

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 <i>favorably correlated potential</i> 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 <i>boundary case</i>. 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 <i>conditional and localized opportunity</i> 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: <i>“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.”</i>
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 <i>moderate rather than high</i> , 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: <i>“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.”</i> 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.		<i>“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.”</i> Finally, we added a cautionary paragraph in the Methods section describing the scope and limitations of the regression analysis: <i>“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.</i> <i>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.”</i>
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 <i>Weighted ranking and grouping</i> 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: <i>"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."</i>
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 <i>"to account for scale effects and avoid disproportionately prioritizing low demand [states] where standardized ratios may appear favorable despite limited potential impact."</i> 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 Prezioso. 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: <i>"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."</i>
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.

Response to Referee #2

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 #2's comments and changes to the manuscript are summarized in the table below.

Comment	Lines Rev artcl	Author Response
This paper leverages existing data sets such as the Distributed Wind Energy Futures Study to evaluate the overlap of potential distributed wind energy generation with areas experiencing large energy burden. The study provides a novel level of spatial resolution and represents a useful step forward in understanding how distributed wind maybe deployed to benefit specific communities. The objectives, methods, results, and discussion are generally well written and easy to follow. However, I have found two major issues and a few minor issues that I would like to see revised ahead of publication.	NA	We thank the referee for their thorough review of this paper and their thoughtful comments. We have addressed all comments in the paper and responded to them below in a way we believe strengthens the paper.
Major Issue 1: Given the dependence upon DWEFS AEP for many of the findings, I did not find much discussion about the methods of the DWEFS and what limitations those previous methods might impart on this study. The most significant concern I have is based on Table 7 in Lockshin et. al, 2025. In that table,	Lines 67-80, Table 1	We have clarified in the text that we exclusively use cost-viable AEP (based on the DWEFS' "economic potential") since they are more relevant to this study's focus on energy affordability and readily-deployable technologies. As suggested, this does arrive at one limitation of this study that results from the DWEFS AEP data, which is that these results (for the referenced Baseline scenarios) assume a 30% federal investment tax

Lockshin presents AEP results for two scenarios: one based on technical viability, and another based on cost viability. Which of these did you use? If you used AEP based on just technical viability, then this seems to directly conflict with the underlying focus on energy affordability. If you used AEP based on cost-viability, are these results dependent upon the status of the federal investment tax credit or incentives? If so, can you comment on the potential impact from recent changes to incentives such as the investment tax credit?		credit as outlined in the 2022 Inflation Reduction Act, as well as location-specific tax credit bonuses (including the energy communities bonus, low-income communities bonus and Tribal lands bonus). Due to the recent changes in federal policy, the investment tax credit and related bonuses are likely to have a reduced impact on offsetting project costs and therefore reduce cost-viable AEP unless other mechanisms are identified, although this might happen on a state, county or project-specific basis. We have included this important limitation in the text in the Methods Section 2.1 as well as the conclusion (Section 5). We have also included a second limitation arising from missing DWEFS data across counties in states such as New Mexico, Kentucky, Indiana, South Dakota, Utah, Nevada and Colorado.
Another related concern that I have is ensuring that the DWEFS data and publications are being clearly referenced. In Line 36 and Table 1 you reference the “Distributed Wind Energy Futures Study (Lockshin et al., 2025).” However, this specific reference is “A parcel-level evaluation of distributed wind opportunity in the contiguous united states” which is not the DWEFS report. Can you please clarify in the text how this reference and the DWEFS report are related? Furthermore, when you mention the DWEFS in the text (line 42) you also reference the DOE WETO Wind Data Hub where the data can be accessed. When I follow the link in this reference (https://wdh.energy.gov/project/dw), it does not take me to a specific data set. After some searching, I believe the link should be	Lines 37-47, 72-75, 482-483	Thank you for flagging this issue. The DWEFS refers to the multi-year research effort dedicated to modeling and exploring distributed wind generation potential in the United States, and the mentioned reports and data products are all outputs of the overarching study. The Lockshin et al. (2025) paper is the most up-to-date publication of the core study results and details the updated methodology and scenario results. The underlying data has been published in the WETO Wind Data Hub as another study output. We have corrected this link to point to the BTM dataset. We have clarified this in the text (introduction and other relevant mentions).

https://wdh.energy.gov/project/dw/data or https://wdh.energy.gov/ds/dw/btm .		
Major Issue 2: There are instances where the positive impact of DW on energy affordability is taken for granted or is perhaps just a bit overstated. For example, on line 119 you state “we generate a weighted ranking to identify states where distributed wind deployment could be most effective at improving residential energy affordability.” Another example is on line 349, where the conclusion states “Taken together, these findings enhance our understanding of how distributed wind could impact energy affordability and offer guidance on where such solutions are most likely to have meaningful impact.” In these instances, I am not convinced that you have actually evaluated how DW could positively impact affordability. To your credit, you have clearly shown the relationship of potential DW energy generation to regions where energy affordability is a challenge. However, you have not presented any metrics explicitly demonstrating that DW is more affordable than the existing energy supply. Although this is a major concern, I believe the concern could be easily remedied by removing any statements that overstate impact of DW’s affordability. Alternatively, you could provide support of DW’s affordability compared to existing energy sources, but this would require much more effort.	Lines 148-151, 472-473	We wrote in a more precise way: <i>“After computing both parametric and nonparametric correlations, we construct rankings at the state level and then combine them into a weighted ranking to identify states where DW deployment opportunity could be most strongly aligned with EB.”</i> Also in the conclusions: <i>“Taken together, these findings enhance our understanding of where distributed wind deployment could be most relevant to affordability challenges and offer guidance on where such solutions are most likely to have a meaningful impact.”</i>

Minor issues: Line 30 – You mention that access to renewable energy technologies could be explored as a potential avenue of relief for those affected by high EB. Could you add a sentence or two explaining why you have chosen to explore distributed wind, as opposed to other technologies such as solar PV? Or perhaps a similar study for PV is also warranted?	Lines 32-36	We have chosen DW because the initial motivation for this study arises from a need to understand potential DW impacts and benefits better, which is generally understudied compared to other technologies like solar PV. We have briefly expanded this explanation and added other relevant references.
Multiple locations – DW acronym is used inconsistently. For example, see lines 44 and 46 where both “DW” and “distributed wind” are used.	NA	Thank you for identifying this issue, we have corrected it throughout the entire manuscript to use DW consistently after the acronym is defined.
Lines 120, 127, 205, and 279 – Equation numbers are not present. It seems there is likely an equation missing, or at least formatting issues with the equation references.	NA	Thank you for identifying this issue, this has been addressed throughout the manuscript.
Figure 1 – The caption states “County-level ((e) 2025 – (f) 2035),” but county level results are (c)2025 – (d)2035. Line 282 – Energy burden acronym is already defined earlier in document	Fig. 1	Thank you for identifying this issue, this has been addressed.
Multiple locations – There are many paragraphs that appear as only one or two sentences. I would combine many of these smaller paragraphs together into larger, standard paragraph lengths.	NA	Thank you for this suggestion, we have incorporated it where relevant throughout the manuscript.

Response to Referee #3

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 #3's comments and changes to the manuscript are summarized in the table below.

Comment	Lines Rev artcl	Author Response
Overall Comment The manuscript addresses an important and timely question at the intersection of energy burden and distributed wind deployment opportunity. However, because the grouping framework and regression analysis form the backbone of the prioritization argument, the reader needs additional clarification, justification, and methodological transparency before the results can be fully evaluated. The concerns listed below combine feedback from two co-reviewers.	NA	We thank the referee for their thoughtful review of our paper and appreciate their consideration of other referee comments. We have addressed every comment in the text and written explanatory responses for each below, as well as addressed and responded to every comment from the two other referees.
Major Concerns from Reviewers 1. Missing Composite Scoring Equation The state grouping framework is a central contribution of this manuscript; however, it is currently not possible to fully evaluate or reproduce this framework because the composite scoring equation is missing (Eq. ??). Further, because the composite score directly determines state rankings and group assignments, the absence of this equation prevents a thorough methodological review and raises concerns about reproducibility.	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.

Recommendation: Please insert the full composite scoring equation, clearly defining all terms, weights, normalization steps, and ranking logic.		
2. Clarification and Justification of the State Grouping Logic Closely related to the previous concern, the manuscript would benefit from clearer justification of the grouping logic used to classify states. How was grouping determined? Why two groups and special cases? The reasoning for this particular approach is missing in the methodology. Currently, grouping appears to rely on a combination of composite ranking, correlation between EB and AEPDemand, and narrative “special cases,” which makes the framework difficult to interpret consistently. Recommendation: present a 2×2 quadrant categorization based on need (energy burden level), and Opportunity (AEPDemand level) with spatial correlation or overlap used as an additional diagnostic indicator rather than the primary grouping criterion. Such a framework could help clarify the logic of the composite score and make prioritization decisions more transparent. Or, add more justification for a correlation-driven grouping approach (The authors acknowledge limitations of this approach for states such as California and Texas).	Lines 172-194	We have revised the Methods section to explicitly clarify the logic, intent, and justification of the state grouping framework. The two groups and special cases are designed to distinguish between (i) states where energy burden and distributed wind opportunity coincide spatially and at scale, and (ii) states where distributed wind opportunity exists but affordability-driven alignment is weaker. • Group 1 is now clearly defined as states that rank highly under the composite metric, which integrates energy burden, distributed wind generation relative to demand, and spatial alignment between the two. These states represent contexts where affordability challenges and distributed wind opportunity coincide geographically, justifying prioritization for affordability-oriented DW deployment. • Group 2 is explicitly based on absolute AEP_Demand, independent of EB–AEP spatial correlation. This group highlights states where distributed wind deployment may be attractive for broader energy or economic development goals rather than direct energy-burden mitigation. • Special cases are now formally defined as boundary conditions of the framework, capturing states where extreme EB values, scale effects, or weak spatial alignment prevent clear classification. Their inclusion is intended to avoid overgeneralization and to emphasize the limits of statewide prioritization when spatial coincidence is limited.

3. Limited Interpretation and Integration of Regression Results The regression analysis described in Section 2.5 is presented as an important analytical component of the study; however, its role in the overall framework is unclear. While the models are described as explanatory rather than causal, the manuscript does not clearly explain how the regression results inform the grouping framework or broader conclusions. Specifically, it is unclear whether the regression results are intended to justify the weighting or thresholds used in the composite score, whether they are meant to validate the grouping framework, whether they will be used for future prediction or scenario analysis, or whether they serve only as descriptive background analysis. In addition, the manuscript provides limited interpretation of regression coefficients in substantive or policy-relevant terms. Key modeling choices are also insufficiently motivated, such as the use of a linear specification, the selection of covariates, and the appropriateness of state-level aggregation given acknowledged spatial heterogeneity within states. As a result, the regression results feel analytically disconnected from the grouping framework and do not clearly advance the decision-	Lines 195-224	The mixed-effects and state-level regression models are 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 energy burden (EB) exhibits statistically significant variation across states, motivating state-specific analysis; (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. For this reason, we do believe it is an important analytical component of the study, yet we clarify in the text how this analysis is used in the interpretation of results and how it is not used in group definition.
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making narrative of the paper. Recommendation: Please expand the discussion of the regression models to clarify their analytical purpose, justify key modeling choices, and explain how (if at all) the results feed into the grouping framework or future applications.		
4. Background and Contextualization The authors find “stronger correlations between DW potential and residential energy burden in regions where burden is closely tied to poverty rates and agricultural activities.” The authors also note on Line 170 that to better understand energy affordability, future work must explore “broader economic and geographic factors.” The intersection between distributed wind potential and rural markets has been getting more and more attention, especially under DOE’s <i>Rural and Agricultural Income and Savings from Renewable Energy (RAISE)</i> Initiative. Contextualizing the landscape of current trends (rural electrification, transition away from natural gas) could better inform where there are particular regions or intrastate pockets that may be more suitable for DW. Is it possible to disaggregate AEPDemand further by agricultural sector?	Lines 49-50	While disaggregating AEP_Demand further by agricultural sector was out of the scope of this study, we appreciate the recognition of the importance of this type of work. We have referenced another study [1], performed by some of the authors from this paper that focuses more on DW potential by agricultural sector (primarily disaggregating by land use categories and crop types in agricultural areas). We do not expand on current trends in rural areas as these have been beyond the scope of this study, but acknowledge more clearly in the text that these should be researched and considered when pursuing ag-specific efforts. [1] Crook et al. 2025. Environ. Res. Lett. 20 114055. https://doi.org/10.1088/1748-9326/ae13bc
Minor Concerns from Reviewers 1. Proofreading and Clarity / Grammatical Considerations	NA	Thank you highlighting some grammatical issues. We have proofread the manuscript carefully and believe we have addressed all concerns, including spelling numbers under 10 out.

The manuscript would benefit from a careful proofreading pass once the major concerns are addressed. Line 25: Numbers under 10 should be spelled out.		
2. Technical Details The authors note that “Distributed wind systems typically consist of a single wind turbine or several turbines at heights between 30 and 60 m” but BTM-projects for industrial energy users are typically large – greater than 1 MW, featuring turbines at heights of 80+m. There are some mid-sized DW projects for industrial energy users closer to the 60m mark, but it may be helpful to delineate how DW technical specifications may change based on end-user (e.g., residential is likely to be small wind turbines [10kW or less, at 30m hub height] vs. industrial is more likely to be larger wind turbines).	Lines 16-17	We have clarified that distributed wind systems may have taller hub heights, and they are not limited to the range we state either. We also included references based on known U.S. projects. We have decided not to expand on hub heights by end-user as this is not the focus of the study.
Line 22: Stimulating local economic development is a strong claim for BTM systems, which are the focus of this analysis. This would be more common for a utility-scale FOM system where a larger workforce is needed to support development, and where there may be potential for local tax revenue generation or similar.	Lines 20-24	We have clarified that BTM may stimulate local economic development primarily through opportunities for workforce development as well as income generation. This is certainly different and smaller in scale than local tax revenue generation by FOM projects yet still an important value proposition for BTM systems.
The analysis draws on outputs from the DWEFS, which utilizes threshold capital expenditure to show economic feasibility	Lines 29-31, 110-117,	This study does not account for whether maximum system costs will be feasible for high EB/low NER households, although this is indeed an important consideration. This type of analysis would have

based on maximum viable system costs. Maximum system costs will likely not be feasible for high EB/low NER households. Do the authors account for this in the analysis? Also please define the NER metric earlier in the manuscript, as you do with energy burden in Line 23.	441-443	significantly expanded the scope and the required data or resources. We have made note of it in the Conclusion section as a limitation/area for future study. We have also expanded the definition for the NER metric in the Introduction and Section 2.2.
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