Thank you for your reply, and for considering my review. I think that your article has results which are worth to be communicated. But I also feel that more modifications are still needed to avoid unnecessary irritations or misunderstandings. In the following I have summarized my main concerns, related to your responses.

Responses to general comments

"*The land component of the models has been kept constant, too. A corresponding remark was added in Section 2.2.*"

I am sorry, but I could not find any sufficient explanation on the land component in section 2.2. Your remark that land parameters are fixed. But Schmidt et al. (2010), who describe the Hhi-max simulation, do not mention that the land component has been kept constant in this experiment. As they do not give any specific details on the land model configuration, it must be assumed that the land model is normally included and thus interacts with the atmosphere as in the underlying base model ECHAM5, for which Roeckner et al. (2003, 2006) are cited.
I think this is an important detail for the interpretation of the results and the conclusions which can be drawn. As long as this is not sorted out, you cannot claim that the oscillations observed in the atmosphere are self-sustained by the atmosphere basic dynamics. Still it is interesting to find such oscillation in simulations where no interactive ocean model is included.

"*Agreed! The longest periods in Tab.2a are shown just for completeness. They are not really used in the paper. The corresponding error bars in Tab.2a are large and thus are a warning. Nevertheless it is interesting to see that the longest periods of HAMMONIA and of WACCM find approximate counterparts within combined errors in ECHAM6.*"

The explicit mentioning of the 341 year period in the key-points and abstract gives the message that you consider them as important enough to be highlighted. If you avoid this, the reader would not become disappointed when understanding later that the error bars are so large. It is certainly interesting enough to point out the multi-decadal time scales, which you diagnose in a system without an ocean component.

Responses to specific comments

"*This is a misunderstanding: We did not claim an atmospheric origin of the oscillations, but we said that the oscillations are atmospheric properties. We do not know yet the origin of the oscillations, as was stated several times in the paper. We certainly agree that clarification will presumably need a number of steps.*"

If no claim is intended in an atmospheric origin of the oscillations, the wording needs to be adjusted in several places across the whole manuscript. I find it very irritating to read for instance in the Key Points: "self-sustained oscillations linked to the atmosphere basic dynamics" although you respond that you do not claim an atmospheric origin of the oscillations. "linked to atmosphere basic dynamics" in my understanding implies that the atmosphere is the cause.