We would like to thank the anonymous reviewers for their comprehensive comments. Below, we provide our responses in detail and describe the corresponding changes in the manuscript.

Referee #2

RC2: *lines 79-84: a brief justification may be needed here on why this study focuses on the "soil temperature profile" (by looking at the 1 m below ground in section 3.1) and not , also, the uppermost soil layer (= surface) temperature which is ultimately connected to the radiative and heat fluxes that drive the overlying air temperature, the surface climate parameter of main interest.*

AC: *As reported in authors' response to RC1 comments, we will add the soil temperature response to afforestation at three additional soil depths in results section.*

Changes to manuscript: *We will add the soil temperature response to afforestation at depths of 2 cm (close to uppermost soil layer and surface temperature), 20 cm and 50 cm, in addition to 1 meter.*

RC2: *line 98: as opposed to which PBL scheme in WRFb-NoahMP?*

Changes to manuscript: *we will add in line 99 "..as opposed to MYNN Level 2.5 TKE in WRFb-NoahMP.."*

RC2: *lines 125-128: is thermal diffusivity κ (see below) parameterised in the RCMs land surface schemes (and therefore derives from moisture affecting heat capacity, as you mention) or it is taken as a constant from look-up tables? Could this property be shown for each model (especially if the authors feel it would assist interpretation of results)? From textbooks (pages 397-398 of McIlveen (2010) or section VIII.B. Conduction of Heat in Soil in Hillel (2003)) the thermal diffusivity is defined as:*
$\kappa = k/(\rho C)$ where k = thermal conductivity ρ = density C = specific heat capacity The authors may consider the information in the Chen and Kling (1996) for better introducing and perhaps diagnosing in future studies, the thermal diffusivity κ.

AC: *Thermal diffusivity is time-dependent and is parameterized in LSMs depending on the soil type, soil composition (organic matter content, mineral components), bulk density and soil wetness. We are not able to provide this hard-coded quantity for each model, as it is not usually a model output variable. Although, in our experiments soil composition and soil types are unchanged between the two land-use change scenarios, and only changes in soil wetness could have impact on thermal diffusivity. Also, RCMs do not account for possible occurrence of heat sources or sinks (such as organic matter or carbon decomposition) in the realm where soil heat flow takes place. In this way, we use soil moisture response to afforestation as an implication of changes in thermal diffusivity.*

Changes to manuscript: *We will better introduce thermal diffusivity. We will also add a column in table 1, where we are going to provide the parameterization schemes used from each model for calculation of thermal conductivity and volumetric heat capacity.*

RC2: *lines 128-129: the fact that "GHF is calculated as the residual of surface energy balance because*

the actual GHF outputs were not available in most models" assumes that model surface energy budgets are balanced, something that it may not be the case for, e.g., WRF (section 3.3 in Constantinidou et al., 2020a)
***AC**: We agree that this should be mentioned*

***Changes to manuscript**: In Line 128-129 we will add the phrase "we define as energy input into ground the residual energy amount resulting from available radiative energy (net shortwave + incoming longwave radiation) minus the sum of turbulent heat fluxes, without accounting for likely deviation of surface energy budget from assumed balance in models (Constantinidou et al. 2020) "*

***RC2**: lines 169-170: Would it be useful to also show (in the Supplementary), not only the forest minus grass effect on the "annual amplitude of soil temperature (AAST) at 1 meter below the ground surface" (as done here), but the absolute value of annual land surface (skin) temperature as well?*

***AC**: We do not think that an annual mean of surface temperature would add value in our results. The large inter-model spread in AAST originates from summer temperature differences, whereas winter soil temperature sensitivity to afforestation is pretty small. Thus, we focus on summer season. Also, the surface temperature response is strongly based on the residual of surface energy balance and has already been examined in previous studies established in LUCAS FPS (Davin et al 2020, Breil et al 2020). Moreover, in our revised manuscript, the soil temperature response to afforestation at 2 cm below the ground (close to uppermost soil layer and surface) across seasons will be added.*

***RC2**: line 230: Regarding the afforestation response of GHF, "Scandinavia appears to be the most sensitive among the regions". Any reasons?*

***AC**: The intensified coupling between surface and atmosphere in Scandinavia is caused by two factors; first, in Scandinavia forests consist of needleleaf forests with higher surface roughness (mixing-facilitating characteristic which enhance the heat exchange) compared to broadleaf trees dominating in the rest regions. Second, strong reductions in cloud fraction are noted in many models over Scandinavia, with result to intensify the albedo effect with afforestation.*

***Changes to manuscript**: In our revised manuscript, we will highlight the above-mentioned factors which affect the land-atmosphere coupling with afforestation in Scandinavia.*

***RC2:** lines 425-427: Can you also connect the results with the overarching ambition expressed in line 65 to "better constrain and represent the LUC biophysical forcing in regional climate simulations over Europe"?*

***AC**: Our sentence had unclear meaning, please let us reform this phrase*

***Changes to manuscript**: in line 65, the sentence "The crucial need to better constrain and represent the LUC biophysical forcing in regional climate simulations over Europe" will change to "The crucial need for the assessment of LUC biophysical impacts on regional scale over Europe".*

***RC2**: lines 431-432: these proposed evaluations should critically include the land surface temperature, as in Constantinidou et al. (2020b)*

***AC**: Agreed*

***Changes to manuscript**: we will include the proposed study.*

Minor/Technical Comments

The English need to be checked again as there are a few grammatical error or suboptimal expressions (some of them are listed below).

***RC2**: line 48: more correctly "On the contrary"*
***AC**: Agreed*

***RC2**: line 85: "second heat conduction law" can be written, more neatly, as "Fourier's second law of heat conduction". Same in lines 120, 226, 289*
***AC**: Agreed*

***RC2**: line 122: in equation (1), strictly, the derivative symbols should be replaced with partial differentials*
***AC**: Agreed*

***RC2:** line 127: "is the only variable which influence" should be "is the only variable which influences"*
***AC**: will be corrected*

***RC2**: line 291: "since affecting" should be replaced with "since it affects"*

***AC**: will be corrected*

***RC2**: line 402: replace "conducted an approach of" with "employed"*

***AC**: will be corrected*

References:

Breil, M., Rechid, D., Davin, E. L., Noblet-Ducoudré, N. de, Katragkou, E., Cardoso, R. M., Hoffmann, P., Jach, L. L., Soares, P. M. M., Sofiadis, G., Strada, S., Strandberg, G., Tölle, M. H., and Warrach-Sagi, K.: The Opposing Effects of Reforestation and Afforestation on the Diurnal Temperature Cycle at the Surface and in the Lowest Atmospheric Model Level in the European Summer, 33, 9159–9179, https://doi.org/10.1175/JCLI-D-19-0624.1, 2020.

Davin, E. L., Rechid, Di., Breil, M., Cardoso, R. M., Coppola, E., Hoffmann, P., Jach, L. L., Katragkou, E., De Noblet-Ducoudré, N., Radtke, K., Raffa, M., Soares, P. M. M., Sofiadis, G., Strada, S., Strandberg, G., Tölle, M. H., Warrach-Sagi, K., and Wulfmeyer, V.: Biogeophysical impacts of forestation in Europe: First results from the LUCAS (Land Use and Climate across Scales) regional climate model intercomparison, https://doi.org/10.5194/esd-11-183-2020, 2020.