Response to Reviewer Comments

We appreciate the reviewers' constructive comments that are helpful for improving the quality and presentation of the manuscript. We have considered all review comments very carefully and made improvements to the text in the revised paper.

In the following text, the black color denotes the original comments from reviewers, and the blue color represents the responses from authors.

<u>Response to RC1</u>

This manuscript presents the China New-type Power Systems Meteorological (CNPS-Met) dataset, a novel 25 km daily gridded product for the Chinese mainland covering 1980-2016. The authors validate their dataset against three existing gridded products (CN05.1, CMFD, CDMet), demonstrating superior performance for most meteorological variables. They further analyze the spatial patterns of these power-system-relevant events. Overall, the topic is highly relevant for ESSD, the methodology is sound and addresses a clear limitation of traditional optimal interpolation (OI) methods, and the dataset fills a critical gap at the intersection of meteorology and energy system. The manuscript is well-structured and the results are presented clearly. I would like to recommend an acceptance after a major revision.

Re: Thank you very much for your constructive comments on this study. We have revised the manuscript carefully according to your suggestions. Below are our point-by-point responses.

Major Comments

1. Methodological Justification (Lines 216-236): The spatially adaptive optimal interpolation (OI) scheme proposed in this study is sound, which dynamically adjusts the influence radius based on local station density. While the concept is clear, the paper would benefit from a more explicit demonstration of its impact. For example, how does the resulting field vary spatially across China? This would visually confirm that the algorithm is effectively expanding the radius in data-sparse regions (e.g., Tibetan Plateau) and contracting it in data-dense regions (e.g., Eastern China).

Re: Thanks, we agree with your opinion. In the revised manuscript, we have added a new figure (Figure 2, Figure R1 below) to show the spatial distribution of the influence radius in the spatially adaptive OI scheme across China. Results indicate that, the influence radius varies with the station density, that is, it is larger in data-sparse regions and is smaller in data-dense regions, which generally captures the spatial distribution of stations (Fig. 1a in the manuscript), suggesting that the spatially adaptive OI scheme proposed in this study could dynamically adjust the influence radius based on the density of local observations.

The above discussions have been provided in Lines 240-249 in the revised manuscript.

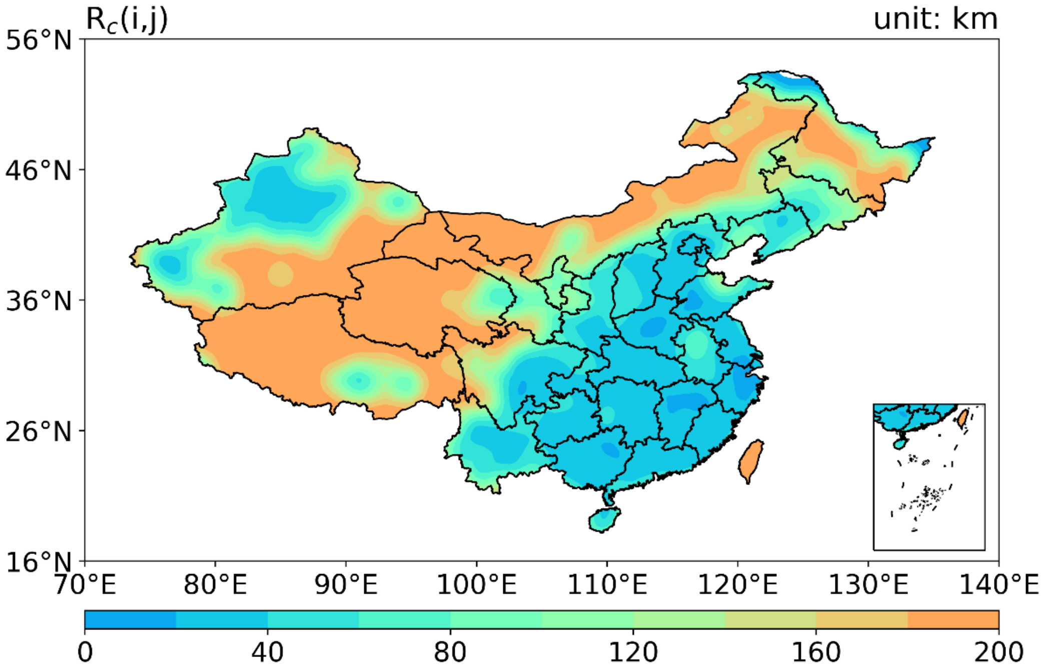

Figure R1. Spatial distribution of the influence radius (unit: km) in the spatially adaptive OI scheme.

2. Clarification on the Minimum Station Threshold (Lines 227-236): It seems that the performance of the new OI scheme is also influenced by the chosen parameters (minimum station threshold). However, the manuscript does not specify the actual value of Nmin, please explicitly state it. Furthermore, what was the rationale for specifying this threshold?

Re: Yes, the performance of the new OI scheme is influenced by the parameter N_{min}. This parameter is set to $N_{min}= 5$, meaning that for each grid point, the scheme dynamically expands the search radius until the number of available observation stations within the search region reaches at least 5.

The rationale for setting $N_{min}= 5$ is as follows. First, when $N_{min}< 5$, in extremely data-sparse regions (e.g., Northwest China), the search radius remains too small, which may cause assimilation results based on only a few stations (e.g., 1-2 stations) to become not robust due to insufficient representativeness or accidental errors (Figure R2a). Second, when $N_{min}> 5$ (i.e., 10 or 15), this could lead to missing values of the influence radius in the data-sparse regions (Figs. R2c-d).

The above discussions have also been provided in Lines 232-239 in the revised manuscript.

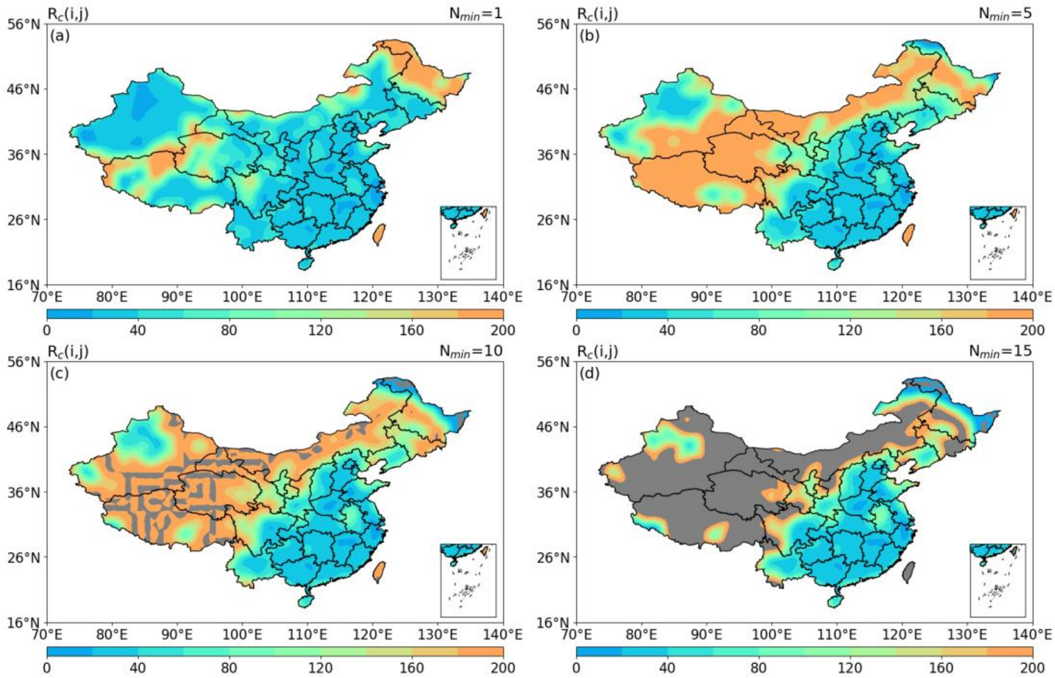

Figure R2. Spatial distribution of the influence radius (unit: km) in the spatially adaptive OI scheme with different minimum station threshold such as (a) N_{min}= 1, (b) N_{min}= 5, (c) N_{min}= 10, and (d) N_{min}= 15. The missing values are denoted as gray shaded color.

3. Verification independence (Lines 163-167): The validation uses CN05.1, CMFD, and CDMet. It is crucial to clarify the degree of independence of these datasets from the observations used to create CNPS-Met. All three validation datasets are themselves interpolated products based on station data. While CNPS-Met demonstrably performs better, the validation is not entirely independent (i.e., it's not a comparison against withheld station data). The authors should validate their results against an independent truth, or explicitly state this limitation and perhaps frame the validation more as a "comparison against existing state-of-the-art gridded products".

Re: In this study, we validate the performance of CNPS-Met dataset against several products generated by different methodologies. This comparison can effectively evaluate the performance of different data generation schemes in the same region, and show the comparative advantages of CNPS-Met dataset.

To address your concern, we select 500 stations as independent validation stations and the remaining 2098 stations as assimilation sites (Figure R3). Verification results of independent truth from January to December 2013 (Figure R4-R5) indicate that the assimilation effects of 2-m specific humidity, precipitation, 2-m air temperature, 10-m wind speed, and surface pressure at 500 independent validation stations are generally consistent and good, with relatively lower *MREs* compared to other datasets.

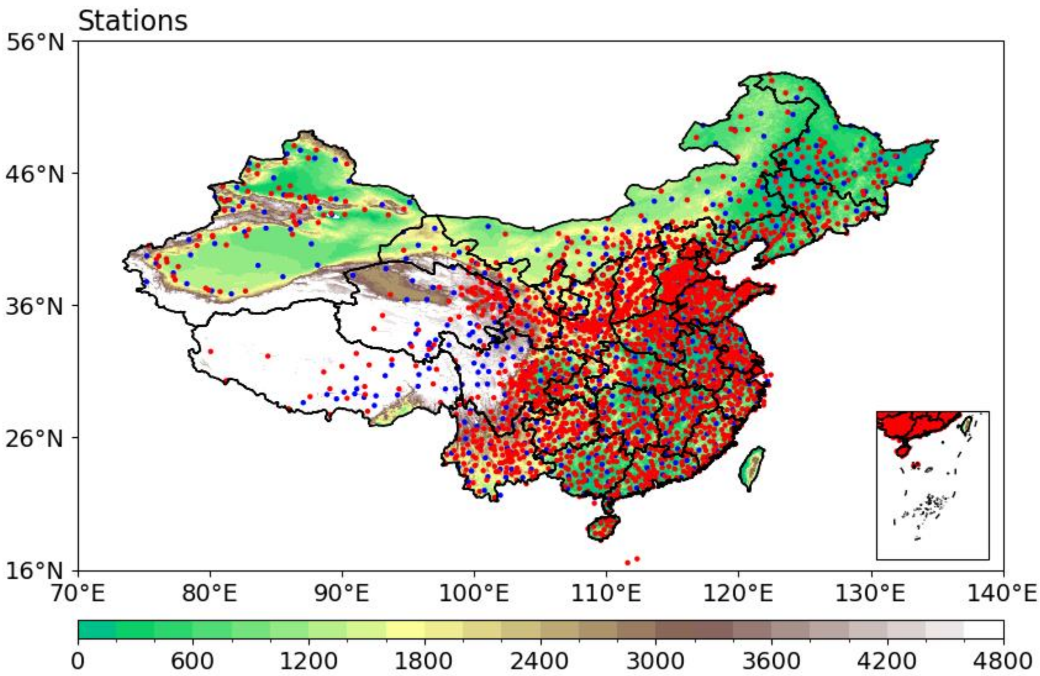

Figure R3. Distribution of ground-based meteorological stations for assimilation (red dots) and verification (independent truth; blue dots).

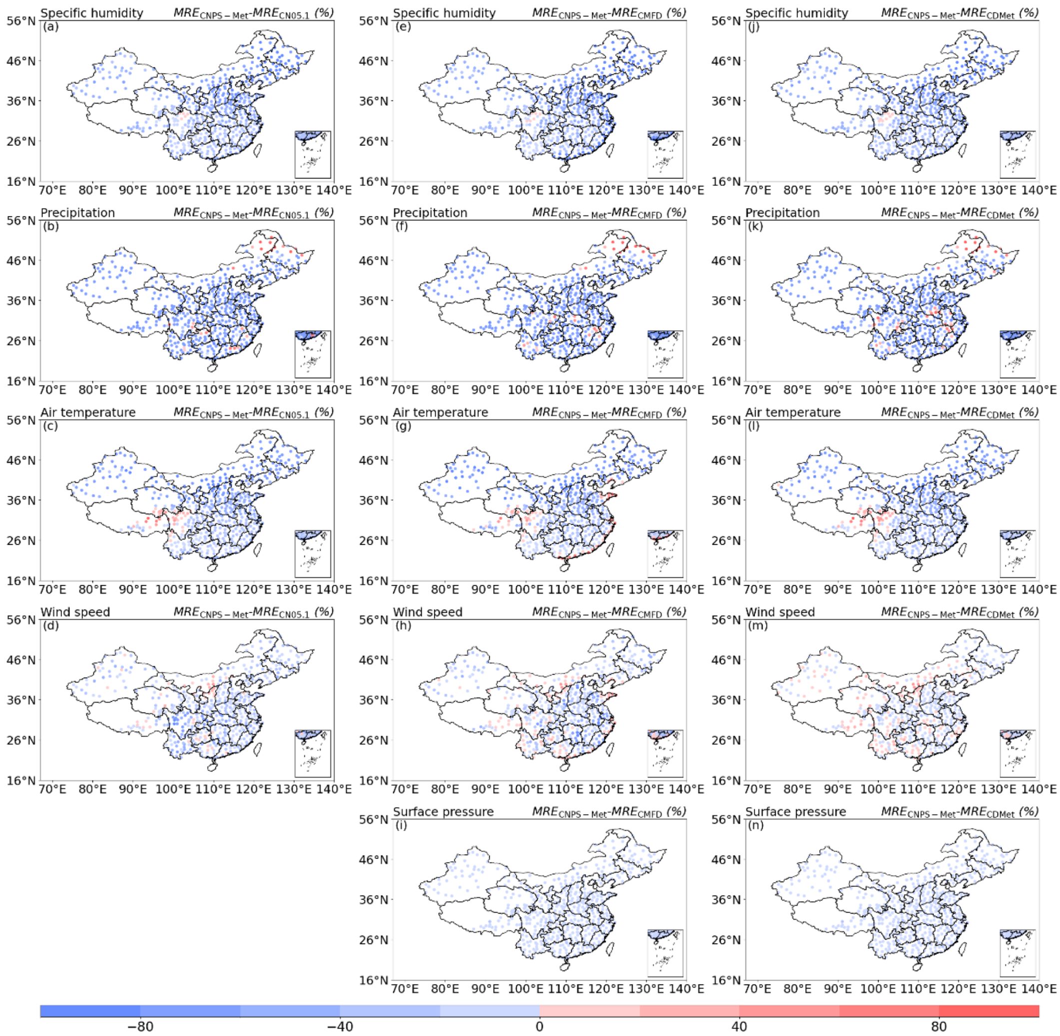

Figure R4. Spatial distribution of the differences in the mean *MREs* at the validation stations (unit: %) between three dataset pairs: (a-d) CNPS-Met and CN05.1 ($MRE_{CNPS\text{-}Met}$ minus $MRE_{CN05.1}$), (e-i) between CNPS-Met and CMFD ($MRE_{CNPS\text{-}Met}$ minus MRE_{CMFD}), and (j-n) between CNPS-Met and CDMet ($MRE_{CNPS\text{-}Met}$ minus MRE_{CDMet}). The differences are shown for (a, e, j) 2-m specific humidity, (b, f, k) precipitation, (c, g, l) 2-m air temperature, (d, h, m) 10-m wind speed, and (i, n) surface pressure.

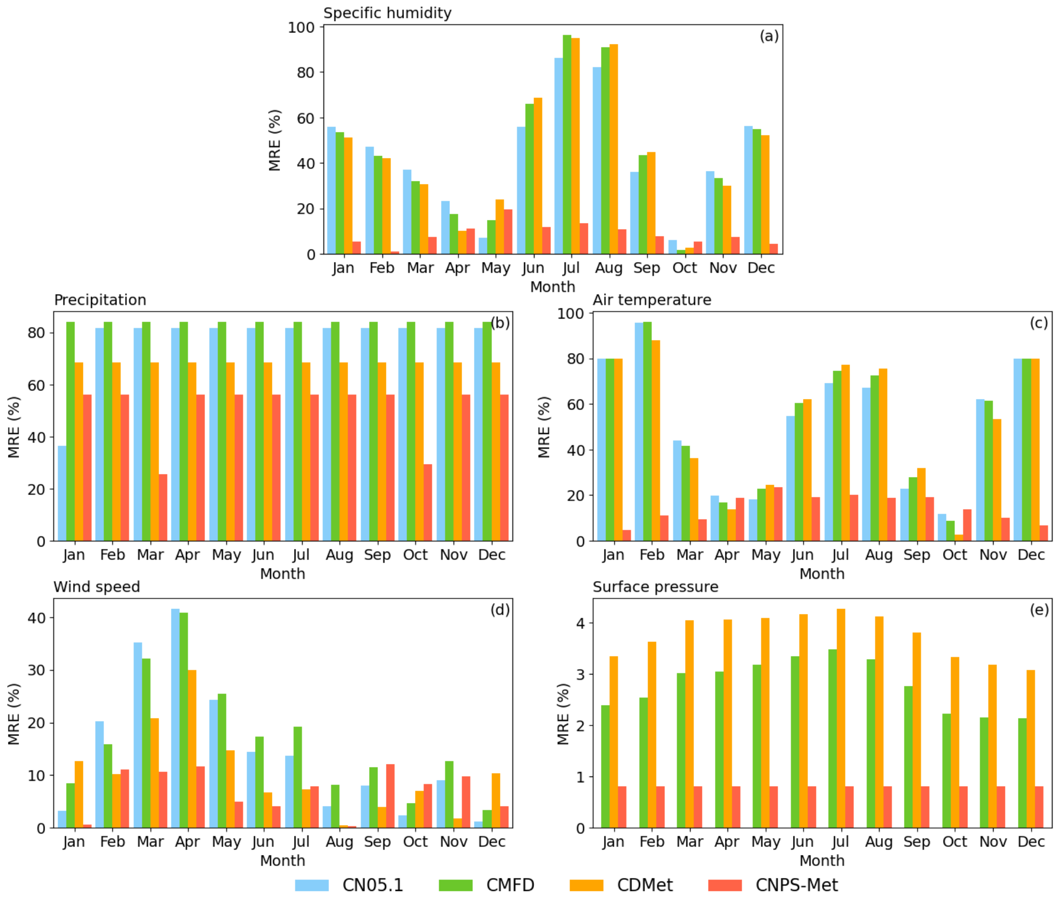

Figure R5. Monthly variation of the mean *MREs* at the validation stations (unit: %) for (a) 2-m specific humidity, (b) precipitation, (c) 2-m air temperature, (d) 10-m wind speed, and (e) surface pressure in different datasets.

4. Spatio-temporal Mismatch in Event Definition (Section 2e): This is a critical point. The high-impact weather events are defined using hourly thresholds, but the CNPS-Met dataset has a daily temporal resolution. How are these hourly events identified from a daily dataset? For example, a day with a mean wind speed of 15 m/s could still have an hourly gust exceeding 25 m/s. The manuscript must clearly explain this. Besides, is the “frequency” reported the number of days where the event occurred, or an estimate of the number of hours? This distinction is fundamental for the utility of the dataset.

Re: Thanks, we understand. In fact, the CNPS-Met dataset is generated by assimilating hourly *in-situ* observations into hourly ERA5 reanalysis, therefore, the minimum temporal resolution of the meteorological variables is 1 hour. On this basis, high-impact weather events are identified according to their respective definitions. After all such events are identified at hourly scale, they are aggregated to the daily scale. In other words, the CNPS-Met dataset supports both hourly and daily temporal scales. The hourly variables, including all meteorological elements and high-impact weather events, are subsequently stored and published online at daily scale.

We need further clarify that, the "frequency" refers to the number of days where the event occurred (i.e. the number of days where the event occurred at least once), rather than an estimate of the number of hours. For example, if a grid point experiences a high-impact weather event for at least one hour on a certain day, then that day is marked as a high-impact weather event day for that grid point.

The above discussions have been provided in Lines 349-361 in the revised manuscript.

5. Uncertainty in the Composite Weather Index (CWI) (Lines 322-330, Eq. 13): The CWI is an interesting metric, but its formulation needs clarification. (1) The term "" is not clear, does this represent the maximum possible value of the variable, or the maximum value observed in the dataset at that location? (2) The notation "" in the piecewise function appears to be a typo. Please clarify the condition(s) for calculating the product.

Re: Thanks, we have revised the Eq. (14) and improved the associated introductions. Please see Lines 366-371 in the revised manuscript.

$$CWI = \begin{cases} \prod_{k=1}^{n} \frac{\alpha_k - th(\alpha_k)}{\max(\alpha_k) - th(\alpha_k)}, & \alpha_1 \geq th(\alpha_1), \mathrm{L}\ , \alpha_n \geq th(\alpha_n) \\ 0 & , else \end{cases} \quad (1)$$

where α represents a high-impact weather event composed of n meteorological variables, with the index of each variable is denoted by subscript k (k= 1, 2, …, n). The threshold and the daily maximum value of the k-th variable (α_k) are denoted as $\mathrm{th}(\alpha_k)$ and $\max(\alpha_k)$, respectively. The $\max(\alpha_k)$ represents the multi-year daily maximum value of the k-th variable in the corresponding different grid point.

6. Interpretation of Wind Speed MREs: In this paper, the MRE is selected to validate the dataset performance. The authors note that the improvement in wind speed is "relatively modest" yet it exhibits the "smallest MREs among all meteorological variables". This apparent paradox needs clarification. Is it because wind speed is inherently harder to improve (due to its intermittency), but also easier to estimate with low relative error because the values themselves are small? The discussion could be expanded to explain why MRE is a suitable metric for wind speed, and whether other

metrics like RMSE might tell a different story.

Re: Thanks, we need to clarify the expression "relatively modern" means that, compared with the improvement of temperature and precipitation etc. in CNPS-Met dataset, the improvement of wind speed is smaller.

We understand that root mean square error (*RMSE*) reflects the root mean square of absolute errors and is sensitive to large errors, while *MRE* reflects the mean relative error and eliminates the influence of dimensions and numerical scales. Since we need to compare variables with different dimensions (e.g., simultaneously evaluating the prediction accuracy of wind speed, temperature, and atmospheric pressure), therefore, *MRE* is selected in this study.

We accept your suggestion and compare root mean square error (*RMSE*) and mean absolute error (*MAE*). Results of *RMSE* and *MAE* are completely consistent with the conclusions of *MRE* (Figure R6-R7). The *RMSE*, *MAE* of wind speed in CNPS-Met is also lower than that of all existing reference products involved in the comparison.

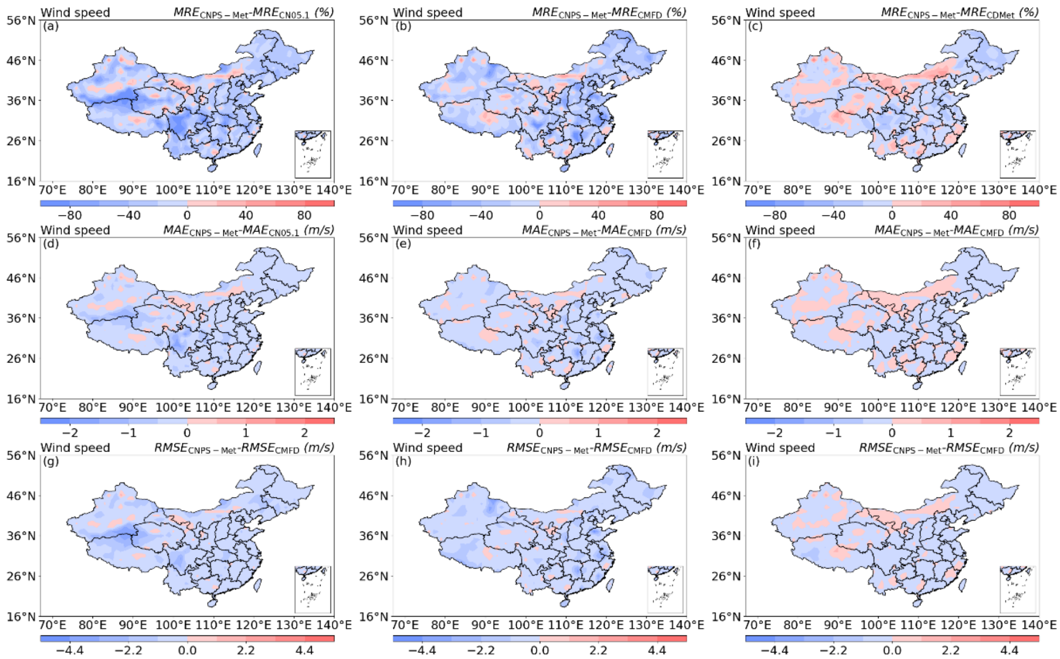

Figure R6. Spatial distribution of the differences in the (a-c) *MREs* (unit: %; averaged over 1980-2016), (d-f) *MAEs* (unit: m/s; averaged over 1980-2016), and (g-i) *RMSEs* (unit: m/s; averaged over 1980-2016) of 10-m wind speed between three dataset pairs: (a, d, g) CNPS-Met and CN05.1, (b, e, h) between CNPS-Met and CMFD, and (c, f, i) between CNPS-Met and CDMet.

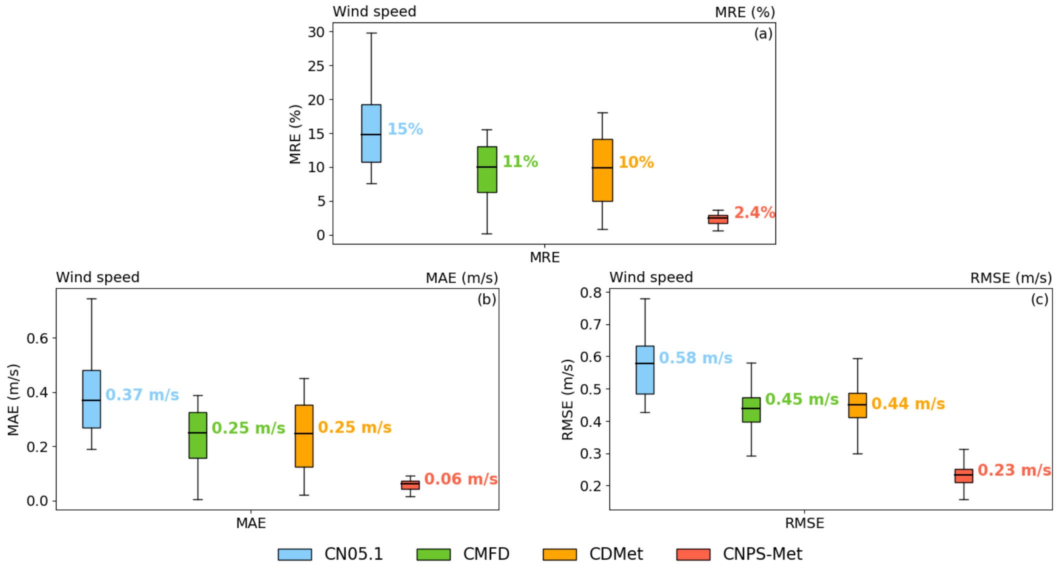

Figure R7. The mean (a) *MREs* (unit: %), (b) *MAEs* (unit: m/s), and (c) *RMSEs* (unit: m/s) averaged over China from 1980 to 2016 in different datasets for 10-m wind speed.

7. Analysis of Intensity Extremes (Figure 7): Figure 7 is visually rich but the criteria for defining an "intensity extreme" at the 90% confidence level is not described in the text or caption. How is this confidence level calculated? Is it based on a statistical test of the intensity values at a grid point compared to the surrounding area, or is it simply the 90th percentile of intensity for that event type? Please add a clear explanation to the text or the figure caption.

Re: Thanks, the "intensity extreme" at the 90% confidence level is obtained through T-test and refers to the 90th percentile of intensity of high-impact weather events. We have added this statement in Lines 472-473 in the revised manuscript.

8. Relevance of Wind Speed Height for Power Systems: The dataset provides wind speed at 10 m. However, for wind power generation, the variable of interest is typically wind speed at turbine hub height (e.g., 70 m). The thresholds for cut-in and cut-out wind speeds defined in Table 2 are based on operational standards that implicitly refer to hub-height wind speeds. Please clarify whether the wind speed provided in the CNPS-Met dataset is intended to represent hub-height conditions. If so, a detailed explanation of the methodology used to extrapolate from 10 m to hub height is essential.

Re: Thanks, wind speed in CNPS-Met dataset is at 10 m above ground level, which can be found in Table 2 and data description section (Lines 603-621) in the revised manuscript.

We agree that cut-in and cut-out wind speeds should correspond to hub height. In this study, when identifying cut-in and cut-out wind speed that are relevant to high-impact weather events, the wind speeds at 10 m are converted to 70 m using the empirical power law method, which can be expressed as:

$$u_2 = u_1 \left(\frac{h_2}{h_1} \right)^{\alpha} \qquad (2)$$

where u_2 and u_1 represent wind speed at 70 m and 10 m, respectively; h_2 and h_1 represent the target height (70 m) and the reference height (10 m), respectively; α is a prescribed constant, taken as 0.14.

We have added the above descriptions to the "Data and methods" section, please see Lines 286-293 in the revision.

9. Clarity on CWI for "Heat and Humid Environment" (Lines 484-489, Fig. 10): The manuscript states that the intensity for this event is based on the CWI, which is dimensionless. However, the definition in Table 2 is simply a threshold. How does the CWI in Eq. 13 apply here? The specific variables (n) and their scaling need to be defined for the HHE event. This is currently unclear.

Re: The Heat and Humid Environment (HHE) is a bivariate composite extreme event, corresponding to the number of composite variables n= 2 in Eq. (14) of the revised manuscript. The variable thresholds listed in Table 2 are the prerequisite conditions for calculating the CWI. Only when both input variables simultaneously meet their respective threshold requirements is a non-zero CWI computed to quantify the strength of the HHE. If either variable fails to meet its threshold requirement, the CWI is set to 0, indicating that no HHE event occurred on that day. In other words, the threshold serves to determine whether an event occurs, while the CWI distinguishes the intensity of events that have occurred.

Minor Comments

1. Line 56: "Chapter 1 in Xin 2023" – It is unusual to cite a book chapter like this. Please provide a proper citation for the book.

Re: We have recited it in Line 56 in the revised manuscript.

2. Line 69: "It has at spatial resolution" – Should be "It has a spatial resolution".

Re: Revised, please see Line 71 in the revised manuscript.

3. Line 79: "699 ground-based observations" – Are these stations, or individual observation points? Please clarify.

Re: This is a typo, we have revised it as "699 ground-based meteorological stations" in the revised manuscript, please see Lines 80-81.

4. Line 121: "In cases of uneven observational coverage, however, the use of a fixed

radius... ” – The sentence beginning here is a comma splice. Consider: “In cases of uneven observational coverage, however, the use of a fixed radius can introduce significant errors...”

Re: Revised, please see Line 123 in the revised manuscript.

5. Line 144: “meteorological stations are densely distributed” – This is redundant with Fig. 1a. Consider removing the text or referencing the figure more directly.

Re: Revised, please see Lines 143-145 in the revised manuscript.

6. Equation (6): The formatting of the piecewise function is not clear for me. Please confirm its expression.

Re: Thanks for your carefully review. The Eq. (6) has been revised as follows:

$$w_{ijk} = \begin{cases} \frac{R_c^2(i,j) - d_{ijk}^2}{R_c^2(i,j) + d_{ijk}^2}, & d_{ijk} \le R_c(i,j) \\ 0, & d_{ijk} > R_c(i,j) \end{cases} \quad (3)$$

where d_{ijk} represents the spatial distance between grid point (i, j) and observation at k^{th} ground-based station; $R_c(i,j)$ represents the influence radius. Please see Lines 214-216 in the revised manuscript.

7. Line 249: “referred as” – Should be “referred to as”.

Re: Revised, please see Line 269 in the revised manuscript.

8. Figure 6 caption: “The concentric circles represent different datasets (from inner to outer: CN05.1, CMFD, CDMet and CNPS- Met.” – The closing parenthesis is missing.

Re: Corrected, please see Line 462 in the revised manuscript.

9. Line 431: “Northern Tibet Plateau” – While commonly used, the standard geographic term is “Tibetan Plateau”, thus, “Northern Tibetan Plateau” is acceptable.

Re: Revised throughout the manuscript.

10. Line 460: “relative weak” – Should be “relatively weak”.

Re: Revised, please see Lines 503-504 in the revised manuscript.

11. Table 2: The table formatting is broken on pages 15 and 16. This is likely a PDF conversion issue, but ensure the final table is clean. Also, the “Impacts” column entries for extreme temperatures and ice accretion are fragmented and difficult to read. Please rewrite them as complete, coherent sentences.

Re: Thanks, we understand. Initially, we tried to place Table 2 in one page, however, this would cause the blank part occurred in the page, which is not allowed by the journal. Therefore, this issue is temporarily unavoidable, and we will further improve it during the later formatting by the editorial department.

Response to RC2

This manuscript presents a valuable contribution to the field of energy meteorology by integrating ground-based observations with ERA5 reanalysis through a spatially adaptive optimal interpolation scheme. The resulting dataset includes both standard meteorological variables and a well-defined set of high-impact weather events tailored to power system operations. The topic is highly relevant to the growing field of energy meteorology, and the dataset has clear potential value for both research and practical applications. Overall, this manuscript is well-structured, and the authors have made a reasonable effort to compare their product against existing datasets. I recommend publication of the manuscript after addressing the following issues.

Re: Thank you very much for your constructive comments on this study. We have revised the manuscript carefully according to your suggestions. Below are our point-by-point responses.

1. Several high-impact weather events are defined using hourly or sub-daily thresholds (e.g., "hourly wind speed $\geq$ 25 m s^{-1}" for cut-out wind speed; "sustained ($\geq$ 3 h) wind speeds $\geq$ 4 m s^{-1}" for galloping). However, the CNPS-Met dataset is provided at daily resolution. It is not clear that how these sub-daily criteria are applied to daily averaged fields. This discrepancy could lead to systematic undercounting or misclassification of events. The authors should clarify the methodology used to derive daily event flags from sub-daily definitions.

Re: Thanks, we understand. In fact, the CNPS-Met dataset is generated by assimilating hourly *in-situ* observations into hourly ERA5 reanalysis, therefore, the minimum temporal resolution of the meteorological variables is 1 hour. On this basis, high-impact weather events are identified according to their respective definitions. After all such events are identified at hourly scale, they are aggregated to the daily scale. In other words, the CNPS-Met dataset supports both hourly and daily temporal scales. The hourly variables, including all meteorological elements and high-impact weather events, are subsequently stored and published online at daily scale.

The above discussions have been provided in Lines 349-357 in the revised manuscript.

2. The spatially adaptive optimal interpolation scheme adjusts the influence radius based on station density, but the manuscript provides no spatially explicit uncertainty

estimates for the analysis fields. Users of the dataset would greatly benefit from information on where the dataset is most and least reliable. Therefore, the authors should consider providing analysis error variance fields or a similar uncertainty metric as part of the dataset.

Re: According to your suggestion, the error variance fields of different meteorological variables in CNPS-Met dataset are provided in Figure R8 below. The error variance can be calculated using Eq. (4) below:

$$\sigma^2 = \frac{1}{N}\sum_{i=1}^{N}\left(e_i - \bar{e}\right)^2 \qquad (4)$$

where σ^2 represents the error variance; N represents the overall sample size, which refers to the period of 1980-2016; e_i represents the *MAE* in i-th year; $\bar{e}$ represents the mean *MAE* during 1980-2016.

Results in Figure R8 show that, the regions with high specific humidity error are mainly distributed in northern Xinjiang, central and western Inner Mongolia, and some regions in North China; the variance of humidity error in most southern regions is less than 0.04 g/kg. The regions with high precipitation error are mainly distributed in the southwest of the Tibetan Plateau, the northeast, and most regions south of the Yangtze River; the variance of precipitation error is generally low, mostly below 0.32 mm, and the overall spatial difference is very significant. The high air temperature error zone is mainly distributed in Northeast China and the central eastern part of Inner Mongolia; the air temperature error variance in most regions of North China and South China is mostly below 0.06 °C, showing an overall distribution characteristic of high in the north and low in the south. The high error regions of wind speed are mainly distributed in northern Xinjiang, central and western Inner Mongolia, and northern Qinghai Tibet Plateau in China; the variance of wind speed error in most regions south of the Yangtze River Basin is generally low, basically below 0.024 m/s; The overall error level of surface pressure is relatively low, with spatial differences smaller than other meteorological elements, and only scattered high error values in some regions of northwest and northeast China.

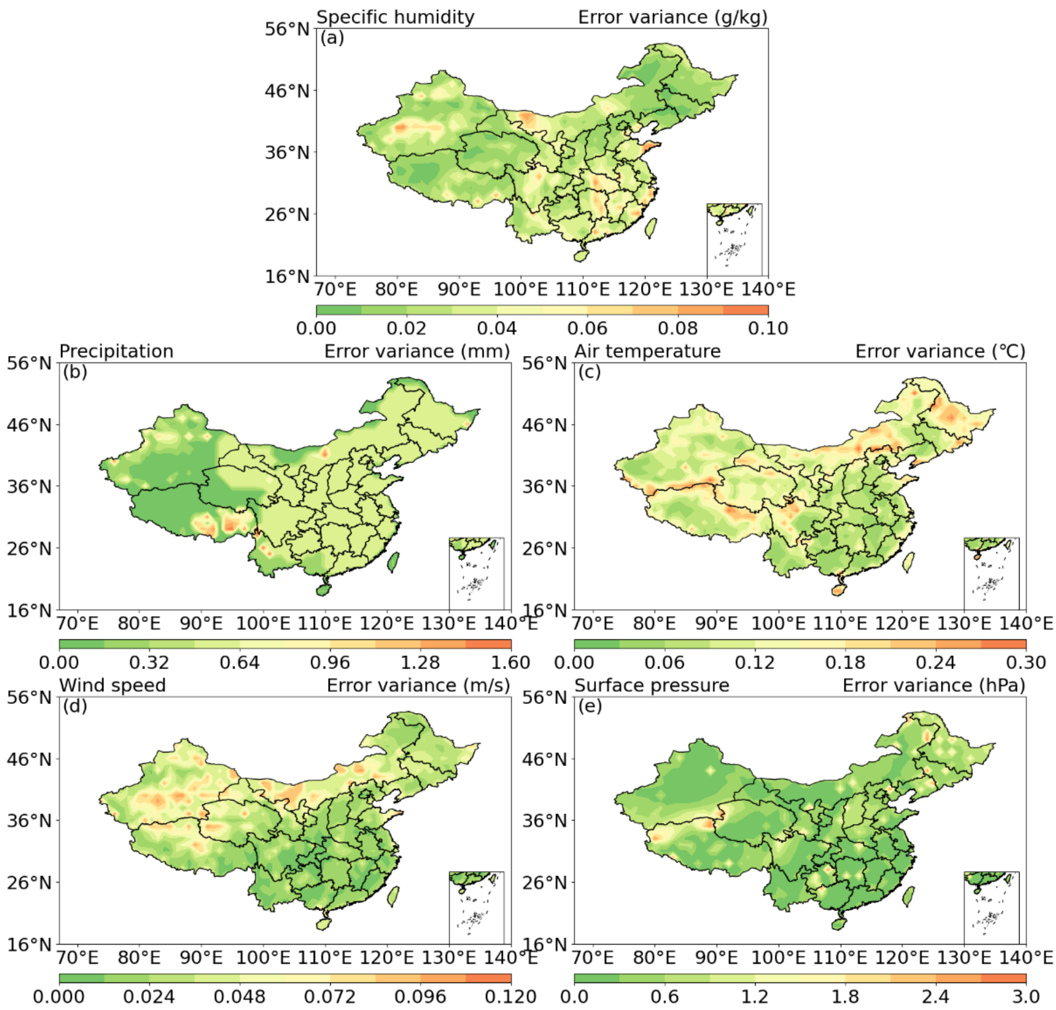

Figure R8. Spatial distribution of the error variance (averaged over 1980-2016) of (a) 2-m specific humidity, (b) precipitation, (c) 2-m air temperature, (d) 10-m wind speed, and (e) surface pressure in the CNPS-Met dataset.

3. The manuscript introduces an improved OI scheme, which is a positive contribution. However, it lacks a direct comparison with a traditional OI baseline using a fixed influence radius. A comparison between the improved OI and traditional OI for a representative period is suggested.

Re: According to your suggestion, the assimilation performance of the new scheme and the traditional scheme is compared over the sample period from January to December 2013 (Figures R9-10). Results show that, compared with the traditional OI scheme (using a fixed influence radius), the new scheme proposed in this study (using a spatially adaptive influence radius) obviously reduces the simulation errors for different regions, different months, and different meteorological variables across China. This indicates that the new scheme proposed in this study outperforms the traditional scheme. The above discussions have been stated in Lines 250-256 in the revision.

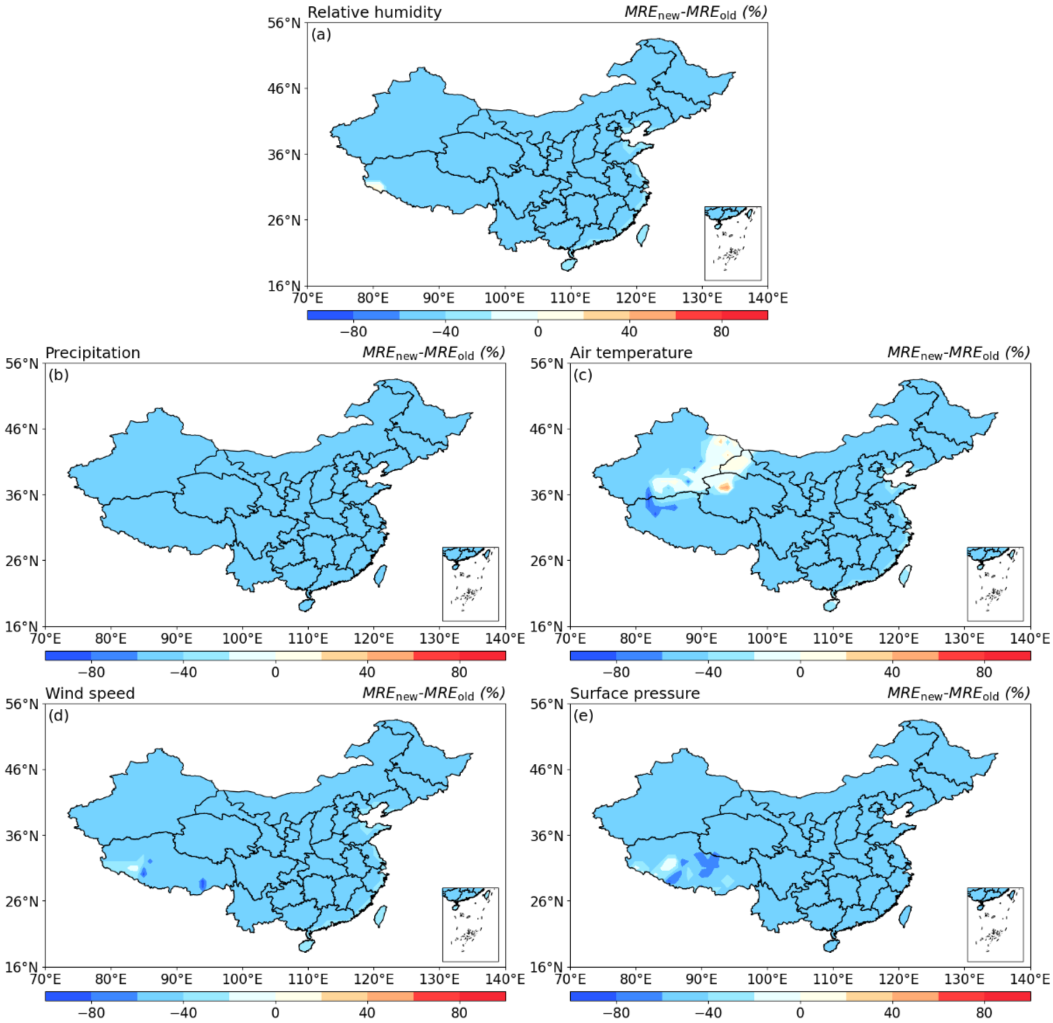

Figure R9. Spatial distribution of the differences of *MREs* between the new scheme and the traditional scheme for (a) 2-m specific humidity, (b) precipitation, (c) 2-m air temperature, (d) 10-m wind speed, and (e) surface pressure in China. The negative (positive) color indicates that the mean *MREs* of the new scheme is smaller (larger) than that of the traditional scheme.

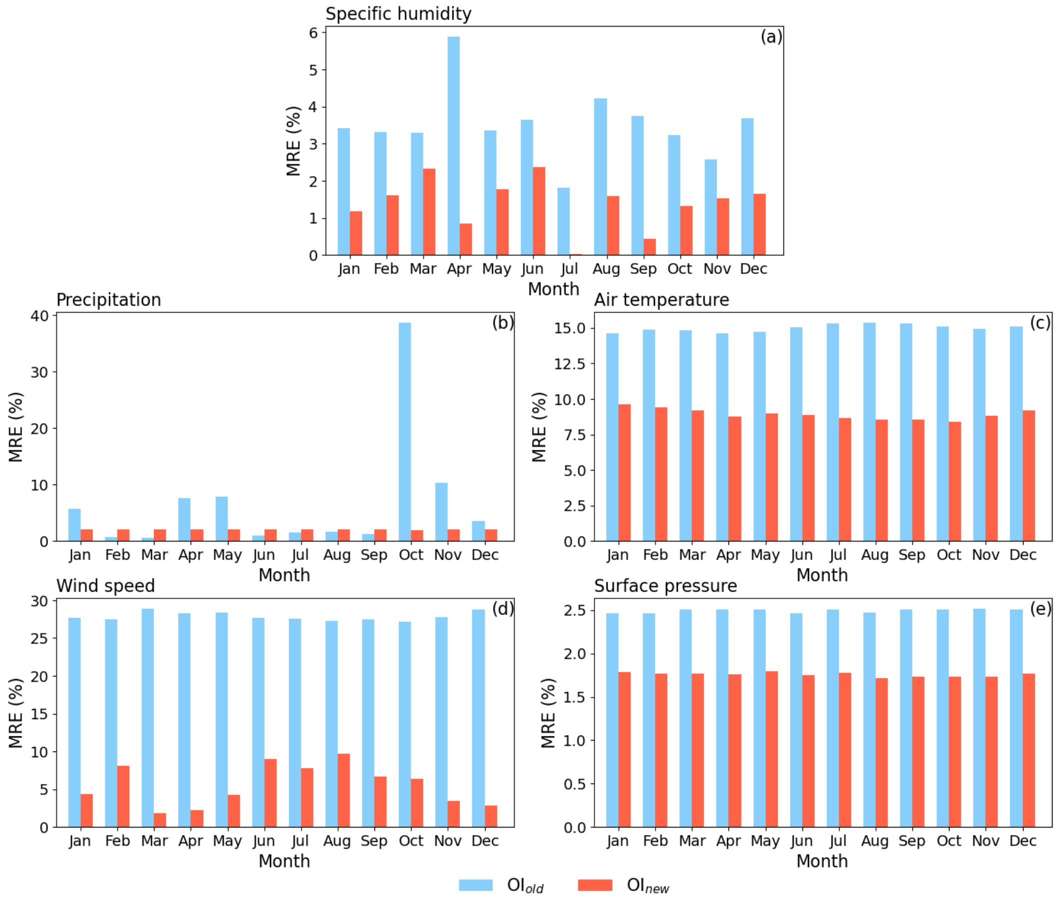

Figure R10. The *MREs* of (a) 2-m specific humidity, (b) precipitation, (c) 2-m air temperature, (d) 10-m wind speed, and (e) surface pressure in different months in China. The blue and red color represent the traditional scheme and the new scheme, respectively.

4. The dataset ends in 2016, nearly a decade ago. Given the rapid expansion of renewable energy infrastructure in China and the increasing frequency of extreme weather events in recent years, a dataset that is regularly updated would be more valuable for long-term applications. It is suggested that the authors consider treating this as a living dataset that can be continuously extended in the future. To reflect its potential for ongoing updates, it may be preferable to remove the time range from the title.

Re: We agree. Our dataset is designed to be a living dataset that can be continuously extended. Accordingly, we have removed the time period from the title to reflect its potential for ongoing updates.

5. Line 49: The number of ground-based stations is stated as “exceed 2400” in the introduction but later given as 2598 in the methods; please reconcile.

Re: This statement is cited from original reference, thus, we follow the original expression from Xu et al. (2019).

6. Line 58: "IPCC AR6" is cited without a complete reference; provide the full citation (e.g., IPCC, 2021).

Re: Revised, please see Line 59 in the revised manuscript.

7. Line 69: "It has at spatial resolution" — should be "It has a spatial resolution".

Re: Revised, please see Line 71 in the revised manuscript.

8. Line 161: "three-sigma rule method" — clarify whether this was applied to daily values or to the full time series; also state how many records were excluded.

Re: The three-sigma rule method was applied to the full time series. Approximately 0.18% records were excluded. These statements have been added in Lines 162-163 in the revised manuscript.

9. Line 237: "statistics for verification" — consider renaming to "Evaluation Metrics" to better reflect the content.

Re: Revised, please see Line 257 in the revised manuscript.

10. Line 249: "referred as" — should be "referred to as".

Re: Revised, please see Line 269 in the revised manuscript.

11. Figure 7: The caption states "intensity extremes (90% confidence level)" but does not explain the statistical method used; add a brief explanation.

Re: Revised, the "intensity extreme (90% confidence level)" is obtained through T-test and refers to the 90^{th} percentile of intensity of high-impact weather events. We have added this statement in Lines 472-473 in the revised manuscript.

12. Line 431: "Northern Tibet Plateau", consider "northern Tibetan Plateau" for consistency.

Re: Revised throughout the manuscript.

13. Line 460: "relative weak" — should be "relatively weak".

Re: Revised, please see Lines 503-504 in the revised manuscript.

14. Line 503: "there is a pronounced 'weather dependency' and 'system vulnerability'" — the use of quotes is inconsistent; consider rephrasing.

Re: Revised, please see Line 546 in the revised manuscript.

Response to CC1

This manuscript introduces the China New-type Power Systems Meteorological (CNPS-Met) dataset. Methodologically, the spatially adaptive optimal interpolation scheme is an enhancement of classical data assimilation techniques. The main contribution is the classification of eleven high-impact weather events explicitly linked to the generation-side, grid-side, and demand-side vulnerabilities, which may be useful for power engineering. I have some comments that I hope the authors will consider.

Re: Thank you very much for your constructive comments on this study. We have revised the manuscript carefully according to your suggestions. Below are our point-by-point responses.

1. The results nearby the complex terrain (e.g., Tibetan Plateau periphery) are not encouraging. Is the error in these regions systematic? A scatter plot of error vs. elevation for these regions would be beneficial to explain the reason.

Re: We understand. To address your concern, the errors nearby and over the Tibetan Plateau (Altitude≥3000 m; Figure R11) are analyzed in Figure R12. Results show that, the error dispersion of air temperature decreases with altitude, whereas that of wind speed increases with altitude. Systematic deviations are found for specific humidity and precipitation at 3400 m~3800 m, as well as for precipitation and surface pressure at 4600 m~5000 m. These could be related to the influence of terrain height and slope, factors that are not accounted for in the OI assimilation scheme.

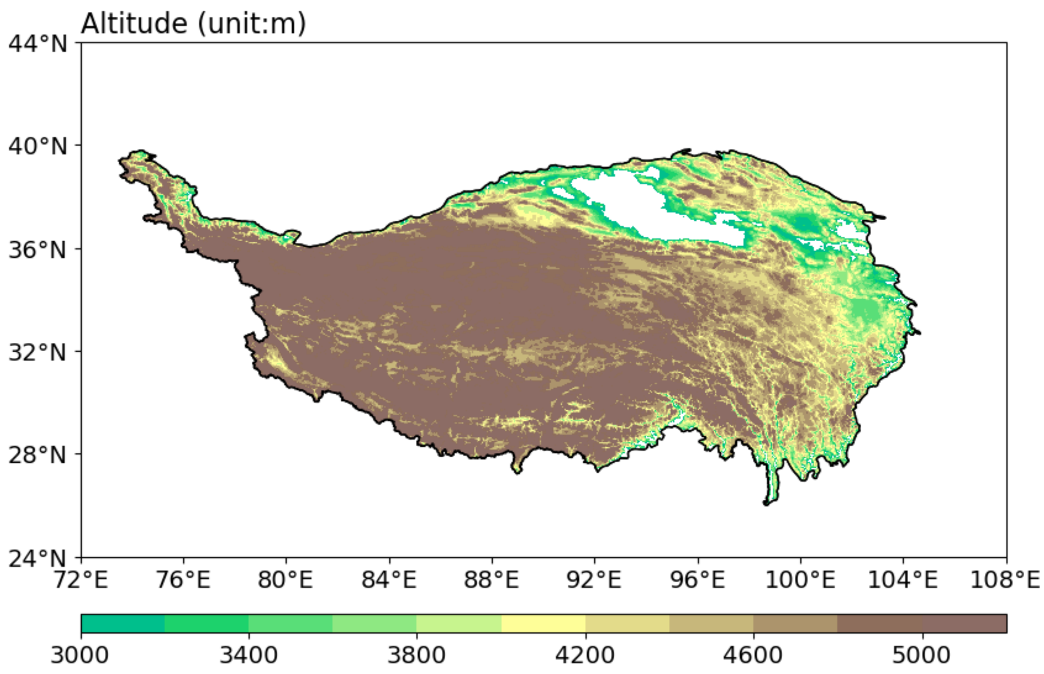

Figure R11. The Tibetan Plateau region with altitude≥3000 m.

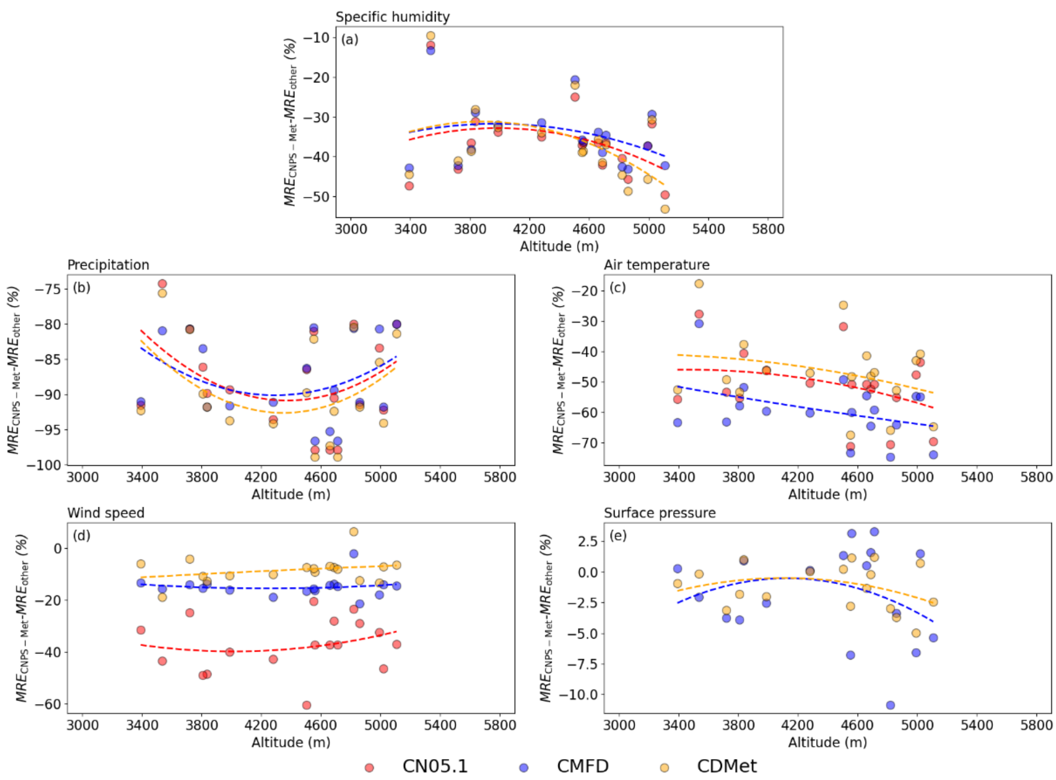

Figure R12. Relationship between the differences in the mean *MRE* (unit: %) and corresponding altitude in complex terrain. (a) 2-m specific humidity, (b) precipitation, (c) 2-m air temperature, (d) 10-m wind speed, and (e) surface pressure.

2. Sub-region Division (Line 332-333, Fig. 1b): The seven sub-regions are divided "according to the spatial distribution and organizational characteristics of the power grid". A brief explanation or citation for this specific division would be helpful for readers unfamiliar with China's power grid structure. Why is it more suitable for this analysis?

Re: According to Zhuo et al. (2022), the rationale for such sub-region division has been illustrated in Lines 373-375 in the revised manuscript.

References

Zhuo Z, Du E, Zhang N, et al. Cost increase in the electricity supply to achieve carbon neutrality in China. *Nature communications*, 2022, 13(1): 3172.

3. Dataset Access and Usability (Lines 565-569): The data availability statement provides a DOI. It would be helpful for the reader if the authors could briefly describe in the response to reviews (or in the final manuscript) the structure of the NetCDF files. For example, are all 19 variables in a single file? Are there separate files for different time periods? A small note on the expected data volume would also be practical for potential users.

Re: Thanks, we agree. To facilitate readers' understanding and use of CNPS-Met, we have added the following description in Lines 602-619 in the revised manuscript:

The file name for CNPS-Met follows the pattern: CNPS_Type_History_Daily_Variable_CCYY.nc, and all times are in Coordinated Universal Time (UTC). In this naming convention: "Type" is an abbreviation for meteorological variables and for the generation side, grid side, and demand side of the new power system, represented respectively by "Meteo", "Generation", "Grid", and "Demand", respectively; "Variable" is an abbreviation for the variable name; "CCYY " represents the year (e.g., 1980,1981, ……).

The meteorological variables include: tas (2-m mean temperature), tmax (2-m maximum temperature), tmin (2-m minimum temperature), precip (accumulated precipitation), wind (10-m mean wind speed), rhum (2-m mean relative humidity), shum (2-m mean specific humidity), pres (mean surface pressure).

The high-impact weather on the generation side includes: Vout (cut-out wind speed), Vin (cut-in wind speed), Lowrad (low radiation), Tmaxg (extreme high temperature), Tming (extreme low temperature).

The high-impact weather on the grid-side includes: Icing (ice accretion), Snowing (snowfall), Galloping (conductor galloping).

The high-impact weather on the demand-side includes Tmaxd (extreme high temperature), Tmind (extreme low temperature), and HHE (heat and humid environment).

4. Temporal Coverage: The dataset ends in 2016, which is already a decade behind. While the authors mention future updates, the current end-date limits the dataset's applicability for recent power system analyses. Is there a specific reason for this cutoff? A more concrete timeline or plan for extending the dataset to the present day would significantly enhance its value and should be included in the "Future work" section.

Re: When we initially constructed the dataset, the available meteorological station data covered the period 1980-2016, that is why the version presented in this manuscript ends in 2016.

However, we would like to emphasize that our dataset is designed to be a living dataset that can be continuously extended. The construction of the extended dataset after 2017 is currently ongoing. To avoid any ambiguity, and following the reviewer's suggestion, we have removed the time period from the title to reflect its potential for ongoing updates.

The above discussions have also been provided in Lines 595-598 in the revised manuscript.

5. Lines 96-97: "the methodology employed in the aforementioned datasets is fundamentally based on spatial interpolation" – This is a bit of an overstatement for

CMFD, which the authors themselves describe as integrating remote sensing and reanalysis. Consider rephrasing to “relies heavily on spatial interpolation techniques”.
Re: We have revised the expressions, please see Lines 98-99 in the revised manuscript.

6. Line 108: "Hunt et al., 2007" – There is an extra comma before "et al." Please correct to "Hunt et al. 2007" for consistency with other citations.
Re: Revised, please see Line 110 in the revised manuscript.

7. Equation (2) and (5): The use of superscript T for both transpose and iteration number (as) is confusing. Please use distinct notation, e.g., for the iteration.
Re: In this study, the transpose of the matrix is denoted by the subscript T, and the number of iterations is denoted by the subscript γ. We have revised throughout the manuscript.

8. Line 247: “The MRE and RMSE closer to 0” – This phrase is grammatically incomplete. Consider: “Values of MRE and RMSE closer to 0, and R2 and EF closer to 1, indicate better estimation performance”.
Re: Revised, please see Lines 267-268 in the revised manuscript.

9. Table 2 (Impacts column): The impacts for extreme high temperature and ice accretion are fragmented and run together. Please rewrite them as complete sentences for clarity.
Re: Thanks, revised.

10. The notation “” appears to be a typo. Please clarify the intended condition(s).
Re: Revised throughout the manuscript.

11. Line 332: “seven sub-regions are divided” – This phrasing is awkward. Consider “seven sub-regions are defined” or “the study area is divided into seven sub-regions”.
Re: Revised, please see Line 373 in the revised manuscript.

12. Line 364: “show the lower variability” – Should be “show lower variability”.
Re: Revised, please see Line 407 in the revised manuscript.

13. Line 434: “Cut- in wind speed are” – Subject-verb agreement error. Should be “Cut-in wind speed is”.
Re: Revised, please see Lines 477-478 in the revised manuscript.

Response to CC2

This manuscript introduces the CNPS-Met dataset, a daily, 25-km gridded product (1980–2016) specifically tailored for China's new-type power systems. By integrating ground observations from over 2,000 stations with the ERA5 reanalysis using a spatially adaptive Optimal Interpolation (OI) scheme, the authors provide essential meteorological variables alongside 11 high-impact weather events categorized by power system vulnerabilities. The dataset addresses a critical gap in energy-meteorology by aligning atmospheric data with operational needs (generation, grid, and demand). To further enhance the manuscript's clarity and formalize the presentation for readers, the following minor refinements are suggested:

Re: Thank you very much for your constructive comments on this study. We have revised the manuscript carefully according to your suggestions. Below are our point-by-point responses.

1. Lines 48–49: Suggest changing "exceed" to "exceeding" for grammatical consistency, and "observations" to "observation stations" for better clarity.

Re: Revised, please see Line 49 in the revised manuscript.

2. Lines 50–51 & 53: Please consider replacing "ground-based observations" with "ground-based observation stations." This change clarifies that the scarcity refers to the physical distribution of the monitoring network rather than the data points themselves.

Re: Revised throughout the manuscript.

3. Line 58: Consider adding "the" before "increasing frequency" for grammatical correctness.

Re: Revised, please see Line 58 in the revised manuscript.

4. Line 61: "high-quality" should be hyphenated when used as a compound modifier before "gridded dataset.

Re: Revised, please see Line 61 in the revised manuscript.

5. Line 62: To enhance readability, you might rephrase "power system-relevant high-impact weather events" to "high-impact weather events relevant to power systems."

Re: Revised, please see Lines 62-63 in the revised manuscript.

6. Lines 69–75: Suggest changing "over China region" to "across the China region" or "covering the Chinese mainland" for a more idiomatic academic expression. Please correct the grammatical errors in these lines. "It has at spatial resolution" should be changed to "It has a spatial resolution," and the comma after "2020" should be replaced with a period to separate the two independent sentences. For consistency and technical

accuracy, please replace "observations" with "observation stations" (e.g., "...approximately 2,400 ground-based observation stations"). This clarifies that the number refers to the monitoring locations, not observation records.

Re: Revised, please see Lines 70-72 in the revised manuscript.

7. Line 78: Please ensure there are spaces between the numbers and units (e.g., "4 km × 4 km") and consider adding a comma after "2020" for better readability.

Re: Revised, please see Line 79 in the revised manuscript.

8. Line 89: Please change "high-impact weather event" to " high-impact weather events" to ensure plural agreement with "datasets."

Re: Revised, please see Line 90 in the revised manuscript.

9. Line 121: "...uniformly distributed, In cases of uneven...". The comma should be replaced with a period to separate the two independent sentences.

Re: Revised, please see Line 123 in the revised manuscript.

10. Line 141: The list of variables contains redundant conjunctions ("and... and..."). Please rephrase to: "...relative humidity at 2 m, surface pressure, and precipitation."

Re: Revised, please see Lines 139-141 in the revised manuscript.

11. Line 143: Please provide a brief mention of the specific quality control methods used (e.g., spatial consistency or range checks), which would greatly benefit the readers.

Re: Before the assimilation, all observations undergo unit conversion and format unification, and outliers are removed using the “3-σ criterion”. Please see Lines 159-163 in the revised manuscript.

12. Line 145: Please ensure consistent formatting for figure citations throughout the manuscript. You have used both "Figure 1a" (Line 139) and "Fig. 1a" (Line 145).

Re: The rule for citing figures in this study is: use a form similar to Figure for the first citation, and use Fig. for other times.

13. Line 155: "...at horizontal resolution of 1° ×1°..." should be "...at a horizontal resolution of 1° ×1°...".

Re: Revised, please see Line 155 in the revised manuscript.