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Comment

Interactive comment on “ESP v2.0: enhanced method for exploring emission impacts of future scenarios in the United States – addressing spatial allocation” by L. Ran et al.

L. Ran et al.

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We would like to thank the reviewers for their positive and very helpful comments. We believe that we have addressed each comment in modifications that we have made to the manuscript. Below, the comments are numbered, and a response to each is provided. We also provide both clean and markup versions of the revised document.

Comments by Ben Hobbs, Reviewer #1

Comment 1.1: It is desirable to have a method that can site new and operate new power production facilities in a way that reflects new technological, policy, and economic trends, as we and others attempt to do with spatially and temporally explicit

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electricity market models [1].

Response: We agree that a methodology for siting such sources would be a very desirable component of a long-term emission projection system. The reviewer provides an excellent reference to accompany this discussion, and we have chosen to add that citation and several others, including Cohon et al. (1980), which involves multi-objective power sector siting, and Kraucunas et al. (2014), which describes the PRIMA modeling framework that includes an electric utility siting component. To address the comment further, we clarify in the Introduction that our methodology does not currently include point source siting. Later, in Conclusions, we highlight this omission as a limitation and state that it may be explored for incorporation in future versions of ESP.

Comment 1.2: I would add just one other limitation to their list, which is that the methodology does not account for shifts in emissions locations due to changes in electricity generation technology and resulting alterations in siting patterns. Nor does it down-scale emissions to an hourly level consistent with daily meteorology. The latter is needed to account for correlations of high demand (and thus emissions) periods with the warm meteorological conditions conducive to tropospheric ozone formation. Accounting for such finescaled temporal relationships should receive more attention because impacts during ozone episodes may be more than proportionally affected by emissions changes [2].

Response: These are excellent suggestions. We have reworked the conclusions to discuss limitations in more detail. In particular, we highlight that the temporal and spatial resolution of the underlying components of ESP v2.0 are not currently capable of addressing the effects on energy and emissions of meteorological variability. We mention, however, that it could be advantageous to explore using ESP conjunctively with a more detailed electric sector model that incorporates a finer temporal resolution and that treats dispatch considerations more fully. We also now reference the Chen et al. article indicated by the reviewer.

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Comments by Anonymous Reviewer #2:

Comment 2.1: The findings of these case studies are highly dependent on the ICLUS inputs, so the methods used in ICLUS to extrapolate population and land-use changes out to 2050 should be described more fully.

Response: The reviewer's suggestion to provide additional detail regarding ICLUS processes and assumptions is very helpful. While a detailed discussion of ICLUS is beyond the scope of this paper, we have added a table to the document in which we provide additional information about the projection and point to a reference from which more information is available.

Comment 2.2: It would also be helpful if the authors would add a few sentences to better describe the future regulations included in the energy systems modeling, since these assumptions have a strong impact on the case study results.

Response: We have attempted to address the reviewer's comment in our description of MARKAL in the new Table 1. This text provides information about the origin of the projection and which regulations are included. We also now clearly indicate that regulations that have not been finalized are not included.

Comment 2.3: The authors might also clarify how readers can access the ESP v. 2.0 tools and case study outputs for use in other modeling studies.

Response: A section is included at the end of the paper discussing model and data availability.

Comment 2.4: The authors should consider making growth factors and surrogate shapefiles available for intermediate years between 2005 and 2050.

Response: It is our preference to distribute data for 2005 and 2050 only at this time as the surrogate files are computationally intensive to develop and require considerable storage space. We indicate that interested parties may contact us for additional information.

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Comment 2.5: I recommend that the authors clarify that the growth factors shown in Figure 4 represent "2050 population / 2005 population".

Response: This change has been made.

Comment 2.6: The caption for Figure 5 could better distinguish between the regional growth factors shown in the left panels and the county level growth allocations shown in the right panels. (Use of the term growth factors in both cases is confusing.)

Response: We have updated the text for the caption to more clearly reflect the information in the panels.

Comment 2.7: Captions for Figures 9 - 12 would be easier to read if they used full descriptions of the cases being compared, rather than summary labels.

Response: We address this comment by adding parenthetical expressions to describe each of the scenarios being compared.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/8/C842/2015/gmdd-8-C842-2015-supplement.zip>

Interactive comment on Geosci. Model Dev. Discuss., 8, 263, 2015.

GMDD

8, C842–C845, 2015

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