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Interactive comment on "A collection of sub-daily pressure and temperature observations for the early instrumental period with a focus on the "year without a summer" 1816" *by* Y. Brugnara et al.

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We thank the anonymous referees for their very useful and detailed comments, their suggestions significantly improved the quality of the paper. Hereafter we reply to each specific comment of each referee, text in bold is copied from the reviewers' reports.

ANONYMOUS REFEREE #1:

The paper "A collection of sub-daily pressure and temperature observations for the early instrumental period with a focus on the 'year without a summer' 1816" by Brugnara et al. represents an interesting data rescue effort focused on the early instrumental period. It demonstrates that climate reconstructions based on instrumental data are still far to be considered a closed work, being the unexploited data stored in the archives probably more than those already used for the early instrumental period. The paper contains no sensational scientific results: the pressure probably is not the most suitable variable to highlight the uniqueness of the event in 1816 and it would be useful to give equal importance to temperature and precipitation data too (many data are available for both these variables in Europe). However, it is a fair job of data recovery that may attract the attention of the scientific community on the still open issue of early instrumental period and that, thanks to the supplementary material, provides excellent suggestions for an extension of the data rescue activity. For these reasons I suggest the present paper for a publication on Climate of the Past.

I have few corrections to suggest to the authors before the final publication.

Page 1746 lineas 26-27: "Another difficulty arises from data quality, in particular for temperature: the homogenisation with modern data is usually not an easy task". I do not completely agree with this sentence: recent results on the application of a wide set of homogenisation tools to a benchmarking dataset (see Venema et al., 2012, published on this same Journal http://www.clim-past.net/8/89/2012/cp-8-89-2012.html) proved that almost all relative homogenisation algorithms improved the homogeneity of the temperature data. In the present case, however, the digitization of few years instead of the complete temporal series does not permit the application of the most common homogenisation procedures.

As shown by Böhm et al. (2010), the homogenisation of early instrumental temperature observations requires an additional effort with respect to those made after ca. 1850. Moreover, in our paper we deal with sub-daily data, whose homogenisation requires more advanced techniques than for monthly means (e.g., Della-Marta and Wanner, 2006; Auchmann and Brönnimann, 2012). Even if we had complete temporal series, the homogenisation of the temperature observations for 1815-1817 with respect to modern data would be a very challenging task, nearly impossible for some records for want of metadata and reference series.

Page 1754 lines20-21: "We assumed all times to refer to local solar time, since official standardised times did not exist." Is this valid also for ship data? Please clarify this point.

We can be quite confident that it is valid for ship data as well. The reason is that the position of the ship was usually measured only once per day (using the elevation angle of the Sun for the latitude and a chronometer for the longitude): the most convenient time to do that is local noon. Most of the ship data have meteorological observations made at the same time of the coordinates observation, and the time indicated is always 12:00. This is now mentioned in the text.

Page 1756 lines 10-13: "At some observatories, however, the barometer hung in a heated room, in which case we will have an unknown error, usually with some seasonal cycle. Note that we rarely know the location of the barometer from metadata." It is not clear to me for which observatory the location is known and for which others it isn't, is it possible to specify the three possibilities (outside, inside unheated and inside heated) in table 1? For barometers located inside into a heated room the use of outside temperature probably does not produce better results than using a constant temperature value equal to the annual mean (assuming that heating maintain a constant temperature through the year).

Metadata are very limited in this sense, especially for those records which do not have the temperature of the barometer. Outside barometers are unlikely and we have only one case where metadata indicate an heated room: Brunswick (Maine). For this special case we actually assumed an arbitrary constant temperature of 18, similarly to what the reviewer suggested. The reviewer is right that this should be clearer from Table 1. In fact, thanks to his/her comment we realized that Table 1 wrongly indicates the use of climatology for that particular station. This was corrected introducing a new abbreviation (HR=heated room) and a sentence was added to Sect. 2.3.3. A similar mistake concerned the station of Quebec City: we wrongly indicated that climatology was used, instead the observations were corrected by the data source using outside temperature.

Page 1760 lineas 6-7: "To achieve this, we linearly interpolated all pressure observations to four daily equally-spaced time steps: 00:00, 06:00, 12:00 and 18:00 UTC." Why linearly? Daily cycle should be better fitted by a trigonometric function. Is this due to the low number of sub-daily observations?

The average daily cycle of pressure is small compared to day-to-day variability (some of our pressure records barely have the resolution to see it) and the differences by using a trigonometric function would be negligible, also considering that observations usually coincide with maxima and minima of pressure (early morning and afternoon, respectively). Perhaps some small improvements in accuracy (few tenths of hPa) can be expected for the 00:00 time step (06:00 for North America), which we do not use in the analysis.

We added a sentence on this in Sect. 2.3.7.

Page 1763 lines 1-3: replace "much lower variability" with "much lower spatial variability"; Figure 4 should be figure 7 (both at line 1 and 3).

Actually what we mean is the temporal variability (this is what Fig. 7 shows). It is however also true for spatial. We changed to “much lower spatial and temporal variability". Thanks for noticing the mistake in the figure number.

Page 1767 lines 7-10: this sentence should demonstrate that an average 2p.m. temperature of 19 for summer 1816 is representative of an heatwave, but its comparison with the absolute maximum of 34 over the 1800-1825 period does not convince.

What we mean is that 31 is a very high value for that station if compared to the summer average of 19. We rephrased this sentence to make it clearer. Note that after a more detailed inspection of the data, we no longer refer to the entire period 1800-1825 because of a possible inhomogeneity around 1812.

Table 1: move “Y = available, N = not available" into a parenthesis after “Loc = exact location (within 100 m) from metadata"; move “TB = temperature of the barometer, TA = outside air temperature, CL = outside temperature climatology, CO = observa- tions already corrected for temperature” into a parenthesis after “TCorr = data used for temperature correction"

Done.

In figure 4 a comparisons with other data-sets with available data for that period, such as the Berkley Earth Surface Temperature (available for Europe since mid 18th century) and the HISTALP data-set (available for the Greater Alpine region since mid 18th century), would be welcome to strengthen the conclusion of the authors.

Data in Figure 4 are from Dobrovolný et al. (2010), who used eleven homogenised temperature series for central Europe, ten from HISTALP plus the series of Prague. We are confident that this reconstruction, which was focused on Central Europe, represents the climate anomalies of that region better than large-scale focused datasets.

Ship data were presented but never used in the analysis. It would be interesting to exploit also these data, at least showing a case study that make use of them. Maybe those available around India could be useful to validate the hypothesis that a delayed summer monsoon caused late torrential rains there.

We agree that using some ship data would be interesting. The problem is that we recovered only a few records (marine data was not our main target), moreover we cannot correct the biases in pressure the way we did for land stations in Europe. We decided to write a small section before the Conclusions (Sect. 3.4) that describes and briefly analyses SST observations from a voyage from England to the Indian Ocean through Cape of Good Hope. Robust conclusions cannot be obtained from a single record, however this particolar record has the peculiarity of providing daily means (based on 12 observations at 2-hour interval) instead of instantaneous observations and allows some speculation on the distribution of SST anomalies in the tropics (in particular of a strong negative anomaly in the Indian Ocean in July/August 1816), which could have had an impact on the monsoons.

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ANONYMOUS REFEREE #2:

The study by Brugnara et al., "A collection of sub-daily pressure and temperature observations for the early instrumental period with a focus on the "year without a summer" 1816", presents another important step forward in making more (sub-)daily historical data available for the scientific community. Previous studies about the climatic and social impacts caused by the Tambora eruption provided already a broad description using monthly to seasonal mean climatic data combined with some documentary evidence on extreme weather anomalies for Europe. The current study advances our knowledge by providing (sub-)daily pressure and partly temperature data to better analyse weather patterns leading to extreme events in the post eruption phase. The paper is well written and the methods and steps taken to retrieve the historical data are described in a clear way including relevant equations. The analysis of some events presented in this study is still very limited due to a lack of more relevant temperature and precipitation data. Given the importance to make such historical data available, I nevertheless suggest publication in Climate of the Past with minor revisions taking into account very few points below.

Introduction:

Page 1745, line 28f.: Please mention here also the initiative at ECMWF for ERA-20C (e.g. Poli et al., 2013) for 1901-2010.

Added: "A similar enterprise was independently undertaken for the period 1900-2010 within the EU project ERA-CLIM (Poli et al., 2013; Stickler et al., 2014)".

Page 1746, line 13: Please refer here to the original source of the used data as

well and not only to the secondary reference, sth. like "based on the monthly reconstruction by Casty et al. (2005, 2007). . . xy shows. . ." (e.g. Luterbacher and Pfister, 2015). In the later part of the paper: To which extent are your results comparable/different to the previous monthly reconstruction, used pressure data and analysis by Casty et al. (2005, 2007) or Luterbacher and Pfister (2015)?

It was not the aim of this study to reconstruct monthly SLP or temperature that are comparable with other datasets. Since we corrected pressure data using the gridded SLP reconstruction of Küttel et al. (2010), monthly averages of our corrected dataset for Europe mimic that reconstruction.

A reference to Casty et al. (2007) has been added, but we preferred not to change the structure of the sentence.

Methods:

Page 1756, line 14ff.: I don't really agree that using a temperature climatology from 20th century reanalysis starting from 1871 is a good idea. 20CR does not assimilate any land temperatures and there is also a low coverage of assimilated SSTs in the early period. The problem is not severe here as it got only applied to few stations and the statistical correction in the final step might overcome some problems. It would be fair however to mention the potentially large uncertainty of the climatology due to the above mentioned points. Independent from using 20CR, I'm in general not sure if using such a climatology is a good idea (i.e. for continental climates and/or high altitudes). See also next point.

Page 1759, Eq (8): Using a long-term climatologic mean for daily temperatures can easily lead to $> \pm$10-15 K deviations relative to the unknown daily temperature. Are you sure that simply using the standard lapse rate a in Eq. (8) might

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not be more accurate on daily basis for unknown temperatures? In both cases, large errors (> 5 hPa) are easily possible for stations at high elevations (also the 20CR climatology based on a coarse grid will usually not capture the topography/elevation and a reasonable Ts here).

In our work the 20CR climatology is used for the temperature correction at only four stations: New Bedford, New Haven, Nuuk and Paris (1815 only). It is used at three additional stations for the reduction to sea level: Brunswick, Cambridge and Gdansk. All these stations are at elevations below 70 m. Climatology is also used to fill gaps in temperature observations, however this is a rare occurrence (ca. 0.2% of the total observations across the dataset, but none at stations above 300 m) because when temperature is missing, pressure is usually missing too.

It is true that continental climates can have large deviations from climatology on a sub-daily scale. On the other hand, it is unlikely that the barometers were exposed to extreme temperatures, therefore climatology might actually be a better solution than observed outside temperature in some cases. In either way, large errors are indeed possible on the single values. Research into this question and additional digitisation of 19th century data for Canada is ongoing.

We added a sentence on this in Sect. 2.3.3, and referred to it also in Sect. 3.1 and 3.3.3.

Page 1760, line 3: That there are no to little flags for stations at high elevation is surprising given the concerns above and my own experience to calculate their P0. Table 1 indicates that with exception of Madrid the records from high altitudes were already corrected in previous studies. Nevertheless, did you evaluate them here again and if so, what were e.g. the neighbours for stations > 400 m for estimating the differences with nearby stations and which standard errors are there for different seasons? Using Eq. (8), one degree difference for

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Ts leads already to an error of around 0.36 hPa for Hohenpeissenberg or 0.25 hPa for Madrid etc. when estimating P0. Temperature uncertainties of e.g. 10 K would lead already to 3.6 and 2.5 hPa deviation in P0. What was the specific problem with Madrid?

Quality control was made by looking for unrealistic SLP values and by comparing the interpolated 4-daily SLP series, independently from altitude and source. Only large errors that resulted in evident spikes in difference plots were flagged (e.g., digitisation errors). In some cases, clear inhomogeneities (i.e., large jumps in the mean pressure) led to the removal of long periods.

Concerning high altitude stations, all of them had in-situ air temperature observations available, whose errors are usually < 5 K (see Böhm et al., 2010). Madrid has a large data gap between August 1815 and December 1816, and before the gap the observations have sudden jumps not compatible with sorrounding stations. Probably the barometer had some problem and there had been some attempt to recalibrate it. Moreover, pressure readings in 1814-1815 have a very coarse resolution (ca. 3 hPa); pressure readings from 1817 are given in a different unit (Castilian inches – we forgot to mention this unit in Sect. 2.3.1, this has been corrected), clearly a different barometer was used. We only used data from 1817 in the analysis.

Note that, after further evaluation, we decided to discard the series of Växjö (southern Sweden) from the case studies. This was the record with the largest number of flagged observations and with the largest statistical correction (-15 hPa in winter). A sentence was added to Sect. 3.1 to explain this choice.

Results:

Page 1762, line 12: Why not use MERRA here like in section 3.3.1 instead of 20CR?

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Higher spatial and temporal resolution offered by MERRA are not really necessary here, since we are dealing with a large scale feature. In Sect. 3.3.1 we used MERRA to estimate the climatological daily cycle of pressure, which can strongly depend on local conditions and requires hourly data to be properly resolved.

Moreover, for consistency with the rest of the paper we now use the 1961-1990 climatology, which is not available from MERRA.

Page 1763, line 1+3: should be Fig. 7 in both cases.

Thanks.

Page 1763, line 5: Would remove or explain “surprisingly" here e.g. with respect to Fig. 5.

Changed to “The summer of 1817 has an higher variability in Europe than that of 1816. There are indeed indications that the summer of 1817 was also a very wet season in central Europe, although not particularly cold (see Fig. 4); in Geneva, for example, it was one of the wettest summers of the period 1799-1821, that of 1816 being the wettest (Auchmann et al., 2012).”

Page 1764, line 24f.: Add sth. like “. . .according to Eq. (8). . ." and “. . . about 2.5 hPa at low altitude and less for higher station elevations"

Now reads “according to Eq. 8, considering a standard atmosphere, an uncertainty of 20 m (which applies to most stations) results in an uncertainty in SLP of about 2.5 hPa near the sea level, or less for higher elevations."

Page 1767, line 17f.: A brief qualitative comparison of your reconstruction with

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the maps for July 1816 shown in Luterbacher and Pfister (2015) based on the Casty et al. data would be nice here.

Most of our records are too short for such a comparison. Those long enough are the same used by Casty et al., with the only exception of Turin.

Page 1772, line 6: Although the focus is clearly on retrieving the historical data, I would suggest to add some concluding sentences here how your results/examples agree and advance earlier studies mentioned in the introduction/literature about the climatic features following Tambora.

New sentences have been added to the conclusions, including one that refers to a new section on the paper (SST analysis, Sect. 3.4).

Figures:

Figure 1: Although mentioned in the figure caption, a colour bar for the time would be helpful given the long time span.

A colour bar was added.

Figure 4: The anomalies relative to 1801-1830 are difficult to understand from today's perspective. It would be helpful for the reader to indicate also the deviation from more recent temperatures in addition (or mention the difference of the historical mean from the recent climatologic mean somewhere in the text).

We added the anomalies from 1961–1990 in the figure. To have more consistency in the analysis we used this period as modern climatology where possible (e.g., Fig. 5-7, 14).

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Figure 5: Lon and Lat info is much too small.

Map axes in Fig. 5 have been removed because they badly affected the legibility of the maps.

Figure 6: Numbers of the colour bar are too small. Lon and lat info might be helpful for the maps. Please use an increment of 0.5 hPa for isolines in 6a to be consistent with increments in b and c.

Fig. 6-7 have been redrawn using different graphics and projection.

Figure 9+10, 12+13: The colour differences are hard to see for 5 hPa intervals. A less continuous colour bar could be useful here if possible.

We changed the colour palette as suggested. In addition we adjusted the distance power parameter used by the inverse distance interpolation algorithm so that isobars better represents actual observations in case of large gradients (e.g., in the Alpine region).

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ANONYMOUS REFEREE #3:

This article by Brugnara et al. presents a valuable new set of historical subdaily pressure and temperature observations spanning the US and Europe for an early period of the 19th century. After presenting a detailed description of the data preparation, the authors focus their attention on the years following the famous Tambora volcanic eruption, using the recovered data to explore case studies of synoptic scale circulation and temperature patterns experienced from 1815 to 1817.

The authors have clearly done a lot of work in collating and preparing the early instrumental observations for analysis. The level of detail that they describe will be very helpful for future historical climatology studies. The exploration of the 1815 to 1817 extreme events are also make for fascinating reading, and provides another timely example of the value of historical data.

Overall, I enjoyed reading this paper, and believe that it is worthy of publication in Climate of the Past, subject to some minor revisions.

General comments.

1. I think that more could be said about the new data provided by this work that lies outside the 1815 to 1817 period. This could be provided by adding some explanation at the start of section 3 on why the post-Tambora period is being studied, or in the concluding remarks.

We added a couple of sentences in the conclusions to meet the reviewer's suggestion.

2. Several of the figures would be much easier to understand if they were re-

created using different colour schemes.

We used a new color scale for Fig. 9–10, 12–13.

3. A lot of corrections and adjustments are made to the pressure observations. This is clearly important, but the relative size of each adjustment is not always clear. Additionally, I am unsure as to how the first round of data processing compares to the statistical corrections applied. If feasible, it might be worth creating a schematic, that outlines these steps and clarifies the average size/importance of the adjustments made, or provides an example from one source.

It is difficult to generalize, the magnitude of the different corrections depends very much from the station's characteristics and from the time of the year. Showing an average of the contributions would be somewhat misleading. The reduction to sea level is obviously dominant for inland stations (up to 127 hPa at Hohenpeissenberg in the Alps), while the temperature correction becomes particularly important (up to 6 hPa) in summer, but also in winter for continental climates (the use of outdoor temperature for the correction probably leads to large errors there, see also Fig. 3). The statistical correction affects more the stations with insufficient metadata. In general, not considering the reduction to sea level, the large majority of both physical and statistical corrections are within ± 5 hPa.

Below are some specific comments, as well as some technical suggestions to help improve readability.

Specific comments

Pg 1747, final paragraph: This section seems a bit disjointed and specific, com-

pared with the rest of the introduction. It might be better to move this information to sec- tion 2.3.3. An overview of the paper structure would be a better way to conclude the introduction.

We moved the paragraph to the end of Sect. 2.2 (temperature is now mentioned in this section's title) and replaced it with a description of the paper structure, as suggested.

Pg 1749, final paragraph: I understand why you removed the 1816–1817 Paris data from the University of Barcelona, but how did these removed observations correlate with the data digitised by the University of Bern? Even giving a correlation coefficient would give future data users more confidence in the full 1811-1820 series.

They are exactly the same observations, but those from the University of Barcelona are not corrected for temperature. Thus the correlation is nearly 1, to be precise 0.99. We rephrased the paragraph to make this clearer.

Section 2.2. Please provide a couple of references to support your comments about the history of barometers. Alternatively, expand the sentence referring the reader to Middleton (1964) to indicate that you obtained all of this information from his work.

We added a sentence at the beginning of the section.

Pg 1752 Eq 1: Where did this equation come from?

We added a reference to the text.

Pg 1755 L13: Why did you choose 2pm for the assumed local observation time?

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We removed the sentence because such situation actually applied only to the early stages of the data rescue. Eventually, with a more careful analysis of metadata, it was always possible to assign at least a qualitative time to each observation.

Section 2.3.4: Can you estimate the average difference that the correction to local gravity made on the pressure readings?

We added a sentence on this.

Similarly, can you provide some basic statistics on the impact that the interpolation step had on the data? It would be good to know the range of adjustments made during this stage. Also, how did you linearly interpolate data for stations that only had one observation per day?

We added a sentence on the statistics for each of the four time steps. As expected, the mean differences from the observed values has a maximum at 0 UTC and a minimum at 12 UTC.

When only one observation per day is available, we just interpolate between two consecutive days. For example, if 1000 hPa are measured at 09:00 UTC on day x and 995 hPa at 09:00 UTC on day $x+1$, the interpolated values at 12:00 UTC on day x will be $1000 + (995 - 1000) \cdot 3/24 = 999.4$ hPa (note that values at 18:00 and 00:00 are considered missing in this case).

Section 3.3.1. Did you re-interpolate the statistically corrected data back to the synoptic hours mentioned in section 2.3.7, or apply the adjustments to the already interpolated values? This is not clear.

The latter. We added a sentence to clarify this. Note that since the interpolation is linear, the two approaches lead to identical results for nearly all values. The only

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exceptions (max 8 per year) are those values at the transition between seasons.

Pg 1764 L12: What is the resolution of the 20CR?

Added in Sect. 2.3.3.

Section 3.3.3: The tenses are a bit confusing in this section.

Changed present to past in a few sentences.

Section 4, second last paragraph: I think it is a good idea to remind the reader of the supplementary section here. Most of the analysis is only on 1815–1817, but you are actually providing a lot more data to the ISPD.

See the general comments.

Figure 1: If you can, it would be helpful to add an inset map showing some of the places mentioned in the text (Exeter, Paris, Ylitornio, St Petersburg etc), for non-European readers. The colours of the ship routes are also very confusing. Maybe try a graded scale instead (from light to dark).

We added an inset map of Europe showing all places mentioned in the text (many of them are probably not so familiar even for European readers), and a legend for time.

Figure 6: Define SD in the caption, as well as the text.

It was an editorial choice to replace “standard deviation" with an abbreviation.

Figure 8: Could you specify the stations that have the largest corrections? It

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might also be interesting to explain these differences in the text.

In the revised version we printed on the plot the names of the stations with mean absolute corrections larger than 5 hPa, and also added a few comments in the last paragraph Sect. 3.3.1 (although metadata is usually insufficient to explain the source of the errors for the single stations).

Figures 9-10, 12-13: The colours used are very difficult to distinguish. Have a look at http://colorbrewer2.org/ and see if you can use a diverging scale that can be more easily understood.

This was indeed a much needed improvement. We also improved the interpolation employed to draw the isobars.

Technical comments

Pg 1744, L23: 'the first', rather than 'a first'

Pg 1745, L4: 'Some of these professionals', instead of 'Some of them'

Pg 1745, L6: 'universities', not 'university'. It would also be good to see a reference here, for those interested in reading more on the role of meteorological observations and the role of the upper class. Maybe Golinski (2007)?

Pg 1745, L6: 'begun' not 'began'

Pg 1745 L8: 'on board', not 'on board of'

Pg 1745 L9: ’French Revolution’ is capitalised

Pg 1745 L14: A reference here would also be nice.

Pg 1745 L28: Add reference to Cram et al. (2015).

Pg 1746 L29: Perhaps say ’does not require such specific exposure’, instead of ’does not have to be outside’. Also provide a reference for this.

Pg 1747 L4: add ’historical’ before ’observations’ just to clarify.

Pg 1747 L8: Similarly, I would add ’in the early instrumental period’ after ’most of the data’.

Pg 1748 L6: add ’and’ after the University of Bern

Pg 1748 L9-10: ’with the exception of a few stations’

Pg 1748 L24: ’give an idea of’, not ’give an idea on’

Pg 1751 L8: ’among’, rather than ’inside’

Pg 1751 L9: ’exposure’ rather than ’exposition’

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Pg 1753 L12: ’of a few year’, rather than ’of few years’

Pg 1756 L27: Add a comma between ’summer’ and ’differences’

Pg 1762 L1: Do you have a more recent reference for storm track analysis? Maybe Matulla et al. (2008)?

Pg 1763 L7: ’suspicion’ rather than ’suspect’

Pg 1763 L11: I suggest rewriting to read ’...among the variability of most of the series, the lack of metadata...’ or something similar. The current sentence is hard to understand.

Pg 1764 L2: ’the data are’, rather than ’the data is’

Pg 1764 L20: ’relative’, not ’relatively’

All these very appreciated technical comments have been applied in the revised version.

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