Review result of "Spatiotemporal patterns of temperature inversions and impacts on surface $PM_{2.5}$ across China" (egusphere-2025-4751)

Response to Reviewer #2:
reviewer's comments are given in blue,
our responses are given in deep red.

Some of the content in the manuscript have been revised and updated.

We would like to thank the editor and reviewers for carefully reading the manuscript and providing detailed and constructive comments, which have helped a lot in improving the manuscript. We quote each comment below, followed by our response.

1. Is ~16 m significant?
   Response: We fully agree with your assessment. You are correct that a thickness difference of ~16 m is marginal. We have removed the specific mention of the "~16 m" thickness difference from the Abstract. The revised sentence now focuses on the robust contrasts in frequency and intensity, which are the primary distinguishing features between regions.

   Revised abstract text: "Distinct regional patterns emerge: SBIs dominate in northern China and are significantly stronger 1.3 °C higher than EIs, whereas EIs prevail in eastern China."

2. Line 26, we don't really say that $PM_{2.5}$ is a driver.
   Response: Thank you very much for your suggestion. We agree that the term "driver" could be misinterpreted. Our intention was to emphasize the central role of $PM_{2.5}$ in causing air pollution-related problems. To be more precise, we have changed "a major driver" to "a key indicator and a primary pollutant" in the revised manuscript.

   Revised text: "Fine particulate matter ($PM_{2.5}$, aerodynamic diameter ≤ 2.5 μm) is a dominant component of atmospheric aerosols and a key indicator and a primary pollutant of air pollution in China"

3. Line 39, specific weather systems could also trigger TIs.
   Response: Thank you very much for your suggestion. We agree that specific synoptic-scale weather systems are crucial triggers for temperature inversions. Following your advice, we have revised the manuscript to include the role of anticyclones (high-pressure systems) and frontal systems as key weather systems that foster TI formation. The corresponding text in Line ?? has been modified accordingly.

   Revised text: "TIs arise through multiple mechanisms, including nocturnal radiative cooling, warm-air advection, topographic confinement, as well as under specific synoptic conditions such as stationary high-pressure systems (subsidence warming) and frontal zones."

4. Line 42, a reference is needed for the mentioned wind speed.

   Response: Thank you very much for your suggestion. A relevant reference (Czarnecka et al., 2019) has now been added to support the statement regarding wind.

5. Line 43, warm advections do not necessarily be synoptic-scales.

   Response: Thank you very much for your suggestion. We agree that warm advection can occur across various scales. Our intention was to refer specifically to synoptic-scale warm advection as a driver for elevated inversions. We have also revised the text accordingly, please refer the revised manuscript.

   Revised text: "In contrast, elevated inversions (EIs) are often associated with synoptic-scale phenomena. Key driving mechanisms include synoptic-scale warm advection, subsidence within high-pressure systems, and frontal overrunning."

6. Line 48, does this "ventilation" mean vertical dispersion?

   Response: We thank the reviewer for raising this important point for clarification. In the context of atmospheric pollution, "ventilation" typically refers to the combined effect of both vertical dispersion and horizontal advection (wind-driven transport) that together act to remove and dilute pollutants from a source region. To be more precise and to avoid potential ambiguity, we have revised the sentence to explicitly include both vertical and horizontal processes. We have replaced "pollutant ventilation" with "atmospheric pollutant dispersion and transport".

   Revised text: "The resulting suppression of atmospheric pollutant dispersion and transport operates through three interconnected mechanisms."

7. Please provide the locations of all used air quality monitoring stations in Fig. 1. Please also provide terrain features to show if the 10 km threshold result in any large differences in terrain features.

   Response: Thank you for these valuable suggestions regarding spatial representation, which have significantly improved our manuscript.

   Updated main map: As suggested, we have revised Fig. 1 to include the locations of all used air quality monitoring stations and have overlaid terrain features. This demonstrates that the 10 km pairing threshold generally does not span large terrain gradients.
   Station matching rationale: The 10 km threshold was strategically chosen to balance spatial representativeness and sample size. It was identified via a saturation analysis (see new Supplementary Fig. S1) as the point where increasing the distance yields diminishing returns in viable station pairs, thus ensuring robust national-scale coverage without compromising the physical relevance of the $PM_{2.5}$ data to the sounding profile.
   Full network context: To provide full transparency and contextualize our site selection, we have added a new supplementary map (Supplementary Fig. S2) showing the complete, unfiltered national network of all radiosonde and air quality stations. This map visually affirms that our 10 km filtering resulted in a representative subset of stations with extensive geographical coverage.

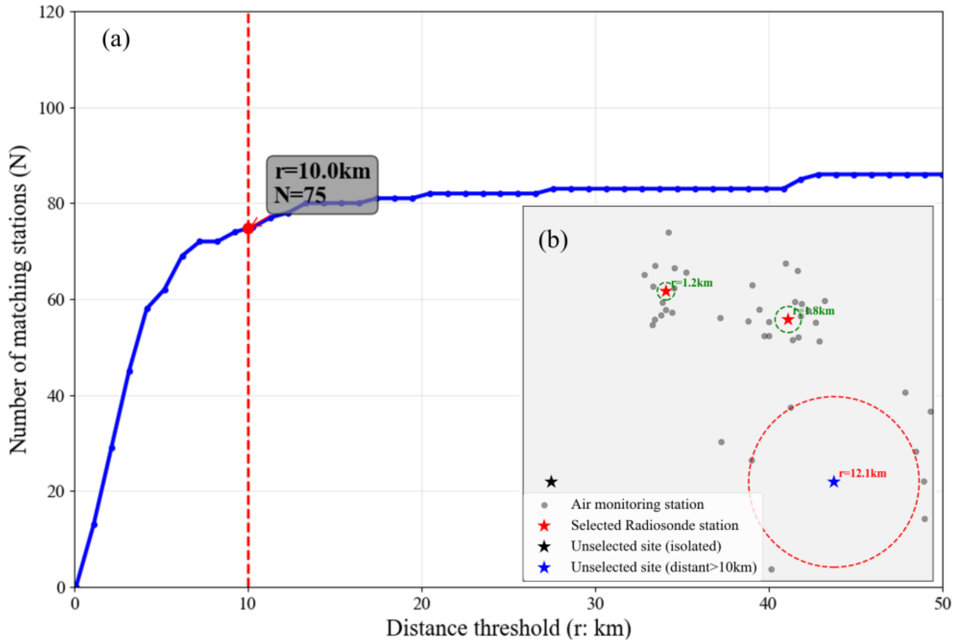

Fig S1. Site filtering method. (a) Relationship between distance and the number of matching stations. (b) Example diagram of site matching process. ‘r’ represents the distance from the nearest air quality monitoring station to the sounding station.

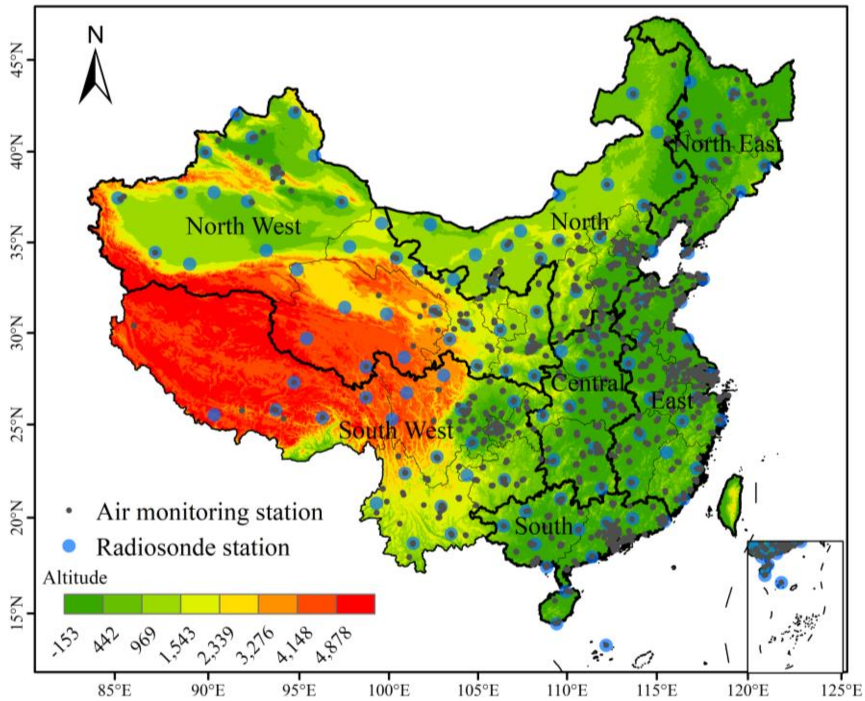

Fig.S2 Distribution of all radiosonde stations and air monitoring stations in China as of 2021. The inset map in the lower right shows the South China Sea Islands.

8. Line 115, please provide the valid bottom height for radiosonde observations.

Response: We confirm that the valid bottom height of the radiosonde observations used in this study is 0 m (Surface Level).According to the operational observation standards of the L-band sounding system in China, the initial surface meteorological data (including temperature, pressure, and humidity measured by the co-located surface weather station) must be input into the sounding system as the initiating record before the balloon release.

Therefore, our vertical profiles consist of: The Surface Layer (H = 0 m): Based on in-situ surface station observations. The Upper Air Layers: Followed continuously by the radar-tracked measurements. This data integration ensures that there is no blind zone at the surface, allowing for the precise identification of Surface-Based Inversions (SBIs) starting strictly from the ground.

Revised: "The GTS1 digital radiosonde provides vertical profiles of temperature, pressure, humidity, wind speed, and direction from the surface (0 m AGL) to ~30 km, with 5–8 m nominal vertical resolution (sampling frequency 1.2 s). By integrating in-situ observations from the co-located surface weather station as the initial record, the valid bottom height for all profiles is strictly maintained at 0 m."

To visually demonstrate this data structure and the validity of the bottom height, we have provided two new items in the Supplementary Material:

Table. S1 A comprehensive list of the 75 radiosonde stations, including their specific station IDs, geolocation (latitude/longitude), and elevation.

Station_Name	Station_ID	Latitude	Longitude	Altitude	Region
Aksu	51628	41.12	80.38	1108	North West
Ankang	57245	32.7166	109.0333	291	North West
Anqing	58424	30.623	116.9672	63	East
Baise	59211	23.9027	106.6063	176	South
Beihai	59644	21.4475	109.1836	14	South
Beijing	54511	39.8061	116.4694	32	North
…	…	…	…	…	…

Note: Please refer to the Supplementary Material for the complete list of 75 stations.

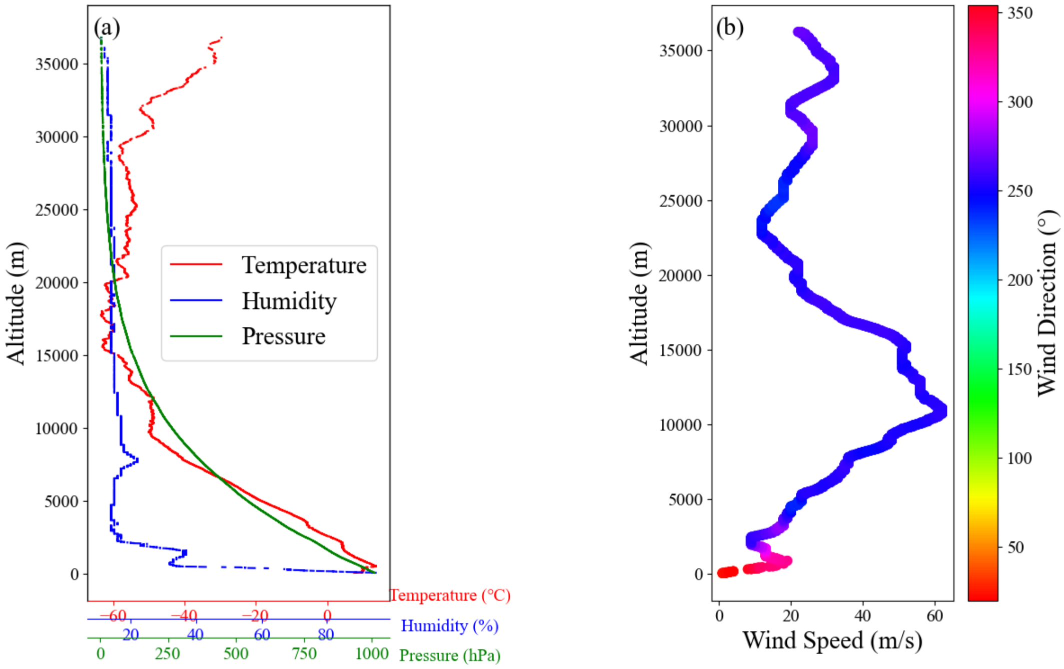

Figure.S3 A representative vertical profile (Temperature, Pressure, Humidity, Wind Speed/Direction) from the Beijing station (Nov 7, 2017, 08:00 BJT).

9. Maybe lapse rate is a better parameter for TI strength.
   Response: We thank the reviewer for this comment. It is substantively the same as Question 22, and therefore, a unified response is provided under Question 22. Please refer to that section for our detailed reply and the corresponding revisions made to the manuscript.

10. Line 162, it should be "a daily mean $PM_{2.5}$ concentration exceeding 75 ug m^{-3}can be identified as a pollution event". However, the authors could use this threshold to identify pollution events, do not necessarily follow the official standards.
    Response: We sincerely thank you for this constructive comment. You are absolutely correct that the GB3095-2012 standard strictly defines a pollution event based on the daily mean PM2.5 concentration, rather than hourly value.

    As you kindly noted, our study aims to capture the specific meteorological conditions (temperature inversions) at the exact moments of the radiosonde launches (08:00 and 20:00 BJT). Using a 24-hour average would smooth out these critical sub-daily variations. Therefore, we followed your suggestion to clarify that while we reference the official threshold value 75 μg m^{-3}, we are applying it to hourly data for the purpose of this specific analysis.

    We have revised the text in Section 3 (Method) to explicitly state this distinction and ensure accuracy: " According to China's National Ambient Air Quality Standards (GB3095-2012), the standard for a pollution event is based on a daily mean $PM_{2.5}$ concentration exceeding 75 μg m^{-3}. In this study, to align with the instantaneous radiosonde observations at 08:00 and 20:00 BJT, we applied this concentration threshold 75 μg m^{-3} to the hourly $PM_{2.5}$ data to identify pollution events corresponding to the sounding times."

11. Line 171, if a TI event's depth is more than 200 m, will it be counted twice?

Response: We thank you for this thoughtful question, which allows us to clarify our methodology. Your concern regarding potential double-counting would be valid if we were binning by the depth or vertical extent of the inversion. However, in this analysis, we are binning strictly by the base height of the inversion layer.

Each inversion event is assigned to one, and only one, 200-m height bin based on where its base is located. Therefore, regardless of how thick an inversion layer is (i.e., whether its depth is 100 m or 500 m), it is counted only once in the bin corresponding to its base height. This approach ensures that no double-counting occurs.

12. Figure 2, it seems this 200 m threshold could not tell more details about the proportion of SBI. Please provide such information in line 172.

Response: We thank the reviewer for this important suggestion. We agree that the original 200-m binning obscured the specific contribution of SBIs (base height < 100 m).

In direct response, we have revised the analysis as follows: We have regenerated Figure 2 using a 100-m vertical resolution. This change directly and clearly isolates the SBI contribution in the 0-100 m bin. This refined 100-m resolution allows for a clear separation between SBI and the EI. The revised text now explicitly states: "The TI proportion peaks sharply in the lowest 0–100 m bin, which comprises exclusively SBIs and accounts for 22.2% of all inversion events." The fine-scale distribution of SBI base heights (10-m bins) and the full vertical extent of EIs up to 2000 m are provided in Figs. S4, respectively.

Revised:

The base height of TI sets the effective lid for vertical pollutant dispersion. To resolve its vertical structure within the boundary layer, we quantified the vertical distribution of inversion events. This is defined as the percentage of the total number of inversion events falling within each 100-m height bin from the surface to 2 km (Fig. 2). A distinct non-linear profile emerges: The TI proportion peaks sharply in the lowest 0–100 m bin, which comprises exclusively SBIs and accounts for 22.2% of all inversion events. This peak is followed by a rapid decline to a minimum of 3.1% at 600–700 m, and then a gradual recovery to 5.1% at 1800–1900 m—i.e., a "rapid decline, then slow recovery" with height.

Marked regional contrasts accompany this vertical pattern. In the near-surface layer (0–100 m, i.e., the SBI), the Northwest exhibits the highest TI proportion 31.6%, followed by Southwest 30.5%. In comparison, Central, East, and South China show lower near-surface SBI proportions, with South China the lowest 9.8%. The pattern reverses aloft: for elevated inversions in the 1000–2000 m layer, South China records the highest proportion 56.0%, whereas northern regions are much less affected. Taken together, intense near-surface trapping by SBIs preferentially occurs over inland northern and southwestern China, while EIs are more common and reach higher altitudes in the southeastern coastal belt. A potential driver of this dipole is the diurnal temperature range (DTR): larger DTRs over inland regions enhance nocturnal radiative cooling, favoring strong SBIs; weaker DTRs in maritime-influenced southeastern areas favor synoptic processes that produce EIs."

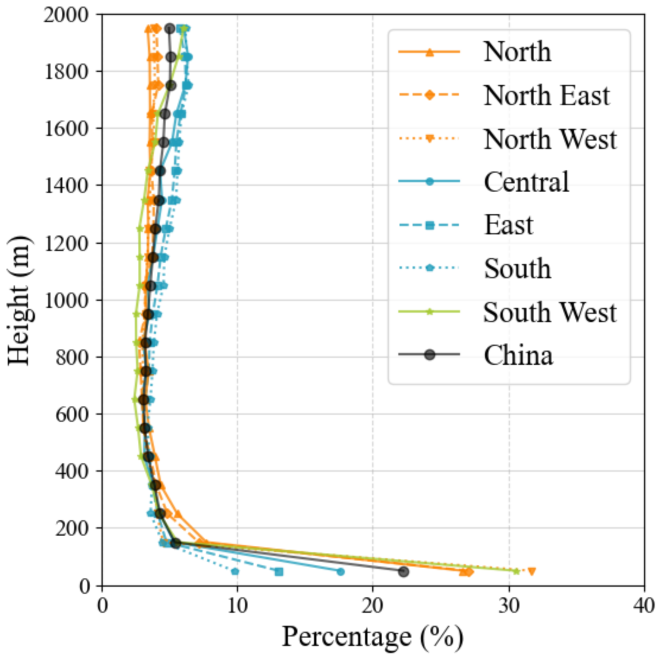

Fig.2 Vertical distribution of inversion base height: fraction of total inversions per height bin.

Supplementary material: To provide even deeper insight and address the spirit of the reviewer's comment, we have also conducted a more granular analysis. We include new supplementary figures:

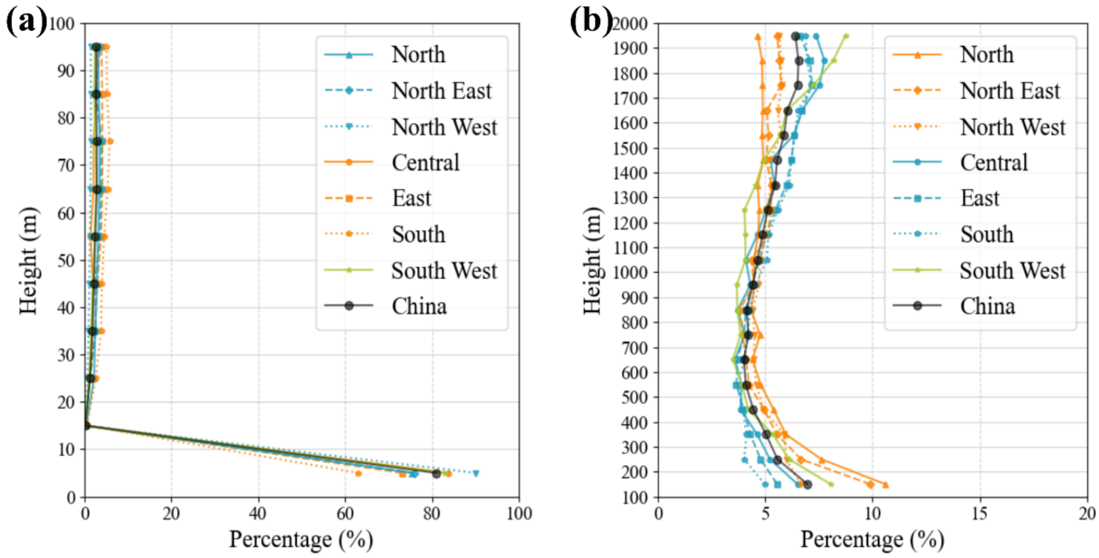

Fig S4 (a) The fine-scale distribution of SBI base heights at 10-m intervals (0-100 m); (b) The full vertical distribution of EI base heights at 100-m intervals (100-2000 m).

13. Line 180, “plausible”?

Response: We thank you for this refinement regarding our word choice. Following your suggestion, we have replaced "plausible" with "potential" to more accurately describe the role of the Diurnal Temperature Range (DTR) as a driving factor. The revised sentence in Section 4.1 now reads:

"A potential driver of this dipole is the diurnal temperature range (DTR): larger DTRs over inland northern and southwestern regions enhance nocturnal radiative cooling."

14. Figure 3, the consistent color bar conceals lots of useful information especially for SBI.

Response: We appreciate this insightful observation regarding the visualization of SBI frequencies. You are correct that the consistent color bar, while covering the full range of TI frequencies, tends to compress the dynamic range of Surface-Based Inversions (SBIs), thereby obscuring some finer spatial details. Our primary intention in Figure 3 was to enable a direct magnitude comparison between SBIs and EIs. Using a unified color scale allows readers to visually grasp the relative dominance of different inversion types across regions (e.g., immediately seeing that EIs are significantly more frequent than SBIs in eastern China). Adopting different scales might mislead readers regarding these relative magnitudes. However, to address your valid concern and reveal the hidden spatial details of SBIs, we have generated a supplementary figure (Fig. S15) with an independent color scale optimized specifically for the SBI range. We have also added a note in the caption of Figure 3 to direct interested readers to this supplementary figure for a more detailed view.

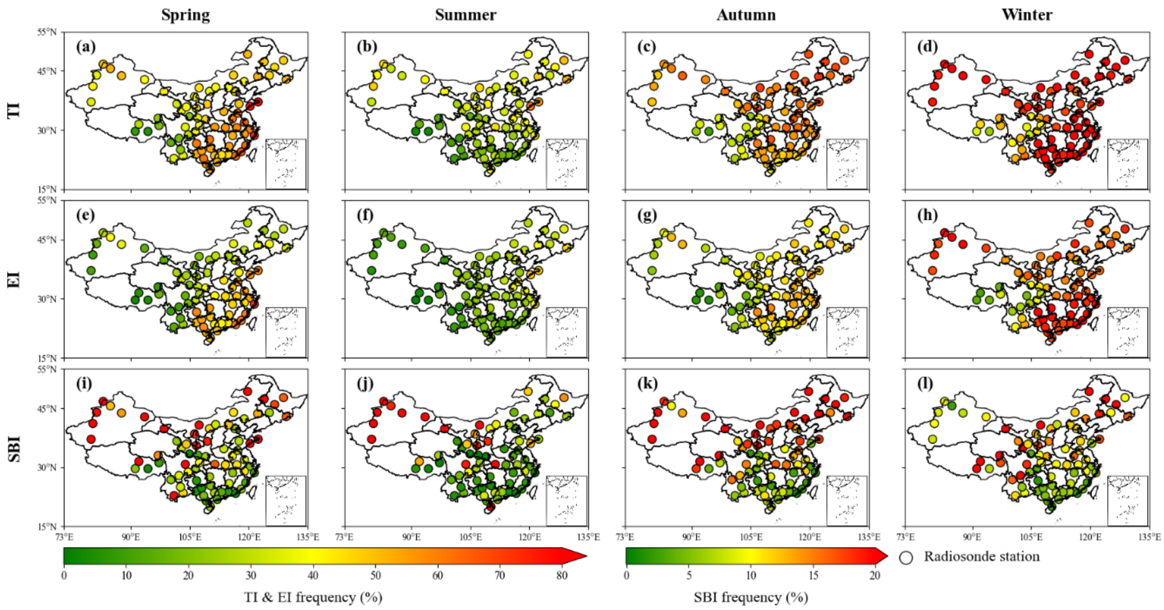

Fig. S15. Spatial and seasonal distribution of occurrence frequency for different types of TI.

15. Line 191, grammar issue.

Response: We thank you for your careful reading. You are correct that the original sentence lacked grammatical parallelism and logical precision, particularly in the clause "whereas the Southwest is lowest." Following your advice, we have revised the sentence to ensure the subject and verb agree logically and to maintain parallel structure with the preceding clause. The revised text in Section 4.1.1 now reads:

"Regionally, Northeast China records the highest frequency (59.5%), whereas the Southwest exhibits the lowest (31.3%; Table 1)."

16. Line 192, please make the first factor clearer.

Response: Thank you for this constructive suggestion. We have revised the text to explicitly describe the altitude discrepancy that leads to the lower observed inversion frequency in the Southwest. Specifically, we clarified that while our study focuses on inversions within the 0–2,000 m range (boundary layer), the dominant inversion structures in the Sichuan Basin are lower-tropospheric inversions (LTIs) that typically occur at higher altitudes of 2,200–3,400 m (Feng et al., 2020). Consequently, these high-altitude inversions are not captured in our statistics. The revised sentence reads:

"The suppressed occurrence in the Southwest likely reflects two factors: (i) in the Sichuan Basin, the dominant signal is lower-tropospheric inversions (LTIs) which typically occur at altitudes of 2,200–3,400 m (Feng et al., 2020)—well above the 0–2,000 m height range examined in this study—resulting in their exclusion from our frequency statistics;"

17. Line 201-202 & 208, shouldn't these sentences be placed in the previous paragraph?

Response: We thank you for identifying this structural inconsistency. You are absolutely correct. The sentences discussing the "east-high, west-low" gradient and the bias caused by the fixed launch time (BJT 08:00 vs. local solar time) pertain to the spatial distribution of inversions. However, they were misplaced in the second paragraph, which focuses on seasonal variability.

Following your suggestion, we have moved the sentences describing the 'east-high, west-low' gradient to the end of the first paragraph of Section 4.1.1. Additionally, to further enhance the logical flow, we have repositioned the sentence 'National averages by type indicate SBIs ~13% and EIs ~43% annually...' to immediately precede the statement 'Elevated inversions (EIs) dominate total events nationally.

This adjustment ensures that all discussions regarding spatial patterns and their underlying causes (including the time zone effect) are consolidated in the first paragraph, while the second paragraph remains dedicated to seasonal and diurnal variations.

18. Line 202, when the term "peak" is used, please do not mention so many months. Please rephrase this sentence.

Response: Thank you for this precise correction. You are absolutely right. Using "peaks" for multiple months dilutes the term. We have rephrased the sentence as follows to more accurately describe the pattern without misusing "peak":

Revised text: "SBI occurrence shows relatively high frequencies (>24%) in February, March, September, and October, whereas EI frequency exhibits a sharp, single peak in January (75.2%)."

19. Line 211, "are more likely to occur".

Response: Thank you very much for your suggestion. We have revised the phrasing to 'are more likely to occur' as you suggested.

20. Line 214-219, the authors tried to explain the seasonal features of TI, but more detailed and in-depth analysis are needed (better with some evidences from meteorological data).

Response: We thank you for this insightful suggestion. You are correct that relying solely on solar forcing is insufficient to explain the seasonal variability. Following your advice, we conducted a detailed statistical analysis of the mean wind speed and relative humidity within the boundary layer (0–1000 m) using the radiosonde profiles. This vertical range was selected as a representative proxy for the lower troposphere, encompassing the primary domain of pollutant dispersion and inversion development while ensuring statistical consistency across seasons (regardless of the fluctuating boundary layer height). The results, presented in the new Fig. S7 (Supplementary material).

The analysis shows that winter is significantly drier than summer across all regions (Fig. S7b). For instance, in Northeast China, the mean relative humidity is ~49% in winter compared to ~71% in summer. This dry atmosphere significantly enhances nocturnal radiative cooling efficiency. Contrary to the assumption that inversions require calm winds, our data shows that boundary-layer wind speeds are actually higher in winter than in summer (Fig. S7a), likely due to the winter monsoon. This analysis suggests that the high frequency of winter inversions is not due to stagnation, but rather because the strong thermodynamic forcing (extreme dryness + long nights) is powerful enough to override the mechanical turbulence caused by higher winter winds.

We have revised Section 4.1.1 to include this mechanism and added Fig. S7 to the Supplementary Material to support these findings.

Revised: "In winter, short days and long nights promote the development of deeper and more persistent inversions through enhanced radiative cooling. With sunrise as late as around 07:20 BJT, the weak early-morning insolation preceding the 08:00 sounding is insufficient to dissipate the TI layer significantly. Furthermore, the wintertime atmosphere is significantly drier (Fig. S7), which minimizes downward longwave radiation and thereby maximizes the efficiency of nocturnal surface cooling. The subsequent ~10 hours of low-angle daylight provide insufficient thermal energy to significantly disturb the stable surface layer before 20:00. Consequently, the 08:00–20:00 frequency difference contracts to 14.8% in winter"

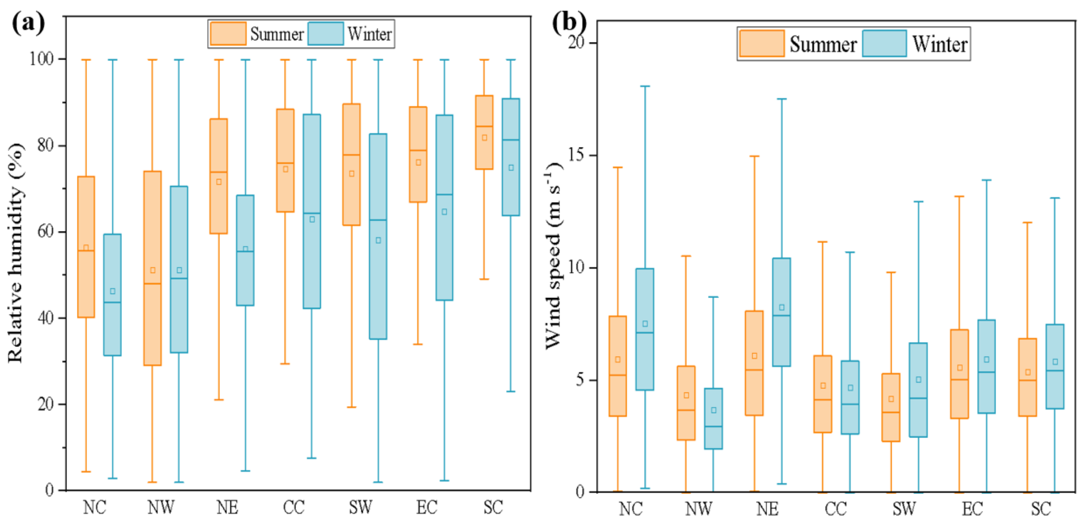

Fig. S7 Seasonal comparison of meteorological conditions within the boundary layer (0–1000 m).

21. Line 232, this sentence is hard to understand.

Response: We thank you for pointing out this unclear phrasing. You are absolutely correct that the original sentence suffered from both grammatical inaccuracy (subject-verb disagreement) and ambiguity regarding what "TIs" specifically referred to. As you inferred, our intended meaning was that inversion strength (specifically, the temperature difference ΔT) is the most critical parameter regulating vertical diffusion.

Following your comment, we have revised the sentence to be precise and grammatically correct: "Among the key TI parameters, strength exerts first-order control on pollutant dispersion."

22. Section 4.1.2, the TI strength defined in this paper is strongly related to the thickness of the TI. Thus, if a TI is very tick but with smaller temperature lapse rate, the strength could still be very strong. Please also see question 9.

Response: We appreciate this valuable suggestion regarding the characterization of inversion intensity. In our study, we adopted inversion strength (ΔT = Tt − Tb) as the primary metric for TI intensity, which is widely used in similar investigations (e.g. Li et al, 2016; Huang et al, 2021; Liu et al., 2022) and facilitates direct comparison with existing literature on inversion–pollution relationships. Our analysis demonstrates that ΔT exhibits a clear and robust association with surface $PM_{2.5}$ concentrations across most regions, supporting its utility as a predictor of pollution accumulation. While we have not incorporated lapse rate into the main analysis, we recognize its potential relevance. To address your point, we have prepared a supplementary figure illustrating the spatial distribution of lapse rate derived from our inversion data. The Fig. S16 shows that lapse rate generally follows patterns similar to ΔT.

You are correct that the lapse rate ($\Delta T/\Delta H$) provides a normalized measure of thermal stability per unit thickness, theoretically indicating the local thermodynamic stability gradient. Following your suggestion, we calculated the lapse rate and examined its correlation with surface $PM_{2.5}$ concentrations to compare its predictive power against TI strength (ΔT). The results led to the following observations: Statistical Evidence: As shown in the revised analysis (Figure. S17 and Table. S4), the empirical data indicates that ΔT exhibits the strongest correlation with $PM_{2.5}$, followed by inversion thickness, while the lapse rate actually shows the weakest correlation.

We attribute this result to the fact that ΔT represents the total "energy barrier" (or heat deficit) that must be overcome by surface heating to erode the inversion. A thin inversion layer might have a very high lapse rate (very stable locally), but due to its small volume and low total heat deficit, it can be easily destroyed by morning turbulence, failing to trap pollutants effectively over long periods. In contrast, a thicker inversion with a large cumulative ΔT (even with a moderate lapse rate) imposes a massive energetic constraint on vertical mixing, serving as a persistent "lid" for pollution accumulation.

Revised text: “Additionally, we discussed the scenario where the lapse rate ($\Delta T/\Delta H$) was used as a proxy for inversion strength, as elaborated in supplementary Fig. S16, S17 and Table. S4.”

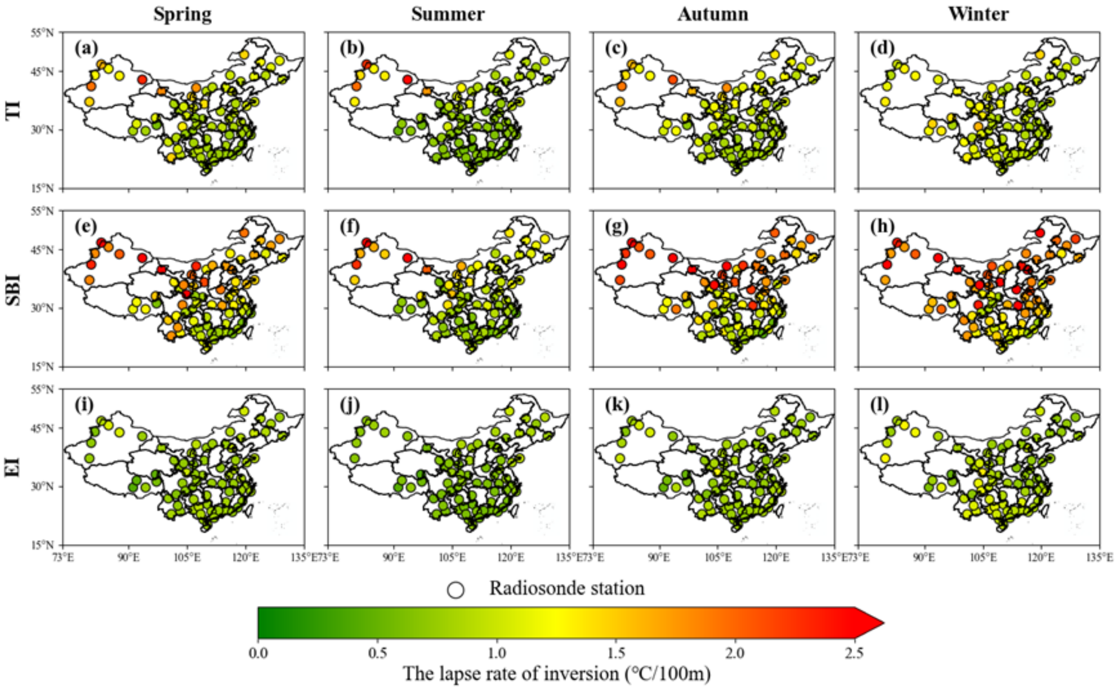

Fig. S16. Spatial and seasonal distribution of lapse rate for different types of TI.

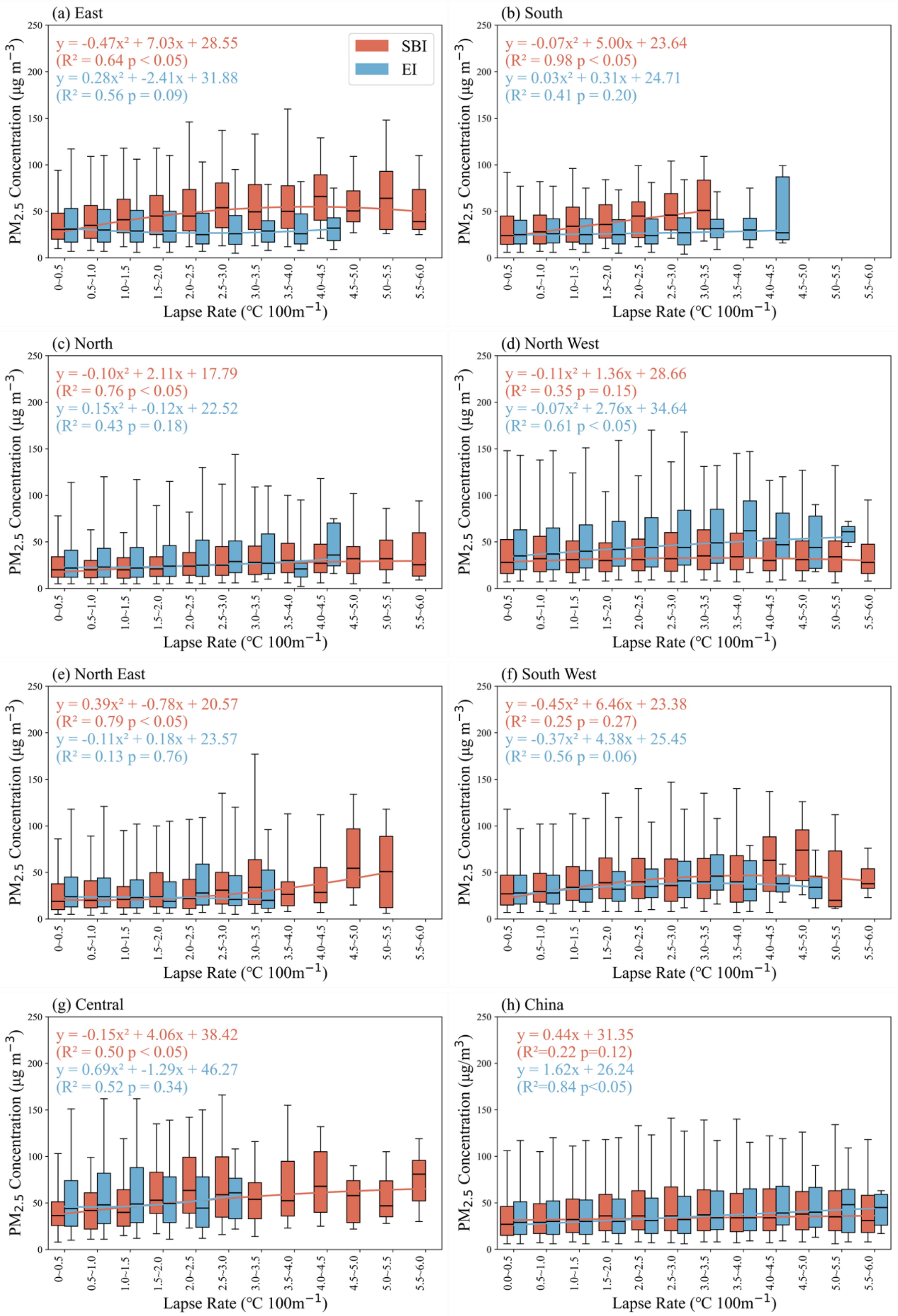

Figure. S17 Fitting relationship between lapse rate and $PM_{2.5}$ concentration across seven regions of China from 2016 to 2021. The ends of the boxes, the ends of the bars, and the short line across each box represent the 25th and 75th percentiles, the 5th and 95th percentiles, and the median, respectively. Each strength interval contains a sample size ≥ 10.

Table. S4 Correlation coefficients between inversion parameters and $PM_{2.5}$ concentrations. '\' indicates insignificance

Region/R^2	Inversion Strength		Inversion Thickness		Lapse Rate	
	SBI	EI	SBI	EI	SBI	EI
East	0.98	0.82	0.63	0.42	0.64	\
South	0.97	0,92	0.66	0.66	0.98	\
North	0.90	0.78	0.53	\	0.76	\
North West	0.47	0.78	0.84	0.92	\	0.61
North East	0.83	\	0.76	0.86	0.79	\
South West	0.94	0.66	0.74	\	\	\
Central	\	\	0.58	\	0.50	\
China	0.72	0.66	0.66	0.28	\	0.84

23. It seems the SBI and EI have very close thickness. However, for EIs, especially for those occurred over higher altitudes, generally have thicker depth, even reach kilometers. For now, because we don't have a full access to the data, we don't know if such thick TI exists.

Response: We thank you for this insightful comment. You are correct that synoptic subsidence inversions in the free troposphere can be very thick (kilometer-scale). However, we would like to clarify our specific selection criteria and how they explain the observed thickness: Selection Criteria ($H_{base} \leq$ 2000 m): To focus strictly on inversions relevant to surface air quality, we only selected inversion events where the inversion base height was less than or equal to 2000 m. Inversions initiating above this level were excluded as they are typically decoupled from the surface boundary layer.

It is important to clarify that we did not impose a ceiling on the inversion top. If an inversion started at 1500 m and extended to 3000 m, our algorithm would correctly identify its thickness as 1500 m. The fact that our derived average thickness for EIs is relatively small (comparable to SBIs) reflects the physical nature of these low-base EIs. They are predominantly nocturnal residual layers, shallow frontal zones, or top-down capping layers, which are inherently thinner than the deep, pure subsidence inversions found in the middle/upper troposphere.

The "thin" EI results are a true representation of the specific subset of inversions (low-tropospheric start) that impact surface pollution, rather than a result of data truncation.

24. Please rephrase line 259.

Response: We thank you for pointing out the awkward phrasing in this sentence.Following your suggestion, we have rewritten this part to improve readability. The revised text in Section 4.1.3 now reads:

"Spatially, TIs are notably thicker in the eastern and southern regions compared to the northern and western inland areas. Specifically, the average thickness in East, South, and Central China is 223 m, whereas it is 209 m in Northeast, North, and Northwest China."

25. The calculation of the thickness of TI when both SBI and EI exist was not clear.

Response: We thank you for pointing out this ambiguity in our methodology. You are correct that the handling of multi-layer inversions needs explicit definition to avoid confusion.

We have clarified in Section 2.2 that we adopt a "layer-based" statistical approach. Specifically, when both an SBI and an EI (or multiple EIs) appear in the same profile, they are identified and recorded as separate inversion events. The statistics for TI thickness presented in this study represent the average thickness of all individual inversion layers identified, not the cumulative thickness per profile.

Revised Text in Section 2.2: " The temperature profile was scanned upward from the surface, and the first continuous layer satisfying $\Delta H \geq 100$ m and $\Delta T \geq 0.5$°C was identified as the inversion for that sounding. To specifically target thermodynamic processes influencing surface air quality, this study considers only inversion layers with a base height $H_b \leq 2000$ m."

26. There is an issue about the statistical analysis of the relationships between TIs and $PM_{2.5}$. For now, there are only two profiles of temperature per day for most of the stations, and the observing time are 08:00 and 20:00 BJT, which are usually accompanied by relatively low planetary boundary layer height. Such bias is more significant over western regions, since 08:00 BJT there means 06:00 or even 05:00 LST. Such low PBLH is usually the result of the lack of solar heating and is thought to be the major reason for trapping air pollutants during early morning and evening. Therefore, the identification of PBLH and the analysis of TIs and $PM_{2.5}$ under different PBLH is very important here.

Response: Thank you for your suggestion. You are absolutely correct that the co-variation of low PBLH and strong inversions makes causal attribution challenging.

To exclude the potential confounding influence of low Planetary Boundary Layer Heights (PBLH), we utilized PBLH data from the Beijing station (July 2017 to January 2018). Beijing was selected as a representative site due to the high frequency of both temperature inversions and pollution episodes. We filtered the dataset to include only observations with a PBLH below 500 m and examined the relationship between $PM_{2.5}$ concentrations and TI strength under these low-boundary-layer conditions. As shown in Fig 1, the results demonstrate that a significant positive correlation persists between them. This confirms that the thermodynamic stability provided by the inversion layer exerts a direct suppression effect on diffusion, independent of the geometric depth of the boundary layer.

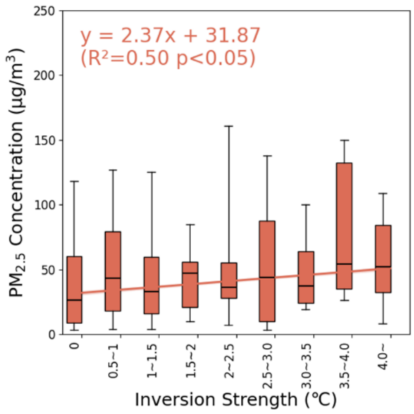

Fig 1. Fitting relationship between TI strength and $PM_{2.5}$ concentrations (PBLH < 500 m).

27. Line 360, does the “intensity” here means strength? Please make these terms consistent.

Response: We sincerely thank the reviewer for pointing out this inconsistency in our terminology. The reviewer is absolutely correct. "Intensity" was used interchangeably with the previously defined parameter "strength" (ΔT), which caused unnecessary confusion. We have now standardized our terminology throughout the entire manuscript by using "strength" exclusively to refer to the parameter ΔT. The specific sentence has been corrected to "Nationally, $PM_{2.5}$ increases near-linearly with strength for both SBI and EI." We have carefully reviewed the text to ensure consistency.

28. Line 363-364, the path of incorporating real-time diagnostic information into nowadays operational models (mostly numerical models) is unclear. It is better to say, improve vertical resolutions and the forecasting skills of temperature inversions in numerical models could benefit the forecast of air pollution events.

Response: We thank the reviewer for this insightful comment and for proposing a more precise and actionable recommendation. We agree that the original phrasing was vague. We have fully adopted the reviewer's suggested direction to enhance the clarity and scientific rigor of our conclusion.

The revised text now reads:

"These findings offer concrete avenues for improving air pollution forecasting. (i) Improving the vertical resolution and the predictive skill of temperature inversions in numerical models could enhance the forecast of air pollution events, particularly in winter."