

Response to Referees

We very much appreciate the thoughtful comments from the referee. We have sought to address as many of the comments as possible and believe that the manuscript is much improved with these modifications. You will also find a marked-up and a clean manuscript version attached to this submission. We thank the journal's editors for the opportunity to submit these revisions. Our detailed responses to referee comments and changes to the manuscript are summarized in the table below.

	Referee	Comment	Authors' Response
1	4	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.	We have corrected it throughout the entire manuscript to use DW and EB consistently
		Data and Methodology	
2	4	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.	We agree that climate-related and housing-quality variables are important determinants of energy burden that are not directly incorporated in this analysis. We have added the following limitation to the manuscript: “This analysis does not directly incorporate weather-related variables (e.g., heating/cooling degree days) or housing characteristics (e.g., age, thermal efficiency), both of which can influence EB independent of income. Because EB is calculated from realized electricity expenditure, some of this variation is implicitly embedded in the data, but the models do not isolate it, so its influence on covariates such as poverty and agricultural employment cannot be ruled out. Future work incorporating these variables directly could refine the predictors identified here.” (Lines 81-85)
3	4	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.	We have clarified this assumption in the text: “Economic and demographic data used in this analysis reflect the most recent available estimates rather than year-specific projections, as projected demographic and economic data at comparable county level resolution were not available for this study. Only DW generation potential and electricity demand vary by scenario year. This represents a data availability constraint rather than an assumption of no future change, and the

			framework presented here could be readily applied with updated demographic projections as they become available” (Lines 86-90)
4	4	Please clarify the source of the residential electricity expenditure data and describe how were these expenditures calculated	Residential electricity expenditure is drawn from the NLR SLOPE dataset (NLR, 2025) as indicated in Table 1, the same source used for electricity demand, which provides county-level electricity consumption and expenditure disaggregated by residential, commercial, and industrial sectors. Per SLOPE, electricity expenditure projections are based on 2016 U.S. Energy Information Administration data from the Annual Energy Outlook 2019; projection methodologies are described on the SLOPE website. County-level residential expenditure values represent electricity expenditure aggregated across residential parcels within each county, using scenario-specific (2025 or 2035) business-as-usual projections. Table 1 has been updated to explicitly list this dataset as the source for both energy total demand and residential electricity expenditure.
6	4	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.	A clarification has been added to Section 2.2: "For sector-specific analyses, the denominator is replaced by the corresponding sector's demand alone; for example, residential AEPDemand divides cost-viable residential AEP by residential electricity demand only. All demand values are drawn from the NLR SLOPE dataset, which provides county-level electricity consumption disaggregated by sector (NLR, 2025)." (Lines 101-103)
6	4	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.	We have added a clarifying sentence (Lines 115-118) to Section 2.2 noting that this county-level aggregate formulation differs from conventional household-level or census-tract-scale EB metrics (Ross et al., 2018; Scheier and Kittner, 2022), and that the county-level approach is adopted to maintain spatial consistency with the DWEFS data and support the spatial prioritization objectives of this study.
7	4	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.	We agree the naming warrants clarification and have added a sentence (Lines 126-128) to Section 2.2 explaining that the EB nomenclature is retained for consistency with the residential EB metric rather than to imply an exact equivalence with household-level energy burden definitions.
8	4	Line 117: Please explain what additional insight NER provides beyond EB.	We have clarified in Section 2.2 that NER provides additional value beyond EB by remaining mathematically defined and interpretable across the full income range, including at very low income levels where EB can become distorted or undefined. The following sentence was added: "While EB measures the

			proportion of income spent on energy -- offering a direct sense of affordability -- it can produce distorted or infinite values for households with very low or near-zero income (Scheier and Kittner, 2022). NER addresses this by remaining mathematically defined and interpretable across the full income range, while also providing smoother statistical properties and a complementary interpretive angle." (Lines 129-132)
9	4	Line 125-Since NERGDP is derived from EBGDP, please explain why both EBGDP and NERGDP are needed and how their interpretations differ.	We clarify that EBGDP and NERGDP, like EB and NER, are mathematically related but offer complementary interpretive framings: EBGDP communicates the share of economic output absorbed by energy costs (a burden-oriented framing), while NERGDP communicates economic return relative to energy expenditure. Retaining both supports consistency with the residential-level EB/NER pairing and allows readers to interpret results through either lens. (Lines 145-148)
10	4	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.	We have added a recent citation supporting the 6% threshold: Dreho et al. (2020), a widely cited ACEEE report that explicitly uses the 6% benchmark to define high energy burden households. The existing text already notes that our EB is calculated using electricity expenditure only rather than total household energy costs, making our values not directly comparable to the 6% benchmark. This limitation is stated explicitly in the manuscript: "these thresholds include all energy sources (e.g., natural gas), whereas we calculate EB for electricity only." (Lines 151-152)
		Results:	
11	4	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.	We have added a clarifying paragraph at the end of Section 2.4 explicitly addressing the role of each sector. While AEPDemand and EB-related metrics are computed for residential, commercial, and industrial sectors, the composite ranking and grouping framework is based specifically on residential-sector metrics. This choice is guided by the empirical finding that residential AEPDemand shows the most pronounced correlation with residential EB across all sectors analyzed (Sect. 3), making it the most informative metric for identifying states where DW potential spatially aligns with energy affordability challenges. Commercial and industrial sector results are presented as descriptive context in Section 3 but do not directly influence group classification.
12	4	Please provide stronger justification for assigning 50%, 25% and 25% for the weights.	The weighting rationale was substantially expanded in the previous revision round in response to similar comments from Referees 1 and 3, and is presented in full in Section 2.4. We have additionally clarified that the weights are intentionally goal-driven rather than derived from statistical optimization, and that alternative

			weighting schemes are possible, with the following sentence added: "These weights are not derived from statistical optimization and alternative weighting schemes are possible; however, the chosen structure directly supports the study's focus on affordability-relevant siting rather than maximum energy generation."
13	4	Line 186: The phrase "that ranks highly" could be clarified for readability?	The phrase has been revised to "states with the highest composite rankings according to Eq. 8" for improved clarity. (now Line 205)
14	4	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.	During the analytical process, several alternative weighting formulations were evaluated, varying the components and weights assigned to correlation, AEPDemand, and absolute demand. Additionally, multiple statistical robustness procedures and tests were conducted throughout the analysis — including comparisons across parametric and nonparametric correlation methods, Box-Cox transformation sensitivity checks, and pairwise scenario comparisons — which consistently supported the overall patterns reported. A summary of the weighting sensitivity analysis has been added to Section 3.4: while the exact composition of groups varied across schemes, the overall framework consistently distinguished states with high spatial alignment between EB and DW potential from those with high DW potential but weaker EB alignment, supporting the interpretive value of the grouping approach even if specific state assignments may shift under alternative weights. This is consistent with the explicitly goal-driven and transparent nature of the weighting scheme described in Section 2.4.
15	4	The manuscript mentions multicollinearity test was conducted however it is not discussed in the results section in detail.	Multicollinearity results are reported in Section 3.5 for both the mixed-effects model ("Diagnostics of the mixed-effects model show no multicollinearity") and the state-level regressions ("no multicollinearity among main-effect predictors, $VIF \leq 5$ "). The VIF threshold of 5 used to assess multicollinearity among main-effect predictors before including interaction terms is described in Section 2.5. These results indicate that multicollinearity was not a concern in any of the models presented, and no further action was required beyond confirming this diagnostic.
16	4	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.	We agree and have revised the terminology throughout the manuscript, replacing phrases such as "strong alignment" and "strong correlation" with more cautious language including "most pronounced," "meaningful association," and "positive spatial association," consistent with the weak-to-moderate nonparametric correlation values reported in Tables 2–4.

17	4	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.	Consistent with the previous response, we agree and have revised the terminology throughout the manuscript to use more cautious descriptive language. As discussed in the Conclusion in Sec. 5, we do not claim strong spatial correspondence but rather highlight location-specific opportunities, i.e. in lines 496-498: “DW deployment could provide benefits to targeted areas facing significant economic hardship or could serve as a stimulus for agricultural regions, where energy needs may be high”
18	4	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.	Alabama and Texas are both discussed extensively in the manuscript as Special Cases in Section 3.4 and further elaborated in the Discussion (Section 4.1). For Alabama, the manuscript explicitly states that it "represents a boundary case where EB is the highest but DW alignment is comparatively weak," that it "ranks near the national median for both total and residential AEPDemand," and that "DW in Alabama could be viewed as a conditional and localized opportunity — potentially relevant in select counties — but unlikely to serve as a primary or statewide solution." For Texas, the near-zero correlation between EB and DW potential is reported numerically (Table 4, nonparametric correlation = -0.05) and attributed to the state's high electricity demand diluting AEPDemand and its large internal diversity. We have reviewed both sections and confirmed that the discussion is appropriately cautious and supported by the reported data.
19	4	Line 365: What does author mean by significance of the state level random effect? Is there any statistical test done for this claim?	We have clarified in Section 3.5 that the importance of the state-level random effect is supported by the intraclass correlation ($ICC = 0.40$), indicating that approximately 40% of the total variance in EB is attributable to state-level differences beyond what is explained by the fixed-effect covariates. This substantial ICC confirms meaningful interstate variability and motivates the state-specific grouping framework.
20	4	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 state-level regression results are reported in full in Section 3.4, including correlation values, key predictors, interaction terms, and model fit statistics for all selected states in Tables 1, 2, and 3. Regarding selection criteria, the states were chosen based on the composite ranking framework described in Section 2.4: Group 1 states represent those with the highest composite rankings (strong spatial alignment between EB and DW potential combined with substantial demand scale); Group 2 states represent those with the highest AEPDemand independent of EB alignment; and Special Cases represent states that do not clearly fit either group but exhibit distinct features warranting separate consideration. This selection is explicitly tied to the grouping framework rather than statistical sampling, and the states are therefore representative of distinct deployment-

			relevant contexts rather than a random sample of the broader dataset. We have added a clarifying sentence to Section 2.5 making this explicit.
21	4	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.	The state-level models are evaluated using a comprehensive set of diagnostics already reported in Section 3.5, including adjusted R ² , Kolmogorov-Smirnov tests for residual normality, Breusch-Pagan tests for homoskedasticity, and VIF for multicollinearity. These diagnostics directly demonstrate the improvement over the mixed-effects model, which showed significant heteroskedasticity and mild non-normality. Given that the state-level models are used as interpretive and explanatory tools rather than predictive ones, we consider the reported diagnostics sufficient to support the claims made.
		Discussion and Conclusion	
22	4	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.	We have added a sentence to Section 4.2 clarifying that agricultural employment likely acts as a proxy for broader rural socioeconomic disadvantage — including lower and more variable incomes, older housing stock, and limited access to utility programs — rather than reflecting a direct causal mechanism, and should be interpreted accordingly.
23	4	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.	We note that the DW potential used in this analysis is not raw technical potential but cost-viable AEP, derived from the DWEFS framework (Lockshin et al. 2025), which already incorporates economic feasibility through net present value, payback period, threshold capital expenditure, industry-validated capex and opex, and local electricity rates. This provides a stronger basis for deployment-relevant conclusions than spatial overlap alone. Nevertheless, we agree that adoption rates, financing mechanisms, and ownership structures are not modeled, and we have reviewed the Discussion to ensure language around EB alleviation is framed as potential opportunity rather than guaranteed outcome, consistent with the existing cautionary language already present in Section 4.
24	4	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.	We agree and therefore the manuscript includes explicit cautionary language on this point in Section 4: "DW potential does not by default indicate energy cost savings, so care must be taken to ensure there will be reasonable savings before DW project design or implementation" (Lines 480-481) and "Households or communities with elevated EB may also find investments in DW projects unaffordable, especially without suitable financing or funding options." (Lines 484-485) The grouping framework is also explicitly described as an interpretive tool rather than a prescriptive prioritization scheme. Nevertheless, to more directly address the referee's concern, we have added a sentence to Section 4

			acknowledging that correlation-based prioritization does not account for policy environment, permitting constraints, utility structures, and technology adoption barriers, all of which must be considered in any actual deployment strategy.
25	4	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.	Alabama and Texas are explicitly identified and discussed as Special Cases precisely because they deviate from the patterns observed in Group 1 and Group 2. In Section 3.4, Alabama is described as "a boundary case where EB is the highest but DW alignment is comparatively weak," with DW characterized as "a conditional and localized opportunity — potentially relevant in select counties — but unlikely to serve as a primary or statewide solution." Texas is discussed in terms of its near-zero spatial correlation between EB and DW potential (nonparametric correlation = -0.05), attributed to its vast electricity demand diluting AEPDemand and its large internal demographic and economic diversity. Both cases are further discussed in Section 4.1, where Alabama is used to reinforce "the importance of distinguishing between non-negligible DW opportunity and deployment relevance at scale" and the need for "complementary strategies in states where wind alignment is limited." We therefore consider these limitations already addressed in the manuscript and respectfully refer the referee to Sections 3.4 and 4.1.
26	4	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.	We agree. This limitation is explicitly acknowledged in Section 2.1: "This analysis does not directly incorporate weather-related variables (e.g., heating/cooling degree days) or housing characteristics (e.g., age, thermal efficiency), both of which can influence EB independent of income... Future work incorporating these variables directly could refine the predictors identified here."
27	4	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.	We agree and note that the underlying DWEFS data (Lockshin et al. 2025) already considers high-resolution wind resource/speeds in its modelled data, and we also note that the manuscript already acknowledges in the Conclusions that: "correlation results do not indicate definitive cost savings, but they highlight an opportunity to explore energy hardship relief through strategic deployment of DW technologies. Future research could... explore cost-benefit analyses of DW deployment in specific counties." We have added a sentence to the Conclusions explicitly recommending future research into wind sufficiency thresholds, projected bill savings, and household-level affordability assessments as natural next steps following this spatial prioritization framework.