

## Review of egosphere-2024-520 “The Paris low-level jet during PANAME 2022 and its impact on the summertime urban heat island”

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This manuscript uses Doppler wind lidar measurements from two sites (one urban and one suburban) for the period 15 June – 31 August 2022 to analyse the summertime characteristics of the nocturnal low-level jet over Paris. The characteristics of the jets identified are similar to previous studies of low-level jets over Europe. Using surface wind and temperature observations, the study also investigates the relationship between the jet and the urban heat island. Vertical velocity variance over the city at 238m agl from the lidar is found to be a better predictor of the urban heat island than 10m winds at nearby rural site. There is not necessarily a relationship between jet strength and the urban heat island with some strong jet nights leading to strong urban heat islands. This is undoubtedly an interesting dataset and the continuous nature of the observations allows a study of the jet evolution which is not usually possible with less frequent radiosonde observations. However, I have some questions outlined below which need considering before the manuscript can be published.

The authors thank the referee for the detailed review of our paper and for the important feedback which is key for improving the quality of the manuscript. Modifications on the manuscript are highlighted with red text and here in this document we included the page and number line of the track changes manuscript.

### Major comments

1) I did sometimes struggle to see what the novelty was in the results – the behaviour of the LLJ seems broadly in line with what would be expected based on other studies at similar latitudes / locations. The novelty seems to be around the interaction of the LLJ and urban environment, but the results seem to indicate the LLJ is not strongly affected by the urban land surface. The main result on the UHI seems to be the link with  $\sigma_2$  which is irrespective of the LLJ. It would be good to focus on highlighting the novelty in a revised manuscript and particularly why the focus on the LLJ rather than UHI more generally.

The Conclusions section has been edited to highlight the original contribution of this work more clearly. Additionally, we list the key features below:

- For the first time, continuous Doppler Lidar measurements have allowed the acquisition of high-resolution vertical profiles of wind and turbulence in the Paris City center. This is an environment in which profile observations within the atmospheric boundary layer are urgently needed for the evaluation of high-resolution modeling (NWP, LES), and for gaining an improved understanding of the links between the synoptic background flow and the surface-driven processes for numerous applications, such as urban hydro-meteorological studies or air pollution transport.
- The adaptation and implementation of an algorithm for the automatic detection of the LLJ allow for the characterization of the phenomenon over an entire summer period. This is novel because no information had been available on the LLJ characteristics in

the Paris region thus far, with previous LLJ studies often limited to a few case studies. We find that the LLJ is a frequent nocturnal phenomenon (70% of nights) over Paris, and the results provide a detailed description of its speed and height in relation to flow dynamics (wind direction). While mechanical turbulence can also be produced by other mechanisms, our results reveal that the nocturnal LLJ is an important driver for nocturnal mixing processes in the UBL, with a clear impact on near-surface conditions.

- This study deploys an original approach by linking a regional-scale phenomenon (the LLJ) to the near-surface heat distributions ( $\Delta\text{UHI}$ ), through the Atmospheric Boundary Layer (ABL) dynamics. Usually, the urban canopy layer UHI is studied only by analyzing near-surface variables. Our study highlights that considering the 4D variability of ABL dynamics provides valuable insights into the drivers for mixing and advection processes that impact the heat distribution near the surface.
- As Section 3.7 discusses, cloudiness and surface wind speed both exert control of the UHI intensity. Selecting only cloud-free days from the study period, we isolated the surface wind speed control (Fig. 9a). However, another key contribution of this work is presented in Fig. 9b. We found that vertical mixing presents a better relationship than surface wind speed for the UHI intensity, allowing for a more accurate prediction of the UHI.
- While the vertical mixing in the atmosphere can be induced by other synoptic conditions different from a LLJ, Fig. 9b shows that moderate and strong vertical mixing is more likely to be present for nights with a LLJ event.

2) Much of the analysis focuses on the different classes of  $\sigma_w$  2 however these are somewhat arbitrarily determined based on previous research. Can you justify the choice of thresholds? Does this depend on the location / the height at which the measurements are made? Is there a more objective way of determining what they should be?

As discussed in Section 3.2, in many descriptive studies about the LLJ, the authors present a profiles-oriented classification based on the  $WS_{\text{LLJ}}$ . In most cases, the wind speed intervals for such classification seem to be selected arbitrarily and vary between studies. Here, we propose a jet event-oriented classification based on the vertical mixing associated with each event. This system is proposed because our objective is to determine the relation between a LLJ event and the UHI which develops over a nocturnal period.

Given we are presenting a three-month data collection constituted by 30-minute average profiles, the data set is of sufficient size to apply a statistical approach and the vertical velocity variance thresholds are derived from the full dataset statistics. Given weak variance conditions were rather frequent in summer 2022 (50% of the nights with average  $\sigma_w$  2 < 0.1), we find that these estimates do well represent the range of impacts of the LLJ on near-surface air temperature distributions. Interestingly, when applied to a different dataset (Paris summer 2023, not shown in this paper), the same thresholds still provide a meaningful characterisation in relation to the  $\Delta\text{UHI}$  although the distribution of LLJ characteristics is different in terms of jet strength and altitudes. While the  $\sigma_w$  2 classification proposed in this study based on the frequency distribution appears sufficiently meaningful, we are

investigating the links between variance indicators and other measures of atmospheric stability (temperature inversion intensity, bulk Richardson number) in future studies to see whether a more process-based classification can be determined.

### Minor comments

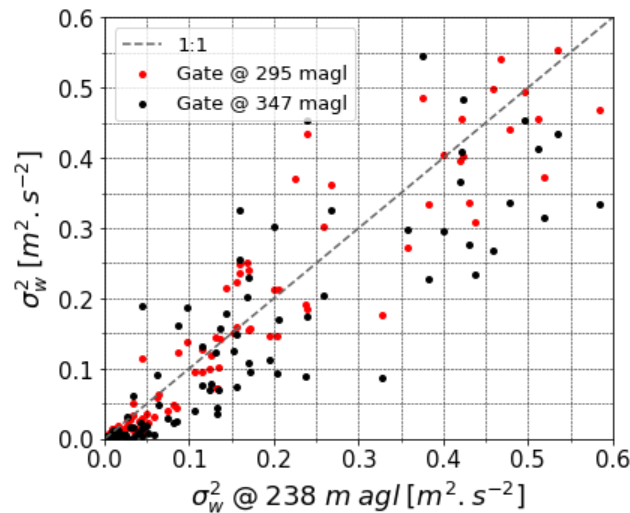
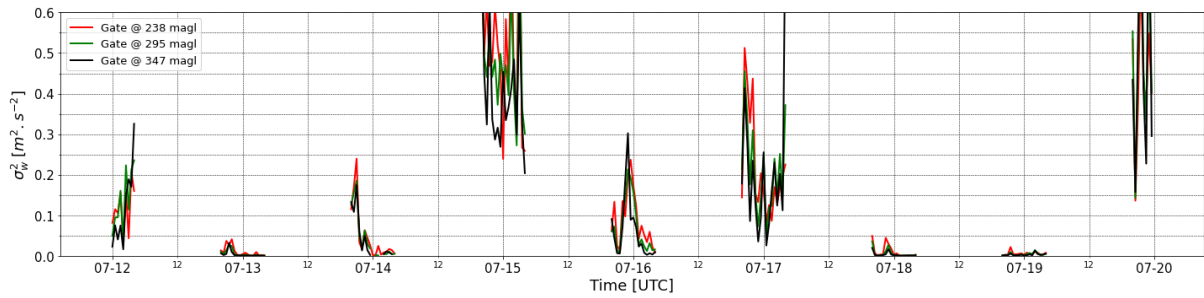
1) p9, line 219. I do not understand what the difference in core time  $< 15$  h means. Just above you stated that a LLJ event must have a LLJ detected in at least 4 consecutive profiles. Does this not mean the time between consecutive LLJ detections has to be 30 mins anyway? Please clarify.

A LLJ event is here defined to have a duration of at least 2h, defined by the time difference between start and end. However, in the 2h time window, we allow a maximum of 1 profile without LLJ detection. Hence the minimum jet event can consist of 3 profiles with LLJ detection over a period of 2h. In this case, there is one instance when the time difference is 1h between LLJ profiles. The sentence has been edited in the manuscript for clarity:

p9, line 231: "LLJ event detection: a LLJ event is considered as a coherent detection if lasts at least 2 hours (i.e. four 30-min averaged profiles). However, an event is valid if at least three profiles are detected over the course of this period with the following criteria for consecutive detections"

2) p10, line 270. You have chosen to use the first range gate (238m agl) to calculate  $\sigma_w^2$ . I have sometimes seen issues with data at the first range gate being a bit lower quality / noisier than further up. Did you see any issues with the data at the first range gate which might affect the results? Or to put it another way, are the results very dependent on whether you use the first gate, or say the 2nd / 3rd gate? In a sense this is a slightly arbitrary choice of height at which to calculate  $\sigma_w^2$ , so it would be good to know the results are not too sensitive to this choice.

We agree that the height at which we assess the vertical velocity variance is somewhat arbitrary as it is defined by the instrument characteristics and measurement setup. However, we have not noticed data quality issues on the first range gate. For comparison, we plot  $\sigma_w^2$  derived at the first, second and third range gates (Figure below). These figures show that our conclusions are not especially sensitive to the choice of the range gate, as it illustrates that the height appears rather representative of the vertical mixing conditions in the nocturnal urban boundary layer. Of course, we acknowledge that representativity is especially challenged for LLJ at core altitudes  $< 250$  m agl. We are working on novel scanning strategies and measurement setups to reduce this uncertainty in future studies. A comment was added on the Section 3.2..



3) p12, line 291. Here, and throughout the manuscript, you give very precise values for the heights of measurements (in this case 238m). In reality, given the range gates of the lidar are 75m, it is hard to justify this level of accuracy.

The length of the range gate refers to the size of the probing volume. The position of the measurement corresponds to the center of this volume, and can be accurately determined by the wind lidar with a sub-meter accuracy. Please note that the display resolution of the analysis is 25 m which is derived from the physical range gate resolution of 75 m. The core height statistics are discussed relative to the height above ground level. Then, the accuracy of the numbers makes sense considering that the height above ground level of the first range is 238 m. Also, some statistics like mean and median are provided as integers.

4) p13, Figure 4. The figure mentions classes 1, 2 and 3. These are not actually defined in the text. I assume these correspond to the low, medium and high vertical mixing classes?

Yes, those classes correspond to the low, medium, and high vertical mixing classes. The caption figure has been edited for clarification with the following sentence:

p13: "In the legend class 1 (red), 2 (yellow) and 3 (blue) correspond to low, medium and strong vertical mixing classes, respectively."

5) p13, line 323. I wonder why you don't also give a value of the wind speeds reported in the Karipot et al (2009) study? It's hard to compare meaningfully with the current results otherwise.

The wind speed values from Karipot et al (2009) have been included for clarity (see p14, line 343).

6) p14, line 344. "influence" -> "influences"

This has been modified in the manuscript.

7) p14, lines 348-350. I wonder if this sentence needs some qualification? I agree  $\sigma_w^2$  is related to the surface layer turbulent heat fluxes in a convective boundary layer. I'm not sure this is necessarily the case in a stable boundary layer. It is also certainly true that shear generated turbulence can lead to vertical velocity variance and so surface layer turbulent heat fluxes are not solely responsible for  $\sigma_w^2$ .

Thank you for pointing this out. In the cited studies, the decay and increase of  $\sigma_w^2$  are linked to the diurnal cycle of the turbulent sensible heat flux that describes the surface-driven buoyancy in the CBL. In fact, Barlow et al 2015 and Banta et al. 2006 reveal with an analysis of the vertical velocity skewness that the LLJ is indeed the source of turbulence in such conditions, forming an "upside-down" boundary layer structure. For clarity, the sentence has been edited as follow:

p15, line 371: "In general, the shape of the three median profiles agrees with those reported in previous studies (Banta et al., 2006; Bonin et al., 2015). In cities, unstable or neutral stratification and higher  $\sigma_w^2$  are maintained by the added urban heat (Theeuwes et al., 2019)."

8) p16, line 379. "the IO mechanism is highly relevant .. in the Paris region". This does seem to be the case for the example you look at, but since you have deliberately chosen an example without any other synoptic forcing this is perhaps not surprising?

This sentence requires quantification to justify the relevance of the IO mechanism in the Paris region. 28 out of 49 of the studied jet events display a clear IO signature in the hodograph, indicating the IO mechanism was highly relevant for the LLJ formation in summer 2022. The sentence has been edited for clarification (see p17, line 402).

9) p16, line 380. " $T = 2\pi f$ " should be " $T = 2\pi / f$ "

This has been modified in the manuscript.

10) p16, line 381. " $\theta=48.8466$ ". Needs units (degrees). It also seems to be an unjustifiably precise number, particularly when you then give the period T as ~16 hours.

Thanks for pointing this out. The period remains as T ~16h while the latitude has been provided with two digits " $\theta=48.84$ ".

11) p16, line 384-385. "wind speed amplitude". I find this a confusing phrase. You really mean the amplitude of the variations / oscillations in wind speed. Wind speed amplitude

could just be the wind speed itself. I would suggest rephrasing to make sure your meaning is clear.

The term “wind speed amplitude” has been replaced by “wind speed oscillation” throughout the entire manuscript.

12) p16, line 385. “with a ~~a~~ clockwise change”

This has been modified in the manuscript.

13) p16, line 389. “cases ~~that~~ do not show”

This has been modified in the manuscript.

14) p17, line 401. “decay ~~up~~ from about”

This has been modified in the manuscript.

15) p17, lines 402-403. This sentence is misleading. As written, it implies that the CBL during the morning is responsible for the gradual decay in jet core wind speed overnight. This cannot be true. What I think you mean is that the onset of the CBL in the morning leads to the jet core wind speed starting to increase again and hence is responsible for the minimum wind speed being right before sunrise.

The formation of the CBL enhances the vertical motions and increases the surface-driven buoyancy, breaking the atmospheric stratification and reducing the intensity of the wind speed at the level of the jet core. The sentence has been edited to express this more clearly:

p17, line 425: “The WSLIJ gradually starts to decay from about 4 h after sunset until it reaches a minimum right before sunrise, which is associated with the jet dissipation givenis the formation of the convective boundary layer during the morning.”

16) p18, Figure 8b. I assume the number at the start of the line is a wind direction, but the caption doesn't actually say what. Is it the wind direction at sunset or at jet formation? I assume this is what WDLIJ SS means in the title? Again, not actually defined anywhere.

Thank you for pointing this out. The labels represent the wind direction values at sunset or the initial wind direction if the jet is formed in the middle of the night. The caption of the figure has been modified for clarification.

17) p19, lines 425-427. This sentence is confusing “These cases present differences .. regarding the 11 and 13 August...” Which cases? Differences compared to what? Are you trying to say that the 11 and 13 August are different to the other cases?

The sentence has been edited to ensure the clarification:

P 19, line 453: “A different situation is observed for the seven jets detected between the 06-13 August. Persistent synoptic conditions are prevail for the first five detections between

06-10 August, with similar nocturnal evolution of core characteristics and vertical mixing. However, the last two detections on the 11 and 13 August (outliers in Fig.7) differ clearly from this period as they have short duration, low core height, weaker wind speed, and low vertical mixing”

18) p20, line 460. This sentence is wrong. You have written that UHI is proportional to  $\sigma_w^2$  and  $\sigma_w^2$  is proportional to ZLLJ. In the first case, the relationship is definitely not linear according to figure 9b, and in the second case it also does not appear to be true (though it's harder to judge from looking at the colour of the dots in figure 9b. I agree there is a relationship between these variables, but they are not proportional.

Certainly, the intention is to express that the UHI intensity presents a clear non-linear relationship with  $\sigma_w^2$  which in turn is dependent on the core height of the jet. Perhaps using the concept of proportionality is not the right approach to express this idea, that is why the manuscript has been edited to ensure clarity for the reader.

p21, line 463:“Table 2 presents a summary of the average values of  $\Delta$ UHI and the LLJ characteristics, corresponding to each  $\sigma_w^2$  class, revealing that in general low values of vertical mixing are associated with strong  $\Delta$ UHI and calm winds and shallow core heights, whereas strong mixing relates to weak  $\Delta$ UHI and strong wind speeds and greater core heights. ”

19) p21, line 475. “see ~~section~~ Section 2.4)”

This has been modified in the manuscript.

20) p22, line 495. “such as ~~that~~ height, ...”

This has been modified in the manuscript.