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Comment on "Weekly precipitation cycles? Lack of evidence from United States surface stations" by D. M. Schultz et al.

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[1] Schultz et al. (2007) (hereinafter referred to as S07) recently published the results of their search for a weekly cycle in U.S. rain-gauge measurements, claiming that their null results contradict those of a study (unpublished at the time) by Bell et al. (2008) (hereinafter referred to as B08) of satellite and rain-gauge data. S07 failed to note 1) that the satellite results described by B08 were for the years 1998 and after, whereas their data ended in 1992, and 2) that the analysis by B08 of rain-gauge data for the years studied by S07 were consistent with their conclusions. B08 in fact show that the weekly cycle in rainfall over the SE U.S. becomes detectable after about 1990. We discuss the methods used by S07. We suggest that a more focused approach—if guided by physical theory—can extract far more useful information from a dataset than generic statistical searches such as described by S07. **Citation:** Bell, T. L., and D. Rosenfeld (2008), Comment on ''Weekly precipitation cycles? Lack of evidence from United States surface stations'' by D. M. Schultz et al., *Geophys. Res. Lett.*, *35*, L09803, doi:10.1029/2007GL033046.

1. Introduction

[2] Given the lack of a complete physical model of the effects being looked for and the statistical problems inherent in dealing with precipitation data, it is perhaps not surprising that reports on the results of searches for a weekly cycle in precipitation are in aggregate rather confusing. Examples of recent studies include *Bäumer and Vogel* [2007], *Schultz et al.* [2007] (hereinafter ''S07''), and *Bell et al.* [2008] (hereinefter ''B08''), where many references to earlier work can be found. Interpreting the results of searches for weekly cycles at different times and places can be perplexing, especially if one assumes that weekly cycles are the same wherever they are present. (There are now good physical reasons to believe that the changes in precipitation caused by aerosols will vary quite a lot with time and place.) In addition, some published research includes little or no statistical analysis of the data, especially in earlier periods when computers were less powerful; the results can be hard to evaluate because it is hard to tell how much the results are muddied by noise from small-sample effects. Some research uses statistical methods that are not really appropriate to the problem. Rainfall statistics deviate considerably from the assumptions made in many standard statistical software packages.

2. The Study by *Schultz et al.* [2007]

[3] Potential sources of confusion that are easily rectified appear in the publication by S07. S07 examine records for 219 U.S. rain-gauges (including Alaska and Hawaii) for the 42 years 1951–1992. They contrast their results with those of B08 (which, oddly enough for *GRL*, was not in print when *GRL* published the paper by S07, and so was unavailable to *GRL* readers), concluding ''... we did not find significant differences in the amount of rain at any of the stations for just the summer months (June, July, August), contradicting the results of Bell et al. (submitted manuscript, 2007) [*sic*].'' They report similar contradictions for rain occurrence. In both these cases, S07 fail to mention something important: the Tropical Rainfall Measuring Mission (TRMM) data used by B08 start in 1998. The rain-gauge data used by S07 end in 1992. The results they are comparing are based on datasets having zero overlap in time.

[4] B08 did, however, also analyze rain-gauge data for 1901–2005. They found that the weekly cycle in the gauge data during the years when there are satellite data is reasonably consistent with the weekly cycle in the satellite rain estimates. S07, however, fail to note that B08 found that a weekly cycle is undetectable in the period that overlaps with that examined by S07.

[5] The ''contradictions'' reported by S07 are nonexistent. We are unclear why S07 did not mention these points, since all of our circulated manuscripts included this information.

[6] It is true that the lack of a weekly cycle before about 1990 that S07 see in their U.S. gauge data, also reported by B08 for the SE U.S., is disturbing if one assumes that the weekly cycle is the same everywhere and at all times. B08 offer a possible physical explanation why the nature of the weekly cycle might change with time: the well-documented change over the decades in the concentrations of particulate types over the U.S. This ''explanation'' is a plausible conjecture, not a fact. Validating it will require considerably better understanding of how different aerosol types affect storm development, and how their concentrations varied in space and time over the last century.

[7] B08 offer physical reasons why a weekly cycle, if present, should be different in the summer from other seasons, why it should be different in the western and eastern U.S., and why it should be different over land and over the nearby Atlantic. The concerns S07 raise about differences among other researchers' results for the weekly cycle in coastal regions [*DeLisi and Cope*, 2001], the western Atlantic [*Cerveny and Balling*, 1998], and Germany

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[*Bäumer and Vogel*, 2007] are all generated by this same questionable assumption that a weekly cycle is the same everywhere. Conflicting analysis results in different times and places are not in themselves evidence that the results are incorrect, because enhancement of rainfall at one place requires a compensating effect somewhere else in time and/or space.

[8] S07 raise questions about the work of *Cerveny and Balling* [1998] (hereinafter referred to as CB98) a number of times in their paper. CB98 detected a weekly variation in satellite-estimated precipitation over the Atlantic off the east coast of the U.S. CB98 also noted that there was a weekly cycle in ozone and carbon monoxide measurements taken on Sable Island in the Atlantic. S07 suggest that CB98 are invoking "questionable causal links" with the weekly cycle in precipitation because ozone and carbon monoxide are "not aerosols". CB98 explicitly state that they use these gases as "indicator[s] of pollution transport into the North Atlantic Ocean"—a reasonable assumption, in our opinion.

[9] S07 also raise questions about the results of CB98 because *DeLisi and Cope* [2001] found no signs of a weekly cycle in rain-gauge data from sites near the U.S. east coast. As discussed above, there are good physical reasons to suppose that, if there were a weekly cycle at coastal stations and over the nearby ocean, they would be different. The fact that CB98 and *DeLisi and Cope* [2001] see different weekly cycles does not, *per se*, invalidate either of their results. In fact, B08 found that a weekly cycle in TRMM-era data over the non-coastal SE U.S. appeared to be reversed over the nearby Atlantic relative to the inland SE U.S. rainfall cycle (they propose a physical mechanism to explain this), suggesting that the coastal stations in between might well display ambiguous weekly cycles. (Note, however, that the data used by *DeLisi and Cope* [2001] and CB98 cover periods earlier than TRMM.)

3. Statistical Tests

[10] A number of S07's tests appear to require examination of statistics derived separately for each rain gauge. If one thinks of S07's goal as one of climate-change detection (a weekly cycle in rainfall is, after all, just a rapid and repeated climate change), one realizes that a number of problems with such tests that have been encountered in climate research may be lurking in the statistical approaches followed by S07. Addressing the issues we mention below would probably only reinforce the null result S07 obtained, since these issues suggest modifications that would require even stronger weekly signals for the signals to be detected. The issues should, however, be kept in mind if the methods advocated by S07 are applied to other investigations.

[11] One problem, described in detail by *Livezey and Chen* [1983], involves corrections to be used when one is applying a statistical test to many different sites. If a test at one site has, say, a 5% chance of indicating a spurious, accidental anomaly under the null hypothesis, then one might expect tests of n independent sites to yield $0.05n$ spuriously anomalous results [more, actually; see *Livezey and Chen* 1983]. One must take this phenomenon into account in searching for signs of a climate signal in multisite test results. It is not clear to us how S07 address this problem. (It is worrisome, in fact, that S07 find far fewer stations exceeding the 0.90 confidence level than would be expected by chance.) Corrections like those suggested by *Livezey and Chen* [1983] are necessary (and large!) and should be included in any approaches modeled after those of S07.

[12] A second problem that often complicates climate-change detection is data dependence. Rain data are both spatially and temporally correlated. Long-term averages of rain-gauge data are correlated over hundreds of kilometers (e.g., regarding monthly averages [see *Morrissey*, 1991]). It is unclear to us how S07 dealt with the effects of data dependence in their calculations.

[13] A third lesson learned in climate-change research is that one can improve the detectability of climate change by appropriately averaging the data over many sites instead of examining each site separately. This is because, in general, the greater the number of statistical tests one performs, the bigger a signal has to be in order to be declared significant [e.g., *Livezey and Chen*, 1983]. Searches for climate change that use averages based on physical insight, such as is provided by climate models, are much more powerful than searches that look for statistically significant changes at a multiplicity of sites. *Hasselmann* [1979] and *Bell* [1986] provide helpful discussions of this issue. It is why "global-average temperature" is discussed so much.

[14] Based on physical reasoning, B08 used averages of satellite and other data over the noncoastal SE U.S., for the summertime only. A weekly cycle was evident in the averages. Based on the physical insight that aerosols should affect storms maximally during the most convectively unstable part of the day, it was expected that the signal should be even stronger if only afternoon data were used in the average, and this was indeed the case: the statistical significance of the afternoon signal was far higher than for the 24-hour daily means. B08 note that the weekly cycle of morning rain is somewhat reversed from that of afternoon rain, perhaps in compensation for the additional midweek release of convective instability in the afternoons. This, and the reversal of the weekly cycle over the nearby Atlantic, remind us that the same driver can have different dynamic responses in different times and places (a conjecture entertained by *Bäumer and Vogel* [2007]). Averaging, unguided by physical theory (e.g., using 24-hour daily averages), can obscure rather than enhance the weekly signal. It probably explains why S07's Figure 1 is so uninformative.

[15] Our point is that by taking into account current physical understanding of how aerosols might affect storm development, B08 were able to construct averages of the data that emphasized where and when the expected signal should be strongest. (This should be done a priori.) Because S07 examined all sites and times indiscriminately, mixing data from times and places where no weekly cycle is likely (contributing only "noise" to the statistics) with data where weekly cycles might conceivably have existed, the power of their statistical procedures to detect a weekly cycle is much lower than the statistically focused approach used in B08.

[16] For this same reason, the assertion by S07 that their statistical methods are more powerful at detecting a weekly cycle than harmonic analysis (fitting to 7-day sinusoids) is incorrect. Fitting the data to a 7-day sinusoid in effect makes the assumption that rainfall is higher for a 3–4-day period, and then lower for the next 3–4-day period. Harmonic

analysis replaces the testing of each of the 7 days independently with a test of this more restrictive assumption, and, if the real weekly cycle is somewhat like what is implicitly assumed by the sinusoidal fitting—which is physically likely—the harmonic analysis is a more powerful signal-detection device than the kind of day-by-day multiple testing recommended by S07.

[17] For all these reasons, even if S07 were to analyze gauge data contemporaneous with the satellite data examined by B08, they would probably report seeing no signs of a weekly cycle, especially if they were to modify their tests to incorporate the needed corrections for multi-site statistical testing and data dependency.

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