

Clim. Past Discuss., author comment AC2  
<https://doi.org/10.5194/cp-2021-52-AC2>, 2021  
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## Reply on RC2

Martin Skoglund

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Author comment on "Climate variability and grain production in Scania, 1702–1911" by  
Martin Karl Skoglund, Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-52-AC2>, 2021

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**In the following, I have divided the reviewers comments into parts, where I answer each part separately. All references are collected in the end of this reply.**

### Reviewer comments part 1:

#### General comments

The article studies the relationship between climate variability and grain production in Scania (southern Sweden), between 1702 and 1911. The study also claims, in the abstract, that it will shed new light on the climate history of the region by homogenizing the Lund instrumental series. The article, therefore, has several goals and is ambitious. As some of my questions, have been dealt with in the discussion already, I try to focus on some other issues that could improve the manuscript. The manuscript has potential, but the manuscript could use a thorough revision when it comes to structure. In its current form, it is difficult to follow the authors line of thought. Sentence structure is often speculative and results and approaches needs to express intent more clearly.

My initial impression was that the manuscript would be improved if it was written as two separate articles and it would make it easier for the author to focus on grains and climate. The first article could present the homogenization of the Lund series, which then could be used as a background for a more in-depth approach to write the second article, which would compare climate variability and grain production. As it reads now, the meteorological series from Lund is not actually mentioned until page 14, which is followed by an analysis, results and discussion (section 2.3). As such, the manuscript provides new perspectives and approaches, but the manuscripts structure and formulations need to be improved.

The reason for my suggestion to write two articles instead of one article is that I am unfamiliar with the Lund series. Does it cover the 1753-2020 period (the end period is not mentioned)? What are the gaps (months, years, days?) mentioned on line 386? This section is extremely condensed, and I would prefer if the series was introduced more thoroughly. As it is, there are no references to the series. Also, it reads somewhat peculiar when it is noted that there are series from 'nearby regions' (line 392), mentioning both Uppsala (c. 500km) and Central England time series (c. 1000km), and I wonder if the distance of 500km between Lund and Uppsala, or 1000km between Lund and England could be considered 'nearby'? This is a matter of semantics, however, I think that the series and the correlation deserves a more in-depth discussion than this. Especially

considering the results and the correlations in Table 2. The results are interesting. It is intriguing that the correlations during the months May-October, between Lund and Stockholm, Uppsala and Berlin-Dahlem are stronger than those with neighboring Copenhagen (c. 40km). Why is this? Is it a question on instrumental reliability, series validity or climatic factors? Answering this question is not the purpose of this article, but it seems quite central for the analysis and series in general. Moreover, in Figure 6 and 7, the series is extended to 1701 even though this is not mentioned earlier. In summary, the approach is interesting, but it needs more context. Finally, on page two, it is said that the climate in Scania resembles that of England. Why then is the correlation with the Central England time series the lowest of all? What part of the 'relatively mild' (line 33) Scania climate is comparable to England? Also, what is the correlation period analyzed in Table 2, is it the studied period 1753-1922 or 1753-2020? A general description of the climate in Scania, without comparisons to other European regions, could make it clearer.

Answer: The main purpose of the climatic reconstruction and homogenization is to obtain climate indicators relevant to grain production in Scania during the study period. This is why the reconstructed temperature series is extended back to 1701, which should be explained more clearly in the manuscript (why I do not attempt a similar approach for precipitation is explained on lines 426-428).

The instrumental temperature measurements in Lund began in 1753 for daytime measurements and in 1768 for measurements in the evening and continues until today, albeit with a few gaps and inhomogeneities, as discussed in the manuscript (see lines 385-388). Efforts to standardize and make the measurement process more transparent and scientific were undertaken in the last decades of the 19<sup>th</sup> century (see Tidblom, 1876). The series can thus be divided into two parts, the early instrumental measurement period from 1753 until 1860 (measurements until 1870 were published by Tidblom, 1876, see line 387 in beginning of section 2.3) and the latter instrumental period after 1860. The series for the latter period is referenced at lines 442-443. This periodization is mirrored by those in the data sets on grain production, i.e. 1702-1865 and 1865-1911.

Regarding the distance between the stations of temperature measurements, it is quite common for temperatures to be spatially correlated across large areas at a monthly or seasonal time-scale. This can be seen for example in Tab. 2, which is why it is possible to bridge series from stations many hundreds of kilometers distanced from each other. Examples in previous research of this are for example Parry & Carter (1985) who bridged Edinburgh to Central England, roughly 400 km apart, Nordli (2004) who interpolated from Uppsala to Trondheim at 550 km apart, and Dobrovoly et al. (2010) who reconstructed Central European temperatures using instrumental data from Switzerland, Germany, Austria and the Czech Republic (including Vienna and Geneva 800 km apart).

For the method employed in the manuscript, the most important factor for homogenization and filling of gaps is the number of network series, which should be at least four, and spatial correlations should be at a minimum 0.4. With higher spatial correlations and more network series, the results become more robust. As can be seen in Tab.2, Lund and Copenhagen has the largest spatial correlation. Nevertheless, the reviewer brings up the question of why summer temperatures in Copenhagen has a lower correlation coefficient with those in Lund compared to Stockholm, Uppsala and Berlin-Dahlem (while also noting that answering this question is not the purpose this manuscript). Furthermore, the reviewer asks whether the differences are due to instrumental reliability, series validity or climatic factors.

First of all, the results of the ACMANT procedure should not be taken as a questioning of the instrumental reliability and validity of the network series. In the procedure, the network series are assumed to be homogenous (Domonokos & Coll, 2017). As noted on lines 390-394, all the network series have been subjected to homogenization efforts.

However, the Copenhagen series is subject to more gaps than the other series. The Copenhagen series starts in 1768 and has gaps between the years 1768-1781, 1789-1797, meaning that from a statistical point of view it is more susceptible to outliers. Regarding the validity and reliability of the Lund-series itself, it is discussed on lines 385-387 and 410-419, and the purpose of the homogenization of the Lund-series in the manuscript is of course to improve its reliability and validity. The last potential factor the reviewer brings forward to explain the differences in correlation coefficients between the series is climate. As can be seen in Tab. 2, there is a clear reduction in the correlation coefficients between Lund and the network series in the summer months compared to the winter months. In other words, there is a climatic seasonal aspect to spatial correlations in temperature. Given all this, it could be that the correlation coefficients between Lund and Copenhagen is affected by some outliers in monthly summer temperatures, yielding slightly lower coefficients compared to the other network series.

With respects to gaps in the early instrumental period, i.e. 1753-1860, there is one large gap for all measurements between the years 1821 and 1833, as well as some smaller gaps in daytime measurements February 1794 as well as between April and September in 1846 and 1847. For evening measurements there is one gap between 1809-1813 and some smaller gaps in February through July in 1794, December 1801, parts of August and from middle of September until the middle of November in 1804, late August and early September, most of October and a dozen days in November in 1805 as well as September and October in 1806. The series is referenced in the beginning of section 2.3 at line 387, see Tidblom (1876).

The reviewer had the impression that the manuscript could be divided into two parts, because it would make it easier for the author to focus on the relationship between grain production and climate variability. The reviewer also points out that the Lund series is not mentioned until page 14. Currently, sections 1.3, 1.4, 2.4, 3.1, 3.2 and 3.3 are almost entirely devoted to the subject. Furthermore, section 2.1 and 2.2 are entirely devoted to producing series on grain production. Section 2.3 revolves around the climate data, most of which is devoted to the homogenization and gap filling of the Lund temperature series. Hence, I would argue that the manuscript is already very much focused on the relationship between grain production and climate variability, as almost all sections are devoted to it, and the other sections are necessary building-blocks in that venture. That being said, I propose to write an Appendix, where I elaborate and present more descriptive statistics on the temperature series homogenization and gap filling (including aspects from the discussion above), including discussing the apparent warm-bias found in other series in central Europe, and the apparent cold-bias found in this study for the late 18<sup>th</sup> century.

Regarding the resemblance of the climate of Scania to England (and northern France) on page 2, the statement will be revised. Climatically, Scania is usually categorized as having a continental climate, more comparable to central and eastern Europe than England and northern France that have an Atlantic maritime climate, the latter type of climate being warmer in the winter and, on average, slightly colder in the summer than the continental climate (Metzger et al, 2005). The intended purpose of the statement mentioned above (appears on line 31, see also the whole paragraph on lines 30-40) was to highlight the capacity for intensive grain production (where climate together with other factors, most notably soil, are the most important determinants), which is comparable to areas of England and northern France, *as well as* large parts of central and eastern Europe.

I agree with the reviewers' questioning of the comparison to England and France (although I would insert here that two regions can have a similar climate but experience different climate variability, this is especially clear in regards to precipitation). The more apt comparison in this introductory contextualizing part of the manuscript would be to compare Scania to areas characterized by intensive grain production *and* a similar climate

in continental Europe (see Metzger, 2005 for a commonly cited climatic stratification of the environment in Europe).

Regarding the capacity for intensive grain production, there are no robust historical estimations of such to my knowledge, although Olsson & Svensson (2010) compares the development of agricultural production in Scania to other areas in Europe during the 19<sup>th</sup> century. Slither van Bath (1963) collected grain yield ratios for a wide array of localities across Europe, including Scania. However, these do not control for the sowing intensity per area unit, and thus not useful for this type of comparisons. There are several contemporary estimations of the capacity of grain production or long-term winter-wheat yields (e.g. EEC, 2011; Boogard et al, 2013:135,138 & Schils et al, 2018:114).

Finally, regarding the reviewers question on Tab. 2: it shows the correlations between Lund and the network stations in years during the homogenization period when there are available measurements from Lund (i.e. 1753-1870, excluding the gap years of 1821-1833).

Reviewer comments part 2:

In section 1.1. There is quite a lot of focus on climate in the 16<sup>th</sup> and 17<sup>th</sup> centuries whereas climate development during the 19<sup>th</sup> century, receives very little space. Maybe Figure 1 could be employed for a more extensive discussion on climatic development? The section could be strengthened by placing focus on the studied period.

Answer: It should be pointed out here that the period that receives the most attention in section 1.1 is the 18<sup>th</sup> century (lines 75-85), which forms the first half of the study period. Nonetheless, I agree that the 19<sup>th</sup> century should receive more attention. In line with the comments made by RC1, I proposed that a description of the occurrence of frost in the growing season in Scania could be added to the manuscript, using 19<sup>th</sup> century data. In relation to this, I could expand upon the climate in the 19<sup>th</sup> century, including long-term averages of monthly and seasonal temperatures and precipitation.

Reviewer comments part 3:

Section 1.2. Farming in Scania. The theme is farming and harvesting, but I would like to see more information on harvest dates and threshing, especially as it is of great relevance for the analysis and the clustering (and because Scania seems to differ from other parts of the Nordic countries?). Some of this is already mentioned in the discussion with RC 1. Adding to this discussion, maybe there could be a sentence or to describing the length of the growing season (line 589)?

To my knowledge, there is no research that clearly shows that sowing and harvesting dates in Scania differ significantly from other parts of Scandinavia. While I do point out in the manuscript that Scania has a longer growing season than other parts of Scandinavia, this does not imply that sowing and harvesting dates differ correspondingly (however, the length growing season is still crucial, for example a longer growing season minimizes risks related to early autumn and late spring frosts, or detrimental harvest weather). As I argue in the manuscript, any such account has to control for the type of crop variety being cultivated. In my own unpublished investigations, I have found little differences in sowing and harvesting dates in Scania and other parts of northerly Sweden or parts of Norway, at least for barley and other spring grains. For autumn-rye, harvesting dates are also similar, whereas sowing dates vary to a greater extent. I argue in the manuscript, in line with archaeobotanical research, that most of the barley and rye varieties cultivated in Scania were similar to those grown in more northerly parts of Scandinavia, at least until the 19<sup>th</sup> century. See the response to RC1 for more information on harvest and sowing dates in Scania, where I also agreed with the reviewer that they could be added in a summarized

version to the manuscript. Regarding threshing dates, they are even more rare than harvest and sowing dates, and presumably they are much more variable than the latter. On line 295 I note the important fact that tithes were collected *before* threshing.

Reviewer comments part 4:

Section 1.4 mostly seems like a presentation of previous research and I wonder if this could be presented earlier in the manuscript as part of 'previous research', especially the presentation of the research conducted by Edvinsson et al. (2009).

Answer: Section 1.4 indeed is a summary of previous research on the theme 'crop diversity and resilience' that is relevant to the manuscript. As it stands, the section appears well situated where it is to the author. However, as the reviewer suggests, the research by Edvinsson et al. (2009) and Utterström (1957) could be presented in a summarized form earlier in the manuscript as it has bearing specifically to farming conditions in Scania during the study period.

Reviewer comments part 5:

Section 2 'sources and methods' contain a presentation of methods and sources, but it also contains an analysis, which makes the manuscript structure confusing. The homogenization is a result in itself and should (preferably) not be presented in a section called 'sources and methods'.

I think a more classic structural approach of the manuscript (where material, method and analysis are presented in separate sections) could work better and it would improve readability.

Answer: A similar point was made by RC1, and I do agree with the points made by both reviewers on the structure of the manuscript, where sources, methods and analysis each should appear in clearly distinguishable sections.

Reviewer comments part 6:

### **Specific comments**

The manuscript has a very speculative language. This affects the overall impact and scientific quality of the manuscript. The term 'relative' is used frequently and situations or climatic conditions are often described as 'relative'. A search indicates that the word 'relative' appears 62 times in the manuscript (four times in the abstract) in various contexts to describe a myriad of situations. For example, (line 65) is explained that 'relative peace' dominated in the 1700s compared to previous centuries. I am not familiar with Swedish history, what does this indicate in terms of wars and skirmishes? Is the frequency or magnitude of skirmishes that defines 'relative' peace? However, I do not think this historical overview of the political history of the region is necessary because the author does not return to this subject or its impact on harvests or threshing.

This use of relative continues throughout the manuscript. On page four, the term 'relative' occurs five times. On line 94, 'relative lack of wood' and on line 105 'in their relative specializations', and then on line 115, 'with relatively much...' and later in the same sentence 'saw a relatively large increase'. Finally, in the footnote on page 4 'relatively abundant'.

Relative is a subjective term and should be avoided as it in a scientific investigation provides no actual perspective of change, magnitude or proportions. For instance, what does a 'relative lack of wood', indicate or describe? Is it an indication of amount of wood,

distance to wood (as in a forest), lacked access to firewood? And on line 85 (page 3) it is explained that the 1810s and 1840s were 'relatively cold', relative to what place and period? Relative to the 1600s? Was it colder all year round, was only the summers colder or was the 1810s colder than previous/later periods (decades) or what is just cold in comparison to warmer periods (decades)? Even in the results (line 464) it is said that the analysis shows 'relatively large negative associations' and Figure 13 includes an analysis of 'relatively wet years'. How wet is a 'relatively' wet year, is it possible to quantify and explain this in the text? Is a 'relatively wet year' wetter than 'normal' years, and if so, how much?

Finally, describing things as being 'relative' are vague and it raises a lot of questions. I think that rewriting and rephrasing many of the vague sentences would greatly improve the manuscript and make it easier to evaluate the content. The results would also stand out more clearly.

Answer: The reviewer brings up issues related to using vague terms too much in a scientific context and that especially the word 'relative' occurs too frequently in the paper. I agree with the reviewer on the proposal to rewrite and rephrase many of these lines. Regarding the questions the reviewer raised on the specifics of the wet years shown on Fig. 13, they are described, together with dry years (Fig. 12), on lines 455-458.

Reviewer comments part 7:

### **Technical comments**

Line 14. In the abstract it says that new cultivars were being 'imported' at the end of the period from other parts of Europe. This is not discussed in the manuscript. Did the import start at the end of the 19<sup>th</sup> century or were new cultivars in use all across Scania by the end of the studied period? Why were they imported, was it caused by changes in climate, demand or other issues? As I read the article, there already seemed to be an extremely diversified variation of grains, but the new cultivars are just one variant of autumn-wheat and autumn rye? The questions are rhetoric but stems from how the subject is approached and the changes that occurred in the later part of the investigated period.

Answer: The introduction of the early improved cultivars on a large scale is discussed on lines 116-118 and on lines 500-506. However, the fact that they were mostly imported from other parts of Europe is not mentioned and this information will be added to the manuscript. The introduction on a large-scale of early improved cultivars began in the end of the 19<sup>th</sup> century, which is implied in the manuscript but this could be clarified (Forsberg, 2015: 11-12). The review on landrace definitions and classifications made by Zeven (1998) also discusses this process.

For example, in 1891, the first share-holding company specializing on the distribution of such cultivars was founded in Svalöv, Scania (Leino, 2017). Smaller imports of early improved cultivars occurred throughout the 19<sup>th</sup> century, and possibly earlier as well. In parish descriptions from Malmöhus in 1828 there are some occasional mentions of 'Probsteier-rye' and 'Dutch winter-rye' (Bringeus 2013). The process was gradual, and around the turn of the century farmers in Scania were probably growing a mix of old and new varieties (Leino, 2017). This information will also be added to the manuscript.

The question of why they were imported and introduced is a whole research topic in itself. Based on my reading of the literature, it should be seen as a mix of ideology and demand. In the early phases, it was probably more ideological, spearheaded by the promoters of the agricultural reform movement in the 18<sup>th</sup> and 19<sup>th</sup> centuries (see for example Jones, 2016). Actual demand picked up in the late 19<sup>th</sup> century as the early improved cultivars were better suited to the type of agriculture that was increasingly manifesting itself in that

period through better plowing, drainage, precision-sowing and fertilization (Leino, 2017).

The older varieties were certainly more diversified than the early improved cultivars. However, the latter should not be confused with the homogenous varieties obtained through modern plant breeding in the 20<sup>th</sup> century. Leino (2017) describes how the early improved cultivars were still obtained through 'mass selection', whereas modern plant breeding is oriented towards selecting for *one* particular strain.

Reviewer comments part 8:

Line 121. It says that wheat-varieties (plural) will be included in the 1865-1911 period, but there is only one type (Autumn-wheat) mentioned in Figures 11, 12 and 13. In Figure 13 the caption reads 'during relatively wet years', but the figure only shows different periods, not years.

Answer: In line with the discussions of grain varieties in the manuscript and the answer above, autumn-wheat should be understood as plural. Fig. 13 shows the relationship between grain and climate indicators (seasonal, monthly) during wet years. Wet years are defined on line 455-458. However, to clarify and make the manuscript more transparent, I propose to add all the wet and dry years in an Appendix.

Reviewer comments part 9:

Line 132-135. I think this comparison of soils is unnecessary.

Answer: Lines 132-135 describes research of rye landraces (distinguishable groups of rye varieties) and is relevant to the findings and discussion of the manuscript see lines 572-591.

Reviewer comments part 10:

Line 156. Is this 'however-sentence' suggesting that black oat varieties were grown in Scania or just in neighboring provinces? In the previous sentence it was already stated that it is uncertain (remove 'more' in 'more uncertain' as there are no different levels of uncertainty).

Answer: It is suggesting that black oat varieties were grown in Scania. I propose to remove the word 'more' from the sentence.

Reviewer comments part 11:

Line 168 (section 1.4), it is concluded that the diversity of grain varieties "testifies to a relatively flexible farming system in terms of sowing and harvest dates as well as the ability to produce under differing agrometeorological conditions, not least during colder and wetter periods". Could the author please elaborate and explain how a large variety of grains give indications about farming systems and capability of adapting to different agrometeorological conditions? This subject almost seem like an article in itself (I do not think that the existence of different varieties is proof of different systems, we can assume, but it is still just an assumption), but if there is more information, please elaborate. For example, are successful harvests seen, by the author, as an indication of adaptation?

Answer: Regarding farming systems, it is not stated in the manuscript that the existence of different grain varieties is proof of different systems (I assume that the reviewer means *farming* systems). As I describe on line 103-105, farmers in Scania practiced a mixed farming system, where livestock husbandry and grain production were integrated and mutually dependent. Under this broad category of mixed farming systems, there were one-

field, two-field and three-field systems (defined by the proportion of systematic fallow), either of which was practiced by most villages until the 19<sup>th</sup> century see lines 95-97, 99-100 or 127-129). Sometimes the terms one-course, two-course and three-course rotations are used. These definitions are in common usage in the Swedish (and European) historiography (Myrdal & Morell, 2011).

There has been a connection made in the literature between the type of farming systems and the composition of grain production, notably between autumn-rye and two-field and three-field systems due to the supposed need for a full year's fallow after harvest (see the comment made in relation to late-rye on line 127). However, I have not found any such connections looking at data from Scania (unpublished investigations). As farming systems are defined in the manuscript, there is no question in the literature as to there being different systems (variants on the mixed farming system), and I would agree with the reviewer that the cultivations of different grain varieties do not imply different systems. The argument in the manuscript is rather that in the farming systems in Scania, farmers could and did grow many different crops, but in particular barley, rye and oats. Within each type of grain, there were multiple varieties that offered further flexibility in sowing and harvesting.

In relation to the first question the reviewer asked, I first refer to lines 175-181 where I discuss the topic of adaptation from a theoretical perspective. The distinct concept of resilience is defined on lines 172-174. Following these definitions, I would like to emphasize that in the lines specified by the reviewer (lines following line 168), I do not explicitly state that the large variety of grains give indications of the farming systems ability to *adapt*, merely that it offered "the ability to produce under differing agrometeorological conditions", which I would rather put under the "resilience" category. Similar arguments have been made by Michaelowa (2001) for the case of England (which he compares to France), by multiple scholars for the cultivation of both autumn-rye and barley in Finland (see for example Taavitsainen et al, 1998; Holopainen & Helama, 2009; Solantie, 2012; Huhtamaa et al, 2015) and by Berg (2007:11) regarding the use of both spring- and winter-crops, as well as other garden crops in Sweden in the 18<sup>th</sup> and 19<sup>th</sup> centuries.

Section 1.4 is called "Crop diversity and resilience" presents the available research on this topic and follows an extensive discussion of the various grain varieties (Section 1.3). The answer to the reviewer's question can be found in Section 1.3 and Section 1.4. In essence, the answer boils down to the differences in phenology for different grains. Different grains were sown or harvested during different times during the year, had disparate requirements in terms of accumulated temperature, sunlight and precipitation at distinct times of the year, and finally, some varieties were more susceptible than others to different weather shocks (see Section 1.3 and references above). In the discussion section of the manuscript, Section 3.2, I note that there was a difference between the grains in their respective relationships to temperature and precipitation, although mainly limited to the summer months (see lines 609-611).

A contributing and complicating factor in this regard was that there was much variety *within* each grain variety (in contrast with the mass selections of grain varieties described above, and especially the latter modern plant breeding, the older grain varieties were selected more randomly across a given field, incorporating thousands or millions of roughly similar but distinct grain seeds. I refer to this variety in the discussion lines 611-612, see references therein).

Regarding the final question, successful harvests would be a sign of adaptation, given the definition stated on lines 174-181. However, take heed of the fact that I include exaptation in this definition of adaptation. I would emphasize that this manuscript is mainly concerned with the overall relationship between grain production and climate



variability and not to what extent farmers were aware of and exploited this relationship to their advantage. Nonetheless, as I propose on lines 567-573, the results indicate some degree of long-term adaptation for relatively cool and humid conditions, at least as far as grain production is concerned.

Reviewer comments part 12:

Line 333. The figure could be improved. It reads that the figure reveal that all clusters are in Hjárnarp and Toastarp. However, looking at the figure, I cannot see the mentioned places.

Answer: The location of the specified parishes are noted on line 334. I could add parish numbers and a related legend listing all the parishes to the map, although that would be confusing since the analysis is mainly concerned with groups of villages and the county of Malmöhus as a whole. The number of villages are too many to make the legible on a map on this scale (there are 381 villages in the HDSA sample, see Table 1).

Reviewer comments part 13:

Line 358. Says that there are four clusters. Should be three? I guess it is the same in Figure 2 caption.

Answer: Should be three clusters.

Reviewer comments part 14:

Lines 459. The text has references to clusters in Figure 9. There are, however, no clusters mentioned in the figure caption and it is a bit confusing to understand what is indicated. The figure caption explains that the figure shows grain and temperature/hydroclimatic indicators, where are the clusters? A similar reference to clusters and Figure 10 is found in lines 467-468.

Answer: The clusters are indicated by the # next to each type of grain. For example Barley #2 is barley production in cluster 2. This should be included in the figure captions or notes.

Reviewer comments part 15:

Line 466. The authors says that spring and autumn gives 'almost no statistically significant results', but values are not presented. This was also mentioned by the first RC

Answer: Only statistically significant values have colored cells. This fact should be included into the descriptions to the figures, as suggested in response to RC1. The correlation coefficients between each type of grain and each month or season are included into the respective cells in all figures, where they can be easily observed by the reader.

Reviewer comments part 16:

Line 460. Could r-values be included so that the reader could understand the difference between 'slight negative values' and 'relatively weak'? This would help to follow the author's line of thought.

Answer: Regarding the figures, r-values are included as described in the answer above. However, the reviewer is perhaps referring to r-values being included into the actual text, and I propose to do this in the parts of the manuscript where it is relevant.

## Reviewer comments part 17:

Line 521. This sentence seems to suggest that a correlation of 0.31 is high, as in strong? In the manuscript it states that it is 'quite high' (quite is again a relative term), but is 0.31 high in comparison to other studies, results or years? If correlation is 0.31 then it is low. Please elaborate on this subject and clarify what is meant.

Answer: The points brought up by the reviewer are important, and the language in the manuscript should be revised so instances where the word high (as in strong) is used, they should be replaced with the word "strong" or an equivalent. Similar measures should be taken with the word low (as in weak), which should be replaced with the word "weak" or an equivalent, as to improve readability and clarity of the manuscript.

The evaluation of a specific correlation coefficient should be done in line with statistical principles *as well as* the context of the study. To take a trivial example, if I found a correlation coefficient of 0.8 when correlating the number of times the letter 'k' appears in my text document when I press the physical key "k" on my laptop keyboard (let us say with a total  $n$  of 100), I would consider this weak. In the context of studying the relationship between grain yields and temperature variability on "the margins of agriculture" in Finland, a correlation coefficient of 0.8 would have been seen as very high (see Huhtamaa, 2015, where correlations rarely reach  $\geq 0.4$ ).

Returning to the specific example brought up by the reviewer, the correlation of 0.31 is not *strong* (it is in the bottom of a range of statistically significant correlations between 0.31 and 0.74), and the specified sentence should be revised accordingly. In other figures, for example Fig. 9, a correlation of 0.29 is high as in strong, considering the time span involved and the lack of detail in the climate variables (compared to figures in Palm, 1997 or Edvinsson et al, 2009, who also studies southern Sweden). It is also strong compared to the other climatic indicators included in the analysis. Overall, it should be expected that *lower* correlation coefficients be obtained when analyzing longer time periods, given adaptation on the part of farmers and the plant material. Furthermore, with a lower level of detail in the climate data, one would also expect lower correlation coefficients (both Edvinsson et al, 2009 and Beillouin et al, 2020, makes similar arguments). It should also be contextualized within the historical context of farming. On the very margins of agriculture, one might expect higher correlation coefficients from even seasonal or annual climate indicators. For example, consider northern Scandinavia where late spring and summer temperatures, roughly May through August (MJJA), is more clearly distinguished as the most limiting factor for grain production (Nordli, 2003 & Nordli et al, 2004 could use harvest dates from central Norway to reconstruct MJJA temperatures). Implied by these high correlations is also the higher frequency of harvest failure, since it means that harvests shift with inter-annual temperature variability.

In Scania in contrast, grain production was constrained by a more complex combination of agro-meteorological indicators. Precipitation, the nutrient quality of the soil (in northern Scandinavia grain farming was mostly done on nutrient-rich soils where soil depletion often led to farm abandonment, see Antonsson, 2004) as well as the humidity of the soil appear to have had a similar importance as temperature (see Utterström, 1957 or Edvinsson et al, 2009).

Thus, when Fig. shows a correlation of 0.28 between total grain production in Cluster 2 and June temperatures, this is a strong correlation. Consider: the length of time (ca 150 years), the lack of detail in the climate data (monthly or seasonal averages), that temperature was not the dominant constraining factor and finally the correlations between grain production and other monthly and seasonal climatic indicators. I propose to add a discussion along these lines to the discussion section in the manuscript.

Reviewer comments part 18:

Line 533. What does 'this trend' refer too? Was the shift towards wheat reinforced by new variations of autumn rye? Please elaborate or improve sentence.

Answer: This refers to the fact that the new varieties of autumn-rye (the early improved cultivars) were more temperature-sensitive. The sentence should be improved to clarify this.

Reviewer comments part 19:

Line 552. The first sentence reads that grain production was 'mainly constrained by precipitation', but the next sentence reads 'However, instead of focusing on just temperature...'. This is a bit confusing. Should it be 'Instead of focusing on precipitation...'?

Answer: Correct, should be revised accordingly.

Reviewer comments part 20:

Line 556. What argument is referred too? The dominating role of precipitation? Is this an argument or a result of previous studies?

Answer: Argument made by Utterström (1957) and Edvinsson et al (2009). Utterström (1957) is missing in the reference list of the manuscript and will be added in a revision.

Reviewer comments part 21:

Line 558. What period is here referred too? The late 19<sup>th</sup> century (line 556) or the study-period? This is needed to follow the line of thought without having to read all other studies.

Answer: Refers especially to the 18<sup>th</sup> century. 'period' should be replaced by 'the 18<sup>th</sup> century'.

Reviewer comments part 22:

Line 606. The sentence after the parenthesis is very speculative. "A relatively... probably... to some extent."

Answer: Should be revised in line with previous comments and answers above.

Reviewer comments part 23:

Line 646. What does 'total production' refer to?

Answer: Should be 'total agricultural production'.

Reviewer comments part 24:

Line 652. What is considered as the pre-industrial period in Scania? This historical term appears twice in the manuscript, but I do not know when the industrial period started in Southern Sweden or how it relates to the agrarian revolution (Line 60). Is the import of new cultivars (line 14) part of the agrarian revolution or the industrial revolution?

Answer: Roughly the period before the late 19<sup>th</sup> century. Industrialization in Scania began

ca 1850. The agrarian revolution took place between roughly 1750-1870 in Scania, and was characterized first and foremost of dramatic increase in agricultural production relative to population. This increase in agricultural production has been associated with institutional changes (enclosures), new crop rotations and crops, increased market integration, new technological implements and an increased use of iron in agricultural tools. The subsequent industrial period brought about, among other things, chemical fertilizers, increased mechanization of labor, new crop breeding methods and the use of steel in agricultural tools.

Reviewer comments part 25:

Caption in Figure 6 and 7. A technicality, but I would rather see the sources for this figure included in the captions instead of referring to the footnote in Table 2.

Answer: Sources could be added to Fig. 6 and Fig. 7 captions.

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