**Response to Referee #1**

The authors thank the referee for the very detailed and constructive review and the time spent to analyse the manuscript. The review illustrates in many parts how our study should ideally have been conducted. Concerning the major points raised in relation to the used climate models we are still convinced, that the models used are suitable for our study despite the fact that there are substantial dynamics in this field and more recent models are available. Following the suggestions of the referee, our major revisions would include describing the downscaling process of the climate data to the vineyard scale including a schematic flow diagram, improving the statistical analysis of the validation of the weather generator including more weather stations, and including a further emission scenario, which will certainly improve the results. Our answers to the specific comments are given within the text in blue font below.

1. **General comments**

The rationale of the paper is based on the need to improve knowledge of local climate and soil moisture in order for vineyard regions to respond appropriately to climate change. The paper emphasises the need to downscale from global climate model predictions using regional climate models in order to assess effects on the water budget of grapevines at vineyard scale, so is in line with contemporary research trends. A weather generator and a water balance model are also used, accounting for local variations in soil characteristics, the complexity of the terrain, and crop management practices, in order to provide predictions of drought risk at vineyard scale.

The overall aim of the paper is therefore in line with objectives of international research into the application of climate models to assess impacts of and develop appropriate responses to climate change by downscaling model projections of future climate to vineyard scale. Although the general approach is fine, there are several areas of weakness in the paper, as discussed in the following section. In particular, some aspects of the methodology used in the paper seem to be rather dated and not clearly described. In particular, I would have expected that more recent climate models would have been used, given the rapid climate model development that has taken place over the past decade.

2. **Specific comments**

2.1 Soil moisture versus temperature

Line 31-32: I disagree with the general statement that 'Within the existing production areas, water shortage is probably the most dominant environmental constraint (Williams and Matthews, 1990) ….', which the authors appear to suggest applies globally. In many parts of the world, it is clear that temperature has a greater impact on grape production and wine quality, especially in 'New World' regions where irrigation is a standard practice.

**Response:** We agree that temperature has a greater impact on grape production and wine quality than water availability. Nevertheless, water is an important limiting factor not only under irrigated conditions. The statement "within the existing production areas" refers to areas, where temperature conditions are not a limiting factor for the cultivation of grapevines. The need to irrigate in 'New World' regions is an example that water shortage is an environmentally limiting factor within those regions. To avoid misunderstandings, we suggest to write: 'Within the existing production areas, where temperature conditions are in general favourable for cultivation, water shortage is probably the most dominant environmental constraint (Williams and Matthews, 1990)… '

2.2 Dated climate models

The climate models used in this research appear to be quite old and outdated (van der Linden and Mitchell, 2009) given the rapid developments in model design and downscaling techniques over the past decade. Even the web link for the ENSEMBLES Final Report states 'This object has been archived because its content is outdated.' (https://climate-adapt.eea.europa.eu/metadata/publications/ensembles-final-report). It is therefore unclear why more recent climate models from the CMIP5 and CMIP6 evaluations, or the available EURO-CORDEX model data are not used in this work. Recent publications referenced in this paper (e.g. Gutiérrez et al. 2019) appear to suggest that EURO-CORDEX is the preferred model framework for contemporary research, and there are many publications over the past decade that have been based on CORDEX climate model data.

**Response:** We agree that using a regional climate model ensemble from the ENSEMBLES project raises the question why data from EURO-CORDEX (the successor of ENSEMBLES) were not used in this study. When we started with the project (in 2015), all nodes of the Earth System Grid Federation (ESGF) to download the EURO-CORDEX data were out of service for several months without information of a date of return. The evaluation of the performance of the climate models against observational data of Kotlarski et al. (2014) showed that the improvements of EURO-CORDEX compared to ENSEMBLES were not very significant (a detailed comparison is described in section 4.6 of this paper). Kotlarski et al. reported comparable bias ranges for EURO-CORDEX and the ENSEMBLES simulations. Since we used a weather generator in our study, we needed only the change signals of the climate simulations. High-resolution RCM ensemble simulations of Feldmann et al. (2013) showed that the relative change of mean precipitation is quite uniform in the study region (with the limitation that Feldmann et al. focused on the near future 2011-2040), but suggesting that also the higher spatial resolution of EURO-CORDEX (12 km vs. 25 km) is of limited added value for our study. Overall, we concluded that the use the ENSEMBLES instead of EURO-CORDEX would not have a significant effect on the results, and, because we also had to start with the work, we decided to use the ENSEMBLES simulations. We suggest to include parts of the argumentation above in the paper to make it clear to the reader why the used models have their value.

Feldmann, H., Schädler, G., Panitz, H.-J., and Kottmeier, C.: Near future changes of extreme precipitation over complex terrain in Central Europe derived from high resolution RCM ensemble simulations, International Journal of Climatology, 33, 1964-1977, 10.1002/joc.3564, 2013.

Kotlarski, S., Keuler, K., Christensen, O. B., Colette, A., Déqué, M., Gobiet, A., Goergen, K., Jacob, D., Lüthi, D., van Meijgaard, E., Nikulin, G., Schär, C., Teichmann, C., Vautard, R., Warrach-Sagi, K., and Wulfmeyer, V.: Regional climate modeling on European scales: a joint standard evaluation of the EURO-CORDEX RCM ensemble, Geosci. Model Dev., 7, 1297-1333, 10.5194/gmd-7-1297-2014, 2014.

The dated nature of the climate modelling component of this work is also evident by the reference in Section 2.2.2 to application of the 10 selected models to the old A1B emission scenario (Line 152), which was developed over 20 years ago and has since been replaced by RCP scenarios (about ten years ago) and more recently by SSP scenarios (Tebaldi et al. 2021 – see below).

Tebaldi, C., Debeire, K., Eyring, V., Fischer, E., Fyfe, J., Friedlingstein, P., Knutti, R., Lowe, J., O'Neill, B., Sanderson, B., van Vuuren, D., Riahi, K., Meinshausen, M., Nicholls, Z., Tokarska, K.B., Hurtt, G., Kriegler, E., Lamarque, J.-F., Meehl, G., Moss, R., Bauer, S.E., Boucher, O., Brovkin, V., Byun, Y.-H., Dix, M., Gualdi, S., Guo, H., John, J.G., Kharin, S., Kim, Y., Koshiro, T., Ma, L., Olivié, D., Panickal, S., Qiao, F., Rong, X., Rosenbloom, N., Schupfner, M., Séférian, R., Sellar, A., Semmler, T., Shi, X., Song, Z., Steger, C., Stouffer, R., Swart, N., Tachiiri, K., Tang, Q., Tatebe, H., Voldoire, A., Volodin, E., Wyser, K., Xin, X., Yang, S., Yu, Y., Ziehn, T., 2021: Climate model projections from the Scenario Model Intercomparison Project (ScenarioMIP) of CMIP6. Earth System Dynamics 12, 253–293. https://doi.org/10.5194/esd-12-253-2021

**Response:** The use of the weather generator allowed us to scale the climate change signal of a climate simulation under a specific emission scenario to another emission scenario, based on a scaling factor (eq. 1, line 159) depending on the development of global mean temperatures (which depend on emission scenarios) simulated with the MAGICC model. We chose the RCP8.5 scenario (line 164-165) and will make this more clear in the text.

In addition, there is no serious critical assessment of the models selected for use in this work, particularly in relation to other potential sources of future climate model predictions mentioned above. For example, there is no serious evaluation of model bias associated with the different climate variables used to predict drought stress. How well do the selected models perform compared with more recent generations of climate model? Only generalised qualitative comments are made in this regard.

**Response:** We agree that a detailed model evaluation for the purpose of the study at hand should ideally be an integral part. However, with respect to our study it is difficult to define the important weather phenomena, which allow a selection of meteorologically reasonable climate models. For example, we made an evaluation of the impact of the length of dry spells on the occurrence of drought stress, but we found that this impact was in general small compared to the impact of the overall precipitation amount, since the soils serves as a storage for water. The complexity of the situation is enhanced by the diverse landscape characteristics and because the transition of moderate stress (at -0.3 MPa soil moisture tension respective predawn leaf water potential, positive for grapevines) to severe drought stress (at -0.6 MPa) corresponds to only 6-11 % change in available soil water capacity depending on the soil type. Since the authors from the climate-modelling field involved in this study contributed in kind (without funding), a detailed model evaluation proved to be infeasible.

We used a climate model ensemble already used in Maraun (2013) (this information is missing and we would add this to the revised paper). In this paper, the models were selected with the aim to separate the two sources of uncertainty, model errors and internal climate variability, regarding precipitation. In detail, three models from the MetOffice (UK) were excluded in our study, because the wind speed variable was on a different grid than the other weather variables (with possible implications on physical consistency of the weather variables). We concluded, in order to cover the uncertainty of future climate developments, this ensemble would be adequate for our purpose. We suggest that we include this argumentation into the paper to clarify the path taken.

Douglas, M.: When will trends in European mean and heavy daily precipitation emerge? Environmental Research Letters, 8, 014004, http://dx.doi.org/10.1088/1748-9326/8/1/014004, 2013.

Concerning the critical assessment of the models, particularly in comparison with more recent climate simulations, we refer again to Kotlarski et al. (2014), where an evaluation of EURO-CORDEX and ENSEMBLES were performed for temperature and precipitation. In addition, the general comments in the discussion section (line 407-410) suggest that the projections of the used climate models are comparable with more recent climate simulations.

Line 407-410 of the discussion: "This bandwidth is comparable with the results of the REKLIES–DE project (±20 % for annual precipitation, region Germany and drainage basins of large rivers, 2070–2099 compared to 1971– 2000), calculated with 37 climate simulations including the EURO–CORDEX data (Hübener et al., 2017; Bülow et al., 2019). Additionally, similar seasonal shifts (increase of winter and decrease of summer precipitations) were reported in this study."

The evaluation of model bias associated with the different climate variables used to predict drought stress would need additional research. Model biases have different sources. In the case of our study, the climate variability within a grid box is also a potential source of a bias, because climate model data represent spatial means of the grid boxes. The grid box containing the weather station Geisenheim contains not only vineyards but also parts of the Taunus mountain range (about 200-400 m higher in altitude). In this part of the grid box, it is too cold to grow grapevines and annual precipitation is about 200 mm higher compared to the drier parts of the grid box, where viticulture is performed (we referred to this issue in the introduction, line 51-54, (see below) because we are very aware of this "problem"). Even if a climate simulation would perfectly simulate a spatial mean of a grid box of an observational climate, the grid box mean can have a substantial bias compared to the (point) data of a weather station contained in the grid box, an intrinsic problem of an approach to relate climate simulations on a larger scale to individual weather station data.

Line 51-54: 'Predictions on a high spatial resolution are a challenge in climate impact studies and mainly limited by the size of one grid box of regional climate models (RCMs). Although climatic conditions within a grid box may change from being suitable for vineyards to areas unsuitable for the cultivation of grapevines, climate change impact studies for European viticulture where often forced to be performed based on the spatial resolution of the underlying gridded climate model data.'

We observed substantial biases between climate model data and observed weather station data, when comparing historical periods (e.g. 1971-2000). We also observed spatial shifts of simulated precipitation patterns compared to observed precipitation patterns in the study region. At a small scale, the source of a bias of an individual climate model compared to data recorded by a weather station remains unclear. Using a bias as parameter to assess the quality/reliability of a projected climate change signal of a climate simulation would therefore need further research. Since we only used the change signals of the climate simulations, which are more stable at a small scale, climate model biases were less important for our study.

In general, impact models for grapevines like the used water budget model (or e.g. models for phenological development) were developed based on measurements taken in vineyards and weather stations located in or not far away of those vineyards. Therefore, for our case, the weather generator was the central tool to produce transient time series. There are two key features, on the one hand, the preservation of the statistics of observational weather patterns of a weather station at the transition from observed data to projected data, and on the other hand, the production of future time series incorporating the climate change signals of climate simulations.

We suggest to discuss our methodology of applying climate models with respect to our case study in more detail in comparison to other and thus different approaches to address the reviewers concerns.

2.3 Lack of clarity regarding spatial downscaling methods

The methodological steps from the 10 climate model predictions to the daily weather generator, and subsequently to the water balance model at vineyard scale could be more clearly described. A schematic flow diagram outlining the steps involved in the methodology in Section 2.2.2 would be helpful.

**Response:** We agree to the suggestion to add a schematic flow diagram to illustrate with more detail the downscaling steps from the climate data at 25 km resolution to the vineyard scale. We suggest describing the overall process more detailed in a supplement to the paper, including the schematic flow diagram. We also notice that the scaling of the climate change signals based on eq. 1, with the opportunity to scale the signals to different emission scenarios, is difficult to understand for the reader. An additional figure, showing the scaling factor as a function of the year, for different emission scenarios could help to better understand the methodology.

Section 2.2.2 seems to suggest that the regionally downscaled climate model data are provided at a spatial resolution of 25 km, and that these data then drive the weather generator at the same resolution. Is this resolution sufficient to provide realistic spatial variability within vineyard regions in complex terrain? I found that the progression from 25 km to vineyard scale climate predictions is not well explained. In Section 2.4 it is stated that 'The study was based on the high spatial resolution of individual plots.' (Line 201), and that the digital elevation model (DEM) data appear to be at 1 m spatial resolution, while soil information is at approximately 25 m resolution (see below).

Line: 397: 'The soil data go back mainly to soil mappings conducted from 1947–1958 (Böhm et al., 2007), where at distances of 20 m x 20 m, respectively 25 m x 25 m, soil samples down to 2 m depth were taken and analyses performed.'

**Response:** The weather generator was not driven at the same spatial resolution (see more details below in the next response) as the RCMs. The DEM (at 1 m resolution) was used to calculate the mean slope and aspect of a single vineyard. Slope and aspect are then included in the calculation of reference evapotranspiration (section 2.3, line 177-178). The soil information at approximately 25 m x 25 m was used to derive the necessary soil water storage capacity data to run the water balance model for each single vineyard (section 2.4, lines 205-213, but we admit that the spatial resolution information of the soil data is missing in this section and will clarify this in the text). These two variables change substantially within the terrain and are important features of the water balance model approach.

Much of the subsequent analysis of results in the paper is based solely on the one weather station at Geisenheim, but there is also significant discussion of future drought stress in relation to individual vineyard plots (i.e. much finer resolution). It would be good to have a clearer explanation of how the model predictions of climate data at 25 km resolution are linked to the individual vineyard plots, presumably via assessment against weather station data and using the DEM and soil data in order to downscale to vineyard scale. For example, it is not entirely clear what is meant by the following statement:

Line 424: 'In order to downscale from the spatial means of grid box data of the RCMs to the spatial scale of station data, we used a weather generator to produce point data on the same scale as the weather stations and to simulate small–scale weather patterns'.

This statement suggests that climate variables from the regional climate models represent an average over 25 x 25 km grid squares (or volumes), but the underlined section above is unclear as 'weather station scale' is not defined. Figure 1 shows weather stations located within the two vineyard regions, often separated by only 2-5 km – is this what is meant by 'the same scale as the weather stations', or is 'weather station scale' a notional area represented by a single weather station (which may vary with terrain complexity)? If so, how is the weather generator used to downscale from 25 km resolution to 2-5 km resolution? Section 2.2.2 seems to be vague on this matter. In reality, the Rheingau vineyard region could be located within only one 25 x 25 km regional model grid cell. It is therefore unclear how the 12 or so weather stations located across the region are used to provide higher spatial resolution information in order to 'simulate small–scale weather patterns'.

**Response:** The climate simulations from 10 climate models were produced for each weather station (with observational data from 1959-1988) shown in Figure 1. A single vineyard plot (spatial resolution described in section 2.4, lines 200-205; and partially visible in Figures 11 and 12) was allocated to the nearest weather station. Therefore, each individual weather station represents an area defined by all vineyards that have the shortest distance to that weather station. We notice this information is missing in section 2.2.2 and we would add this to the revised manuscript. We think, with the vineyard plot specific data for reference evapotranspiration and soil data this approach is sufficient to provide the spatial variability of the terrain. We suggest an additional figure, maybe in a supplement to the manuscript, showing the time series of observed data (1961-1988) followed by the multi-model-mean for 1989-2100 for precipitation and/or the climatic water balance for all stations in order to illustrate the differences between the stations within the region.

Also, the statement on Line 441: '..we downscaled the grid box means of climate models to station (point) data in order to reduce the bias…' is vague and unhelpful, as it is obvious from the comments above that each grid box may contain several weather stations against which climate model output could be evaluated. It is therefore unclear how the model bias is assessed and/or reduced in this study.

**Response:** The Figures 6-9 show a smooth transition from the observed data of the weather station Geisenheim from 1961-1988 to the climate simulations from 1989-2100 and illustrate the reduction of the bias. Plotting the grid box data of the climate simulations directly would result in sudden shifts (biases), different for each climate simulation, at the transition from 1988 to 1989 (the transition from observed to simulated data). An evaluation of the climate model output of the grid box means against the weather stations would only confirm the existence of a bias between those data. This is, from our point of view, not really necessary, if the methodical steps will be explained with more detail as suggested by the reviewer, which would then also illustrate with more detail, how the model bias is reduced.

The maps shown in Figures 11 and 12 suggest that a fine spatial resolution of drought stress was achieved, although the spatial resolution of the mapped data is not indicated in the caption.

**Response:** We will add this information of the spatial resolution in the caption.

As mentioned previously, maybe a schematic flow diagram would help to illustrate in detail the steps taken to downscale data from climate models to provide soil water information at vineyard scale.

**Response:** Thank you for this suggestion. We see from the comments above that relevant information is missing and a more detailed explanation of the downscaling methods is needed and will provide this information.

2.4 Lack of model validation

As mentioned previously, most of the results were presented for one site (Geisenheim), and no validation against other sites was shown. Although this study is '….applied to individual vineyard plots of two winegrowing regions….' (Line 508), there appears to be no real validation of the results at vineyard scale. A set of high-resolution maps is a produced (Figures 11 and 12), but the lack of validation against data from a range of weather station sites would be needed to assess their true value. Figures 6, 7, 8 & 9 indicate that there is significant overlap between the climate model data (1980s to 2100) and available observations for at least some regional climate stations (1980s to 2020), which should allow a comprehensive statistical analysis of model performance.

**Response:** The model validation needs to be separated into the validation of the weather generator, which produced the climate data (used for Figures 6-9) and the validation of the water balance model, which produced data about water balance of each single vineyard (Figures 11 and 12). The water balance model was developed with the aim to cover the most important variables affecting the water balance. It was validated for three single vineyards of the study region with different characteristics (slope and aspect, different usage of cover crops, row spacing and soil characteristics, see Hofmann et al., 2014, https://www.frontiersin.org/articles/10.3389/fpls.2014.00645/full). This approach should ensure that the water balance model could also be used for other vineyards in the region and that the calculated water balance developments were realistic. The model and previous versions thereof have been used and validated in different vineyard sites across Europe (i.e. Lebon et al., 2003; Pellegrino et al., 2006; Gaudin et al., 2014). A validation of the results at a larger vineyard scale (i.e. many different sites) is unfortunately not possible because the required water balance data are only available for a few vineyards (not area-wide) and observations.

References (not already mentioned in the paper):

Gaudin, R., Kansou, K., Payan, J.-C., Pellegrino, A., and Gary, C.: A water stress index based on water balance modelling for discrimination of grapevine quality and yield, OENO One, 48, 1-9, 10.20870/oeno-one.2014.48.1.1655, 2014.

Also, the validation results discussed in Section 3.1 are mostly subjective (e.g. '….no substantial bias of mean values or monthly sums between observed and synthetic values were apparent. (Lines 228-9)), and should be made more convincing through the use of rigorous statistical analysis to investigate more fully the differences between the distributions of observed and predicted variables (for a number of climate stations). Otherwise, it is not possible for the reader to properly assess the efficacy of the model downscaling and evaluate the conclusions reached in this study.

**Response:** The validation results in section 3.1 refer to the weather generator (WG) and its capability to reproduce a climate observed from weather stations. To calibrate the WG, observed weather data from 1959-1988 (the 'baseline climate') were used. We agree to improve the statistical analysis and to include more weather stations.

Related to the previous comment, it would have been useful to comment more fully on the results shown in Figure 2. The synthetic data in this figure show lower rainfall, higher evapotranspiration and higher solar radiation compared with observations, in addition to the smaller range of their frequency distributions. Assuming that the model predictions are correct, is it possible that this reflects a general change in weather patterns under the selected scenario from cloudy low-pressure systems to clearer high-pressure systems? If so, what other climate risks could be associated with such a trend (e.g. increased frost frequency)?

**Response:** The synthetic data of Figure 2 do not include model predictions. The weather generator parameters describing the statistical structure of the observed climate (derived from the observed climate, 1959-1988) were not modified. We will try to improve the text for clarity.

2.5 Scenarios unclear

Although both the A1B and RCP8.5 scenarios are mentioned in Section 2.2.2, there is no indication of which scenario is used in the subsequent analysis sections (until Section 4 – Discussion). A significant omission is that none of the figures in the results sections mention the scenario that has been applied to achieve the results shown in each figure (it should be included in the captions). It should also have been emphasised that the RCP8.5 scenario represents 'business as usual' and is therefore the most extreme emissions scenario. Comparative maps of different scenarios (e.g. RCP4.5 and RCP 8.5) would be an interesting addition, alongside evaluation of any differences in the seasonality of drought risk that might occur under different scenarios. Referring to other studies, it is mentioned that 'Noteworthy, the projected bandwidth for precipitation for the mitigation scenario RCP2.6 are less than half of those for RCP8.5 (Hübener et al., 2017).' (Lines 416-417), but there is no attempt to undertake such a comparison between scenarios in this study.

**Response:** It is mentioned at the end of section 2.2.2 (line 164-165) that we chose the high baseline emission scenario RCP8.5. However, we agree that a comparative map for a different scenario could improve the results and that the emission scenario RCP8.5 needs to be discussed in more detail. Recent literature (Hausfather and Peters, 2020; Burgess et al., 2021) also suggests that RCP8.5 is increasingly implausible because it requires a very high and increasing coal use, which diverges from observed trends and energy projections of global $CO_2$ emissions.

Hausfather, Z., and Peters, G. P.: Emissions - the 'business as usual' story is misleading, Nature, 577, 618-620, https://doi.org/10.1038/d41586-020-00177-3, 2020.

Burgess, M. G., Ritchie, J., Shapland, J., and Pielke, R.: IPCC baseline scenarios have over-projected CO2 emissions and economic growth, Environmental Research Letters, 16, 014016, https://doi.org/10.1088/1748-9326/abcdd2, 2020.

2.6 Statistical interpretation

There is no detailed interpretation of the p-value trends shown in Figures 6b and 7b, only the brief statements:

Lines 290-1: 'For seven simulations, the projected trends were significant after the year 2073 (Mann–Kendall trend test, $p < 0.05$, Fig. 6b).'

Lines 292-3: 'The statistical significance of the trends was comparable to the trends of precipitation (Fig. 7b).'

Presumably, the null hypothesis being tested is that predicted precipitation trends are no different from zero, but the trends in p-values for the 10 models for both annual precipitation and climate water balance are only very briefly discussed. It seems to me that until about 2030 most models show no trend in precipitation, while by about 2070 eight out of ten models appear to show a statistically significant trend (in a couple of cases a negative trend). A major shift seems to take place between about 2030 and 2050. In contrast, the results for the climate water balance shown in Figure 7 seem markedly different, with only two models showing a statistically significant trend by about 2030, and much less agreement between models as to future trends. It would be useful to have further discussion of likely mechanisms here (in Section 3.3.1).

**Response:** Thank you for pointing this out. The contrast between the precipitation trends (Fig. 6) and trends of the climate water balance (Fig. 7) is related to the increase in reference evapotranspiration (see Table 3, line 337). We agree to discuss this more detailed.

Similarly, it would be useful to have more critical analysis of the results shown in Figure 10. The remarkable difference between the potential drought stress for the two periods (1989-2018 and 2041-2070) is not adequately explained. Presumably, the wide range of values shown for 2041-2070 for both regions could be explained by three poor-performing models, and if they were removed the differences between 1989-2018 and 2041-2070 may actually be minimal (but there is no such critical analysis here). There are some rather vague qualitative comparisons of 'bandwidth' in modelled precipitation mentioned in Section 4 (Discussion), and in relation to model evaluation, it is stated that 'This bandwidth could be reduced if the extreme models at the upper or lower edge would be excluded, but since no direct model flaws were detected, this would exclude possible future climate realisations.' (Line 411-12).

However, based on the information provided in the paper there does not seem to have been any serious attempt to undertake model validation (and I am not sure what a 'direct model flaw' is). There therefore seems to have been a lack of detailed critical analysis of the rather dated climate models used in this study, as mentioned earlier, and this seems to be a major weakness of this work.

**Response:** We focused on describing the uncertainty of the ensemble in terms of possible future climate developments rather than discussing individual models of the ensemble. A serious validation of the climate models, which would maybe end up in an exclusion of a climate simulation, is critical and this could not be performed for the reasons of feasibility mentioned above. What we meant with "direct model flaw" could also refer to errata of climate data, usage restrictions or other reported issues. Those issues could lead to an exclusion or withdrawal of a model in a project, for instance as reported by the Reklies-De project (see point 4. of http://reklies.wdc-climate.de/ but only in German). We suggest formulating "severe shortcomings" instead of "direct model flaw". To the knowledge of the authors such shortcomings regarding the used climate models were not reported elsewhere.

2.7 Lack of future research directions

There is no clear statement in the discussion outlining where this research might lead and what topics would be worth following up.

**Response:** We will add more information concerning this point, for both, the viticultural as well as the climate modelling perspective.

3. **Technical corrections, including typing errors and English expression**

There are a lot of problems with basic English expression which in some cases make the explanations confusing. Some suggested changes are indicated below:

**Response:** We appreciate all corrections and will incorporate them into the revised manuscript.

In several places:

Replace 'row distance' by 'row spacing'

Replace 'approx.' by 'approximately'

I would also suggest that the word 'bandwidth' is replaced with 'uncertainty' or 'variability' throughout the text as it provides a better indication of its significance in this application.

Line 11: Fix punctuation to – 'Extended periods without precipitation, observed for example in Central Europe including Germany during the seasons from 2018 to 2020, can lead to water deficit…..'

Lines 22-23: Replace 'Possible adaptation measures depend highly on local conditions and to make targeted use of the resource water,….' with 'Possible adaptation measures depend highly on local conditions and are needed to make targeted use of the resource water, while….'

Line 34: Replace 'Soil moisture decreased across Europe…..' with 'Soil moisture has decreased across Europe…..'

Line 38: Replace 'Despite of some newly emerging wine regions….' with 'Despite some newly emerging wine regions….'

Line 39: Replace '…..economically important grape cultivation of Europe.' with 'economically important grape cultivation in Europe.'

Line 50: Replace '…..cover crop or canopy management up to the implementation of irrigation systems.' with '…..cover crop or canopy management as well as the implementation of irrigation systems.'

Line 51: Replace 'Predictions on a high spatial resolution…' with 'High spatial resolution predictions…'

Line 53: Replace 'where' with 'were'

Line 57: Replace '….. the latter one also included possible changes in interannual variability.' with '….. with the latter study also including possible changes in interannual variability.'

Lines 61-64: Replace '….Moriondo et al. (2010) for expected changes for the premium wine quality area of Tuscany on a fine spatial resolution (1 km x 1 km, based on downscaling climate projections to station data and spatial interpolation). Only a few studies used data from soil maps including AWC as input data (Fraga et al., 2013; Moriondo et al., 2013), but on a spatial resolution still to coarse to represent the heterogeneity within growing regions. Recently, fine scale variability within growing regions were assessed…..' with '….Moriondo et al. (2010) for expected changes in the premium wine quality area of Tuscany at a fine spatial resolution (1 km x 1 km, based on downscaling climate projections to station scale using spatial interpolation). Only a few studies used data from soil maps that included AWC as input data (Fraga et al., 2013; Moriondo et al., 2013), but often at a spatial resolution still too coarse to represent the heterogeneity within growing regions. Recently, fine scale variability within growing regions has been assessed…..'

Lines 69-70: Replace 'Especially AWC, slope and aspect are very heterogeneous in steep slope regions and thus is the supply of and demand for water.' with 'AWC, slope and aspect are particularly heterogeneous in regions of complex terrain resulting in variability in the supply of and demand for water.'

Line 72: Replace 'like' with 'such as'

Line 73: Replace 'adapted' with 'modified'

Line 78: Replace '….the main objective of the study was to quantify the likelihood of risk for future water deficit on…' with '….the main objective of this study is to quantify the likelihood of risk of future water deficit on….'

Line 81: Replace '… for the characterization of vineyard landscapes…' with '… in order to characterize vineyard landscapes…'

Line 90: Replace '… bounded by the Rhine river to the south and the ridge of the Taunus mountain range in the north, and the vineyards near….' with '… bounded by the Rhine river to the south and the ridge of the Taunus mountain range in the north, as well as the vineyards near….'

Line 95: Replace '…in the west…' with '…. to the west…'

Line 99: Replace 'at' with 'on'

Line 102: Replace 'predominant' with 'particularly'

Line 109: Delete 'as'

Line 110: Delete 'was'

Line 118: Replace 'We worked with four time series, two observed and two synthetic series.' With 'We worked with four time series, two observed and two synthetic.'

Line 121-123: The sense of the sentence 'All stations recorded precipitation and the station Bensheim additionally temperature and relative humidity (also used for the other stations at the Hessische Bergstraße).' is unclear.

Table 1: Some column headings need reformatting.

Lines 184 & 185: Replace '…in form of…' with '…. in the form of….'

Lines 197-199: Poor English expression.

Line 215: Replace '… to assess…' with '…of …'

Line 216: Replace '…as drought…' with '…as a drought …'

Line 229: Replace '…on the other side…' with '…on the other hand …'

Line 233: Replace '…. for dry years…' with '…. of dry years…'

[There are many more, but I don't have time to correct them all. I suggest that a native English editor is used to eliminate the remaining issues.]

Referencing errors:

Line 141: Replace 'Garofalo et al., 2018' by 'Garofalo et al., 2019'

Lines 483 and 492: Replace 'Van Leeuwen et al., 2017' by 'Van Leeuwen, C., and Destrac-Irvine, 2017'