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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of vernal equinox solar eclipse on temperatures and wind directions in Switzerland" *by* Werner Eugster et al.

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We copy in the reviewer's comments and critique in blue and provide our response in black.

Reviewer: This paper reports the effect of the solar eclipse in March 2015 on a network of measurement sites in Switzerland. The effects of topology are relevant for this region, and this is probably the most comprehensive study of eclipse meteorology over a multi-altitude network to date. The authors seem particularly interested in comparing two versions of the "cold cored cyclone" as presented by Clayton and modified by Aplin and Harrison, since the trajectory of the 2015 eclipse makes Switzerland ideal for such a test. Altogether this is a thorough and competent study, at a higher standard

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than many eclipse meteorology papers, and I am happy to recommend publication with some minor revisions.

Thank you very much for this positive assessment.

Reviewer: The main concern I have is to do with the structure of the paper. The authors present their data analysis methods before describing the data analysis itself and this makes for a disjointed read. For a journal that doesn't use a "methods" section like ACP I would recommend moving the specific analysis techniques to the section on, for example, analysing temperature effects (or whatever it is).

This can be done. We suggest to move the contents of Sections 2.3–2.8 in the discussion version to the respective paragraph where the results are presented (this is what this reviewer recommends in the detailed feedback below).

Reviewer: Occasionally the data analysis decisions do not appear to have any theoretical basis, for example, the choice of a gamma distribution for the temperature changes, and perhaps also the diurnal variation in the diffuse fraction. The gamma distribution is justified by the authors because it permits others to see their measured temperature changes in context, however this could be achieved with a cumulative probability distribution to all the data, without assuming a shape for the curves, so I am not sure what the gamma distribution really brings here. In general, the use of a purely empirical approach may not be a problem in itself, but the authors should state that this is the approach taken and explain why.

Using parametric distributions in statistics, such as the Gamma distribution, has many benefits, but the reviewer is correct that there is no extremely firm theoretical basis for such a statistical approach. The Gamma distribution has a wide range of applicability, and also covers the special case of an exponential distribution. Thus, we believe that this is a good starting point for readers who do not want to use lookup tables to provide a probability estimate for a given temperature drop measured anyware to compare a

new measurement with the existing ones.

We however see the reviewer's point and suggest to use the empirical cumulative distribution estimates in Table 3. In our discussion version these values could be calculated from Eq. (2) with the parameters given in Table 2, and thus are somewhat redundant information. With the suggested changes we would have distribution-independent information in Table 3, which is certainly an improvement.

Reviewer: The figures are generally of good quality but occasionally the captions should be edited so that the figures can be understood without reference to the main text.

We will revise the captions accordingly.

Reviewer: The caption to figure 2 was particularly obtuse from this point of view.

Originally, the individual panels were separate figures with relatively long captions. With the aggregation to one figure with five panels, we had to reduce the caption length and thus information content. Obviousely we shortened the text too much and are happy to expand it in the revision to make this figure better understandable independently from the main text.

Reviewer: In Figure 8 I didn't understand why and how the probability was used – shouldn't this be explained in the main text, if it is really needed at all.

The basic principle of statistical comparisons is to compare a given result (i.e. our measurements) with a potentially fully random result. As mentioned in the caption we used the uniform distribution (i.e., each wind direction change is as likely in a random system) for comparison. If our measurements do not deviate from such a random outcome, the ΔProbability value is 0.00; if it is > 0, then our measurements indicate higher probability during solar eclipse than what we would expects in a random system (and if it is < 0, the probability is lower).

We will find a better way to describe this. We assume that using the term "probability" in the blue text items on the figure was confusing this reviewer. Of course one can always also debate on what a random outcome would be (we assumed uniform distribution), but we do not interpret this feedback in the way that this assumption was questioned.

Reviewer: And on Figure 10, I (personally) think wind vectors would be a clearer way to indicate the change, which would then fit better with your figure 1. The use of colour to indicate flow directions is not intuitive.

The key issue is the following: if a wind vector is presented, most readers confuse the angle of the vector with the geographic direction of wind (see example in Fig. 1 below). It is almost impossible to present wind direction **differences** in the same way as absolute wind directions are presented. That's why we used symbols with colors to represent the wind direction **differences** on these panels. In general the blue–red gradient is widely used in meteorology to show negative–positive deviations from something (e.g. temperature anomalies). What we could do is to simplify the color scheme to only use the blue–red gradient with white at zero difference instead of the rainbow-color-gradient currently used in Figure 10 to be more intuitive with our color scheme.

Initially we of course hoped to find a way to subtract the local influence on wind direction from the measurement in a way that would leave us with the synoptic large-scale wind direction, but this proved almost impossible in the complex terrain of Switzerland; even the low-laying parts which are ±flat actually experience channeled flow (as shown by Wanner and Furger, 1990, cited in our paper).

Reviewer: P1 L3-4 This sentence is confused between eclipse meteorology and the broader scientific benefits of studying eclipses.

(actually on P2) We'll revise the text to eliminate this confusion and separate the two aspects more clearly.

Reviewer: P1 L34 Should this be 1600km?

(actually on P2) Yes, this was a typo, thank you for making us aware of it. Corrected.

Reviewer: P2 L3-4 This sentence is ambiguous about whether a total or partial eclipse was seen at the two quoted locations. I believe the 1999 eclipse was total over south west England which would imply it was 97% at Reading and perhaps total at the other location, but please check and clarify.

(actually on P3) We'll reword. See also our explanation of the confusion between partial and total eclipses below. A partial eclipse by definition is an eclipse that has no location on the Earth where totality can be seen. Thus, we need to be more clear about partial occultation during the event of a total eclipse vs. partial occultation of a partial eclipse. This will be reworded.

Reviewer: P4 L26 I am not sure what you mean by "model" - are you simply referring to the loess fitted values?

Yes, this is the model fit. We will revise the wording to avoid potential misunderstandings. In statistics a "model" is everything fitted to the data which goes beyond the data themselves, but the term is not used in this way everywhere; e.g. "modelers" using global circulation models try to make the separation between "model" as the whole system and "modules" or "algorithms" for simpler statistical and prognostic model components. We will most likely refer to the "fit" in our revised version to avoid the confusion with the term "model".

Reviewer: P4 L28 is "instationarities" a proper word?

The proper word is probably "non-stationarity" but we will double-check with an expert in both English and time-series statistics for the revisions.

Reviewer: P5 I recommend moving most of the material on this page to the sections

where you actually discuss each effect, as explained above.

This refers to Sections 2.3 to 2.8 in the discussion version of our paper. The suggested reorganisation can of course be done. We will revise our manuscript accordingly.

Reviewer: P5 L18 Can you explain what this does so that people who don't use this particular software are able to reproduce your work?

Yes, we will expand this text and provide an additional general reference to bootstrapping (which can be done in many ways, but maybe the term is not yet as widely known as we thought). In short, nonparametric bootstrapping is a computer-intensive method to obtain a best estimate for statistical uncertainty (e.g. the 95% confidence interval as we do it here) by performing many simulations with subsets of data records randomly selected out of all available records. In this way uncertainty related to individual outliers or extreme values in a given dataset are becoming less important for the uncertainty estimate, and thus the uncertainty estimate obtained via nonparametric bootstrapping is a rather robust and reliable uncertainty estimate. We will explain this in more detail in the revised version.

Reviewer: P6 L4 Why gamma? (as discussed above)

See response above with our suggestion for the revisions.

Reviewer: P6 L7 Both SE and SD are used for errors in this paper, can you be more consistent?

The meaning of SE and SD is not the same, hence we use either or depending on what the context is: SE is the standard error of the mean and describes the uncertainty of the mean. SD is the standard deviation and describes how far a way from the mean a single observation lies. To obtain SE for the mean of a time series we would have to correct for serial autocorrelation, which is another confusion that many readers (even those with adequate statistical background) normally have, and hence we avoided to

go into this aspect of serial autocorrelation by simply using the purely descriptive SD (e.g. for difference in short-wave radiation).

We can of course modify our text and consistently report SE also for the time series.

Reviewer: P7 eq 4. Is this another example of an entirely empirical fit, or is there some reason why the diffuse fraction varies with time during the day that is not explained?

Yes, this is an empirical fit. Unfortunately, the sky was not perfectly cloudless during the eclipse. That the ratio between diffuse and direct radiation is a function of solar elevation angle is well known, but we should have explained this in the text. Here we used Δt (time difference from local noon) for simplicitly, but we probably should better first calculate the solar elevation angle and then use this as the independent variable instead in order to be more physically-based. The parameter estimates would still be empirical best fits, but with elevation angle instead of Δt as the independent estimate.

Reviewer: P7 L7 Explain image analysis here rather than in the methods

This will be done.

Reviewer: P7 L17-20 Are you effectively working out the long wave albedo here? And if so, would it help to say that?

According to Glickman, T. S. (ed.) (2000), Glossary of Meteorology, American Meteorological Society, http://glossary.ametsoc.org/wiki/Albedo the definition of albedo is: "Albedos commonly tend to be broadband ratios, usually referring either to the entire spectrum of solar radiation, or just to the visible portion." This does not include long-wave radiation and thus we do not think that a ratio between back-radiated long-wave radiation (which is a fraction of the long-wave radiation emitted by the Earth surface, not a radiation component from the sun) and emitted long-wave radiation should not be termed "albedo". We however realized that the term is used in some papers and textbook, hence we suggest to write about long-wave radiation balance and add the

term “long-wave albedo” with quotes and in parentheses.

Reviewer: P7 L31 explain bootstrapping here rather than in methods section

This will be done.

Reviewer: P8 L4 would it help to compare the temperature changes in the literature for partial versus total eclipses, even if it is just to show there is no real difference?

This comment is fully understandable and we initially also struggled with terminology. The astronomical terminology uses the terms “total”, “partial” and “annular” eclipses. With all three there can be “partial occultation” (as we call it), but in the case of a partial eclipse there is no location on the earth with totality. The theoretical differences in solar short-wave radiation remaining is small between total and annular eclipses, and partial occultation of a given fraction at a site should not depend on the fact whether an eclipse is partial or total. In our understanding much of the temperature drops reported in the literature are strongly affected by cloudiness during the time of observation. This means that if we were to dwell more into analysing various factors we would have to separate the effects of total vs. annular (only very few reports) vs. partial eclipses as a function of degree of occultation and cloudiness. Information on cloudiness is however in most cases not a quantitative information that could be easily used for such an investigation and hence we decided not to add such an analysis.

Reviewer: P8 L6-8 See comments above

We will move the information from Section 2.8 here and show empirical cumulative distribution estimates in Table 3 instead.

Reviewer: P8 L15 Can you take a couple of sentences to explain the normal diurnal variation in the mountain valley winds? This seems a unique local meteorology that not everyone will be familiar with.

Yes, will will do this. Most likely other readers will also benefit from a short introduction on mountain valley wind systems.

Reviewer: P11 L18 Annular eclipses don't cause full occultation, and in terms of the meteorological effects are analogous to partial rather than total eclipses.

We will reword. We did not claim that the effects were the same, but we wanted to express that most literature reports on temperature drops are either from total eclipses, or from annular eclipses, whereas reports on temperature drops from partial eclipses are rare. We'll carefully revise to avoid potential confusion in our statement.

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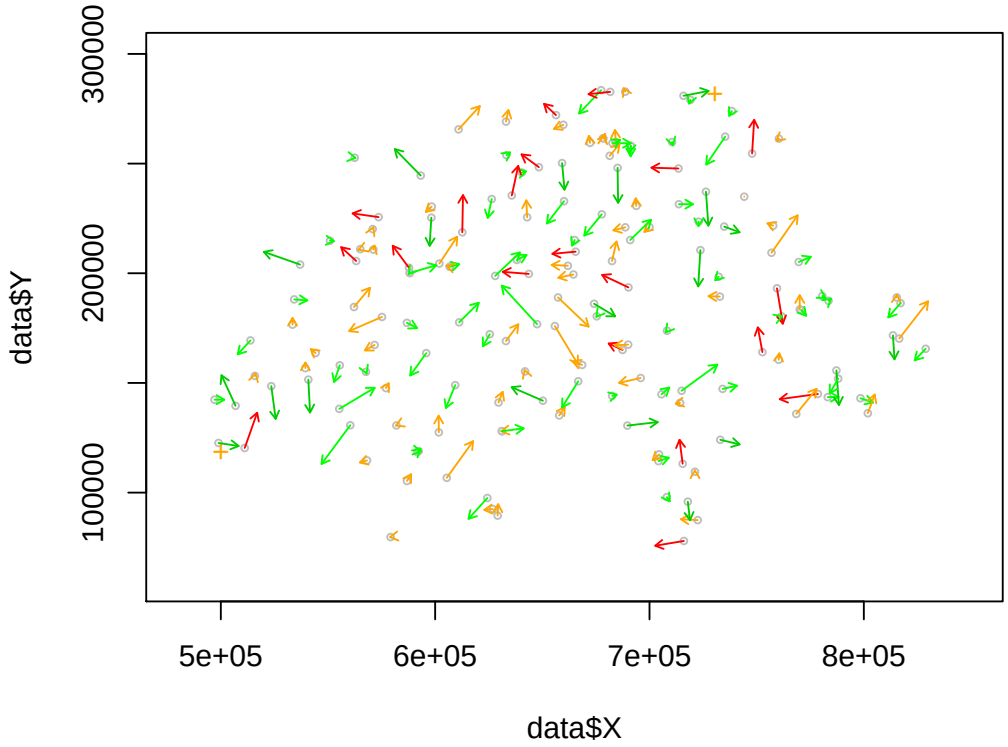

Fig. 1. Example how absolute wind direction vectors would look like.

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