Authors' response to anonymous referees #1 and #2

We thank the two anonymous reviewers for taking the time to read and comment on our manuscript. Below we provide detailed comments to the reviewers' questions and suggestions, and describe the improvements we made for the revised manuscript.

Response to Anonymous Referee #1

The study by Pallant et al. aims to investigate the interactions between microbial depolymerization and climate change, specifically the influence of temperature and moisture on SOC decomposition and the differential response of soil layers to warming and drought, using the JSM model. Despite the study's promising title and significant potential contributions to future SOC research, it falls short in several key areas, preventing it from being a comprehensive study. In its present state, it seems incomplete, suitable at best for a sensitivity analysis within a broader, more thorough investigation. I recommend withholding publication until substantial improvements are made.

We apologise for the apparent mismatch between the title and the content, and adjusted the manuscript's title. Additionally, the updated manuscript includes clearer descriptions of the study's objectives, novelty and scope, in line with our response to the comments and suggestions raised by anonymous reviewer #2. We realise that many of the issues raised by the reviewer below were caused by an incomplete description of some of the model processes, where we assumed that in-text references to previously published manuscripts would suffice. We apologise for this confusion and made substantial changes to the text: The methodology section was revised, in particular with regard to our choice of model parameter values as well as the vertical process representation in JSM and the SOC decomposition processes influenced by soil moisture and temperature along a vertical gradient. We also rewrote the introduction to better outline the study's scope and objectives. Overall, we made these changes to increase the comprehensiveness of the study, and improve the manuscript's readability and understandability to the journal's readers.

General comments:

The title's implication of a drought-focused study misaligns with the content, which predominantly consists of sensitivity tests devoid of any empirical assessment. The absence of observational data reduces the study to hypothetical model outputs, with an overemphasis on parameter sensitivity to temperature and moisture.

We regret the apparent mismatch between the title and the content, and adjusted the title to "Modelling the impact of climate-substrate interactions on soil organic matter decomposition with the Jena Soil Model" so that it better reflects the paper's scope.

This is indeed a modelling study with a strong focus on the sensitivities of depolymerisation rates to changes in temperature and soil moisture changes. These are processes that have partially been overlooked in dynamic modelling studies before, in particular the soil moisture impact. In this work, we investigate the effect of these temperature and moisture sensitivities parameterised based on data from a lab study by Allison et al. (2018). While JSM is a relatively new model, the processes and parameters for JSM and its predecessor

COMISSION have been successfully tested against observations for various applications by Yu et al. (2020, 2023), Ahrens et al. (2015, 2020), and Fleischer et al. (in prep, but see https://meetingorganizer.copernicus.org/EGU22/EGU22-11276.html). Making frequent reference to these existing studies, we did not conduct additional comparisons between model outputs and observational data in this study. Our main reason to do so is a lack of suitable data: e.g. available respiration data would only cover the first few years of the simulations for the experiments, and SOC measurements from warming/drought experiments would be highly impacted by changes in plant productivity (litter inputs). A discussion of this study limitation was already part of the original manuscript at the end of section 4.4, which was further extended by mentioning the competition between plants and microbes for resources, through e.g. changes over time in plant litter inputs and microbial CUE.

This study also overlooks other crucial dynamics in SOC, such as diffusion and advection, which could significantly influence SOC distribution within soil columns and thus its response to soil temperature and moisture changes. The omission of factors like soil texture and vegetation dynamics further limits the study's scope.

We disagree on that point: JSM as it was applied in this study already includes the above-mentioned processes, which are described in Ahrens et al. (2015, 2020) and Yu et al. (2020). Diffusion, advection, and soil texture are all considered by the model. Soil texture influences soil moisture and water fluxes but also the sorption of DOM and microbial residues via the maximum sorption capacity, Qmax. For the sake of brevity, these are not described in detail in this manuscript, but we mention them in the methodology section of the updated manuscript with the appropriate references to their process description with equations. The aim of our study is to investigate in detail the effects of interacting temperature and moisture sensitivities in a dynamic Michaelis-Menten term. To our knowledge, this has not been done before in a comprehensive manner and this warrants an in-depth modelling study. This novel aspect of our study is better highlighted in the thoroughly revised introduction of the updated manuscript.

To fit the objectives for this study, we made the conscious choice to not have an active response of the vegetation to moisture and temperature changes and use constant litter forcing over time, to be able to pinpoint the changes in modelled SOC dynamics in response to the climatic drivers in isolation from those driven by changes in litter inputs.

The introduction should be revised to avoid methodological details and instead provide a comprehensive overview of the factors controlling SOC dynamics. Additionally, the methodology does not adequately discuss SOC discretization or the interplay between the topsoil and subsoil layers.

We feel that a comprehensive overview of the factors controlling SOC dynamics falls beyond the scope of the introduction, since we want to focus on the interplay of soil temperature and soil moisture sensitivities in a microbial-mineral model at different soil depths. We discuss the various processes that are regulated by temperature and moisture, with a strong emphasis on microbial depolymerisation rates and reverse Michaelis-Menten kinetics. The introduction now specifically covers the different temperature and moisture sensitivities of microbial decomposition processes, the role of organo-mineral interactions, and *depth-specific processes. We refer to previously published work with JSM for other controlling factors of SOC decomposition that are beyond the temperature and moisture sensitivity scope of this manuscript.*

We very much agree with the reviewer that the methodology section should have included a more thorough description of the interplay between top and subsoil layers. We added this information and renamed Section 2.2 to "Vertical process representation in the Jena Soil Model". Additionally, the descriptions of SOC advection and diffusion mentioned in the previous comment were included in the methodology.

I have confined my comments to the methodology section because the paper's foundation needs strengthening before the results and discussion can be meaningful.

We thoroughly revised the introduction and methodology sections to incorporate the above comments to improve the paper's foundation, especially with regard to the scope of this study and the JSM model process descriptions.

Specific comments:

L20: Clarify the term "long-term" to provide proper context.

We changed to "We find that soil warming leads to SOC losses at a timescale of a century"

L20: Define "SOC-specific Q10" to elucidate its relevance to the study.

We agree, and the updated abstract no longer contains this text. Instead, we use a more general description, referring to specific Q10Km values associated with different enzymes for the breakdown of litter or microbial residues. These two Q10 values (below 1 (0.7), and above 1 (1.3)) have opposing effects on decomposition and interact with the amount of microbial biomass via the Michaelis-Menten term. This introduced a different sensitivity between topsoil and subsoil as microbial biomass decreases with the amount of root litter inputs and advective and diffusive transport inputs which decrease with depth.

L21: Distinguish between "reduce" and "accelerated" processes to prevent ambiguity.

The revised abstract no longer contains this sentence. Instead, we write "While absolute SOC losses were highest in the topsoil, we found that the temperature and moisture sensitivities of Km were important drivers for SOC losses in the subsoil – where microbial biomass is low and mineral-associated OC is high."

L23: Replace "SOC gain" with "SOC accumulation" to accurately reflect changes in SOC cycling during different environmental conditions.

We agree and rephrased to "SOC accumulation" in the whole manuscript.

L24: When using the term "decomposition rate," additional details should be provided for clarity. It might be preferable to use "SOC loss" for simplicity and better understanding.

We rephrased to SOC balance instead of SOC loss, since it can be both a SOC gain or loss.

L25-26: You need to be clearer. Maintain consistency in the comparison of temperature and moisture effects on both topsoil and subsoil.

We rephrased to "In this study, we show that while absolute SOC changes driven by soil warming and drought are highest in the topsoil, SOC in the subsoil is more sensitive to warming and drought due to the intricate interplay between K_m, temperature, soil moisture, and mineral-associated SOC"

L28-29: These assertions require supporting references for validation

We added the missing references and included Fan et al. (2020), Crowther et al. (2019) in the first part, as well as Davidson (2020) and Kirschbaum (2006) in the second part.

L34-35: The phrase "most important" is subjective and should be reworded to reflect that soil temperature and moisture are significant controlling factors. This section would benefit from an expanded discussion on the key factors that regulate soil dynamics.

The wording might be perceived as subjective and we rephrased to "Soil temperature and soil moisture are two primary controlling factors of microbial decomposition rates, and thereby the carbon turnover rate of soils (Davidson and Janssens, 2006; Moyano et al., 2013; Yan et al., 2018)*."*

Additionally, we put higher emphasis on the importance of SOC stabilisation on mineral surfaces as another important factor determining the fate of SOC stocks (Ahrens et al., 2020; Dwivedi et al., 2017; Sokol et al., 2022), and discuss the differences between top and subsoil layers. Overall, the revised introduction discusses the most important factors that fall within the scope of this study.

L35: Clarify the term "de(stabilisation)" for better understanding.

This sentence is no longer part of the revised introduction.

L37-39: Reformulate these lines for enhanced clarity and readability.

We apologise for unclear wording and rephrased to "Given the importance of SOC stocks and their sensitivities to climate change, a better understanding and representation of these complex interactions in coupled C cycle-climate models is extremely important for a better understanding of the carbon-climate feedback (Todd-Brown et al., 2014)."

L41: Specify the models referred to in this line.

This sentence is no longer part of the revised introduction.

L43-80: Shift the detailed methodological exposition to the methods section, using the introduction to provide a broader overview.

We thoroughly revised this part of the introduction and eliminated the equations from the text to make it more understandable and less technical.To our knowledge, previous works have primarily focussed on the temperature sensitivities of Vmax (or the apparent sensitivities of (heterotrophic) respiration rates). The novelty of our study is the focus on the temperature *and moisture sensitivities on depolymerisation rates through the half-saturation constant (Km), which to our knowledge has not been shown in a modelling study before. Reverse Michaelis-Menten kinetics (compared to forward kinetics) is less commonly used in models, and may be less known to the readers of Biogeosciences. Therefore, we think the conceptual figure and text help to clarify why microbial depolymerisation rates can both increase or decrease in response to soil warming and drought/soil wetting through Km, especially at low microbial biomass concentrations.*

L83: A reference is missing and should be included.

This sentence is no longer part of the revised introduction.

L88: What about peatlands? Inclusion or exclusion of them would significantly change your findings. It is important to note that the moisture function, decomposition term, and vertical dynamics of SOC distinctly vary in wetland environments.

This sentence is no longer part of the revised introduction. But we agree with the reviewer that this is definitely true for peatlands, and the traditional empirical model approaches we refer to in the introduction also don't describe SOC-moisture-temperature dynamics in these ecosystems very well (or even do consider them at all, see e.g. Chadburn et al. (2022)). Our results are for a mineral soil profile, so we feel that explicitly discussing peatlands in the introduction falls outside of the scope. Peatlands and their different moisture dynamics, however, are briefly discussed at the end of Section 4.4.

L95-99: Avoid repeating information from the methods section to streamline the text.

We removed the redundant information on JSM.

L113: Incorporate the previously mentioned introductory information here for coherence.

As mentioned in the earlier comments above, we included additional information for JSM on the processes of diffusion, advection, and soil texture. We made textual edits to the introduction and methodology sections, and updated Fig. 2 and its caption (see comment below on L116).

L115: Briefly explain each parameter, including those in Table 1, and cite the source of the formulations. Additionally, explain the rational for such formulation for DOC?

The parameters and constants from Table 1 are all explained in the text, with references in the text and Table 1.

The formulation of Eq. 1 describes the depolymerisation of litter and microbes to DOC. Hence, it is not a formulation for DOC. JSM is based on the COMISSION model, so we included the references to this model by adding Ahrens et al. (2015, 2020). The reverse Michaelis-Menten kinetics formulation for depolymerisation is based on the derivations by Tang and Riley (2019) that we mentioned in the previous sentence. We apologise for the unclear language and rephrased to "The depolymerisation rate (mmol C m^{-3} $hour^{-1}$) of litter or microbial residues into the dissolved organic carbon (DOC) pool (following Ahrens et al., 2015, 2020) is described as:"

Eq1: For this and all subsequent equations, it is necessary to specify the units of the calculated flux. Additionally, clarify if any conversions were performed to derive these units from the parameters listed in Table 1.

We added the units of all fluxes (Eqs. 1 and 5) and apologise for some small typesetting mistakes we found and corrected in the superscript of the units in Table 1. The temperature conversions of $Q_{10,Km,P}$ and $Q_{10,Km,R}$ (Allison et al., 2018) from the lab study's reference temperature of 16 °C to JSM's reference temperature of 20 °C is described in the paper using Eq. 3, and separately marked in Table 1 with an asterisk. No other conversions were made. The model itself runs on a half-hourly timestep and all units in the model code are defined uniformly using SI units (mol, K, seconds). For the sake of clarity (as these e.g. maximum depolymerisation rates or microbial growth rates are commonly measured in other time units (e.g. days, hours) or concentrations (mol, or mmol per soil volume)), and easy cross-referencing, however, we chose to report the values in Table 1 as they were presented in the original model description papers by Ahrens et al., 2020 and Yu et al., 2020. We added this information to the caption below Table 1.

L116: Although the layered structure of the soil has been mentioned and is depicted in Figure 2, there is a noticeable absence of any depth indicators in the equations or in the estimations provided.

We added this clarification in the methods section in section 2.2 "Vertical process representation in JSM", to explain that all C pools and forcings are functions of time and depth. In addition, the caption of Fig. 2 was changed so that it clearly mentions that all soil layers (the grey cylinders) contain all C pools (black boxes) and C fluxes (black arrows).

Figure 2: The figure needs to be revised for clarity, including an explanation of the brown arrows and a clear differentiation between soil pools and depth indicators.

The brown arrows represent the above and belowground inputs. We revised Fig. 2 and separated these labels into 'aboveground litter' and 'root litter', and moved the label for the belowground arrows (root litter inputs) to the left. Every model pool is present in each soil depth. We clarify this in the updated caption of Fig. 2 (see previous comment).

Table 1: Complete the table by labelling parameters that lack units as "unitless." Additionally, is there a reference for the parameter R that can be provided?

R, the universal gas constant, is not a parameter but a well-known natural constant, as mentioned under Eq. 3, L.173. Since the 2019 redefinition of SI base units, it has an exact numerical value with zero uncertainty, and we report the universal gas constant in J K^{-1} mol^{-1}, adhering to the journal's use of standard SI units. A reference would be https://en.wikipedia.org/wiki/Gas_constant. Because R is such a well-known constant, we removed it from Table 1 to avoid further confusion. We added '–' to Table 1 for unitless variables where applicable, and also updated the Table heading from "Parameter" to "Parameter/constant" for clarity.

L145: The nutrient dynamics have a significant influence on uptake and other soil processes. While you state that the model accounts for nutrients, there's a missed opportunity to thoroughly explore and discuss this aspect within the current study.

We focus on one novel aspect in this study: the interaction of soil moisture and soil temperature sensitivity of a dynamic Michaelis-Menten term. Our focus on the C cycle with the temperature sensitivities of different litter types, as well as the various warming and drought experiments already reveals a large range of complex interactions between microbes, organo-mineral interactions, and climatic variables within this dynamic modelling system. Extending this work by including nutrient dynamics and its temperature and moisture sensitivity leads to a path far outside the domain covered by our study's model approach. At the end of the discussion of the updated version of this manuscript we now mention the inclusion of nutrient dynamics among the future research directions with novel modelling frameworks such as JSM.

L147: Consideration of root distribution across various soil depths is missing. An analysis of how roots are spread throughout the soil profile could offer valuable insights into soil dynamics.

We apologise for not clearly communicating that we consider root distribution across various root depths. Figure 2 and sections 2.1 and 2.2 of the manuscript were revised accordingly. The root distribution is the main factor for different microbial biomass levels in different depths (Ahrens et al., 2020). We included this important information and its reference in section 2.2 of the revised manuscript.

L153: Include the sorption/desorption equation to complete the methodological description.

Sorption and desorption are clearly described in Ahrens et al. (2015, 2020), which we refer to in L.187-191 and L.207 - 212 of the updated manuscript. While checking this sentence we found and corrected two additional small mistakes:
1) In the description of the activation energies, all Q10 values used in this study are based on literature, and not calibrated.
2) Lines 155 – 158 of the original manuscript are the Caption of Table 1. We corrected this formatting error and moved this text back below Table 1.

L184: What do you mean by first model run? Explain the rationale behind the first model run and the selection of a 100-year period.

We rephrased this section by describing this model run after the spin-up as the ambient model run, to be consistent with the rest of the manuscript. The selection of a 100-year simulation period was done to allow the different SOC pools to return to steady state after the respective disturbances (warming, drought) so that the changes in SOC stocks between the start and the end of the experiments could be calculated (Table 2).

L185: The decision to focus solely on the 0-50 cm soil layer raises questions. Should processes like advection or diffusion be considered, the entire soil column to its maximum depth would likely experience changes during the spin-up period. Why then, is the analysis limited to this specific depth range?

The processes of advection of DOC and diffusion to represent bioturbation are considered in JSM. We added this information to section 2.1 (L.135 - 136). In JSM, there are 15 soil layers, of which 6 layers between 0 and 50 cm depth, and we made this more clear in the methodology section. These 6 layers experience the strongest influence of transport. Soil *layer thickness increases with increasing soil depth, at the following depth intervals: 0 – 6, 6 – 13, 13 – 20, 20 – 26, 26 – 36, and 36 – 50 cm. So we did not focus on only one layer between 0 – 50 cm, but compared the processes in the vertical profile setting with multiple soil layers. By comparing a shallow and a deeper layer, we were able to look at the differences along a gradient of microbial activity – the majority of microbial activity and the resulting heterotrophic respiration flux takes place in the upper soil layers. Additionally, the difference between the shallow and deeper layer illustrate the effect of decreasing root litter inputs with depth.*

JSM also provides model outputs below 50 cm, which were checked but not shown in the manuscript. These deeper soil layers indeed take many years to reach equilibrium, as the reviewer points out. So another, more practical reason for choosing to focus on the layers until 50 cm depth is that this significantly reduces the necessary model spin-up time and thereby the use of computational resources. Despite these layers not being in steady state after spinup period, we did check the patterns in the layers between 50 and 100 cm depth and found they are similar to what we show in the manuscript. We also found that for the drought and warming experiments, these deeper layers needed more than 100 simulation years to reach a new steady state after disturbance, but they did eventually reach it.

The model's vertical process representation and inputs are described more clearly in the methodology section of the revised manuscript (Section 2.2). We also added the description of the model's steady state check after the spinup period using linear regression.

L186: Detail the approach used to assess steady-state conditions.

We used a linear regression model to check the steady state assumptions and mention this in the methods of the revised manuscript. It is also visually very clear from the model results, e.g. in Figure 3.

L189: Clarification is needed on the term "all ambient soil temperature." Does this refer to temperatures at every soil depth? Moreover, to isolate the impact of temperature alone, it would be necessary to perform simulations with varying temperature and moisture, and then compare these to simulations with only temperature variation to accurately determine temperature sensitivity.

We apologise for unclear wording. Temperatures are depth-specific, which we better clarified in the methods section (2.2). We have improved the description of how the depth-specific litter, temperature and soil moisture data were generated in section 2.2, which now explicitly states: "The soil moisture and soil temperature forcing data from 2000 - 2012 are referred to as ambient soil moisture and soil temperature data from this point."

These depth-specific temperatures were increased by 4.5 K for each depth. Similarly, the depth-specific moisture values were decreased by 10, 20 or 30% in the different drought experiments. We have improved the description of the different warming and drought experiments in section 2.4.

We did consider the isolated effects of soil warming or drought by including model experiments where either temperature Fig. 3, blue line) or soil moisture (Figs. 3 & 4, purple line; Fig. 5, yellow line) was kept at its original, depth-specific input values.

L194-196: Justify the chosen experimental values.

The full justification for these values was provided in the original manuscript in lines 166-183, under a separate section of the manuscript titled "2.3 Choice of Q10,Km values for polymeric litter and microbial residues''. In short, the values are based on a lab study on the temperature sensitivities for different enzymes involved in the breakdown of soil carbon substrates (Allison et al., 2018). The Q10,Km value for the depolymerisation of litter was based on measurements for the enzymes β-xylosidase and total oxidase, and the Q10,Km value for the depolymerisation of microbial residues was based on measurements for the enzyme leucine aminopeptidase.

L201: What was the rational for 10% changes and why only between 0.9-0.7 values?

We apologise for not clarifying this in the methodology section. Part of the rationale was somewhat hidden in the introduction, and we now clearly state the rationale in Section 2.4 of the revised manuscript. We used the following rationale (L. 258 - 267): "Soil drying is expected for most of the globe (Wang et al., 2022b and references therein), but drought intensity is uncertain and may vary locally (Cook et al., 2020; Hsu and Dirmeyer, 2023). Soil moisture change projections are very uncertain: 60 projected global lateral and vertical distributions of future soil moisture were highly diverse both in their predicted spatial and vertical distributions (Berg et al., 2017). The multi-model mean of this study showed reductions in subsurface and deep surface soil moisture up to 30% by the year 2100. Given the large divergence between these model projections, we chose to simulate drought by reducing the depth-specific soil moisture values from the forcing dataset in steps of 10%, to be able to compare the effects of a relatively mild versus increasingly stronger droughts on SOC stocks."

L210: Confirm whether equilibrium was reached after 100 spins and discuss mass balance and steady-state conditions for each run.

We used a linear regression model to check the steady state assumptions and expand the text describing the steady-state check in the methodology (section 2.4). Mass balance checks are performed in the QUINCY-JSM model code, and for every run we have log files showing that the mass balance is closed. Furthermore, the QUINCY-JSM model is part of the EucFACE-MIP data-model comparison project, and a reference to the detailed descriptions of the mass balance checks for C, N and P are recently published in Jiang et al. 2024).

L216: The mention of R packages is less critical than a thorough analysis of the outputs. Consider focusing on the methods to output evaluation here.

We expanded this section with a short description of the smoothed curve shown in Figures 3-5 which visually supports the data points plotted for the 100 simulation years. We strongly feel that the mentioning of R packages with citations are important and should remain part of the manuscript, as we are grateful to the many (scientific) open source software developers for their important contributions, and acknowledge this by crediting their work with citations.

Response to Anonymous Referee #2

This paper conducts a series of model experiments using the JSM soil model to explore the response of modeled SOC to warming and drought, with specific focus to depolymerization terms and their temperature sensitivity.

To accomplish this, the authors impose several general warming and drought experiments. I think it would be useful to develop the model experiments following from a specific objective. For example, if the authors want to know how soils are likely to change with climate change in a given location, they may apply one or two warming scenarios using forcings derived from the Shared Socioeconomic Pathways (running the temperature and/or moisture changes over time rather than stepping up by a global average) at the site in Germany. While it is much more work to extract the appropriate forcing information, it gives a much more specific sense of what the model expects, and the potential to evaluate predictions.

There may be other objectives, such as to conduct a sensitivity analysis of JSM model parameters. With this objective, the authors could do a literature survey of possible parameter ranges and explore the SOC response space given systematic combinations of parameter values. Some attention needs to be given to the parameters chosen for analysis. Why were they chosen and not others? What physical or chemical significance do they have?

We thank reviewer #2 for their comments on our manuscript and helpful suggestions. We apologise for the unclear formulation of the scope and objectives for our study, and aimed to improve their descriptions, especially in the introduction. Specifically, our study focuses on modelling the sensitivities of depolymerisation rates to changes in temperature and soil moisture changes. These are processes that have partially been overlooked in dynamic modelling studies before. In this work, we aim to study the effect of these temperature and moisture sensitivities parameterised based on data from a lab study by Allison et al. (2018).

For our model study, we do make use of scenario based estimates for climate change (4.5 degree warming by the year 2100 from RCP8.5, Soong et al. 2020). While this is different from the by the reviewer suggested warming scenario with gradual temperature increases over a 100 year period, our study does provide insight into the possible direction and magnitudes of change in SOC stocks following a temperature increase. Future soil moisture projections are highly variable in time, space, and very climate model-dependent (Berg et al., 2017), so that selecting one specific forcing dataset for our example site would not provide much new insights beyond what the existing forcing data and drought scenarios provide.

The choice of model parameters has been based on field and lab-based literature, and we describe this more clearly now in the methodology section of the revised paper. Several of these parameters have been tried and tested in the previous model studies by Yu et al. (2020, 2023) and Ahrens et al., (2015, 2020) and the ones that are newly introduced for this study (based on lab results from Allison et al. (2018)) received their own paragraph (2.3) in the methods section. The methodology was revised to better convey this to the readers.

To understand how well JSM models the SOC response to temperature or soil moisture, authors could compare model output to data.

Given the long timeline of the simulations (100 years) and the novel focus of our study on the sensitivities of microbial processes to temperature and drought at different soil depths, we on the one hand see that such data are not readily available and therefore a comparison of the model outputs to observational data falls outside the scope of the study. In line with our reply to reviewer#1, we argue that on the other hand, available respiration data, for example, would only match the first few years of the simulations for the experiments and allow us to verify the bulk flux from the complete soil profile under 'ambient' climatic conditions (and not match the various the warming/drought experiments), and SOC measurements from warming/drought field experiments would be highly impacted by changes in plant productivity (litter inputs). A discussion of this study limitation is already part of the original manuscript. Please note that while JSM is a relatively new model, the processes and parameters for JSM and its predecessor COMISSION have been successfully tested against observations for various applications by Yu et al. (2020, 2023), Ahrens et al. (2015, 2020), and Fleischer et al. (in prep, but see https://meetingorganizer.copernicus.org/EGU22/EGU22-11276.html).

There are many options and I think this study could be quite interesting if expanded to fit into one of these frameworks, or a different one, as long as there is some clearer justification for the chosen experiments.

The kindly suggested options by the reviewer, as well as the comments by reviewer #1, indicate we were not successfully relaying the intended scope and objectives for our study in the original manuscript. We apologise for the unclear language and thoroughly revised the introduction to make them more clear, alongside a better description in the methodology section explaining why these parameter values for the temperature sensitivities of the depolymerisation rates for litter and microbial residues were chosen from the lab-based study by Allison et al. (2018).

L46: I think there needs to be more description about what Km means in physical terms, and why it is important. As written, it seems a bit arbitrary to explore the temperature sensitive of a fairly abstract parameter in the kinetics equation and not the other sources of temperature sensitivity in the model.

The half-saturation constant, Km, is not an abstract parameter at all, but describes the substrate binding affinity of enzymes to different substrates for microbial depolymerisation, i.e. plant litter or microbial residues (Tang and Riley, 2019). We agree with the reviewer that this was not explained well in the introduction, and have included a better description of Km in the revised introduction.

*Km is also the concentration (in this case of microbial biomass, Cb) where the depolymerisation rate is 0.5*Vmax (Fig. 1). Therefore, it is an important determinant in the reaction rates of enzyme kinetics: A low half-saturation constant value would mean that the reaction rate is only limited at very low microbial biomass concentrations (e.g. in the subsoil). A high value means that the reaction rate will only be unlimited at very high microbial biomass concentrations (e.g. in the topsoil).*

The value of the half-saturation constant itself is sensitive to temperature and soil moisture and thereby has the potential of further accelerating or counteracting SOC decomposition rates in a warming climate (e.g. see Allison et al., 2018; Davidson and Janssens, 2006;

Davidson et al., 2012). To our knowledge, this has not been explored in a dynamic modelling setting before, which makes this study very novel. We have put higher emphasis on this in the completely revised introduction.

Other temperature sensitivities, through the maximum reaction rates (Vmax) of the microbial depolymerisation and uptake rates, as well as the sorption and desorption rates, are also active in our study - these maximum reaction rates are also affected by 4.5 degree warming through their respective Q10 values (listed in Table 1). These temperature sensitivities, however, are more well-established in the microbial-mineral SOC model literature (Wang et al., 2012, 2013) than the temperature sensitivities of the Km which were, with the interplay with soil moisture sensitivity, the focus of this study. These warming effects on SOC decomposition are presented and discussed at length in sections 3.1 and 4.1 of the manuscript.

L193: I found the choices of Q10 parameter experiment a little confusing. Q10=1 and Q10=literature values makes sense, but I didn't quite understand the hypothesis underlying the choice to set both parameters to either litter or residue.

We apologise for this confusion and expanded the motivation for this choice in sections 2.3 and 2.4 of the methodology, where we refer to the lab-based measurements of Allison et al. (2018). Setting these values to either all litter or all microbial residues was done to showcase the effects of an overall Q10,KM of 1.3 and a Q10,Km of 0.7. Setting both parameters to either litter or residue was hence not based on a mechanistic hypothesis but rather as an exploration of the edge cases of all Q10,KM below 1 and all Q10,Km above 1. The effects of temperature sensitivity of Km have not been investigated in a dynamic modeling study before, and also the use of a negative temperature sensitivity of Km has not been investigated before, which is why we explore these different feedback directions in the paper. In the methods section, we have made clearer that this was intended as a sensitivity study and model experiment rather than testing a specific hypothesis.

L223: Rather than visual inspection to determine steady state, you could set some quantitative measure such as <1% change in stock (or a moving average) over the last 100 years.

We used linear regression to determine the steady-state assumptions and now mention this in the methods of the updated manuscript.

L230: Sulman et al. 2018 (https://link.springer.com/article/10.1007/s10533-018-0509-z) demonstrated that different soil C models had widely different assumed temperature sensitivities of mineral associated carbon. Can you make a compelling case that MAOM is less temperature sensitive than microbial processes?

Yes, in a review article on uncertainty in soil C feedbacks, Bradford et al. (2016) recognise the important role that microbes play in the stabilisation and formation of stabilised SOC, which is less sensitive to warming (Fig. 3 in Bradford et al. (2016), and see Tang and Riley (2015)). We discuss this in Section 4.1, and included these references in the revised manuscript.

The five models analysed in Sulman et al. (2018) contain four models that include microbially mediated decomposition rates (CORPSE, MIMICS, RESOM and MEND), of which only one (the MEND model) has non-linear representations of mineral SOC protection, comparable to JSM, which can be decomposed by microbes (not possible in RESOM). Therefore, the widely-ranged values reported for the other four models reflect the differences in model structure, as each value must be somehow calibrated to fit inside its specific model framework, rather than reflect values which are process-based as is the case in the MEND model and JSM.

In our study, we make use of the values reported by Wang et al. (2013) for the temperature sensitivity of sorption and desorption parameters $Q_{10,adsorption}$ and $Q_{10,desorption}$ (Table 1). In this study (for the MEND model), Wang et al. (2013) developed and tested parameter values for explicitly modelling the desorption and adsorption of SOC to mineral surfaces based on literature (reported in Table 3 of Wang et al., 2013). An application for JSM (with its predecessor model, COMISSION) has been successfully reported by Ahrens et al. (2020). We updated the description of sorption and desorption and its temperature sensitivity in the methodology section 2.1, so that it clearly reflects that these parameters are literature-based and have been successfully applied in earlier versions of this model for studies that focused on organo-mineral interactions (Ahrens et al., 2020).

L267: Define here which pools you consider to be in POC vs MAOC. I think this may be the first occurrence of the abbreviation so you should define the terms as well.

We agree and apologise for the very late definition in the original manuscript. We improved this by introducing the abbreviations, and which C pools are considered for MAOC and POC in section 2.1 of the methods.

Figures 5 seems very similar to Figure 4, and not additive to the effect of temperature shown in Figure 3 – yellow line. Why is that? Does this imply that SOC in JSM is more sensitive to soil moisture than temperature?

The figures indeed look similar, because the effect of drought and warming on SOC stocks through the half-saturation constant, Km, as shown in Fig. 5 is of a much smaller magnitude than the effect of drought + warming (Fig. 4, without temperature sensitivity of Km). Drought rapidly increases the amount of POC in the topsoil as litter inputs accumulate over the simulation period (Fig. A2, 0–6 cm orange and pink lines), which makes the temperature effect on Km very small. The temperature effect through the half-saturation constant can be better observed in the deep soil layer, as Km becomes more important at lower microbial biomass concentrations (Fig. 1, Eq. 1). We added this discussion point to section 4.3 of the revised manuscript (L.490 - 496).

Additionally, we would like to point out that the results shown in Figure 4 also include the effects of soil warming by 4.5 degrees, as is the case in ALL model experiments (but not in the ambient model run, dark blue line Fig. 3). We colour coded the lines in Figs 3 – 5 for easy visual comparison between figures: In Figure 3 and 4, the purple lines are identical, and in Fig. 3 and 5, the yellow lines are identical. We apologise for not clarifying this better in the text and figure captions, and included this important information in the captions of Figs. 3 - 5 and the results section of the revised manuscript.

L334: The temperature sensitivity of adsorption and desorption seems like potentially important parameters.

Yes, they play a role in explaining the overall lower SOC losses from the subsoil in response to soil warming, as the amount of adsorbed carbon is larger in the subsoil (Fig. A1). The MAOC pool is not directly affected by microbial depolymerisation (Fig. 2), but temperature does affect the desorption and adsorption rates between MAOC and the DOC and microbial residues pool. Compared to the temperature sensitivities of the maximum depolymerisation rates of litter/residues (Q_{10} of 2.16) and microbial uptake (Q_{10} 1.98)), the temperature sensitivities of adsorption (Q_{10} of 1.08) and desorption (Q_{10} of 1.34) are very low. These literature-based Q_{10} values reflect the current scientific understanding that MAOM is less temperature sensitive than POC (Bradford et al. 2016, Tang and Riley, 2015). As a result, the lower temperature sensitivities of adsorption and desorption contribute to the overall lower apparent temperature sensitivity we observed for the total SOC pools when the ratio of MAOC:POC is high (i.e., in the subsoil). We discuss this at length in L.397 – 407 of the revised manuscript and added the references to Bradford et al. 2016; Tang and Riley, 2015). In addition, we mention the literature-based temperature sensitivities of sorption and desorption in the methodology section (L.187 - 188).

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