This is an interesting research on the annual variation in *Plasmodium* infection in peripheral blood from Great tits. The manuscript is well written and the research design is correct. However, at the end of discussion it appears to be clear that authors are treating with two different and isolated populations of Great tits affected by different Plasmodium specires. If that is the case, they are comparing two different host-parasite interactions. This is also interesting because it is a way to show similarities and differences between both host-parasite interactions but I think it must be clearly stated from the beginning and not at the end of the manuscript. Both host-parasite interactions coincide in the annual pattern of variation although in one population parasitaemia reach higher levels than in the other. From here some differences could occur and should be clarified: Are there differences in reproductive parameters between host populations? In addition, I suppose that there are not mixed infections because you mention that individuals infected by each parasite come from different host population but this should also be clearly stated.

Minor comments:

Line 153: I suggest that you make the effort to define and use a single term throughout the entire manuscript.

Line 160, Age was included in analyses but you do not mention how age was calculated.

Figure 1 and associated results: The relationships between winter, spring and autumn parasitaemia are interesting but, as you include three years of study, I wonder if it would be possible to study also how autumn parasitaemia affects winter parasitaemia the following year. Perhaps you have not enough sample size for that comparison but, if that is the case, you should mention it in the text.

Line 282: I suggest you analyse infections by other blood parasites in both Great tit populations including *Leucocytozoon*. Interaction among blood parasite infections could affect to your results.