

Thank you, all the authors, for putting together important manuscript. Here are my observations and suggestions:

General Comment:

- Please consider using the abbreviation consistently once it has been defined and also define it when it is used for the first time. Example: Distributed Wind (DW) and Energy Burden (EB) are defined but the full term appears again in some of the places. Similarly, add CONUS full first when used first.

Data and Methodology

- Table 1 shows several important datasets however there are additional factors that are important when looking at energy burden which is missing. EB is strongly influenced by the climatic condition through heating and cooling requirements. Variables such as heating degree days, cooling degree days, Heating and cooling demands, housing age/quality are important while analyzing energy burden. Counties with older houses might have higher energy burden regardless of income level. Please discuss how this omission may affect the interpretation of the reported results and consider addressing these or mention this in the limitation carefully.
- The analysis combines datasets from multiple year. While this approach is understandable given the data availability, it would be helpful to include brief discussion of whether socioeconomic and demographic conditions were assumed to be constant between baseline and future years.
- Please clarify the source of the residential electricity expenditure data and describe how were these expenditures calculated
- Line 89-90: The manuscript states that AEPDemand is calculated using total electricity demand, but later discusses residential, commercial, industrial, and total AEPDemand metrics. It would be helpful to explicitly describe how demand is calculated for each sector.
- Line 103: The energy burden equation is calculated using county-level aggregate expenditures and estimated aggregate income. Is there any literature which has done similar analysis? If yes, I would suggest to cite that as it differs from the conventional household-level energy burden metric commonly used in the literature. Noting (wherever appropriate) that this is county level aggregation indicator rather than household-level EB would be helpful to the readers.
- Line 110: Please justify why this formula is called energy burden relative to GDP? This looks more like energy cost intensity/energy stress. If similar approaches have been used previously, citing relevant references would strengthen the justification.

- Line 117: Please explain what additional insight NER provides beyond EB.
- Line 125-Since NERGDP is derived from EBGDP, please explain why both EBGDP and NERGDP are needed and how their interpretations differ.
- The manuscript defines energy poverty using $EB > 6\%$ based on Colton (2011). While this threshold is widely used in the literature, the authors may also wish to reference more recent studies and federal energy justice frameworks that continue to employ or discuss the 6% benchmark, thereby strengthening the justification for the selected threshold. Additionally, these thresholds are based on total energy expenditure whereas this study focuses on electricity related expenditure. Please make sure any limitation is clearly stated.

Results:

- Line 136-137: The manuscript states that residential, commercial, and industrial sectors are included in the analysis. However, most of the results, figures, and discussion focus on residential EB and residential AEPDemand. The role of commercial and industrial sectors in the final findings and state classifications is not clearly presented. Please clarify whether these sectors influenced the ranking framework and final conclusions, or consider presenting additional sector-specific results.
- Please provide stronger justification for assigning 50%, 25% and 25% for the weights.
- Line 186: The phrase “that ranks highly” could be clarified for readability?
- Given that ranking framework depends on selected weights, Adding sensitivity analysis for ranking using alternative weighting schemes would strengthen confidence in the robustness of the state rankings.
- The manuscript mentions multicollinearity test was conducted however it is not discussed in the results section in detail.
- Line 310: The manuscript frequently refers to "strong alignment" between EB and DW potential. However, the reported nonparametric correlations in several Group 1 states range from approximately 0.22 to 0.37, which are more representative of weak-to-moderate or moderate relationships. Please consider using more cautious terminology when describing these associations.
- Since the highest reported nonparametric correlations are below 0.4, the manuscript may benefit from discussing the practical significance of these effect sizes. While the observed

relationships are positive, they do not indicate exceptionally strong spatial correspondence between EB and distributed wind potential.

- Some state-specific interpretations need more discussion. For example, Alabama appears to have very high energy burden but weaker distributed wind alignment. Similarly, the explanation for Texas should be supported with additional evidence or stated more cautiously.
- Line 365: What does author mean by significance of the state level random effect? Is there any statistical test done for this claim?
- Line 373-378, authors has mentioned that regressions was done for key states but it is not added in the results. Also, Please clarify the criteria used to select these states and discuss whether the selected states are representative of the broader dataset.
- The manuscript states that the state-level regressions exhibit stronger statistical performance. Consider reporting additional diagnostic statistics such as residual plots, AIC/BIC values, RMSE, or cross-validation metrics to better demonstrate the improvement relative to the mixed-effects model.

Discussion and Conclusion

- Agricultural employment is identified as a significant predictor of energy burden. This needs to be highlighted in results and please provide additional discussion regarding the underlying mechanism linking agricultural activity to higher energy burden and clarify whether this variable may be acting as a proxy for other rural socioeconomic characteristics.
- Throughout the Discussion, the manuscript suggests that DW deployment could alleviate or reduce energy burden in highly ranked states. However, the analysis only identifies spatial overlap between energy burden and DW potential. Since actual deployment, adoption rates, financing mechanisms, ownership structures, and electricity bill savings are not modeled, these conclusions should be stated more cautiously.
- The discussion occasionally implies that states with stronger EB–AEPDemand correlations are better candidates for reducing energy burden through distributed wind. However, correlation alone does not guarantee successful implementation or meaningful affordability improvements. Additional factors such as policy environment, permitting, financing, utility structures, and technology adoption should be acknowledged.
- States such as Alabama and Texas provide important counterexamples to the overall narrative. Additional discussion of why these states deviate from expected patterns could provide valuable insight into the limitations of distributed wind as a strategy for addressing energy burden.

- The manuscript should more explicitly acknowledge that important determinants of energy burden, including electricity prices, climate conditions, housing quality, building efficiency, and housing age, were not included in the analysis and recommend for future analysis.
- While the study successfully identifies the locations where DW and energy burden coincide, additional analysis would be required to see if the wind is sufficient to install the system, if installed to quantify potential affordability, bill savings, can household afford the system?, how much EB will actually decrease, how much of the electricity bill is offset etc.