Response to Anonymous Referee #2

We thank Referee #2 for his/her comments, which are addressed as explained below. The Referee's text is reported in Italic and our responses in roman.

In this manuscript, the authors present an analysis of the role of variability in rainfall (amount and frequency) on average heterotrophic respiration for dry ecosystems. It is an attempt to provide a mechanistic explanation of the 'Birch' effect, showing that respiration pulses are more relevant under low precipitation frequency and to a lesser extent to low rainfall amounts. The analysis relies on a very simple stochastic model that links a stochastic representation of rainfall to changes in moisture and subsequent changes in heterotrophic respiration. The resulting probability density functions (PDFs) are then expressed in terms of their first and second moments. I appreciate the effort of the authors in highlighting the limitations of the model, because due to its simplicity, it excludes a number of process commonly integrated in soil respiration models. However, I agree with the authors in that the minimal model presented here is enough to capture the main dominant processes that capture the stochastic nature of respiration pulses. This is enough to provide a general understanding of the problem, but these results should be used with caution when attempting to represent other type of systems; e.g. systems with high seasonality in litter inputs or water-logged. I have only very minor comments, and can recommend the manuscript for publication after small modifications.

The reviewer nicely summarized the philosophy of our contribution—indeed, we do not aim to describe in a fully mechanistic way all processes involved, but to analytically link the statistical properties of heterotrophic respiration to those of precipitation. This approach represents also the main novelty of this contribution. To further emphasize model limitations along the lines suggested by the reviewer, we can add a further sentence in the Discussion:

"In addition to these limitations, our results should also be interpreted with caution when rainfall seasonality is important, because the assumption of stochastic stationarity may not be met, requiring the derivation of a different probability density function of soil moisture (e.g., Vico et al., 2017). Nevertheless, our results will still hold for parts of the year when the rainfall regime is relatively stable."

Minor comments:
• Line 100. The parameter gamma is an important parameter because it controls the exponential decay in both terms of equation (3). Please give a more intuitive explanation of this parameter.

We can include the explanation: "γ can be interpreted as the number of average rainfall events needed to replenish the plant available soil water."

• Line 105. I think the term 'stochastic steady-state' is not appropriate. A steadystate refers to a fixed point in the phase plane, which is not the case for the stochastic case here. I suggest using the term 'stationary' instead.

Following this suggestion, the term "steady state" will be changed to "stationary" throughout the manuscript.

• *Eq. (8). I think it would be helpful for the reader to split this equation in two lines, with the first line using an inverse function notation, so it is clear that your aim is to find the inverse of equation (7).*

This would be a good idea had we defined a function $R_d = f(x)$. Then the inverse $x = f^{-1}(R_d)$ could be used in Eq. 8. We have not defined such a function and more simply expressed respiration as a function of soil moisture in Eq. 7. We would prefer not to add a further symbol *f* here and leave the notation as is. However, we can add a clarification:

"… the PDF of soil moisture is evaluated at moisture values corresponding to given respiration values. This is done by inverting Eq. (7) and expressing *x* as a function of R_d"

Also, I think it's important to explicitly mention that finding this inverse implies that you can only use this approach for bijective functions, and not for non-bijective functions. There are many functions in the literature that show that respiration as a function of soil moisture has a maximum at an intermediate level, after which respiration declines with moisture (e.g. Skopp 1990, SSSAJ 54:1619). These functions are non-bijective and therefore have no inverse over their entire domain. A consequence of this, which I think is partially addressed by the authors, is that you can only use this approach when predicting the behavior of respiration at low soil moisture levels.

The reviewer is correct that a non-monotonic (and thus non-bijective) function would require considering separately the different domains of soil moisture in which the respiration function is (locally) monotonic. To clarify this issue, we will add the following sentences:

"We note that Eq. (7) is monotonic in the domain $0 \leq x \leq 1$, which allows defining unambiguously the inverse of $R_d(x)$. Had we used a non-monotonic $R_d(x)$ function (e.g., for applications of this approach to soils experiencing long saturation periods), the derived distribution approach would have required splitting the *x* domain into two—one for each monotonic branch of $R_d(x)$."

References

Vico, G., Dralle, D., Feng, X., Thompson, S. and Manzoni, S.: How competitive is drought deciduousness in tropical forests? A combined eco-hydrological and eco-evolutionary approach, Environ. Res. Lett., 12(6), 065006, doi:10.1088/1748-9326/aa6f1b, 2017.