The paper tackles the problem of climate change in the Mediterranean, exploring the role of teleconnections and local feedbacks in modulating future projections. To this aim, climate simulations are designed and run using a state-of-the-art climate model. The authors find climate change in agreement with previous literature (warming and drying), however their findings suggest reduced amplitudes in change. They explain these differences with the improved ability of the model in simulating SNAO teleconnections and land surface feedbacks.

AU: We thank the reviewer for a very through revision comments and useful feedback regarding our manuscript. Please find below our responses to the reviewer's concerns. We have specified how we would address all of the major comments when revising the manuscript. We have also made an effort to address the minor comments. We will focus our attention also to improve the readability of the text and make it more concise.

Major comments 1. I acknowledge the huge work the authors did in carefully revising and discussing the literature, and in performing and discussing many analysis, but this makes the paper very long. My first general recommendation is to somehow shorten the manuscript, to facilitate the reader to be focused on the key messages delivered by the paper. For instance, discussions of previous findings is sometimes too detailed and redundant: accepted knowledge on summer climate in the Mediterranean region should be described in detail in the Introduction (or in a dedicated Background section) and briefly recalled when necessary in the text.

AU: Thank you for the comment. Following the reviewer's suggestion we will work on the manuscript to make it more comprehensible. Particularly, we will summarize the discussion of the previous studies in a more concise way. The relevant information on the Mediterranean climate, located in sections 3.2 and 3.3, will be moved to the Introduction or to the dedicated background section.

2. The main finding of the paper is the different amplitude of future projection of the Mediterranean climate simulated by the CM2.5 model in comparison with CMIP3 and CMIP5 simulations. To illustrate this crucial aspect, the authors refer to the existing literature on the topic. However, when quantitative differences are discussed, comparison with a Figure would be helpful. I suggest the authors to add some figures (in the supplement) showing projections of the future Mediterranean climate (precipitation and temperature) in the CMIP5 ensemble or, if downloading CMIP5 data is too time consuming, at least the CM2.1 model output, to show differences with the same model at lower resolution and not including the improved land model LM3.

AU: Thank you for the advice. We will be happy to include a supplementary figure showing future projections using the CM2.1 model output, analyzed for the summer season. We agree that this will be a good way to illustrate the impacts of the improved land model and increasing spatial resolution. Also, we would like to reference specific figures from Delworth et al. 2012, comparing the annual values in the CM2.1 and CM2.5 future projections.

3. Data: why NCEP reanalysis are selected for comparison? On the same period, ERAI data are available at higher resolution. ECMWF datasets are also available for the 20th century. Same question about precipitation data: why University of Delaware? Testing other temperature/precipitation datasets (CRU, EOBS) would change your results? In general, comparing your results with different datasets would improve the robustness of your conclusions.

AU: We took the reviewer's comment into consideration and will make an effort to address it during the revision process. Yes, we agree that a verification of the results with different datasets would improve the robustness of the conclusions. However, we would refrain from using the 20th century data sets such as ECMWF's ERA-20C or NOAA-CIRES 20th Century Reanalysis. Both of these data sets are based on the assimilation of the surface pressure values, which makes them a less plausible

reference in the analysis of the upper level atmospheric dynamics. On the other hand, input data in the CRU TS time series is not fully homogenized, which constitutes a major limitation of the data sets. We will include a comparison using alternative datasets, where it is possible, and extend the discussion appropriately. We will also include the justification of the data choices in the response to the reviewer's and the revised version of the manuscript.

4. One main issue of the paper is the choice of the time window to be analysed. The aim of the paper is to study summer Mediterranean climate, so that you select JJA. However, through the paper, different periods are selected for different analysis: JA or just July. I recommend to homogenise the period to be analysed (preferably JJA), for better comparison of the results. If there are specific reasons to analyse different period, these reasons should be highlighted.

AU: The choice of the time window is dictated by the method being most adequate to the analyzed climate component.. The teleconnection of SNAO with the climate over the Mediterranean region is manifest mostly during the peak summer, i.e. July-August. On the other hand the analysis in section 3.3. "Summer climate regime over the eastern Mediterranean" focuses on the month of July. "The choice is driven by the fact that the magnitude of subsidence and the Etesians is at its maximum in July while the response of the Rossby waves to monsoon rainfall is also the strongest (Tyrlis et al. 2012, Lin et al. 2007, Lin et al. 2009). The justifications of all the time window choices are included in the manuscript, but we will make our best to highlight this information.

5. Methods section is rather long and sometimes confused: EOF analysis is described twice, the description of correlation/regression analysis is not really necessary here, as well as the reference to figures discussed later in the paper. I recommend to focus the section on the description of more sophisticated methods, such as EOF and stormtrack definition, and leave the description of correlation/regression analysis to the Results section. The section should be then shortened and optimised.

AU: Thank you. Following the reviewer's suggestion, we will make an effort to clarify the methods section, and rewrite it in a more concise way.

6. Model validation: in Figures 1-4 you compare the CTRL simulation to the NCEP data, and I see some important biases in terms of intensity (SLP in monsoonal regions) and location of some features (axes of the anticyclonic circulations at 500 hPa). This is due to the fact that in NCEP reanalysis there is GHG forcing, which is not included in the CTRL simulation (as you also highlight in the text, P21, L34-37). It would not be more consistent to compare the CTRL simulation to a different period, i.e. a period of 20C reanalysis/precipitation less affected by GHG forcing?

AU: Comparing CTRL simulation with a different or longer period of observations than the recent three decades would be a more appropriate solution, from the perspective of contributing forcing components. Therefore for precipitation (University of Delaware climatology) we have used a longer period of data, i.e. 1900-2010. On the other hand, for the analysis of atmospheric circulation we prefer to use a shorter period of NCEP/NCAR2 (DOE) data set rather than the longer Twentieth Century Reanalysis. We justify this choice with the findings in Krueger et al. 2013. This study has shown that the early part of the SLP record (first half of the 20th century) in the Twentieth Century Reanalysis suffers substantial inhomogeneities, most likely associated with the increasing number of observations and improved measurement techniques. Moreover, taking into account that the Twentieth Century Reanalysis assimilates only surface pressure reports, sea ice, and sea surface temperature distributions, the expression of the high level atmospheric variables could be highly uncertain in this dataset. To address the reviewer's concern on consistency in the GHG forcing between the compared data sets, we offer a solution and compare the observations with the historical runs of the model as an alternative to the control run.

7. SNAO simulation: the analysis of SNAO impact in Figure 5 is not compared with any reanalysis product. The SNAO impact on climate in Europe is explained with variations in the stormtrack (Figure 7): why reanalysis data are shown and not model simulation?

AU: This manuscript focuses on the summer Mediterranean climate, analyzed from the perspective of the contributing factors, simulated in the GFDL CM2.5 model. In this regard, we devote a part of our attention on the capabilities of the model to simulate the regional (Mediterranean region) impact of the SNAO. The representation of the observed features of SNAO, based on the SLP in different time periods, is shown in Figure 6. However, the observed impacts of SNAO (including precipitation and temperature) are the main objective and has been shown in detail in Folland et al. 2009 and Blade et al. 2012. We would prefer to make a reference to the existing literature rather than to repeat an existing analysis, especially to maintain brevity given major comment 1 requesting that we shorten the manuscript length.
Yes, Figure 7 shows that the North Atlantic stormtrack is a good proxy to explain the SNAO impact on northwestern Europe (as shown also in Folland et al. 2009). Thus a comparison of the simulated and observed stormtracks would be a valuable input for a study focusing on northwestern Europe. However, the main objective of this study is different. By showing Figure 7 we intend to emphasize the sensitivity of the relationship between the SNAO and the storm tracks to the chosen period. For example, the relationship is stronger for the period starting in mid-century (i.e. 1950-1990, dominated with the dipole SNAO pattern (Figure 6e)), than for the later period (1970-2011, dominated with the monopole over the British Isles (Figure 6g), suggesting that the representation of SNAO derived from the recent decades can be obscured by other climate components. We will clarify this point in the text of the manuscript.

8. Figure 11: the caption of the figure and discussion at P17 should be improved. You first state that you compute EOFs for the CNTR simulation, than you project the HIST-PROJ fields onto the CTRL EOF to get the 1860-2100 time series. Than you state that you also compute EOFs separately for HIST and PROJ. Then you discuss the 1860-2100 time series in Fig. 11c, then you go to HIST and PROJ EOFs in Fig. 11ab, and finally you discuss the contribution of the 1860-2100 SNAO to the end-ofcentury projection of precipitation (Fig. 11de). I find this discussion confusing. This is a crucial point of the paper and should be presented clearly. I recommend the authors to improve the readability of this section. Moreover, the discussion of the SNAO impact on temperature projection should be significantly expanded.

AU: Following the reviewer's suggestion, we will clarify this part of the discussion, as well as the caption of Figure 11 and the corresponding methods.

9. Results: the differences between CM2.5 and CMIP5 models in projecting the paper Mediterranean climate are explained with a) better representation of the SNAO teleconnection and b) improved representation of the land-atmosphere interaction by the LM3 model (see also the Abstract). However, the improvements of the LM3 model are not presented in the paper, nor how these new features actually improve the representation of the land-atmosphere interactions (e.g. representation of soil moisture, evapotranspiration, albedo). A brief presentation of the LM3 model as well as a discussion of how it improves climate simulation in the Mediterranean is needed.

AU: Yes, we agree that including additional information, referring to the improvements in the land model would certainly refine the manuscript. We will make an effort to add a very brief description of the LM3 model as well as include appropriate references. However, an attribution of the differences in the future projections between CM2.1 and CM2.5 to the particular component of the land model is beyond the scope of this manuscript. Addressing that issue would require a new and differently designed analysis.

10. Conclusions: most of the paper is devoted to the analysis of the SNAO teleconnection and its impact on future climate change in the Mediterranean, which show a significant (P17, Figure 11) impact on precipitation in southern Europe. And in the abstract you indicate this as one of the main results of the paper. Conversely, in the Conclusions you somehow reduce the importance of the SNAO impact (P22,

L38-40), explaining the differences with the CMIP5 simulations as a consequence of the improved land model. This point needs to be clarified.

AU: Following the reviewer's suggestion, we will clarify the main message of the manuscript. We agree that the analysis of the impacts of the SNAO teleconnection consumes a significant part of the manuscript. This part shows a significant contribution of the SNAO to precipitation over southern Europe. However, the comparison of the SNAO impacts between CM2.5 model and CMIP3/CMIP5 models suggests that the SNAO can not explain the difference in the future projections between these models. The apparent stark contrast between the CMIP3/CMIP5 and CM2.5 regional projections could more likely originate from the enhancements in the LM3 land model incorporated to CM2.5 at high spatial resolution, rather than the impacts of the SNAO. Future work should focus on understanding differences in land surface responses in this region to the SNAO and projected climate change.

Minor comments:

P2, L15: the connection between the Mediterranean and the African monsoon has been robustly described as Mediterranean → Africa (see papers by Raicich et al. 2003 and Rowell 2003 [https://doi.org/10.1175/1520- 0442(2003)0162.0.CO;2]). The influence of the African monsoon on the Mediterranean is less clear: Ziv et al. 2004, but also Fontaine et al. 2011 [https://doi.org/10.1002/joc.2108], actually find a link between convection in Africa and subsidence in the Mediterranean, however the mechanism is still not clear (see Gaetani et al. 2011 [https://doi.org/10.1029/2011GL047150]). Indeed, the Asian monsoon could be dominant in modulating the Mediterranean-Africa connection. Please modify the sentence to account for this aspect.

AU: Thank you for the comment. We will apply the correction.

P2, L26-29: please add a reference.

P3, L18-20: please add a reference.

AU: Thank you, we will add the references.

P3, L43: "fixed levels of radiative forcing", do you mean 'radiative forcing from fixed levels of emission/concentration'?

AU: Thank you for the correction.

P4, Methods: is the model fully coupled? How many vertical levels are in the ocean model?

AU: We will add the information.

P6, L15-16: this sentence should be moved to the Results section.

P6, L18-19: what do you mean with "vector time series"? The time series of the vector containing spatial data?

P7, L39: from Figure 4, precipitation magnitude is actually, not "apparently", larger than observations.

P7, L45: "none of the CMIP5. . ."

AU: Yes. Thank you for the correction. We will apply all of the above suggestions.

P8, L1: do you mean that the CM2.5 runs in the CMIP5 archive are better than other models in the archive? Or do you refer to the runs you analyse in this paper? If this is the case, you should provide a figure to support this statement.

AU: Thank you. We referred to the CMIP5 analysis shown in Kelley et al. (2012). We will clarify this and add the reference where appropriate.

P8, L10-13: when discussing the impact of NAO and SNAO on European climate, add references.

AU: We will add the references.

P8, L17: "and rather wet conditions".

AU: Thank you, we will apply the correction.

P8, L18-19: add references on future projections of SNAO.

AU: We will add the references.

Section 3.2: the objective is to test the capability of the model in simulating the SNAO as an independent internally-generated mode of climate variability. However, the long introduction at P8-9 does not actually help in understanding why this is necessary. Is the internal variability modulated at multidecadal time scales? Is this modulation externally forced? Please try to clarify motivations and objectives of the section.

AU: As stated in the introduction of section 3.2, the purpose is to analyze the capability of the CM2.5 model to simulate the SNAO as an independent, internally generated climate component, which would prove the physical validity of the statistically-derived component. Yes, we agree that the introduction of section 3.2 is too long. Following one of the previous comments, we will move part of the information to section 1 and clarify the main purpose of section 3.2.

P8, L20-26: this paragraph is confusing: on the one hand, it is true that different approaches/datasets may lead to uncertainty in the observed SNAO-Mediterranean teleconnection; on the other hand, uncertainties in model simulations originate from model shortcomings. Therefore uncertainty in the real and model worlds could originate from both intrinsic non-linear nature of the phenomenon and inadequate statistical/modelling tools. Please rephrase.
AU: Thank you, we will clarify the paragraph.

P9, L8-9: I cannot understand why and how anthropogenic forcing should intensify SNAO contribution (to the summer atmospheric circulation over North Atlantic). Please explain.
AU: We will make an effort to elaborate more on this issue.

P9, L34: add the figures for July and August to the Supplement.
AU: Thank you, we will take this comment into consideration.

P9, L35: what is the interest of comparing with the HadCM3 model?
AU: This section analyzes the capability of CM2.5 in simulating SNAO and compares it with the available results of other models, in this case HadCM3 and HadGEM1 in Folland et al. 2009.

P9, L42: is it HadGEM1 or HadCM3?
AU: The statement is correct.

P13, L6-7: why an East Mediterranean index is used to compute correlation in Figure 8d, instead of the first EOF for NCEP omega?
AU: Figure 8d shows the correlations computed based on three-decade time series of NCEP omega at the mid-atmospheric level. Taking into account the relatively short length of the data set (compared to 1000 years CTRL run) and a relatively smaller plausibility of the data at the middle and higher atmospheric levels, we refrain from applying an EOF analysis to the NCEP dataset. In our consideration, applying an EOF analysis to such a short time series could lead to degeneracy of the derived eigenvalues. In other words, the EOF mode derived from the NCEP dataset could be easily a spurious combination of several modes, rater than a realistic representation of the SNAO mode. Therefore for computing correlations using such a short data set (Figure 8d), we prefer to use full time series rather than time series of computed EOF.

P14, L25-26: what do you mean with "estimated at the original model resolution"? Do you mean "computed"?
AU: **Yes, Thank you for the correction.**
P14, L32: east.
P17, L1-3: I don't understand why you refer to Fig. 10a (showing end-of-century projections) to discuss changes in SNAO. You could maybe use this figure to support your analysis of future SNAO.
AU: We are using this figure to support the analysis of future SNAO, but also the interpretation of the analysis of the already observed SNAO changes. The fingerprint of the future changes, derived from the sea level pressure projections, is consistent with the observed evolution of SNAO and thus it may constitute a possible contribution of the anthropogenic component already observed in the 20th century. We will try to clarify this issue in the manuscript.

P17, L14-16: it would be preferable to present the regression method to estimate the SNAO impact here rather than in the Method section.
P17, L43: “warming is lower over . . . than . . .”
AU: Thank you, we will take both of the comments above into consideration.
P18, L7-12: it is not clear to me whether you are discussing you results (in Figure 10) or previous findings. If you discuss your results, please add more references to Figure 10, otherwise add a reference to a paper.
AU: Yes, we will follow the advice.
P20, L39-41 and 42-45: please add references.
AU: Thank you, we will add the references.
P21, L20: “preindustrial value”.
P22, L9-10: please add a citation to CMIP5 results.
AU: Thank you, we will follow the advice.

Figures: for better comparison, figures presenting climate change in the Mediterranean should share the same geographical boundaries. Same recommendation for figures presenting SNAO and Asian monsoon teleconnections, respectively.
Figure 6: what do contours represent? The sign looks reversed with respect to the standard SNAO pattern. Could you please fix this, not to mislead the reader?
Figure 7: does it make sense to project the SNAO index derived from 20CR onto NCEP data? Why not just analyse one dataset?
AU: We agree with the reviewer that it is usually easier to use just one data set. However for the sake of consistency with an earlier part of the analysis, we used the 20CR dataset instead of the NCEP dataset. In the earlier part of the analysis we used the 20CR dataset, because the alternative ones, such as NCEP-NCAR1 or NCEP –DOE, would be too short for the analysis of the evolution of SNAO during the 20th century.

Figure 8: Do you perform EOF on omega 500 and 300 together? Or is EOF analysis performed separately on omega 500 and 300? If this is the case, which time series do you use for correlations?
AU: Yes, the EOF analysis is performed separately for each level. The method is described in the manuscript but we will clarify and highlight this information.

Figure 10: wind is displayed at which level? Is not model resolution 0.5?
AU: Thank you for the correction. We will also add the necessary information regarding the level.

Figure 11: in panels d and e you show regressions, while in Fig. 9c you show correlations.
Figure 12: is omega at 200 or 500?
See P18, L23. Supplement: please follow the logical order of the paper to number the figures. Also please write complete captions, avoiding to refer to captions in the main text.
AU: Thank you for all the comments and advice regarding the figures. We will make an effort to improve the quality of the figures and we will adjust and correct the respective captions.