Response to Referee #1

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 editors for the opportunity to submit these revisions. Our detailed responses to referee #1's comments and changes to the manuscript are summarized in the table below.

Comment	Lines Rev artcl	Author Response
The manuscript Spatial and economic prioritization for distributed wind by Abril Guevara et al. explores locations that behind-the-meter distributed wind energy could be deployed to benefit customers experiencing high energy burden. One of the underlying datasets to the analysis, the Distributed Wind Energy Futures Study, has a valuable resolution to provide localized guidance as to where distributed wind projects could be advantageously deployed. While this manuscript has potential to be helpful in distributed wind decision-making, I have two significant concerns and urge major revisions prior to publication.	NA	We thank the referee for their thoughtful review of the manuscript. We have addressed both major concerns and minor concerns in a way that makes the manuscript stronger.
Major concern #1. Categorizing states with low wind resource into Group 1 that represents High Economic Burden and High Distributed Wind Potential	Lines 282, 374	We agree with the referee's comment. To more accurately reflect the characteristics of the states in this group, we have renamed it from "High-Need, High-Potential States" to "High-Need/Demand, Favorably Correlated Potential". This describes more literally what we did in our weighted ranking and emphasizes that these states exhibit high energy burden in areas where distributed wind potential is favorably aligned, without implying high absolute wind resource.

Is it appropriate to include states like Georgia and Louisiana in Group 1: High-Need, High-Potential States? High-need, sure. But high-potential? You even state on Lines 214-216 that Louisiana's AEP and AEP_{Demand} are low and assign all the reasoning behind the placement into Group 1 to EB. The wind resource in states like Georgia and Louisiana is quite low. Take a look at the USGS Wind Turbine Database. If large utility-scale developers are cautious about building wind farms with higher hub heights in such states, is it acceptable to suggest that Louisiana and Georgia have high potential for lower hub height BTM DW? I recommend reevaluating this dual nature classification when some states are heavily skewed by one of the two considerations.	Lines 172-175, 293-295, 304-308, 314-319	We agree that absolute wind resource alone would not justify categorizing states such as Georgia or Louisiana as "high potential." To address this, we changed the group name and clarified throughout the Methods, Results and Discussion sections that Group 1 reflects *favorably correlated potential* rather than high absolute wind resource. Those states are included in this group because county-level variation in residential distributed wind potential aligns closely with variation in energy burden, meaning that higher-EB counties systematically coincide with relatively greater AEP_Demand, even when statewide wind resources are modest. We explicitly note that this does not imply suitability for extensive wind development (either utility-scale or distributed), but rather highlights opportunities for targeted, behind-the-meter or community-scale distributed DW deployment where affordability benefits may potentially be greatest. We have revised the text accordingly to avoid misinterpretation.
I hold the same concern for Alabama as I do for Louisiana and Georgia. I completely agree with your statement that the need is "urgent" from an EB standpoint, but what are you suggesting here for DW's role? Again, this is a state with very low wind resource and I urge caution on drawing relationships concerning the potential of DW relative to high EB. Alabama is indeed noteworthy when it comes to EB. This does not imply that DW will be a successful solution to that problem.	Lines 332-341, 413-418	We agree that high energy burden alone does not imply that distributed wind (DW) is an appropriate or effective solution, particularly in states with limited wind resources such as Alabama. We have revised both the Results and Discussion sections to explicitly characterize Alabama as a *boundary case*. Specifically, in the Results, we now emphasize that Alabama ranks near the national median in AEP_Demand and does not meet the top composite thresholds, despite its exceptionally high energy burden. We explicitly state that DW in Alabama could be viewed as a *conditional and localized opportunity* rather than a statewide solution.

		In the Discussion section, we further clarify that Alabama may not warrant broad deployment prioritization and that any DW application could be supplementary and limited to select local contexts.
Lines 278-270: Again, I am deeply concerned about the inclusion of states like Georgia and Louisiana, which represent some of the lowest wind resource in the continental United States, alongside Iowa, which has some of the highest wind resource, in Group 1 which you characterize as having high residential wind potential.	Lines 403-406	We added this clarification: *"Louisiana, while ranked only 28th in residential AEP_Demand and 32nd in total AEP, exhibits one of the strongest EB–AEP_Demand correlations nationally. In this context, "potential" reflects relative, localized suitability rather than statewide wind abundance, suggesting that targeted, behind-the-meter or community-scale deployment in specific counties may be impactful even in low-resource states."*
Lines 296-297: Particularly for Alabama, are there regions that you identified that have high poverty rates, agricultural industry, and viable wind resources? If so, please add them to the Special Cases discussion so that energy planners in these specific locations can benefit from your analysis. Also, how are you defining "viable wind resources"?	Lines 332-341, 413-418	As mentioned above, we have revised the manuscript to clarify that Alabama's distributed wind potential is *moderate rather than high*, ranking near the national median in AEP_Demand. While Alabama exhibits extreme energy burden and a modest EB–AEP_Demand correlation in some regions, this alignment is weaker than in Group~1 states and does not support statewide prioritization. We now explicitly frame Alabama as a conditional and localized opportunity where distributed wind may play a supplementary role in select counties, rather than a primary strategy for addressing energy burden.
Lines 311-314: "It is important to clarify that while variables like unemployment and poverty frequently emerge as key covariates in explaining EB, this does not imply that distributed wind deployment will directly reduce those underlying socioeconomic conditions. However, by potentially lowering energy costs in areas where these conditions are prevalent, distributed wind may help ease energy-related hardship and indirectly contribute to improved quality of life." This is a great	Abstract, Lines 50-52, 214-224	We strengthened the language in the abstract to explicitly note the non-causal nature of the findings: *"While noting that these associations do not imply causal effects, we group states into two categories and special cases based on correlation strength and DW potential. This highlights potential opportunities to improve energy affordability through targeted siting of distributed wind projects."* In the Introduction, we added clarifying language to set expectations early in the paper:

takeaway and I think you should use it as a caution alongside your results much earlier in the text.		*"While our analysis does not imply causality, it identifies meaningful spatial and statistical associations between distributed wind (DW) potential and energy burden (EB), as well as between EB and adverse economic conditions."* Finally, we added a cautionary paragraph in the Methods section describing the scope and limitations of the regression analysis: *"This modeling framework identifies key demographic and economic factors statistically associated with energy burden. It does not imply that interventions such as distributed wind deployment will directly directly alter the covariates involved.* *The mixed-effects and state-level regression models are therefore used as explanatory and diagnostic tools to contextualize the spatial correlation results and composite ranking framework. These models are not used to determine the weights, thresholds, or group classifications directly, nor are they intended for prediction or causal inference. Instead, they serve three purposes: (i) to confirm that EB exhibits statistically significant variation across states, motivating state-specific analysis framework; (ii) to identify socioeconomic factors consistently associated with elevated EB, providing interpretive context about high EB regions that emerge in the correlation analysis; and (iii) to assess whether states grouped by correlation patterns exhibit distinct EB drivers. In this way, the regression results support interpretation and validation of the grouping framework rather than defining it."*
Major concern #2. Concerns about the methodology, including weighting procedures and adding in counterbalancing factors to better align with the study aims. Additionally, some level of proofreading would have avoided the most significant equation being	Lines 152-161	We clarified the weighting rationale in the *Weighted ranking and grouping* subsection (Section 2.4). The composite ranking assigns 50% weight to correlation-based metrics to ensure that spatial alignment between distributed wind potential and energy burden is the dominant criterion, consistent with the study's primary objective. This threshold was selected as the minimum weight that guarantees correlation influences rankings without overwhelming scale-based

dropped, which made understanding the analysis difficult.		considerations such as AEP_{Demand} and absolute electricity demand. Additional text was added to explain why lower or higher weights (e.g., 45% or 60%) would respectively underemphasize alignment or overemphasize correlation at the expense of deployment relevance. The full composite scoring equation (Eq. 8) has also been restored and clearly defined.
Lines 120, 126, 205, and 279: The reference to an equation is listed as Eq. ??	Lines 167-169	Equation added as well as its description.
Line 120: Why does the ranking emphasize correlation strength so heavily, and how did you decide to go with half the total weight instead of, say 45% or 60%? There needs to be some scientific reasoning behind the weighting scheme that isn't clear.	Lines 156-161	This clarification was added in the Methods section: *"A weight of 50% was selected to ensure that correlation serves as the dominant but not decisive criterion in the composite ranking. Assigning less than half the total weight would allow scale-based metrics to outweigh alignment between DW potential and EB, undermining the central objective of the study. Conversely, assigning substantially more than half the weight would risk over prioritizing statistical alignment at the expense of deployment relevance and practical impact. The 50% threshold therefore represents a balance point at which correlation is guaranteed to influence rankings while preserving sensitivity to demand magnitude and generation scale."*
Line 125: Just the letter v is here.	Lines 167-169	We acknowledge the error in which the composite scoring equation (formerly referenced as Eq. ??) was inadvertently omitted due to a formatting issue during manuscript preparation. This omission understandably made the methodology difficult to follow. Revision made: • The full composite scoring equation has now been restored (Eq. 8), with all variables, weights, and ranking steps explicitly defined. • All references to "Eq. ??" (Lines 120, 126, 205, and 279 in the original submission) have been corrected.
Lines 130-133: Why not simply normalize the AEP_{demand} results by each state's Total	Lines 162-166	We do normalize by state electricity demand, which we have clarified in the text better. We also include residential demand as a

Electricity Demand? Maybe I'm missing something because of the Eq. ?? = v issue. Could you elaborate, scientifically, as to why you included residential demand as a counterbalancing factor, beyond just improving the alignment of the ranking with the study's aim? The current phrasing in the transparency note could be easily misconstrued as cherry picking to achieve the results you wanted.		counterbalancing factor *"to account for scale effects and avoid disproportionately prioritizing low demand* [states] *where standardized ratios may appear favorable despite limited potential impact."* This better reflects the ranking objective of identifying states with higher potential benefits from DW relative to EB.
Additional comments: Line 16: Specify that you are speaking of hub heights. Also, do you have a reference for the height information? Many FOM turbines, in particular, have larger hub heights than 30-60 m.	Lines 16-18	We have clarified these are hub heights and have expanded this definition to include (as a general reference but not a hard cutoff) hub heights up to 80m per the 2024 Annual Technology Baseline [1] and the 2024 Distributed Wind Market Report [2]. [1] NLR (National Laboratory of the Rockies). 2024. "2024 Annual Technology Baseline." Golden, CO: National Laboratory of the Rockies. https://atb.nrel.gov/. [2] Lindsay Sheridan, Kamila Kazimierczuk, Jacob Garbe, and Danielle Preziuso. 2024. "Distributed Wind Market Report: 2024 Edition". U.S. Department of Energy.
Lines 19-22: Do you have references or elaboration you can provide here? Help the reader understand how, for instance, BTM applications can stimulate local economic development when appropriately sited.	Lines 20-24	We have expanded the text to explain that local economic development could be driven by DW through job creation or revenue generation and provided references.
Line 63: Can you please explain the exclusion of Alaska and Hawaii?	Lines 69-70	The Distributed Wind Energy Futures Study, from which the underlying DW data is sourced for this study, does not have data available for AK and HI thus they could not be included. We have explained this in the text.

Section 2.2: Offer context for each metric as to what it means when they are high or low.	Lines 85-125	We revised Section 2.2 to explicitly describe the substantive interpretation of each metric, clarifying what high and low values indicate in terms of energy affordability, economic stress, and distributed wind deployment relevance.
Lines 126-129: The concept of the two groups and special cases is confusing. Why two groups? What are the special cases? States that don't fall into either classification? For what reasons? How do you intend each of the multiple classifications to uniquely highlight potential priority areas? There needs to be some kind of contextual link between your classification methodologies and what each is expected to elaborate to the reader.	Lines 172-194	The criteria for classification behind Group 1, Group 2 and the special cases have been expanded in the Methods Section 2.4. The two groups were defined based on salient results from the analysis as well as the study motivation to understand how DW and EB trends relate to each other spatially. The special cases are states that do not fit into the group classification but have noteworthy and relevant results that we expand upon.
Line 136: "states modeled as random effects" – what does this mean? Can you provide some background understanding of mixed- and fixed-effects modeling to help your readers?	Lines 200-203	The explanation was expanded in Section 2.5.
Lines 175-176: How significantly does the transformation alter the ranking?	Lines 257-262	We made this clarification: *"Box--Cox transformation was applied only for descriptive visualization and Pearson correlation diagnostics. These Pearson correlations (using Box--Cox–transformed variables) and Kendall's Tau correlations (using untransformed variables) produced similar results on the rankings, indicating that the results are robust to transformation choice. However, all state rankings and group classifications are based on untransformed metrics and nonparametric (Kendall's Tau) correlations, ensuring that the transformation does not influence the study's prioritization results."*
Lines 177-181: Can you add some commentary as to why DW would be economically favorable in these states?	Lines 265-269	We have expanded the text to explain that economically feasible and favorable DW potential results from different combinations of factors such as strong wind resource, competitive DW costs compared to

Wind resource? Cost of electricity? Policies?		elevated electricity rates, policies like net metering for revenue generation or favorable ordinances impacting siting and system sizing, all of which are captured by the dWind model from the Distributed Wind Energy Futures Study (DWEFS).
Lines 182-186 and Figure 5: Texas and Georgia are important enough to warrant discussion in the text and mention in Figure 5's caption, but they're not actually shown in Figure 5. Should the reader be looking elsewhere to visualize the takeaways for these states?	NA	Figure 5 shows states with a high percentage of counties with AEP_{Demand} above the national median. Texas and Georgia have a high absolute number of counties with AEP_{Demand} above the national median, but they are not as high a percentage out of their total counties. This is why they have been deemed important to call out but are not reflected in Figure 5; the text and caption explain this. We have decided not to make modifications.
Line 218: "specially" -> "especially"?	Line 309	This has been corrected, thank you.
Line 222: "thigh" -> "the high"?	Line 324	This has been corrected, thank you.
Section 3.5: This section is two sentences long with no tables, graphics, analysis, or numeric metrics. It should probably be deleted if it's receiving so little attention from the authors.	Lines 341-357	Thank you for the suggestion, we have combined and expanded the original Sections 3.5 and 3.6 into the new Section 3.5 more cohesively.
Conclusions: Most of these paragraphs are 1-2 sentences long and could easily be linked. Additionally, it would be helpful to include a paragraph on how a state-level energy decision maker will benefit from this work, particularly if you have identified their state in one of the highlighted groups. What are some next steps they could take?	Lines 348-354	We appreciate the suggestion and have now streamlined and combined some Conclusions paragraphs. We have also added some takeaways on how this study could be helpful to state or local decision-makers.
References: Need to be organized according to last name instead of first name to make citations easily findable from references in the text.	Lines 493-530	Thank you for identifying this issue. References are now organized alphabetically according to the Copernicus template for manuscripts on WES.