General comments:

The manuscript has been improved by the authors' intensive effort to make thoughtful revisions in response to the previous comments. Through the revisions, the contribution of the proposed simple model in the broader context of urban climate research has become clearer. Sections 4.1 and 4.2 are also significantly improved by providing detailed descriptions and interpretations. Regarding Section 4.3, the interpretation seems to be correct, at least if we assume that the observed data were obtained under ideal conditions. On the other hand, there remains a question as to whether the contributions of factors other than the changes in heat capacity associated with urban development to the trends in ΔT and ΔDTR are relatively negligible. One of the reasons for that would be a lack of discussion about the absolute values of long-term changes in T and DTR at each of the urban and rural sites. Although I have the same opinion as reviewer 2's previous comment, that is "I am not sure if section 4.3 is necessary for this study", this should not affect the main parts and conclusions of this study. I think the proposed model is helpful in understanding the essence of the basic mechanisms of UHI.

We sincerely appreciate your positive evaluation of our revision efforts and your recognition of the improved clarity regarding the proposed model's contribution.

Regarding Section 4.3, we fully agree with your insight that the long-term trends in ΔT and ΔDTR observed in real urban environments are the result of complex interactions among various factors, not solely changes in heat capacity. Accordingly, we have revised the manuscript to explicitly acknowledge these limitations. We clarified that the purpose of Section 4.3 is not to claim that heat capacity is the exclusive driver, but to examine whether the physical behaviors and long-term trends observed are consistent with the mechanisms highlighted by our simplified time-integrated SEB framework.

Furthermore, as suggested, we have added a discussion on the absolute values of long-term changes in mean temperature and DTR at both urban and rural sites to provide a more comprehensive context. While we acknowledge, as you and Reviewer 2 noted, that Section 4.3 is not strictly necessary for the derivation of the study's main theoretical results, we have opted to retain it as a supplementary analysis that bridges our theoretical model with empirical observations. Finally, regarding Figures 8 and 9, we have increased the font size of the axis labels to improve readability.

Minor comments:

[1] l. 213 ηS --> η_S (S is a subscript)

Thank you for pointing this out. This has been corrected in line 213.

[2] l.263 "the difference in elevation between them is 38.4 (40.3)m" Which station is larger? Is ΔT used in the analysis applied to height correction?

Seoul is 38.4 m higher than Yangpyeong, whereas Suwon is 40.3 m lower than Icheon. To clarify this, a negative sign has been added before 40.3 in line 263.

The temperature differences (ΔT) used in this study were not corrected for elevation. However, since the elevation differences between the paired stations remain nearly constant in time, their influence can be regarded as an approximately constant offset and thus does not affect the long-term trends of ΔT and ΔDTR, which are the focus of this study.

[3] l. 284 : (colon) --> . (period)

Thank you for pointing this out. This has been corrected in line 284.

[4] l.291 and Eq. (5) Is S_0 (constant solar radiation) the value for a clear-sky day at local noon? If so and my understanding is correct, I wonder the solar radiation is overestimated by applying the noon value for the whole daytime (12 hours)?

S_0 is used as a representative forcing scale and is chosen to be of the order of the clear-sky solar radiation at local noon. We agree with the reviewer that applying a simplified daytime solar forcing can overestimate the daily integrated solar radiation compared to a realistic diurnal cycle. However, the primary objective of this theoretical model is not to derive precise quantitative values, but to elucidate the qualitative roles and fundamental mechanisms of key physical parameters.

Specifically, our aim is to analytically demonstrate how an increase in heat capacity leads to a damping of the diurnal temperature range, or how a decrease in albedo results in an increase in the mean temperature. In this context, while the exact temporal representation of solar forcing (e.g., noon value versus daily average) may affect the absolute magnitude of the results, it does not alter the core physical processes or the qualitative relationships that this study aims to highlight.

To clarify this point, an explanation of S_0 has been added in lines 291–292.

[5] l.377 The term “UCI” might be inappropriate here, because it is not clear whether the lower daytime temperature in urban than rural sites is due to the UCI or to other factors associated with location differences (e.g. elevation and latitude).

We agree with the reviewer that attributing the lower daytime urban–rural temperature difference observed at a specific time directly to a “UCI” can be influenced by site-specific factors such as elevation and latitude. However, the focus of this study is not on the absolute magnitude of the temperature difference at a given time, but rather on the long-term trend (slope) of the urban–rural temperature contrast.

The decrease in daytime UHI identified in this study represents a temporal change associated with the urbanization process over time. Such a change is expected to be only weakly related to factors such as elevation or latitude, which remain nearly constant in time. In this context, the weakening of daytime UHI is interpreted as a strengthening of UCI characteristics.

Nevertheless, to avoid potential ambiguity, the corresponding description has been revised with more cautious wording in lines 377-379.