

This paper presents a detailed evaluation of wind and solar energy generation estimates derived from E3SM SCREAM regionally refined simulations over California, with comparisons against HRRR, E3SM-25km NARRM, and EIA-reported capacity factors. The topic is timely and relevant, particularly given the growing importance of high-resolution meteorological modeling for renewable energy assessment. The modeling framework and the scope of the analysis are potentially valuable.

However, in its current form, the manuscript requires substantial revision before it can be considered for publication. The primary issues concern clarity, precision, and organization of the writing, as well as conceptual ambiguities in the framing of the research questions and interpretation of results. Detailed comments are provided below.

Major comments

1. Several abbreviations are introduced without clear or consistent definition, particularly in the abstract.
 - (1) The abbreviation RRM is used inconsistently. The manuscript refers to “regional mesh refinement” in the context of CARRM and “regionally refined model” in the context of NARRM. A consistent naming convention should be adopted and clearly introduced at first use.
 - (2) SCREAM is introduced in a convoluted manner in the abstract (“derived from the US Department of Energy’s Simple Cloud-Resolving Energy Exascale Earth System Model (E3SM) Atmosphere Model (SCREAM)”), which makes it unnecessarily difficult to identify what SCREAM stands for. The full name should be stated clearly and directly at first mention.
 - (3) ERA5 and MERRA-2 are repeatedly referenced without explicitly stating that they are reanalysis datasets, nor is it specified which meteorological variables from these datasets are used. In multiple places, the manuscript refers to the reanalysis data as “meteorological models” or “meteorological model inputs,” which is inaccurate and potentially misleading.
2. The research questions listed in the introduction are not well separated conceptually and often mix scientific questions with methodological choices.
 - (1) First goal (generation modeling assumptions): This appears primarily to be a methodological sensitivity analysis, yet it is framed as a main scientific objective. It is not clear, until later sections, why this comparison is essential for the broader goals of the paper. If this is a key component, the motivation and what the “generation modeling assumptions” represent should be stated more clearly in the introduction.
 - (2) Second goal (meteorological models): The phrase “what do meteorological models represent” is vague. It is unclear whether this refers to differences in simulated meteorological fields or differences in model physics. The authors should be more precise in their wording. In addition, the comparison between SCREAM-3kmCARRM forced by a 2005–2014 climatology and HRRR simulations from 2018–2022 is not a fair comparison. While the manuscript argues that monthly climatology justifies this approach, interannual variability

during 2018–2022 may still strongly affect the results, and this issue is not adequately addressed. There is also an inconsistency in how the “2010climo” period is defined: it is described as 2005–2014 in the text, whereas Table 1 lists 2010–2019. This discrepancy should be clarified and made consistent.

3. The manuscript states that SCREAM-RRM requires nudging because it lacks a scale-aware deep convection scheme. However, nudging does not directly address the absence of scale-aware convection. The authors should clarify the actual purpose of nudging in these simulations, for example whether it is primarily used to constrain large-scale circulation. The choice of a 2-day relaxation timescale also appears relatively weak and should be justified. In addition, the explanation of how “reduced nudging strength near the surface and model top” allows “free-running conditions over California” while maintaining large-scale constraints elsewhere is not sufficiently clear.
4. Section 3.1 emphasizes substantial uncertainties in the EIA monthly data and explicitly states that model–EIA differences should be interpreted as “discrepancies” rather than biases. Given this, the scientific value of using EIA data as the primary reference for evaluating model performance is unclear. The authors should clarify how conclusions about model skill can be drawn when the reference dataset itself is highly uncertain, and whether the PLUSWIND dataset might provide a more reliable benchmark.
5. Section 3.2 (“Evaluation group design and key questions”) is vague and reads more like informal notes than formal academic writing. This section should be rewritten to clearly and concisely describe the evaluation framework.
6. The statement that “each following subsection presents results by energy type (wind or solar)” is misleading, as the first evaluation (generation modeling assumptions) applies only to wind. A consistent subsection addressing generation modeling assumptions for solar is missing.
7. Sections 3.3.2–3.3.6 are unevenly structured. Some subsections emphasize resolution effects even though they are nominally focused on meteorological model differences, while others contain only a single paragraph. The organization of these subsections should be improved to better align with the stated evaluation goals.
8. In Figure 4 and subsequent figures, the EIA curves appear different between the top and bottom panels. The reason for this difference is unclear and should be explained.
9. In some regions (e.g., Kern County and Southern California), the seasonal cycles between datasets do not appear as similar as claimed in the text, for example the statement that “both models captured the July peak well.” These interpretations should be revisited and aligned more closely with what is shown in the figures.

10. Lines 475–485 discuss deficiencies in the simulation of large-scale circulation, but then abruptly conclude that large-scale circulation plays a relatively minor role. The logical connection between these statements is unclear and requires clarification.
11. Section 3.4.1 aims to assess the impact of meteorological models on solar energy, but the results primarily describe model behavior without clearly answering this question.
12. Section 3.4.3 is titled “large-scale circulation,” yet large-scale circulation is not discussed in the current version of this section. In addition, the conclusions discuss generation modeling assumptions for both wind and solar energy, whereas the introduction and the corresponding research question frame this issue almost exclusively in terms of wind (“How much do generation modeling assumptions impact the wind energy?”). This inconsistency should be addressed.

Minor comments

1. Introduce full names before abbreviations for SHOC, P3, RTE+RRTMGP, SPA, and MPAS.
2. Section title 2.2.2: “Souce” or “Source”?
3. Line 176: replace “simulations” with “this type of experiment simulates”.
4. Line 185: provide the full name for MPAS.
5. Line 221: “eia-processor” → “EIA-processor”.
6. Lines 225–230: rewrite in a more formal, non-conversational style.
7. Line 245: clarify what “independent-plant ratio > 15%” means and how it is computed.
8. Line 262: clarify what “models” refers to.
9. Line 274: “preprocessing” is duplicated.
10. Line 428: “suggests” is repeated.
11. Figure 7 caption needs revision, as large-scale circulation is one of the drivers influencing wind CF, rather than a separate category from the direct drivers.