Reply to Pascal Yiou

Christian Pfister and Oliver Wetter

As our article challenges the main argument of the Chuine et al. (2004) paper, according to which the summer 2003 was the warmest since 1370, it was to be expected that the mathematician Pascal Yiou, the scientific leader of the French Climate School in Gif sur Yvette, would enter the debate.
We thank him for his short comment (SC) and the recommendation of additional literature which we are going to include in the references. We think, we know how to write a scientific paper. Thus, the reference to Day and Gastel (2006) seems not to be necessary. In this context, we take the liberty to point to some of the issues of the Chuine et al. (2004) paper which Dr. Yiou defends without reservation.
We arranged our reply in a logical order.

SC 1: *In principle, this is an interesting paper, with a lot of useful information on Swiss historical wine growing.*

REPLY: Thank you for acknowledging that our paper includes some useful information. However, you should consider that the focus is on comparing the warmth of the record breaking year 1540 with 2003 rather than on just providing information on historical vine (not wine) growing in Switzerland. You seem to have overlooked that this issue also involves dealing with the underlying weather patterns in all aspects, as the many papers on the 2003 event demonstrate. It is well known that the assessment of pre-instrumental extreme values from time series analysis needs to be supported by both time series analyses and the study of narrative documentary evidence (e.g. Battipaglia et al. 2010).

SC 2: *Wetter and Pfister do not give useful technical information on their temperature reconstruction. The manuscript does not contain the necessary methodological elements to reproduce the results. The "reconstruction" section only mentions the use of regression, but this is too vague to be useful.*

REPLY: Indeed, we didn't publish the needed data on the internet to reproduce the reconstruction yet. This will be done in short time.
The methodology of our paper agrees with the standard palaeo-climatological procedure of calibration and verification. It is principally exactly the same as was described in Wetter and Pfister (2011). The procedure was mentioned on page 2705, line 20-22, albeit certainly too shortly:
"The 17 GHD series presented in the previous section were homogenised with regard to dating style, data type and altitude following the procedure described in Wetter and Pfister (2011)"
On the other hand, we have to remind Dr. Yiou that the raw data of the Chuine et al (2004) paper are not available either. According to an e-mail by Valery Daux, who was a co-author of the paper, the data "come from different burgundian sources the main one being Dijon city. This series was originally compiled by Angot (1883) and used by Le Roy Ladurie." (mail to Oliver Wetter on 7th Nov. 2011). However, these raw data are not included within the supplementary material on internet. So it is not possible to reproduce the results. Daux does not mention either that Angot (1883) already copied the Dijon series from Lavalle (1855). Working with documentary evidence hinges on the critical evaluation in the preparation of the original source material, because it is well known that copying numbers is error prone. Labbé and Gaveau (2011) recently visited the archives of Dijon and produced an up-dated and

corrected series from this vineyard. They detected various errors in the published Dijon series of which the most fatal relates to 1523: Chuine et al (2004) claimed that "the next highest anomaly during the whole period since the 14th century [after 2003] was 4.10 °C in 1523. This confirms and refines the conclusions of previous studies about the exceptional warmth of the 2003 summer in France." However, according to Labbé and Gaveau (2011) the vintage in this year was rather late, i.e. the summer was cool.

SC 3: *"The paper of Chuine et al. (2004) contains a lot of information on the methodology and phenological models used".*

REPLY: For comparing record breaking extreme events statistical analyses *on their own* not also dealing with individual extreme years are of limited value.
The basic data in the Chuine (2004) paper are given in days before September 1st. This makes replication more complicated and is not in line with the standards of phenology working with Days of Year (DOY).
More importantly it needs to be questioned, why the authors included August temperatures into their model. The classic by Mullins (1992) which is not quoted in the Chuine et al. (2004) article comes to the conclusion stating that "the ripening phase […]did not exhibit a clear dependence upon temperature (p.139). Dr. John Gladstones, a distinct agricultural scientist, who got an award from Office International de la Vigne et du Vin, Paris, writes about a "widely observed phenomenon that temperatures of the first two or three growing season months, or alternatively the date of flowering, can usually predict quite closely the dates of veraison and maturity to follow. …] The later phenological intervals [i.e. veraison] show little response to temperature, and tend to be constant from year to year." (p. 17). These considerations are highly relevant to the case of 2003 and 1540. In these years, we think it can be excluded that temperatures in August mattered for the ripening date of the grapes. This fact is supported by statistical analyses. Guerreau (1995) who is also not quoted, confirmed conclusions by Pfister (1984) that the grapevine harvest date depends on temperature from April to July. We could replicate this result from a stepwise regression of our new series, and also from the data given in Chuine et al. (2004).

SC 4: *The comment was accepted by two reviewers, but Keenan's reply was very aggressive and unscientific (and would not alter it), so that H. Grassl decided not to publish the exchange. In this comment, Chuine et al. demonstrated that Keenan was "off" and he had misrepresented their paper. Therefore, I consider that continuing to cite Keenan (2007) is promoting a fruitless controversy and this is not acceptable in Climate of the Past (or any scientific journal).*

REPLY: That the critique of Keenan was rejected by the editor of "Nature" is irrelevant. What matters, however, is the validity of his key argument that reads as follows: "The model-estimated temperature for 2003 is 5.86ºC (8.10 standard deviations) greater than the 1960–1989 mean temperature. For 2003, the observed temperature was only 3.50 ºC (4.84 standard deviations) above the 1960–1989 mean temperature. That is, the model overestimated the temperature for 2003 by 2.36ºC (3.26 standard deviations)". The observational temperatures were provided by Meteo-France. (Keenan 2007, p. 255). From para 3 it is concluded that August temperatures did in no way matter for the extremely early grape harvest in 2003.

SC 5*: The authors also seem to have a superficial knowledge of the content of Chuine et al. (2004). They state that "[: : :] the AMJJA temperature anomaly estimated for 2003 is 2.4 C higher than measured temperatures in Paris (Keenan, 2007)." (Conclusion section).*
The temperature reconstruction of Chuine et al. (2004) was made for Burgundy, not Paris. Moreover, the observed temperature in Paris in 2003 was certainly underestimated, because

the Montsouris trees (were the meteorological station is based) were watered during the heatwave. There is no watering or irrigation for grape in Burgundy.

REPLY: The reference to the Dijon temperatures is easily overlooked, because it is only included in the supplementary material on internet, whereas Paris temperatures are mentioned in the article itself. Dr. Yiou's remark about the alleged inhomogeneity of the Paris temperature series is irrelevant. Even considering Dijon temperatures, it has to be stressed that the temperature reconstruction according to your model was 2.36 °C higher in 2003 than it actually was measured in Dijon not considering the fact that August temperatures should not be included (see para 3). The observed April to July temperature deviation in Paris corresponding to the ripening process in this extraordinary year is only 2.5ºC above the 1960-1989, which well agrees with our new Swiss series Please let us know in detail whether and why we (Keenan, Wetter and Pfister) are wrong in this point and why you did include veraison into your model.

SC 6: *The relation between spring drought and summer heatwave is the object of many recent papers. Some of those papers point toward the necessity of a drought in southern Europe. Drought in central or northern Europe does not induce summer heatwaves (Quesada et al., 2012;Zampieri et al., 2009;Vautard et al., 2007). Do the authors have information on spring droughts around the Mediterranean basin? This would be an interesting added value to this paper.*

REPLY: To be precise: It is true that drought for itself does not induce summer heat-waves, but it tends to greatly exacerbate temperatures in the context of blocking anti-cyclonic situations. This is well known from many papers dealing with the 2003 heat-wave (e.g. Sereviratne et al. (2006), Fischer et al. (2007).
Indeed we do have information about a severe drought in autumn and winter, 1539/40 in (Northern) Italy and Spain, a point to be discussed in the next paper, where seasonal precipitation in Switzerland and Poland will be assessed. In this context, we will also deal with relationship between heat-waves and suppressed evapo-transpiration. In this context, we will, of course, relate to the Quesada et al. (2012) paper.
The extreme heat and drought in1540 is already described in Pfister (1999) which is quoted in the Chuine et al. (2004) article, though a link to this record breaking year is missing in the text (see also Glaser et al. 1999).

SC 7: *When looking at the reconstructions (figure 4), I am struck by the biases in the verification*
periods. This problem is particularly acute for extremely warm (e.g. 1824) or cold (e.g. 1778) years.

REPLY: Biases in the verification years 1778 and 1824 are approx. 0.6 °C which equals more or less the standard error of estimate (SEE = 0.52 °C). Nevertheless 50 year verification periods correlate between $R^2 = 0.6$ and 0.77 ($p = 0.001$) which is quite good.

SC 8: *The axes of the graphics are hard to read. There are a few basic rules in textbooks on "how to write scientific articles" (e.g. Day and Gastel, 2006, chapter 17) for axis labels, for which MS excel default options are not suited. For instance, showing increments every 9 units (1507, 1516, 1525, etc., in figure 6) makes the figure very unreadable.*

REPLY: Figures will be redone according to the instructions given by the anonymous referee. We have to state that MS excel default does not calculate wrong. Furthermore it was

not a problem to use Excel in our previous papers which have been published in Climate of the Past and Hydrological Sciences Journal, nor was the use of Excel criticised by the anonymous referee.

In the end, Dr. Yiou does not bring forward convincing arguments that might invalidate our main conclusion that 1540 was warmer than 2003. We are grateful for his indirect acceptance of our main point and his valuable critique.

References:

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Mullins, M. G., A. Bouquet, W., Larry E., (1992). Biology of the grapevine. Cambridge, Cambridge University Press.

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