Answers to comments from Reviewer #1

Dear reviewer,

Thank you for reading the manuscript and for providing valuable feedback and constructive comments that helped to improve our manuscript. In the following, we address each of your comments:

1. Neither soil properties affect plant water available water or soil water holding capacity affect crop drought stress is exactly new finding. I believe the novelty of this study needs to be clearly stated, such as GLAI at farm level can be used as a reliable indicator of crop growth and stress response to guide management.

Thank you for this comment. The novelty of our work is that we demonstrate that satellite derived growth proxies allow to identify drought impacts on crops, and that we can see the impact of soil properties on crop response during drought, at the scale relevant for farmers in their fields. Many remote sensing studies are motivated by the claim that remote sensing can be used to detect stress, but to our knowledge there is yet no study that establishes/ demonstrates the link from differences in satellite derived plant trait proxies to differences in soil properties. Moreover, a quantification of the effects of differences in soil properties on time series of growth under field conditions does – to our knowledge - also not exist yet. We stated the novelty of the study in the revised manuscript more clearly at the end of the introduction (L 77-88), in the discussion (L265-267, 304-306, 341-344) and in the conclusions (351-356).

2. Are there yield data as well?

This is an on-farm study, with the challenge that not all farms have yield data available, so unfortunately we only have yield data for some of the fields. However, in the PhD thesis of the first author (Sjulgård 2024), a strong correlation was found between the peak GLAI and winter wheat yield in the year 2021 when including the fields with yield data available and with additional farm fields in the same region.

> Sjulgård H. 2024. The potential of agricultural management to alleviate extreme weather impacts on Swedish crop production. Acta Universitatis Agriculturae Sueciae. (2024:84). https://doi.org/10.54612/a.107ri2j3pt

3. Heat stress cannot be separated from this setting. Since accumulative temperature is one variable throughout the analysis, I would suggest including in the introduction and expanding in the discussion the impact of heat on wheat growth. Besides, dry years often have high temperatures too, especially that the DMI is associated with both precipitation and temperature.

The year 2018 was unusually dry and warm in Sweden, and the lack of precipitation has been referred to as the main reason for the yield losses (Bakke et al. 2020, Beillouin et al. 2020, SMHI 2018). We added this to the manuscript in lines 114-116. Beillouin et al. 2020 stated about cereal yields in 2018 that “In Northern Europe, no negative impacts of high temperature were observed, possibly because maximum temperature values averaged over JJA rarely exceed 25 °C” (where JJA refers to June, July and August). Moreover, heat stress mostly affect winter wheat negatively during grain formation which was not assessed in our study, and growth is not as much influenced by heat when it is not exceeding the optimum temperature for wheat growth, which is as high as 20 ° to 25 °C (FAO). Therefore, we did not include heat stress in this study. Regarding the DMI, because higher temperatures potentially can cause higher evapotranspiration and in turn more drought stress, we include both temperature and precipitation data in our aridity index DMI.

Bakke SJ, Ionita M, Tallaksen LM. 2020. The 2018 northern European hydrological drought and its drivers in a historical perspective. Hydrol Earth Syst Sci. 24(11):5621–5653. https://doi.org/10.5194/hess-24-5621-2020

Beillouin D, Schauberger B, Bastos A, Ciais P, Makowski D. 2020. Impact of extreme weather conditions on European crop production in 2018. Philos Trans R Soc Lond B Biol Sci. 375(1810):20190510. https://doi.org/10.1098/rstb.2019.0510

SMHI. 2018. Skörd av spannmål, trindsäd och oljeväxter 2018 [Internet]. [accessed 2024 Oct 25]. https://jordbruksverket.se/om-jordbruksverket/jordbruksverkets-officiella-statistik/jordbruksverkets-statistikrapporter/statistik/2022-03-02-skord-av-spannmal-trindsad-och-oljevaxter-2018--preliminara-uppgifter-for-riket

FAO. Wheat growth and physiology - E. Acevedo, P. Silva, H. Silva [Internet]. [accessed 2024 Nov 13]. https://www.fao.org/4/y4011e/y4011e06.htm

4. Please indicate field management (e.g., irrigation, fertilisation especially concerning manure and slurry, organic or conventional concerning herbicides) and cultivar for all sites in both years in M&M if the information is available. Only mentioning in the discussion that "minimised variation in these factors by selecting fields that were managed by the same farmer in 2018 and in 2021" does not address the uncertainties.

Thank you for that comment. We know that all the fields have conventional cropping systems (and not organic), none of the fields was irrigated, and we complemented the description of the field sites by adding this information to the M&M section (L95). While management can affect growth, we do not compare absolute growth values between fields but focus on identifying drought stress within each field by analysing significant year-to-year differences. For comparisons within the same field, as in shown Fig. 4c, we use percentage differences in growth proxies, assuming minimal changes in management practices over years for the same crop and farmer. However, we agree that potential management variations between years could introduce some uncertainty. But we assume that the farmers manage their fields according to best practice. We extended the discussion about the management practices in the revised manuscript, see lines 329-338 and the M&M section (L95-98).

5. I am glad that field management is finally brought up although in very end of the discussion (L318ff). I suggest expanding on the management practices especially that the authors highlighted them to contribute to climate change adaptation, considering there are studies on how management and tillage affect plant drought responses concerning crop performance including growth and yield. Although it is emphasised that commercially operated field studies are important, separating from field experiments and controlled potted experiments, the outcome from field research and potted experiments should not be dismissed. Instead, studies focusing on the difference among wheat cultivars (e.g., Tewolde et al. 2006), which is not directly comparable with the results presented in this study, are discussed. Several relevant studies to consider:

Khan S, Anwar S, Shaobo Y, Gao ZQ, Sun M, Ashraf MY, Ren AX, Yang ZP. 2020. Soil water consumption, water use efficiency and winter wheat production in response to nitrogen fertilizer and tillage. PeerJ 8: e8892.

Li YB, Hou RX, Tao FL. 2020. Interactive effects of different warming levels and tillage managements on winter wheat growth, physiological processes, grain yield and quality in the North China Plain. Agriculture Ecosystems & Environment 295: 106923.

Sun, Q., A. K. Gilgen, R. Wittwer, G. von Arx, M. G. A. van der Heijden, V. H. Klaus, and N. Buchmann (2024), Drought effects on trait space of winter wheat are independent of land management, New Phytol, 243(2), 591-606.

Wittwer, R. A., V. H. Klaus, E. Miranda Oliveira, Q. Sun, Y. Liu, A. K. Gilgen, N. Buchmann, and M. G. A. van der Heijden (2023), Limited capability of organic farming and conservation tillage to enhance agroecosystem resilience to severe drought, Agricultural Systems, 211, 103721.

Thank you for the comment. We agree that pot, greenhouse or field plot studies, are important to understand certain factors, and hence complement on-farm studies like ours. Because most earlier studies which assessed relationships between GLAI and soil properties have been conducted in pot/plot/field experiments, we stated that on-farm research, such as our study, is also important "to capture the heterogeneity of environmental factors in the landscape." (L63-64). We added in the Introduction section a sentence stating that field and potted experiments are also valuable to emphasize the complementary between pot/plot/field research (L 62-63). At the end of the discussion section, we also added a few sentences about the advantages and disadvantages/challenges of on-farm studies (L 339-341).

6. Please carefully check the references. I am not sure whether there are mistakes with citation tools or multiple papers were cited for the wrong reasons. For instance, I did not see LAI or GLAI data in Irfan Ullah et al. 2021and Mroz et al. 2023 so that it is unreasonable to state "some studies found no relationships between timing of peak GLAI and crop yield (Irfan Ullah et al. 2021; Mroz et al. 2023) (L282)". Other instances include "GLAI at the heading stage of winter wheat has been shown to decrease with a high degree of soil compaction (Lipiec et al. 1991) (L56)" - LAI was used in the cited study. Are LAI and GLAI interchangeable? In the referred work, "Soil physical properties and growth of spring barley as related to the degree of compactness of two soils", spring barley rather than winter wheat was studied.

Thanks a lot for carefully reading our manuscript and for noticing this, there was indeed a mistake in the sentence, and it should have been that Irfan Ullah et al. 2021 and Mroz et al. 2023 found no relationships between the timing of heading to wheat yield, and not between the timing of peak GLAI to wheat yield. The timing of the heading stage is estimated around the peak GLAI. However, during our revisions of the discussion section, this sentence was removed from the manuscript.

Thank you also for pointing out a mistake in the reference to Lipiec et al. 1991, who used spring barley in their study. This was correctly referred to in the discussion ("Lipiec et al. (1991) who found decreasing GLAI at the heading stage of spring barley with a higher degree of soil compaction."), but a mistake was made in the introduction where we wrongly wrote winter wheat. We corrected this in the introduction. Green leaf area index is referred to both as GLAI and LAI in many studies, but some studies also include yellow and dead leaves in the LAI measurements. Lipiec et al. 1991 do not state if they only used green leaves or not, but until the start of senescence, the different leaf area index measurements should be very similar.

7. In the last paragraph of discussion 4.1, it reads very confusing as the terms are switching between many relevant but distinguished concepts, such as green (aboveground?) biomass as approximated by GLAI and total crop biomass, biomass and yield, growth rate and crop

performance, timing of peak GLAI and heading stage. Please also consider clarifying plant available water capacity, soil water storing capacity, and soil water retention and avoiding interchanging the terms.

Thank you for this helpful comment. We rewrote the text and now use the same wordings throughout the manuscript in the revised version.

8. Discussing the individual index of growth rate interpreted from GLAI, peak GLAI and timing of peak GLAI in this setting would only make sense after separating from the drought response (e.g., L283ff). You could consider statistical methods such as mixed effects models for such analysis.

Thank you for the suggestion. To do so, we performed multiple linear regressions that included also the average monthly summer DMI for the corresponding year (2018 or 2021) as an explanatory variable (in Tab S3).

Minor points:

9. Certain content in the introduction can be rearranged, such as consider moving "The Sentinel satellites provide a high spatial resolution optical imagery of up to 10 metres… (L63ff)", "Radiative transfer models describe the relationship between leaf and canopy traits and spectral properties of plants using physical principles…" to M&M.

We excluded "The Sentinel satellites provide a high spatial resolution optical imagery of up to 10 metres… (L63ff)" from the introduction, because we already mention the revisit time for our study area in the M&M. We kept "*Radiative transfer models describe the relationship between leaf and canopy traits and spectral properties of plants using physical principles…"* in the introduction, as we think it is good for the reader to already in the introduction understand what radiative transfer models are. However, we connected it better to the previous sentence in the revised version of the manuscript (L 67-70).

10. Please be consistent and use "peak GLAI" rather than only "peak" (e.g., temperature sum at peak GLAI) in text and figure legend.

Thank you for pointing this out, we have changed to a consistent wording.

11. In Fig. 2, please explain the dashed line in the caption of legend.

The dashed line shows the plateau of the linear plateau model. We now explained it in the figure caption in the revised manuscript (new Fig. 3).

12. I wonder if Fig. 5 is essential to keep in the main text. It only serves to show one value. Consider move it to the supplement.

Thank you for the suggestion, we agree that it would fit better in the supplements. As suggested earlier, we also made it into a table with multiple linear regression results instead (Table S3).

13. Statements such as "Due to similar weather conditions (i.e., similar DMI) across all fields within a specific year, the varying crop responses to drought stress between fields imply that

additional factors than the weather must have had an impact on crop development (L289ff)" does not bring any new information. Please keep the language concise throughout.

We carefully read through the whole manuscript and improved the text.

14. There is a Tab. S1 and a Table S1 in the supplement. Please sort it out.

Thank you for pointing that out, we corrected it and renamed the first and second table in the supplements.

15. Data availability should include the data used for analysis in this study too. Suggest including GLAI curves with the inferred slopes for all sites in the supplement.

We created GLAI development curves in both years for all fields and added them into the supplements (Fig S2 and Fig. S3*).* We also stated in the Data availability section that *"Data of the growth proxies are available from the corresponding author upon request"* in line 360.

Line-specific comments:

16. L13ff "Knowledge about …": This sentence is very long and complicated. Please simplify or separate into shorter sentences.

Thank you for pointing that out, we simplified this sentence in the revised manuscript to *"Knowledge about the influence of drought on crop development and the role of soil properties for crop drought severity is important in drought risk analysis and for mitigating drought impacts at the landscape level." (L 13-15).*

17. *L17 "interactions of plant-growth and soil parameters during variable weather conditions…": might be an overstatement for this study; the potential impact of soil properties on plant drought responses can be interpreted from the analysis, however not the feedback from the plant growth on the soil. Maybe some interactive effects of soil properties and weather conditions on crop growth but not clearly analysed. Besides, I am not sure about "variable weather conditions" by comparing two field seasons that are 3 years apart. Please revise this sentence.

We rewrote the abstract to better state the novelty of the study in the revised manuscript. The sentence "*interactions of plant-growth and soil parameters..."* was excluded.

18. L18 "a dry year and a year with normal weather conditions": Suggest mentioning these are the year 2018 and year 2021.

We agree, and we added the years 2018 and 2021 in the text (L 18).

19. L24 "An increase in peak GLAI in the dry year…": seems to be comparing among sites where differences should not be addressed as an increasement. Please revise.

You are correct, we changed the word "increase" to "higher" (L23)

20. L26 "suggesting that soil properties play a role in crop response to drought": Before making this statement, differences among fields in e.g., field management, cultivar, and local climate need to be addressed. Same goes to L333ff in conclusion.

We investigated the differences in local climate, and the monthly average DMI showed that the climatic conditions were similar between fields, as shown in the new Figure 2. Our results showed that there was a higher winter wheat growth rate during the extremely dry year in soils with higher plant available water capacity, and less differences in growth rate between the two years in soils with higher plant available water capacity. Due to these relationships, we think it is fair to "suggest that soil properties play a role in crop response to drought". Farmers manage their fields according to "best management practices".

21. L27 "a higher amount of plant available water capacity…": Should be "higher amount of plant available water" or something like "higher soil capacity for plant available water"; Unclear what "crop performance" refers to.

Thank you for pointing that out, we rewrote this sentence to "a higher plant available water capacity" (L25-26). The word "better crop performance" referred to that we found a faster growth rate, and we rewrote this in the revised version (L26).

22. L28 "observed lower growth rate, lower peak GLAI, and earlier peak…": suggest changing to "lower estimated growth rate"; please add "GLAI" after "earlier peak".

Thank you for pointing that out; we corrected it in the revised version of the manuscript (L22).

23. L29ff: please add "in" before "the year" for "compared to the year"; "demonstrate that satellite imagery can be used to quantify plant soil-weather interactions at scales relevant to commercial farming" is not well supported by the results and analysis such as there is no "plant soil-weather interactions" nor it is quantified.

To better state the novelty of our work, we rewrote the sentence in the revised version of the manuscript, and instead wrote that "*Our case study demonstrates that satellite derived crop growth proxies can identify crop responses to drought events, and that satellite imagery can be used to discover impacts of soil properties on crop development at scales relevant to commercial farming.* (L28-30).

24. L30ff "Our investigation serves as a first step towards supporting drought risk management, drought adaptation and communication activities on this important topic": I would strongly suggest being specific on what the "first step" is for, e.g., for the GLAI analysis using satellite imagery at field/farm level, than leaving it too broad, that e.g., involving communication activities would be a stretch.

Thank you for the suggestion. With the better focus of the aim of the manuscript (as described in detail above) we rewrote the abstract in the revised version and removed this statement, because as you say, we do not discuss communication activities in this manuscript.

25. L44ff "Soils with higher resilience to drought…": What is soil resilience to drought? This sentence is unclear. Please revise.

We referred to soils that help mitigate drought conditions. We rewrote this sentence to make it more clear. In the revised manuscript, we changed the sentence to "*Soils that allow water to infiltrate and can store sufficient amounts of water to sustain plant growth can mitigate drought conditions* (L45-46).

26. L57 "there is still limited information about how soil properties affect crop development under various weather conditions": I would say there has been more than limited studies which should be referred here to summarise the long-researched soil property impact on crop growth,

although the results may vary by crop species, scales, and other environmental factors which should not be simply omitted.

We apologize for the wording, the sentence should have been more detailed mentioning winter wheat GLAI development and extreme weather conditions at the field level (as many studies are conducted in field trials). We changed the sentence to "there is still limited information about how soil properties affect winter wheat GLAI development under extreme weather conditions, at scales relevant to commercial agriculture" and we also added that "*GLAI may vary by crop species, scales and environmental factors"* (L60-62).

27. L79 "around 40% of the cropping areas with winter wheat in Northern Europe had yields below the 10th percentile" I found this stat not very straightforward as it is not intuitive for what percentile 40% of the cropping areas should yield below. Suggest revising and improve clarity.

Instead of explaining the drought in 2018 based on yield losses in the Introduction section, we now expanded on explaining the dry conditions during year 2018 in the M&M section based on the weather data (L113-119). This sentence was therefore removed.

28. L122ff "The GLAI was derived from the radiative transfer model PROSAIL following the approach described in Graf et al. (2023)… We randomly generated combinations of leaf and canopy parameters according to a uniform or Gaussian distribution (Table S1)": Table S1 (Tab. S1 in supplement) is identical to Table 5 in Graf et al. 2023 using the same parameters beside GLAI. I am not familiar with this approach, but I wonder if Graf et al. 2023 and two other papers cited by Graf et al. 2023 (Wocher et al. 2020 and Danner et al. 2021) on the parameters in the table should be cited here as well.

- ***Wocher M., Berger K., Danner M., Mauser W., Hank T. RTM-based dynamic absorption integrals for the retrieval of biochemical vegetation traits Int. J. Appl. Earth Obs. Geoinf., 93 (2020), Article 102219, 10.1016/j.jag.2020.102219***
- ***Danner M., Berger K., Wocher M., Mauser W., Hank T. Efficient RTM-based training of machine learning regression algorithms to quantify biophysical & biochemical traits of agricultural crops ISPRS J. Photogramm. Remote Sens., 173 (2021), pp. 278-296, 10.1016/j.isprsjprs.2021.01.017***

Thank you for this observation. Graf et al. (2023) indeed used the work by Wocher et al. (2020) and Danner et al. (2021) as a starting point and updated the GLAI, leaf chlorophyll and leaf carotenoid content using empirical relationships derived from multi-year field phenotyping data. In Graf et al. (2023), where table S1 can be found, it is made clear which contributions came from which study. To make it easier for readers of this manuscript we agree that citing Wocher et al. (2021) and Danner et al. (2021) is appropriate, and we added it in Line 139.

29. L200 Fig. 3 (c): I am confused by this figure that it seems the number of fields presented here don't sum up to 13.

Thank you for observing this, for unknown reasons the number in almost all bars was one larger than it should have been (e.g., showing 4 instead of 3), but in the text the correct numbers were mentioned. We corrected the figure in the revised version of the manuscript (new Fig. 4).

30. L249 Fig. 6 (c): Apologies for the clumsy question - as there are 5 circles in the plots, do they correspond to 10, 20, 30, 40, and 50 or 55? Why is the 5th circle having a smaller gap than the other ones?

The last circle is only the outside border of the plot area and does not belong to any explained variation. The rings are 10%, 20%, 30% and 40% explained variation, starting from the smallest. We added this explanation in the figure caption (Fig. 6).

31. L268 "Reduced wheat biomass during drought has been shown in earlier studies (Villegas et al. 2001; Zhang et al. 2018), and according to Villegas et al. (2001), the decrease in biomass und drought is mainly due to lower growth rate": please correct this sentence.

Thank you for pointing that out; we rewrote this sentence in the revised version of the manuscript (L276-277).

32. L273ff "highlighting the importance of faster growth to mitigate drought impacts": this statement can only be drawn given specific drought severity, duration, and timing which I found missing in the discussion.

Thank you for pointing this out, the statement was a bit too general. We found a relationship between a faster growth rate to peak GLAI in the dry year, and this suggests that a higher growth rate is favourable during dry conditions to obtain higher yield. A higher peak GLAI has been related to higher yield in other studies e.g. by Lambert et al. 2018, Yamamoto et al. 2023 and Sjulgård 2024. We rewrote the sentence in the revised manuscript to "suggests that a faster growth is important to obtain higher maximum biomass and in turn higher yield during dry conditions (L280-281).

Sjulgård H. 2024. The potential of agricultural management to alleviate extreme weather impacts on Swedish crop production. Acta Universitatis Agriculturae Sueciae. (2024:84). https://doi.org/10.54612/a.107ri2j3pt

Lambert M-J, Traoré PCS, Blaes X, Baret P, Defourny P. 2018. Estimating smallholder crops production at village level from Sentinel-2 time series in Mali's cotton belt. Remote Sensing of Environment. 216:647–657. https://doi.org/10.1016/j.rse.2018.06.036

Yamamoto S, Hashimoto N, Homma K. 2023. Evaluation of LAI Dynamics by Using Plant Canopy Analyzer and Its Relationship to Yield Variation of Soybean in Farmer Field. Agriculture. 13(3):609. https://doi.org/10.3390/agriculture13030609

33. L283 "not unambiguous": please revise.

As mentioned in an earlier answer, we excluded this sentence.

34. Fig. S1: Suggest including long-term climate variability e.g., shown as error bar.

Thank you for this suggestion, we included error bars in this figure in the revised version, and moved it to the M&M section (now included in the new Fig. 2)

Answers to comments from Reviewer #2

Dear reviewer,

Thank you for reading the manuscript and for your feedback. In the following we have addressed each of your comments:

What is the novelty of this research? The fact that the different responses of two fields to drought could be related to soil properties is not novel. The author should make it clear how these different signals could be attributed to soil at larger scale and how this analysis lead us to landscape level. Or make the novelty of the analysis very clear.

Thank you for this suggestion. Reviewer #1 had a similar first comment: The novelty of our work is that we demonstrate that satellite derived growth proxies allow to identify drought impacts on crops, and that we can see the impact of soil properties on crop response during drought, at the scale relevant for farmers in their fields. Many remote sensing studies are motivated by the claim that remote sensing can be used to detect stress, but to our knowledge there is yet no study that establishes/ demonstrates the link from differences in satellite derived plant trait proxies to differences in soil properties. Moreover, a quantification of the effects of differences in soil properties on time series of growth under field conditions does – to our knowledge - also not exist yet. We stated the novelty of the study in the revised manuscript more clearly at the end of the introduction (L 77-88), in the discussion (L265-267, 304-306, 341-344) and in the conclusions (351-356).

The authors showed only two extreme fields (Fig. 3) to present the responses of different fields. However, it is still not clear how this varies in other fields. I strongly recommend that the author provides the results for all 13 fields in the supplementary with information of soil data on each subplot. Without this, we cannot understand how the results of field which had different responses are related to soil.

Thank you for this suggestion. We do show data for all fields (Fig 4c, Fig 5) and we used Figs 4a and 4b as illustrative examples. Additionally, in the revised version of the manuscript we also included plots for all 13 fields in the Supplementary materials, both plotted against temperature sum and calendar date (new Fig. S2 and Fig. S3).

Another important lack is related to measurements of soil field capacity. The authors have measured field capacity at -10 KPa for all fields. However, based on Table S1, the clay content varies between 10 and 58%. Measuring FC at -10 KPs is valid for sandy soils and not for clayey soils which is measured at -33 KPa. So this has resulted in a wrong procedure for determining the plant available water capacity that biases the results and conclusions.

We understand your argument, but within the same study, it becomes difficult to compare the results between soil samples when using different matric potentials for different soils. Because we have a gradient in texture, it also becomes unprecise to divide all soils into either sand or clay soils. In Europe, it is common to use -10 kPa as we did, and to define plant available water as the difference between the water contents at -10 kPa and wilting point. In e.g. the US, -33 kPa is more commonly used for field capacity (e.g. Nemes et al. 2011). In the revised version of the manuscript, we added a reference of using -10kPa as field capacity (L180)

Nemes A, Pachepsky YA, Timlin DJ. 2011. Toward Improving Global Estimates of Field Soil Water Capacity. Soil Science Society of America Journal. 75(3):807–812. https://doi.org/10.2136/sssaj2010.0251

The paper lacks a lot when it comes to presenting management data. It is not clear to what level these variations could be related to management factors. When was planting dates in different years and fields? Are there different cultivars? If they are different, how they differ in terms of phenological development? What are fertilizations? What are disease effects? All these information must be clearly provided and at the end reflected in the interpretation of results. Without this, it was difficult to me to make interpretation.

The exact sowing date is unknown but note that winter wheat was used in this study, which in our region is sown within a short time window around the middle of September, and which is almost dormant during the cold winter, and then starts to grow again in spring. The exact sowing date is less important for autumn-sown crops in comparison to spring-sown crops when comparing the growth during spring due to the winter period, as the start of the growing season in spring can be more important for early growth than the sowing date.

Reviewer 1 also had a very similar comment (comment 4), and here is the same answer: "We know that all the fields have conventional cropping systems (and not organic), none of the fields was irrigated, and we complemented the description of the field sites by adding this information to the M&M section (L95). While management can affect growth, we do not compare absolute growth values between fields but focus on identifying drought stress within each field by analysing significant year-to-year differences. For comparisons within the same field, as in shown Fig. 4c, we use percentage differences in growth proxies, assuming minimal changes in management practices over years for the same crop and farmer. However, we agree that potential management variations between years could introduce some uncertainty. But we assume that the farmers manage their fields according to best practice. We extended the discussion about the management practices in the revised manuscript, see lines 329-338 and the M&M section (L95-98)."

The response of crop to drought depends on time of drought and the time of temperature increase. At which phenological stage did drought happen? I think here more detailed information on the stage of growth and onset of drought and temperature increase are required:

Thank you for this suggestion. The unusual dry conditions started already in May in 2018, during the vegetative growth period. We now included this information in the M&M section (L114-117). In the revised version of the manuscript, we also added a more detailed figure showing the temporal development in temperature and precipitation during the growing season for the two years (new Fig 2).

The paper provides zero information on climatic variation between two years.:

Maybe you accidently missed the figures in the Supplementary Information. In Fig. S1 we showed the average monthly De Martonne Aridity Index between May and July for the two years 2018 and 2021, and also the long-term average between 1991 and 2020 for the fields (in the previous version of the manuscript). We have now also added a new figure, now Fig 2, to the manuscript, as also mentioned in the previous answer. The original Fig. S1 was moved to the M&M section and is now included in the new Fig. 2 of the revised version of the manuscript.

"Were the severity of drought and temperature increase the same in all years?":

We included two years in this study, and the motivation in choosing year 2018 and 2021 was to compare an extremely dry year (2018) with a year with normal weather conditions (2021). Here, the new Fig. 2 shows the differences in average aridity (May to July) between the years and average temperature and precipitation.

The authors used the concept of temperature sum. Do they mean growing degree days? If yes, what were the base temperature?

Temperature sum and growing degree days are quite similar, but we used temperature sum because we had average temperature data available for the fields, while growing degree days requires data of maximum and minimum temperatures. The temperature sum was assessed by adding up the daily mean temperatures exceeding the threshold value of 0 °C. As base temperature, we used "the daily mean temperatures exceeding the threshold value of 0 °C, where growth for winter wheat starts (Porter and Gawith 1999)" (line 149-150 of the revised manuscript).

3a. The crop response are significantly different even in April. This cannot be related to drought. I think the author should clearly justify using climate data that the drought already started in April. Otherwise these difference is related to factors which was overseen.

The differences in these original plots (with calendar days on the x-axis) come from the fact that the two years have a different temperature profile (temperature time series). Growth generally started earlier in 2021 – which is visible when growth is plotted against day of the year. When this is taken into account and growth is plotted against temperature sum (as in our revised manuscript, now Fig 4a-b), this effect disappears.

To not confuse readers, we now plotted the figures (now Fig. 4a and b) against temperature sum instead of calendar dates. We think it is still important to also include figures with GLAI plotted against calendar date, for example to see around what time of the year e.g. the peak GLAI occurs. We therefore also included graphs with GLAI plotted against calendar dates in the supplementary materials for all fields in the revised version of the manuscript (Fig. S3 of the revised manuscript).

In line 23, the author mentioned that "we found a lower growth rate, lower peak GLAI and earlier peak GLAI". This is surprising to me, because the fig.3 shows that the peak occurs at about the same time.

When including all fields in the comparison, there was a significantly earlier peak GLAI in the dry year, as seen in Figure 4c and in Figure 5. For the interpretation of Figure 4a and b, we refer to the answer in the previous question.

The author showed different vegetation responses in different years, however it is still not clear to what level it is related to yield data. How much these two responses did influence the final yield?

Similar answer was given to rewiever #1 (2^{nd} comment): This is an on-farm study, with the challenge that not all farms have yield data available, so unfortunately we only have yield data for some of the fields. However, in the PhD thesis of the first author (Sjulgård 2024), a strong correlation was found between the peak GLAI and winter wheat yield in the year 2021 when including the fields with yield data available and with additional farm fields in the same region. Similarly, the peak GLAI has been related to crop yield in earlier studies e.g. by Lambert et al. 2018, He et al. 2020 and Yamamoto et al. 2023.

Sjulgård H. 2024. The potential of agricultural management to alleviate extreme weather impacts on Swedish crop production. Acta Universitatis Agriculturae Sueciae. (2024:84). https://doi.org/10.54612/a.107ri2j3pt

He J, Shi Y, Zhao J, Yu Z, He J, Shi Y, Zhao J, Yu Z. 2020. Strip rotary tillage with subsoiling increases winter wheat yield by alleviating leaf senescence and increasing grain filling. Crop Journal. 8(2):327–340.

Lambert M-J, Traoré PCS, Blaes X, Baret P, Defourny P. 2018. Estimating smallholder crops production at village level from Sentinel-2 time series in Mali's cotton belt. Remote Sensing of Environment. 216:647–657. https://doi.org/10.1016/j.rse.2018.06.036

Yamamoto S, Hashimoto N, Homma K. 2023. Evaluation of LAI Dynamics by Using Plant Canopy Analyzer and Its Relationship to Yield Variation of Soybean in Farmer Field. Agriculture. 13(3):609. https://doi.org/10.3390/agriculture13030609

5 is confusing. Why the correlation was shown for only growth rate and peak GLA? What about the other?

In Figure 5 in the old version, we show the results for all growth proxies, and the timing of peak GLAI, i.e. temperature sum at peak GLAI, is also part of the figure. However, based on comments from reviewer #1, we removed the figure and present the results in a supplementary table in the revised version of the manuscript (Tab S3).

Line 314,"in soil properties changing over time". This is surprising to me, soil properties do not change over 2-3 years which makes such interpretation less convincing.

Soil properties are dynamic, but the time scales at which soil properties change can vary from weeks (e.g. concentration of mobile nutrients) to geological time scales (e.g. soil texture). In the context of our study, it is important to consider whether the measured soil properties (such as soil organic carbon content or plant available water capacity) change within 2-3 years or not. Therefore, we stated in line 322-324 (in the revised version) that "a number of studies has shown only small year-to-year changes in soil organic carbon content (Krauss et al. 2020), water content at field capacity (Alam et al. 2014) and bulk density (Alam et al. 2014; Alnaimy et al. 2020) within given soil management systems.". This, together with the fact that the same crop is grown in both 2018 and 2021, and the same farmer is managing the fields between years and hence management practises were similar, provides support that the soil properties that were measured in this study would probably not have changed significantly from year 2018 to 2021.