We have thoroughly amended the article according to reviewers' remarks. Thereafter are the comments of both reviewers (in italics) with our corresponding answers (in blue). The text is also available with revision marks.

More particularly, the main following points have been addressed:

- We have compared our approach with the one published by Wongnak et al. (2022)
- We have explained the choice of using a log-transformation of the data
- We have explained why our analysis is not focused on inter-annual variations but rather gathers all year data to fit a site specific equation
- We have computed Vapor Pressure Deficit and related it to Summer tick densities
- We have included more recent references in the introduction and the discussion sections

Some other minor points are also addressed (e.g. « mountainous » climate was replaced by « continental » climate).


***Response to reviewer 1:***

*1. The same data have earlier been analysed and published in Scientific Reports (Wongnak et al. 2022). It is really hard to see what the current analysis adds relative to the Wongnak paper? In Wognak, 3 patterns of phenology is presented (Figure 6), and it is analysed relative to weather variables. A lot of info is repeated.*

Figure 6 and related patterns from Wongnak et al. (2022) relies on a graphic interpretation of data (median data for the different sites) and thus differ from our analysis based on statistical modelling using harmonic regressions. Our model does not show any autumn peak for eastern sites on the contrary to the description presented in Wongnak et al. (2022). A point in the discussion has been added accordingly:

L301. We used the same data set as Wongnak et al. (2022), who derived patterns applied to the different sites. However, their results relied on a descriptive analysis of the data, whereas our conclusions are based on a statistical model based on harmonic regressions relating tick abundance with time, thus dedicated to the assessment of tick phenology. Even though both analyses often converge, some differences may be highlighted, as for instance the absence of any autumn peak in eastern sites in our analysis.


2. Analysis.
*Why use a log-transformation, rather than a negative binomial distribution usually fitting such data very well?*

In his article (For testing the significance of regression coefficients go ahead and log-transform count data Methods Ecol Evol - 2015), Ives demonstrated that linear models based on log transforming data are robust and sometimes more than related GLM assuming negative binomial distribution, even though they should be handled with care if it is to relate to the back-transformed data. We were not interested in these latter as we are mainly dealing with tick phenology, not abundance (see below). Furthermore, using linear models makes the significance of regression coefficients more straightforward.

*Line 196-197. You do not test for different seasonality between years – why not?*
*Line 216. It appear that you infer whether a year is good or not just by looking at residuals visually. We then do not know if this is by chance, or significantly different for a given year.*

Our study aimed to assess tick seasonal patterns from longitudinal data collected in sites differing from a climatic point of view. These patterns were assessed for an average year (called « typical year » in the article) by fitting a statistical model on the data from all the years investigated for a given site. Fitting a pattern on each year was not the aim of the article and such a fitting would have lowered the power of the whole analysis. For the same reason, we did not look for any significant difference between years. Differences between model and data was only assessed graphically to highlight the between-year variability which we don't deal with in detail further.

In order to be clearer, we added a sentence in the objective section at the end of the introduction:

L98. Our objective was to assess these patterns for an average year, called typical year thereafter, by the use of the whole set of data in order to maximize the power of our estimation, thus ignoring the between-year variations in the analyses.

*Figure 4 and 5. You estimate the peak abundance a given year. That is new compared to Wongnak, as far as I could judge. If this is the main new thing, you need to focus your paper to a much larger degree. Also, table I present the same datapoints as in figure 4. You can also add data points from figure 5 to the table I; possibly also data on abundance (reported in fig. 6). In figure 6, add a regression line. I think you should consider doing a similar analysis for the peaks.*

Our work gives the seasonal evolution of tick abundance for a « typical » year. Its main interest lies in the assessment of tick phenology for sites differing by their climatic situation. Tick abundance at the peak for a given site and a given year depends on many other factors than climate, notably host abundance, influencing the number of feeding ticks, which have not been taken into account in the model. On the contrary, we assume that winter tick abundance depends more on their activity level and therefore on temperature, as humidity is usually sufficiently high in this season. This explains why we focused on the relationship between winter tick abundance and temperature and on its variation between sites.

In a sake for clarity, we added the following sentence in the « data and models » section :

L193. On the contrary to peak tick abundance, which depends on many factors which have not been accounted for in the model (e.g. host abundance), we hypothesize that winter tick abundance relates to corresponding temperature, which has an influence on tick activity. To analyze these potential links,…

This explains also why we did not include peak abundance data in table I. Winter abundance and temperature have been added, as well as a regression line on figure 6.

*Line 66-71. Not so relevant. Can be deleted.*
Done

*Line 115. "controlled population". What does this mean? Fenced?*
A precision was added

*Line 123. Be explicit on how many sampling occasions were missing.*
Done

*Line 179. What is meant by an "important decrease in AIC"?*
This was replaced by « we reach a plateau thereafter »

*Line 302. There are several other paper having flagged year round.*
We added in the discussion section the references that focused on a comparison between locations differing from their climatic characteristics (i.e. Alonso-Carné et al., 2015; and Qviller et al., 2014).

***Response to reviewer 2:***

**Introduction section**

*L50: Please provide the brackets in the reference '(IPCC,2021)'*
Done

*L64-66: I think you should use more updated references here since these refer to 20 years ago. In recent years, new evidence in the distributional changes of I. ricinus in Europe have been published. Please, consider the following references:*
*•      Hvidsten D, Frafjord K, Gray JS, Henningsson AJ, Jenkins A, Kristiansen BE, Lager M, Rognerud B, Slåtsve AM, Stordal F, Stuen S, Wilhelmsson P. The distribution limit of the common tick, Ixodes ricinus, and some associated pathogens in north-western Europe. Ticks Tick Borne Dis. 2020 Jul;11(4):101388. doi: 10.1016/j.ttbdis.2020.101388*
*•      Garcia-Vozmediano A, Krawczyk AI, Sprong H, Rossi L, Ramassa E, Tomassone L. Ticks climb the mountains: Ixodid tick infestation and infection by tick-borne pathogens in the Western Alps. Ticks Tick Borne Dis. 2020 Sep;11(5):101489. doi: 10.1016/j.ttbdis.2020.101489.*
*•      De Pelsmaeker N, Korslund L, Steifetten Ø. High-elevational occurrence of two tick species, Ixodes ricinus and I. trianguliceps, at their northern distribution range. Parasit Vectors. 2021 Mar 18;14(1):161. doi: 10.1186/s13071-021-04604-w.*

These references have been added in the introduction section.

*L80: The medical term is Lyme borreliosis, not Lyme disease. Please, correct it throughout the manuscript.*
Done

*L99: Please revise the correct spelling of this citation*
Done

**Results section**

*L193: Argasid ticks have nymphal instars, but not ixodids. I suggest you use just 'nymphs', please correct it throughout the manuscript.*
Done

*L197: 'between sites exhibiting different patterns'*
Done


**Discussion section**
*L345-46: It is the first time you refer it. If this was evaluated, please provide some information about it in the Materials and methods section (how this was measure?). Was this evaluated in general? Did you check it for each sampling location? What kind of relation did you expected? Please, provide this in the supplementary materials. Saturation deficit has been well studied and determines the questing behaviour of I. ricinus (https://doi.org/10.1007/s004360000209). Some authors have described a non-linear relationship between saturation deficit and questing behaviour of nymphs (https://doi.org/10.1186/s13071-017-2112-x; https://doi.org/10.1186/s12898-017-0141-0). Do the measurements you collected enable the calculation of saturation deficit? Your findings could similarly support such relation, although the climatic variable employed in this study was not exactly the same the authors used, but correlated to it.*

Daily mean VPD was computed from observed daily mean temperature and relative humidity data and averaged between years, for each site. Similarly to what has been carried out for January temperature and tick abundance, mean August tick abundance has been related to the corresponding mean VPD on the different sites. Only a weak relationship has been found. The corresponding figures were included in the Supplementary material. These points are discussed.


*L361: 'compared to'. Please correct with 'compared with'*
Done

**Reference section**
*Some references cited in the main text are missing in the reference section. Please, provide them accordingly. The references a refer to are the following:*
- *'Pérez-Eid, 2007' used in L130*
- *'Wongnak et al, 2021' used in L149-150*
- *Babenko (1958) (in Korenberg, 2000): I'm not sure about how this reference is cited in the main text (L293). Please, revise it if it is necessary*

References for Perez-Eid (2007) and Babenko (1958) have been added. Wongnak et al. was indeed published in 2022 (but on line in 2021). This has been corrected.