therefore, such studies are not relevant for this chapter. 2c. The models primarily described and used in the chapter include SWAT, EPIC and APEX. These are appropriate models for the types of analyses presented in the chapter and are commonly used for related research. Based on the information presented, the analyses and accuracy of references are appropriate. As noted in comments for chapter 9, the models used are using crop growth and crop characteristics that are greatly out of date and therefore do not well represent current crop performance which impacts water quality – see Ren et al. (2022). Ren, D., B. Engel, M.R. Tuinstra. 2022. Crop improvement influences on water quantity and quality processes in an agricultural watershed. Water Research, Volume 217, 2022, 118353, https://doi.org/10.1016/j.watres.2022.118353 2d. The conclusions follow from the data and information presented in the chapter. Additional clarity is needed in the conclusions that an increase in pesticide loss or

	nutrient loss due to lands converted to corn or soybeans that are used in biofuels does not necessarily create a significant water quality issue unless thresholds of concern were reached. The data presented in the chapter do not suggest such thresholds were crossed nor even approached. It is possible on local levels this could be the case but data presented in the chapter do not support this. The research recommendations are somewhat broad in some cases. For instance, the recommendation to examine biofuel feedstocks on leaching to groundwater and impacts on drinking water nitrate and pesticides is interesting but no evidence was presented that this likely represents a significant issue. The need for the recommendation that further research on glyphosate is needed because it is commonly found in surface water samples is unclear; finding glyphosate is not surprising given its widespread use and properties but given its level relative to thresholds of concern this is a somewhat surprising recommendation. 2e. The uncertainties and limitations presented are appropriate. The spatial nature of change and impact is important and is captured here. The inflation reduction act will spend nearly \$18.5 billion on climate smart practices (e.g. conservation practices) that have potential to have profound impacts on soil health. A recommendation that acknowledges this and the need for the work in accounting for these practices should be considered. 2f. The figures are helpful in supporting the discussion and conclusions of the chapter.
Hill	2a. Overall, this chapter is clear and technically accurate, except for the analyses based upon the conclusions of Chapter 6. 2b. Section 10.3.3, as it relies on Chapter 6, needs to be revised. 2c. In Line 838, the interpretation of the data from Fig. 10.19b is incorrect, especially over the timeframe of RFS/RFS2. 2d. Yes, mostly, but those based upon Chapter 6 are not supported. 2e. Yes 2f. The print resolution of Fig. 10.14 should be increased for better readability.
Hoekman	2a. Chapter 10 is reasonably clear and accurate in presenting information regarding the potential impacts of biofuels upon water quality. Because these impacts result primarily from growth of corn for production of corn ethanol and soybeans for production of biodiesel (and renewable diesel), it is appropriate to focus this study on water quality effects in regions of the country where these crops are grown. Although not as significant, the issue of indirect water quality impacts should at least be mentioned. These impacts pertain to growth of biofuel feedstocks in other regions of the world. Due to the difficulty in quantitatively attributing a fraction of total biofuel production and use to the RFS Program, it is not possible to reliably attribute a fraction of the water quality impacts to the RFS. This is not a major problem, however, as it is of

	equal or greater interest to understand the impacts of total biofuel usage, rather than the (much smaller) impacts of RFS-attributable biofuels. 2b. To my knowledge, Chapter 10 does cite and appropriately interpret the most recent relevant data. In particular, inclusion of the recent SWAT modeling of the Missouri River Basin (MORB) region is a major improvement over the information used in the prior RtC2 report. 2c. I am not qualified to judge the selection and use of the models used in Chapter 10 to assess the water quality impacts of biofuels. I note, however, that the SWAT model has been widely used and accepted within the modeling community. Application of SWAT to model impacts in the MORB region appears to represent an important advancement in our understanding. 2d. Yes, the conclusions and recommendations in the Chapter 10 Synthesis section are adequately supported by the materials presented. 2e. The most important uncertainties and limitations associated with the conclusions regarding current and near-future impacts of biofuels upon water quality are identified in Chapter 10. One possible additional area of uncertainty/limitation to consider involves indirect water quality effects resulting from production of biofuel feedstocks in other regions of the world. Potential impacts further into the future (10-15 years out) have additional, unaddressed uncertainties resulting from unknown market, economic, policy, and regulatory changes. 2f. For the most part, these materials are very helpful, though a few improvements could be made: • Figures 10.2 and 10.3 appear to be very similar. Only upon close examination of the figure titles can the reader learn that Figure 10.2 refers to pollutant concentrations, while Figure 10.3 refers to pollutant loads. This distinction should be made more clear. • Figure 10.6 could be clarified by indicating (next to each map) that the top map refers to groundwater public water systems and the bottom map refers to surface water public water systems. • Figure 10.10 could be clarified by indicating (next to each map) that the top map refers to the period 2008-2012, while the bottom map refers to the period 2008-2016. • Figure 10.15 shows spatial distribution maps for four pollutants, but includes only three color-coded legends. Is the legend labeled “N reduction” meant to apply to both NO₃ and TN pollutants? • To provide some sense of scale, state borders should be shown in Figure 10.16. General Comments Though very small at present, volumes of advanced biofuels produced by thermochemical conversion of various forms of biomass (forest waste, municipal solid waste, agricultural
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waste, etc.) are likely to increase in the future. In addition, production of RNG from digester-based technologies is becoming common. Can anything be said about potential water quality impacts of such fuels and processes?

Interest is also growing in the process of converting ethanol to jet fuel. Does this introduce any additional water quality concerns? The broader area of sustainable aviation fuels (SAF) should also be discussed briefly. At present, SAF is derived principally from vegetable oils and FOGs. Water quality impacts of SAF have not yet received much attention, but this clearly is an area of growing interest.

Clarification: These comments were meant to highlight the fact that many different feedstocks and conversion processes are now being used to produce small volumes of advanced biofuels. I am not aware of significant existing literature that examines the potential water quality impacts of these processes and fuels. However, I thought it worthwhile to bring these issues to EPA's attention, as they are likely to become more important in the future.

Some information sources to consult for additional insights include the following:

Thermochemically-produced biofuels

- DOE-BETO: [Thermochemical Conversion Processes | Department of Energy](#)
- Fulcrum Bioenergy: [Home | Fulcrum BioEnergy \(fulcrum-bioenergy.com\)](#).
- Enerkem: [Enerkem: From waste to Biofuels and Renewable Chemicals | Disruptive technology](#)
- Sungas Renewables: [Home - SunGas Renewables](#)
- Topsoe: [Renewable energy and CO2 to synthetic diesel and jet fuels | Synthetic fuels | Topsoe](#).
- Infinium: [Company History — Infinium \(infiniumco.com\)](#)

Sustainable Aviation Fuels (SAF)

- CAAFI Program: [CAAFI - Home](#)
- U.S. DOE: [Sustainable Aviation Fuels | Department of Energy](#)
- IATA Program: [IATA - Sustainable Aviation Fuel \(SAF\)](#)
- Honeywell: [What is Sustainable Aviation Fuel? \(honeywell.com\)](#)
- EESI: [An Introduction to Sustainable Aviation Fuels | Article | EESI](#)

Alcohol-to-Jet (ATJ)

- PNNL: [Faster and Cheaper Ethanol-to-Jet-Fuel on the Horizon | PNNL](#)
- TEA analysis: [The Alcohol-to-Jet Conversion Pathway for Drop-In Biofuels: Techno-Economic Evaluation \(bts.gov\)](#).

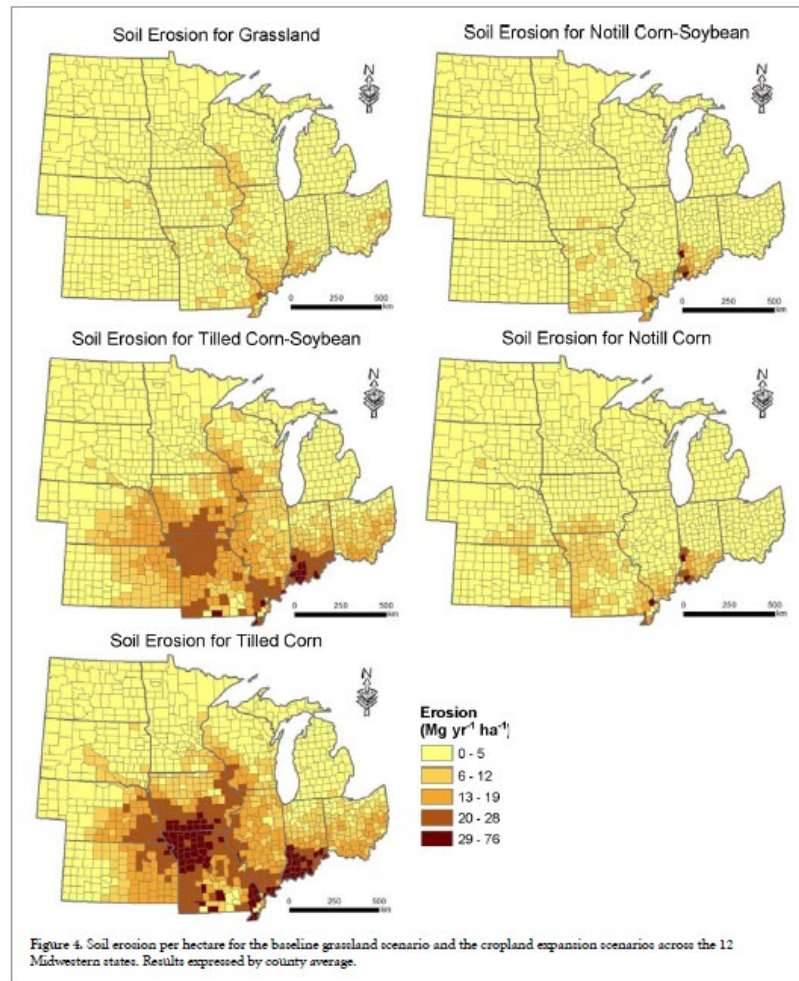
	• Honeywell: Ethanol To Jet (honeywell.com). • Gevo and Axens: Gevo and Axens partner on ethanol-to-jet technology (hydrocarbonprocessing.com) Figures 10.4 and 10.5 show that from 2004 to 2013/2014, total nitrogen (TN) conditions changed very little in wadeable streams throughout most regions of the U.S., while total phosphorus (TP) conditions worsened in many regions. This seems strange, as the primary cause – fertilizer application – is likely the same for both pollutants. Can this situation be explained? Considerable discussion is included regarding fuel leaks from underground storage tanks (USTs), and comparisons between biofuels and conventional fuels. But nothing is mentioned about fuel spillage during vehicle refueling or vehicle leaks during operation. Are these sources considered too small (or too uncertain) to mention? In reviewing Chapter 10, a few typographical and other minor errors were detected. These are identified below: • Page 10-5, line 159: change “existing” to “exiting” • Page 10-23, line 445: these temperatures should be °F, not °C • Page 10-28, line 545: change “Figure 10.11” to “Figure 10.12” • Page 10-39, line 731: should refer only to Figure 10.17, not to Figure 10.16 • Page 10-41, line 781: “... in contrast to air quality and water quantity ...” • Page 10-44, Figure 10.18: The bars labeled “Gas” should be “Gasoline.” The same correction applies to Figure 10.19 on page 10-45.
Kaffka	Chapter 10. Water Quality³² 2a. I think most readers would find it helpful if the Key Findings section began with a preamble that describes the basis for the chapter’s findings, and the uncertainty associated with the studies and methods it relies upon. EPA is attempting to describe landscape process across a huge geographic region of the United States, which is highly diverse and constantly changing in response to economic, policy, climate, new technology and other dynamic influences. Because of the scale of the region studied, the number of empirical studies on water quality, especially those conducted over time at a meaningful local scale, are limited. Long term studies are the most valuable for the purpose of this chapter, but they are hardest to carry out given the short-term character of most scientific funding and the scale and heterogeneity of the region studied. They are few in number. As a consequence, in the summary and elsewhere in the chapter, there is much use of

³² Most of my comments and answers to these questions are contained in the page and line number comments below. It was easier and made greater sense to comment where questions or issues arose directly in the text.

	imprecise, speculative language (e.g., conversion of land ...may...could...). The use of such language does not lead to clarity, but reflects the state of knowledge. EPA should state clearly at the beginning in the Key Findings why the agency must be tentative about some conclusions, or uncertain about actual emissions values. EPA does a good job of summarizing the data and modeling limitations in section 10.7.4 (page 10-52) that qualify its ability to be precise or to predict accurately future trends. It would be well to include a summary of these qualifications in the preamble I suggest here. 2b. Some studies that are relevant have been overlooked. I think EPA overlooks the effects on LCLM in the US, and especially in the western areas of the corn(soy) belt, of China's rapid increase in finished feed manufacturing and meat and egg production that occurred during the same period as that affected by the RFS1 and RFS2. China's rapidly increasing demand for whole soybeans during this period was largely unanticipated and had significant effects on land use around the world. This was most strikingly the case in South America, but also in the US as well. Over the period of the 1980s to current, soybean acres increased by a larger amount than corn acres, though both increased. This was particularly the case in areas of the western corn belt where technology change and cost advantages in exporting whole beans played a role in increased annual crop production. I have suggested some additional citations and comments below where I think they are most appropriate (see comments below for section in the report). Otherwise, EPA has done a very good job of finding and summarizing most of the literature on this topic. Chapters have to be divided up and some literature that might be appropriate for this particular chapter may be included in others. This is unavoidable. 2c. Where empirical observation is lacking, models can be used both retrospectively and prospectively. Models have strengths and weaknesses. There is a common saying that all models are wrong, but some are useful. If models are well constructed and at least partially calibrated, they can provide insight into possible trends, limited by the assumptions that structure the model. All these predictions have uncertainties associated with them. EPA is required to report on water quality effects of the RFS to congress. It may need to rely on few or in effect even a single study to do so, but the limitations of this approach should be clearly stated. EPA's new analyses in the RtC3 rely on a set of related analyses and modeling papers. Some of these papers reappear throughout the report. They provide EPA with the kind of analysis thought to be needed for the RtC3. Since EPA makes substantial use of a subset of analyses here (Lark et al., 2020; Chen et al., 2021; Zhang et al, 2021), it should also make clear that there are in some cases critiques of those studies. I have discussed this issue at greater length in Chapters 5, 14 and my general comments (Question 5). GREET is the gold standard for LCA analysis and is relied upon to determine the carbon intensity of fuels in many studies and by several jurisdictions. It has been improved steadily over time by the researchers who created and manage it at
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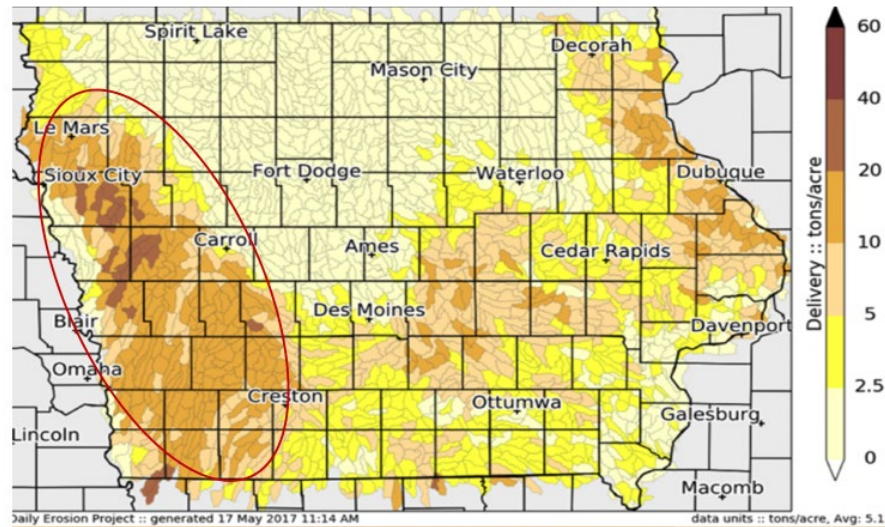
	Argonne national lab, and remains current. It is superior to most other models of its kind, particularly MIRAGE, used by the EU. Clarification: No specific changes are asked for here. I was relating my view of the relative value of these different models. I am critical of the Mirage model. It supports policy making in the EU which, over time, has been erratic. I would advise EPA not to rely on published work dependent on the use of the Mirage model. That was not the case in this report, but might occur in other circumstances. EPIC is also a widely used model for soil processes, soil water modeling and erosion modeling purposes. It has been widely used for research and scientific publication and is also subject to ongoing improvement as knowledge advances in this field. The EPIC model was used by Zhang et al, one of the primary sources for this chapter. This study evaluated an extensive area of the MORB, and relied on land conversion estimates from Spawn et al, 2020, for its input on grassland to crop conversion. Similar results and methods (Lark et al, 2020) have been disputed primarily because of reliance on CDL data in these analyses (Dunn et al., 2017; Conover et al., 2021; cited elsewhere in RtC3), and concerns about the interpretation and validation of predictions based on the methods used. Concerns about the CDL values that apparently are common across several studies may or may not be accurate. Zhang et al summarize the limitations affecting their conclusions: (1) There is no empirical validation, only modeled estimates are reported, which cannot be validated; (2) A tendency for EPIC to overestimate soil erosion is reported, one of their primary results. The amount of over-estimation (given as 14%), may be greater than the increases modeled under some scenarios, and greater than what might be measured in some of the locations modeled; (3) no explicit modeling of crop and crop management at a parcel basis is done due to computational, and likely data limitations; (4) wheat was excluded from the model despite its common occurrence in the western corn belt. (5) Tillage systems were not modeled explicitly, though some results are presented using the assumption of universal adoption of reduced or not till methods. These are associated with significantly lower emission losses; (6) grassland types were not differentiated; (7) overestimation of the length of grassland use, which leads to inflated estimates of SOC losses and perhaps other nutrient losses (especially N); (8) static estimation of dynamic processes; which change over time; (9) possible underestimation of soil carbon accumulation under corn production or C/S/W rotations. A summary of their self-professed limitations would be useful when the study is introduced. Their figure 6 (copied below) reports interesting results. Losses for no-till corn, and no till corn-soybean reported in the figure appear to be very little different from those reported for erosion from grassland. Given the errors of estimation cited in the paper and listed above, these differences may not be important or at least quantifiable. Data for the adoption of no-till or reduced tillage methods for corn and soybean report a majority of acres treated in this way. In water limiting locations like the Dakotas, no till methods are likely to be moisture conserving and thus adopted commonly. Lastly, they miss or ignore reports on increasing SOC from large scale
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summaries of empirical soil test data and modeling from some of the locations they model (see Clay et al., 2012).



It is not surprising the perennial grasslands are more stable and less prone to emissions than cultivated fields. The uses of the SWAT model cited in this chapter support that well-understood and reported phenomenon. SWAT models soil and water processes at a larger scale than EPIC, the watershed scale. The studies cited that focus on conversion to perennial biomass crops within erosive landscapes (Ha et al., 2020; Ha et al, 2018; Ha and Wu, 2017; Wu and Zhang, 2015) support that increases in cellulosic biomass crops in areas vulnerable to erosion would reduce erosion and related emissions. This is a reasonable conclusion.

Chen et al (2021) used the **SWAT** model to estimate emissions from the largest area, the entire MORB. They reported the most complex and sophisticated use of SWAT. It was difficult to assess adequately all the methods used in the time allowed for this review. Their general conclusion was that hotspots in western IA and northeastern MO were responsible for most discharge from the region. However, sensitivity to erosion and related nutrient losses in these upland areas of the Midwest was already widely known and understood. See the figure below for example.



Source: <https://4rplus.org/2018/09/25/soil-prepared-future/>

Chen et al's modeling results confirm that sensitivity, identifying these areas as hotspots. It is my understanding that much of this region was already farmed before the 2008-16 time period analyzed. Attributions for emissions from grassland conversion in ND and SD were based on CDL data, and derived from earlier works that have in part been disputed. Those concerns transfer to Chen et al's results. Emissions in ND and SD were reported to be much lower than the hotspot areas further south in the MORB, and since conversion of grassland to cropland was estimated for an overly broad or simplified grassland category, these modeled values should be interpreted with caution when assessing emissions losses.

BEIOM was new to me. It links input-output analysis with life cycle assessment. This is novel approach so far as I can tell. I/O analysis is commonly used in economic analysis to determine the economic effects of a particular new activity or technology, including direct, secondary and tertiary effects on employment, construction, and the effects of new wealth created throughout a community. It is sometimes criticized at times for relying on out of date data or because there are no exact analogues within the data base for a new industry or process. But it does provide reasonable approximations for the consequences of a new economic activity broadly under static conditions (prices, current matrix of economic activities, and assumptions, fixed relationships among activities, and others). As long as these assumptions and limitations are understood, results can be usefully interpreted. BEIOM (Lammers et al., 2021) goes further in assigning environmental consequences to economic activities identified via I/O analysis (see figure below). It extends LCA, such as done in GREET, across a much wider economic framework.

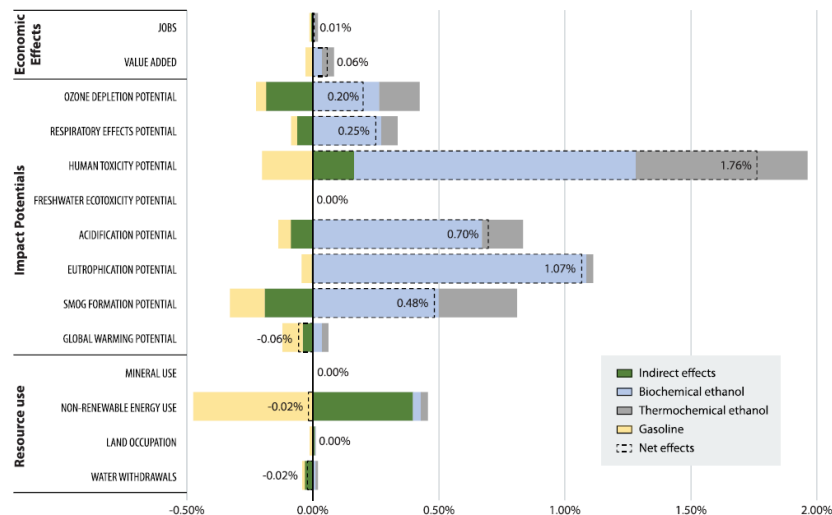
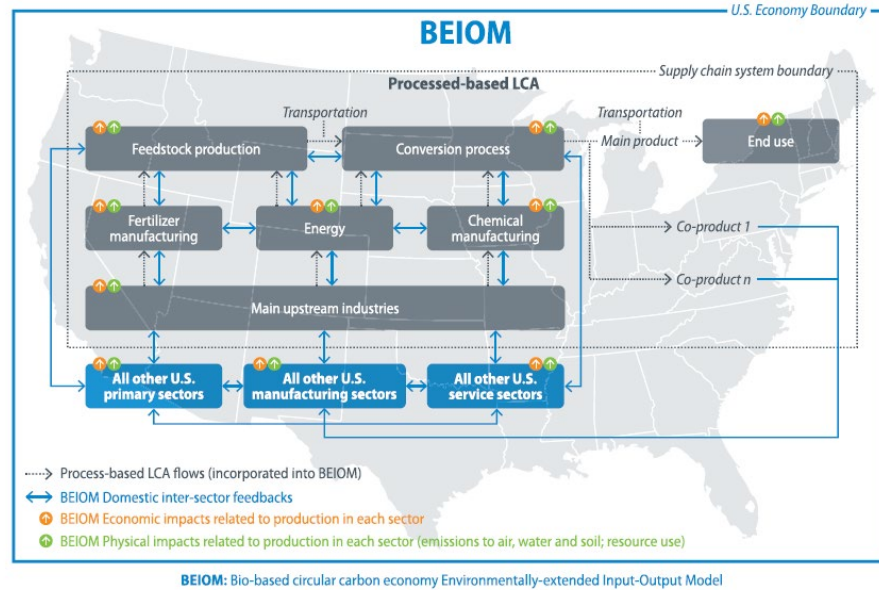


Figure 2. Net effects of an expanding U.S. bioeconomy: scenario comparison to the baseline. Note: "Indirect effects" account for the changes in all other sectors of the economy except biochemical ethanol, thermochemical ethanol, and gasoline.

The results reported are of great interest, but tend to be small. For example, a 1.76% increase in human toxicity potential is estimated for the use of thermochemical processes and cellulosic feedstocks. It seems hard to accept that estimates of relatively small effects modeled at the scale of the entire economy are significant to the second decimal place (see fig 2 above), especially given the static character of I/O analysis and the tendency for technology to evolve and improve. EPA and readers should keep this in mind.

However, the modeling exercise is interesting and suggestive. It is clear that compared to agriculturally derived feedstocks and biofuels, petroleum production has a much smaller landscape footprint and related effects. This is one of the advantages of fossil fuels. The decision to rely on bio-materials derived from the

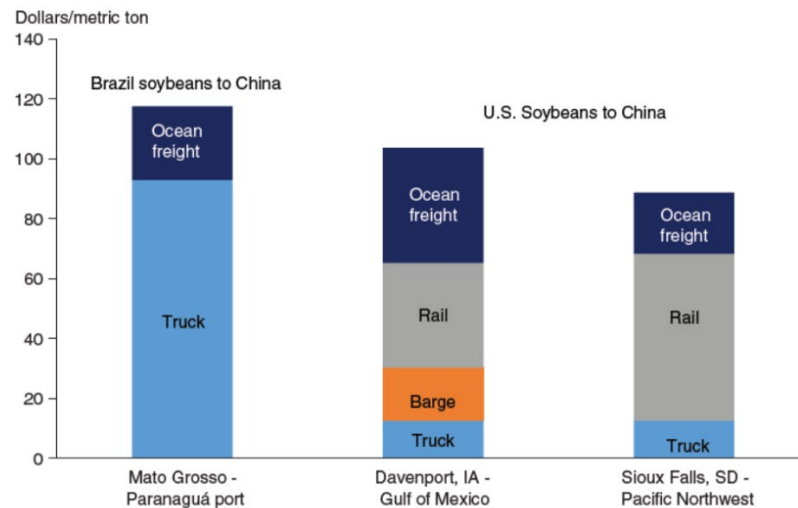
	landscape with their lower energy density is also a decision to tolerate some of the unavoidable consequences of LCLM correlated with farming. 2d. There is no doubt that modifying unmanaged landscapes in order to convert them to a condition suitable for farming causes perturbations to their use by incumbent wildlife, and may increase emissions. Arguments made for sustainably intensifying agriculture in current areas as much as possible to avoid landscape conversion in unmanaged landscapes elsewhere are based on this fact. EPA is charged with identifying specific outcomes from the RFS2 over the 2008 to 2016-time frame, apart from other factors influencing LCLM. This is challenging. Trends observed in 2008 to 2016 are part of much longer time series of change in land management due to many interacting factors. The expansion of annual crop production along the western edge of the corn-soybean production region of the US has been developing for many years. Most of the attribution of LCLM is attributed to increased corn production, but soy production has expanded as well and exports from this region serve Asian export markets, which have increased steadily over a long period, including during the period studied by EPA. 2e. Largely discussed above and in the comments made within the text. 2f. Yes. See comments below. Overview Page 10-3, lines 81-83: <i>“Water quality impacts of these changes, in the context of this report, are examined for groundwater, freshwater (rivers, lakes and streams), drinking water, and coastal waters.”</i> Lines 92-93: <i>“... Movement to water bodies of pesticides associated with biofuel feedstocks <u>may</u> also be an important impact that is explored here.”</i> Conditional language. Page 10-4, lines 104-105: The use of tile drainage does not involve bypassing soil as stated here. Rather, nutrients may be removed from soils with drainage water, and then transferred to surface drainage waters rather than more slowly to GW in the absence of tile use. Page 10-5, lines 131-132: In the absence of direct observation, a modeling exercise is used to estimate increased emissions from the Missouri River Basin (MORB). A very large and diverse area is integrated for this analysis. This risks over-generalization. (lines 138-139): <i>“ Since corn and soybean production have increased, correlational evidence suggests that biofuel production has also contributed to these changes.”</i> Is “inference” meant here? EPA frequently uses tentative language to describe the contribution of the RFS to emissions: for example (line 146), <i>...“increased fertilizer N and P <u>could result</u> in increased leaching...”</i>, etc. There is little doubt that intensive row crop farming results in emissions, but EPA has already identified significant uncertainty associated with attribution of LCLM to the RFS (Chapters 5/6). In most instances, it may not be different from zero or much
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above zero. That is not to say that the approximately 30% of corn grown and used for biofuels, net of DDGS, does not have measurable landscape effects. But EPA's task is focused only on the additional level of effects attributable to the RFS.

As a reader, I found it difficult at times to keep this in mind as the text shifts back and forth from general effects of row crop production, to attribution specifically to RFS related effects.

Page 10-5, lines 151-161: CRP conversion results from an act of congress. There are many factors that influence subsequent crop production on former CRP lands but the fate of land dropped from enrollment due to declines in public support cannot be reliably attributed to the RFS. For example, there has been a significant expansion in soybean exports to Asia via the Pacific North West ports. These beans largely originate in the western corn(soy)belt, due to cost advantages to these ports supported by freight rail networks.

Figure 18
Soybean transportation costs to China, selected locations, 2017



Note: Transportation costs to Shanghai, China, are shown.

Source: Data taken from Salin (2018).

Gale et al, 2019³³.

High prices for soy related to increased demand for whole bean exports to China to support the unprecedented scale of China's finished feed industry expansion (Gale 2015, Gale et al., 2019) are just as likely to support land conversion on former CRP land or from cropland pasture, hay or fallow as domestic demand for corn ethanol or soy biodiesel. Corn and soybean are rotational complements. If soy is grown, corn is commonly grown the next year.

³³ Gale, F., Valdes, C., and Ash, M., 2019. Interdependence of China, United States and Brazil in Soybean Trade. USDA-ERS

Figure 1
Manufactured feed output, 2012



Source: USDA, Economic Research Service analysis of data from China, U.S., and European Union (EU) feed industry associations, and United Nations Food and Agriculture Organization.

Fig. 1 (above). “After nearly four decades of transitioning from a largely plant-based diet toward greater meat consumption, China is now the world’s largest producer of livestock products and has also emerged as the largest manufacturer of animal feed...”³⁴

The development of improved soybean hybrids adapted to this region during the period of growing exports increased yields per acre and added to other factors supporting increased soy production in former wheat producing regions of the western corn belt (Gale et al., 2019; page 31). When soy is grown, corn is also commonly included in rotation, so both have to be evaluated together. Both crops in rotation lead to combined emission losses.

Page 10-10. Fig. 10.2a-c and Page 10-11, Fig. 10.3a-c: Time trends in [total N], [total P], and sediment imply mixed results with declines appearing to be more common than increases (USGS data cited).

Stets et al (2020) is cited as reporting little change in N but slight increases in P loading at the continental scale. It is not clear what importance is given to this report?

Page 10-12: EPA’s data on rivers and streams support Stets conclusions about Total N, but changes in Total P were greater. Agricultural regions were not distinguishable from other landscape types, and EPA states that the cause for TP increases are not well-established (figs 10-4 and 10-5), (and therefore not attributable to the RFS).

Page 10-17. Corn and soybean are frequently rotated and have to be evaluated together for effects on N and P. While more N is applied to corn, soybeans have less aggressive root systems and are planted later in the year, so may recover less N in early spring when tile drainage losses may be largest. Soy also leaves little crop residue, so surface transport of nutrients via erosion may be greater following soy than following corn. Farmers

³⁴ Gale, 2015. Development of China’s feed industry and demand for imported commodities. FDS-15K-01; USDA-ERS https://www.ers.usda.gov/webdocs/outlooks/36929/55490_fds-15k-01.pdf

commonly plant both crops because of their complementarity and to hedge economic risks. The relative profitability of these crops compared to each other varies, so attribution primarily to just one crop is difficult.

Page 10-18. Pesticides

EPA summarizes the results of several surveys for pesticides and their residues in surface waters (primarily) or groundwaters. Neonicotinoids were the most common insecticides found, although at low levels. They are used for seed treatments at low rates per acre (more for soybeans than corn) but apparently move through the soil or via surface transport (erosion and runoff) to creeks, streams and rivers. Corn herbicides like atrazine and glyphosate are found commonly in surface waters draining corn/soybean growing regions.

Clarification: Most attribution of environmental effects from the RFS is attributed to corn, so the comment about large amounts of neonicotinids used as a seed treatment when soybeans are grown due to larger seeds and much large seed populations was left unsated. It should be noted by EPA.

Page 10-23, lines 435-437. EPA cites Thomas et al (2014, 2009) to state that much remains unknown. It seems clear however, that pesticides are detected in runoff and drainage for annual cropping systems in the Midwest, so this statement is confusing.

Page 10-24, lines 463-464. Dissolved Organic Carbon losses are noted as potentially greater from grasslands than from row crops. Indicating that all crops and farming landscapes have emissions issue, even though they differ.

Page 10-26, Fig. 10-19 is not useful for distinguishing among activities due to similarities of coloration among many activities. It is hard to interpret.

Page 10-25, lines 514-516: *...“Simulation results suggest that the SWAT model can be used to adequately estimate stream flow and riverine sediment and nutrient loads throughout the MORB after model calibration and validation.”*

It is unclear if this is the authors’ statement or EPA’s? What is meant by: “adequately estimate”...? What is adequate?

The largest rates of putative land conversion (LCLM) modeled by Lark et al and used by Chen et al (same group) to simulate nutrient emissions (TN, TP) were reported for ND and SD (page 10-28, lines 537-538), but quantitatively larger nutrient losses were estimated for established agricultural landscapes in IA, NE MO, and KS. EPA citing these authors notes that the percent change in land use in ND and SD was small, despite the vivid color contrasts used in the figures reproduced.

Page 10-32, lines 565-568: Results from Chen et al should be viewed as water quality effects from agriculture in general, and are not specific to biofuel feedstock production.

Lines 568-572: This study could not resolve farming practices to the field level, so results reflect some combination of the three scenarios presented. It could (and likely-SK) does underestimate conservation practice adoption, (lines 576-577), so could (and likely) overestimates emissions increases.

Lines 573-575: The precise type of grassland conversion is not specified in these studies, so it is likely that the consequences of land conversion are overestimated. CDL data are difficult to work with, may be insufficient quality for this purpose. This is discussed in greater detail in my comments on Chapter 5 and Chapter 14.

CRP land conversion was not directly related to the RFS, but due to acts of congress which reduced funding for this purpose during the years overlapping with these analyses. Land conversion back and forth among hay/pastured grasslands and fallow lands (sometimes referred to as cropland/pasture) and annual cropping are long-term practices in the regions modeled and are affected by many factors. Rotation of these lands emphasizes the cropland conversion portion of the term. Thus, these studies represent potential worst-case conditions.

EPA acknowledges that these studies (collectively) are best used for estimating the direction of change in land use in the region analyzed, not absolute amounts or emissions resulting. EPA also acknowledges that these changes cannot be attributed to biofuel feedstock production *per se* but to the overall evolution of agricultural practices in this region. It is also the case that the paper by Lark et al., has been criticized as overestimating land conversion effects of the RFS and their consequences (Conover et al, : see also comments and the exchange posted at the Argonne National Laboratory website: https://greet.es.anl.gov/publication-comment_environ_outcomes_us_rfs). Without judging the relative merits of the Lark et al study and its critiques, these unresolved controversies should be more fully noted when EPA suggests relying on Lark et al and related studies.

Page 10-33, lines 595-597: EPA does state that the estimates from Lark et al are at the high potential end for land conversion and conflate the effects of several crops and their demand and price in markets, and that the possible range includes zero and thus values close to zero.

Page 10-33 Conservation practices

CEAP I and II, and APEX are discussed.

Page 10-34, lines 625 to 640: NRCS survey and modeling data suggest that losses of sediments and nutrients are distributed unevenly, with a few acres contributing disproportionately to losses. Over time, losses have diminished. The greatest improvements (reductions) in surface losses were observed in the northern and southern plains, some of the areas highlighted by Lark et al.

Lines 655-657: But subsurface losses, were estimated/modeled to have increased in the same areas, and soil carbon was estimated to have increased marginally (Table 10.3, page 10-35).

Diverse beneficial conservation practices are discussed. Six of eight conservation practices are cited as improving over time based on NRCS survey data.

Page 10-39, lines 728-734: The cost of implementing some practices was estimated as below the value of recovered nutrients/materials, indicating the need for profitable farming enterprises that generate sufficient profit to invest in conservation measures and the need for prudent public subsidies. The simulated areas in the MORB where emission

reductions are greatest do not overlap with areas with the greatest value for trapped nutrients (Fig 10.16 and 10.17).

Page 10-40. 10.4 Likely Future Impacts

Page 10-40: Renewable diesel (RD) made from fats, oils and greases (FOG) is discussed. In general, EPA may have underestimated the rate of production increase and adoption of FOG-based RD. Increased production and use at levels greater than expected have allowed CA's LCFS to meet or exceed its carbon emission reduction targets. CA has a large portion of total transportation fuel use in the US. Several large refinery conversions or de novo construction projects to produce RB from FOGS and some vegetable oils are underway in CA and elsewhere in the US.

Page 1-40 to 10-41, lines 757-760: *"...As discussed in section 10.5, for these and other reasons the estimated potential water quality effects per megajoule (MJ) of biofuel appear to be decreasing for corn ethanol as practices improve, even though the total potential effects per gallon had been increasing as the industry grows."* ... This statement seems contradictory. EPA should explain what it means more clearly. Perhaps they intend to say that per unit emissions are declining while total (industry) emissions are increasing as the industry expands. (?).

Page 10-41. 10.5 Comparisons with Petroleum

Page 10-42, lines 797-799: The BEIOM model links Input-output analysis to LCA. BEIOM reports to account for total direct and indirect effects from fuel production and use. Results indicate (unsurprisingly) that farming activities have the largest effects (Fig. 10.18 and 10.19).

Page 10-46: Corn ethanol improved over time, while soy BD did not. This may have to do with the large rates of yield gain occurring in corn production and larger gains in total factor productivity.

Ecotoxicity declined for corn, while it increased for soy. Eutrophication per unit of ethanol has declined due to increasing ethanol yields and efficiency.

Page 10-48. Horizon Scanning

Conversions of land to switchgrass (or other perennial grasses) was discussed.

Pages 10-48 to 10-49, Lines 936-939: The statement is made that 250 acres of land in switchgrass located strategically in the Kansas River watershed would reduce the loss of N by up to 220 M lbs. This is nearly a million pounds per acre of switchgrass. There is a typo here and EPA should check these values.

10.7 Synthesis

Page 10-50, lines 972-984: EPA relies too heavily on a study by Chen et al, that relies on other contested work, without adequately evaluating or reflecting the disagreements over methods and results that have been raised in the literature about this larger body of work.

	Later: Page 10-51, lines 1028-1030: EPA states that the range of estimates within this work (and they should add in the work of others with different findings) is the largest source of uncertainties about the additional effects of the RFS on emissions.
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3.3.1.4 Chapter 11: Water Use and Availability

Reviewer	Comments
Engel	2a. The chapter provides a clear and technically supported analysis of water use impacts of the RFS. By far, the greatest impact on water use occurs from increased corn and soybean production that is irrigated. Some of the irrigated corn and soybean production are used for biofuels and some of this is attributed to the RFS and thus the impact on water use is high. Biorefineries have decreased water use (especially for ethanol production) and soy-based renewable diesel requires little water relative to that required by corn-based ethanol. Changes in plant genetics have increased yields, especially for corn, per unit of water consumed. For dryland areas, this would represent a decrease in water use per unit of corn-based biofuel produced and is not captured in this chapter. 2b. The most important literature to water use associated with biofuels and the RFS is overviewed and cited. These studies are interpreted correctly as well. I have no suggestions for additional data, literature or studies that should be added. 2c. The SWAT modeling presented in the chapter is appropriate. Although not highly familiar with GREET and WATER, the use of these models based on the description and information presented are reasonable. 2d. The conclusions around water use are well supported by information and data presented in the chapter. The amount of water use per mass of corn and soybeans produced has also decreased over time due to crop improvement as well as irrigation technology changes and improvements. The impact of crop improvement in the recent decade and into the future should be acknowledged. The regional impacts on groundwater supply and aquifers are significant in some locations as concluded. The biorefinery use of water is well described and appropriate conclusions drawn. The conclusions around analysis of water use for petroleum-based fuel and biofuels are well supported. Several of the recommendations while interesting and having impacts in other areas do not seem to be significant needs to support future analyses needed for this report. Review of the recommendations with this lens would be beneficial. 2e. The uncertainties and limitations are reasonable. In most instances, these have little impact on the high level conclusions, but as in prior chapters are potentially important in quantifying local impacts if these are important to quantify. Quantifying local impacts will continued to be a significant challenge into the future due to limitations in the needed spatial data for assessment.

	2f. The graphics in the chapter are useful and support the discussions and analyses and resulting conclusions.
Kurkalova	2a. Chapter 11 material is clearly organized and presented, and appears technically accurate. 2b. With the main focus of the chapter on irrigation, the literature review summaries of the information provided by three primary methods/sources, survey-based data collected by USDA, satellite-based data allowing for finer spatial and temporal resolution than the surveys, and model-based studies, are highly informative. I commend the authors for clearly detailing the differences in the alternative data sets providing the information on the spatio-temporal patterns of irrigation (section 11.3.1): • USDA-NASS Census of Agriculture, • Survey sample-based: USDA-NASS Irrigation and Water Management surveys; USGS water use data; CEAP • Satellite and other remote data To further the point that differing data collection approaches might and often do result in different estimates, I suggest a brief discussion of the differences between two depictions of the distribution of, for example, irrigated corn acreage, that has been developed using alternative data (please see the suggestion about Figures 11.12 and 11.5 below). 2c. The literature reviewed in the chapter refers to three models, SWAT, GREET and WATER. The use of the models appears appropriate to the respective tasks. SWAT was used to evaluate the changes in streamflow attributable to changes in LCLM. A combination of GREET and WATER models was used to compare the life-cycle water consumption of corn ethanol, soybean biodiesel, and petroleum. 2d. The conclusions and recommendations described in the chapter synthesis section are supported by the material presented. 2e. The major uncertainties and limitations associated with the chapter's conclusions are clearly identified. 2f. Glossary and multiple graphics are useful for conveying the information referenced and discussed in the chapter. A few suggestions for improvement are provided below. Figure 11.4: I found the use of two vertical (Y-) axes in panels (a) and (c) confusing. Could the legend for each of the panels be revised to explicitly state which time trends are scaled to the left-hand Y-axes, and which – the right-hand Y-axes? Figure 11.5 depicts very useful information about irrigated corn acreage in 2017 and changes in it from 2007 to 2017. Could a similar figure be added about

	irrigated soybeans? Such information would be useful given the unmet need to quantitatively evaluate how soybean acreage responded to the RFP Program. Figure 11.6: the left- and the right-hand side panels look approximately the same. I suggest either deleting the figure or possibly pointing to specific differences between the two panels that the authors would like to highlight. In this draft, there appears to be no discussion specifically pointing to the difference between the panels. Figures 11.7, 11.9: again, providing soybeans counterparts could be useful. Figure 11.9, panel (a): not clear what the dashed line refers to. Figure 11.12 is very informative. A brief discussion of the differences between panel (a) of this figure (year 2012, LANID&CDL) and the left-hand side panel of Figure 11.5 (year 2017, Census of Ag) could be of interest for illustration of both the changes over time and the differences in data sources. Figure 11.16: the scale of the legends is too small to be easily readable. Scaling up the figure to a whole page (with a landscape orientation) or letting each panel occupy a whole page may help with the problem.
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3.3.1.5 Chapter 12: Terrestrial Ecosystem Health and Biodiversity

Reviewer	Comments
Davis	2a. Chapter 12 provides a thorough review of land use for crops and associated impacts on biodiversity. Much of this discussion is related to common management used for corn and soybean agriculture, despite whether or not it is used for biofuels. The entire impact, similar to other chapters, depends on the assumptions about the attribution of the RFS to land use change for biofuels. Interestingly, while a full review is provided of the BMPs for agriculture and conservation practices that reduce negative impacts, there is very little review of the potential impacts from other advanced biofuels (perennial and woody crops are briefly mentioned, but there is no mention of biogas). The land use impacts of agriculture in comparison with gasoline are more clearly described in this chapter (specifically line 616-625) than other chapters, despite citing some of the same literature that was cited in previous chapters. The distinction was made clear here and could be used for the overall report synthesis. 2b. The literature review seems complete, except for the lack of information cited to describe biogas and other advanced biofuels (including stover, which is only mentioned briefly). There is a comparison made between biofuel and gasoline that only analyzes literature estimates of land use change that would affect habitat. Air-borne emissions that affect biodiversity were not compared, yet the downwind emissions from ethanol production are identified as drivers of change on line 98; it would be

	good to consider the potential air-borne exposure of plants and wildlife that result from gasoline and oil production systems. Line 300 refers to addition chemical loads associated with genetically engineered corn and soybean. While this is true in some cases, there are also GMO corn varieties that result in lower pesticide loads (e.g. Bt). Stover removal is addressed in line 678-682, but not tied specifically to land areas affected. There should be information, even if from industry records, about areas where this is practiced. 2c. No original modeling work was done for this chapter. It reports results from modeling exercises previously published. I did not review these external sources in detail. 2d. Yes. Generally, this chapter was well-written. 2e. Yes, but the section on limitations does not identify other advanced biofuels (e.g. biogas) that were not reviewed. 2f. Yes. Add locations with stover removal for bioenergy to land use analysis.
Hill	2a. Overall, this chapter is clear and technically accurate, except for the analyses based upon the results of Chapter 6. 2b. In line 224, the specific epithet of <i>Fusarium graminearum</i> needs correction. The interpretation that the land-use change estimated in Lark et al. (2022) in lines 480–487 is too high is only relative to the analysis in Chapter 6 and should be changed. The statement in line 629 should be revised given the potential for algal biofuel impacts on biodiversity as described in https://iopscience.iop.org/article/10.1088/1748-9326/ab8d7f/pdf. 2c. The model use here is appropriate. 2d. Yes, mostly, but those based upon Chapter 6 are not supported. 2e. Yes 2f. Yes
Hoekman	2a. Chapter 12 provides material related to biofuels' impacts on terrestrial ecosystem health and biodiversity in a clear and accurate manner. Due to difficulties in attributing specific land cover/ land management (LCLM) changes across different locations to the RFS Program, it is not possible to quantify the impacts of RFS on terrestrial ecosystem health and biodiversity. However, it appears that for most metrics investigated, expanded production and use of biofuels in general had overall negative impacts. Therefore, it stands to reason that the overall impacts of the RFS Program are also negative, though to a smaller extent. Chapter 12 does an excellent job of explaining that ecosystem health and biodiversity can be affected by both habitat loss and by exposure to pesticides,

	with the extent of these factors varying considerably from one species to another and one situation to another. 2b. To my knowledge, the most important data and relevant literature are cited and interpreted appropriately in Chapter 12. For the most part, these data and literature bolster the conclusions from the earlier RtC2 report. An important new analysis performed for the RtC3 report showed that a significant amount of land conversion from perennial cover to biofuels feedstocks (corn and soybean) occurred in areas adjacent to or within critical habitat of threatened and endangered (T&E) species. While not clearly attributable to the RFS Program, this finding raises alarms that further expansion of biofuels could create additional risk to T&E species. 2c. I am not qualified to judge the selection and use of the models used in Chapter 12 to assess terrestrial ecosystem health and biodiversity. 2d. Yes, the conclusions and recommendations are generally supported by the information presented in Chapter 12. However, most of these conclusions are quite general and non-quantitative. If possible, some semi-quantitative conclusions should be provided regarding the extent to which renewable fuels in general (though probably not the RFS Program in particular) impact terrestrial ecosystem health and biodiversity. Of course, it should also be pointed out that any quantitative estimates have high uncertainty, and that additional work is necessary to reduce this uncertainty. 2e. Chapter 12 adequately identifies the most important uncertainties and limitations regarding current and near-future impacts of biofuels upon terrestrial ecosystem health and biodiversity. Potential impacts further into the future (10-15 years) have additional, unaddressed uncertainties resulting from unknown market, economic, policy, and regulatory changes. 2f. For the most part, these graphics, glossary, and other supporting information are useful. However, Figure 12.3 is a bit confusing. The same legend appears in both top and bottom panels, though this is not necessary. Also, it would be clearer to highlight the Lower Ohio River Valley region shown in the bottom panel by drawing a box around this area in the upper panel. General Comments Chapter 12 focuses primarily on the direct impacts of biofuels (particularly corn ethanol and soy biodiesel) on terrestrial ecosystem health and biodiversity in the U.S. Are indirect effects occurring in other regions of the world considered insignificant, and not worthy of mention? In discussing “Likely Future Impacts” (Section 12.4), it is mentioned that corn ethanol may have reached a plateau around 16 billion gallons per year (bg/y), but that biodiesel values are continuing to increase. Some additional discussion should be provided regarding the possible extent of biodiesel increases (as well as renewable diesel and SAF) over the next decade, and the potential impacts that this could have on terrestrial ecosystem health and biodiversity.
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	In addition, the current plateau of 16 bg/y of corn ethanol may not continue if mid-level ethanol blends (E15-E30) become more widespread, as is currently being advocated by some. Additionally, there is growing interest in converting ethanol to jet fuel through so-called alcohol-to-jet (ATJ) processes. These developments could result in substantial future increases in total ethanol production. It would be useful to consider here the potential future impact of increasing ethanol production to 20-30 bg/y.
Kaffka	2a. I have relatively few comments to make on Chapter 12, since this is not my area of specialty. Here is how I would summarize the primary messages in the chapter. If some important content is missing, then EPA might consider adding additional emphasis: There are some assertions in this chapter and the RtC3 overall that are generically true, obvious and indisputable: Annual row crops have different effects on associated and native species than hay fields, managed pastures and native grasslands. Some of these effects are harmful to some species, though other species may benefit. Species adapted to native grasslands (plants, insects, birds) tend to be harmed when conversion to annual cropland occurs, especially if conversion occurs on a large scale without providing sufficient refuges for such species. Mixed landscapes may benefit even those species adapted to native habitats, and benefit the row crops produced by providing IPM benefits. Insecticides used on row crops can and do harm some species of insects, birds and bats that are not direct targets of control. Herbicides used on row crops may affect other plant species in the landscape that are not targets of control, especially on field edges. Some grasslands have been converted to annual cropping for the production of corn, soybeans and wheat over the last two decades, especially in the western corn belt. This process has been occurring for many decades, but appears to have increased in scale and rate in the last two decades, especially in some years. Some land conversion in this region is attributable to increased demand for corn and soybeans and is related to the industrial use of these crops for biofuels. The amount of conversion and the types of lands converted have been estimated by different authors, and there exists disagreement about methods used for estimation and the types and amounts of land affected. These disagreements should be more directly discussed by EPA. Some of the land converted is attributable to the RFS, but attribution at the local and regional scale is imprecise and the amount attributed to the RFS is quantitatively small over the larger productive landscape. All management activities on farmland have consequences for other species, especially row crop production. Harm also can occur from the management of hay crops when nesting birds and other species are disturbed during critical life stages by harvesting. In general, larger effects are attributed to corn and soybean production than to forage production. Diverse best management practices including conservation features such as reduced tillage, precision agriculture, conservative use of pesticides, installation of

	erosion and water management features such as grassland strips along field contours, vegetative buffers, filter zones, the use of cover crops during otherwise fallow periods and other practices that reduce generic emissions from row crop production and may benefit several incumbent species. Rotation with perennial (forage) species can be beneficial, especially in more marginal landscapes and can and does occur. Good public policy would support the use of these practices as widely as possible. Unstated by EPA is that economically healthy farms can better adopt new Best Management Practices and add beneficial landscape conservation features, and that the economic health of farms is related to the RFS and biofuel production from crop surpluses in general. 2b. This chapter is clearly organized and largely well written. It covers material about species present in the western corn belt with which I am unfamiliar. The literature reviewed here about these species seems thorough and reasonable. But it was difficult for me to keep track of all the details about individual species as they are discussed. In addition, there is some discussion of benefits to some species from agricultural expansion or from mixed agriculture-grassland systems. Could EPA summarize the different reports cited in a single or few tables that compare the effects of row crops and hay crops (managed grasslands) to unmanaged grasslands on birds, bats and other species of concern by group (+, -, 0) by types of species, e.g. birds, etc? This would make clear to readers that some effects are negative, some positive, and others neutral. It would make clear that tradeoffs are involved and choices ultimately must be made about which species are of most concern, why, and what interventions are needed. Table 12-2 might be a good basis for an assessment table. The tables of endangered or affected species at the end of the chapter are helpful. Chapter 12 includes the general assumptions used throughout the RtC3 about the potentially adverse effects of corn and soybean production, especially in comparison to a presumed natural grassland or managed (hay) grassland reference point. The chapter makes use of papers by Lark et al. (2022, 2020; 2015), as do other chapters in the RtC3, that suggest that grassland conversion in areas of the western corn belt and other portions of the Midwest have increased during this period of concern (2008-2016), with generally adverse consequences for native wildlife, plants and insects. All additional data sources that I can find indicate that net grassland conversion has occurred, especially in the north central region. But the amounts, locations, and methods of assessment differ and there is some disagreement about how best to assess grassland changes. These differences are discussed in greater detail in my comments for Chapter 5 and Chapter 14. EPA, here or elsewhere, should critically evaluate the reliability of both the original papers' assertions and the criticisms of methods and results that have been made.
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	There is no adequate overall objective technical analysis or resolution of these differing views in this report. 2c. EPA summarizes a large amount of literature on the effects of row crop production on native species of birds, bats, insects, and plants. The literature itself is inconsistent in its assessment of effects. This is due to the fact that effects are local and variable, in part scale dependent, and results are reported from studies from only a few locations. To EPA's credit, it cites some studies that suggest that landscapes with both row crop production and natural grassland features can benefit at least some species found in the north central region. They also mention that grassland management (hay harvesting) can have adverse effects on nesting species, though this at times is not clearly related to grassland preservation and use. EPA might discuss directly that commonly there are tradeoffs among public goods, including species preservation, among different types of land management and different levels of, or mixtures of activities that involve a range of management effects on native species. These tradeoffs and variable effects complicate attribution of harm. Here as elsewhere, EPA relies on a set of papers³⁵, and related work to support the conclusion that increased corn and soybean production is primarily the cause of LCLM, leading to habitat changes and wetland losses. These papers seem especially useful for EPA's purposes, but other data and assessments may also be useful for this chapter that EPA did not consider. For example, a recent report by the US Fish and Wildlife Service on waterfowl populations in the US includes their fig 3 (inserted below, see also Table B1) on the numbers of prairie pothole features in western NA, including the north central states included in EPA's analysis of LCLM effects and the RFS. During the period from 2003 to current, the numbers of such features have varied but in general appear to have increased, or at least not declined. This suggests some caution at least when attributing large scale loss of these features to increased row crop production based on modeling studies. It may be that in some areas of the western corn belt, these features have declined, while increasing elsewhere in the NC region. EPA may be able to work with USFWS to more accurately identify these effects in the western corn belt.
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³⁵ Lark, TJ; Hendricks, NP; Smith, A; Pates, N; Spawn-Lee, SA; Bougie, M; Booth, EG; Kucharik, CJ; 949 Gibbs, HK. (2022). Environmental outcomes of the US Renewable Fuel Standard. *Proc Natl Acad Sci USA* 119: e2101084119. <https://dx.doi.org/10.1073/pnas.2101084119> ; Lark, TJ; Salmon, JM; Gibbs, HK. (2015). Cropland expansion outpaces agricultural and biofuel policies in the United States. *Environ Res Lett* 10: 044003. <https://dx.doi.org/10.1088/1748-9539/10/4/044003>; Lark, TJ; Spawn, SA; Bougie, M; Gibbs, HK. (2020). Cropland expansion in the United States produces marginal yields at high costs to wildlife. *Nat Commun* 11: 4295; Zhang, X; Lark, TJ; Clark, CM; Yuan, Y; LeDuc, SD. (2021). Grassland-to-cropland conversion increased soil, nutrient, and carbon losses in the US Midwest between 2008 and 2016. *Environ Res Lett* 16: 054018.

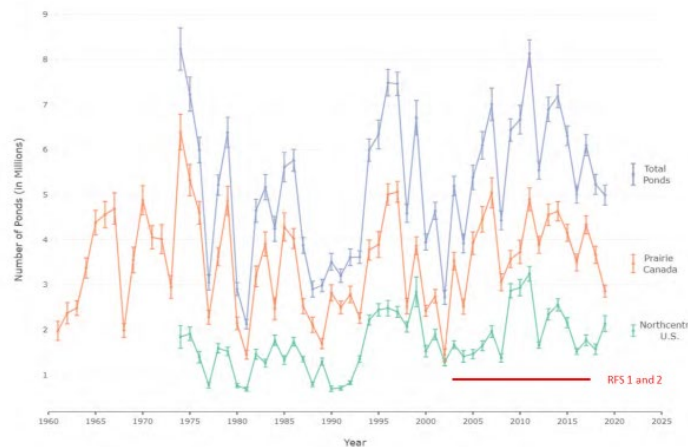


Figure 3. Number of ponds in May and 90% confidence intervals in Prairie Canada, the northcentral U.S., and both areas combined (Total ponds). In: *Waterfowl Population Status, 2022*. US Fish and Wildlife Service

2d. **Page 12-3, lines 67-68:** There are several terms that require definition in this statement.

What is meant by biodiversity and how is it measured? Answer (lines 71-72: *"This chapter addresses biodiversity in general terms, by discussing both changes in the number of species within individual taxonomic groups (e.g., birds, insects) and abundance within species or types of species."*

Are "general terms" sufficient to advise congress about policy, or to guide regulatory action?

What is meant by health? How will it be defined?

Lastly, is the use of a bioindicator a reliable indicator of actual landscape effects?

Page 12-6, lines 151-155: EPA asserts that the effects of corn and soybean production are similar across all diverse end uses, and that studies with diverse objectives and time periods can be integrated to give a general picture of ecosystem effects.

Question: diverse bird species are seed eaters and can use harvested fields for feed, especially during winter. Is this considered in this study?

Habitat loss was considered more important in one study than pesticide use (lines 175-176, Hill et al, 2014) but management was more important in another (lines 182-185, Stanton, 2018). In Stanton et al, mowing of hay fields (grasslands) was considered to be the largest mortality factor.

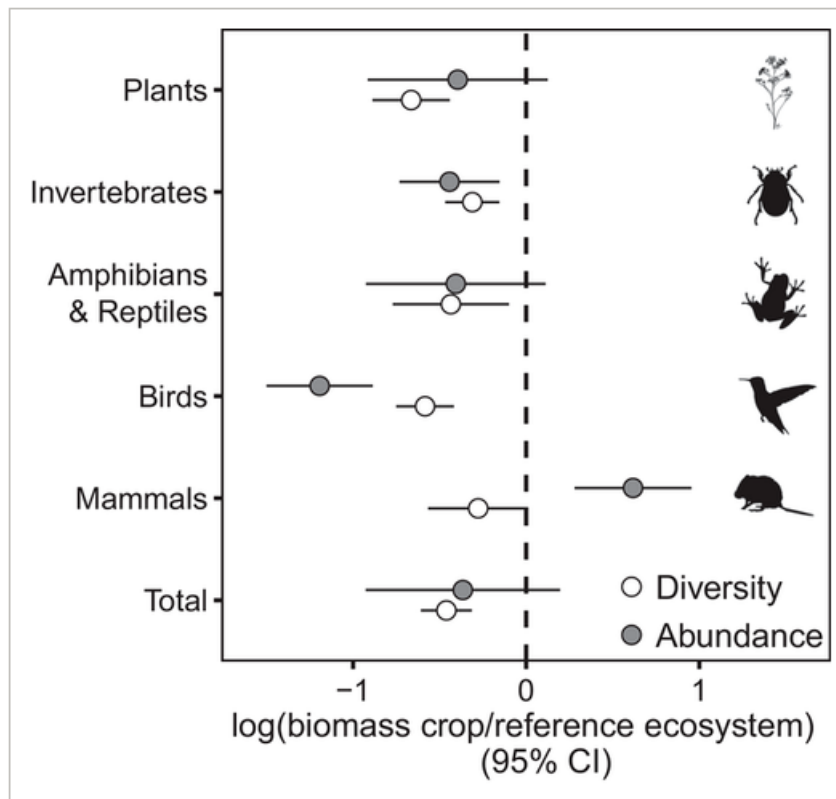
Page 12-8, lines 205-213: Evans and Potts (2015) found variable effects from habitat changes except for a few species, with some positive and some negative effects. This emphasizes how local adverse and beneficial effects are within a highly diverse landscape. Their analysis might be given more discussion.

On Birds, Bats, Pollinators and rare and endangered plants, interesting results and studies are discussed, but overall, the results presented tend to be mixed. Given

	that long-term agroecological changes are occurring in this region, including the years examined, and crop production is slowly shifting northwards in the corn belt regions of the US, due to, the best approach to species preservation seem likely to be specific programs that identify key habitats at the local scale and programs to encourage land owners to preserve these habitats be to highlighted. Page 12-17: lines 443-447: <i>“Furthermore, the accuracy of remotely sensed estimates of land use change depend on the scale at which they are tested, with lower accuracy at smaller scales, and when comparing change versus static amounts (Copenhaver et al., 2021; Lark et al., 2020; Dunn et al., 2017). However, <u>as long as the error is random and not biased, the estimated effect over many patches of land is on average likely to be accurate.</u>”</i> I think this statement needs additional discussion. It relates to larger disagreements about the reliability of methods and definitions of grassland discussed elsewhere. Page 12-18. Fig. 12.3. The figure seems to highlight central corn belt locations, not those in focus in this chapter. It needs better explanation and is in general not very helpful. Page 12-2. In general, EPA is careful and tentative about concluding large-scale effects from the RFS program on land conversion and related species. This seems appropriate. Page 12-21, section 12.3.4. The potential benefits of conservation practices are well discussed. In general, economic prosperity in the agricultural sector helps pay for conservation practices beneficial to wildlife and emissions reduction. Page 12-23, lines 591-594. Soy oil can be surplus with respect to soybean meal, the primary reason to grow the crop. This surplus oil use, is a market clearing mechanism for byproducts of the primary product. Page 12-23, Horizon Scanning. I think the prospects for alternative (non-crop based feedstocks) currently are limited. Increasing crop yields, increasing crop efficiency, the blend wall for ethanol (Chapter 6), and the costs and difficulties of converting refractory materials to useful forms of energy make short term prospects for alternative feedstocks at scale weak, despite some of their putative benefits for wildlife species and the larger landscape. Page 12-25. Corn stover use is discussed. This has been the focus of study in the agronomic literature, but not cited here³⁶. There have been three substantial investments in stover utilization, which have been either delayed at scale or abandoned (Poet-Emmettsburg IA, Abengoa-southwestern KS, and Dupont (Nevada, IA)). In my view, successful use of corn stover where amounts are in
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³⁶ <https://www.sciencedirect.com/science/article/pii/S0961953422001088> Aghaei et al, 2022. Biomass and Bioenergy. Xu et al., 2019. Soil organic carbon response to corn stover removal. <https://onlinelibrary.wiley.com/doi/full/10.1111/gcbb.12631>

	excess in high yield farming areas would reduce the demand for fuels made for perennial grasses. Page 12-27: lines 717-718. <i>“Any effects may be relatively small compared to those of total U.S. cropland, but may be more important regionally or locally.”</i> I agree. EPA has a difficult task. Large scale modeling analyses are ultimately unsatisfying when species effects are local. It would be useful if EPA could support future work at the local scale to help it meet its reporting obligations. I would recommend working directly with the relevant land grant institutions in each area of interest. Agronomists, local extension economists and local wildlife specialists are most likely to have a fine-grained understanding of land uses and their consequences. The absence of this approach in the RtC3 overall is a significant oversight. 2e. [no response provided] 2f. [no response provided]
Landis	2a. This is my primary area of expertise and <u>I find there are major gaps in the topics addressed in this chapter</u> (see next question). For those topics that are covered, I have a favorable impression of the clarity and technical accuracy of the materials presented in this chapter. The authors have done a very good job of explaining the background behind each concept/factor they covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results. 2b. • International effects of land use change on biodiversity alluded to in Chapter 16 should have a corresponding section in this chapter. • The concept of ecological (aka. evolutionary) traps may be useful to introduce in this chapter. “Ecological traps occur when the behavioral cues (such as vegetation structure) used by organisms for habitat selection do not correlate well with adaptive outcomes (Schlaepfer et al. 2002)” ○ Robertson, B.A. and Blumstein, D.T., 2019. How to disarm an evolutionary trap. <i>Conservation Science and Practice</i>, 1(11), p.e116. • Multiple recent studies of the impacts of bioenergy cropping systems on taxa and ecosystems services of interest were not covered in the chapter. Below I list key papers that I am aware of by taxonomic group. Multi-taxa assessments • Núñez-Regueiro, M.M., Siddiqui, S.F. and Fletcher Jr, R.J., 2021. Effects of bioenergy on biodiversity arising from land-use change and crop type. <i>Conservation Biology</i>, 35(1), pp.77-87.

**Figure 2**
[Open in figure viewer](#) | [PowerPoint](#)

Estimated impacts of replacing reference ecosystems with potential bioenergy crops on the world's flora and fauna. Impacts are calculated as the amount of biodiversity (diversity and abundance) in bioenergy croplands relative to reference ecosystems (log response ratio) (points left of 0, less biodiversity in bioenergy crops than reference ecosystems). For example, a value of -0.5 on the x-axis means that for approximately every 3 species or individuals detected in bioenergy crops, 5 species or individuals are detected in reference ecosystems.

From Núñez-Regueiro et al. 2021

Birds

- Blank, P.J., Williams, C.L., Sample, D.W., Meehan, T.D. and Turner, M.G., 2016. Alternative scenarios of bioenergy crop production in an agricultural landscape and implications for bird communities. *Ecological Applications*, 26(1), pp.42-54.
- Fletcher Jr, R.J., Robertson, B.A., Evans, J., Doran, P.J., Alavalapati, J.R. and Schemske, D.W., 2011. Biodiversity conservation in the era of biofuels: risks and opportunities. *Frontiers in Ecology and the Environment*, 9(3), pp.161-168.
- Gottlieb, I.G., Fletcher Jr, R.J., Núñez-Regueiro, M.M., Ober, H., Smith, L. and Brosi, B.J., 2017. Alternative biomass strategies for bioenergy: implications for bird communities across the southeastern United States. *GCB Bioenergy*, 9(11), pp.1606-1617.

	- Landis, D.A., Gratton, C., Jackson, R.D., Gross, K.L., Duncan, D.S., Liang, C., Meehan, T.D., Robertson, B.A., Schmidt, T.M., Stahlheber, K.A. and Tiedje, J.M., 2018. Biomass and biofuel crop effects on biodiversity and ecosystem services in the North Central US. <i>Biomass and bioenergy</i>, 114, pp.18-29. - Robertson, B.A., Doran, P.J., Loomis, E.R., Robertson, J.R. and Schemske, D.W., 2011. Avian use of perennial biomass feedstocks as post-breeding and migratory stopover habitat. <i>PloS one</i>, 6(3), p.e16941. - Robertson, B.A., Doran, P.J., Loomis, L.R., Robertson, J.R. and Schemske, D.W., 2011. Perennial biomass feedstocks enhance avian diversity. <i>GCB Bioenergy</i>, 3(3), pp.235-246. - Robertson, B.A., Landis, D.A., Sillett, T.S., Loomis, E.R. and Rice, R.A., 2013. Perennial agroenergy feedstocks as en route habitat for spring migratory birds. <i>BioEnergy research</i>, 6, pp.311-320. - Robertson, G.P., Hamilton, S.K., Barham, B.L., Dale, B.E., Izaurralde, R.C., Jackson, R.D., Landis, D.A., Swinton, S.M., Thelen, K.D. and Tiedje, J.M., 2017. Cellulosic biofuel contributions to a sustainable energy future: Choices and outcomes. <i>Science</i>, 356(6345), p.eaal2324. - Roth, A.M., Sample, D.W., Ribic, C.A., Paine, L., Undersander, D.J. and Bartelt, G.A., 2005. Grassland bird response to harvesting switchgrass as a biomass energy crop. <i>Biomass and Bioenergy</i>, 28(5), pp.490-498. - Uden, D.R., Allen, C.R., Mitchell, R.B., McCoy, T.D. and Guan, Q., 2015. Predicted avian responses to bioenergy development scenarios in an intensive agricultural landscape. <i>GCB Bioenergy</i>, 7(4), pp.717-726. - Werling, B.P., Dickson, T.L., Isaacs, R., Gaines, H., Gratton, C., Gross, K.L., Liere, H., Malmstrom, C.M., Meehan, T.D., Ruan, L. and Robertson, B.A., 2014. Perennial grasslands enhance biodiversity and multiple ecosystem services in bioenergy landscapes. <i>Proceedings of the National Academy of Sciences</i>, 111(4), pp.1652-1657. Bats - Ober, H.K., Jones, G.M., Gottlieb, I.G., Johnson, S.A., Smith, L., Brosi, B.J. and Fletcher Jr, R.J., 2020. Bat community response to intensification of biomass production for bioenergy across the southeastern United States. <i>Ecological Applications</i>, 30(7), p.e02155. Natural enemies (biocontrol agents) and pest suppression are not discussed. - Helms IV, J.A., Ijelu, S.E., Wills, B.D., Landis, D.A. and Haddad, N.M., 2020. Ant biodiversity and ecosystem services in bioenergy landscapes. <i>Agriculture, Ecosystems & Environment</i>, 290, p.106780. - Landis, D.A., Gardiner, M.M., van der Werf, W. and Swinton, S.M., 2008. Increasing corn for biofuel production reduces biocontrol services in agricultural
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	landscapes. <i>Proceedings of the National Academy of Sciences</i>, 105(51), pp.20552-20557. • Landis, D.A. and Werling, B.P., 2010. Arthropods and biofuel production systems in North America. <i>Insect Science</i>, 17(3), pp.220-236. • Meehan, T.D., Werling, B.P., Landis, D.A. and Gratton, C., 2012. Pest-suppression potential of midwestern landscapes under contrasting bioenergy scenarios. <i>PLoS One</i>, 7(7), p.e41728. • Robertson, G.P., Hamilton, S.K., Barham, B.L., Dale, B.E., Izaurrealde, R.C., Jackson, R.D., Landis, D.A., Swinton, S.M., Thelen, K.D. and Tiedje, J.M., 2017. Cellulosic biofuel contributions to a sustainable energy future: Choices and outcomes. <i>Science</i>, 356(6345), p.eaal2324. • Skevas, T., Swinton, S.M., Meehan, T.D., Kim, T.N., Gratton, C. and Egbedewe-Mondzozo, A., 2014. Integrating agricultural pest biocontrol into forecasts of energy biomass production. <i>Ecological Economics</i>, 106, pp.195-203. • Werling, B.P., Dickson, T.L., Isaacs, R., Gaines, H., Gratton, C., Gross, K.L., Liere, H., Malmstrom, C.M., Meehan, T.D., Ruan, L. and Robertson, B.A., 2014. Perennial grasslands enhance biodiversity and multiple ecosystem services in bioenergy landscapes. <i>Proceedings of the National Academy of Sciences</i>, 111(4), pp.1652-1657. • Werling BP, Meehan TD, Robertson BA, Gratton C, Landis DA. Biocontrol potential varies with changes in biofuel–crop plant communities and landscape perenniality. <i>Gcb Bioenergy</i>. 2011 Oct;3(5):347-59. • Zhang, Y., Haan, N.L. and Landis, D.A., 2020. Landscape composition and configuration have scale-dependent effects on agricultural pest suppression. <i>Agriculture, Ecosystems & Environment</i>, 302, p.107085. Soil arthropod biodiversity • Zahorec, A., Reid, M.L., Tiemann, L.K. and Landis, D.A., 2022. Perennial grass bioenergy cropping systems: Impacts on soil fauna and implications for soil carbon accrual. <i>Gcb Bioenergy</i>, 14(1), pp.4-23. Line 258. (Sánchez-Bayo and Wyckhuys, 2019; has been widely criticized for methodological reasons. This reference is better. • Raven, P.H. and Wagner, D.L., 2021. Agricultural intensification and climate change are rapidly decreasing insect biodiversity. <i>Proceedings of the National Academy of Sciences</i>, 118(2), p.e2002548117. Line 297. Bees are also exposed to pesticides when they consume pollen from agricultural weeds
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	• Wood, T.J., Kaplan, I., Zhang, Y. and Szendrei, Z., 2019. Honeybee dietary neonicotinoid exposure is associated with pollen collection from agricultural weeds. <i>Proceedings of the Royal Society B</i>, 286(1905), p.20190989. Line 306+. Perennial biomass crops harbor more plant diversity than annuals. • Werling, B.P., Dickson, T.L., Isaacs, R., Gaines, H., Gratton, C., Gross, K.L., Liere, H., Malmstrom, C.M., Meehan, T.D., Ruan, L. and Robertson, B.A., 2014. Perennial grasslands enhance biodiversity and multiple ecosystem services in bioenergy landscapes. <i>Proceedings of the National Academy of Sciences</i>, 111(4), pp.1652-1657. Line 332+. Dicamba drift is also associated with impacts on plants in field margins causing effects on pollinators. • Bohnenblust, E.W., Vaudo, A.D., Egan, J.F., Mortensen, D.A. and Tooker, J.F., 2016. Effects of the herbicide dicamba on nontarget plants and pollinator visitation. <i>Environmental Toxicology and Chemistry</i>, 35(1), pp.144-151. • Egan, J.F., Bohnenblust, E., Goslee, S., Mortensen, D. and Tooker, J., 2014. Herbicide drift can affect plant and arthropod communities. <i>Agriculture, Ecosystems & Environment</i>, 185, pp.77-87. • Johnson, N.M. and Baucom, R.S., 2022. Dicamba drift alters plant–herbivore interactions at the agro-ecological interface. <i>Ecosphere</i>, 13(11), p.e4274 Line 373-81. Rusty-patched bumble bee and Mitchell’s stayr butterfly are T&E species similarly affected by loss of perennial habitat. 2c. Not applicable to this chapter 2d. In general, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. However, at minimum <u>I recommend including a review of the literature on the effects of changing agricultural landscape complexity on terrestrial ecosystem health and biodiversity.</u> I suspect this could lead to a shift in the language of the Chapter conclusions to include: Impacts to date from biofuels on domestic terrestrial biodiversity, as an indicator of ecosystem health, are primarily due to corn and soybean feedstock production for ethanol and soy biodiesel. <u>Reduction in agricultural landscape complexity in part due to</u> shifts in perennial plant cover to corn and soybeans, and corn and soybean production practices are main drivers of effects. 2e. The chapter does a good job identifying uncertainties and limitations associated with its conclusions. I agree that the largest uncertainty is the amount of land converted that is attributable to RFS. 2f. The graphics, glossary and supporting materials are helpful in clarifying and supporting the conclusions of the chapter.
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3.3.1.6 Chapter 13: Aquatic Ecosystem Health and Biodiversity

Reviewer	Comments
Engel	2a. With the exception of the statement in the conclusions “Water demand for feedstock production reduces stream flow and changes flow patterns that are important for supporting fish diversity.”, the clarity and technical accuracy are reasonable. This statement is not supported by material presented in the chapter. This is a general statement that may have some elements of truth but deserves more analyses and discussion of nuances if it is to be included. 2b. If there is interest or concern about changes in flow, much more data and discussion of literature is needed. In some US regions, stream flows are increasing. Models and data indicate that for some regions the conversion of pasture to corn and soybeans increases flow. Seasonal variability is important as are changes that may result from additional corn and soybean production. Many of the impacts in flow changes are localized as well. Changes in rainfall and temperatures further confound analyses. 2c. The SWAT model for the use cases presented in the chapter is reasonable. It is an appropriate model for these kinds of analyses. The SWAT modeling in this and other chapters has been focused on the Missouri River Basin, and this is understandable given the changes in land use toward corn and soybeans in the basin. Such modeling for other regions may also be insightful. 2d. As noted above, the conclusion around impact on stream flows is not supported in the chapter and many nuances exist around flow that were not explored. Other conclusions are supported within the material presented in the chapter. The recommendations are appropriate and logical. 2e. Registration of pesticides requires extensive assessments on endangered species. The uncertainties and limitations and materials in the chapter do not appropriately acknowledge EPA has conducted such assessments that are related to this issue. These efforts include FIFRA, FQPA, and ESA. The prior EPA assessments for water that have been conducted as well as endangered species assessments associated with water that have been conducted are likely useful in this chapter. 2f. Supporting materials were appropriate. No additional suggestions.
Hoekman	2a. Chapter 13 clearly presents a good summary of issues related to the impacts of biofuels on aquatic ecosystem health and biodiversity. The complexity of this area is accurately represented by discussing impacts on a variety of aquatic species of stream flow changes, pesticide usage, sediment changes, nutrient levels, and other factors. On the whole, these impacts appear to be negative with respect to aquatic ecosystem health and biodiversity. As with other environmental impacts discussed in earlier chapters, these adverse effects cannot be confidently attributed to individual, discrete causes, such as the RFS Program. Yet, it stands to reason that some portion of these adverse effects is due to the RFS Program.

	2b. To my knowledge, the most important data and relevant literature are cited and interpreted appropriately in Chapter 13. For the most part, these data and literature sources support and strengthen the conclusions from the earlier RtC2 report. An important new analysis conducted for the RtC3 report (described more fully in Section 10.3.2) investigated water quality changes resulting from conversion of grasslands to biofuel crops in the Missouri River Basin (MORB). This analysis suggests that although such changes in land cover and land management (LCLM) are likely to increase surface water nutrient concentrations slightly, this will have relatively small overall impacts on aquatic ecosystem health and biodiversity. This new analysis also shows that the LCLM changes within the MORB occur in areas overlapping with ranges of some threatened and endangered (T&E) species, thus increasing concerns in this domain. 2c. I am not qualified to judge the selection and use of the models used in Chapter 13 to assess the impacts of biofuels upon aquatic ecosystem health and biodiversity. 2d. Yes, the conclusions and recommendations described in Chapter 13 are supported by the material presented. As pointed out in Section 13.7.2 (page 13-46, lines 1042-1043), "The scientific literature continues to emphasize the negative effects of altered water flow and water quality on aquatic biodiversity." However, as in previous chapters, many of the conclusions are qualitative only, and the impacts attributable to the RFS Program cannot be determined. Hence, the recommendations for more research to quantify impacts and assess the specific role of the RFS Program are very appropriate. 2e. I believe that Chapter 13 adequately identifies the most important uncertainties and limitations regarding current and near-future impacts upon aquatic ecosystem health and biodiversity. Potential impacts further into the future (10-15 years out) have additional, unaddressed uncertainties resulting from unknown market, economic, policy, and regulatory changes. 2f. Generally, the graphics, glossary, and other supporting information in Chapter 13 are useful. However, Figure 13.3 could be improved. All four panels of this figure could fit on a single page if the format were changed to that used in Figure 13.4 and Figure 13.6. This format would emphasize the change in fish multi-metric index (MMI) between the periods of 2008/09 and 2013/14, rather than emphasizing what the MMI conditions were in each time period. General Comments Section 13.4 (Likely Future Impacts) takes a very narrow and short time view of the future, considering only impacts of the RFS program out to 2025. (In fact, this is a general weakness of the entire RtC3 report.) It would be useful to discuss possible developments at least 10-15 years into the future. During this longer period, it is entirely possible that much greater conversion of land to corn and soybean agriculture could occur. Section 13.6 (Horizon Scanning) does take a longer view of biofuel feedstocks, but limits its consideration to cellulosic feedstocks (corn stover and switchgrass) and algae. It seems much more likely that increased conventional feedstocks will increase over this
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	time frame, in response to growing demands for mid-level ethanol fuel blends, renewable natural gas (RNG), renewable diesel fuel (RD), sustainable aviation fuel (SAF), and other regulatory programs such as California's LCFS. • In reviewing Chapter 13, a couple of typographical errors were detected, as noted below: • Page 13-33, line 746: the 2nd appearance of "Figure 13.12" should be "Figure 13.13" • Page 13-46, line 1017: change "al" to "all"
Landis	2a. For those topics that are covered, I have a favorable impression of the clarity and technical accuracy of the materials presented in this chapter. However, landscape-scale effects of agriculture on aquatic ecosystems required increased coverage. The authors have done a very good job of explaining the background behind each concept/factor they covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results. 2b. Recent studies of the impacts of agriculture on aquatic biodiversity and function are listed below. • Bennett, A.F., Nimmo, D.G. and Radford, J.Q., 2014. Riparian vegetation has disproportionate benefits for landscape-scale conservation of woodland birds in highly modified environments. <i>Journal of applied ecology</i>, 51(2), pp.514-523. • Blann, K.L., Anderson, J.L., Sands, G.R. and Vondracek, B., 2009. Effects of agricultural drainage on aquatic ecosystems: a review. <i>Critical reviews in environmental science and technology</i>, 39(11), pp.909-1001. • Chester, E.T. and Robson, B.J., 2013. Anthropogenic refuges for freshwater biodiversity: their ecological characteristics and management. <i>Biological Conservation</i>, 166, pp.64-75. • Dollinger, J., Dagès, C., Bailly, J.S., Lagacherie, P. and Voltz, M., 2015. Managing ditches for agroecological engineering of landscape. A review. <i>Agronomy for Sustainable Development</i>, 35, pp.999-1020. • Hines, J., Ebeling, A., Barnes, A.D., Brose, U., Scherber, C., Scheu, S., Tschardtke, T., Weisser, W.W., Giling, D.P., Klein, A.M. and Eisenhauer, N., 2019. Mapping change in biodiversity and ecosystem function research: food webs foster integration of experiments and science policy. In <i>Advances in Ecological Research</i> (Vol. 61, pp. 297-322). Academic Press. • Law, A., McLean, F. and Willby, N.J., 2016. Habitat engineering by beaver benefits aquatic biodiversity and ecosystem processes in agricultural streams. <i>Freshwater Biology</i>, 61(4), pp.486-499.

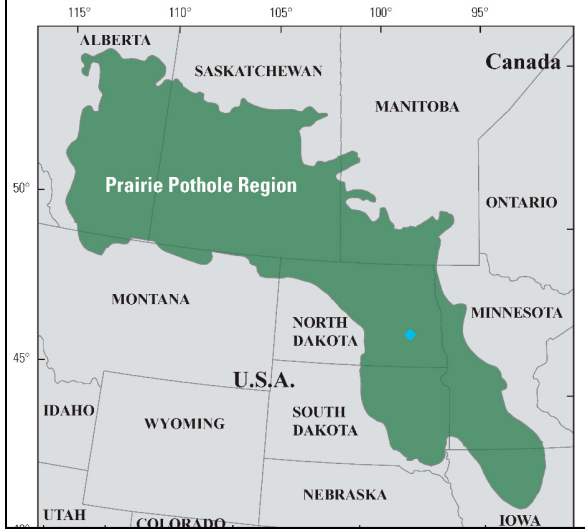
	• Rideout, N.K., Lapen, D.R., Peters, D.L. and Baird, D.J., 2022. Ditch the low flow: Agricultural impacts on flow regimes and consequences for aquatic ecosystem functions. <i>Ecohydrology</i>, 15(5), p.e2364. • Triana, J.S.A., Chu, M.L. and Stein, J.A., 2021. Assessing the impacts of agricultural conservation practices on freshwater biodiversity under changing climate. <i>Ecological Modelling</i>, 453, p.109604. • Trochet, A., Dechartre, J., Chevalier, H.L., Baillat, B., Calvez, O., Blanchet, S. and Ribéron, A., 2016. Effects of habitat and fragmented-landscape parameters on amphibian distribution at a large spatial scale. <i>The Herpetological Journal</i>, 26(2), pp.73-73. • Walton, R.E., Sayer, C.D., Bennion, H. and Axmacher, J.C., 2021. Open-canopy ponds benefit diurnal pollinator communities in an agricultural landscape: implications for farmland pond management. <i>Insect Conservation and Diversity</i>, 14(3), pp.307-324. • Westgate, M.J., Crane, C., Smith, D., O'Malley, C., Siegrist, A., Florance, D., Lang, E., Crane, M., Hingee, K., Scheele, B.C. and Lindenmayer, D.B., 2022. Improved management of farm dams increases vegetation cover, water quality, and macroinvertebrate biodiversity. <i>Ecology and Evolution</i>, 12(3), p.e8636. 2c. I have had some exposure to EPIC modeling. Specifically, I am aware that in the past it was limited in how it could be used to infer landscape-level effects as the individual management unit outcomes were combined across the area of study and therefore could not account for adjacency effects. I am also generally aware of SWAT's and GREETS capabilities as watershed and life cycle analysis models respectively. I accept that EPIC, SWAT and GREET are standards used in analysis of bioenergy cropping system impacts. I have no prior exposure to BEIOM and cannot comment on its appropriateness. 2d. In general, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. However, at minimum <u>I recommend including a review of the literature on the effects of changing agricultural landscape complexity on aquatic ecosystem health and biodiversity.</u> I suspect this could lead to a shift in the language of the Chapter conclusions. 2e. The chapter does a good job identifying uncertainties and limitations associated with its conclusions. I agree that there are large uncertainties regarding the impacts of neonicotinoids in aquatic systems. 2f. The graphics, glossary and supporting materials are helpful in clarifying and supporting the conclusions of the chapter.
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3.3.1.7 Chapter 14: Wetland Ecosystem Health and Biodiversity

Reviewer	Comments
Hill	2a. Overall, this chapter is clear and technically accurate, except for the analyses based upon the results of Chapter 6. 2b. Line 820 needs a word after “were.” 2c. The descriptions in this chapter are supported by the literature. 2d. Yes 2e. Yes 2f. Yes
Kaffka	2a. I am not a hydrologist or wetland ecologist. I have worked in wetland related systems where agriculture is practiced in the Upper Klamath Basin, and the Delta region of central California, but these are quite different wetlands than those discussed in this chapter. So my comments are those of an interested reader with training and professional experience in agronomy and agroecology. Having worked in agriculture, I understand how soil and field heterogeneity, even at a small scale, influences management decisions. In general, I am concerned that a broad-brush approach to estimating wetland losses and gains across this large region based on high level modeling does not adequately account for the actual land management occurring and for decision making by farmers, including decisions to maintain some of the vulnerable wetlands discussed in this chapter. This chapter focuses on one of the most difficult assessment problems in the RtC3: assessing wetland losses and the consequences of those losses. The region affected in the US is huge, extremely heterogeneous, and the range of possible effects is likewise large. The scope of the analysis in Chapter 14 is reduced somewhat to areas in the north central region called the prairie pothole region (PPR), though information from other parts of the Midwest is included as judged relevant. I find terms like “ecosystem” and “health” used in the title to be ambiguous and inappropriate for a technical assessment with policy implications. A better title might be: <i>“Changes in wetland structure and function due to agriculture in the PPR and their effects on hydrology and diverse dependent species.”</i> Different data sources for wetlands and estimates of wetland loss are cited and included. These estimates differ quantitatively, cover somewhat different time periods and wetland classifications, and derive from different sources. They agree that wetlands have been lost during the period analyzed, but differ substantially in the amount of loss estimated. These discrepancies limit EPA’s ability to attribute changes in wetlands to agriculture specifically and the RFS in particular. This is especially true for the more ephemeral wetland types in the Prairie Pothole Region (PPR). Increases in precipitation, attributed to climate change and the response of

land owners to these changes, among other factors, are confounded with wetland losses. Landscape heterogeneity affects all the conclusions in the report and EPA's ability to attribute land use effects to the RFS.

- 2b. A better description of the PPR and its diversity across the region modeled would enhance the discussion in this chapter. I would include an image like the one here from McKenna et al, (2019) to provide perspective on the scale of the region and its location. The majority of this large region occurs in Canada.



The region discussed is very diverse climatically and physically. It is undergoing weather/climate changes with significant effects on hydrology. At the same time, agricultural producers are responding to dynamic economic factors while adjusting to a range of other technical and climate factors influencing farming decisions (Joshi et al, 2019).

Agriculture is not uniform across this large region and effects on hydrology likely differ significantly as well. For perspective, I sought out additional information in sources not included in the chapter, specifically: *Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin; USDA-NRCS, 2022; Agriculture Handbook no 296*. This publication describes land use by subregions within the Midwest that share soil and landscape features which constrain land use (LCLM) and result in approximately similar regional patterns of use. I copied material from the handbook to add images included below that for me provide a clearer picture of landscape dynamics in this large and diverse region. Data from this source also include additional estimates of major land uses including crops and grasslands. Area M in the Handbook (colored green), the major corn belt area in the US, is dominated by land use for corn and soybean production, but it is of interest that NRCS estimates that approximately 30% of the land in the heart of the corn belt (green colored area) is devoted to grassland pasture and deciduous forest (Fig M-1). Adding fallow lands and water features increases this total amount.

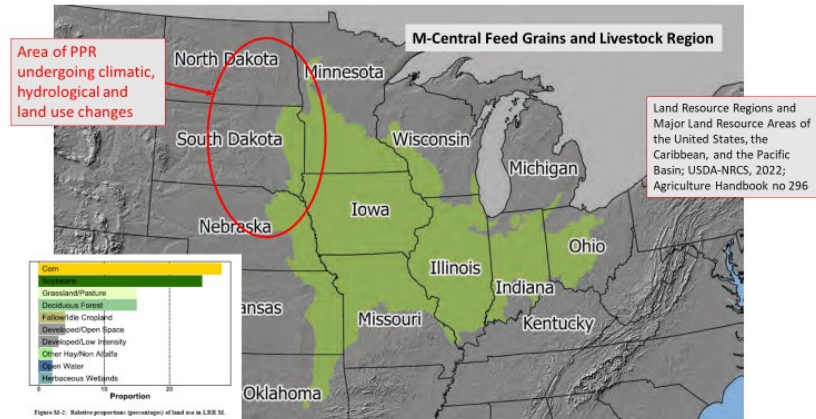
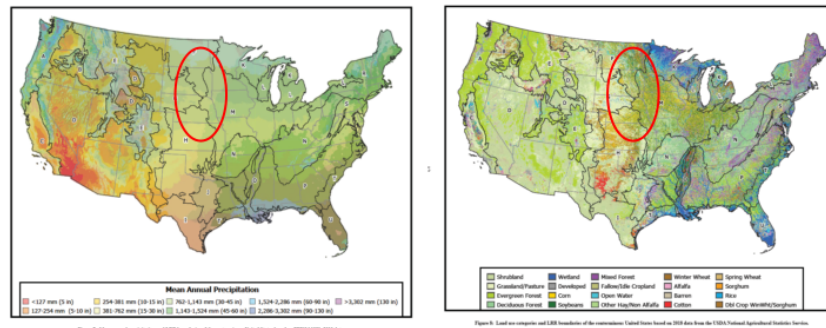


Figure M-1: Location and size of Land Resource Region M, which covers 731,180 square kilometers (282,310 square miles) from Ohio to the Great Plains and from Minnesota to Oklahoma

I added a circle in red that roughly includes the area of focus for Chapter 14 as well. It includes parts of area M, but others as well.

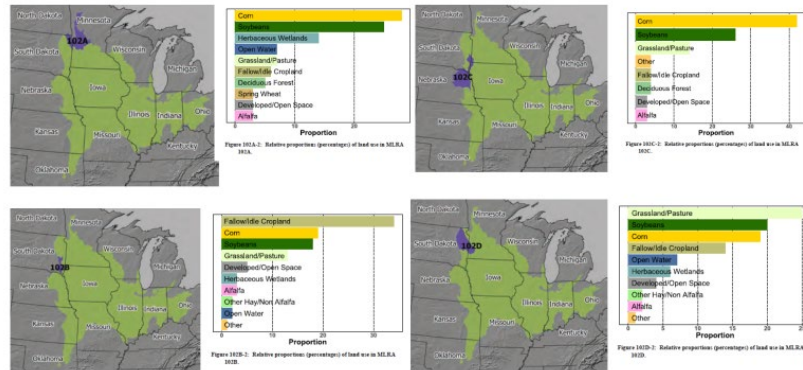
Land use for farming across the PPR region is constrained and influenced by substantial landscape heterogeneity. Figures 7 and 8 from the Handbook provide useful depictions of that heterogeneity with respect to precipitation and correlated land uses, including NRCS's classification of major land resource regions. The PPR includes four major land classification regions according to NRCS's scheme. These differences make clear visually the challenge EPA and the public face when identifying changes in land use in the region, and especially when trying to attribute cause and effect.



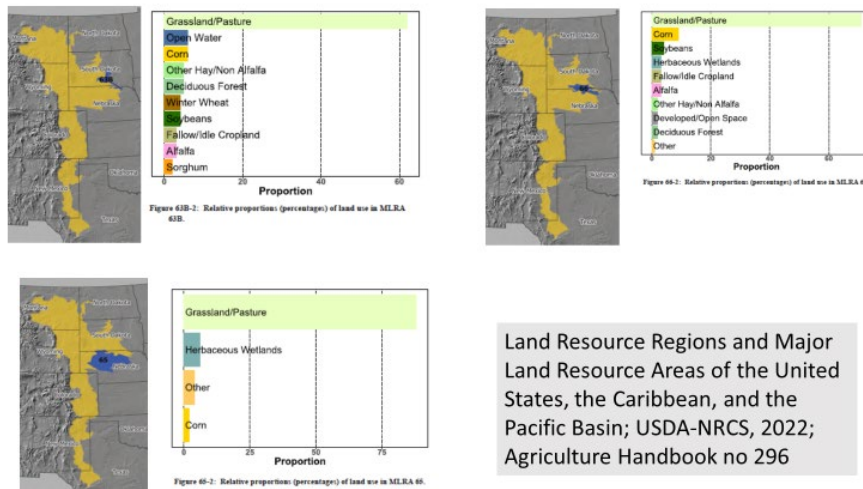
The PPR described in Chapter 14 includes four different major regional land resource classifications, used by NRCS to describe land use and constraints. Figures 7 and 8 provide useful visual information about the heterogeneity that characterizes the region discussed. NRCS, Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin United States Department of Agriculture, Agriculture Handbook 296

Handbook 296 also includes descriptions of land use in the transition areas (circled). A few that illustrate more local patterns of land use and landscape heterogeneity are copied here from the Handbook and included in the figures below. Some regions in the figure are dominated by crops, while others have large amounts of fallowed land and others large amounts of grassland pasture. They illustrate the diversity of land uses across this region. Hydrology and water management

undoubtedly vary and differ among these regions in the ways described in Chapter 14, but effects seem likely to be highly variable and local.



Examples of heterogeneous land use in different areas of the PPR; from: Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin; USDA-NRCS, 2022; Agriculture Handbook no 296



Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin; USDA-NRCS, 2022; Agriculture Handbook no 296

These examples illustrate that areas throughout the PPR differ significantly in the amount of grassland present and the character of farming. There may be some regions that act as hotspots of change, while others are much less affected. The RtC3 generalizes drainage and hydrological effects broadly. It would be helpful if EPA included some additional analysis of landscape diversity and farmer responses effects. Consultation with local land grant university experts and regional associations focused on agriculture would be helpful.

Chapter 14 also repeats a problematic characterization that is common throughout the RtC3, i.e., underestimating the role of soybean production on crop choice, attributing most change to corn production for ethanol. At least some regions in the PPR seem to produce more soybeans than corn. This appears to be due to export markets to Asia and freight rail advantages to west coast ports to satisfy those markets. Corn follows soy in rotation in these regions and may be the second

crop rotated in many instances. This is not recognized adequately in the report. In figure 55B below, soybean acres dominate, and herbaceous wetlands, the landscape resource of concern in this chapter, is estimated as 10% of land use. Grassland pasture equals corn. Would this pattern of land use be judged as acceptable in this particular region? It is unclear.

55B—Central Black Glaciated Plains

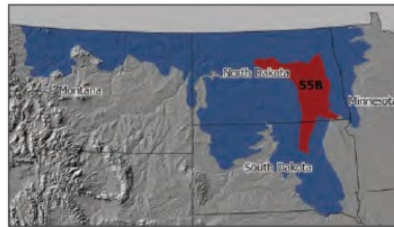


Figure 55B-1: Location of MLRA 55B, which covers 3,686,700 hectares (9,109,900 acres), within Region F.

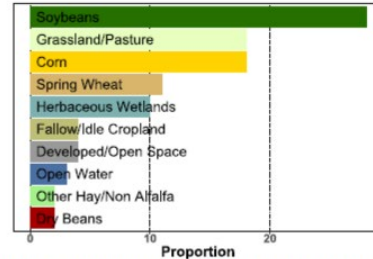


Figure 55B-2: Relative proportions (percentages) of land use in MLRA 55B.

Land Resource Regions and Major Land Resource Areas of the United States, the Caribbean, and the Pacific Basin; USDA-NRCS, 2022; Agriculture Handbook no 296

- 2c. Not relevant except in so far as reference is made to Lark et al, 2020. This is discussed in greater detail elsewhere in comments for chapters 5, 6, 10, 12 and below.
- 2d. EPA concludes that loss of palustrine or ephemeral wetlands is occurring across the region analyzed, and that these losses are due to increases in annual cropping, in combination with consistently increased precipitation. It summarizes different data sources and literature describing this process. Despite EPA's assertions, it remains unclear to me if losses in one region within the PPR are made up for by gains in others due to increasing precipitation across this region.
- 2e. This may be one of the most difficult and uncertain areas of focus in the RtC3. The body of the report includes a great deal of material describing actual and potential losses of primarily seasonal or ephemeral wetlands in the PPR. They attribute these losses to interactions between changes (increases) in precipitation and agricultural expansion. But in the end return to conclusions in Chapter 6 that the amount of change due to the RFS is small, and cannot be distinguished from zero.
- EPA discusses older and existing programs that supported wetland maintenance, but suggest that they have been insufficient in recent years. This is an area of uncertainty, especially given data not included in the report from the USFWS (2022) and NRCS (2021), discussed above.
- 2f. Yes, but I think it would be useful to include an image of the PPR as a whole as noted above in question 2a. In addition, including some images from the NRCS 2022 report on major land use categories in my view would add greater clarity to the issues that EPA is trying to account for in this chapter, which is one of the more challenging assessment efforts in the RtC3.

Page 14-2: Key findings

Lines 28 to 33: The use of the term “cropland expansion” is incorrect. Land in principal crops declined in the US or remained steady, but corn and soybean acres increased as a portion of total principal crop acres.

A figure of 88% for grassland loss is used, but this figure has been and is a contested value in the technical literature, including literature cited in the RtC3 report. EPA does not adequately address this disagreement when asserting this value for grassland loss.

It is asserted that 275K acres of wetlands in the Prairie Pothole Region were lost. But alternative data from USFWS (2022, see below) indicate stable levels and in some years increases in wetland acres, which would imply an increase in the area of prairie potholes in the north central region during some years in the period analyzed.

This may be due to increased annual precipitation in recent years (*Page 14-3, lines 61-62*). If that is the case, previous years’ lower levels of acres may have been due to the occurrence of drier years.

Lines 50-53: With all land management decisions, some species are favored, some disadvantaged: *...“waterfowl (ducks, geese, swans) as a group have not experienced declines over the past decade, possibly due to availability of food (grains), increased precipitation, and the interspersion of ponded waters and agricultural fields along migration routes.”*

When managing landscapes for wildlife, fish, invertebrates, etc., it is difficult to optimize for all species simultaneously. In this statement, EPA recognizes that there are benefits for many iconic waterfowl species from the interspersion of crop fields with wetland habitats, demonstrated by the robust populations of waterfowl using these landscapes. Landscape management practices can favor some species while disadvantaging others, and rarely can conserve all species of concern. How are such tradeoffs evaluated?

Page 14.3: Overview

Lines 76-81: EPA refers to *other parts of the country*. This is unclear. Does EPA mean other parts of the Midwest outside the north central region or outside the margins of the western corn belt?

Lines 83-88: Definition of wetlands: *“(1) inundation or saturation by surface water and/or groundwater at a frequency sufficient to support, at least periodically, plants adapted to wet environments; (2) predominance of undrained hydric soils; or (3) presence of non-soil substrate saturated by shallow water at some time during the growing season.”*

Lines 90-93: Data for federally listed species associated with wetlands are cited from 1988 and 1996 references. Are there more recent assessments available?

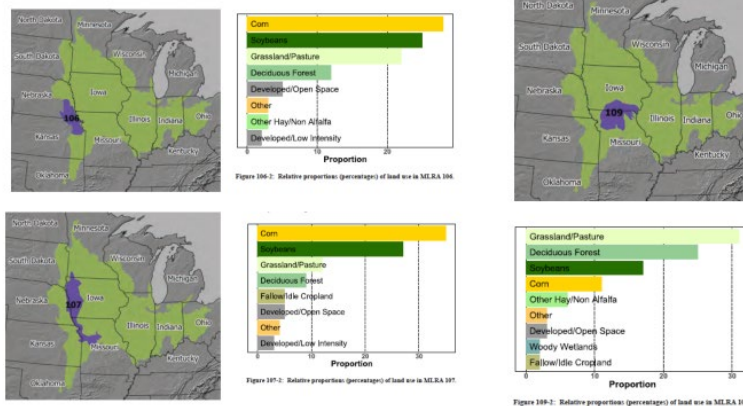
Page 14-4, lines 102-107: EPA asserts: *“Wetlands lost in recent years (e.g., since 2007) have relatively greater ecosystem impacts because the functions of remaining wetlands, while only partially mitigating for adverse effects of cumulative losses, have greater value for maintaining sustainable freshwater resources to support environmental and*

human needs, including agriculture. Large historical losses have reduced the resilience of U.S. wetlands, thereby amplifying the environmental risks associated with modern-day disturbances.

Lane et al. (2022) is cited as a source for this statement. This paper includes a comprehensive literature review and integrates this literature to provide a conceptual framework for analyzing ephemeral wetlands and the ways in which their structure and function can be maintained in landscapes undergoing change due to increased precipitation (attributed to climate change) and changes in landscape management following from that change (increased farming activities). One of the coauthors of Lane et al is lead author of chapter 14. The chapter describes well the concepts and concerns derived from the meta-analysis of literature included in Lane et al., (2022).

In that meta-analysis, the vulnerability of ephemeral watershed features, particularly in the prairie pothole region of the US is emphasized, and attributed to both climate change (increased precipitation primarily) and farmer reactions to increased precipitation. These reactions include changes in crops produced (more corn and soybeans compared to wheat-fallow, hay or grassland and hay rotations) and modifications including water management features and drainage to control water as seasonably flooded areas increase on lands where farming intensity is increasing.

The mixture of land uses and transition states in this large region, however, is highly diverse (NRCS-USDA, 2022). A series of images and land use assessments across the region from the NRCS publication included above highlight that diversity and also that there are regions in the transition areas of the western corn belt and in southern Iowa/northern MORB where large amounts of grassland pasture and natural grasslands remain important landscape features.



EPA states that the existence of “tipping points” have been demonstrated and cite Lane et al, (2022), which presents primarily a conceptual understanding of this process. Much of this discussion uses conditional language and cites the need for additional research. In the PPR, while much land apparently was converted from grasslands to croplands, some land was converted from cropland to grasslands. This appears to be a dynamic

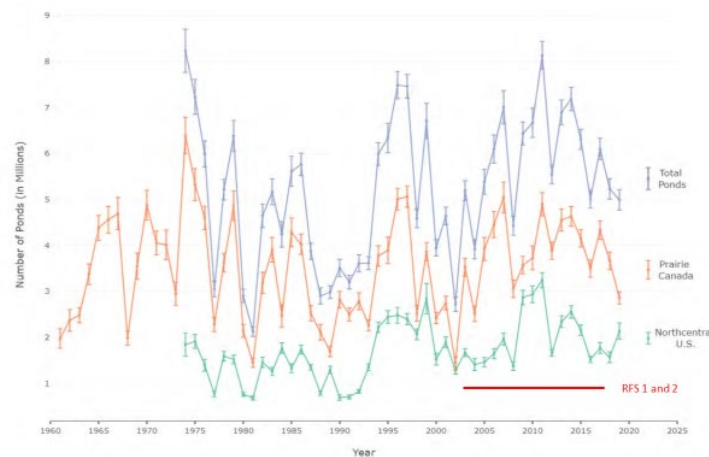
	process in the region. Does that mean that the beneficial characteristics of grasslands for wetland species will not or cannot reestablish across this diverse region? Page 14-5, lines 121-133: Progress on no net loss was achieved up to 2002, but this trend may have been reversed since. This seems to be true, even with differences in how data is used to analyze this process. Page 14-5 to 14-6, lines 142-143: Biofuel crops are described as “water thirsty”, but short season annual crops generally use less water than perennial crops or grasslands (see also <i>fig. 14.3, page 14-8</i>). EPA should be careful when making such statements and use language that better reflects quantitative assessments. In chapter 12, the potential for perennial crops like switch grass to use more water than annuals is acknowledged. Other modeling studies cited in the RtC3 indicate no effect on water use or discharge³⁷. These inconsistencies should be acknowledged. Fig. 14.3. the term ecosystem “health” is used. This is not a term of science. It would be better to use “functions” instead. Page 14-10. Focus of the chapter is on palustrine wetlands; > 6M acres are reported in the prairie pothole region of the US for this type of wetland. Fig 14.5 shows net losses of palustrine wetlands and estuarine wetlands. Estuarine losses are > palustrine losses. <i>Fig. 14.6</i> combines the two categories making the two figures less comparable. It would appear that most losses reported since 2002 in <i>fig. 14.6</i> are estuarine. But these are not discussed in the chapter. Page 14-3 to 14-4. Waterfowl are reported to have benefitted from mixed wetland/crop production landscapes, especially corn and soy fields. Lines 371-372: “Flock size is higher in wetlands interspersed with agricultural fields.” Line 373: But ... “population data for other water birds (i.e., not waterfowl) are too limited to determine national or regional trends ...” Lines 390-391: ... “<i>It is an open question when conversion or consolidation of wetlands for agriculture begins to have lethal or sublethal effects...</i>” on some species... which species are referred to? Other species seem to benefit... Lines 395-403: “<i>Janke et al., (2019) found that duck abundances continued to increase even in waterbodies with very high percentages of uplands cropped, possibly due to increased abundance in invertebrate prey resulting from nutrient runoff that increases primary productivity, which then translates into increased invertebrate productivity....</i>” ...” the “<i>wetland productivity hypothesis,</i>” which states that wetlands in intensively farmed landscapes provide better feeding areas for northward-migrating ducks than those in grassland-dominated landscapes.”
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³⁷ Zhang, X; Lark, TJ; Clark, CM; Yuan, Y; LeDuc, SD. (2021). Grassland-to-cropland conversion increased soil, nutrient, and carbon losses in the US Midwest between 2008 and 2016. *Environ Res Lett* 16: 054018. Cited in Chapter 12:

	Page 14-5, line 406: , Lark et al., (2020) is cited to document grassland loses. These estimates should be used with caution and in Chapter 6 are regarded as an overestimate. Amphibians. Page 14-6 and following: No comments. Wetland nutrient loading: Page 14-22. Mitsch’s estimates for needed wetland area to reduce N loading to gulf = 1% to 2 %. Later estimates by Mitsch are for a similar amount (1%)³⁸. EPA also reports estimates for 2 5 to 7 % of the land area. Page 14-25, lines 716-719: <i>“Recent evidence from the PPR indicates that the prolonged flooding from increased precipitation and changes in snowmelt that has been occurring since the early 1990s is being exacerbated by high rates of wetland ditching and consolidation for agriculture.”</i> Page 14-28, lines 809 to 820: Attribution to the RFS. There have been some ephemeral wetland losses in the PPR, perhaps large amounts, but likely also some gains. Gains and losses are likely variable on a yearly basis, and there may be areas with greater losses (hotspots), but also areas with gains or in steady state. EPA relies heavily on modeling, inference and generalized estimates for this large region to infer harm. <i>“The data needed to quantify the exact number, area, types, or locations of wetlands drained for biofuel production attributable to the RFS Program on wetlands are not available. However, given what is known about historical and recent rates of loss of palustrine wetlands, concentration of wetland losses in areas with high densities of water bird breeding habitats (Lark et al., 2020; Figure 14.7b), and colocation of biorefineries with observed areas of wetland conversion (Figure 1 in Wright et al., 2017), <u>it can be inferred that wetland biodiversity and ecosystem health have likely been adversely impacted.</u>”</i> Another source of information about ponds and wetlands, however, is provided in a recent USFW Service report (2022) on the nation’s Waterfowl Population Status³⁹. Data is provided in their figure 3 (below) and table B1 in the report on <u>acres of May ponds and wetlands</u> that suggests that these areas have varied significantly from year to year for decades, but in general remained stable, or even increased during the period of time potentially affected by the RFS. Variation seems to be a significant characteristic of such wetlands in the PPR. I cannot judge how this data source relates to those considered by EPA, but it would be useful if EPA could evaluate these data and their relationship to the data used in this chapter. For example, it is not clear to me if May wetlands are congruent with ephemeral wetlands in EPA’s sense, or how to reconcile this data with EPA’s inference that losses of such wetland are noteworthy effects from the RFS. Perhaps these are not the same wetland types or do not overlap exactly.
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³⁸ Nitrate-nitrogen retention in wetlands in the Mississippi River Basin. William J. Mitsch , John W. Day, Li Zhang, Robert R. Lane. Ecol Eng 2005, (24) 267-278. These authors model the area of wetlands needed to reduce N losses from farmlands in the Mississippi River Basin (excluding the Missouri River Basin) to reduce deposition in the Gulf of Mexico, and conclude that an area larger than those achieved under then current wetland protection programs would be needed.

³⁹ <https://www.fws.gov/media/waterfowl-population-status-2022> Red bar indicating RFS years added.



Page 14-30: Box 14-1: This is a useful summary of wetland conservation programs. There are several USDA programs that support wetland conservation in agricultural areas and have been successful:

“Since 1991, restoration of more than 5 million acres of wetland and grassland habitats in the PPR through the CRP and ACEP has had positive impacts on water storage, reduction in sedimentation and nutrient loading, plant biodiversity, carbon sequestration, and wildlife habitat (Gleason et al., 2011).” “NAWCA grant funds in combination with 2:1 partner matching contributions from more than 6,300 partners have contributed to the protection, restoration, and enhancement of approximately 30.7 million acres of wetlands and associated upland habitats in all 50 U.S. states...”

Additional data come from Hellerstein et al., 2019⁴⁰, a source that EPA has not cited but should add to its final report. Table 3.22.1 below comes from that source. They report that through 2012 at least, total palustrine and estuarine wetlands in the US showed no significant change, and that through 2017, that the Wetland Compliance program was protecting up to 75 % of all wetlands in the US. That leaves 25 % unprotected, as much as 266K acres. Claassen et al. (2017)⁴¹ was cited for their analysis of the effectiveness of the Wetland Compliance Program (WC) and concluded that, across their study area (the Prairie Pothole Region of Montana, the Dakotas, Minnesota, and Iowa), WC was likely to be effective at preserving about 75 percent of potentially productive wetland acreage, offered limited protection on 15 percent, and was likely ineffective on the remaining 10 percent. In that study, WC was considered to be effective when expected compliance penalties exceeded the profitability of farming the drained wetland

⁴⁰ Daniel Hellerstein, Dennis Vilorio, and Marc Ribaud (editors). *Agricultural Resources and Environmental Indicators, 2019*. EIB-208, U.S. Department of Agriculture, Economic Research Service, May 2019.

⁴¹ Claassen, Roger, Maria Bowman, Vince Breneman, Tara Wade, Ryan Williams, Jacob Fooks, LeRoy Han-sen, Rich Iovanna, and Chuck Loesch. 2017. *Conservation Compliance: How Farmer Incentives Are Changing in the Crop Insurance Era*, ERR-234, U.S. Department of Agriculture, Economic Research Service, July.

acreage, and ineffective when there was no compliance penalty, and moderately protective otherwise.⁴²

Table 3.22.1

From 1992-2012, total palustrine and estuarine wetland acreage showed no statistically significant change

Year	Acres
1992	111.3
1997	111.1
2002	111.2
2007	111.3
2012	111.2

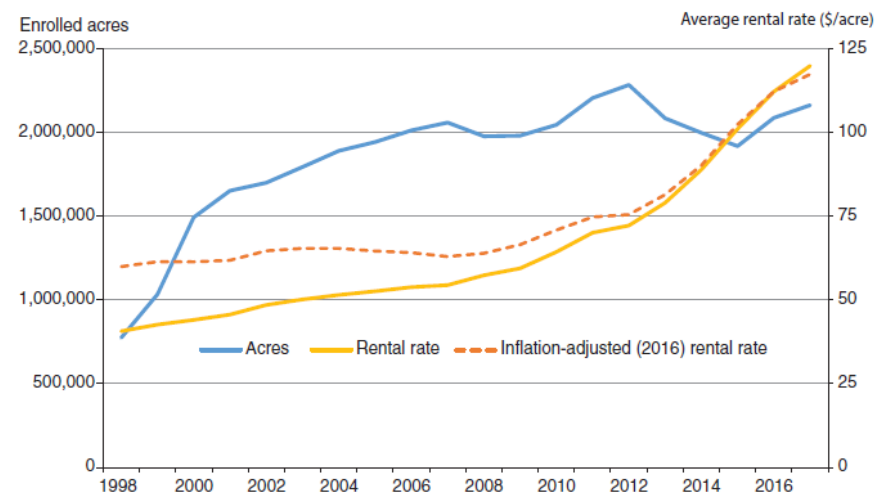
Note: The reported values are the acreage totals for each NRI survey year. The NRI is a statistical survey of natural resource conditions on non-Federal land in the United States including privately owned lands, Tribal and trust lands, and lands controlled by State and local governments.

Source: USDA, Economic Research Service using data from USDA, Natural Resources Conservation Service (NRCS), 2015.

Additionally, they report that CRP wetland acreage trended upward, despite declining enrollment overall.

Figure 3.22.1

Total wetland-related field acreage and average per-acre rental expenditures have increased substantially over time in the CRP



Note: The CPI-U Consumer Price Index is used to convert nominal values to real (adjusted for inflation) 2016 U.S. dollars.
Source: USDA, Economic Research Service using contract data from USDA, Farm Service Agency, Conservation Reserve Program (CRP).

In part, they describe natural hydrologic processes affecting wetlands: "...Over time, a natural back-and-forth shift between dryland and wetland habitat results in variations

⁴² Page 119, In: Daniel Hellerstein, Dennis Vilorio, and Marc Ribaud (editors). *Agricultural Resources and Environmental Indicators, 2019*. EIB-208, U.S. Department of Agriculture, Economic Research Service, May 2019

	in wetland acreage. Natural shifts occur, for example, when changes in groundwater levels turn dryland to wetland, or vice versa. Between 1992 and 2012, about 1.5 million wetland acres became dryland and some other type of aquatic habitat, while about 1.6 million dryland and non-wetland aquatic habitat acres became wetland...” EPA should evaluate and integrate these additional data sources and analyses (some of them appearing since the RtC3 was first drafted) in estimating wetland losses and gains with respect to the RFS in Chapter 14. Consultation with scientists working directly in these landscapes could be helpful. Critics of the use of CDL data to estimate grassland conversion in the PPR include some authors working closely with the farm communities in some of the affected states. These references do not seem to be cited by US EPA. Reitsma et al (2015)⁴³, evaluated LCLM change in SD during the period 2006 to 2012, when crop prices and net profits were exceptionally high. Pressure to increase annual crop production by converting grasslands was increased during this period. To test this, they evaluated data from the CDL and from high resolution remotely sensed images from the National Agricultural Imaging Program. They found that the CDL had large errors in assessing actual land use compared to the use of NAIP image data (see also Reitsma et al., 2016). They relied on image analysis instead. They reported that grasslands were converted to cropland during this period, while some cropland was converted to grasslands. They reported that net conversion of grasslands occurred during this period, varying greatly by region within the state (a net increase of 2.6 % of lands converted to crops, including forage crops (alfalfa) (approximately 1.2 M ac). At the state level, 9.9M ha (approximately 25M acres) of grasslands remained unchanged (Table 1, page 2366). Most land converted to cropland was of higher quality than land converted from cropland to grasslands. 20K ha (50K acres) of lands with excess water issues were converted from cropland to grassland. This suggests that some lands at least were allowed to be used as wetland habitats. Such locations may be especially beneficial to the wetland species described as of concern and endangered by EPA. They listed several factors (unranked) leading to these land conversion events: 1. Unprecedented returns to annual crops during the period, 2. changes in land ownership (owned to rented/leased use), 3. Improvements in technology, 4. Government policies, 5. Climate change (increased temperatures and precipitation), and 6. Changes in the agricultural workforce (primarily an aging farmer population less willing to maintain livestock). In a subsequent analysis, Joshi et al (2019)⁴⁴, expanded the Reitsma et al (2015) study to include Nebraska and added years up to 2014. Both studies evaluate the potential for conversion based on land capability classes with higher classes being better suited to
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⁴³ Reitsma, K.D., B.H. Dunn, U. Mishra, S.A. Clay, T. S. Sutter, and D.E. Clay. 2015. Land-use change impact on soil sustainability in a climate and vegetation transition zone. *Agron. J.* 107:2362–2372; Reitsma, K.D., Clay, D.E., Clay S. A., Dunn, B.H., and Reese, C. 2016. Does the U.S. Cropland Data Layer provide an accurate benchmark for land-use change estimates? *Agron J.* 108(1)266-272.

⁴⁴ Joshi, D. R., Ulrich-Schad, J., Wang, T., Clay, Dunn, B. H., Clay, S. A., Bruggeman, S.A., and Clay, D. E., 2019. Grassland retention in the North American Midwest after periods of high commodity prices and climate variability. *Soil Sci. Soc. Amer.*

	crop production. They found that less land in higher capability classes was converted than this measure of suitability might suggest, despite strong financial incentives for conversion. They attributed this result to farmers' desires to conserve natural features with high ecological values on their lands and that improved financial returns was overestimated as a motivational factor. They cite several other surveys of farmers' attitudes to support this conclusion⁴⁵. This suggests that current conservation programs aimed at preserving important features of the PPR have had some success in influencing land management decisions. Comparisons with Petroleum: Mixed results are reported. Some are contradictory. Horizon Scanning: Climate change will have unpredictable, and likely contradictory effects. 14.7 Synthesis Lines 976-978: It is asserted that cropland expansion was mostly from generic grasslands (88%) and that 3% was from wetlands (275K acres), largely in the prairie pothole region during 2008-2016. Lines 981-985: NRCS provides a smaller estimate for the number of wetland acres lost (24.3K ac) for the entire US from 2012 to 2017. Lines 987 -988: Since 2007, the US has lost 120.3K ac of marsh-like wetlands and gained 205.9K acres of lake-like habitats in the US. All three of these estimates are different and cover somewhat different periods and wetland classifications, and derive from different sources (See also USFWS; https://www.fws.gov/media/waterfowl-population-status-2022). These sources agree that wetlands have been lost in the most recent decades, but differ substantially in the amount of loss estimated. These discrepancies complicate EPA's ability to attribute changes to agriculture specifically and the RFS in particular. Lines 1059-1065: EPA mentions the difficulty of separating the effects of increases in precipitation in the prairie pothole region from changes in regional hydrology and habitats associated with management. Lines 1001-1004: Migratory waterfowl are thought to benefit from access to agricultural fields and have maintained or increased populations, including those dependent on seasonal wetlands for breeding. Other species have fared less well.
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⁴⁵ Doidge, M., H. Feng, and D.A. Hennessy. 2017. Farmers' motivations for land conversion in the Prairie Pothole Region of North and South Dakota. Proc. Fourth Biennial Conference on the Conservation of America's Grasslands, Fort Worth, TX. 15–17 Nov. 2017. Floress, K., S. Garcia de Jalon, S.P. Church, N. Babin, J.D. Ulrich-Schad, and L.S. Prokopy. 2017. Toward a theory of farmer conservation attitudes: Dual interests and willingness to take actions to protect water quality. J. Environ. Psychol. 53:73–80. doi:10.1016/j.jenvp.2017.06.009; Haigh, T., W. Schacht, C.L. Knutson, A. Smart, J. Volesky, C. Allen, M. Hayes, and M. Burbach. 2019. Socioecological determinants of drought impacts and coping strategies for ranching operations in the Great Plains. Rangeland Ecol. Manag. 72:561–571. doi:10.1016/j.rama.2019.01.002;

	EPA (lines 1039-1041) reports recent negative trends in habitat suitable for these same species. Lines 1021-1029: Amphibian species are thought to have declined due to habitat loss during this period and may be the most vulnerable class of organisms to changes in hydrologic structures. Lines 1009-1011: Increased water management and drainage of farmland is correlated with increased precipitation on farmland in the prairie pothole region. Changes in the character of water features have resulted. Lines 1013-1020: Data from Iowa on pesticide detection is reported, but most of the chapter focuses instead on the prairie pothole region.
Landis	2a. For those topics that are covered, I have a favorable impression of the clarity and technical accuracy of the materials presented in this chapter. However, landscape-scale effects of agriculture on wetland ecosystems requires increased coverage. The authors have done a very good job of explaining the background behind each concept/factor they covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results. 2b. Recent studies of the impacts of agricultural landscape structure on wetland biodiversity and function are listed below. • Čapkun-Huot, C., Fyson, V.K. and Blouin-Demers, G., 2021. Landscape composition predicts the local abundance of painted turtles (<i>Chrysemys picta</i>). <i>Herpetology Notes</i>, 14, pp.215-223. • Eric, A., Chrystal, M.P., Erik, A., Kenneth, B. and Robert, C., 2022. Evaluating ecosystem services for agricultural wetlands: a systematic review and meta-analysis. <i>Wetlands Ecology and Management</i>, 30(6), pp.1129-1149. • Hambäck, P.A., Dawson, L., Geranmayeh, P., Jarsjö, J., Kačergytė, I., Peacock, M., Collentine, D., Destouni, G., Futter, M., Hugelius, G. and Hedman, S., 2022. Tradeoffs and synergies in wetland multifunctionality: A scaling issue. <i>Science of the Total Environment</i>, p.160746. • Mitchell, B.J., Terry, C.V., Ringelman, K.M., Kemink, K.M., Anteau, M.J. and Janke, A.K., 2023. Wetland occupancy by duck broods in cropland-dominated landscapes of the United States Prairie Pothole Region. <i>The Journal of Wildlife Management</i>, p.e22347. • Mitchell, M.E., Shifflett, S.D., Newcomer-Johnson, T., Hodaj, A., Crumpton, W., Christensen, J., Dyson, B., Canfield, T.J., Richmond, S., Helmers, M. and Lemke, D., 2022. Ecosystem services in Iowa agricultural catchments: Hypotheses for scenarios with water quality wetlands and improved tile drainage. <i>Journal of Soil and Water Conservation</i>, 77(4), pp.426-440. • Préau, C., Tournebize, J., Lenormand, M., Alleaume, S., Boussada, V.G. and Luque, S., 2022. Habitat connectivity in agricultural landscapes improving

	multi-functionality of constructed wetlands as nature-based solutions. <i>Ecological Engineering</i>, 182, p.106725. - Roberts, H.P., Willey, L.L., Jones, M.T., Akre, T.S., King, D.I., Kleopfer, J., Brown, D.J., Buchanan, S.W., Chandler, H.C., deMaynadier, P. and Winters, M., 2023. Is the future female for turtles? Climate change and wetland configuration predict sex ratios of a freshwater species. <i>Global Change Biology</i>. - Wiegman, A.R., Myers, G.H., Augustin, I.C., Kubow, M.L., Fein-Cole, M.J., Perillo, V.L., Ross, D.S., Diehl, R.M., Underwood, K.L., Bowden, W.B. and Roy, E.D., 2022. Potential for soil legacy phosphorus release from restored riparian wetlands within an agricultural landscape. <i>Biogeochemistry</i>, 161(2), pp.137-156. Line 143 new refence on the relative water use of biofuel crop plants - Hamilton, S.K., Hussain, M.Z., Bhardwaj, A.K., Basso, B. and Robertson, G.P., 2015. Comparative water use by maize, perennial crops, restored prairie, and poplar trees in the US Midwest. <i>Environmental Research Letters</i>, 10(6), p.064015. Lines 893-897 See below for new conservation focused practices aimed largely at improving water quality - DeLong, C., Lindahl, C. and Johnson, K., 2021. Leading at the edge: A roadmap to advance edge of field practices in agriculture. <i>Journal of Soil and Water Conservation</i>, 76(2), pp.41A-43A. - TNC. 2021. Edge of Field Practices in Agriculture. https://www.nature.org/en-us/what-we-do/our-priorities/provide-food-and-water-sustainably/food-and-water-stories/farming-for-water-wildlife/ - Schulte, L.A., Niemi, J., Helmers, M.J., Liebman, M., Arbuckle, J.G., James, D.E., Kolka, R.K., O'Neal, M.E., Tomer, M.D., Tyndall, J.C. and Asbjornsen, H., 2017. Prairie strips improve biodiversity and the delivery of multiple ecosystem services from corn–soybean croplands. <i>Proceedings of the National Academy of Sciences</i>, 114(42), pp.11247-11252. - Craig, A.J., Rehmann, C.R., Alt, L.M., Schulte, L.A. and Soupir, M.L., 2022. Prairie strips reduce fecal indicator bacteria concentrations in simulated runoff. <i>JAWRA Journal of the American Water Resources Association</i>. - Hladik, M.L., Bradbury, S., Schulte, L.A., Helmers, M., Witte, C., Kolpin, D.W., Garrett, J.D. and Harris, M., 2017. Neonicotinoid insecticide removal by prairie strips in row-cropped watersheds with historical seed coating use. <i>Agriculture, Ecosystems & Environment</i>, 241, pp.160-167. 2c. Not applicable to this chapter 2d. In general, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. However, at minimum I
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	<u>recommend including a review of the literature on the effects of changing agricultural landscape complexity on wetland ecosystem health and biodiversity.</u> I suspect this could lead to a shift in the language of the Chapter conclusions. 2e. The chapter does a good job identifying uncertainties and limitations associated with its conclusions. 2f. The graphics, glossary and supporting materials are helpful in clarifying and supporting the conclusions of the chapter. Figure 14.2 is especially helpful in pointing out landscape structural effects on wetlands and aquifer regeneration.
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3.3.1.8 Chapter 15: Invasive and Noxious Plant Species

Reviewer	Comments
Davis	2a. This chapter text was clear, but the content reviewed topics that were not fully resolved. The assumption about corn and soybean not having an effect on invasiveness was not entirely consistent with the assertion that herbicide-resistant weeds will continue to grow (e.g. lines 24-26 seemingly conflict with lines 34-36 of the Key Findings). There is no new analysis of biofuel impacts on invasive plants included, which seems inconsistent with the priority recommendations from the last triennial review (RtC2), as stated in lines 213-215. The discussion about sorghum was confusing. Is it, or is it not an advanced biofuel according to the RFS? The discussion about Miscanthus x giganteus and Panicum virgatum leaves the impression that these could be classified as invasive, and require formalized plans for RRR (lines 361-372), but there is also body of literature suggesting some varieties are not invasive. Do the authors propose to develop a RMP for these advanced biofuel feedstocks to reduce risk? The list of advanced biofuels labeled under the RFS is short, so this topic warrants more detailed discussion. In section 15.1.3, it may be useful to add that chemical applications are also addressed in Chapter 12. The section on conservation practices, 15.3.4 – line 263, identifies the sole reliance on corn, soybean, FOGs, and imported sugarcane for biofuel feedstock as a way to avoid negative consequences. I disagree with this and think it would be best to remove this sentence. Instead, I suggest expanding on the point made in lines 270-273 about adopting field and farm-scale practices the protect herbicide susceptibility. 2b. I recommend a more detailed discussion of the literature than was presented in some sections.

	For example, Quinn et al. (2015) is a reference that is included in the chapter, but the report does not address the listing and criteria for low risk of invasion described in that study. 2c. No new analysis was provided, but there is opportunity for new analysis. Starting on lines 491-514, the authors describe a tiered weed assessment that could serve as a framework for evaluating invasion risk of biofuel feedstocks. 2d. The bullet point starting on line 559 is a bit redundant with the second bullet point (line 537). The research recommendations highlight the need for a new weed risk assessment tool to allow for more informed decisions. Is it possible to apply the analysis of what is needed for the WRA directly to available data to demonstrate the value of the new tool envisioned here? 2e. The chapter identified a need for research on novel feedstocks that pose an invasion risk. It would be beneficial to add the need for research on feedstocks that are expected to be <i>less</i> invasive too. 2f. Figure 15.2 was very useful in clarifying the status of herbicide-resistant species. There is a need for a few more terms to be defined, including RRR, WRA, RMP.
Landis	2a. For those topics that are covered, I have a favorable impression of the clarity and technical accuracy of the materials presented in this chapter. The authors have done a very good job of explaining the background behind each concept/factor they covered and why they are potentially important. The report strives to provide a balanced view of the available data and modeling results. 2b. The development of viable seed-bearing seed forms of <i>Miscanthus</i> spp. poses an enormous risk of invasiveness that does not receive sufficient emphasis in this chapter. • Anderson, E.K., Lee, D., Allen, D.J. and Voigt, T.B., 2015. Agronomic factors in the establishment of tetraploid seeded <i>Miscanthus</i>× <i>giganteus</i>. <i>GCB Bioenergy</i>, 7(5), pp.1075-1083. • Bonin, C.L., Mutege, E., Snow, A.A., Miriti, M., Chang, H. and Heaton, E.A., 2017. Improved feedstock option or invasive risk? Comparing establishment and productivity of fertile <i>Miscanthus</i>× <i>giganteus</i> to <i>Miscanthus sinensis</i>. <i>Bioenergy Research</i>, 10, pp.317-328. In general, the Overview section seems a bit dated. Recent relevant literature includes: Lines 104-106. Recent estimates of the cost of invasive species, and cost to eradicate invasive <i>Miscanthus</i>.

	• Diagne, C., Leroy, B., Vaissière, A.C., Gozlan, R.E., Roiz, D., Jarić, I., Salles, J.M., Bradshaw, C.J. and Courchamp, F., 2021. High and rising economic costs of biological invasions worldwide. <i>Nature</i>, 592(7855), pp.571-576. • Lowry, C.J., Matlaga, D.P., West, N.M., Williams, M.M. and Davis, A.S., 2022. Estimating local eradication costs for invasive <i>Miscanthus</i> populations throughout the eastern and midwestern United States. <i>Invasive Plant Science and Management</i>, 15(3), pp.115-121. Lines 333+. Risk of polyploid species as bioenergy crops • Lambertini, C., 2019. Why are tall-statured energy grasses of polyploid species complexes potentially invasive? A review of their genetic variation patterns and evolutionary plasticity. <i>Biological Invasions</i>, 21(10), pp.3019-3041. Lines 352-372. Recent papers on potential pathways for <i>Miscanthus</i> invasion, impacts on biodiversity. • Flint, S.A., Shaw, R.G. and Jordan, N.R., 2021. Effects of selection regime on invasive characteristics in an emerging biomass crop, switchgrass (<i>Panicum virgatum</i> L.). <i>Sustainability</i>, 13(9), p.5045. • Perrier, A., Hardion, L., Rozan, A., Staentzel, C. and Combroux, I., 2019. <i>Miscanthus x giganteus</i> crop fields hide a genotype of the invasive <i>M. sacchariflorus</i>. <i>Weed Research</i>, 59(6), pp.446-457. • Turnage, G., Byrd, J.D. and Madsen, J.D., 2022. <i>Miscanthus x giganteus</i> growth and control in simulated upland and wetland habitats. <i>Invasive Plant Science and Management</i>, 15(1), pp.25-32. • Williams, M.A. and Feest, A., 2019. The effect of <i>Miscanthus</i> cultivation on the biodiversity of ground beetles (Coleoptera: Carabidae), spiders and harvestmen (Arachnida: Araneae and Opiliones). <i>Agricultural Sciences</i>, 10(7), pp.903-917. Also, in regard to herbicide resistance Lines 120-121 and 261-263 Herbicide resistant weeds are causing some farmers to reduce use of no tillage which we know has proven conservation benefits. • Van Deynze, B., Swinton, S.M. and Hennessy, D.A., 2022. Are glyphosate-resistant weeds a threat to conservation agriculture? Evidence from tillage practices in soybeans. <i>American Journal of Agricultural Economics</i>, 104(2), pp.645-672. • Clapp, J., 2021. Explaining growing glyphosate use: The political economy of herbicide-dependent agriculture. <i>Global Environmental Change</i>, 67, p.102239. 2c. Not applicable to this chapter
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	2d. In general, I find that the conclusions and recommendations described in the chapter synthesis are supported by the material presented. 2e. The chapter does a good job identifying uncertainties and limitations associated with its conclusions. However, I feel the mounting evidence of the potential invasiveness of <i>Miscanthus</i> and the potential costs to control it should be emphasized. 2f. The graphics, glossary and supporting materials are helpful in clarifying and supporting the conclusions of the chapter.
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3.3.1.9 Chapter 16: International Effects

Reviewer	Comments
de Gorter	2a. I am concerned about the logic used in analyzing the international effects of increased demand for ethanol or BBD in the United States. If increased ethanol causes an increase in the U.S. corn price, corn prices go up everywhere in the world. The environmental effects of increased corn production in the rest of the world is part of the international effects on the environment. If corn prices rise, so too do soybean prices because marginal profits in corn production must be equal to that in soybean production. If you plotted the price of soybeans over the price of corn (\$/bu), the average is a flat line and equal to 2.5 (using Chicago prices). If corn and soybean prices rise, so too for wheat, rice, and other coarse grains. See the table in Box 1.3 in The Book. The emphasis is on the international environmental effects of U.S. biofuel consumption in changing U.S. trade in biofuels. That is only one avenue in which U.S. biofuels affects the environment outside its borders. 2b. The EPA Report should analyze the effects of U.S. biofuel consumption on biofuel prices (and how that impacts world biofuel production) and on feedstock prices and how that impacts world feedstock prices. The environmental effects would then have to be derived. Searchinger (2008) started this whole process. I suppose the huge models calculating the life-cycle effects of GHGEs due to U.S. biofuel consumption would include the changes in world biofuel and feedstock production, and one could derive the non-GHGE environmental effects therefrom. It would be a very ambitious research agenda. 2c. See my comments above [now below] on this page [table]. 2d. The Synthesis concludes “Attribution of international effects from the RFS Program remains challenging due to complex interrelationships among other major drivers of observed change.” This is true. It would take a lot to determine the environmental effects in the rest of the world due to U.S. biofuel consumption.

	It then goes on: <i>International environmental effects that are clearly attributable to the RFS Program due to U.S. ethanol and biodiesel imports could not be quantified</i> I would degree with that. It then goes on to say: <i>Imports—a mechanism for international effects identified in Section 204—have fallen drastically since peaking before the RFS Program in 2004–2006.</i> Are biofuel imports the only mechanism for international effects we are allowed to assess the international environmental effects of, as per Section 204? If I look at page 5.1 of RtC1 (2011), it states: <i>...calls for EPA to report to Congress on the environmental impacts outside the United States caused by U.S. biofuel use. Therefore, this chapter focuses on potential impacts in foreign countries from implementation of the revised Renewable Fuels Standard program (RFS2) standards. Specifically, this chapter presents the current international production and consumption of two biofuels (ethanol and biodiesel), discusses the composition of future biofuel production, examines current and projected import and export volumes, and discusses the environmental and other impacts of direct and indirect land use changes.</i> <i>International trade is the primary mechanism through which U.S. biofuel policy will affect foreign nations. Ethanol and, to a much smaller degree, biodiesel, have become global commodities. Both are produced in many countries (Figure 5-1) and both are traded in international markets. Primary producers of ethanol are Brazil, the United States, the European Union, India, and China. Brazil is the only significant exporter of ethanol (See Table 5-1). Based on computer modeling, changes in U.S. production and consumption volumes, such as those in RFS2, are predicted to result in land allocation impacts that have global ramifications through international trade and market price. As a crop price rises, land may be reallocated to grow more of that crop in response to market price; conversely, as a crop price declines, land may tend to be reallocated to grow less of that crop in response to market price.</i> It starts out as if trade in biofuels is the only mechanism but then states: <i>changes in U.S. production and consumption volumes, such as those in RFS2, are predicted to result in land allocation impacts that have global ramifications through international trade and market price. As a crop price rises, land may be reallocated to grow more of that crop in response to market price; conversely, as a crop price declines, land may tend to be reallocated to grow less of that crop in response to market price.</i> It explicitly states here that if the price of corn rises in the rest of the world due to the RFS2, it will have international environmental effects. Going back to the current report,
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	<i>The hypothesis that U.S. demand for sugarcane ethanol attributable to the RFS Program played a significant role in the observed changes in Brazil's ethanol production and associated environmental effects is not supported by available evidence.</i> That is true. It even does not even make logical sense, even without empirical evidence. If Brazil imports ethanol, it is due to their own policies or markets. 2e. Yes, it does a good job identifying the uncertainties and limitations. And in the list of recommendations, it includes the statement: <i>Further analysis of where, when, and why land managed for agriculture is changing, and understanding how biofuel policy may interact with the causes for such increases (e.g., changes in land use associated with changes in U.S. exports of commodities such as corn and soybeans), are important topics to address uncertainties associated with current land use change models</i> It recognizes that U.S. biofuel consumption increase world prices of corn and soybeans, for example, that should be a priority for further research. Comments on Chapter 16 - International Effects Lines 13-16 <i>Attribution of international effects from the RFS Program remains challenging due to complex interrelationships among other major drivers of observed change. There are relatively few studies on this topic specifically, though many on international effects from biofuels more generally</i> Just use your attribution of increased corn production to the RFS2 and scale down the results of the “many on international effects from biofuels more generally”. <i>International environmental effects that are clearly attributable to the RFS Program due to U.S. ethanol and biodiesel imports could not be quantified.</i> The international effects of the RFS or any reason for an increase in ethanol or BBD consumption in the U.S. is not reflected through trade in biofuels only. It is reflected in the increase in corn and soybean prices worldwide. If the increase in ethanol consumption increases corn prices, then it increases corn prices worldwide. It is a world market. Ditto soybeans and BBD. Ditto ethanol. Line 22 <i>Imports—a mechanism for international effects identified in Section 204</i> It is “a” mechanism or is it the only mechanism we are allowed to assess? Lines 28-31 <i>The hypothesis that U.S. demand for sugarcane ethanol attributable to the RFS Program played significant a role in the observed changes in Brazil's ethanol production and associated environmental effects is not supported by available</i>
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evidence. Ethanol production in Brazil has been supported by domestic policies in Brazil for decades

There is a world market for ethanol. When Brazil or the U.S. increases ethanol consumption (for whatever reason), the world price goes up. Ethanol production in both Brazil and the U.S. increases, whether the demand increase came from Brazil or the U.S. It is not a “sugar-cane production mandate” in Brazil. It is an ethanol demand mandate and imported ethanol can be used. Likewise in the U.S.

Regardless of the source in the change in demand for ethanol, the “*role in the observed changes in Brazil’s ethanol production and associated environmental effects*” is the same if it comes from increased demand in the U.S. or Brazil.

The same logic applies to palm oil. An increase in BBD demand using soybean oil means palm oil will fill the void (see Figure 5 – the relative prices are flat and will continue to be so because on the margin, you can use palm oil for biodiesel or for other uses).

So being an importer or exporter does not really matter. As world vegetable oil prices rise, there are land use changes everywhere. Same for ethanol. An increase in a billion gallons of ethanol consumption due to a U.S. mandate will have the same land use changes worldwide if Brazil increases ethanol consumption by a billion gallons due to their mandate. It is a consumption mandate, not a mandate to produce domestic biofuels.

The fact that the U.S. exports ethanol means it is efficient and minimizes total world resources. If it was imported, it means the rest of the world is more efficient and minimizes world resource use.

Lines 36-37

Seasonal, interannual, two-way ethanol trade with Brazil appears to benefit both nations.

I disagree here. Ethanol is identical product, regardless of source. The reason for two-way trade is California’s LCFS. It causes shuffling – lower GHG ethanol is imported and higher GHG ethanol is exported with no net effect on ethanol price. Just a waste of resources having two-way trade. There are several other products where the LCFS causes shuffling.

Figure 16.2

It may be more informative to put in miles equivalent as well. Ethanol gets 70% of the miles per gallon relative to gasoline. Diesel gets 35% more mpg than gasoline, and BBD 92% of the mpg of diesel.

Lines 112-118

*...provide ethanol to the United States and to make up for lost U.S. exports of corn and soybeans. **Yes, this happens. If the price of corn and soybean go up in the U.S., they did so worldwide** This chapter reviews empirical evidence relevant to the linkages with international environmental effects **yes, increase corn and soybean prices increases environmental effects at home and abroad to facilitate analysis of***

the potential relationship among imports and exports with the RFS Program.
Imports and exports of what? Biofuels only? That is too narrow of a focus.

The data show that in the years following passage of EISA, trends in biofuel production and trade evolved differently than some models predicted. For example, exports of commodities including soybeans remained strong,

Does not prove biofuels did not have a big impact on corn and soybean prices. U.S. exports of soybean oil dropped by 70% from its peak in 2010. And U.S. market share of total world soybean trade has dropped precipitously since 2006. Besides, BBD encourages more oil intensive vegetable crop production like palm oil, canola, and rapeseed oil (higher soybean oil prices relative to soybean meal can cause that).

and the United States became a net exporter of biofuels (not importer), driven primarily by ethanol exports in recent years.

The U.S. exports ethanol because the price of ethanol is higher in the rest of the world.

Box 16.1

Let me revise your Box 16.1.

1.1 Effects of biofuel imports to the U.S.:

RFS Program increases domestic biofuel demand. This increases the price of the biofuel. This increases the price of the feedstock (for corn-ethanol, see equation [1] in [deGorter Summary Biofuel Theory](#) – for equation [1] analogue for soybean oil-biodiesel, see papers listed on bottom of p. 3 in [deGorter Summary Biofuel Theory](#)). *Ceteris paribus*, corn and soybean exports decline and corn ethanol and biodiesel exports decline (or imports increase if importer). Foreign corn and soybean prices and production increase, part of the production increase will be used in foreign biofuel production to partially offset the decline in U.S. biofuel exports or increase in biofuel imports.

These chain of events are the primary drivers of the environmental effects of an increase in mandate biofuel consumption in the United States.

Note: the increase in mandated biofuel consumption in the United States does not increase biofuel exports. For that to occur, one would have to have a dynamic story where previous policies or market developments had forced excess capacity in say ethanol production (the RFA claims 17 billion ethanol production capacity currently in the U.S.; there was always huge excess capacity in biodiesel production). And because previous events paid for fixed costs, it pays to export because the mandate had generated excess capacity, lowered production costs (and from learning by doing, etc. – like some external economies were generated).

It is stated in Box 16.1:

environmental effects associated with where and how changes in global production occur.

That is true. But there is one world price of ethanol. A one-billion-gallon increase in mandated corn ethanol in the United States has identical environmental effects as a

one-billion-gallon increase in mandated ethanol in Brazil (including land use change effects.). These are consumption mandates for a biofuel; not production mandates for a specific source of a biofuel.

Lines 178-179

Agricultural extensification and deforestation have been documented in countries that are major exporters of biofuels to the United States, including Brazil, Argentina, and Indonesia.

Two observations:

First, biofuel mandates in Europe and the United States can impact the price of corn and soybeans in Brazil, Argentina, and Indonesia without having a major change in bilateral trade in biofuels; it directly impacts the price of corn, soybean oil and soybeans, and trade between other countries. Hence, biofuel mandates in Europe and the United States directly impact agricultural extensification and deforestation in these developing countries without necessarily changing the bilateral trade in biofuels that much.

Second, the agricultural extensification and deforestation in these developing countries are not just due to their biofuel production; it is equally due to U.S. and European mandates. See my earlier paragraph where there is one world market price for biofuels.

Lines 182-184

but significant uncertainty remains about the amount and type of land use changes that can be quantitatively attributed to U.S. biofuel consumption.

We just established it does not matter if it is a U.S. or Brazilian ethanol consumption mandate.

So, the rest of Chapter 16 puts too much emphasis on trade in biofuels as the final indicator of environmental effects of mandates.

Lines 536-537

However, U.S. biofuel exports cannot be directly attributed to the RFS Program because they do not generate RINs.

U.S. biofuel exports cannot be attributed to the RFS for the reasons I stated above. However, it has nothing to do with the fact that exports do not generate RINs. An ethanol producer sells ethanol on the open market – they are not obligated parties. Refiners are and they have nothing to do with ethanol production. Refiners buy RINs from blenders and blenders use the proceeds to discount the price of ethanol to consumers in both PE10 and PE85. This increases consumption of ethanol. The higher the D6 RIN price, the higher ethanol consumption is, *ceteris paribus*. So, RIN prices reduce exports?

Lines 596-598:

U.S. import tariff of 341% on Indonesian biodiesel exports! Indonesia puts export taxes that raise the world price; how do they subsidize up to 341%?

Pages 16-27 to 16-40

There is too much emphasis on how using palm oil for biodiesel will have very high negative environmental effects compared to using soybean or rapeseed oil.

There are several things one must keep in mind.

First, the substitution between vegetable oils is very high. Look at Figure 5. The relative palm/soybean oil price is flat all through history. On the margin, they are obviously highly substitutable in biodiesel, industrial or even food consumption. How else can you explain the flat line in Figure 5?

The implication is if you use soybean oil for biodiesel, palm oil will be used to replace that soybean oil (not 100% of it as prices go up and total vegetable oil consumption declines).

This means the environmental effects of using soybean oil instead of palm oil does not have the reduction in palm oil production you expect, or the environmental savings you expect.

In fact, a gallon of biodiesel uses much less land if palm oil is used. See this [paper](#) for some more ideas and context for the issues. Palm oil uses a lot less land per unit BBD. But as I note, the substitution between vegetable oils is so high, it really does not matter that much which vegetable oil is used in biodiesel; the vegetable oils are simply re-allocated to the very different uses, including biodiesel in other countries. So, your Table 16.1 is not that informative.

The EPA has no pathway for palm oil? See this [paper](#). Or this [paper](#).

Lines 658-661

Indonesian biodiesel production and exports appear to be highly dependent on trade and domestic policies. When the sector is challenged by declines in export markets or other adverse circumstances, the government of Indonesia has supported the sector through various policy mechanisms that either support exports (e.g., lowering export levies), domestic demand (e.g., blending mandates), or lower production costs (e.g., direct subsidies).

You list three policies:

1. Lower export levies
2. Increase domestic consumption mandates
3. Lower production costs through direct subsidies

	The first two cause biodiesel exports to decline (and the third policy justifies a 341% import tariff from the U.S.?). How do we not know that the first two policies listed tax exports more than the third policy subsidizes biodiesel exports from Indonesia? Lines 679-680 <i>Since deforestation associated with palm oil is not strictly a biodiesel phenomenon ..., it is expanded on here as an area of potential concern.</i> The discussion above shows it does not really matter that much because vegetable oils are highly substitutable, and a gallon of vegetable oil from palm uses much less land. Lines 700-703 <i>Palm oil has been linked with a number of environmental impacts, including tropical deforestation, forest fires, methane emissions, and peat soil degradation. While palm oil is not produced in the United States, concerns have been raised about the possibility that the RFS Program is contributing to palm oil expansion and its related environmental effects</i> Of course. Using soybean oil for BBD in the U.S. will increase the soybean oil price⁴⁶ and so too palm oil prices (Figure 5 shows the relative palm oil/soybean oil price is flat). Higher palm oil prices mean higher palm oil production. Line 831 <i>16.5.3 Attribution of Palm Oil Expansion to the RFS Program and U.S. Biofuel Consumption</i> Basically, whatever soybean oil is used for BBD consumption in the United States, it reverberates throughout the world and palm oil prices increase by the same percentage as soybean oil prices (see Figure 5). You do not need an approved pathway for palm oil for this to happen. That is why I argue there is too much emphasis on trade in palm oil BBD as the only mechanism by which the U.S. RFS could impact the environment in Indonesia and Malaysia. Lines 844-845 <i>the extent palm oil is a substitute for those vegetable oils, the RFS Program may indirectly increase the demand and price for palm oil globally. The evidence related to this second mechanism is reviewed in this section in greater detail.</i> There is nothing the EPA Report can write to conclude that palm oil is not a very high “substitute for those vegetable oils” and so yes, increased BBD consumption will <u>directly</u> “increase the demand and price for palm oil globally” Lines 848-852 <i>When soybean oil prices increase relative to other vegetable oils, consumers who can, may shift some of their consumption to other oils such as canola, corn, peanut,</i>
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⁴⁶ Look at Figures 3 and 4. It is quite dramatic.

sunflower and palm oil. Palm oil tends to be the lowest cost vegetable oil globally but local market prices vary. In addition, palm oil is not a perfect substitute for food uses of soybean oil as it has different cooking and taste characteristics

True. See EU data in the mid-2000s when huge amounts of rapeseed oil was used for BBD. Imports of palm oil sky-rocketed and were used for food. But Figure 5 shows prices keep a constant percentage difference so on the margin, uses such as biodiesel or industrial uses make palm oil high substitutes to other vegetable oils (but at the fixed percentage difference in price). It may not be “a perfect substitute for food uses of soybean oil as it has different cooking and taste characteristics” but vegetable oils have a lot of other uses and why is the line flat in Figure 5?

Line 860-862:

They found a positive and statistically significant relationship between palm oil imports and the price of soybean oil whereby a 10% increase in the price of soybean oil would have caused a 12.3% increase in the supply of palm oil to the United States

Fine but what is the price-to-price elasticity? Can the study rebuke Figure 5 and conclude it will cause palm oil prices to deviate from that line permanently? And that only random market shocks cause the relative palm oil price to coincidentally and only by chance end up on that line at some future date? Who would believe that? This is the type of analysis required.

Lines 868-871

It makes no sense to me. If soybean and palm oil prices increase the same amount in percentage terms (the ratio is constant), then importing palm oil may not be directly related. Vegetable oils move all around the world. The price effects determine the increase in palm oil production.

Lines 872-875

For illustrative purposes, the U.S. EPA (2010) modeling estimates suggest that for every one billion gallon increase of U.S. soybean production in 2022, the soybean oil price increases by 47% (FASOM) or 31% (FAPRI) depending on the model used

I simulated a one billion increase in biodiesel demand using the model in

- Drabik, Dusan, Harry de Gorter, and Govinda R. Timilsina. 2014. “The Effect of Biodiesel Policies on World Biodiesel and Oilseed Prices.” *Energy Economics* 44: 80—88.

But assuming no trade and I get a soybean oil price increase of 44% but with a demand elasticity of -0.30 for soybean oil and -0.4 for soybean meal and a supply elasticity for soybeans of 0.30.

With trade, I expect these demand elasticities to be much much more elastic so the price increases of FASOM and FAPRI are far too high. And the implied increase in palm oil imports too high too, especially when all vegetable oil markets get a chance to adjust. See Figures 3 and 4: the share of soybean oil does not stay high as substitution of other vegetable oils take place worldwide. And the relative production of vegetable

oil intensive crops increases, because of the increase in soybean oil prices causes all other vegetable oil prices to increase as well. This happened in Canada and Europe after the first increase in soybean oil prices around 2008.

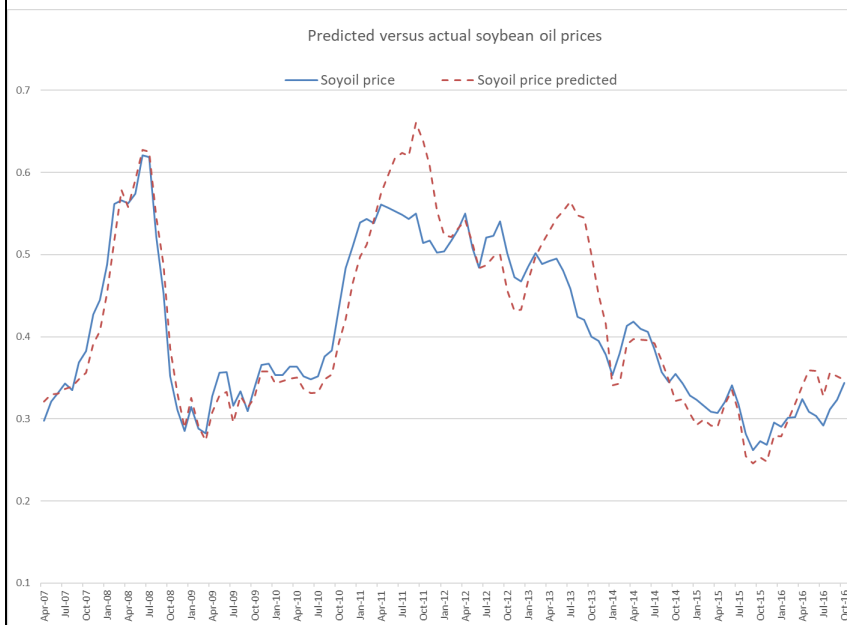
Lines 883-885

34% of soybean oil used in BBD will be backfilled with palm oil imports.

I would not expect it to be this high in terms of imports (but did it for Europe in the mid-2000s?). We should be finding out now that there is a big push for renewable diesel (RD). In fact, here is an [analysis](#) asking why soybean oil prices are twice the price of palm oil in 2021? Also note the gap between Argentina/Brazil soybean oil prices and that in the United States. Due to import tariffs but

But please note that most (not all, depending on the elasticity of world demand for vegetable oils) of the increase in soybean oil consumption as BBD in the U.S. will be backfilled world-wide by increases in consumption of all other vegetable oils in the United States and in the rest of the world (the effect on palm oil prices and production does not necessarily have to be reflected in increased U.S. imports of palm oil; it could be increased imports of other oils or reduced exports of soybean oil, and increase domestic consumption of other vegetable oils).

Here is the way to predict soybean oil prices – use the analog of equation [1] for ethanol but apply to soybean oil-biodiesel prices.



Lines 903-905

Soybean end use data from USDA do not suggest that U.S. soybean oil exports were reduced and diverted to domestic uses, as exports have remained steady aside from market year 2020/2021 (section 3.2.1.1 and Figure 3.14).

Here is a graph of U.S. soybean oil exports.



Lines 925-930

The authors evaluated a range of assumptions about the flexibility of substitution (elasticity) between vegetable oils given the relatively small amount of empirical evidence in this area.

I think my Figure 5 contains a lot of empirical evidence.

They found that the inclusion of other potential sources of substitution (other vegetable oils and fats), and the choice of elasticity value used in model simulations, had a large influence on the resultant palm oil demand. This, in turn, has important implications for interpreting other model outputs including estimates of land use change and simulated "backfill."

Yes, it will and with vegetable oil prices following each other so closely over time, the land use change from an extra billion gallons of BBD demand in Indonesia should have very

Hill

2a. Overall, this chapter is unclear and in part technically inaccurate, including the text dependent on the analyses performed in Chapter 6.

2b. The conclusion in Lines 28-31 is not supported, and the use of the word "significant" is ambiguous. The interpretation in Lines 100–107 is a major problem as noted in my answer to Charge Question 4. The statement in Lines 239–240 is speculative and can easily be explained as trade efficiency. The statement in Lines 1001–1003 is not supported by the data provided.

The most glaring omission in this chapter is the lack of detailed treatment of potential market mediated effects. These are noted in 1.3 of Box 16.1 but are largely unexplored in this chapter. This omission is also notable because in 2.4, it is stated that "EPA interprets "impacts" to include both effects directly influenced by the RFS Program (e.g., by the cultivation of feedstocks for use as a biofuel as a

	result of the RFS) as well as those indirectly influenced by the RFS Program (e.g., the displacement of other crops by corn, leading to greater cultivation of those crops elsewhere to meet the market gap). These market mediated effects must be shown here in RtC3. 2c. No. The study cited in Lines 919–925 needs further consideration. As noted in the recent NASEM report, “Current Methods for Life Cycle Analyses of Low-Carbon Transportation Fuels in the United States”, the GTAP-BIO model does not allow for natural forest or grasslands to be converted to agricultural production (See report Fig. 9-1, and Plevin et al. 2022; J. Clean. Prod.). 2d. Those conclusions and recommendations based on Chapter 6, or in relation to the points I made above, are unsupported by the material presented. 2e. Uncertainties and limitations receive only a cursory treatment in this chapter. This is a point for improvement. 2f. Yes
Smith	2a. The chapter is clearly written and appears to be accurate in what it presents. 2b. Yes. I agree with the authors that there are a lot of uncertainties in the international effects. The largest potential effects are the indirect effects – the RFS changes agricultural commodity prices, which causes international changes in production and the resultant environmental effects. These are small only if the RFS had small effects on ethanol production (as claimed in Chapter 6). 2c. I understand the comment that “international market-mediated effects are not substantively addressed in this report due to time and resource constraints, and this remains a priority for future study” (page 16-22). However, I think the chapter needs to at least summarize the large literature on indirect land use change. 2d. Yes 2e. Yes 2f. Yes

3.4 All Chapters

3.4.1 Question 3: Considering all chapters you reviewed, as well as the background chapters in Part 1, does the report address the requirements of Section 204 as set forth in Chapter 1 and as reflected in the scope set forth in Chapter 2? If not, please describe what should be added, removed, or changed.

Reviewer	Comments
Davis	Generally, the report addresses the requirements, but there is a strong emphasis placed on the most abundant feedstocks that are more difficult to distinguish as direct effects of the RFS because of their relationship to other markets/influences/policies. The analysis in the report is weakened by the lack of information describing advanced biofuels that are more directly and exclusively linked to the RFS mandates.
Engel	Based on my understanding of Section 204, the report addresses the requirements of Section 204. There are no suggestions for addition or removal of information to address Section 204.
Hill	All Chapters (Charge Question 3) No. The report is missing important national and international effects, such as fuel market effects and international market-mediated effects. As stated in Section 2.4, these must be included. Also, I believe that EPA's interpretation of "environmental impacts outside the United States" in Section 204, as stated in Section 2.4 Lines 312–317, is incorrect. EPA's argument is that the placement of the requirement after (a)(1)–(3) implies that the scope of environmental effects to be considered is greater for domestic impacts than for international impacts. This, however, is inconsistent with the wording of Section 204, which does not in any way specify a difference in how domestic or international effects are treated. It simply explicitly states that international effects should be considered. Also, the placement of the sentence "The report shall include the annual volume of imported renewable fuels and feedstocks for renewable fuels, and the environmental impacts outside the United States of producing such fuels and feedstocks." is right before the sentence, "The report required by this subsection shall include recommendations for actions to address any adverse impacts found." This latter sentence has been interpreted by EPA to include all the impacts in (a)(1)–(3), even though its placement is after the specific language on international impacts, which EPA does not interpret to include all the impacts in (a)(1)–(3).
Hoekman	All Chapters (Q3) For the most part, RtC3 does address the requirements laid out in Section 204 of EISA regarding the environmental and resource conservation impacts to date of the RFS Program. The requirement to address "likely future impacts" is minimally satisfied. Clearly, there are many uncertainties about the future of the RFS Program itself, as well

	as other economic and policy drivers impacting the growth of biofuels in the future. The RtC3 generally avoids these uncertainties by considering future impacts only out to the year 2025. While this does satisfy the letter of the law, it misses an important opportunity to seriously assess much more significant impacts of biofuels (both positive and negative) that are possible in the more distant future (10-20 years out.) Section 204 also requires that the report include an assessment of “... the environmental impacts outside the United States of producing such fuels and feedstocks.” This requirement is largely met in Chapter 16 (International), but some additional assessment and discussion would be helpful. In particular, the issue of indirect land use change (ILUC) should receive greater attention. It is now clear that early assessments, which were performed over a decade ago, greatly exaggerated the environmental impacts associated with ILUC. However, the quantitative impacts of ILUC today are not very well defined in this chapter, and potential impacts in the future are not discussed at all.
Kurkalova	The report does address the requirements of Section 204. The mapping of the statutory language in EISA Section 204 and the RtC3 (Table IS.1) is very clear.
Landis	I find that the report does address the requirements of Section 204.
Smith	Yes, it addresses the requirements, subject to the caveats in my comments above.

3.5 Executive Summary and Integrated Synthesis

3.5.1. Question 4: The Executive Summary provides a high-level overview of the report, and the Integrated Synthesis provides a more detailed overview, including RtC3 scope and content, the report’s conclusions and recommendations, a comparison of RtC3 conclusions with those of RtC2, and a discussion of future reports under EISA Section 204.

- a) Is the presentation of information in the Executive Summary clear? Is the Executive Summary appropriately distilled down from the Integrated Synthesis and the broader RtC3? Please describe any suggestions for improvement.
- b) Is the presentation of information in the Integrated Synthesis clear? Is the Integrated Synthesis appropriately distilled down from the broader RtC3? Please describe any suggestions for improvement.

Reviewer	Additional Comments
Davis	4a. Executive Summary There is no summary of recommendations, just a statement that the report includes recommendations. The authors could prioritize one recommendation from each chapter to briefly highlight in the ES.

	4b. Integrated Synthesis Lines 242-243 indicate no comparison between impacts of biofuel and fossil fuels, but the latter chapters each include a subsection on the comparative impact of petroleum. There is a comparison of the land use change impacts described in Chapter 12 on lines 616-625 that could be summarized in the synthesis section. In the summary of chapter 8 in the IS, the health effects defined regionally are confusing and the wording gives the impression that the few people exposed in rural areas is not as much of a concern as exposure in city centers. This could be reworded to focus on dose-response, which should be the key criteria here to highlight instead of the "fewer people" exposed as stated in line 412. If lower population density means lower exposure <i>per person</i>, that is what matters. If few people are exposed to a high dose, that is just as much a concern. I think this is just a wording issue, but it is an important one. Line 459 highlights a comparison with fossil fuels, where corn and soybean feedstocks cause greater eutrophication than fossil fuel, but other comparisons where fossil fuels have a greater impact are not included in the synthesis. I think this could be misleading for readers. The summary in lines 553-562 describing the general wetland conservation nationally and the associated federal and state programs seems off-topic and is not related back to the purpose of the report. New feedstocks are not mentioned until the section on invasive species and cellulosic ethanol is not mentioned until the specific conclusions (line 608). See general comment about missing information on advanced biofuels. It would be good to add some discussion of the RINs in the summary (see key findings in Chapter 4 for example).
de Gorter	4a. Executive Summary Page ES-1 explains the EPA evaluates GHGEs separately. Should not the effects of the RFS Program on production and prices be the same for the GHGE evolution and the non-GHGE environmental effects described in this Report? Page ES-3, lines 92 only focuses on domestic environmental effects of corn production. What about the environmental effects of corn production outside the United States and of changes in crop production in the U.S. and in the rest of the world? 4b. Integrated Synthesis Line 142-143: I thought the stated goals of the RFS2 are three-fold: to reduce greenhouse gas (GHG) emissions and expand the nation's renewable fuels sector while reducing reliance on imported oil. Figure IS.1 I say explain in my comments on other chapters, when these is more than one policy at the same time, sometimes these other policies counteract the effect of the other (e.g.,

	any state incentives or a tax credit with a binding mandate counteract the effects of the mandate), are redundant (e.g., a mandate is redundant if the tax credit is higher than the ethanol price premium due to the mandate) or reinforces the effect (e.g., the LCFS carbon price is additive with RIN prices).
Engel	4a. Executive Summary The executive summary appropriately recognizes the challenges of assessing the impacts of the RFS versus biofuels more broadly. This continues to be a significant challenge despite the additional focus in an attempt to address this via this report. The executive summary correctly points out the challenges in quantifying locations of impacts of the RFS. These challenges are likely to persist with the next assessment as well. Attribution of effects to the RFS at local levels will be difficult. The executive summary addresses adoption of conservation practices briefly but does not acknowledge the significant commitments to spending to increase adoption of climate smart practices (e.g. conservation practices) in coming years. This has potential to have significant impacts 4b. Integrated Synthesis The Integrated Synthesis and chapters in which changes in land use occur, with a focus in a portion of the Missouri River basin, attribute additional corn and soybeans in this area displacing other crops and grass/pasture fails to acknowledge the great advances in corn and soybean genetics that have occurred over the past decade. These advances have made production of corn and soybeans more profitable than alternatives in many locations and especially in the historically more marginal areas that are the focus of many chapters of this report. The lack of acknowledgement that such advances in genetics are driving these changes potentially leads to a misinterpretation of RFS and biofuel impacts. Closely associated with these genetic advances that further impacts results reported is the continued widespread use of outdated crop growth parameters in models such as those used in work reported herein that likely bias model results. The synthesis appropriately describes the inability to identify specific locations of environmental impacts given limitations in data. This is likely to continue to be a challenge into the future for such analyses. The remainder of the synthesis aligns with key takeaways from the document. The presentation of information is clear.
Hill	4a. Executive Summary The Executive Summary is clearly written, but it contains many of the errors and deficiencies noted in my responses to the chapters above, especially as relate to Chapter 6.

	4b. Integrated Synthesis The Integrated Synthesis is also clearly written, but it too contains many of the errors and deficiencies noted in my responses to the chapters above, especially as relate to Chapter 6. Fig. IS.1 is flawed for reasons I have described above, especially as relates to overlapping and reinforcing policies, and should be deleted.
Hoekman	4a. Executive Summary Yes, the Executive Summary presents a clear distillation of the Integrated Synthesis and the broader RtC3. However, there are a couple of areas that should be emphasized a bit more strongly (meaning they also need to be emphasized more strongly in the Integrated Synthesis). First, the issue of indirect land use change (ILUC) and indirect environmental impacts should receive additional attention. Second, the potential future impacts of biofuels in general should be discussed more fully – whether or not these impacts are likely to be driven primarily by the RFS Program. In particular, potential impacts beyond the year 2025 should be mentioned. 4b. Integrated Synthesis For the most part, information presented in the Integrated Synthesis is clear and is appropriately distilled down from the broader RtC3. However, as mentioned above, issues regarding indirect environmental effects and future impact projections could be strengthened. Additionally, further information about GHG impacts of the RFS Program should be summarized here. Despite the fact that GHG impacts of the RFS Program are not identified explicitly under EISA Section 204 as a reporting requirement for RtC3, this information is of great interest and is highly relevant when judging the overall impacts (pros and cons) of biofuels in general, and the RFS Program in particular. Without considering GHG impacts, the reader is led to conclude that on balance, the RFS Program has had a negative effect on the environment. This certainly seems to be the case when considering overall land change/land management (LCLM) activities, water pollution impacts, air quality, and effects on ecosystems. Presenting the (likely) overall positive GHG impacts of biofuels in general, and the RFS Program in particular, would help offset this negative, but incomplete picture that otherwise emerges.
Kaffka	4a. Executive Summary Yes. This is the first piece of the RtC3I have read and it seems to be clearly written and logical. See line by line comments below for other reactions to individual parts of the text.

	Executive summary⁴⁷ “The purpose of the report is to examine the effects of the Renewable Fuel Standard (RFS) Program on the environment, including the impacts to date and likely future impacts to the nation’s air, land, and water resources.” (ES-1) “The statute requires a focus on environmental and resource conservation issues, including effects on air quality, soil quality and conservation, water quality and availability, terrestrial ecosystems, aquatic ecosystems, and wetlands, and consideration of invasive or noxious species.” (ES-1) Period covered: 2005-2020 Focus: the four biofuels that dominated U.S. production and consumption over this period: (1) ethanol from U.S. corn, (2) biodiesel from U.S. soybean, (3) biodiesel from U.S. fats, oils, and greases (FOGs), and (4) imported ethanol from Brazilian sugarcane. Factors influencing the production and domestic use of corn ethanol: 1. Need for fuel oxygenates during phase of MTBE 2. VEETC (volumetric ethanol excise tax credit; 2004-2010) 3. High oil prices from 2005 to 2015 4. Individual state programs and MTBE bans 5. Changes in the RFS program over this period (?) EPA asserts that: <i>“Nonetheless, by assembling multiple lines of evidence from empirical records and simulation modeling from the peer-reviewed literature, this report concludes that the RFS Program itself likely played a relatively minor role (0-0.4 billion gallons per year) in the growth of corn ethanol in the U.S. from 2002-2012 and may have played a more important role (0-2.1 billion gallons per year) since 2013. The more prominent role of the RFS Program on corn ethanol production in the U.S. in more recent years is consistent with the MTBE phaseout by 2006, expiration of VEETC at the end of 2010, and lower oil prices after 2015.”</i> (ES-2) <i>“As a mandate, the RFS Program could have driven most of the increase in ethanol production and consumption in the United States. However, as events played out, non-RFS factors that are known to influence the market were favorable and appear to explain much of the increase in ethanol production and consumption in the United States.”</i> (ES-2). Land use effects of RFS (corn production): <i>“Given the estimated range in the volume of corn ethanol attributable to the RFS Program, this report estimates the RFS Program’s effect on corn ethanol production and consumption resulted in up to approximately 1.9 million acres of additional cropland</i>
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⁴⁷ This was the first part of the report reviewed. I have made comments below as the occurred to me and included them by line or text.

between 2005 and 2016, and up to approximately 3.5 million acres of additional corn, with many years of no effect.” (ES-2)

“The 1.9 million acres of cropland corresponds with less than 1% of all cropland in 2017, but approximately 20% of the estimated cropland expansion between 2008 and 2016. The 3.5 million acres of corn corresponds with less than 5% of all planted corn in 2017 but up to 35% of the increase in corn acreage between 2008 and 2016.” (ES-3)

The amount of change is small, but may be locally significant if and where land use change occurred. Zero change is included in the range as a possibility.

Potential consequences: increases in soil erosion, pesticide and fertilizer applications (bad per se?), possible loss of semi-natural habitat (why and how important at this scale? Major effects on species like waterfowl? Prairie chickens, etc?) or just a general “concern.”

There seems to be no accounting of increases in soy production apart from biodiesel use...? Including growth of Chinese demand for soybeans, and the need for rotation crops with soy (corn primarily; corn and soybean are agronomic partners in much of the midwest).

Other possible effects:

Wetland loss; small since 2004 (est as 275K ac compared to previous 100M ac) but perhaps locally significant.

*“... Modest national effects do not preclude larger more local effects discussed in the report.” This is a key statement and potential justification for an adverse judgement wrt to the RFS. It will be important to see how EPA weights these unquantified and poorly defined “effects.”

There is no mention of effects on SOM of large amounts of corn residue compared with other crops, i.e., soil conservation. Similarly for adoption of no-till practices.

International effects associated with imported biofuels are even more uncertain than national effects but are likely modest as well given the relatively small quantity of imports relative to domestic biofuel production since the RFS Program went into effect.

EPA asserts that alternative mitigating practices like cover crop adoption are not occurring at a large enough scale to mitigate the environmental effects included in this report. (ES-4)

Cellulosic ethanol from manure AD and biogas capture goes unmentioned. (ES-4)

4b. Integrated Synthesis

“After decades of decline in cultivated cropland since at least the 1980s, increases by roughly 6–10 million acres have been recorded in multiple federal datasets, using a variety of methodologies, following the 2007 to 2012 period.

More than half of the corn and soybean increase has come from:

	... other cultivated cropland (56%), ... while the rest has come from approximately equal proportions of pasture (13%), non-cultivated cropland (20%), and the Conservation Reserve Program [CRP] (11%). Many of these changes are taking place throughout the Midwest, with hotspots in northern Missouri, eastern Nebraska, North and South Dakota, Kansas, and parts of Wisconsin. A portion of these changes are estimated to be due to the RFS Program.” (IS-2) Are these changes accurately quantified? Are results from counterfactual models associated with significant assumptions that themselves guide their conclusions? If so, are these models able to accurately estimate small marginal changes in land use that have multiple contributing factors? Estimated as 0% to 0.5% of all cropland, a total of up to 2M ac (IS-3, line 80). Are the LCLM changes identified so small that they are less than the errors of the land use assessment categories? There are potential sources of error in the data: 1. false categorization of land use types 2. inaccurate measurement of land uses by type 3. are transitions from fallow accounted? What about changes in fallow frequency? 4. How are rotations that include hay crops accounted? The rotation of tame hay fields and/or temporary pastures with arable and other crops is a long-standing agricultural practice (called ley farming), long considered “sustainable.” It occurs worldwide including in the US and in areas called hotspots by EPA here. A snapshot comparison of year to year changes misses the agroecological benefits associated with longer term crop rotations and risks misattribution of such changes to the RFS. The environmental criteria identified are: 1. air quality degradation (NOx, SOx, CO, VOCs, NH3, PM2.5...primarily associated with manufacture?) (Benzene (BTX?) was noted to decrease). 2. hypoxia (Gulf of Mexico?) 3. pesticides (residues, contamination?) 4. sediment (runoff, erosion) 5. nutrients in water (surface and groundwaters?) 6. pathogens in water (how related to crop farming?) All these are side-effects of the primary policy (to generate biofuels and reduce fossil oil usage). They may be classified as unintended side effects of cultivation generally. Is the cost of regulating these side effects greater or less than the benefits accrued from the primary farming activities?
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	Resource issues identified: Soil conservation, water availability, other (IS, line 14) Define: “soil environmental quality” (IS, line 13) Define: “ecosystem health” (IS, line 15). Both terms must be defined. What is meant by “ecosystem” and how can a subjective term like ‘health’ be used to quantify an uncertain concept like “ecosystem?” Define: “biodiversity” (IS, line 15). There are many possible definitions, boundary conditions, time scales, methods for quantification, etc. Define: “Impacts”. (IS, line 15). Positive, negative, neutral? How judged? Based on what baseline or notion of optimality?
Kurkalova	4a. Executive Summary The presentation of information in the Executive Summary is easy to follow and clear. The Executive Summary is appropriately distilled down from the Integrated Synthesis and the broader RtC3. I do not have any suggestions for improvement 4b. Integrated Synthesis The presentation of information in the Integrated Synthesis is easy to follow and clear. The Integrated Synthesis is appropriately distilled down from the broader RtC3. I do not have any suggestions for improvement.
Landis	4a. Executive Summary I found the executive summary to be very well written and clear. I appreciate that lines 45-48 call out the potential impacts of market certainty however, as I have noted above, I think these effects may have been dismissed too lightly and would have large influence on the conclusions of individual chapters. I also appreciate that the executive summary calls out in lines 64-67 that agricultural expansion could account for up to 35% of the increase in corn acres between 2008 and 2016. I think this would be an appropriate place to add a caveat about crossing landscape thresholds that could exacerbate the effects of even relatively small changes on biodiversity and ecosystem services. 4b. Integrated Synthesis I found the integrated synthesis to be very well written and clear. I appreciate that lines 66-72 call out the potential impacts of market certainty however, as I have noted above, I think these effects may have been dismissed too lightly and would have large influence on the conclusions of individual chapters.

	I also appreciate that the integrated synthesis notes in lines 124-129 that even relatively small changes in land use add additional strained already strained environments. I think this would be an appropriate place to add a caveat about crossing landscape thresholds that could exacerbate the effects of even relatively small changes on biodiversity and ecosystem services. Lines 128-29. I suggest adding a reference to the concept of <u>Precision Conservation</u>. E.g. Many of these impacts could be offset with greater adoption of conservation practices <u>including the use of precision conservation</u> [Chapter 3, section 3.2.1].
Smith	4a. Executive Summary Yes, it is clear, subject to the caveats in my comments above. The core of the entire report is the finding that the Renewable Fuel Standard had only a small incremental effect on ethanol consumption. As I detailed in my comments, I believe the authors unreasonably eliminate the possibility that the RFS had larger effects. 4b. Integrated Synthesis Yes, it is clear, subject to the caveats in my comments above. The core of the entire report is the finding that the Renewable Fuel Standard had only a small incremental effect on ethanol consumption. As I detailed in my comments, I believe the authors unreasonably eliminate the possibility that the RFS had larger effects.

3.6 General Comments

3.6.1 Question 5. Please provide any additional comments you may have on the draft RtC3.

Reviewer	General Comments
Davis	There is a lot of important information summarized in this report, and the authors generally did an excellent job communicating the trends and impacts of a complex system. Discussion of advanced biofuel feedstocks and cellulosic ethanol is conspicuously minimized throughout the report. The focus on corn, soybean, FOGs for biodiesel, and sugarcane from Brazil is rationalized by the fact that they are most dominant. The RFS mandates advanced biofuels (see lines 45-47 of Chapter 1), and the effects of these advanced biofuels (despite their small numbers) may be more indicative of the impact of the RFS than anything else. The vast majority of the content reviewed throughout the report is based on corn and soybean, generally, with a distinction made between the impacts that are associated with biofuel under the RFS vs. other driving factors. Advanced biofuels are more clearly set apart as something that is mandated under the

	RFS without as many other drivers of change. There could be important insight gained by reviewing the smaller changes in the advanced biofuels recognized by the RFS. Lines 213-14 in chapter 1 cites the growth of biogas (a 1767% increase), but this is not addressed as part of the impact of the RFS, nor are the environmental benefits and impacts addressed. A commercial permit for perennial grass feedstock is mentioned in chapter 15 (on page 15-15). Stover is mentioned in some of the chapters, but commercial developments for cellulosic ethanol production is not addressed in any of the chapters reviewed. Chapter 2 makes the case for the focus on biofuels that dominate the U.S. “pool” (line 30), but overlooks the fact that this focus does not necessarily reflect the biofuels for which the RFS has had the greatest effect. How can the efficacy of the RFS for advanced biofuels be assessed if the scope is limited to corn and soybean? For attribution sake, even if there were no advanced biofuels, it would be meaningful to describe that (potentially evidence that the RFS was non-binding) when assessing the impact of the RFS. There is a lot of important information summarized in this report, and the authors generally did an excellent job communicating the trends and impacts of a complex system.
de Gorter	Part 1: Background and Drivers I Chapter 1 Introduction and Chapter 2 Scope of the Report My comments on these pages are subsumed in my comments in the other chapters above. Comment on Box 2.3: it would be interesting to evaluate the change in parameters in equation [1] of deGorter Summary Biofuel Theory Because equation [1] is so critical for the translation of a change in biofuel prices on feedstock prices (ethanol and BBD). See how these parameters change here. Yet the relative price of DDGS to corn has trended up. Why is that? It would be a very interesting and important component of any technological change analysis in biofuels.
Engel	Soymeal is not discussed nearly as much as DDGS. It is somewhat the equivalent of DDGS to soy-based renewable diesel and the majority of soymeal is used for animal feed as is DDGS. Thus, the market for animal feed is important to both of these biofuels and their potential growth. Additional discussion of soymeal may be appropriate. Some of the glyphosate use data presented (Table 3.4) doesn't align with values reported in other reputable sources for recent years. From USDA NASS sources: https://www.nass.usda.gov/Surveys/Guide_to_NASS_Surveys/Chemical_Use/ https://www.nass.usda.gov/Surveys/Guide_to_NASS_Surveys/Chemical_Use/FieldCropChemicalUseFactSheet06.09.11.pdf - Glyphosate 66% corn in 2010

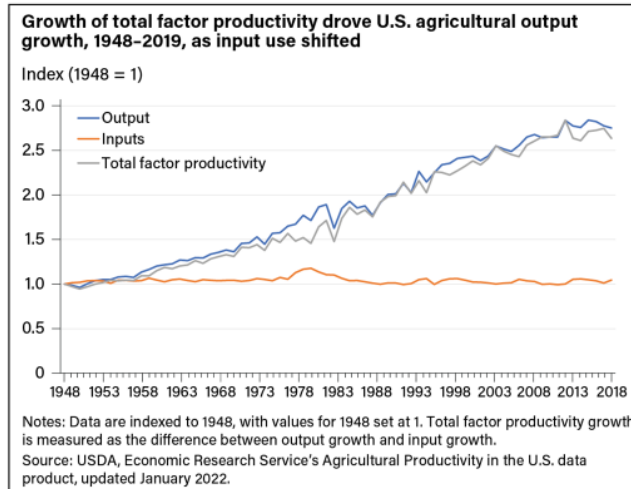
	https://www.nass.usda.gov/Surveys/Guide to NASS Surveys/Chemical Use/2021 Field Crops/chemhighlights-corn.pdf - Glyphosate 41% of corn acres in 2021 The EPA routinely conducts assessments of pesticide impacts on people and the environment through efforts as part of FIFRA, FQPA and ESA. The report does not seem to acknowledge this nor put resulting pesticide impacts from the biofuels and RFS assessment in context relative to these comprehensive EPA assessments of pesticides and their impacts.
Hill	<i>See Chapter specific comments under Part 1.</i>
Hoekman	Attribution of environmental and resource effects specifically to the RFS Program is an important objective, and is required under Section 204 of the EISA. The RtC3 does a much more thorough job of determining this attribution, as compared to earlier triennial reports. However, given the sparsity of data, contributions of other factors, and many other complications, it is still not possible to confidently attribute some important environmental impacts to the RFS Program. When the RFS Program was initiated, it was likely anticipated that this program would be the major driver of biofuels in the U.S. However, now that many years have passed, we see that many other policies, incentives, and mandates are equally (if not more) important drivers of biofuels production and use. Perhaps more important than attribution is simple determination and characterization of all significant environmental impacts from the production and use of biofuels in general, regardless of whether these fuels are attributable to the RFS Program. This is likely to become even more critical in the future, as there are many non-RFS drivers for increased use of biofuels. Some of the most important drivers are the growing number of regional policies and regulations (such as California's LCFS), rapid deployment of digester-based renewable natural gas (RNG), promotion of higher-ethanol blended gasolines (E20-E30), rapid expansion of renewable diesel (RD) production, and increasing demand for sustainable aviation fuel (SAF). EPA could provide a valuable service by exploring the potential environmental and resource effects (including GHG impacts) resulting from these many likely renewable fuel developments over the next 10-20 years. It would also be useful to discuss interactions (if any) between the RFS Program and these other renewable fuel developments. A few specific suggestions for clarification in the Integrated Synthesis section are offered below: • Page IS-4, lines 109-111: "Trends suggest that the potential lifecycle effects from biofuels are decreasing over time as industries mature and practices improve." It should be clarified that these decreasing lifecycle effects refer to biofuels on a volume specific basis (per gallon) or energy-specific basis (per MJ).

	As absolute volumes of biofuels increase, total effects are likely to increase, not decrease. • Page IS-8, Figure IS-2. This figure shows total, cumulative biofuel volumes over the period of 2005-2020. The current situation is quite different. It would be useful to provide a more recent snapshot – perhaps showing the period of 2018-2020 – in which case corn ethanol would not appear to be so dominating. • Page IS-12, Footnote No. 12 indicates that “The oxygenate used in U.S. gasoline has changed through time, from lead in the 1920s - 1980s ...” This is incorrect, as leaded gasoline additives are not oxygenates. Individual Reviewer Recommendations 1. De-emphasize the focus on attribution of environmental effects of the RFS Program. Section 204 of the 2007 EISA requires a triennial report on the environmental impacts of the RFS Program. In previous triennial reports, specific attribution of these impacts to RFS (as opposed to other causes) was not a major focus. To address this shortcoming, the current RtC3 effort focused considerable attention on the issue of attribution. The RtC3 report does an excellent job of highlighting the complexities and uncertainties in determining the attribution of environmental impacts to various causes. The methods and data sources used by EPA in making these assessments may be technically defensible but are also debatable. Different methods and data sources that are also technically defensible lead to different attributional conclusions. By presenting only EPA’s preferred attributional methods, the RtC3 is set up to encourage a “battle of the sciences” with others who promote different attributional methods and results. This could diminish the credibility of the entire report and draw attention away from the overall excellent assessments of a wide range of environmental impacts. To avoid this problem, I recommend that EPA diminish the focus on attribution and increase the focus on total environmental impacts of biofuels – regardless of the specific causes of these impacts. While there clearly is some interest (and legal requirement) in parsing out these impacts to the RFS Program, there is greater general interest in understanding the overall total effects on the environment from producing and using large volumes of biofuels, regardless of the cause. I note that the title of this report is “Biofuels and the Environment,” not “RFS-Attributed Biofuels and the Environment.” 2. Include more information about the GHG effects of biofuels. A primary reason for implementing the RFS Program is to reduce lifecycle GHG emissions from the transportation fuels sector. I would expect that most people, when considering the environmental impacts of biofuels, think first about GHG impacts. Yet, this topic is barely mentioned in the RtC3 report. Admittedly, this is a large and controversial area, fraught with many complexities and uncertainties. Just as with the attributional issues mentioned above, it would be very difficult
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	(and maybe impossible) to present a universally-acceptable view regarding the GHG impacts of biofuels. Yet, EPA has done considerable work in this area, and has established methodologies used to make lifecycle GHG assessments for numerous biofuel pathways. Furthermore, addressing GHG emissions would present a more complete picture of the total environmental impacts of biofuels. Without this, the reader is left with the conclusion that on balance, biofuels have quite negative impacts on the environment. Including GHG impacts may help offset this overall negative view and provide a more neutral assessment. Some expanded discussion of these GHG issues would add considerable value to this RtC3 report. 3. Extend the time horizon of the report. The Section 204 requirement to address likely future environmental and resource impacts of the RFS Program (or biofuels in general) is minimally satisfied by projecting results out to the year 2025. However, the value of the RtC3 could be improved substantially by extending this time horizon further – perhaps by an additional 10-15 years. Clearly, the uncertainties in such projections increase as the time horizon lengthens. Nevertheless, there are already numerous biofuel developments underway, which can be identified today, and whose future impacts could at least be described qualitatively. For example, production of renewable diesel (RD), sustainable aviation fuel (SAF), and renewable natural gas (RNG) are currently increasing at a rapid rate. Also growing, though less aggressively, is production of biofuel blendstocks via thermochemical processes (pyrolysis and gasification) applied to biomass feedstocks such as forest waste, agricultural waste, and municipal solid waste. Cellulosic ethanol produced via biochemical treatment of lignocellulosic feedstocks (which in the EISA of 2007 was projected to reach 15 billion gallons by 2022) is still a minor contributor, but could grow over this longer term horizon. Additionally, there are many policy and economic forces pushing towards higher use of ethanol, including efforts to utilize E15 year-round and to develop mid-range (E20-E30) ethanol fuels and vehicles. There is also growing interest in producing jet fuel from ethanol via alcohol-to-jet (ATJ) thermochemical processes. Finally, emergence of e-fuels during this longer time horizon is possible. I recommend that EPA include a more focused discussion of these various biofuel developments over a longer time horizon. At least qualitative assessments of fuel volumes could be made, along with likely environmental and resource impacts of such volumes. This would also serve the purpose of raising a red flag to alert the reader about potential areas of future concern.
Kaffka	The RtC3 is an impressive and comprehensive effort. EPA has assembled a large amount of technical literature and provided a careful analysis of a very difficult and challenging problem: how to attribute a large set of diverse, regionally and locally variable environmental consequences to the Renewable Fuel Standard, in the midst of many

	other significant influences. In general, I agree with EPA's assessment about the relatively small influence of the RFS on crop and land use during the period evaluated. As EPA noted in its analyses, commodity markets are complicated and subject to diverse influences including trade and international food and feed stocks, inflation, the value of the dollar against other currencies, unstable political events, weather and yield variation, and more. An influence on markets in one year by the RFS may be offset in other years, so estimating an average effect of the RFS remains uncertain. In that circumstance, EPA's cautious attribution assessment is appropriate. I have six general comments here, some of which also appear in individual chapter reviews. These comments reflect my perspective as an agronomist and agroecologist. Based on discussions during our public meetings, the comments included here differ from the perspectives of other reviewers with different training and viewpoints. 1. EPA should contrast the topics covered in the RtC3, with the RFS policies' benefits in a preamble, to provide congress and the public a clearer perspective on the tradeoffs associated with the RFS. There were and are environmental effects associated with the RFS, but large amounts of alternative fuels and measurable reductions in the use of petroleum, the primary objectives of the policy, were achieved. In addition, the agricultural economy of the US has been strengthened, a not insignificant benefit. No one can know the optimal amount of land that should be planted to major crops in the United States. Counterfactual economic modeling can estimate the number of acres that might have been planted in the absence of the RFS, but that estimate will not be the correct value for the optimum land use for corn, soybeans or other major crops in the US. From my perspective as an agronomist, the US agriculture sector is a remarkable asset and important national resource. Policies that contribute to its flourishing at a reasonable cost should be supported, while at the same time making prudent public investments to help individual land owners collectively reduce adverse consequences of the policy where possible. In general, this involves supporting investment in conservation practices and modifications to working lands and supporting problem-solving and technology innovation research at the nation's land-grant institutions and USDA-ARS. In the past, this had led and continues to lead to improved efficiency of farming, while reducing emissions on a per product and landscape basis. But the need for continued progress in this area remains. The United States (together with Canada) is blessed with large amounts of land with high quality soils suitable for farming, a largely beneficial climate that supports the production of key agronomic crops, and innovative private companies and land grant university/USDA research systems that support continuous agricultural innovation and optimization by skillful farmers. In rounded terms, the US contains about 5% of the world population, but about 20% of the worlds' best cropland. These circumstances result in a condition of steady technical progress, increasing
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returns to total factor productivity (Hellerstein et al., 2019; Fuglie et al., 2012)⁴⁸ and chronic surpluses for the best adapted annual crops like corn, soybeans and wheat. Given increasing human populations (now officially 8B people), the need for improved diet quality, and the desire to reduce the effects of agricultural land use expansion into fragile habitats elsewhere in the world, this circumstance is a public good and an ecological benefit at a large (global) scale.



Technology advancement has allowed U.S. farmers to produce more crops and livestock while using less labor and land. From 1948 to 2019, the quantity of farm labor used in the production of U.S. agricultural commodities fell by three-fourths and **land use declined by a fourth**. During the same time, U.S. agricultural production rose to about 2.7 times its 1948 level.

See: Daniel Hellerstein, Dennis Vilorio, and Marc Ribauda (editors), *Agricultural Resources and Environmental Indicators, 2019*. EIB-208, U.S. Department of Agriculture, Economic Research Service, May 2019.

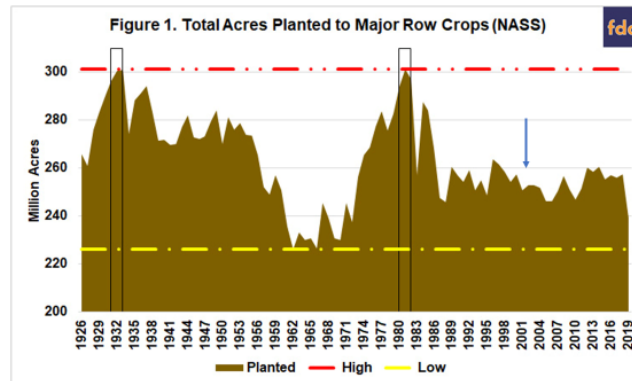
<https://www.ers.usda.gov/amber-waves/2022/october/u-s-agricultural-output-has-grown-slower-in-response-to-stagnant-productivity-growth/>

Surplus is a characteristic condition of US agriculture and the US has long been an exporter of feed and food grains. This is a beneficial circumstance for the US and the world as a whole because surpluses provide stability and resilience for the world's food supply. Even with exports, however, domestic surpluses have persisted due to yield gains and increasing efficiency. These have adverse effects on crop prices and well-being in the agricultural sector. Production years when prices below the cost of production for key commodities have occurred commonly.

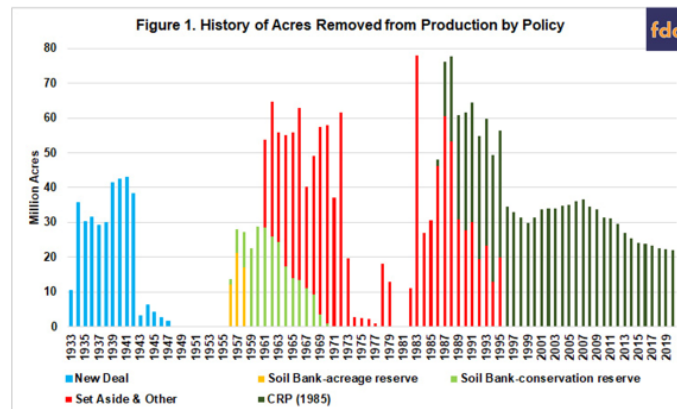
There is a long history of policy interventions by congress designed to support the agricultural sector during such events. These have often been controversial, and involved subsidies, transfer payments, favorable crop insurance, land use restrictions, and other policies. These are summarized well in references from Coppess et al (2020) associated with selected figures from their publications that I have included below. Since the 1930s, there have been two major cycles in the amount of land in major crops produced in the United States. During the 1960s and again in the 1980s, farmers were paid not to produce corn crops due to problems associated with surpluses. The number of acres diverted (unplanted) then was

⁴⁸Daniel Hellerstein, Dennis Vilorio, and Marc Ribauda (editors). *Agricultural Resources and Environmental Indicators*, 2019. EIB-208, U.S. Department of Agriculture, Economic Research Service, May 2019.; Fuglie, K.O., Wang, S.L., and Ball, V.E. 2012. *Productivity Growth in Agriculture: an International Perspective*. CABI, 392 p. It is the interaction among inputs, new technology and management that drives increases in resource use efficiency and increases in crop yields. In agriculture, increased CO2 levels also help. Steady long-term increases in the yield of corn and soybeans, for example, continue to contribute to surpluses in commodity markets that are also used productively for industrial purposes, including biofuels.

roughly equal to the number of acres currently supplying corn and soybeans for biofuels. In a sense, to the degree that the RFS has helped support crop prices, it can be seen as the most successful of all of these programs because it allows fuel markets to bear the cost of crop support under conditions of surplus instead of congress, while also creating alternative fuels that reduce conventional petroleum use.



Coppess, J., N. Paulson, G. Schnitkey, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink](#)



Coppess, J., G. Schnitkey, N. Paulson, K. Swanson and C. Zulauf. "Production Controls & Set Aside Acres, Part 1: Reviewing History." *farmdoc daily* (10):117, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, June 26, 2020. [Permalink](#)

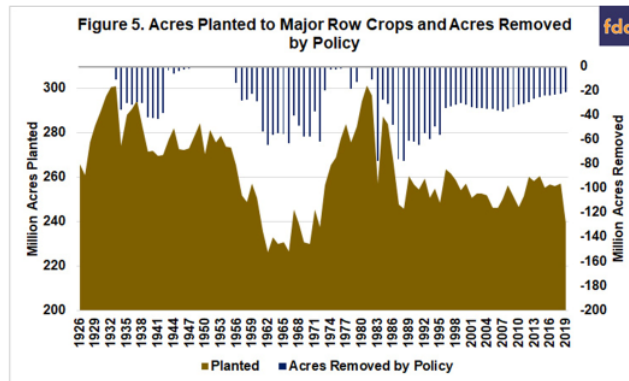


Figure 5 illustrates the total acres planted to the major row crops and the acres removed from production or planting by policy. Acres removed by policy are indicated by the navy bars and work down from zero on the right-hand side. The brief history of acres removed by policy can be divided into the parity system of the 1930s, which used allotments to restrict planted acres of surplus crops. Next was the Soil Bank (1956 to 1958) which coincided with allotments for cotton and wheat but also removed additional acres in an acreage reserve and a conservation reserve program (*farmdoc daily*, February 20, 2020). It was followed by specific feed grains legislation in the 1960s that used Payment-in-Kind policy to encourage farmers to reduce acres planted to corn and the other feed grains (barley, oats, rye and sorghum).

Coppess, J., N. Paulson, G. Schnitzky, C. Zulauf and K. Swanson. "Acres & Crisis: Prospective Plantings and Perspectives from the Past." *farmdoc daily* (10):66, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign, April 9, 2020. [Permalink: https://farmdocdaily.illinois.edu/2020/04/acres-crisis-prospective-plantings-and-perspectives-from-the-past.html](https://farmdocdaily.illinois.edu/2020/04/acres-crisis-prospective-plantings-and-perspectives-from-the-past.html)

The growth of the ethanol and biodiesel markets predated federal legislation. As reviewed by EPA, starting in 2003, incentives to make ethanol from corn increased with the substitution of ethanol for MTBE, a gasoline oxygenate thought to be harmful to groundwater. This occurred first in California, but spread quickly to other states and regions of the US. Substitution for MTBE as use as fuel oxygenates created a reliable demand for corn ethanol.

Another factor supporting increased use of ethanol in gasoline was an increase in the price of oil that occurred in the early 2000s. The price of oil rose relative to the price of corn, which remained low initially. But since corn is approximately 70% energy (starch + corn oil), the prices of oil, corn and ethanol became linked in markets. It seems that this was unavoidable and would have occurred in the absence of particular policies. During the 2003 to 2007 period, it became more profitable to blend gasoline with ethanol than to use unblended gasoline. This had consequences for the oil industry as well and the way in which gasoline was manufactured, that further supported the use of corn for ethanol. These developments led to the rapid expansion of dry mill ethanol facilities in the Midwest, especially in Iowa, but not limited to Iowa. Some of these were farmer-owned cooperatives.

Subsequently, the US congress passed the first legislation designed to support the production of gasoline alternatives, the RFS 1. It was followed a few years later by the RFS2. The RtC3 is largely a report on the effects of the RFS2 program on several environmental characteristics in areas where corn (and soybeans) are produced, in part for use as alternative fuels. The specific intention of these acts was to support the increased production of alternative fuels in the US. This has occurred. The acts can be seen to be successful with respect to their primary objective. The US is now the world's largest ethanol producer and one of the largest producers of biodiesel. Adverse environmental effects associated with production of these biofuels are unintended consequences of the primary focus of these laws.

As noted in the RtC3, the development of the dry mill ethanol industry in the US was largely underway and mostly completed by the time that the RFS2 was passed

	and took effect. These acts or the prospect of these acts undoubtedly had effect of providing policy certainty for investors, including farmer cooperatives. The use of corn and soybean surpluses for energy, an industrial use, were market clearing mechanisms that reduced the surplus stocks to use ratios common for both crops, sustained crop prices above the costs of production, and helped to increase for a time the overall prosperity of the country's agricultural sector. In helping to improve the profitability of farming, they helped maintain America's enormous and critically important agricultural production capacity. Increased wealth supports a beneficial cycle of increased investment in innovative technology, which improves total factor productivity and the overall resilience of the agricultural sector. Money is also available for additional land conservation practices and land management improvements compared to extended periods of hardship. The adoption of reduced tillage methods, and creation of buffer areas near water courses to absorb nutrients, for example, have been shown to retard emissions to surface waters from annual cropping systems (Chapter 10). When farming is profitable, these types of modifications to farming landscapes become more affordable and adopted on more acres. These are all beneficial policy outcomes, not just for the US, but for the larger world which depends on the productivity of US agriculture to help reduce hunger and improve the quality of human diets. The economic health, increasing efficiency and productivity of American agriculture is of importance to the entire world's agricultural economy. Adverse, but unintended consequences must be weighed against the benefits of the policies. That is the job of congress, not the agency. The RtC3, in being focused on estimating actual environmental effects of the RFS, has for the most part been well bounded and careful in its assessments. EPA, by following the statutory limitations expressed in section 204 on its assessment task has acted correctly. The RtC3 should not be seen as vehicle to advance particular policy perspectives on the overall benefits or harms of renewable fuel production and use from crops. 2. Different perspectives on the importance of unintended effects of farming on the environment. Changes in agricultural land use in the US result from a multidecade process, driven by many diverse influences, including unpredicted ones, and also likely by climate change. Thousands of land owner/farmers are involved in this process, along with their bankers. Choices are influenced by public policies. It is an evolutionary process. The US agriculture system has proven to be adaptable and resilient in this way. Perspectives on this process vary, as well as on the benefits and costs associated with this adaptation process. Some of the disagreements about policy effects can be considered to be arguments about the proper baseline for crop production in the US. Most active farming involves the transformation of natural landscapes. The exception is grazing of existing grasslands, but even this might alter the characteristics of unmanaged grazed lands. Landscape transformation is the largest alteration of nature associated with farming. Emissions occur whenever land is farmed. There is no emissions-free agriculture.
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	The level of emissions is always local, but at the larger scale, there is a generalized relationship between the intensity of farming and the amount of emissions observed. In general, the greater the level of productivity, the greater the chance for emissions from farming, especially when agricultural inputs are yield enhancing, like fertilizer or tile drainage of wet areas in fields. Improvements occur over time in the levels of emissions occurring or in the management of the side effects they cause. But some level of emissions is a necessary cost of economically efficient farming that benefits society as a whole. Society (broadly defined) and in the case of the RFS, congress, set the level of emissions that are acceptable relative to the social benefits of an economically efficient agriculture with high levels of productivity. This balance is constantly shifting, and is achieved by political processes. Technology development and agricultural research by the nation's land grant university system and USDA-ARS often focuses on improving the relationship between productivity and emissions of all kinds. This process has been and continues to be successful, if at times uneven, as evidenced by studies on changes in productivity with respect to resource use over time cited above. De Wit (1992)⁴⁹ argued that it is socially beneficial to tolerate some emissions from the best farming areas because of the greater agricultural productivity and efficiency of farming in those areas, and results in land sparing in other less productive regions. There is a tradeoff between local effects and global goods. But when discussing local effects, it is important to also keep in mind the benefits derived. Total farmland use for major crops has declined over the last two decades. In part this is due to increases in production on the best lands, reduction in export markets for major commodities due to increases in production in other parts of the world, especially South America, tariffs on soybean exports to China, and other causes. The fate and environmental effects of these land reversions over time from annual agriculture to fallow grasslands or other endpoints is not discussed by EPA, but presumably the acres abandoned reverted to more natural conditions, increased landscape diversity locally, and provided habitat where they occurred. Locations where land reversion occurs shift around the larger agricultural landscape. Reversion occurs in less productive areas, or areas with declining irrigation resources, or due to state level programs, and climate shifts among other factors. The largest decline in acres during the period analyzed by EPA in the RtC3 was for hay production. While dairy sector demand for forages has remained largely constant, beef production and consumption has declined, reducing market demand. Limitations of irrigation sources for hay production in western states may also have led to the decline in hay acres. At the same time, there has been an increase in corn and soybean production. Emissions from these acres are discussed by EPA in the RtC3. A small percentage of these acre increases is attributed to the RFS2 for corn, (but not uniformly across
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⁴⁹ De Wit, C.T.; 1992. Resource use efficiency in agriculture. *Agricultural Systems*, 40:125-151
<https://www.sciencedirect.com/science/article/pii/0308521X9290018J>

	the period evaluated, see Chapter 6), but no quantitative attribution is assigned to biodiesel made from soybeans (Chapter 7). There are different views about the desirability of these developments. Some see conversion of previous land uses on farms like hay production or hay-grazing lands to annuals as a loss of a public good. The public good is grassland type habitats that support different wildlife species compared to annual crops. Another good that may be foregone is a higher level of soil organic matter that might have accumulated in undisturbed grasslands compared to the same fields under annual crop production. Soil erosion and water management and ephemeral wetlands can be adversely affected, and these effects are noted and reported by EPA in the RtC3. Large scale losses of soil organic matter may or may not be true locally given the large amounts of residues produced by corn crops, and limited rates of accumulation of SOM in dryland landscapes. See for example, Clay et al., 2012. In semi-arid areas where irrigation occurs, SOM may increase relative to non-irrigated locations. It may also be affected by the duration of grass or hay production, if hay crops are part of common crop rotations using annuals. Longer-term rotations between hay crops and annuals should not be analyzed only in the annual phase, but rather over time as part of a longer-term land management strategy. Snapshots in time miss some of these changes. In addition, to the degree that climate change has increased precipitation in parts of the western corn belt, the economic competitiveness of corn and soybean production may shift to that region and is likely currently shifting to previously less suitable areas in the Dakotas and Minnesota. (Hoffman et al., 2020; Hay and Todey, 2011). ⁵⁰ This is a long-term process and is exogenous to effects from the RFS2. It can be seen as an adaptation to changing climate. Changes in land use and the optimum locations for crop production must be viewed over a long-term horizon and seen at a large scale. The entire productive landscape of the Midwest is integrated economically, and is constantly evolving
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⁵⁰ ...” Using maize as a benchmark, we estimate that if warming continues unabated through the first half of the 21st century, the best climatic conditions for rainfed maize and soybean production may shift from Iowa and Illinois to Minnesota and the Dakotas with possible modulation by soil productivity.” Hoffman, Kemanian, and Forest. 2020. Env. Research Letters 15(9). The response of maize, sorghum and soybean yield to growing phase climate revealed with machine learning. DOI 10.1088/1748-9326/ab7b22.

ASABE. Louisville, KY. “Crop yields can be negatively impacted by excess water as well as insufficient water. The northwestern portion of the Corn Belt, including South Dakota, North Dakota, Minnesota, and Iowa, is situated in a transition zone from moist sub-humid to semiarid climates, and is, therefore, susceptible to both floods and drought. Precipitation has increased in the upper Midwest over the last 50 years, but less is known about changes in evapotranspiration (ET), which, along with precipitation, determines water excess or deficit. The objective of this research was to evaluate patterns of precipitation and ET in the four-state region over the last 49 years. The results confirm that precipitation has increased over much of the region in the last two decades, particularly outside the growing season. Fall and winter precipitation increases were more pronounced in the Dakotas, while precipitation increases in the spring were more common in Iowa. Areas of increased summer precipitation corresponded to areas of expanded row crop production.” Hay and Todey. 2011. Precipitation and evapotranspiration patterns in the northwestern corn belt and impacts on agricultural water management. ASBE. DOI: [10.13031/2013.38114](https://doi.org/10.13031/2013.38114)

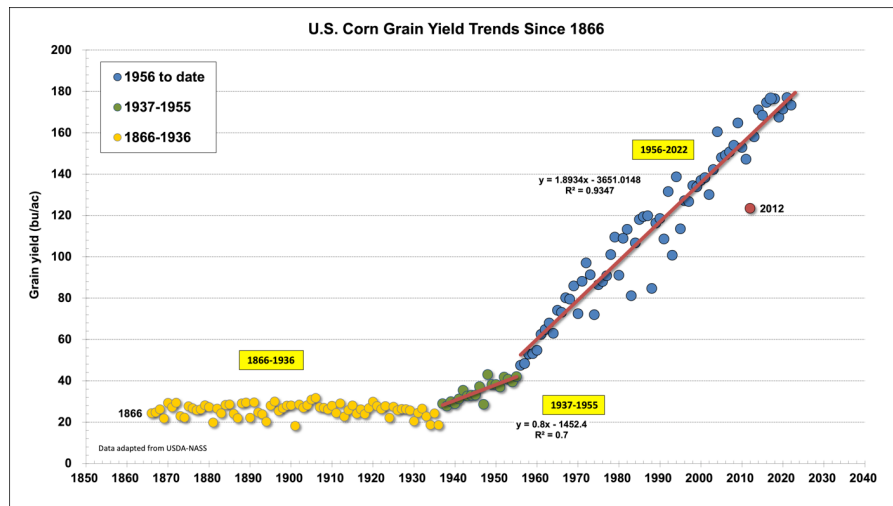
	due to changes in economic conditions, technology, climate and public policy. These changes over time affect where and when crops are grown, and which crops are optimal at a given location. The responsiveness of the US agricultural system and its ability to adapt to these dynamic forces is important for domestic reasons, but also for international food supplies. In the process, shifts in crop land use have, will and should occur. These shifts result from the economic decisions of thousands of land owners and farmer optimizers. Local planting decisions commonly are made in consultation with farm credit providers, but this is not mentioned in the RtC3. Land that becomes less suitable in the traditional areas of the corn belt will likely also shift in uses, perhaps to more perennial crops, or other alternative enterprises, compensating in part for shifts in land use in more northerly regions. These processes require longer time intervals to develop than the timeframe for the RtC3. Political events like the recent disruption in world grain trade from the war in Ukraine play an unanticipated role. So the RtC3 should be seen as a discreet analysis for changes in the agricultural landscape for a subset of time, but EPA should be mindful of evolving land use taking place over a longer time period than that considered in the report. 3. General comments about Horizon scanning in the RtC3: Why crop-based feedstocks are favored and (will) continue to be used. There are multiple reasons why the development and use of cellulosic feed stocks has been slow or even non-existent. If the demand for ethanol remains static or declines, it seems unlikely that more expensive feedstocks and more technically challenging conversion processes will become viable. There are many ways in which the use of cellulosic materials for energy would lead to beneficial landscape outcomes, but ethanol demand would have to increase at a rate faster than crop surpluses currently accumulate to support the use of new feedstocks. The sustainable aviation fuel market may be a pathway for the use of cellulosic feedstocks. Chapter 2.2.6.4, discusses briefly the potential for alternative feedstock use for ethanol production. The Billion Ton Study (Box 2.2, pages 2-16 to 2-17) is also highlighted. In addition, each chapter of the RtC3 includes a section on <i>horizon scanning</i>. In many, the potential for adoption of perennial grasses or other cellulosic feedstocks is mentioned as alternatives to the use of annual crops (corn and soybean). When the RFS1 was adopted by congress, the rapid development of cellulosic biofuels was anticipated. Despite initial optimism, this expectation proved unfounded, and initial mandates for cellulosic fuels (RVOs, D3/7 RINs), were adjusted downwards as time went on. There are many potential benefits to the use of cellulosic feedstocks, especially perennial grasses⁵¹. These include estimated large supplies of diverse cellulosic
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⁵¹ Wood wastes from commercial timber processing and fuel load reduction in western forests are other feedstock examples, and urban organic residuals are increasingly being used for RNG production.

feedstocks⁵², reduced competition between the use of annual crops for food or fuel, perennial cover that reduces soil erosion, increases in soil carbon resulting from perennial root systems and reduced disturbance from tillage, reduced amounts of agricultural inputs like fertilizers, pesticides and irrigation, the improved use of marginal lands, and greater landscape diversity in regions where farming dominates, with potential benefits to wildlife.

Despite these apparent or presumed benefits, cellulosic feedstocks remain a small portion of total alternative fuel production in the US⁵³ and worldwide, and crop-based feedstocks continue to be used for the majority of alternative fuel production. This is likely to continue so long as total ethanol demand is limited to 10 to 15% of transportation fuel demand. This demand is expected to decline as alternative LDVs like BEVs and PHEVs entire the personal transportation fleet⁵⁴.

For crop-based biofuels to make sense, feedstocks must result from yield increases through sustainable intensification, especially improved Resource Use Efficiency (RUE). This has been the case in the US.



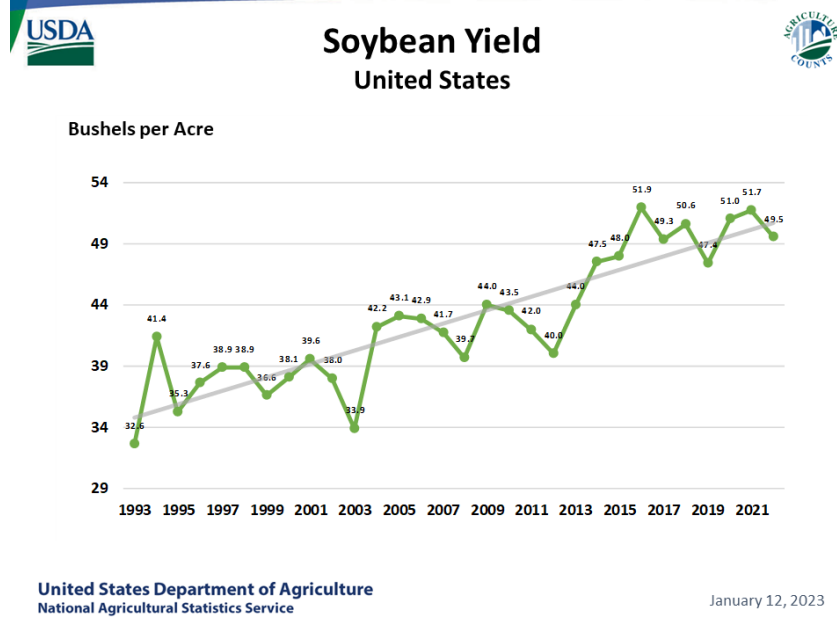
Corn yields in the modern era have increased by almost 2 bu/ac over an extended period and this trend continues to be observed. Source:

<https://www.agry.purdue.edu/ext/corn/news/timeless/YieldTrends.html>

⁵² <https://www.energy.gov/eere/bioenergy/2016-billion-ton-report> ; The use of diverse cellulosic feedstocks at a national scale was seen as a means to overcome supply limitations associated with reliance on crop-based biofuels.

⁵³ Most cellulosic RINS derive currently from renewable natural gas produced from landfills and increasingly from dairy manure storage.

⁵⁴ An alternative use of starch, sugar and oil-based biofuels is in the aviation sector (Sustainable Aviation Fuel_SAF), which is growing rapidly. The growth in this market is likely to compete with and absorb losses in the LDV fuel market. <https://www.energy.gov/eere/bioenergy/sustainable-aviation-fuel-grand-challenge> ; <https://www.whitehouse.gov/briefing-room/statements-releases/2021/09/09/fact-sheet-biden-administration-advances-the-future-of-sustainable-fuels-in-american-aviation/> . EPA should make note of this as it does horizon scanning.



Soybean yields have also increased, particularly since 2003, but at a slower rate. Source: https://www.nass.usda.gov/Charts_and_Maps/Field_Crops/soyld.php

Corn and soybean yields have increased steadily over time while wheat, other small grains, and other minor agronomic crop yields have increased less rapidly or remained static, making them less attractive to farmers. Chronic surpluses for all these crops remain common despite their industrial use as feedstocks for transportation fuels, and co-products like DDGS and other industrial uses like polymers.

RUE improves through technological innovation and investment. It lowers CO₂ emissions per unit product. This progress is the result of technical change and investment resulting from economic health in the ag sector, sustained in part by industrial use of (inevitable) surplus production (for biofuels). Everywhere that surpluses develop from improved RUE, biofuels become a market clearing mechanism that sustains the economic health and productivity of agriculture. Commonly, the surpluses develop prior to the use for biofuels or other industrial uses. This was the case for both corn and soybeans in the US, for sugarcane in Brazil and beet sugar in the EU.

Besides increasing yields, improving RUE, and persistent market surpluses, corn and soybeans are favored for biofuel production in the US because of feedstock quality⁵⁵ (which increases conversion yields and efficiency), feedstock consistency and predictability, and established, extensive production, storage and supply pathways, networks and infrastructure which is missing for cellulosic feedstocks.

⁵⁵ Starch (corn), sugar (sugarcane) and vegetable oils (soybeans, canola) are easy to transform into fuels, are consistent in quality, and have high fuel yields and useful co-products, compared to cellulosic materials, which are difficult (refractory) to convert, derived from more heterogeneous materials, and have larger costs to transport, concentrate and store at biorefineries. See for example: <https://poet.com/pr/epa-actions-trigger-project-liberty-shift>

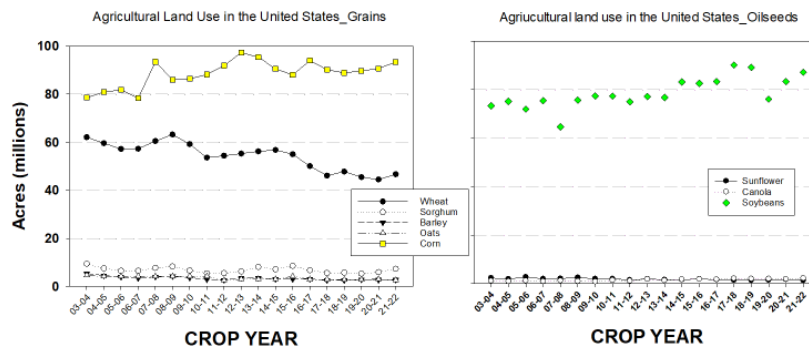
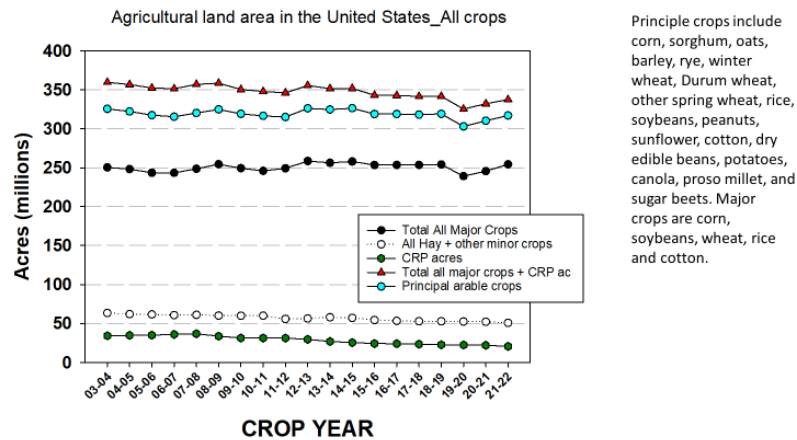
	Another set of developments associated with ethanol production is the potential to capture and reuse or sequester the relatively pure forms of CO₂ that result from fermentation processes. Some ethanol facilities sell CO₂ to commercial users currently. One company in CA is actively developing carbon capture and storage (CCS) opportunities to further lower their ethanol CI. Another feature in the evolution of ethanol production is the adoption of advanced biofuel process that deal with co-products, or which add new technologies that allow for the use of cellulosic biofuels that end in ethanol production. In California, due to the LCFS, corn ethanol refiners are forced to constantly reduce the carbon intensity of the ethanol they produce. Both CA companies now assemble biogas produced on nearby dairies, condition that gas and substitute it for conventional natural gas in their fermentation process. This lowers their ethanol Carbon Intensity (CI). RNG not used for this purpose is used for transportation fuels in adapted RNG engines or injected in pipelines for other users of RNG. Corn oil extraction occurs and in one instance is combined with the use of yellow and brown grease to produce biodiesel. One company has created plans to adopt Lanza Tech technology to convert old orchard wood into ethanol (not yet in place). It is pursuing other technologies based on corn oil, FOG, and other vegetable oils for jet fuels. There are many interesting co-product and complementary conversion processes that are potentially possible, based on otherwise unusable local feedstocks or process by-products. Ethanol producers in other parts of the US have different alternatives and creative pathways to follow, but such developments would be encouraged by supportive policies, similar in effect to the LCFS in CA. These transform corn dry mills gradually into integrated biorefineries. 4. All of these strategies depend ultimately on the use of corn for ethanol in a core facility. Commonly, it has been imagined that cellulosic biorefineries can be built de novo. In a few cases this has occurred, but with some notable failures. But the evolution of first generation facilities like corn ethanol mills into integrated biorefineries that improve the use of co-products and then add new 2nd generation bioenergy technologies and achieve CCS, are pathways that EPA has not considered in its Horizon Scanning exercise. These and other technology development pathways are likely, are occurring, and will be more widely adopted by biorefineries as they make economic sense, depending on policy signals. Debates over the assessment of grassland conversion. Lastly, there is little mention of the potential for higher ethanol blends in gasoline. Adapted engines and higher-octane fuels with ethanol blends in the 20 to 30 % level are reported to provide significant fuel economy gains from traditional spark ignition technologies. There are reported reductions in ethanol volatilization at this level of inclusion. And reduce tail pipe emissions.⁵⁶ As vehicle miles traveled fall, total ethanol
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⁵⁶ http://blog.ucsusa.org/jeremy-martin/the-road-to-high-octane-fuels#_ftn7 ; US DOE-2022. The Road Ahead. Toward a Net-Zero-Carbon Transportation Future. DOE/EE-2539. <http://energy.gov/fuel-engine-co-optimization> ; Leone et al., 2015. The effect of compression ratio, fuel octane rating, and ethanol content on spark-ignition engine efficiency. Env. Sci. Tech. 49:10778-10789, <http://dx.doi.org/10.1021/acs.est.5b01420> ; Theiss et al., 2016. Summary of High-Octane, Mid-level ethanol blends study. ORNL/TM-2016/42. Oak Ridge National Laboratory; Speth, R.L., E.W. Chow, R.L. Malina, S.R.H. Barrett, J.B. Heywood, and W.H.

	demand might not be greatly increased. But additional demand has the potential benefit of bringing into greater use cellulosic feedstocks with their attendant landscape benefits, as highlighted in the RtC3 by EPA. This seems like an omission when forecasting potential developments in the future. Some attention should be given to higher ethanol blends and adapted engine technology during EPA's forecasting exercise EPA should evaluate differences in the estimates used for grassland conversion and particularly assess the differences in views in papers cited in the RtC3 about the amounts, types and locations of grassland conversion, but most especially about the methods used to determine or model these landscape changes. The relevant papers involve the use of large datasets, and sophisticated data analysis and modeling methods. EPA should support a technical analysis and more detailed comparison of contending claims. EPA suggests that cropland acres have increased at the expense of grassland conversion, including de-enrollment of CRP acres. NASS data suggest that total principal crop acres and total crop acres have remained relatively stable over the period that the RFS has been in place, but that declines have occurred in hay and minor crops. Of the principal crops, corn acres have increased from roughly 80 M ac to approximately 90 m acres over the period. While soybean acres have increased from less than 80 M ac to nearly 90 m acres. Wheat and hay have declines though wheat acres may be increasing at the expense of corn and soy acres currently. CRP acres declined, especially since 2009, but are increasing currently to their approximate long-term average for the period considered. The amount of decline in wheat acres plus de-enrolled CRP acres is approximately equal to the number of acres increase in corn and soybeans.
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Green. "Economic and Environmental Benefits of Higher-Octane Gasoline." *Environ. Sci. Technol.* 48 (2014): 6561-6568. ; Irwin and Scott (2017) compare ethanol with BTX additives as octane enhancements on an energy and cost basis. <https://farmdocdaily.illinois.edu/2017/03/on-the-value-of-ethanol-in-the-gasoline-blend.html>; Bolden, A., Kwiatkowski, C.F., and Colburn, T. 2015. New look at BTEX: Are ambient levels a problem? *Env.Sci. and Tech.* 49:5261-5276. Doi:101021/es505316f; Darlington, T., G. Herwick, D. Kahlbaum, and D. Drake. Modeling the Impact of Reducing Vehicle Greenhouse Gas Emissions with High Compression Engines and High Octane Low Carbon Fuels. *SAE Technical Paper 2017-01-0906*, doi:10.4271/2017-01-0906. 2017; Karavalis, G., Short, D. Diep, v., Russell, R., Hajbabaie, M. Asa-Awuku, A, and Durbin, T. D. 2015. Evaluating the effects of aromatics content in gasoline on gaseous and particulate matter emissions from SI-PFI and SIDI vehicles. *Env. Sci. tech.* 49:7021-7031; Von Stackelberg, K., Buonocone, J., Bhave, P.V., and Schwatz, J.A., 2013. Public health impacts of secondary particulate formation from aromatic hydrocarbons in gasoline. *Env. Health* 12:19ff <https://doi.org/10.1186/1476-069X-12-19>

There are many other studies



Source: USDA_NASS

EPA, relying on remote sensing and modeling studies and NRI data also suggest that grassland conversion to annual crops, principally corn and soybeans has occurred during the period. Reference to these same modeling studies appear frequently throughout the report and are relied upon by EPA to infer environmental effects from increased corn and soybean production, a small amount of which is estimated to have occurred due to the RFS (Chapter 6). EPA authors of the RtC3 sometimes were coauthors of the papers cited. This is mentioned in Chapter 1, when EPA discusses differences between the previous report to congress (RtC2) and the current report. This series of papers address EPA's need for data, estimates and/or predictions of the possible environmental consequences of the RFS. The amount of land converted is based mostly on estimates provided by a set of interrelated papers based on the use of data from the CDL, and other related papers⁵⁷. EPA discounts the estimated area of grassland

⁵⁷ Lark, TJ; Mueller, RM; Johnson, DM; Gibbs, HK. (2017). Measuring land-use and land-cover change using the US Department of Agriculture's cropland data layer: Cautions and recommendations. International Journal of Applied Earth Observation and Geoinformation 62: 224-235. <https://dx.doi.org/10.1016/j.jag.2017.06.007> . Lark, TJ; Salmon, JM; Gibbs, HK. (2015). Cropland expansion outpaces agricultural and biofuel policies in the United States. Environ Res

	conversion in this paper and associated studies in its final attribution (Chapter 6), but in the absence of other technical assessments or more local data and analysis, relies on these studies to discuss possible effects on several important environmental properties included in their charge under Section 204. The most significant land use conversion with adverse environmental consequences is asserted to have occurred primarily in the western areas of the corn (and soy) belt where grasslands were transformed into annual crops, in particular, in the Prairie Pothole Region. Increased corn production is cited as the primary source of adverse effects from this land conversion. This overly discounts the importance of soybean production in this region in my view (see comments on Chapter 7). As in most areas of study, the estimates of the amount of grassland conversion, the types of grasslands converted, the locations and particularly the data and methods used for these estimates have been contested (Dunn et al, 2017; Conover et al., 2021, Lee et al, 2017; See also analyses by Taheripour et al⁵⁸. In addition, there are some interesting and valuable references produced by scientists working directly in the land areas and among the communities of land users discussed in EPA's preferred citations⁵⁹. Critiques of the use of CDL data
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Lett 10: 044003. <https://dx.doi.org/10.1088/1748-9326/10/4/044003> . Lark, TJ; Schelly, IH; Gibbs, HK. (2021). Accuracy, bias, and improvements in mapping crops and cropland across the United States using the USDA cropland data layer. Remote Sensing 13: 968. <https://dx.doi.org/10.3390/rs13050968> . Lark, TJ; Spawn, SA; Bougie, M; Gibbs, HK. (2020). Cropland expansion in the United States produces marginal yields at high costs to wildlife. Nat Commun 11: 4295; Wright, CK; Larson, B; Lark, TJ; Gibbs, HK. (2017). Recent grassland losses are concentrated around US ethanol refineries. Environ Res Lett 12: 044001. <https://dx.doi.org/10.1088/1748-9326/aa6446> ; Spawn, SA; Lark, TJ; Gibbs, HK. (2019). Carbon emissions from cropland expansion in the United States. Environ Res Lett 14: 045009. <https://dx.doi.org/10.1088/1748-9326/ab0399> ; Zhang, X; Lark, TJ; Clark, CM; Yuan, Y; LeDuc, SD. (2021). Grassland-to-cropland conversion increased soil, nutrient, and carbon losses in the US Midwest between 2008 and 2016. Environ Res Lett 16: 054018. <https://dx.doi.org/10.1088/1748-9326/abecbe> ; Chen, P; Yuan, YP; Li, WH; LeDuc, SD; Lark, TJ; Zhang, X; Lark, TJ; Clark, CM; Yuan, Y; LeDuc, SD. (2021). Grassland-to-cropland conversion increased soil, nutrient, and carbon losses in the US Midwest between 2008 and 2016. Environ Res Lett 16: 054018. <https://dx.doi.org/10.1088/1748-9326/abecbe> 1197 1198 Zhang, XS; Clark, C. (2021). Assessing the impacts of recent crop expansion on water quality in the Missouri River basin using the soil and water assessment tool. J Adv Model Earth Syst 13: e2020MS002284. <https://dx.doi.org/10.1029/2020MS002284>; Johnston, CA. (2013). Wetland losses due to row crop expansion in the Dakota Prairie Pothole Region. Wetlands 33: 175-182. <https://dx.doi.org/10.1007/s13157-012-0365-x> Johnston, CA. (2014). Agricultural expansion: land use shell game in the U.S. Northern Plains. Landsc Ecol 29: 81-95. <https://dx.doi.org/10.1007/s10980-013-9947-0>

⁵⁸ Argonne National Laboratory website: https://greet.es.anl.gov/publication-comment_environ_outcomes_us_rfs).

⁵⁹ Doidge, M., H. Feng, and D.A. Hennessy. 2017. Farmers' motivations for land conversion in the Prairie Pothole Region of North and South Dakota. Proc. Fourth Biennial Conference on the Conservation of America's Grasslands, Fort Worth, TX. 15–17 Nov. 2017; Floress, K., S. Garcia de Jalon, S.P. Church, N. Babin, J.D. Ulrich-Schad, and L.S. Prokopy. 2017. Toward a theory of farmer conservation attitudes: Dual interests and willingness to take actions to protect water quality. J. Environ. Psychol. 53:73–80. doi:10.1016/j.jenvp.2017.06.009 ; Haigh, T., W. Schacht, C.L. Knutson, A. Smart, J. Volesky, C. Allen, M. Hayes, and M. Burbach. 2019. Socioecological determinants of drought impacts and coping strategies for ranching operations in the Great Plains. Rangeland Ecol. Manag. 72:561–571. doi:10.1016/j.rama.2019.01.002; Joshi, D. R., Ulrich-Schad, J., Wang, T., Clay, Dunn, B. H., Clay, S. A., Bruggeman, S.A., and Clay, D. E., 2019. Grassland retention in the North American Midwest after periods of high commodity prices and climate variability. Soil Sci. Soc Amer.; Reitsma, K.D., Clay, D.E., Clay S. A., Dunn, B.H., and Reese, C. 2016. Does the U.S. Cropland Data Layer provide an accurate benchmark for land-use change estimates? Agron J, 108(1)266-272. ; Kline,

	include those by authors working closely with the farm communities in some of the affected states. Most of these references are not cited by US EPA. Reitsma et al (2015), evaluated LCLM change in SD during the period 2006 to 2012, when crop prices and net profits were exceptionally high. These circumstances imply that pressure to increase annual crop production by converting grasslands was increased during this period. To test this, they evaluated data from the CDL and from high resolution remotely sensed images from the National Agricultural Imaging Program. They found that the CDL had large errors in assessing actual land use compared to the use of NAIP image data (see also Reitsma et al., 2016). They relied on image analysis of grasslands, some of which were converted to annual cropland during this period and annual cropland that was converted to grasslands. They reported that net conversion of grasslands occurred during this period, varying greatly by region within the state (a net increase of 2.6 % of lands converted to crops, including forage crops like alfalfa, approximately 1.2 M ac). At the state level, 9.9M ha (24.5M ac) of grasslands remained unchanged (Table 1, page 2366). Most land converted to cropland was of higher quality than land converted from cropland to grasslands. 20K ha (50 K ac) of lands with excess water issues were converted from cropland to grassland. They listed several factors (unranked) leading to these land conversion events: 1. Unprecedented returns to annual crops during the period, 2. changes in land ownership (owned to rented/leased use, 3. Improvements in technology, 4. Government policies, 5. Climate change (increased temperatures and precipitation), and 6. Changes in the agricultural workforce (primarily an aging farmer population, less willing to maintain livestock that made use of grasslands). In a subsequent analysis, Joshi et al (2019), expanded the Reitsma et al (2015) study to include Nebraska and added years up to 2014 . Both studies evaluate the potential for conversion based on land capability classes with higher classes better suited to crop production. They found that less land in higher capability classes was converted than this measure of suitability might suggest, despite strong financial incentives for conversion. They attributed this result to farmers' desires to conserve natural features with high ecological values on their lands and that improved financial returns were overestimated as a motivational factor. They cite several other surveys of farmers' attitudes to support this conclusion. This suggests that conservation programs aimed at preserving important features of the PPR have been on the whole successful. I have been unable to reconcile these diverse views about the reliability of CDL data and conclusions drawn from its use, due to the highly technical nature of the
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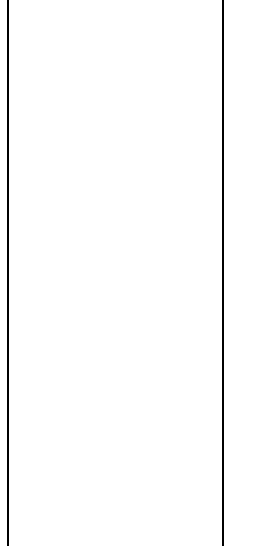
K.L., W. Singh, and V.H. Dale. 2013. Cultivated hay and fallow/idle cropland confound analysis of grassland conversion in the western corn belt. *Proc. Natl. Acad. Sci. USA* 110(31):E2863.
doi:10.1073/pnas.1306646110 ; Reitsma, K.D., B.H. Dunn, U. Mishra, S.A. Clay, T. S. Sutter, and D.E. Clay. 2015. Land-use change impact on soil sustainability in a climate and vegetation transition zone. *Agron. J.* 107:2362–2372
Wang, T., A. Ayesha, D. Hennessy, and H. Feng. 2018. Cropland reflux: Trends in and locations of land-use change in the Dakotas, 2007 to 2012 and 2012 to 2017. South Dakota State Univ. Economics Department.
<https://ageconsearch.umn.edu/record/274938/files/Cropland%20Reflux.pdf>.

	criticisms and assertions offered, especially given the short time period available for review, and the overall length of the RtC3. Plevin et al, 2022, highlights the importance of assumptions about the types of land converted to crop production and its definition with respect to estimating the carbon intensity of fuels produced. This would apply as well to estimates of ecological consequences. It seems to me that this discussion remains unresolved. Given the importance of these studies for EPA's attribution of the environmental effects associated with the RFS, I think EPA should conduct or have conducted independently an analysis of the disputed issues at stake with respect to grassland conversion estimates. This comparative analysis hopefully should be included in the final version of the RtC3 and provided to the congress and the public to help them better judge the consequences of the RFS on environmental effects in the US. 5. Need for more localized state and regional level assessments. EPA seems to have overlooked other sources of information about land conversion and environmental effects from institutions and sources located within the primary areas studied. This issue arises because of a basic approach to attribution that EPA has followed in chapters throughout the RtC3. The agency acknowledges that environmental effects are local, and difficult to quantify at that scale, and that models and modeling efforts can only be general and suggest overall trends, but then relies primarily on models to attribute proportionally environmental effects from farming. In Chapter 14 and perhaps elsewhere, tipping points or irreversible changes are mentioned, based on conceptual analysis. There is another approach that EPA did not consider in making these assessments. EPA has made little use of local knowledge at the state land grant university level, or consult with cooperative extension personnel, did not appear to include much information from NRCS about the implementation of conservation practices, and did not consult with local agricultural organizations, resource conservation districts, water and other agencies that have a close connection to land use where LCLM activities was most of concern, especially in the prairie pothole region. Working with locally knowledge groups and individuals would have provided a check on assertions about environmental effects derived from large scale, remote studies by scientists that may not be familiar with the landscapes they studied. Some level of local assessment, including areas considered hotspots and others where gradients of change exist, would add substance to attributions. This would be a form of ground-truthing. I have cited studies by Reitsma et al, Clay et al, and Joshi et all., from South Dakota State University as examples. There is likely much more information of that type available within the states discussed in the RtC3. If time is constrained, hotspots, where most LCLM occurred might be identified, since it is unlikely that LCLM was uniform across a very large, heterogenous landscape. In some cases, larger rates of conversion may be identified, but with greater precision. But a few local assessments would provide independent verification of the general trends cited by EPA, or provide alternative perspectives.
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⁶⁰ Environmental outcomes of the US Renewable Fuel Standard, 2022. Tyler J. Lark, Nathan P. Hendricks, Aaron Smith, Nicholas Pates, Seth A. Spawn-Lee ,Matthew Bougiea, Eric G. Booth , Christopher J. Kucharika, and Holly K. Gibbs, PNAS, vol 119, no 9; <https://doi.org/10.1073/pnas.2101084119>

	Life cycle assessment models estimate the ILUC effects on fuel CIs or total greenhouse gas emissions based on fuel and feedstock types, farming practices, transportation and logistics, production practices and co-products and their effects. Emissions estimates differ due to choices about the data used, the time frames for analysis, and other factors. Data quality is an important issue. Adding ILUC analysis would significantly lengthen the RtC3, add additional complexity to the analysis, and increase uncertainty further. It would, and does, add complex policy related discussions to the RtC3 that may not be a proper part of the scope of section 204. Since the issue of GHG emissions from biofuels is addressed elsewhere within EPA, this analysis is better without the added complexity of ILUC estimation, especially since effects seem likely to be small, while the uncertainty added of including additional analyses would be large. The literature review task would be time consuming and challenging.
Kurkalova	Overall, the EPA has done a great job with the report. Congratulations! Uncertainties and limitations write-ups A clear explanation of how uncertainties are defined would facilitate the clarity of the uncertainties and limitations sections throughout the report. For example, the differentiation between data uncertainties and the uncertainties related to the process of evaluation of the effects of RFS on the production and consumption of biofuels (system boundaries, choice of time frames, choices among alternative models and approaches, etc.), might help the readers better understand the sources of limitations. One source of scientific uncertainties originates from the varying data accuracy and precision. In Chapter 5, the report does an excellent job in detailing the differences in the alternative data sets providing the information on the spatio-temporal patterns of LULM (section 5.1.2): • USDA-NASS Census of Agriculture, • Survey sample-based: NRI, USDA-NASS annual surveys, FIA • Satellite and other remote data: CDL and NLCD Likewise, in Chapter 11, the report does an excellent job in detailing the differences in the alternative data sets providing the information on the spatio-temporal patterns of irrigation (section 11.3.1): • USDA-NASS Census of Agriculture, • Survey sample-based: USDA-NASS Irrigation and Water Management surveys; USGS water use data; CEAP • Satellite and other remote data To improve the presentation of data limitations, I suggest to provide similarly organized discussions about the differences in the alternative data sources on other relevant farming practices (Chapter 3, section 3.2.1). The report could be more specific on what is known and what is not known about the spatio-temporal patterns of tillage decisions, cover crops, fertilizer use, and chemical pesticides applications.

- The most of the information is already in the report, but a clearer emphasis on what is known and what is not known about the variability across space and time would be very beneficial for understanding the uncertainties and limitations stated, and for pointing to the future research needs.
- For example, for tillage systems and cover crops,
 - USDA-NASS Census of Agriculture not mentioned (see, e.g., Sawadgo and Plastina (2022) for the estimates of dis-adoption of no-till and cover crops based on the county-level Census data)
 - Survey sample-based: CEAP is covered, but USDA-NASS ARMS – not (see, e.g., Claassen et al. (2018) for the estimates of the temporal patterns of no-till based on the ARMS data, and Wallander et al. (2021) – for that of cover crops)
 - Satellite and other remote data are not covered. Seifert et al. (2018), Hively et al. (2019), Beeson et al. (2020), Sharma et al. (2020) provide useful discussion on the current capabilities and limitations of this approach.

Suggested additional references

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Landis	Overarching Recommendations to EPA (other specific recommendations appear in Chapter reports) I have two overarching concerns that influence my responses to the findings of the Draft RtC3. 1) Has to do with the way that the concept of “<u>market certainty</u>” is handled in the report, 2) concerns how pre-existing <u>changes in landscape complexity</u> may interact with potential RFS-induced changes to exacerbate negative environmental impacts. Both issues suggest that the potential impacts of the RFS1,2 may be underestimated by the Draft RtC3. In addition, I would like to note two additional topics that I feel need increased attention in the Draft RtC3. 3) Concerns an expanded emphasis on precision conservation, and 4) addresses the food chain effects of neonicotinoid insecticides. 1. <u>Dismissal of potential market certainty effects.</u> The Draft RtC3 Chapter 6 lines 917- 934 acknowledges a limitation of the assessment regarding how the long-term RFS mandates could have created market certainty that influenced the rapid buildout of the US ethanol biorefinery system and its lasting impacts on land use. <i>“6.3.7 Limitations of the Assessment</i>

*The evidence summarized in this chapter is generally based on modeling results and analyses that use historical economic data on significant factors such as the prices of corn and crude oil. There are a several limitations in the modeling and analyses that may underestimate the impacts of the RFS Program. **Many of these models estimate equilibrium conditions where all markets are cleared. They do not include people in them making decisions, and thus they may underestimate the role the RFS Program played in increasing investor confidence in ethanol production plants by providing a guarantee that there would be a government-mandated demand for ethanol in future years.** The RFS Program provided a level of certainty that there would be a domestic market for ethanol even if crude oil prices dropped to pre-2004 levels and ethanol was not economically competitive with gasoline. The market certainty the RFS Program provided may have accelerated the buildout of the ethanol industry and infrastructure and resulted in greater ethanol production and availability than would have occurred absent the RFS Program (e.g., Figures 6.6 and 6.7). Much of this appears to have been well underway by 2007 and certainly by 2010 (Denicoff, 2007). Potential investors may have had concerns that the federal ethanol tax subsidy would be eliminated or that high oil prices would be temporary. For such investors making decisions on a longer timeline (e.g., 10-15 years), the future requirements of the RFS Program under the statutes may have provided additional confidence in investing in new production capacity, knowing that there would be a minimum level of guaranteed demand for ethanol."*

However, the report subsequently dismisses this potentially large driver saying in lines 938-940. *"All of these factors may be important but are difficult to capture in traditional modeling frameworks and thus are not quantified in the literature. Future research is needed to quantify these market effects."*

I feel this argument has been dismissed too lightly. A quick search of the literature found these potentially relevant articles.

- Hašková, S., Rousek, P. and Horák, J., 2020. Assessment of relation between legislative risk and expected profitability of a subsidized project. *Ad Alta: Journal of Interdisciplinary Research*, 10(2).
- Lade, Gabriel E., C-Y. Cynthia Lin Lawell, and Aaron Smith. "Designing climate policy: lessons from the renewable fuel standard and the blend wall." (2018) *Am J. of Agric. Econ* : 100 (2) 585-599.
- Spangler, K., Burchfield, E.K. and Schumacher, B., 2020. Past and current dynamics of US agricultural land use and policy. *Frontiers in Sustainable Food Systems*, 4, p.98.
- Szabó, Z., 2019. Can biofuel policies reduce uncertainty and increase agricultural yields through stimulating investments? *Biofuels, Bioproducts and Biorefining*, 13(5), pp.1224-1233.

Moreover, farmers who are very sensitive to commodity prices in making planting/management decisions, also experienced this increased market certainty. This became increasingly tangible as local ethanol biorefineries were built and provided an alternative market for corn, often at prices higher than the local elevator was offering.

	The fact that the significant (and durable) increase in the price of corn from an average of \$2.75 to over \$4.00/Bu occurred in 2005-2007 and was coincident with RFS1 (Figure 6.7) would also support a perception by farmers of a long-term and positive change in corn marketing opportunities, potentially supporting production decisions and land values. • Park, J., 2022. Distance to Biorefinery Plants and Its Influence on Agricultural Land Value: Evidence from the United States Midwest Region. <i>Land</i>, 11(9), p.1451. Because these effects could change the amount of biofuel and land use changes attributable to the RFS, with subsequently large impacts on the findings of Chapters 8-15 and subsequently the Executive Summary and Integrated Synthesis, <u>I recommend that EPA more fully investigate the impacts of market certainty as part of the RtC3 and potential impacts on its findings.</u> 2. <u>Omission of agricultural landscape complexity effects.</u> Chapter 5 lines 59-64 notes that changes in land cover and land management (LCLM) is foundational to many of impacts that EPA is required to analyze pursuant to Section 204 of the EISA, and lines 148 -163 define relevant terms including intensification and extensification of agriculture. However, one of the major recognized outcomes of agricultural intensification/extensification is <u>agricultural landscape simplification</u> which is not defined or discussed in the draft RtC3 even though it is highlighted in major conclusions of RtC2 (see RtC3 Chapter 12, Lines 139-141, Chapter 13, Lines 139-140, Chapter 14, Lines 231-233). Changes in agricultural landscape complexity is a well-studied phenomenon with wide ranging effects on biodiversity and ecosystem services. For a variety of economic, technological, and social reasons, similar changes in field-level management practices are often adopted by many land managers, which in sum lead to landscape-level effects. In recent decades, these changes have often been in the direction of landscape simplification. Agricultural landscape simplification is driven by changes in landscape <u>composition</u>, typically reduction in the number of crop and non-crop habitat types that comprise the landscape, and landscape <u>configuration</u>, changes in the size, shape, and spatial arrangement of habitats. The Draft RtC3 documents many of these landscape-level changes but currently fails to fully consider their impact on its findings. Landscape composition and configuration have foundational implications for biodiversity and ecosystem services. As a result, agricultural landscape simplification and its effects have emerged as pivotal concepts in understanding how agricultural systems function. A Web of Science search (2/13/23) on the terms --agricultural landscape simplification -- yielded 286 references addressing effects of landscape simplification on multiple taxa and ecosystem services. These include studies/meta-analyses on; biodiversity (Estrada-Carmona et al 2022), pollinators and pollination (Mallinger et al 2016, Connelly et al. 2015), natural enemies and pest suppression (Rusch et al. 2016, Bianchi et al. 2006), bird communities (Fahrig et al. 2015), and pesticide use and reliance (Meehan et al 2011, Nicholson and Williams 2021), which in turn influence agricultural productivity (Burchfield et al. 2019, Dianese et al. 2019).
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The additive effects of landscape-level change are often more important than individual LCLM decisions. The Draft RtC3 briefly acknowledges this in lines 78-79 of the Executive Summary, *“Despite the finding of relatively modest effects of the RFS Program nationally for the environmental impacts assessed, these may have important cumulative impacts on the environment.”* However, the evidence for how cumulative effects of landscape change influences ecosystem function and services is not well developed nor factored into the findings of relevant chapters. Specifically, Chapter 5 and the subsequent chapters on soil and water quality, water use and availability, terrestrial ecosystem health and biodiversity, aquatic ecosystem health and biodiversity, and wetland ecosystem health and biodiversity, should explicitly consider if *“the relatively modest effects of the RFS Program”* have caused landscape to move beyond tipping points (Grimstead et al. 2018, Lieske et al. 2018, Thies et al. 2008, Watson et al. 2021)

I recommend that EPA conduct a thorough review of the literature on the implications of changes in landscape complexity on biodiversity and agricultural ecosystem functions/processes. These findings should then inform the conclusions and recommendations of the relevant chapters. My expectation is that sufficient evidence exists to influence the findings of Chapters 12-14, potentially Chapters 9-11, and subsequently the Executive Summary and Integrated Synthesis..

References cited.

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- Connelly, H., Poveda, K. and Loeb, G., 2015. Landscape simplification decreases wild bee pollination services to strawberry. *Agriculture, Ecosystems & Environment*, 211, pp.51-56.
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3. Increased emphasis on precision conservation

Precision conservation, i.e. harnessing the tools of precision agriculture explicitly for conservation purposes (Delgado et al. 2008) has emerged as a major concept in agroecosystem management. A Web of Science search (2/15/23) on the term “precision conservation” yielded 129 references. While precision agriculture is referenced in the report, the concept of precision conservation is not discussed. Given that the RtC3 specifically includes sections focusing on the current and future impacts of conservation practices on RFS attributable environmental outcomes, it seems a lost opportunity to not have a section on precision conservation in Chapter 12.3.4 and referenced in other relevant chapters. Therefore, I recommend that EPA conduct a review of the concept of

	precision conservation and its potential to improve environmental outcomes of biofuel feedstock production. • Ali, B. and Dahlhaus, P., 2022. Roles of Selective Agriculture Practices in Sustainable Agricultural Performance: A Systematic Review. <i>Sustainability</i>, 14(6), p.3185. • Delgado, J.A. and Berry, J.K., 2008. Advances in precision conservation. <i>Advances in agronomy</i>, 98, pp.1-44. • Lewandowski, A.M., Tomer, M.D., Buchanan, J.I., Kiel, A., Olson, L., Power, R.L. and Sloan, J.J., 2020. Agricultural Conservation Planning Framework: Watershed applications, research opportunities, and training resources. <i>Journal of Soil and Water Conservation</i>, 75(4), pp.427-433. • Swinton, S.M., 2022. Precision conservation: Linking set-aside and working lands policy. <i>Applied Economic Perspectives and Policy</i>, 44(3), pp.1158-1167. 4. <u>Indirect effects of neonicotinoid insecticides</u> There is increasing evidence that neonicotinoid insecticides are highly persistent in soil and water and have the potential to cause trophic cascades involving both invertebrates and vertebrates. For example, sublethal doses of neonicotinoid seed treatments in slugs are sufficient to cause toxic effects to their predators (carabid beetles) resulting in increased crop damage from slugs (Douglas et al. 2015). The indirect effects of these pesticides in other terrestrial and aquatic food chains are of increasing concern and should be addressed in the Draft RtC3 section 3.2.1.5.2, and referenced in other relevant chapters. Therefore, I recommend that EPA conduct a review of the use and impacts of neonicotinoid insecticides and their impacts on environmental outcomes of biofuel feedstock production. Bakker, L., van der Werf, W., Tiftonell, P.A., Wyckhuys, K.A. and Bianchi, F.J., 2020. Neonicotinoids in global agriculture: evidence for a new pesticide treadmill?. <i>Ecology and Society</i>, 25(3). • Douglas, M.R., Rohr, J.R. and Tooker, J.F., 2015. Neonicotinoid insecticide travels through a soil food chain, disrupting biological control of non-target pests and decreasing soya bean yield. <i>Journal of applied ecology</i>, 52(1), pp.250-260. • Frank, S.D. and Tooker, J.F., 2020. Neonicotinoids pose undocumented threats to food webs. <i>Proceedings of the National Academy of Sciences</i>, 117(37), pp.22609-22613. • Gibbons, D., Morrissey, C. and Mineau, P., 2015. A review of the direct and indirect effects of neonicotinoids and fipronil on vertebrate wildlife. <i>Environmental Science and Pollution Research</i>, 22, pp.103-118. • Lennon, R.J., Isaac, N.J., Shore, R.F., Peach, W.J., Dunn, J.C., Pereira, M.G., Arnold, K.E., Garthwaite, D. and Brown, C.D., 2019. Using long-term datasets to
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	assess the impacts of dietary exposure to neonicotinoids on farmland bird populations in England. PLoS one, 14(10), p.e0223093. • Sánchez-Bayo, F., Goulson, D., Pennacchio, F., Nazzi, F., Goka, K. and Desneux, N., 2016. Are bee diseases linked to pesticides?—A brief review. Environment international, 89, pp.7-11. • Tooker, J.F. and Pearsons, K.A., 2021. Newer characters, same story: neonicotinoid insecticides disrupt food webs through direct and indirect effects. Current Opinion in Insect Science, 46, pp.50-56. Additional Comments • Integrated Synthesis Lines 342-350 Figure IS.2 is called out in the text, but the corresponding figure is labeled IS.3 • Chapter 9 line 158 add underlined words. (1) if corn or soybean production for ethanol <u>or biodiesel</u> displaced ... • Chapter 10 lines 634-36. Cite ○ Basso, B., Shuai, G., Zhang, J. and Robertson, G.P., 2019. Yield stability analysis reveals sources of large-scale nitrogen loss from the US Midwest. <i>Scientific Reports</i>, 9(1), p.5774.
Smith	I have three recommendations that I believe would substantially improve the report. 1. Incorporate the economics literature on investment under uncertainty, which predicts that the RFS reduced the uncertainty inherent in building an ethanol plant and thereby encouraged construction of more plants. All of the modeling in the chapter addresses the amount of ethanol used under an assumption that the ethanol plants have been built. Clarification: Here are some relevant papers. I don't know of studies that model investment in first-generation ethanol. • Dixit, Robert K., Avinash K. Dixit, and Robert S. Pindyck. <i>Investment under uncertainty</i>. Princeton University Press, 1994. • Handley, K., & Limao, N. (2015). <i>Trade and investment under policy uncertainty: theory and firm evidence</i>. American Economic Journal: Economic Policy, 7(4), 189-222. • McCarty, T., & Sesmero, J. (2015). <i>Uncertainty, irreversibility, and investment in second-generation biofuels</i>. BioEnergy Research, 8, 675-687. 2. Report all outcomes in per gallon (or dose-response) terms. What are the estimated environmental effects of each additional gallon of biofuel? 3. The main use of this report will be future policymakers. Write every sentence with an eye to how they can make good policy decisions.

Appendix A

LIST OF REVIEWERS

External Peer Review of U.S. EPA's Draft "Biofuels and the Environment: Third Triennial Report to Congress"

Friday, February 24, 2023: 11:00 AM – 6:45 PM EST

Monday, February 27, 2023: 11:30 AM – 6:30 PM EST

Tuesday, February 28, 2023: 1:00 PM – 3:30 PM EST

Virtual Meeting via Zoom.gov

Reviewers

Sarah C. Davis, Ph.D.

Research Professor
Department of Environmental and Plant Biology
Ohio University
Athens, OH

Stephen R. Kaffka, Ph.D.

Professor, Department of Plant Sciences
Director, California Biomass Collaborative
University of California Davis
Davis, CA

Harry de Gorter, Ph.D.

Professor, Charles H. Dyson School of
Applied Economics and Management
Cornell University
Ithaca, NY

Lyubov A. Kurkalova, Ph.D.

Professor, Department of Economics
North Carolina Agricultural and Technical
State University
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Bernard A. Engel, Ph.D., P.E.

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Purdue University
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Douglas A. Landis, Ph.D.

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Professor, Department of Bioproducts and
Biosystems Engineering
University of Minnesota
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Aaron Smith, Ph.D.

DeLoach Professor of Agricultural Economics
Department of Agricultural and Resource Economics
University of California Davis
Davis, CA

S. Kent Hoekman, Ph.D. (Meeting Chair)

Research Professor, Emeritus
Desert Research Institute
Division of Atmospheric Sciences
Reno, NV

Appendix B

CHARGE TO REVIEWERS

Technical Charge to External Peer Reviewers

Contract No. EP-C-17-017

Task Order 68HERH20F0120 (ERG Task 51)

December 2022

External Peer Review of EPA's Draft:

"Biofuels and the Environment: Third Triennial Report to Congress"

INTRODUCTION

Under Section 204 of the 2007 Energy Independence and Security Act (EISA), the U.S. Environmental Protection Agency (EPA) is required to submit, on a triennial basis, a report to Congress on the impacts to date and likely future impacts of the Renewable Fuel Standard (RFS) Program. EPA published *Biofuels and the Environment: First Triennial Report to Congress*,⁶¹ the first report in the Section 204 series, in December 2011. EPA published *Biofuels and the Environment: Second Triennial Report to Congress*,⁶² the second report in the Section 204 series, in June 2018. The draft of the third report in this series, *Biofuels and the Environment: Third Triennial Report to Congress* (RtC3), is the subject of this peer review.

EISA Section 204 specifies that each triennial report should address the RFS Program's impacts to date and likely future impacts on the following:

- Environmental issues, including air quality, effects on hypoxia, pesticides, sediment, nutrient and pathogen levels in waters, acreage and function of waters, and soil environmental quality.
- Resource conservation issues, including soil conservation, water availability, and ecosystem health and biodiversity, including impacts on forests, grasslands, and wetlands.
- The growth and use of cultivated invasive or noxious plants and their impacts on the environment and agriculture.
- The annual volume of imported renewable fuels and feedstocks for renewable fuels, and the environmental impacts outside the United States of producing such fuels and feedstocks.
- Recommendations for actions to address any adverse impacts found.

ERG, an EPA contractor, has selected nine subject matter experts to serve on an independent external review panel. Collectively, these experts have expertise in nine key areas relevant to RtC3: (1) land use change and remote sensing, (2) agronomy (including tillage practices, water use, and chemical applications [including nutrients, pesticides, and herbicides related to biofuel feedstock production]); (3) agricultural economics, including partial and general equilibrium economic modeling, (4) hypoxia and other impairment of water quality related to feedstock and biofuel production; (5) soil science; (6) air quality; (7) ecology, ecosystems, and biodiversity; (8) biofuel conversion processes; and (9) international biofuel trade and international impacts of biofuel production. During their review of RtC3, peer reviewers will address the following charge questions.

CHARGE QUESTIONS

Part 1: Background and Drivers (Chapters 1 to 5)

⁶¹ U.S. EPA. *Biofuels and the Environment: First Triennial Report to Congress* (Final Report, 2011). U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-10/183F, 2011.

⁶² U.S. EPA. *Biofuels and the Environment: Second Triennial Report to Congress* (Final Report, 2018). U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-18/195, 2018.

For background at the start of the review, all reviewers should read Chapters 1 and 2. While there are no charge questions associated with Chapters 1-5⁶³, reviewers should incorporate any comments they may have on any of these chapters into their response to the chapter-specific charge questions below, as appropriate, or into their response to Question 5 (additional comments).

Part 2: Chapters 6 and 7: Attribution to the RFS Program

Question 1: In the First and Second Triennial Reports to Congress on Biofuels (RtC1 and RtC2, respectively), the Agency could not separate the effects of the RFS Program from the effects of other factors (e.g., market or other policy effects). Many factors simultaneously influenced the production and use of domestic corn ethanol in the U.S., including the need for fuel oxygenates to replace methyl-tert-butyl-ether (MTBE) in gasoline, the Volumetric Ethanol Excise Tax Credit (VEETC), high oil prices, and dozens of individual state biofuel programs and MTBE bans over this period. Because attribution was identified as a major knowledge gap in previous reports, this report includes a new emphasis on attribution, referred to in this report as an “attribution analysis.” Part 2 in this report (Chapters 6 and 7) covers the attribution analysis of the RtC3. For each chapter that you review:

- a) Please describe your overall impressions of the clarity and technical accuracy of material presented in the chapter.
- b) Are the most important data and recent relevant literature cited and appropriately interpreted? Have any important studies that would affect the conclusions been omitted? Please identify any data, literature, or studies you think should be added and describe why you recommend they be included.
- c) Please comment on the selection and use of models (e.g., econometric models, general and partial equilibrium models), the accuracy of analyses, and accuracy of inferences.
- d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement.
- e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.

Part 3: Chapters 8 to 16: Environmental and Resource Conservation Issues

Question 2: In Part 3, Chapters 8 through 16 characterize scientific evidence on potential impacts of biofuel production on air quality, soil quality, water quality, water use and availability, terrestrial ecosystem health and biodiversity, aquatic ecosystem health and biodiversity, wetland ecosystem health and biodiversity, invasive or noxious plant species, and international effects, respectively. For each chapter you review:

- a) Please describe your overall impressions of the clarity and technical accuracy of the material presented in the chapter.
- b) Are the most important data and recent relevant literature cited and appropriately interpreted? Have any important studies that would affect the conclusions been omitted? Please identify any data, literature, or studies you think should be added and describe why you recommend they be included.

⁶³ Note there are also no charge questions for Chapter 17 which is a compilation of other chapters.

- c) Please comment on the selection and use of models where applicable to a given chapter (e.g., GREET, BEIOM, EPIC, SWAT), the accuracy of analyses, and accuracy of inferences.
- d) Are the conclusions and recommendations described in the “Chapter Synthesis” section supported by the material presented? Please describe any suggestions for improvement.
- e) Does the chapter identify the uncertainties and limitations associated with its conclusions? Please describe any suggestions for improvement.
- f) Are the graphics, glossary, and other supporting material useful in clarifying and supporting the discussion and conclusions in the chapter (and supporting Appendices, if any)? Please describe any suggestions for improvement.

All Chapters

Question 3: Considering all chapters you reviewed, as well as the background chapters in Part 1, does the report address the requirements of Section 204 as set forth in Chapter 1 and as reflected in the scope set forth in Chapter 2? If not, please describe what should be added, removed, or changed.

Executive Summary and Integrated Synthesis

Question 4: The Executive Summary provides a high-level overview of the report, and the Integrated Synthesis provides a more detailed overview, including RtC3 scope and content, the report’s conclusions and recommendations, a comparison of RtC3 conclusions with those of RtC2, and a discussion of future reports under EISA Section 204.

- a) Is the presentation of information in the Executive Summary clear? Is the Executive Summary appropriately distilled down from the Integrated Synthesis and the broader RtC3? Please describe any suggestions for improvement.
- b) Is the presentation of information in the Integrated Synthesis clear? Is the Integrated Synthesis appropriately distilled down from the broader RtC3? Please describe any suggestions for improvement.

General

Question 5. Please provide any additional comments you may have on the draft RtC3.

Appendix C

PEER REVIEW MEETING AGENDA

External Peer Review of U.S. EPA’s Draft “Biofuels and the Environment: Third Triennial Report to Congress”

Friday, February 24, 2023: 11:00 AM – 6:45 PM EST

Monday, February 27, 2023: 11:30 AM – 6:30 PM EST

Tuesday, February 28, 2023: 1:00 PM – 3:30 PM EST

Virtual Meeting via Zoom.gov

Note: Daily meeting and Day 2 and 3 discussion start times are fixed; discussion end times may be adjusted by reviewers.

Agenda

DAY 1: Friday, February 24

11:00 AM	Brief Meeting Opening <i>Jan Connery, ERG (facilitator)</i>
	Opening Remarks <i>Christopher Frey, EPA</i>
11:05 AM	Meeting Purpose, Agenda, Process & Reviewer Intros <i>Jan Connery, ERG</i>
11:40 AM	U.S. EPA Office of Research and Development Presentation ... <i>Christopher Clark, EPA</i>
Noon	Public Comments <i>Jan Connery, ERG</i>
12:25 PM	Reviewer Discussion Process <i>Jan Connery, ERG</i>
12:30 PM	BREAK
12:45 PM	Reviewer Discussions
	Chair Opening Remarks to Panel..... <i>Peer Review Chair</i>
	Charge Question 1 (Chapters 6 and 7)
12:50 PM	Chapter 6: Attribution: Corn Ethanol and Corn <i>Peer Review Panel</i>
2:00 PM	BREAK
2:15 PM	Chapter 6: Attribution: Corn Ethanol and Corn (cont.)..... <i>Peer Review Panel</i>
3:20 PM	BREAK
3:30 PM	Chapter 7: Attribution: Biodiesel & Renewable Diesel..... <i>Assigned Reviewers</i>
4:30 PM	BREAK
	Charge Question 2 (Chapters 8 to 16)
5:00 PM	Chapter 14: Wetland Ecosystem Health and Biodiversity..... <i>Assigned Reviewers</i>
5:45 PM	Chapter 16: International Effects <i>Assigned Reviewers</i>
6:45 PM	ADJOURN Day 1

External Peer Review of U.S. EPA’s Draft “Biofuels and the Environment: Third Triennial Report to Congress”

Agenda (cont.)

DAY 2: Monday, February 27

11:30 AM	Reviewer Discussions	
	<i>Charge Question 2 (Chapters 8 to 16, cont.)</i>	
11:30 AM	Chapter 8: Air Quality	<i>Assigned Reviewers</i>
1:00 PM	Chapter 9: Soil Quality	<i>Assigned Reviewers</i>
2:00 PM	Chapter 10: Water Quality	<i>Assigned Reviewers</i>
3:00 PM	Chapter 12: Terrestrial Ecosystem Health and Biodiversity	<i>Assigned Reviewers</i>
4:00 PM	Chapter 13: Aquatic Ecosystem Health and Biodiversity	<i>Assigned Reviewers</i>
5:00 PM	Chapter 11: Water Use and Availability	<i>Assigned Reviewers</i>
5:45 PM	Chapter 15: Invasive or Noxious Plant Species.....	<i>Assigned Reviewers</i>
6:30 PM	ADJOURN Day 2	

DAY 3: Tuesday, February 28

1:00 PM	Reviewer Discussions	
	<i>Charge Question 3 (All Chapters)</i>	<i>Peer Review Panel</i>
	<i>Charge Question 4 (Executive Summary & Integrated Synthesis)</i>	<i>Peer Review Panel</i>
	<i>Charge Question 5 (General)</i>	<i>Peer Review Panel</i>
2:15 PM	Individual Reviewer Recommendations	<i>Peer Review Panel</i>
2:55 PM	Closing Remarks	<i>Jan Connery, ERG</i>
3:30 PM	ADJOURN MEETING	

Appendix D

ORAL PUBLIC COMMENTS



RFA Comments to the External Peer Review Meeting for Biofuels and the Environment: Third Triennial Report to Congress

February 24, 2023

Good morning. My name is Scott Richman, and I am the chief economist for the Renewable Fuels Association, the leading trade association for U.S. ethanol producers. We appreciate the opportunity to comment on the external review draft of *Biofuels and the Environment: Third Triennial Report to Congress*. We also appreciate the public nature of this meeting.

Overall, a considerable amount of research is reflected in the draft report, and clearly a substantial amount of work by EPA staff and others went into writing it. However, there are several issues with the content that we hope will be addressed in the final report.

The RFA commends EPA for its efforts to separate the effects of the RFS on environmental outcomes from the effects of the overall production and use of biofuels, which have also been influenced by market factors and other federal and state policies. However, it is unfortunate that a quantitative attribution analysis was conducted only for corn ethanol, resulting in uneven treatment of different biofuels.

The Agency also conducted an analysis of land cover and land management, for which data for the total cropland category in USDA's National Resources Inventory were separated into four subcategories. However, EPA then combined corn and soybeans into a single category, masking the divergent trajectories of the two crops over the last decade.

Corn acreage reached a recent peak in 2012, and on average over the last five years the area planted to corn has been 7.1 million acres below that level, while soybean area has been 7.5 million acres higher. Since quantitative attribution analysis was done only for corn ethanol, and the EPA concluded that the RFS "likely played a relatively minor role ... in the growth of corn ethanol in the U.S. from 2002-2012," analyzing corn separately would be more appropriate. EPA also gave insufficient consideration to the role of urbanization in causing conversion of other land to cropland.

Additionally, the draft report is exceedingly over-reliant on work by Tyler Lark and associated researchers. The term "Lark *et al.*" appears at least 65 times in the report, not including footnotes. However, the methods used in the 2022 study by Lark *et al.* have been critiqued or refuted by the USDA; researchers from Argonne, Purdue and the University of Illinois system; and even the EPA itself.

EPA noted shortcomings in the study's attribution of increases in corn ethanol production to the RFS, stating that the estimates are "higher than other studies that include other relevant factors because of several assumptions in the underlying economic model ... that increase the estimated effect of the RFS Program."

In a memorandum submitted to the docket for EPA's proposed Set rule, USDA identified "major methodological flaws" with the paper, including "[f]ailure to account for cropland-to-cropland conversions" and "(mis)classification of CRP land as native or longer-term grasslands."

The experts from Argonne, Purdue and the University of Illinois system cited numerous problems, stating, "In conclusion, Lark *et al.* significantly overestimated the land use implications of ethanol production."

One of the authors of the 2019 and 2022 papers by Lark *et al.* is a peer reviewer for the triennial report. Accordingly, there should be heightened diligence to ensure that the review of how Lark's work is used in the report is thorough and objective.

We intend to elaborate on these issues and others in our written comments. Thank you again.

Oral Comments to the Third Triennial Group Review Committee

Steve Vander Griend
ICM Inc.

Thank you for allowing me to speak today.

My comments are primarily focused on Part 3, Section 8, from this Third Triennial Report to Congress which pertains to air quality.

I have personally initiated multiple studies ranging from ethanol's effects on octane performance, to ethanol's impact on vehicle tailpipe and evaporative emissions.

Let me first say that I am extremely frustrated at the conclusions of this report that assumes ethanol makes overall air quality worse. The ethanol industry should be using this report and the models developed by EPA to promote the benefits of ethanol, but instead have to ignore these results since EPA has been using flawed science for the past decade.

This Third Triennial Report, related to vehicle emissions, primarily relies on EPA's 2018 RFS Anti-Backsliding Study which then relies on a model known as MOVES 2014. An EPA model used for estimating vehicle emissions but this model is not based on real world fuels.

There are 2 key flaws supporting the EPA's MOVES model related to vehicle tailpipe emissions and both of these errors lack good scientific judgement. Unfortunately to this day, there are no standards for how test fuels should be blended, or how fuels should be modeled with the inclusion of ethanol.

First, match blending aromatic levels means ethanol is being tested with increasing levels of aromatics in the gasoline blendstock. In EPA's 2017 Fuels Trend Report, that span calendar years 2006 to 2016 EPA states - ***Ethanol's high octane value has also allowed refiners to significantly reduce the aromatic content of the gasoline, a trend borne out in the data.***

Why match blend aromatics in these vehicle studies when we see oil refinery economics and market trends basically match blend gasoline's saturate hydrocarbons. Ethanol today should actually be credited for removing roughly 8 billion gallons of toxic aromatics from the US gasoline supply annually.

Second, utilizing match blending distillation when creating test fuels with increasing ethanol content is also flawed. Since ethanol has a unique impact on this ASTM distillation test method, it is not possible to isolate the effects of ethanol while also creating negative changes to the gasoline base fuel. With match blending aromatics and or match blending distillation, a gasoline base fuel will significantly increase vehicle emissions prior to the addition of ethanol.

This issue with match blending has affected dozens of vehicle studies that have negatively impacted ethanol as a fuel component.

This not only eliminates the forecasted benefits of ethanol but also hides the dangers of not fully identifying the health concerns of toxic aromatics in our fuel supply.

I also believe EPA should remove the referenced CRC E94-2 and E94-3 vehicle study from this Triennial Report. The E94-2 study was again a match blended but CRC E94-3 states this is a splash blended fuel study, meaning ethanol was simply added but the distillation of the fuels raises questions. Also, the 9 to

10 month gap between fuels tested raises concerns and hard to imagine this study passing an independent peer review process.

Another issue with EPA's MOVES model is the effects ethanol has on evaporative emissions. EPA relies on CRC vehicle studies where again the make-up of test fuels should be called into question.

It is beyond comical in my view that in Section 8 of this Triennial Report, it states that E85 increases permeation relative to E10. We should be asking, why are there no referenced studies that look at aromatics impact on emissions from permeation?

Or why there are no studies with increasing ethanol and lower aromatics that speciate evaporative emissions to model the benefits of lowering aromatics? The answer here is that whoever controls how the test fuels are blended control the outcome of the study.

The emission factors going into the EPA's MOVES model are not based on Real World Fuel Changes. If we don't use real world test fuels nor set real world fuel defaults within the MOVES model, do we really expect to model real world emission inventories?

I was a participant of the EPA's Review Group for the last version of the MOVES Model. As it relates to modeling air quality with-in this Third Triennial Report to Congress. All air quality modeling should be quarantined until an independent review is conducted.

Thank you for allowing my comments.
Steve Vander Griend