

# Climate Audit

by Steve McIntyre

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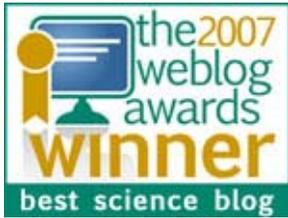
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Sunday, May 27th, 2007 at 4:36 pm

## [More ISO-2000 Weather Stations from Jones and Hansen](#)

By Steve McIntyre

[Anthony Watts](#) writes:

I decided I'd drop some more fun with entropy your way. Here is the USHCN station in Redding, CA # 425725920010 now operated by the US Forest Service at their HQ located at the Redding Airport it used to be operated by the NWS, but that WSFO closed in the mid 90's.

Like Marysville, the site is surrounded by asphalt, and the surface is unnatural - its wood chips over weedmat, and I'll have to say it was hot as hot to walk on.

But the kicker is the "accessories" they've added for convenience of running the hygrometer and for night observations. Yes is another fine high-quality USHCN site. I wonder how many times they forgot to turn off the light? Its a 65 watt bulb by the way.





This entry is filed under [Surface Record](#). You can follow any responses to this entry through the [RSS 2.0](#) feed. You can [leave a response](#), or [trackback](#) from your own site.  
232 Responses to “More ISO-2000 Weather Stations from Jones and Hansen”

[1](#)

[paul](#) says:

May 27th, 2007 at 5:00 pm

Thank you Anthony for the best laugh I have had for a long time. Would it be that the public had any idea what the “consensus” of the IPCC is signing up to. This photo alone should be

enough for everybody to question the validity of the science behind the IPCC.

It matters not if you are for AGW or agin it the data is crap either way.

[2](#)

*David Smith* says:

May 27th, 2007 at 5:32 pm

These photos just blow my socks off. Looks like there's room in the shelter for a hot plate, too, to keep a muffin and a cup of coffee warm while the readings are taken on cold winter nights.

A subtle point: in several of Anthony's photos I've noticed the close proximity of trees. Trees affect sunlight, nighttime radiative cooling and wind, all of which can affect temperature. Any one with a tree in a yard can witness how frost is reluctant to form on grass near a tree, due to radiative blocking. As the trees in the photo grow over the decades, they will progressively affect the temperature reading.

Amazing findings, Anthony.

[3](#)

*STAFFAN LINDSTRÅ-M* says:

May 27th, 2007 at 5:53 pm

#1

Paul, you can have another one here: In Stockholm Sweden EU last September 9 we celebrated 250 years of Temperature Observations (really it was 252 years as Wargentin started these in January 1754 Anders Moberg yes that Anders Moberg may correct me if I'm wrong... (Wolfgang Amadeus Mozart who has relatives in Stockholm BTW was also born in 1756..)) But there were quite some festivities Anders Moberg being there and having a lecture etc. Me being me wondered if 0.75C is enough compensation from the 1850's for UHI effects. (From Hans Alexandersson at SMHI earlier by e-mail) I didn't get an answer I give you the link adress to the SMHI site their policy of linking is not clear to me. The swedish word to look for is "värmeeffekt" HIE Of course we have at least a double heat island as you can see the observatory is on a hill 20-25 m above the surrounding city... BTW can you spot the asphalt? LINK:[http://www.smhi.se/sgmain/lopsedel/medvind\\_observatoriet.htm](http://www.smhi.se/sgmain/lopsedel/medvind_observatoriet.htm)

[4](#)

*Paul Lindsay* says:

May 27th, 2007 at 6:34 pm

As I understand it, the GCMs hindcast the global temperature record compiled from these weather stations quite well. Is there a 65W ~~fudge factor~~ parameterization that takes care of that? By the way, what's the exhaust fan for, the barbecue? Or is it a hair dryer?

[5](#)

*bernie* says:

May 27th, 2007 at 6:40 pm

Is the metal stand standard?

Is there a temperature series that goes along with this location?

[6](#)

*STAFFAN LINDSTRÅ-M* says:

May 27th, 2007 at 6:49 pm

#2 Dave, the trees, bushes around the Stockholm Observatory are perhaps according to WMO reglementation but correct me if I´m wrong houses should not be closer than 50 metres... Would be interesting to see how many stations that would be left after zero tolerance had been imposed... Did I sense a rumble in the IPCC house of cards..?

[7](#)

*Anthony Watts* says:

May 27th, 2007 at 6:53 pm

Oh it's not just in the small cities, some of the rural USHCN sites are getting in on the ISO-2000 action too. Here's some photos taken by my cohort, Russ Steele of another USHCN site in the Sierra Nevada.

This site has two certificates for 50 and 75 years of service. Estimated time by caretaker is about 90 years. This site was automated in 2002 and move from a location near the caretakers cabins to the maintenance shop over looking the dam of the lake. A change in elevation of about 550 feet occurred

USHCN Reported Coordinates of site: Lake Spaulding (39.32N, 120.64W)  
GISS ID 425745010040

Â

## Current Sensor Site General view



(I presume the boat is there for safe getaway in case of catastrophic sea level rise) It is hard to see the sensor shelter, but it's on the right side of the tower above the boat.

Here's a better view:



And finally, what the observing site USED to look like before somebody got the bright idea to move it:



[8](#)

*Peter Hartley says:*  
May 27th, 2007 at 7:12 pm

While we are looking at inappropriately placed weather stations, it might be worth reminding people of John Daly's [discussion](#) of the Badwater station in Death Valley.

[9](#)

*lmjpeter* says:

May 27th, 2007 at 7:41 pm

These images have my sixth grader in stitches. This is such shoddy data collection an eleven year old knows better. I think I see the next science project.

[10](#)

*steven mosher* says:

May 27th, 2007 at 7:56 pm

Keep up the good work Team Anthony.

For grins I looked at the USHCN data for Lake Spaulding. The adjustment for Urban Heating was essentially 0.

So, then I compared that to Hansen's homogeneity adjustment. Some years the adjustment was positive other years negative +.2C then -.2C..cyclical it appeared.

And then in 2000, according to hansen's adjustment Lake spaulding gets these three corrections.

Did the move happen in 2002? or before?

2000 +0.728888889

2001 +0.515555556

2002 +0.428888889

[11](#)

*Anthony Watts* says:

May 27th, 2007 at 8:00 pm

RE: 10 Steve according to Russ Steele's interview of the caretaker, it was 2002, see his complete report [here](#)

[12](#)

*TAC* says:

May 27th, 2007 at 8:30 pm

My first reaction is LOL because I do not know how else to react (on the positive side, one rarely encounters this much cheap entertainment in serious science 😊).

However, having pointed out how bad things are at some of the temperature sites, I am wondering if there might exist a different subset of sites, one that contains carefully

collected representative data that could be relied on to construct a temperature history (without adjustments).

It cannot *all* be this bad, right?

[13](#)

*steven mosher* says:

May 27th, 2007 at 8:31 pm

Thanks Anthony,

You said the change in elevation was 550. Russ, said this

USHCN Reported Coordinates of site: Lake Spaulding (39.32N, 120.64W)

GPS Coordinates of the historical site: 39.3187N, 120.6387W Elevation 5167 Ft

GPS Coordinates of the current site: 39.3248N, 120.6433W, Elevation 5115 Ft

550 Feet would be about 1C According to Hansen 99, where he said they adjusted St Helena site because of a change in elevation from 604M to 436M

Just curious which is it? because if it is 550ft then we should expect to see adjustments in the years beyond 2002.

[14](#)

*Anthony Watts* says:

May 27th, 2007 at 8:34 pm

Ok folks, last set for awhile. just wehn you think I could not possibly post any worse pictures, I yet again have more. I'd like to point out that we've only surveyed about 10 stations in California so far, between Russ and myself, and what you see posted here represents samples from those.

It gives you an idea of the magnitude of the problem.

Ok here is Tahoe City California, GISS ID 425724880010 and an active station. I was unable to locate the observer for this station but was able to speak with the property management people for the nearby condos.

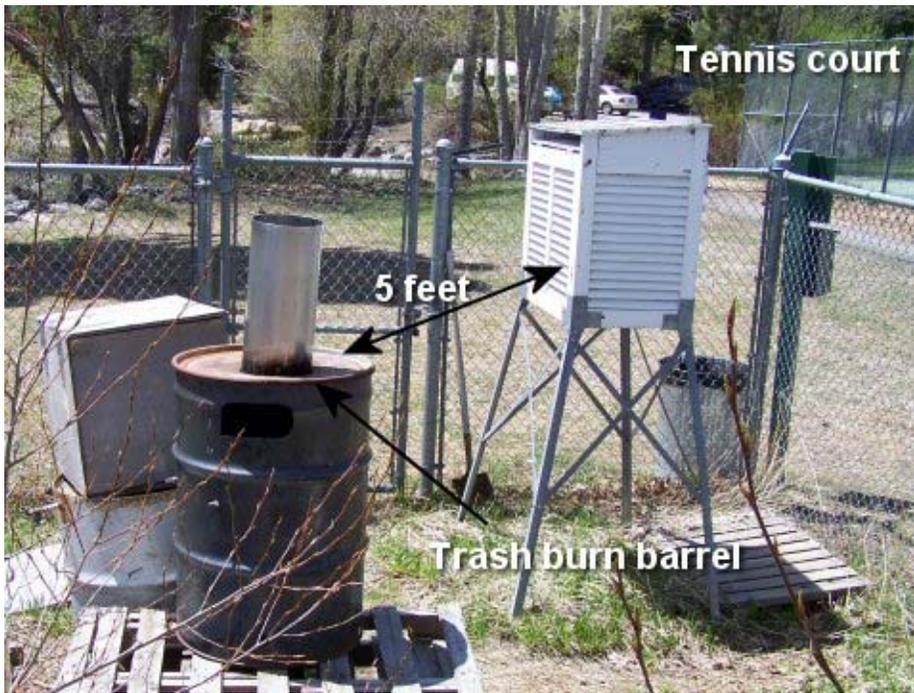
Ok first picture looking south over lake Tahoe. BTW the van is not mine, but belongs to the groundskeeper, and yes thats where he parks it regularly. Note the doggie bags.



Next picture, looking north, whats that, a tennis court within 25 feet of the station? Horrors.



Final picture. It seems the groundskeeper likes to burn the trash and paper he picks up on the grounds, guess where he does it?



Honest folks, I couldn't make this stuff up if I tried.

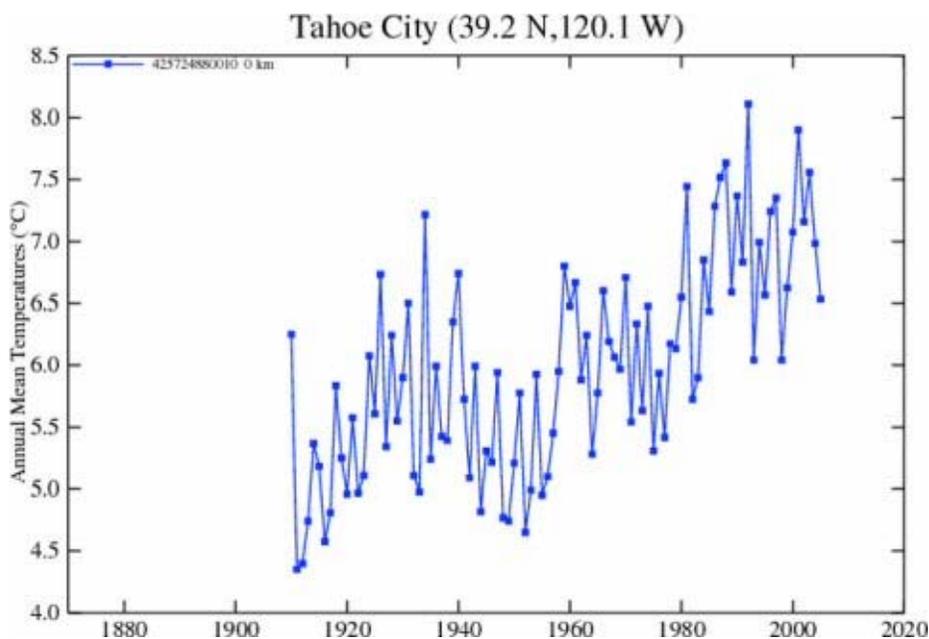
[15](#)

*Anthony Watts* says:

May 27th, 2007 at 8:38 pm

Oh forgot to mention, the property management people say the tennis court and condos was built in the early 80's.

Here is the GISS plot:



[16](#)

*bender* says:

May 27th, 2007 at 8:44 pm

This is unreal. Is it like this in Florida? Alaska? Canada?

[17](#)

*Anthony Watts* says:

May 27th, 2007 at 9:15 pm

Re 13 Whoops Steve that 550 was a typo, good catch. Wish there was a way to edit these posts after the fact.

[18](#)

*Steve McIntyre* says:

May 27th, 2007 at 9:18 pm

Now let's think back to the UHI articles. Basically Jones and the Team compare urban sites to small town and rural sites. All these sites are "small" and "rural" sites. The main "proof" of low UHI is that urban sites are not warming significantly more than rural sites. Obviously we're seeing major micro-site effects at the rural sites. The urban sites are going to be at airports; Jerry B observes that the purpose of the stations at airports is for safety so they are all pretty close to the runways but my guess is that they will not have the rustic inefficiencies of the stations we're seeing here. So all Jones is really proving is that there's an approximate balance between urban UHI and bizarre small town microsite effects.

One thing that may temper this: Hansen supposedly makes a USHCN subset with 0 satellite light, which supposedly is a benchmark. Orland is on this list but not Marysville. I'll post up the unlit list for reference.

[19](#)

*Steve McIntyre* says:

May 27th, 2007 at 9:36 pm

I don't have a concordance of GHCN and USHCN numbers and if anyone knows one, I'd appreciate a reference. Hansen shows lights by GHCN number. Here are the western US sites with lights=0. It includes Lake Spaulding. These would all be high-priority searches in that they are the creme de la creme of Jones and Hansen quality control in the western US.

wmo	loc	id	site	lat	long	urban	code2	country	lights
2026	72371	40	723710040	ALTON	374	-1125	R A	425	0
2612	72583	10	725830010	AUSTIN	395	-1171	R A	425	0
2615	72583	40	725830040	BATTLE MOUNTAIN	4SE	406	-1169	R B	425 0
1843	72286	20	722860020	BEAUMONT	USA	339	-1170	S B	425 0
2608	72582	10	725820010	BEOVAWE	USA	406	-1165	R A	425 0
3143	74501	30	745010030	BLUE CANYON	393	-1207	R A	425	0

1841 72281 30 722810030 BRAWLEY 2SW 330 -1155 S B 425 0  
2027 72371 50 723710050 BRYCE CANYON/FAA AIRPORT 377 -1121 R B 425 0  
2617 72583 60 725830060 CEDARVILLE 415 -1202 R A 425 0  
2035 72376 10 723760010 CHILDS 344 -1117 R A 425 0  
2594 72572 120 725720120 CORINNE 416 -1121 R B 425 0  
1856 72293 10 722930010 CUYAMACA 330 -1166 R A 425 0  
3217 74611 10 746110010 DAGGETT/FAA AIRPORT 349 -1168 S B 425 0  
3219 74619 10 746190010 DEATH VALLEY 365 -1169 R B 425 0  
2299 72471 100 724710100 DESERET 393 -1126 R A 425 0  
3020 74003 10 740030010 ELBERTA 400 -1119 R A 425 0  
3141 74501 10 745010010 ELECTRA PH 383 -1207 R A 425 0  
2028 72371 60 723710060 ESCALANTE 378 -1116 R B 425 0  
2047 72383 10 723830010 FAIRMONT 347 -1184 R A 425 0  
2038 72376 40 723760040 FLAGSTAFF USA 351 -1117 S A 425 0  
2627 72594 10 725940010 FORT BRAGG 5N 395 -1238 R B 425 0  
2040 72376 60 723760060 FORT VALLEY 353 -1117 R B 425 0  
2616 72583 50 725830050 GOLCONDA 410 -1175 R A 425 0  
2042 72376 80 723760080 GRAND CANYON NATL PARK 2 361 -1121 R A 425 0  
2285 72470 10 724700010 HANKSVILLE 384 -1107 R A 425 0  
2631 72597 20 725970020 HAPPY CAMP RS 418 -1234 R A 425 0  
2288 72470 40 724700040 HIAWATHA 395 -1110 R A 425 0  
2308 72480 10 724800010 INDEPENDENCE 368 -1182 R B 425 0  
3144 74501 40 745010040 LAKE SPAULDING 393 -1206 R A 425 0  
2596 72572 140 725720140 LAKETOWN 418 -1113 R A 425 0  
2023 72371 10 723710010 LEES FERRY 369 -1116 R A 425 0  
2057 72389 10 723890010 LEMON COVE 364 -1190 R A 425 0  
2289 72470 50 724700050 LEVAN 396 -1119 R C 425 0  
2294 72471 50 724710050 LOA 384 -1116 R A 425 0  
2613 72583 20 725830020 LOVELOCK/FAA AIRPORT 401 -1185 R A 425 0  
2287 72470 30 724700030 MANTI 393 -1116 R A 425 0  
2295 72471 60 724710060 MILFORD/WSMO 384 -1130 R B 425 0  
2311 72480 40 724800040 MINA 384 -1181 R A 425 0  
2322 72487 20 724870020 MODENA 378 -1139 R A 425 0  
2588 72572 60 725720060 MORGAN COMO SPRINGS 410 -1116 R A 425 0  
2021 72370 20 723700020 NEEDLES FAA AP 348 -1146 R A 425 0  
2628 72594 20 725940020 ORLEANS 413 -1235 R A 425 0  
2291 72471 20 724710020 PANGUITCH 378 -1124 R C 425 0  
1833 72278 80 722780080 ROOSEVELT 1WNW 337 -1111 R B 425 0  
1853 72291 10 722910010 SAN CLEMENTE/ISLAND NAAS 330 -1186 R A 425 0  
1854 72291 20 722910020 SAN NICHOLAS/ISLAND 333 -1194 R A 425 0  
2048 72383 20 723830020 SANDBERG/WSMO 348 -1187 R A 425 0  
2298 72471 90 724710090 SCIPIO 393 -1121 R A 425 0  
2041 72376 70 723760070 SELIGMAN 353 -1129 R A 425 0  
2328 72489 30 724890030 SUSANVILLE AP 404 -1206 R B 425 0  
2049 72383 30 723830030 TEJON RANCHO 350 -1187 R A 425 0

2310 72480 30 724800030 TONOPAH/AIRPORT 381 -1171 R A 425 0  
2584 72572 20 725720020 UTAH LAKE LEHI 404 -1119 R B 425 0  
2620 72591 20 725910020 WILLOWS 6W 395 -1223 R A 425 0  
2611 72583 0 725830000 WINNEMUCCA/MU 409 -1178 R B 425 0  
2593 72572 110 725720110 WOODRUFF 415 -1111 R A 425 0  
2314 72481 30 724810030 YOSEMITE PARK HEADQUARTERS 378 -1196 R A 425 0  
2025 72371 30 723710030 ZION NATIONAL PARK 372 -1130 R A 425 0

[20](#)

*Anthony Watts* says:

May 27th, 2007 at 9:42 pm

RE19 For us less illuminated, could you explain lights=0 ?

[21](#)

*Warwick Hughes* says:

May 27th, 2007 at 9:52 pm

Re 18.

Orland looks well lit to me.

Anyone else want to check ?

I could have my senses skewed after viewing the Tahoe City photos.

[22](#)

*Steve McIntyre* says:

May 27th, 2007 at 9:59 pm

It's to do with satellite imagery Hansen et al 2001 is online

[http://pubs.giss.nasa.gov/docs/2001/2001\\_Hansen\\_etal.pdf](http://pubs.giss.nasa.gov/docs/2001/2001_Hansen_etal.pdf)

Why bother taking a photograph from 5 feet away when you can use a 1990s vintage satellite image? That's what professionals" do according to Hansen ISO-2000 standards.

[23](#)

*bender* says:

May 27th, 2007 at 10:10 pm

Remote sensors like to wing it.

[24](#)

*Steve McIntyre* says:

May 27th, 2007 at 10:16 pm

The last figure on the right is an index of lighting:

GISS [http://data.giss.nasa.gov/gistemp/station\\_data/station\\_list.txt](http://data.giss.nasa.gov/gistemp/station_data/station_list.txt)

745010040 LAKE SPAULDING lat,lon (.1deg) 393 -1206 R1A cc=425 0

725910040 ORLAND lat,lon (.1deg) 398 -1222 R2C cc=425 15  
745000030 MARYSVILLE lat,lon (.1deg) 392 -1216 S3C cc=425 39

[25](#)

*Warwick Hughes* says:

May 27th, 2007 at 10:19 pm

CA Blog,

I see now Orland is not on the "lights=0" list.

But Fort Bragg looks v well lit, be v interesting to get exact lat longs on that instrument.

See NASA earthlights images at.

<http://apod.nasa.gov/apod/ap001127.html>

I am working off a 6MB image.

[26](#)

*Steve McIntyre* says:

May 27th, 2007 at 10:31 pm

At the historical Lake Spaulding site, there wasn't any warming. Its time series looks like Orland.

[27](#)

*Anthony Watts* says:

May 27th, 2007 at 10:32 pm

Ah, ok now it makes sense. I have visited Willows 6W (GISS ID 425725910020 )on the list you posted, and it would be lights=0 except for the cluster of brick buildings (The Canal authority) about 200 feet away on one side with a security light or two. There's the irrigated orchard on the other side 50 feet away surely creates some bias. Then there's the big canal.

I guess Hansen didn't use daylight photos to see what's around these sites?

Here's the pix from Google Earth:



and a view looking west

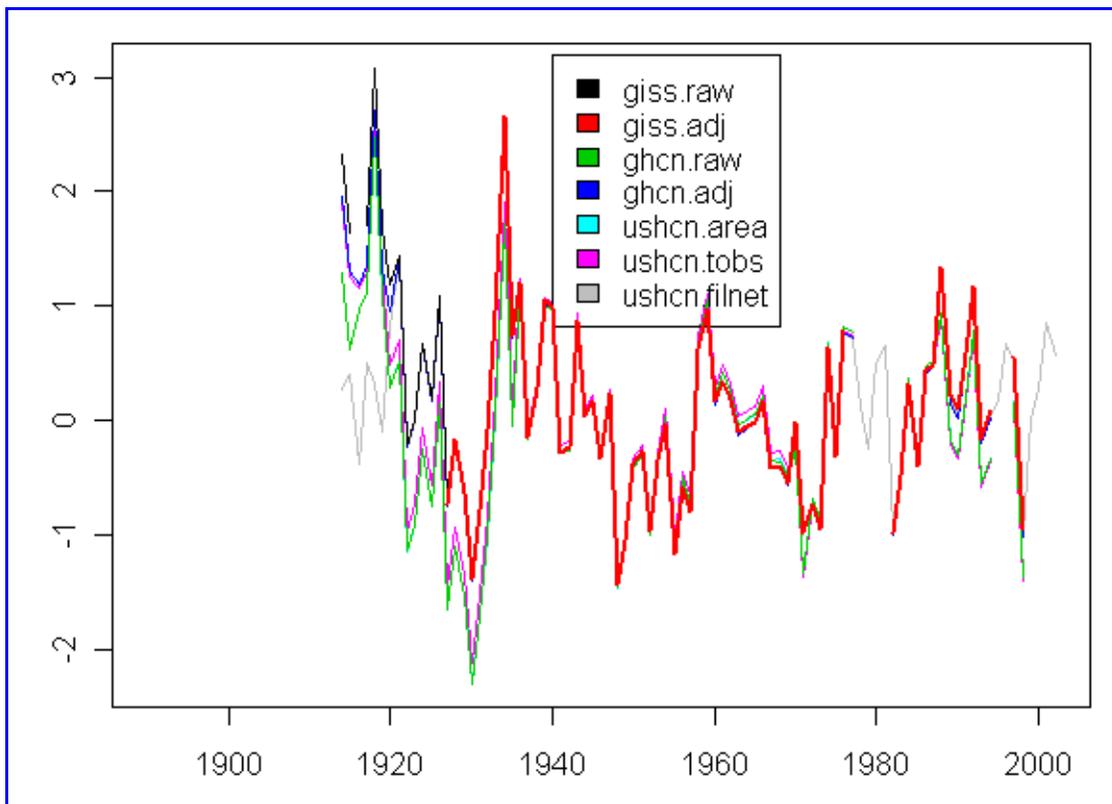


[28](#)

[Steve McIntyre](#) says:

May 27th, 2007 at 10:36 pm

Here's a plot of Lake Spaulding versions converted to 1961-1990 anomaly. GISS doesn't use early portion. USHCN filnet reduces early portion - presumably to allow for the "Rural Cooling Effect".



[29](#)

*steven mosher* says:

May 27th, 2007 at 10:37 pm

RE 12. TAC,

Can They all be this bad? I dunno, Anthony and Russ have just headed out through Norcal and

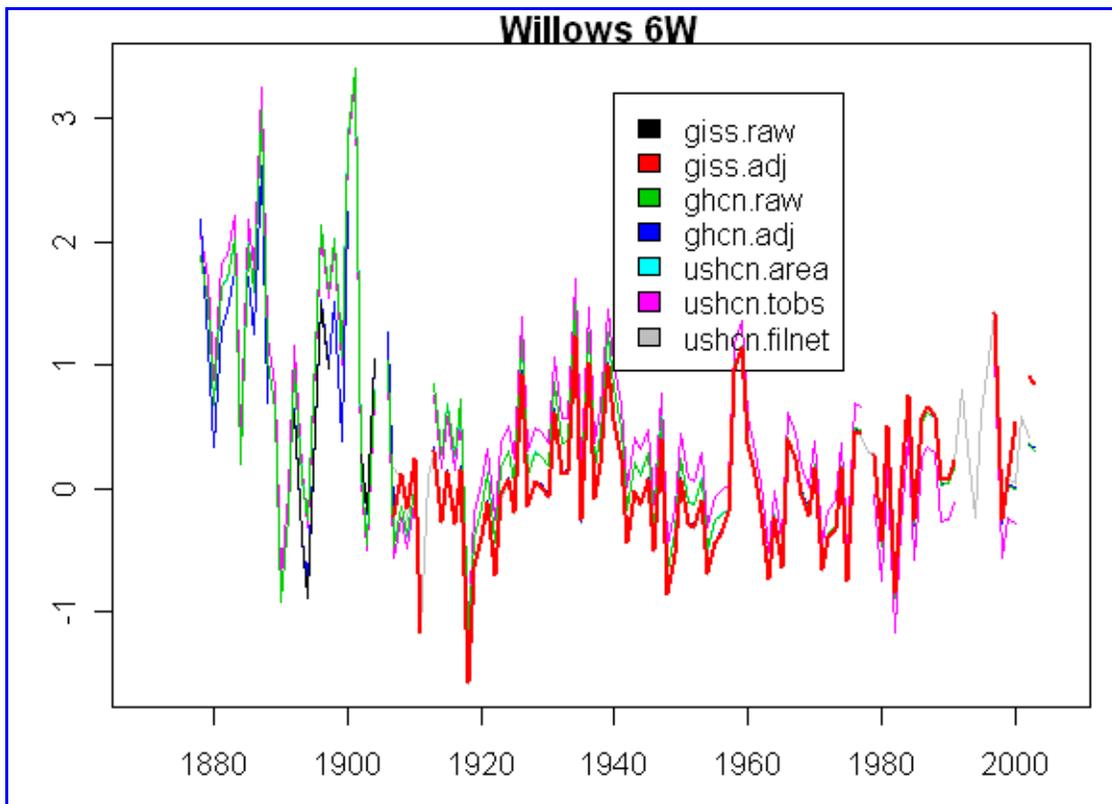
found serious issues ( issues not addressed by adjustment codes)at well 10 out of 10 sites. but its ok because all the bad data gets averaged over a 2x2 grid, so it evens out. Sorta.

[30](#)

*Steve McIntyre* says:

May 27th, 2007 at 10:45 pm

The Willows 6W site doesn't look bad and there's no warming there since the 1930s. Hansen has truncated the warm early portion of the record.



[31](#)

[Russ](#) says:

May 27th, 2007 at 10:51 pm

Re: #25

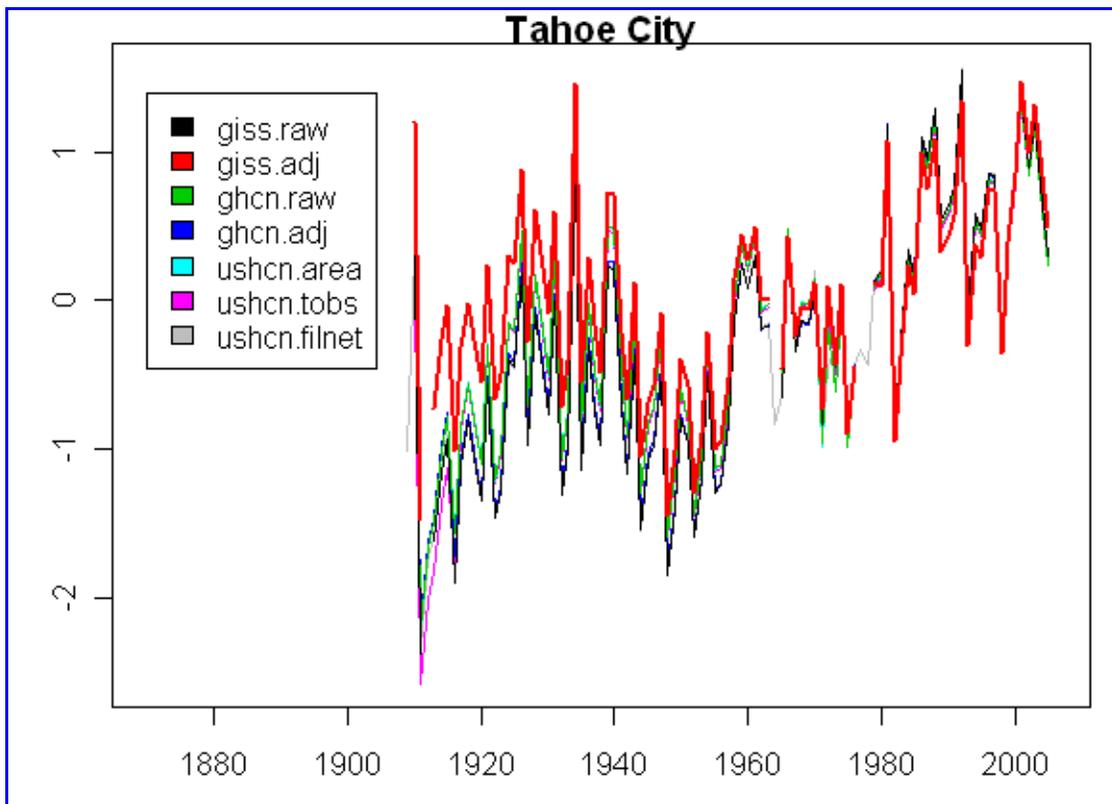
Warwick, I am going Ft Bragg on the 15th of June, and will get the GPS Lat/Long, plus pictures and other survey data.

[32](#)

[Steve McIntyre](#) says:

May 27th, 2007 at 10:55 pm

Here is a plot of an anomaly version of the various Tahoe City versions. Comparing it to Willows 6W, Orland or "historical" Lake Spaulding, it appears to have a large uncorrected micro-site effect. The contrast between this site and Willows 6W is really quite striking. Think of all the attribution studies seeking to explain the post-1970 temperature increase - of course, this is what Doug Hoyt has been saying for years.



[33](#)

Anthony Watts says:

May 27th, 2007 at 10:55 pm

RE 25 "But Fort Bragg looks v well lit, be v interesting to get exact lat longs on that instrument." Warwick we have Ft. Bragg site survey scheduled about June 15th and will have a GPS fix.

[34](#)

steven mosher says:

May 27th, 2007 at 10:58 pm

from personal nighttime observations:

Tonopah airport is no lights for a good reason

Susanville being no lights supprises me.

Willows IS no lights

[35](#)

Steve McIntyre says:

May 27th, 2007 at 11:08 pm

Also take a look at this post on Christy's presentation on Valley vs Sierra temperatures

<http://www.climateaudit.org/?p=697> . I think that Christy is originally from Fresno or some place nearby.

[36](#)

*Anthony Watts* says:

May 27th, 2007 at 11:16 pm

Here's the exact lat/long for Orland GISS ID 425725910040

39.74629 N 122.20027 W and Google Earth pix and pix looking SW

note that while the backlot of the Orland Water Users Association office has remained essentially unchanged in the last 100 years, there has been a lot built up around it. But within 200 feet of the site has been pretty well left alone.





[37](#)

*Don Healy* says:

May 27th, 2007 at 11:27 pm

Anthony's work certainly raises grave questions about the accuracy of the USHCN figures. If he wishes to have all of the stations examined as he is doing, perhaps he could provide a list of the station locations and the inspection criteria he is using, and those of us in other parts of the grid could help expand the geographic scope of his endeavors. Anthony and Steve, thanks for all your efforts.

[38](#)

*steven mosher* says:

May 27th, 2007 at 11:31 pm

steveM have a look at what GISS has for homogeneity adjustment for lake spaulding.

Also, Hansen 99 says that values are averaged over a 2x2 ( lat lon) grid.

Is there anyway to get the final grid data for the grid that holds Marysville?

[39](#)

*Anthony Watts* says:

May 27th, 2007 at 11:52 pm

RE 37, Don you read my mind. I'm in the process of setting up a web photo submission and browser driven database that allows members to join and contribute...setting it up to be a "fill in the blanks" by following a simple procedure. All you need is a digital camera and GPS unit with WASS accurate positioning (\$120 at Wal-Mart)

I've been working on it for the past 2-3 weeks, and I'll make the announcement in a few days. Its a dedicated server with SQL database, RAID, and backup, and its online now and I'm building placeholders for the USHCN and GHCN sites. Will be searchable by name, state, country, GISS ID and more.

It will be collaborative and open for all to use. The goal is to allow qualitative analysis as we've been doing here on this thread today.

[40](#)

*James Lane* says:

May 28th, 2007 at 12:44 am

Anthony, that's a great idea. I hope I can contribute on some Australian sites. It's pretty amazing that this work is being undertaken by amateurs. If Jones and Hansen are busy making adjustments to station data, it would be invaluable, one would think, to have images and details of each site, as well as some history.

[41](#)

*Lars Silen* says:

May 28th, 2007 at 1:06 am

An interesting comparison between different types of screens for temperature measurements (Australia) is found here:

<http://www.wmo.ch/web/www/IMOP/WebPortal-AWS/Tests%20performed/ITR649.pdf>

[42](#)

*EW* says:

May 28th, 2007 at 5:24 am

Here is an article in Czech, comparing temperature differences between a station set in the centre of a city (Brno, in a garden of an abbey, station named Mendel, photo Fig (Obr) 2) and two other stations - one station at the local airport outside the city (Fig/Obr 4, Brno-Turany, 49.15°N 16.70°E) and another one in suburban area, Brno-Zabovresky. The comparisons were done in 2005 and the UHI effect is marked.

The first graph shows differences between min temps(difference Mendel-Zabovresky in blue, Mendel-Turany in yellow). The second graph shows differences between average temps and the third shows differences between max temps, which are biggest.

<http://www.amet.cz/webmendel/MendelClanekPD05.pdf>

[43](#)

*John F. Pittman* says:

May 28th, 2007 at 5:43 am

It would be interesting to have a comparison of the average of about ten temperature anomalies from visually certified sites and compare with Briffa, or some other work in which their graph showed a negative directed divergence from the “official” anomalies starting about 1985. The assumption would be that the dendo’s that showed temperature starting to decline about 1985 or so are correct, and thus the 1990-2005 period is not the warmest in the past century but has returned to “normal”. Would we need to use the latitude and altitude adjustment for temperature? It would seem to me unnecessary for the anomalies data. But of course, our data would not be “aerosol” adjusted. LOL.

[44](#)

*STAFFAN LINDSTRÅM* says:

May 28th, 2007 at 6:20 am

#40

James..Amazing, I don’t know... Without being too preposterous I would say that good and devoted amateurs often do a better job than bad and bored professionals...No names given...If you google SMHI+VÄ,,RMEÄ–EFFEKT(VÄörmeÄ¶effekt=HIE) you get a SMHI article dated 2005 Aug 12... Yours truly “took up” this effect on Swedish Nat Radio P1 program phone-in 2005 May 5 ...But the reason SMHI had this article some 3 months later was a report from WWF Geneva (Sitting close to WMO??) about “hottening” European cities during summers...Mostly capitals I think. Madrid most hottening up but Stockholm in third That was in summer I just checked the pdf document I just searched it for: UHI... Result ..surprise, surprise ZERO Heat island then... surprise surprise again ZERO... You can at least give credit to SMHI admitting there is a strange cigar named UHIE... Perhaps they listened to “my” SR P1 program LOL...

[45](#)

*Geoff Sherrington* says:

May 28th, 2007 at 6:40 am

Ref #41

Broadmadows is only a few km from my home. I know of nothing about the site that would produce intrinsic anomalies. It is ordinary flat suburbia, a few m above sea level, perhaps 15 - 20 km from the sea.

The BOM paper shows between-screen variability in some conditions of 0.2 degrees C. on a

site 8.5 m square. One wonders what the between-site variability would be if the experiment was performed again at a number of sites, some close, some distant. Then what would be the size of between-site relationships on a data grid cell used for modelling? The signal:noise ratios are RS as we used to say in old terms.

As further variables, as per other papers from these CA threads, the height above ground and the time of day are also of interest. The response time of the thermometer has emerged here as a factor of interest as well.

I keep repeating that essential micro experiments of this type are of critical importance BEFORE release of a report calling for hugely expensive global action. I keep saying that it is not the average of max and min temps that is important, it is the heat flux - and the BOM collected but did not report on this valuable data in the paper of # 41. I shall try to contact the BOM about what further work they have done about screens and UHI.

Another micro-experiment. About 1800, Napoleon had planted a huge grove of oaks for naval ship masts, west of Paris. Potentially, these is now a fairly uniform sub-set of specimens for the dendros. The species are the same, the growing conditions near-identical, the history is recorded - maybe they have been studied but now some are auctioned each year to make wine barrels.

When looking at spatially or temporally distributed data such as temperature records at different sites over different times, can I ask the maths experts if the following is old hat and used, or whether it might be useful in the future?

Quote: Three functions are used in geostatistics for describing the spatial or the temporal correlation of observations: these are the correlogram, the covariance and the semivariogram. The last is also more simply called variogram. The sampling variogram, unlike the semivariogram and the variogram, shows where a significant degree of spatial dependence in the sample space or sampling unit dissipates into randomness when the variance terms of a temporally or in-situ ordered set are plotted against the variance of the set and the lower limits of its 99% and 95% confidence ranges.

The variogram is the key function in geostatistics as it will be used to fit a model of the spatial/temporal correlation of the observed phenomenon. One is thus making a distinction between the experimental variogram that is a visualisation of a possible spatial/temporal correlation and the variogram model that is further used to define the weights of the kriging function. Note that the experimental variogram is an empirical estimate of the covariance of a Gaussian process. As such, it may not be positive definite and hence not directly usable in kriging, without constraints or further processing. This explains why only a limited number of variogram models are used like the linear, the spherical, the gaussian and the exponential models, to name only those that are the most frequently used.

When a variogram is used to describe the correlation of different variables it is called cross-variogram. Cross-variograms are used in co-kriging. Should the variable be binary or represent classes of values, one is then talking about indicator variograms. Indicator

variogram is used in indicator kriging.

The experimental variogram is computed by measuring the mean-squared difference of a value of interest  $z$  evaluated at two points  $x'$  and  $x'+h$ . This mean squared difference is the semivariance and is assigned to the value  $h$ , which is known as the lag. A plot of the semivariance versus  $h$  is the variogram.

For observations at locations the empirical variogram is defined as (Cressie 1993):

(see several equations in <http://en.wikipedia.org/wiki/Variogram>)

where  $N(h)$  denotes the set of pairs of observation placed at an approximate distance of  $h$ . Here “approximate distance  $h$ ” is not exactly defined and typically implemented by a certain tolerance. END QUOTE.

I would love to see some cross variograms for temperature and other proxies and for “look to find” variables like sunspot activity.

(I feel uncomfortable in the midst of you experts and do not know if I am pitching at the right level. If I am talking nonsense or going over established ground, will someone be kind enough to tell me?)

[46](#)

*Sound science* says:

May 28th, 2007 at 6:41 am

I really don't see the problem here, GCMs are happy to compensate for these inconveniences. Special subroutines can easily model the effects of doggy bag collection, trash burning and parked cars with discrete event simulations. Have no fear, it's all based on sound science. Trust me.

[47](#)

*bernie* says:

May 28th, 2007 at 7:30 am

I think the pictures are amazing and clearly indicate that the supposed standards are at best vague guidelines. It appears that: (a) Urban and rural distinction is too arbitrary and a better set of station descriptors are need, descriptors that relate to likely temperature and precipitation variation for the immediate geographic area. (b) The history of these stations is important, as some of the above comments illustrate, not just the physical location but also the nature and timing of temperature amplifying or suppressing events, e.g., vegetation growth or removal, asphalt, whitewash vs latex, buildings, a/c installation, CH installation. (c) Given the multiplicity of likely microclimate effects, we need to understand the nature and magnitude of deviations between ground and satellite records on almost a grid by grid basis.

All of the above is agnostic as to magnitude and cause of climate change, but emphasizes

that given the small changes being measured we need a far more stable and reliable way of measuring temperature. However, it can be inferred that many of the man made changes to local microclimates of the WS do in fact create positive bias on the temperature record.

[48](#)

*bender* says:

May 28th, 2007 at 7:36 am

Re #45 The climatologists are well aware of the geostatistical approach to data analysis. There are lots of variograms of weather data in the literature. They are ho-hum. I'm not sure what you hope to get out of it. The spatial statistics you describe don't tell you much for two reasons: (1) they assume stationarity, whereas the earth's surface is heterogeneous; (2) most of the interesting dynamics (and the kind that we're talking about here) are in the time domain.

One area where spatiotemporal geostatistics could be useful (anf there are others) is in the estimation of missing data. But that's another story. One that will take us to RegEM.

R has packages for geostatistics, by the way. Anyone wanting to post a space-time dataset, I'll post a corresponding R script that does what #45 asks. (I've been doing this kind of thing for close to ten years now.)

[49](#)

*muirgeo* says:

May 28th, 2007 at 8:18 am

Once again Steve you reveal yourself as a cynical insignificant nit-picker. I live part of the year in Tahoe City and part of the year in the Sacramento valley. The pictures you show are kinda funny...kinda. What's not funny is the very real increased risk I have of losing my place to fire. What's also not funny is the very real decreasing Sierra snowpack and changes in snow melt trends that threaten California with real serious water shortages. Finally, Lake Tahoe is warming and while not threatening in itself it could lead to loss of the dramatic clarity that Lake Tahoe is known for.

Steve....seriously what is your point? You really don't have one except to say that science and climate science in particular are not exact sciences as if that's news. You've been so focused in on the small and insignificant while missing the obvious and bigger picture.

Lake Tahoe's water temperature increased, on average, 0.027 degrees Fahrenheit (0.015 degrees Celsius) per year.

Over the 33-year period, the temperature increased about 0.88 degrees Fahrenheit (0.5 degrees Celsius). This is similar to warming reported in other big lakes around the world, including the Great Lakes of North America; Lake Zurich, Switzerland; and Lake Tanganyika, Africa.

<http://www.sciencedaily.com/releases/2005/01/050111173540.htm>

<http://pubs.usgs.gov/fs/2005/3018/images/fig1.jpg>

[http://www.climatechange.ca.gov/events/2006\\_conference/presentations/2006-09-15/2006-09-15\\_WESTERLING.PDF](http://www.climatechange.ca.gov/events/2006_conference/presentations/2006-09-15/2006-09-15_WESTERLING.PDF)

[50](#)

*MrPete* says:

May 28th, 2007 at 8:29 am

muirgeo,

Small nits add up to big nits. Most people I know are appalled that Jones would go so far as to mess with data to “prove” that UHI is nonexistent and not impacting 20th(+) century measurements. Placing a realistic UHI factor on the table puts AGW conclusions at risk.

The question is not whether there is climate change, but its cause.

It’s bad enough that we can’t trust the shoddy analysis. It’s horrifying that apparently we can’t even trust the data.

[51](#)

*Reid* says:

May 28th, 2007 at 8:40 am

muirgeo says “Lake Tahoe’s water temperature increased, on average, 0.027 degrees Fahrenheit (0.015 degrees Celsius) per year.”

You’re nitpicking!

[52](#)

*Geoffrey Allan Plauche* says:

May 28th, 2007 at 8:49 am

Re #49,

No no, of course not. We wouldn’t want a little thing like a potentially significant upward bias in near surface air temperature measurements to get in the way of The Cause, now would we?

To add to what MrPete in #50 said, it isn’t just the causes of climate change with which we are concerned it is also with how much climate change. Surely in the interests of science at least (if not also policy) we should want to know how much the earth’s atmosphere really is warming. If it is warming less than the data shows, that would seem to be important.

[53](#)

*bender* says:

May 28th, 2007 at 8:53 am

Re #49

Climate change indeed can be a problem, natural or man-made. So good luck with your issues. The question is how much relief can we expect from things like CO2 mitigation strategies. Lots, or very little? Quantifying uncertainty is not about throwing doubt on the cause. It's about calculating what kind of insurance premium I ought to charge you for me to stay in the black.

[54](#)

*James Erlandson* says:

May 28th, 2007 at 9:38 am

Re 49 muirgeo:

Thanks for the comment. You say, " ... Lake Tahoe is warming and while not threatening in itself it could lead to loss of the dramatic clarity that Lake Tahoe is known for."

The ScienceDaily piece you referenced says,

"It's not immediately obvious what the potential effects of climate change will be to the lake's clarity," said lakes expert Geoffrey Schladow, director of the UC Davis Tahoe Environmental Research Center. "UC Davis and other research teams in the Tahoe Basin have been figuring out how this lake works for 40-plus years. We've had to factor in the emerging impacts of growth and development in the basin.

While you say that Steve is " ... focused in on the small and insignificant while missing the obvious and bigger picture." it would be reasonable to say that focusing on Lake Tahoe's rising water temp misses the obvious and bigger picture of regional development.

[55](#)

*MrPete* says:

May 28th, 2007 at 10:45 am

The scale of human impact on micro/macro climate change is very interesting. On the one hand, it seems through seemingly "minor" activities we greatly impact microclimates, while at the same time it's beginning to appear that larger bodies like the sun may be rather more significant than we give them credit for.

I'm reminded of how a one-time US\$10 expense radically changed our home garden output: 100 feet of 1 1/2 inch black plastic pipe inserted in the head end of our garden watering system resulted in a several week crop speed up; many crops matured that never survived in the past. (We just left the roll of pipe lying on the ground.) Humanly, a simple act with great impact. At the same time, a single hail storm can easily undo our best efforts.

Until seeing these measuring station photos, I'd always assumed the rural stations were

reliable long-term indicators of temperature. Now I'm inclined toward deep suspicion, which is sad.

It seems to me that we're looking at a consistent source of varying "heat noise" in measurements, all on the plus side.

We're seeing a huge variety of real factors that introduce some amount of heating error (not just a bias, but real error).

Are there equivalent potential placement/setup/etc errors that could make the measurements randomly \*cool\*? My current list: place the unit under a drip irrigation spray, near a waterfall, at the side of a car wash, in the middle of an auto-spray-irrigated field, etc. Those would be ridiculous of course... but then we've seen even stranger things on the heating-error side so I wouldn't be too surprised if someone posts a photo of a Stevenson Screen under an irrigation sprayer 😊

[56](#)

*John Hekman* says:

May 28th, 2007 at 10:56 am

The corollary to the famous "we have to get rid of the Medieval Warming Period" will soon be "We have to discredit the satellite temps." Since MSU temps cannot be reported "raw" but must be calculated using mathematical models that adjust for altitude, roll, etc., it would be a real disaster if the wrong people were put in charge of this data reporting. I would not be surprised if an effort is underway already by members of the Team.

[57](#)

*Mats Holmstrom* says:

May 28th, 2007 at 11:02 am

I had a look at the GHCN station positions in

<ftp://ftp.ncdc.noaa.gov/pub/data/ghcn/v2/v2.temperature.inv>

The format is described in <ftp://ftp.ncdc.noaa.gov/pub/data/ghcn/v2/v2.read.inv.f>

I found locating stations by longitude/latitude a bit difficult, so I made a script that read the station longitudes and latitudes and generates a KMZ-file that can be read by Google Earth:

<http://www.esnips.com/doc/3349166f-4a41-4c36-a7d2-cf80292c0aea/ghcn2>

Each station has a placemark and the raw text from the data file. Note that the positions is in degrees with two decimals, so even in the ideal case the error can be a kilometer. Looking at some stations that I know, I see cases with errors of several kilometers, e.g., the station in Karesuando, Sweden, is in the wrong country (Finland). What would be nice is to also have pictures of the stations in the placemarks.

[58](#)

*L Nettles* says:

May 28th, 2007 at 11:02 am

Anthony

It looks like NOAA has done some of this work already. I found the following links to observation histories to be intriguing.

[Manhattan KS](#)

[Atchinson KS](#)

[Burlington Vermont](#)

[Florence South Carolina](#)

[Logan Iowa](#)

[59](#)

[Russ](#) says:

May 28th, 2007 at 11:04 am

Re: #49

muirgeo,

What's also not funny is the very real decreasing Sierra snow pack and changes in snow melt trends that threaten California with real serious water shortages.

This from a Pacific Gas and Electric report who is very interested in the snow pack and snow melt as it is important to the generation of hydro power:

At this time [2002] PG&E's water management team has not observed any significant change in hydroelectric production that can be directly attributed to global warming or climate change.

In addition, when I look at the remote automated stations (RAWS) used by the Forest Service and BLM, far from urban areas in the forest and brush land, the Sierra has been cooling since 2003. These sensors show warming from from the mid 1990 when most stations were installed to about 2002 and 2003. Also, Christy found very little warming in the Sierra from 1903 to 2003. The Sierra RAWS stations are listed [here](#) if you want look for yourself.

[60](#)

[Don Keiller](#) says:

May 28th, 2007 at 11:39 am

Re#49 muirgeo.

You are, of course entitled to your view.

However right now I'm in the midst of marking my students' final undergraduate projects. I have to be nit-picking. Good Science is about nit-picking. If my students presented data

collected in such a slapdash, uncontrolled and arbitrary manner as Steve (and others) have demonstrated, I would fail them without hesitation.

That Governments and World Organisations are taking the temperature record as an unquestioned and unquestionable fact and making (expensive) policy decisions on their basis is nothing short of collective insanity.

[61](#)

*mccall* says:

May 28th, 2007 at 11:44 am

Mr Muirgeo's forecasted "water shortage" follows record snowfalls of 2005-6, I believe — either you have a very short memory or you must have bought the property a year ago? Not exactly a fair application of climate science.

If you want to think that way, consider buying in central CO, many areas had a record snow pack this year.

[62](#)

*Howard* says:

May 28th, 2007 at 12:26 pm

I'm just a dumb country geologist with math skills up to algebra and geometry, so I appreciate Anthony bringing the topic to something even morons could understand. A couple observations.

Non-instrument related temperature measurement bias seems like it would almost always be positive. Non-instrument bias can be broken down into four levels:

Regional (eg, the LA Basin, the White Mountains, redwood forest, Mojave Desert, etc.);

Local (eg, airport, wilderness, park, downtown, etc.);

Site (buildings, trees, bushes, asphalt, concrete, grass, etc);

Instrument Housing (screen type, paint, electronics, etc.)

I would guess that the sources of positive bias would be additive between levels.

Determining station adjustments through time is a tall task. If there are any records available for the instrument housing by the recording agent, that could be used to estimate that level of bias. The changes in Regional, Local and Site bias through time could be estimated for each location by using historical topographic maps, Sanborn fire insurance maps and historical air photos.

This type of investigation is similar to a Phase I Environmental Site Assessment that is conducted by a potential property buyer to determine if there are potential sources of chemical contamination. Non-instrument temperature bias is just another source of "contamination".

This level of investigation, if conducted commercially, would cost between \$1,000 to \$3,000 per site.

[63](#)

*Howard* says:

May 28th, 2007 at 12:37 pm

To clarify #62 above, the cost quoted would be for a report with maps showing the various layouts through time. Calculation of the potential temperature bias? NOT included... a formula for that would need to be developed by someone smart, like a physicist or engineer.

[64](#)

[Warwick Hughes](#) says:

May 28th, 2007 at 1:02 pm

Re 41 and 45

Thank you Lars for that ref to the WMO report ex the BoM.

I see it repeats a classic BoM myth in the 3rd para implying the Stevenson screen was introduced at the “turn of the century”.

There is a real climate history issue here because much late 19C BoM data indicates conditions as warm as the late 20C, a notion which of course the IPCC faithful BoM dislikes. See graphic of “Average of 25 Regional and Remote Stations”, scroll down at, <http://www.warwickhughes.com/cru86/>

In 1991 I researched records of 19C Inter-Colonial Conferences where meteorological matters were discussed and there are many refs to the use of the Stevenson screen from 1880. You can read my scanned 1995 paper from Int J, Climatology at, <http://www.warwickhughes.com/papers/ozstev.htm>

But have no fear, the BoM is busy adjusting out the 19C warmth, see previous ClimateAudit post April 28th, last graphic,

<http://www.climateaudit.org/?p=1492>

I predict that soon the Hadley Centre will incorporate the new, higher warming Australian data in their global gridded data. As they have done in their New Zealand gridded data. Just be aware of the above Geoff when hearing what the BoM have to say about screens.

[65](#)

*John Nicklin* says:

May 28th, 2007 at 2:18 pm

Have you ever wondered if we're just in some parallel universe where logic is not a daily practice.? This kind of stuff just can't be happening in a sane world. Can it?

Were any of the stations set up with the thought of making precision records that would be used the way we are using them now? It seems to me that setting up a network of planet-wide stations to test a hypothesis would have been done with much more rigor. There's just no way that a grad student could design something as slipshod as this network of stations and have any hope of graduating.

[66](#)

*Steve McIntyre* says:

May 28th, 2007 at 3:00 pm

#65. One of the interesting aspects of the poor practices is that practices in many places seem to have deteriorated during the IPCC period. For example, there's a kind of vandalism in moving the Lake Spaulding site (with a decent record) to its present abomination. IPCC might have made a statement on the need to preserve the continuity of these records.

[67](#)

*Richard deSousa* says:

May 28th, 2007 at 3:04 pm

MuirGeo is a Lonnie Thompson advocate. Lonnie Thompson has loudly proclaimed that the snows of Mount Kilimanjaro are melting due to global warming... not! Land use changes (deforestation by the inhabitants) around the famous mountain are responsible for the lack of precipitation thus the loss of snow on the mountain. MuirGeo is also lacking in history. Historically, California is a semi-arid state and every hundred years or so we undergo a severe drought - some as long as several decades.

[68](#)

*Anthony Watts* says:

May 28th, 2007 at 3:30 pm

RE #65 John,

The US Weather Bureau was formed primarily for the purpose of making forecasts and warnings. Here are some historical excerpts

1870: A Joint Congressional Resolution requiring the Secretary of War "to provide for taking meteorological observations at the military stations in the interior of the continent and at other points in the States and Territories...and for giving notice on the northern (Great) Lakes and on the seacoast by magnetic telegraph and marine signals, of the approach and force of storms" was introduced. The Resolution was passed by Congress and signed into law on February 9, 1870, by President Ulysses S. Grant. An agency had been born which would affect the daily lives of most of the citizens of the United States through its forecasts and warnings.

May 30, 1889: An earthen dam breaks near Johnstown, Pennsylvania. The flood kills 2,209 people and wrecks 1,880 homes and businesses.

October 1, 1890: Weather Service is first identified as a civilian enterprise when Congress, at the request of President Benjamin Harrison, passes an act creating a Weather Bureau in the Department of Agriculture.

A weather sensitive sports event of this first year: 15th running of the Kentucky Derby.

1910: Weather Bureau begins issuing generalized weekly forecasts for agricultural planning; its River and Flood Division begins assessment of water available each season for irrigating the Far West.

1914: An aerological section is established within the Weather Bureau to meet growing needs of aviation; first daily radiotelegraphy broadcast of agricultural forecasts by the University of North Dakota

1933: A science advisory group apprizes President Franklin D. Roosevelt that the work of the volunteer Cooperative Weather observer network is one of the most extraordinary services ever developed, netting the public more per dollar expended than any other government service in the world. By 1990 the 25 mile radius network encompasses nearly 10,000 stations.

1941: Dr. Helmut Landsberg, “the Father of Climatology,” writes the first edition of his elementary textbook entitled, “Physical Climatology.” Two women are listed as observer and forecaster in the Weather Bureau.

So as you can see, the US Weather Bureau was formed to provide forecasts to the military, then later as a civilian enterprise, with an emphasis on forecasts and warnings. Climatology was an afterthought, only coming into being around 1941.

The problem that we have today is that researchers think of the data gathering as having been done scientifically, with appropriate controls, when in reality its been mostly ad hoc and left to the whims and nuances of the observer and the location.

[69](#)

*John Nicklin* says:

May 28th, 2007 at 3:36 pm

#66: Thanks Steve

So record keeping has deteriorated during a time when we rely heavily on the data.

I’ll jump out on a limb and assume that in the old days (before IPCC and AGW) people kept detailed records out of scientific curiosity. Why not, we seem to be a species that keeps records of everything and anything. But when the stakes get higher, record keeping seems to become more haphazard almost like it has less importance.

Waiting for the sound of a chainsaw....

[70](#)

*John Nicklin* says:

May 28th, 2007 at 3:46 pm

68: Thanks for the history Anthony. So at least some stations were established for specific

purposes but in an ad hoc fashion that was good enough for their needs but may not be adequate for any other.

[71](#)

*rafa* says:

May 29th, 2007 at 3:23 am

Re#65

The spanish Meteo. Agency runs a fully automated network of several hundredths of stations. However there is still a parallel “manual” network of several thousand stations run by volunteers (the equipment is subsidized by the agency). Both approaches have pros and con’s. You can get the data from the automated ones immediately but you’ll never know if a group of hunters uses to cook barbecues during the hunting season beside the station. On the other hand some of the volunteers do a pretty good job, maybe reporting the station location is no longer appropriate, but other volunteers are less rigorous with the reports. Some of the volunteers report significant differences with the data coming from an automated station close to the “manual” station. The net result is we are far yet from having the “precision” or a widely accepted methodology the IPCC claims we have. As Anthony says (#68) the primary purpose in the old days was forecasting and warning. As an example, the ‘average’ hottest year. ‘Picking the appropriate’ station in the NortEast was 2003, in the center of Spain was 1933, while in the South West was 1866 and 1949 in the South. IMO every researcher should publish not only the results but fully disclose the data sets used, the methodology, the data reduction process, algorithms etc. Unfortunately, I do not seem this practice happening. Best.

[72](#)

*Lars Silen* says:

May 29th, 2007 at 4:25 am

Re # 64 (41,45) Warwick

I submitted the reference simply because it clearly shows the obvious fact that replacing a measurement screen with another type of screen should cause measurement errors. Replacing a screen type with another could cause a temperature measurement error of up to roughly 0.2 deg C based on data in the document. I think (based on discussions here at climateaudit) that the paper actually could indicate too small temperature differences because it probably isn’t enough to specify “painted white”. Regarding the introduction of certain screen types in Australia I am sure Warwick is well researched and I have nothing to add. I think the present situation where past measurements are corrected based on some general correction function without having exact information about the station is extremely dangerous from a scientific point of view. This is comparable to situations where rural stations have been corrected for cooling based on the proximity to UHI influenced urban sites. I have followed Warvicks website for many yars with great interest. You have done a good work!

[73](#)

*Don Keiller* says:

May 29th, 2007 at 8:34 am

Hey has anyone seen the latest posting on RC?  
I'm sure that there are loads of folk here who would be interested....

[74](#)

*JerryB* says:

May 29th, 2007 at 8:48 am

For a bit more history of gathering of weather info, see  
<http://www.siarchives.si.edu/history/jhp/joseph03.htm>

[75](#)

*C\_G\_K* says:

May 29th, 2007 at 9:29 am

Re #73

Looks to me like they are setting up E.G. Beck as a straw man then knocking him down as a way of making it seem like all the arguments from skeptics are as weak as his. What struck me, and what is relevant to this thread somewhat, is that there seems to be an admission that the records and reconstructions from the southern hemisphere are too weak to draw any conclusions from:

Lack of data does not permit robust reconstructions for the Southern Hemisphere

When pressed by a reader about how we can be so sure, in light of this, about global reconstructions, Mann says:

(chapter 6) of the latest IPCC scientific assessment report, model simulations reproduce the gross behavior of the Northern Hemisphere temperature reconstructions, including the anomalous recent warmth, as a result of a combination of natural and anthropogenic radiative forcing. The anomalous recent warmth is associated with the latter. These same simulations predict similar overall behavior for the southern hemisphere.

IMO, putting output from computer simulations on par with real empirical evidence is just not right and is not science. Between the misuse of computer modeling, and the sloppy collection of real temperature data, how can we ever expect to draw accurate conclusions?

[76](#)

*Steve Sadlov* says:

May 29th, 2007 at 9:57 am

From the looks of it, this site could just as well be in Foster City, Fullerton or any other suburb.

[77](#)

*Steve Sadlov* says:

May 29th, 2007 at 9:59 am

RE: #14 - I know exactly where that is. Completely urban / suburban site. The only saving grace is the moderating effect of the lake, but prevailing winds would be offshore at that location.

[78](#)

*Steve Sadlov* says:

May 29th, 2007 at 10:14 am

RE: #19 - Steve M, about 1/2 of the names on that list are serious developments - metro area suburbs, small towns, small cities in terms of population and level of development. Unless the sites named after them are several miles away, there is no way they are zero lights. Here are some examples of ones that I believe are not zero light, based on the names: BEAUMONT, BLUE CANYON, BRAWLEY, DAGGETT, FLAGSTAFF, INDEPENDENCE, LEES FERRY, LEMON COVE, LOVELOCK, NEEDLES, SUSANVILLE, TEJON RANCHO, TONOPAH, WILLOWS, WINNEMUCCA, YOSEMITE. Unless the sites are miles and miles away from the places they are named after, no way they are “zero light” sites. There may be others on the list. The ones I mentioned are names after communities I have personally visited more than once.

[79](#)

*Steve Sadlov* says:

May 29th, 2007 at 10:20 am

RE: #49 - What is the root cause of this year’s low snow pack? Is it lack of moisture, or too much warmth? (Don’t play games, I already know the answer - if you lie, I’ll point it out). And the follow on questions. How was last year’s snow pack, and how was the prior year’s?

[80](#)

*Steve Sadlov* says:

May 29th, 2007 at 12:38 pm

RE: #15 - Interesting possible coincidence / non coincidence regarding the big spike in 1960. Some of you may recall or have heard of a “minor” event which transpired in the area that year. 😊

[sound of tympani ... boom ... boom.... boom,boom.... boom ... repeat ..... bring in trumpet .... ]

[81](#)

*muirgeo* says:

May 29th, 2007 at 10:04 pm

Steve,

You might want to look at this to help you understand the issue.

<http://www.mindfully.org/Water/Global-Warming-Water.htm>

But to answer your question last years California winter precipitation (Dec-Feb) was up. The most recent 6 months precipitation was down. And the most recent and the winter temperatures were up.

<http://www.ncdc.noaa.gov/oa/climate/research/cag3/ca.html>

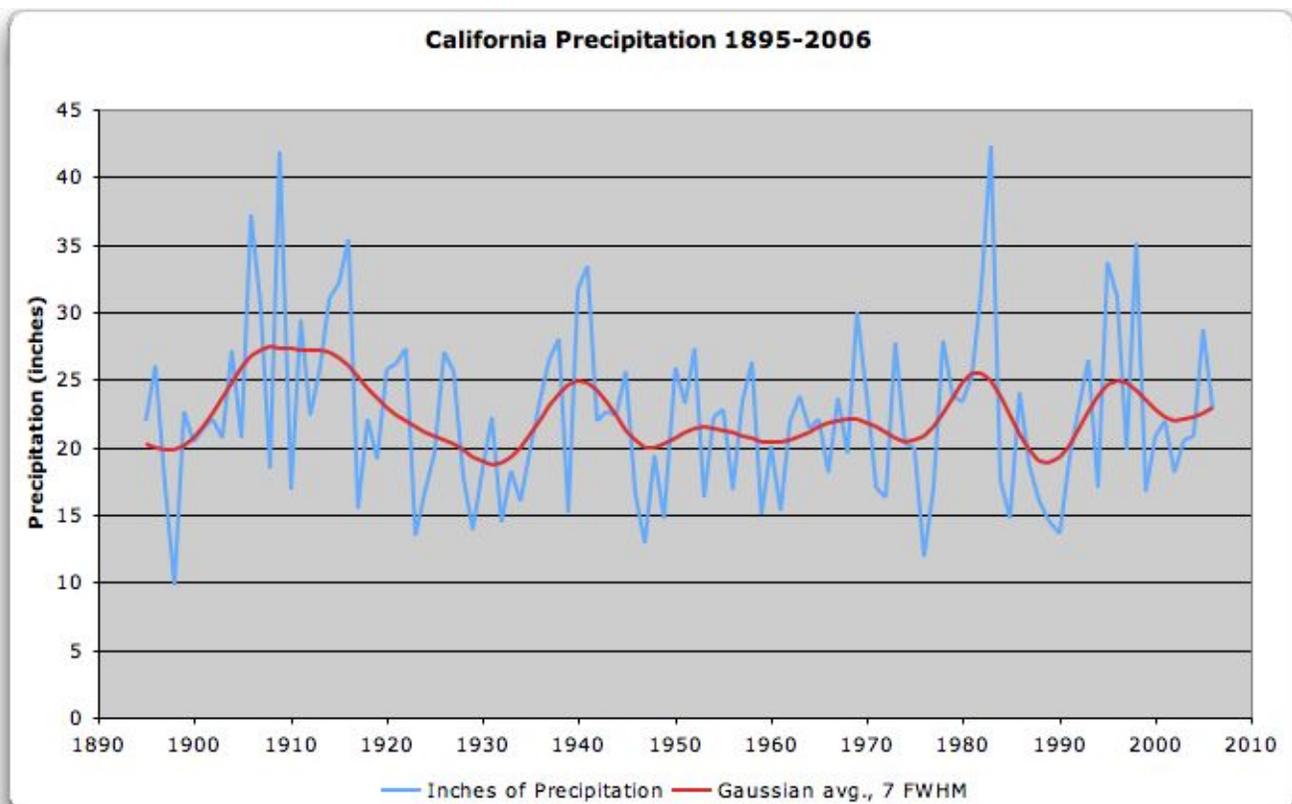
Now you answer the question of why the Sierra spring run off is decreasing? And then answer if it continues it's trend will that be a problem for California? Finally, then answer the significance of this thread in relation to the big picture.

[82](#)

*Willis Eschenbach* says:

May 30th, 2007 at 2:08 am

muirgeo, I'm not clear about what you are calling the "trend". Thanks to your excellent citation of the NOAA precipitation data, here is the historical record of California precipitation:



Now, I don't see any trend there. And in the abstract of "Climate change and Sierra Nevada snowpack", I find:

Most of California's water resources accumulate within the snowpack on mountains until it melts, which is usually in the spring. This study uses a statistical model which links snow water equivalent (SWE) measurements over a 60-year time series to clarify the spatial characteristics of snow accumulation trends in the Sierra Nevada. To determine the effects of a monthly and irregular sampling schedule we analysed daily snow sensor data spanning 28 years. Results are presented for monthly and seasonal maximum changes by range-wide elevation bins and by river basin groupings. We found a strong linear elevational component to monthly SWE accumulation trends. Below 2400 m, less snow is accumulating and it is melting earlier. Higher elevations exhibit greater variability, with most stations accumulating more snow and melting earlier. This could be the result of warmer air masses having higher moisture contents.

I took a look at the "mindfully.org" citation you gave. The chart showing decreasing spring runoff is from a 2003 document published by the "Pacific Institute for Studies in Development, Environment and Security", which also contains the "HockeyStick" ... doesn't inspire confidence. In addition, it does not provide a citation for the chart.

Do you have a better citation for the changes in the runoff?

Many thanks,

w.

[83](#)

*John Nicklin* says:

May 30th, 2007 at 8:36 am

82: Come on Willis, you can see the trend, its a flat line.

73: Don, I went to RC and had a look. Apart from the Beck starwman, they have pretty well re-defined science in general.

[84](#)

*muirgeo* says:

May 30th, 2007 at 9:12 am

Willis,

I don't think I said anything about California precipitation trends. It's snowpack and run off trends that matter to California's water supply.

Here I sit in my Tahoe Condo. I'll soon walk Burton creek...literally I'll walk in the creek

bed because it is dry already. The grass is turning brown and the under brush is heavy from years of mismanagement. Sitting east of Burton Creek State Park this place is at higher risk for not being here in 10 years. Early spring thaws, greater drought index, warmer temperatures and more dead trees from bark beetle infestations are all very real effects of a warming climate that will have very real effects on California's like me.

How I wish the funny story of the ?poorly placed Stevenson box down by Fanny Bridge would make the reality of these other issues go away but I'm quite sure I'll see no "Brookies" in the stream today.

<http://pubs.usgs.gov/fs/2005/3018/>

[85](#)

*Steve McIntyre* says:

May 30th, 2007 at 9:30 am

#84. muirgeo, I've never said that no one should take any policy decisions because of poor data. But equally no one should justify poor data because of "big picture" issues. Analysts can and should observe that aluminum tubes may not be evidence of WMD. Decision-makers may well ignore the information, but that's not up to the analysts.

As to the water situation in your areas, you may be interested in the post <http://www.climateaudit.org/?p=956> which observed (also see recent article by Graham, Hughes et al) that modern water levels in Sierra lakes are much higher than in many past periods e.g. the MWP. In a sense, the fact that your lakes are as nice as they are is evidence that the modern warm period is not as warm in your area as the MWP (as also Miller et al 2006, an excellent article). Medieval dryness in California is probably the explanation for low medieval bristlecone growth rates in the MANNian hockey stick.

[86](#)

*Steve Sadlov* says:

May 30th, 2007 at 9:32 am

RE: #84 - Yes, we are having a drought this year. CLUE! Much of California is a semi arid place. Droughts happen.

Back to my original question. How was the SNOWPACK (not precip, but SNOWPACK) in the 2005 - 2006 and 2004 - 2005 seasons? Answer the question. Don't lie.

[87](#)

*Steve Sadlov* says:

May 30th, 2007 at 9:37 am

RE: #86 - I would add that the current drought, low snow pack also come after a very cold winter. California had record breaking cold during the late January through early March time frame. Remeber all the news about the freeze? But sadly, it was dry cold. Little snow

fell during it. In fact, the combo of high winds and low RH during Feb in particular resulted in a net loss of snow / ice via sublimation. Spell it with me now ... S-U-B-L-I-M-A-T-I-O-N. Your big word for the day.

[88](#)

*MarkW* says:

May 30th, 2007 at 1:52 pm

Let's see if I have this straight. The area within a few miles of muirego's house is a little drier than it was a few years ago. Therefore the entire Sierra's are in serious drought. Such a drought has never happened before. (If it had, muirego would most surely remember it.) And thus the entire world is in serious straights unless something drastic is done immeadiately. If not sooner.

Is that the way the new science works?

[89](#)

*Richard deSousa* says:

May 30th, 2007 at 2:03 pm

LOL.... snow and ice is hard water, or precipitation, in case you're not aware...

[90](#)

*Richard deSousa* says:

May 30th, 2007 at 2:04 pm

My response to #84 on #89, which I forgot to reference.

[91](#)

*muirgeo* says:

May 30th, 2007 at 2:34 pm

#84. muirgeo, I've never said that no one should take any policy decisions because of poor data. But equally no one should justify poor data because of "big picture" issues. Analysts can and should observe that aluminum tubes may not be evidence of WMD.

That's what's frustrating to me Steve. The WMD data was extremely poor. We acted on it spending billions of dollars that could have been better spent looking for alternatives to fossil fuels. IMO we acted on this poor data to help the very industry that contributes to both warming and our dependency on oil. The very industry that our Vice President, our President and our Secretary of Sate made their millions from. The data that suggest my Condo is at greater risk for fire, as well as California being at greater risk for water shortages and drought is as follows;

Decreasing spring run off, surface temperature data, snow precipitation data , forest fire frequency data and lake temperature trends. All just one big coincidence right Steve? Man

how I wish the nit pickers of the WMD data were allowed to get similar press and stall action on the invasion of Iraq back in 2003. But it's the same guys in the shadows working both sides of the issue to push the agenda their way with plenty of dupes and puppets providing cover for them because they think they are on their side or because they can't let go of their broken ideology.

Steve when people can be so blind and biased to the facts before them, when they have to look to all sorts of ways to hold their position no matter how fallible, no matter how contrary the facts...that's when bad things happen to societies. I appreciate oversight of science and data.

[snip - no religious references, you know better]

[92](#)

*Philip B* says:

May 30th, 2007 at 3:42 pm

As [Warwick Hughes](#) has documented, the impact of catchment vegetation growth/regrowth on runoff is substantial. Here in Perth, Western Australia, it would appear to have a greater impact than rainfall variability. Which makes me question whether something similar is happening in California.

[93](#)

*Steve Sadlov* says:

May 30th, 2007 at 4:04 pm

RE: #88 - The area near muirgeo's house (and most of California for that matter) has had a dry, cold and windy "rainy season" versus normal. It's actually a bit overdue. We generally average a really dry year about once every 10 or so and more rarely, a multiyear drought. The last notable drought year in NoCal was 1990 - 1991, the end of a dryish series of years 1987 - 1991, which affected SoCal far more than up here. 1993 - 1994 was sort of dry, but we got late precip that saved us. The nature of the beast - it's where we live. Been that way for all of local recorded history and the proxies would say, beyond that. Nothing to see here, move along.

[94](#)

*Steve Sadlov* says:

May 30th, 2007 at 4:10 pm

RE: #89 - The subtlety that's lost on muirgeo is the fact that the amount of snowpack in spring, in a place such as California, is a function of more than precipitation. In the case of this most recent season, there were two factors conspiring. Not only did we not get "normal" precip (not unexpected once in a while - we have very high annual precip variability in most parts of the state) but we also lost lots of snow pack during Feb to sublimation and wind. That's when we had a severe outbreak of very cold and dry air from the MacKenzie Delta. It decimated the snow pack and there was never a recovery.

[95](#)

*Jonathan Schafer* says:

May 30th, 2007 at 7:10 pm

#88,

Of course. Didn't you read how Nancy Pelosi just went to Greenland and "saw the climate change for herself"? I can recall a discussion on Wunderground where someone else attempted to attribute local recent observations of warmer weather as absolute evidence of climate change.

[96](#)

*Reid* says:

May 30th, 2007 at 7:34 pm

Re #95 "Didn't you read how Nancy Pelosi just went to Greenland and "saw the climate change for herself"?"

Pelosi is now an expert in the Gore tradition. Attention will be paid.

Give her an honorary Ph.D. in Climatology.

[97](#)

*Warwick Hughes* says:

May 30th, 2007 at 8:35 pm

I have just seen a BoM Stevenson screen in rural West Australia small town with what appeared to be an instrument sitting on top, exposed to the sky. It looked like a glass rod circa 30 cm long, poss a cm thick, sitting down snug in what resembled a neat timber bed. Could not get close enough to be sure. A photo was taken from as high as we could get, on a mates camera, so pic may take a while.

I wondered if anybody has seen such an instrument ?

Something measuring some solar factor ?

The box looked in good repair, well used looking locks, in a high fenced enclosure, padlocked, just sand underneath, despite many grassed sites in adjacent park. The enclosure was on the edge of this park, adjacent a road with housing across the road.

[98](#)

*Bob Weber* says:

May 30th, 2007 at 8:54 pm

Chris Davey and Roger Pielke Sr document in the Apr 2005 AMS FORUM at

[http://ccc.atmos.colostate.edu/pdfs/BAMS\\_Davey&Pielke\\_Apr05.pdf](http://ccc.atmos.colostate.edu/pdfs/BAMS_Davey&Pielke_Apr05.pdf) has discriptions and pictures of 12 sites in Eastern Colorado.

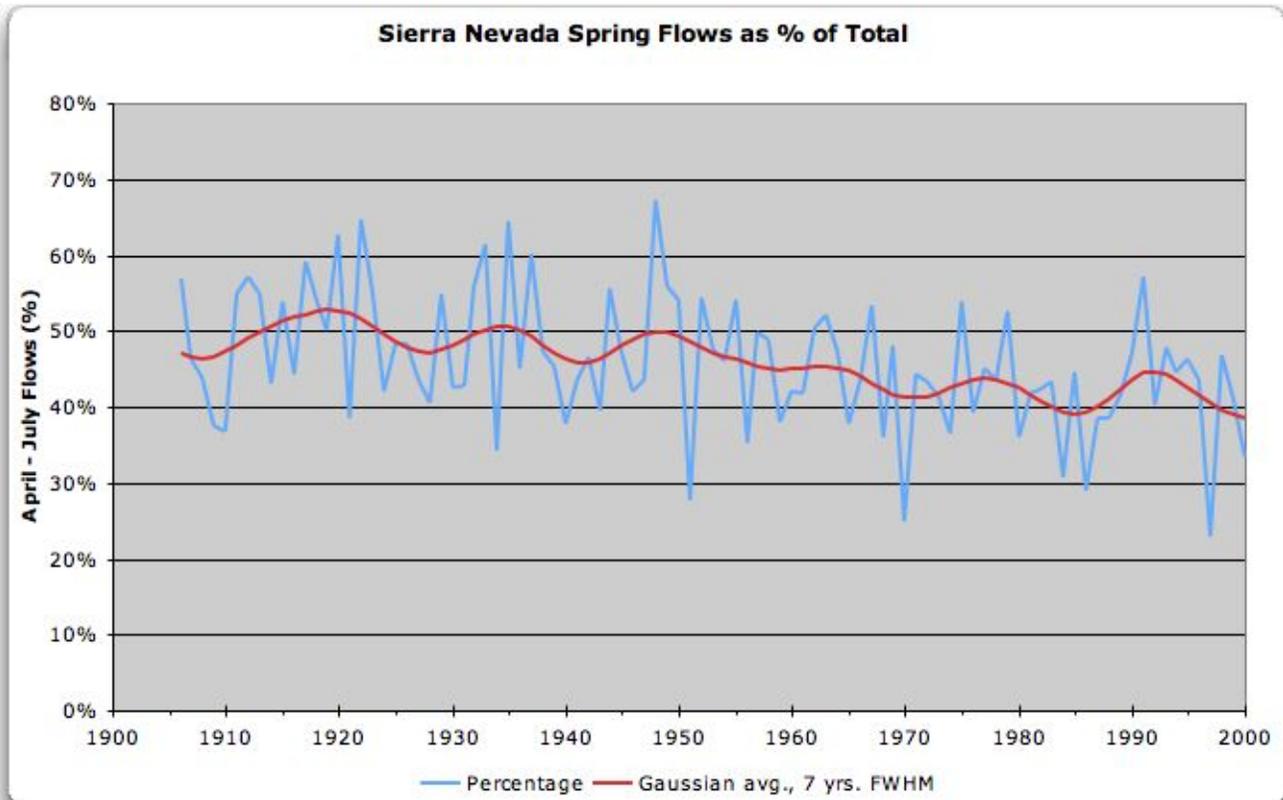
Bob

99

Willis Eschenbach says:

May 30th, 2007 at 10:39 pm

muirgeo, thanks for your post, and the link to information on the Sierra Nevada spring flow. I have graphed it so that I can analyze it.



It looks to me like it was steady from 1900 to about 1945, then decreased to about 1970, and has been steady since then. There is no significant trend post 1960 (the trend since 1970, for example, is less than  $-0.1\%$  per *decade*. That's  $-0.8\%$  per century ...

Now, whatever is causing these changes, there is clearly only a very weak correlation with average temperature – the  $R^2$  value of average California annual temperature vs spring flow is only 0.15. And post 1970, when California temperatures were generally warming, there was no significant trend in the spring flows.

muirgeo, you need to take a hard, mathematical look at data before drawing conclusions. This is particularly true when dealing with natural phenomena with high Hurst coefficients, like rainfall and streamflow data. High Hurst coefficient phenomena create a lot of patterns that look like trends, but are just natural variations. See [Koutsoyiannis](#) for more details.

All the best,

w.

[100](#)

*Roger Dueck* says:

May 30th, 2007 at 10:42 pm

#84

Here I sit in my Tahoe Condo. I'll soon walk Burton creek. Literally I'll walk in the creek bed because it is dry already. The grass is turning brown and the under brush is heavy from **years of mismanagement**.

I think you have summarized the issue quite nicely! The pine-beetle infestation is also a management issue, in that case forest fire suppression, also mis-represented as climate effects.

[101](#)

*MarkW* says:

May 31st, 2007 at 5:16 am

The WMD data was ...

[snip - Steve: OK, I used the analogy, but I don't want to debate the Iraq war. You missed the analogy: a person analyzing aluminum tubes, like a person analyzing the HS, does not exclude other possible reasons to go to war or even other possible evidences of WMD. Only that that particular piece of evidence was flawed. ]

[102](#)

*MarkW* says:

May 31st, 2007 at 6:48 am

I didn't miss the analogy.

muirego specifically stated that all of the evidence was flawed.

If you want to delete my corrections, maybe you should delete the incorrect statements that necessitated the correction as well.

[103](#)

*Gary* says:

May 31st, 2007 at 7:14 am

#39 - Anthony Watts, do you have an update on when your database will be ready?

[104](#)

*Steve Sadlov* says:

May 31st, 2007 at 11:03 am

RE: #99 - I would reckon that the explosive growth of the skiing business and the general increase in small reservoirs must have disrupted aquifers in the Sierra Nevada. Interestingly,

in other parts of California, the replacement of rural land use by suburban and exurban uses has tended to increase spring output - no more need for all those cattle watering dams, etc.

[105](#)

*Ian Hart* says:

May 31st, 2007 at 12:44 pm

#82 - Willis

The data on April to July runoff come directly from the California Department of Water Resources data center. The graph is part of a comprehensive review:

<http://www.energy.ca.gov/reports/CEC-500-2004-073/CEC-500-2004-073-ED2.PDF>

Ian Hart

Communications Director

Pacific Institute for Studies in Development, Environment, and Security

[www.pacinst.org](http://www.pacinst.org)

[106](#)

*Steve Sadlov* says:

May 31st, 2007 at 2:38 pm

RE: #105 - So to summarize the paper, models have been run that predict earlier and possibly less run off. The two key words being "models" and "predict." Nothing in there about actual past results. Typical.

[107](#)

*Anthony Watts* says:

June 1st, 2007 at 1:48 am

RE103 Looks like Monday is when I'll have the online database for USHCN stations ready for entries.

[108](#)

*Steve Funk* says:

June 1st, 2007 at 11:24 pm

The trash burner is probably just being stored there. Burning trash in barrels has been illegal in most of Northern California for a few years. And very few people would be so stupid as to burn trash on top of a flammable wood pallet.

[109](#)

*KevinUK* says:

June 2nd, 2007 at 9:55 am

Steve,

I'm glad that in recent times you've turned your attentions away from dendro studies to the auditing of historic temperature record adjustments. IMO we will never be able to win the AGW battle by having academic debates about 'the noise' that epitomises proxy temperature reconstructions. On the other hand we have a more than fair chance of showing that the current warming trend is an artefact of the 'adjustments' of two organisations (GISS and UEA CRU) who have a vested interest in perpetuating the AGW message. It's becoming increasingly clear thanks to your recent threads that but for the 'adjustments' there is no evidence of a clear warming trend since the 1970s. This particular pillar of the AGW Temple is also about to collapse just like the others (hockey stick, GCM predictions etc).

KevinUK

[110](#)

[UC](#) says:

June 2nd, 2007 at 2:22 pm

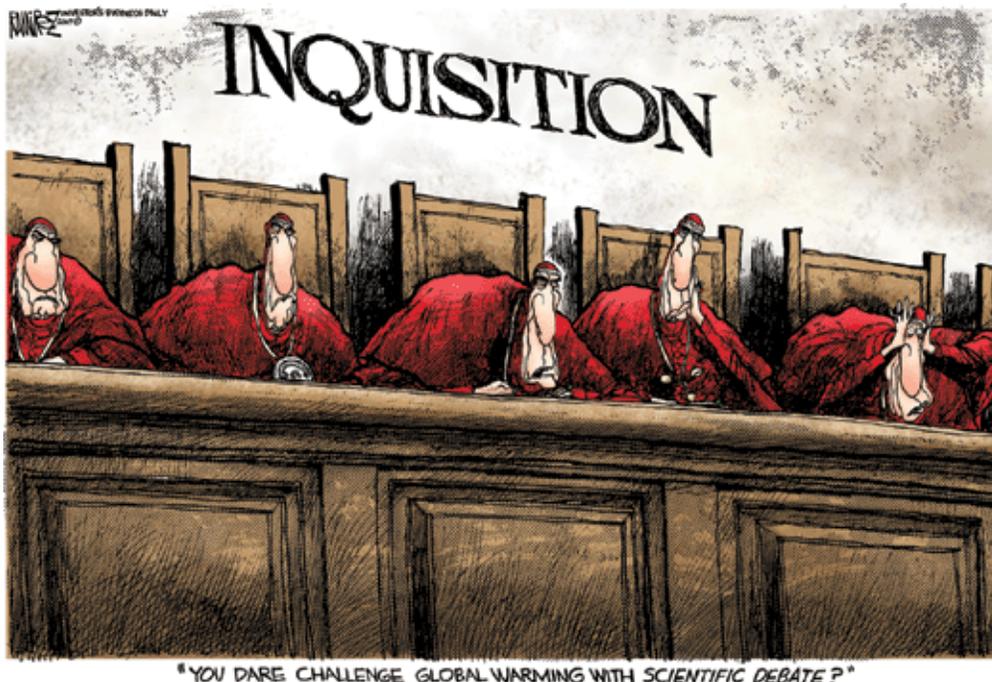
#109

I have some academic background (took some university level courses) here's short comment: AGW or no-AGW, deeply flawed manuscripts have gone through the peer-review process, through the media and criticism is almost illegal now. That's the problem, AGW or no-AGW, that is a different problem. (IOW, count me out of AGW battle, don't want to be involved, and mike probably has my IP address so.. ; ) ).

[111](#)

[MarkR](#) says:

June 2nd, 2007 at 7:19 pm



[112](#)

Neal J. King says:

June 8th, 2007 at 1:23 pm

It seems to me that these issues of placement could throw off the absolute value of the temperature at that location.

But unless you think that the asphalt will get hotter over time, or the electricity from the lamp will get stronger over time, these location peculiarities will only produce a constant temperature offset. Even in the case of the burning can, you have to consider: “Do you expect that they would have been doing more burning, or hotter burning, in order to generate an upward temperature trend?”

If there is no actual trend in temperatures, these circumstances will not produce one.

[113](#)

Steve Sadlov says:

June 8th, 2007 at 4:33 pm

RE: #112 - If a site changed from a good to not so good microsite, there would be an offset at that time. The other factor would be things like the Tahoe City condos getting built. If they were built after the site was established, there would be a ramping offset as they were occupied. As people put more electronic gadgets in their homes during the 90s and 00s, there would be another slight ramp upward.

[114](#)

Neal J. King says:

June 8th, 2007 at 6:36 pm

Steve Sadlov,

- I can see one increment due to development of the environment. But once the asphalt is added, resurfacing won't do anything.

- wrt more electronic gadgets: if increased industrial activity were solely responsible for heating up the cities (more than the countryside), studies of temperature trends *restricted to windy days* would not show much of an increase: because on the windy days, the cities would be sharing their heat with the countryside, and there would be a reduced increase. This point has been studied by Parker in 2004, but there was no difference in the trends for windy days and the trends for other days. The conclusion was that the increase in global average temperature cannot be attributed to the “urban heat island” effect that you are describing.

Reference:

- [http://en.wikipedia.org/wiki/Urban\\_heat\\_island](http://en.wikipedia.org/wiki/Urban_heat_island)
- [http://news.bbc.co.uk/2/hi/uk\\_news/4021197.stm](http://news.bbc.co.uk/2/hi/uk_news/4021197.stm)
- <http://www.nature.com/nature/journal/v432/n7015/abs/432290a.html>

[115](#)

*MarkW* says:

June 8th, 2007 at 7:41 pm

Neal,

You seem to think that there is only one place near a sensor to add asphalt.

There's the parking lot to the east, the road to the west, the apartment complex to the north, etc.

Additionally, the temperature record is made of many stations. Station one had a parking lot added in 1950. Station two had a new road added in 1960, etc.

It's easy for a composite record to show a gradual increasing bias as the area around the sensor deteriorates over time.

In addition, it's not just the area within a few hundred feet of the sensor that affects what the sensor reads.

You'd recognize that, if you weren't concentrating on given snide, nonsensical answers.

[116](#)

*Neal J. King* says:

June 8th, 2007 at 8:16 pm

MarkW,

The other data that go into temperature trend evaluations include:

- isotope ratios of oxygen in trapped gas samples in Vostock, recording a 650,000 year period - unaffected by this station.

- tree-ring studies - unaffected

- studies of corals - unaffected

- satellite studies over decades - unaffected

...

not to mention the studies by Parker (referenced above) that would seem to rule out any significant role for urban heating. Unchallenged since 2004...

[117](#)

*Willis Eschenbach* says:

June 9th, 2007 at 12:42 am

Neal, you say:

... not to mention the studies by Parker (referenced above) that would seem to rule out any significant role for urban heating. Unchallenged since 2004

Unchallenged? Say what? You could start with [this](#). Then you could consider the fact that it's hard to publish anything in *Nature* magazine that disagrees with the AGW party line, as discussed [here](#).

Then you could move on to [this paper](#).

Meanwhile, of course, researchers all around the world continue to report on urban heat island effects both widespread and localized. You might take a look at:

Zhang, J., Dong, W., Wu, L., Wei, J., Chen, P. and Lee, D.-K. 2005. Impact of land use changes on surface warming in China. *Advances in Atmospheric Sciences* 22: 343-348, along with their previous study,

Zhou, L., Dickinson, R.E., Tian, Y., Fang, J., Li, Q., Kaufmann, R.K., Tucker, C.J. and Myneni, R.B. 2004. Evidence for a significant urbanization effect on climate in China. *Proceedings of the National Academy of Sciences* 101: 9540-9544.

When you get done with those, there's

De Laat, A.T.J. and Maurellis, A.N. 2006. Evidence for influence of anthropogenic surface processes on lower tropospheric and surface temperature trends. *International Journal of Climatology* 26: 897-913

For a northern perspective, you might consider the heat island in [Barrow, Alaska](#). As one of the few far north stations, it exerts a huge influence on the Arctic "average temperature". Fairbanks has similar problems.

Near as I can tell, the Parker study is not so much unchallenged as it is ignored. There is general agreement that the Urban Heat Island effect is both real and is having an effect on the global averages. The only question is how much.

Personally, I found the Parker study unconvincing for several reasons.

**First**, a number of studies (such as the Barrow study) have demonstrated that the UHI is real and that it is affected by wind. See, for example,

Yap D. 1975. Seasonal excess urban energy and the nocturnal heat island Toronto. *Archives for Meteorology, Geophysics und Bioclimatology* B 23: 69-80.

Oke TR, Maxwell GB. 1975. Urban heat island dynamics in Montreal and Vancouver. *Atmospheric Environment* 9: 191-200.

Figuerola PI, Mazzeo NA. 1998. Urban-rural temperature differences in Buenos Aires. *International Journal of Climatology* 18:1709-1723.

Morris CJG, Simmonds I. 2001. Quantification of the influences of wind and clouds on the nocturnal heat island of a large city. *Journal of Applied Meteorology* 40: 169-182.

Since this is a well established scientific phenomena, I get very suspicious when Parker fails to find any evidence for it. Extraordinary claims require extraordinary evidence, and his seems shaky.

**Second**, I mistrust the study because he neglected to say how he chose his stations. He says, for example:

To assess the effect of time differences between the reanalysis daily-average winds and Tmin, I repeated the analysis using 26 stations in North America and Siberia that have hourly or six-hourly reports of simultaneous temperature and wind.

Well, that's nice ... but what 26 stations did he choose, and what criteria did he use? The main study looked at "264 stations worldwide" ... what stations?

**Third**, he is not using actual wind data. He is using NCEP/NCAR reanalysis data, which is heavily massaged and smoothed. Why not use real data?

**Fourth**, he compared re-analyzed daily average wind speeds with night-time or early morning (minimum) temperatures. Since average wind speed is generally much higher in the day than in the night, the effects of this are unknown.

**Fifth**, he makes an odd statement. He says:

For stations between 140° E and the dateline, Tmin which occurs most frequently in the early morning was matched with the previous day's speed. This is because the early morning in terms of universal time (equivalent to Greenwich Mean Time) is still in the previous day in the Far East.

This means that when he says the "daily" average winds, he means from zero GMT to midnight GMT. Thus, depending on the location, this could mean:

- 1) Yesterday and last night.
- 2) Half of last night, today, and half of tonight
- 3) Today and tonight.

Seems like that problem alone would be enough to ruin the study ... unless you believe that today's minimum temperature depends on tonight's winds ...

w.

[118](#)

*Neal J. King* says:

June 10th, 2007 at 7:32 am

Willis,

I am in process of looking at your references. This will take some time.

In the meantime, I point out that some of the links (to blue.atmos....) do not seem to work: You have one, and some of your referenced articles have others.

More later...

[119](#)

*Neal J. King* says:

June 12th, 2007 at 5:30 pm

117: Willis Eschenbach:

I've been able to find most of the references you have mentioned, except for the ones at //blue.atmos., which were not accessible.

In response, let's first focus on what Parker's study was trying to show.

Parker did **not deny** an "urban heat island" effect, in the sense that an urban area could have a warmer temperature than a rural area. What he argued was that, in the event of a windy night, the difference between urban and rural temperatures would be vitiated. He further argued that, if the **increase over time** in the globally averaged temperature was due **solely** to increased urbanization or heat-producing activity locally, then the measured trend of increase should likewise be vitiated when looking at the temperature trends only on windy nights.

Parker did indeed find a temperature difference between windy and non-windy nights. In his graphs of minimum temperature over time, the two graphs track quite well, but with an essentially constant offset. In particular, the increase in temperature over time is the same, nearly everywhere: there is no detectible difference-in-trend. The simplest interpretation that comes to mind: i) The UHI works on calm nights; and, ii) the temperature is increasing overall, both on windy and on non-windy nights.

Now let's consider your references:

- The first reference you link to is [Pielke Sr.'s weblog discussion on his comment on Parker's paper](#). He discusses [the paper by Gonzalez et al.](#), which suggests a UHI difference-in-trend temperature issue in Puerto Rico. It is not clear if Parker's study (which was poor in tropical stations) covered Puerto Rico. But what is happening with Puerto Rico may not have been happening with the areas that Parker's study covered: he detected no UHI

difference-in-trend.

- Your second link is to [Pielke's complaint](#) that he couldn't get his comment on Parker's paper published in *Nature*. After some time reading what he wrote (which was not very clear on its first presentation), as well as [the abstract of the note that was published subsequently in GRL](#), I came to the conclusion that the three reviewers delegated by *Nature* were pretty much right: the point Pielke seemed to be making was merely a subsidiary point (that the situation might be even more complicated than Parker suggested) that did not change the significance of what Parker did find: *No difference in temperature trends on windy vs. non-windy days*. That being the case, I don't see why Pielke should expect that every comment on a published paper deserves to be published in the same journal: If a journal undertook such a policy, how many original articles would they be able to publish? Perhaps Pielke should rather seek consolation in the fact that Enrico Fermi's original paper on the theory of the weak interaction was also rejected by *Nature* - for being too speculative.

- The third paper you linked, <http://blue.atmos.colostate.edu/publications/pdf/R-302.pdf>, I couldn't access: "Server not found."

- The next is: [Zhang, J., Dong, W., Wu, L., Wei, J., Chen, P. and Lee, D.-K. 2005. Impact of land use changes on surface warming in China. Advances in Atmospheric Sciences 22: 343-348](#). This paper concludes that some % of the increased temperature trend measured in China is due to UHI and increasing industrialization: about 29%. **What about the 61%?**

- The next is: [Zhou, L., Dickinson, R.E., Tian, Y., Fang, J., Li, Q., Kaufmann, R.K., Tucker, C.J. and Myneni, R.B. 2004. Evidence for a significant urbanization effect on climate in China. Proceedings of the National Academy of Sciences 101: 9540-9544](#). This paper chimes in with the above: It estimates a rate of heating of 0.05-C per decade due to rapid urbanization in China. "Because the present analysis is from the winter season **over a period of rapid urbanization and for a country with a much higher population density, we expect our results to give higher values than those estimated in other locations and over longer periods. Therefore, our estimates do not represent the urbanization effect globally, nor do they represent the average of all seasons over the past 100 years for which station temperature data are available.**"

- I don't know why you are citing the article on the Barrow UHI, because while it demonstrates the existence of the UHI, it also indicates that a windy night will blow away the effect. **To me, this constitutes strong confirmation that Parker's basic methodology is correct: If you want to see what the temperature looks like without the UHI, let the wind blow.** [Hinkel, Kenneth M., Nelson, Frederick K.E., Klene, Anna E., Bell, Julianne H., The Urban Heat Island in winter at Barrow, Alaska. Int. J. Climatol. 23: 1889-1905 \(2003\)](#). "It is clear that a strong UHI develops preferentially on calm winter days." "The strength of the UHI increased as the wind velocity decreased, reaching an average value of 3.2 degrees-C under calm (de Laat, A.T.J. and Maurellis, A.N. 2006. Evidence for influence of anthropogenic surface processes on lower tropospheric and surface temperature trends. International Journal of Climatology 26: 897-913. Here they are clearly claiming that, although global warming trends cannot be natural, they don't match with the

expectations of a greenhouse-effect warming, and that it must be due to some other anthropogenic cause, such as land-use changes. I have two comments: a) When I look at the graphs of Figure 1, as a matter of mathematics, I would expect that lower (blue) curves would, at the right hand side, end up with the same value as the upper (red) curves at the far left. Now, the far-left points are not really visible, but for 3 of the 4 graphs, the curves seem to be leveling off to the left, with a considerable gap between the upper and lower curves. I wonder why they didn't extend the graph all the way to the left? There may be a perfectly innocent reason for this, but since I can't check my expectation against their graph, I'm left wondering if I really understand their methodology of thresholding & averaging. b) It doesn't really address to the question of what could be going wrong with the Parker methodology, which seems pretty straightforward to me.

Now to your objections:

- First: Parker does not deny the UHI, and he most explicitly assumes that it **is** affected by wind! His point is that he doesn't see that it can be responsible for misleading us about the global trend, at least over the stations of his study. (I looked over the references that I could find in your paragraph, and didn't find anything that wasn't addressed adequately by my comment.)
- Second: In addition to the rather brief communication in *Nature* in 2004, Parker published [a longer article in the \*Journal of Climate\*](#). I suppose more details are given there; although I don't have a subscription, so I can't check myself.
- Third: ditto.
- Fourth: ditto.
- Fifth: Actually, there is one more source of information: Parker gave [a talk at an AMS meeting in January 2005](#), that was recorded and made available on the internet. In that talk, he went into some detail explaining about the pains he took to make sure that wind measurements corresponded with the temperature readings. Whatever you think of clarity of his wording in *Nature*, it's clear in the talk that he was thinking about exactly that issue.

[120](#)

Steve Sadlov says:

June 12th, 2007 at 7:30 pm

Why do you assume that anthropogenically generated heat flow is limited to cities? And is there no role played by land use modifications and land management practices?

[121](#)

Neal J. King says:

June 12th, 2007 at 8:13 pm

Steve Sadlov,

**“Why do you assume that anthropogenically generated heat flow is limited to cities?”**

Let's keep our eye on the ball: The purpose of Parker's experiment was to test the commonly proposed hypothesis that the combination of the UHI effect with increasing

urbanization and/or increasing intensity of energy use could generate the *appearance* of an increase in global average temperature, even if nothing were happening to the *actual* global average temperature. The suggestion is that the increase in GAT might be merely an *artefact* of where the weather stations are placed, and how cities may have grown around them.

If you then fall back to the position that this apparent increase is due to anthropogenic heating outside the cities, you've made two key concessions:

- a) Since the temperature increase is widely observed, that this apparent increase in GAT is, in fact, an actual increase, of global footprint & impact; and
- b) It is due to human behavior.

You then must renounce forever after such arguments as:

- climate has always changed, so this is nothing new;
- human beings are too insignificant to change the climate;
- it could be due to variations in solar luminosity;
- maybe sunspots; or
- possibly cosmic-ray fluxes.

All of these explanations inhabit a different conceptual universe than the one you have adopted to explain Parker's results without Parker's explanation.

What does this fall-back position gain you? Well, it allows you to deny that the observed increase in GAT has anything to do with increased C-O<sub>2</sub> concentrations. Hooray! But then, we know that C-O<sub>2</sub> has increased about 33% over 100 years. What *should* that be doing to the heat balance?

**“And is there no role played by land use modifications and land management practices?”**

Yes, there is a role, and in fact the IPCC AR4 report estimates how much of a role that has played. In fact, just to cover the literature reviewed above, that extra input may be needed to explain what is observed in Puerto Rico and some cities in China. But the numbers arrived at so far don't get C-O<sub>2</sub> emissions off the hook.

And if they did, from a policy perspective, would you expect it to be easier to implement a change in land-use than a change in fossil-fuel consumption?

[122](#)

*John Baltutis* says:

June 13th, 2007 at 12:59 am

Re: #119

I've been able to find most of the references you have mentioned, except for the ones at //blue.atmos., which were not accessible.

Probably due to a comma or period in the link you tried.

<http://blue.atmos.colostate.edu/publications/pdf/R-302.pdf> is available [Here \(pdf or html\)](#)

[123](#)

*Neal J. King* says:

June 13th, 2007 at 1:13 am

122: John Baltutis:

The first URL still doesn't lead to a findable server.

The second link does, but the equations of the paper are all messed up.

[124](#)

*MarkW* says:

June 13th, 2007 at 5:16 am

The hypothesis that a windy night will negate the UHI is dependant on the size of the city, the direction of the wind, and any land use changes in the area surrounding the city.

The experiment fails to account for any factor other than the one the author is looking for.

[125](#)

*Steve Milesworthy* says:

June 13th, 2007 at 7:02 am

#124 MarkW

None of the papers you quote directly dispute the conclusions of the Parker papers. The Parker papers accepts that a UHI signal was found in 13 out of 290 stations. Fairbanks, which you refer to is mentioned in the Parker J Clim paper. The J Clim paper also references positions of all the stations and where the data came from, so it should be auditable. I think the J Clim version needs to be read rather than the Nature brief communication.

Your proposition that the underlying hypothesis is wrong is disputed by one of the papers you quote. The UHI effect in winter in Barrow drops from 3C to below 0.5C on a reasonably breezy day (>12 knots).

Hence the hypothesis is plausible, and the test is reasonable given the data available.

“A Demonstration That Large-Scale Warming Is Not Urban” David E. Parker Volume 19, Issue 12 (June 2006)

[126](#)

*MarkW* says:

June 13th, 2007 at 8:42 am

The problem is that the hypothesis is not plausible.

[127](#)

*jae* says:

June 13th, 2007 at 9:46 am

Your proposition that the underlying hypothesis is wrong is disputed by one of the papers you quote. The UHI effect in winter in Barrow drops from 3C to below 0.5C on a reasonably breezy day (>12 knots).

?? Maybe I'm missing something here, but 0.5 degrees is still a lot, when the total warming over a century is reported to be 0.6 degrees.

[128](#)

*Steve Milesworthy* says:

June 13th, 2007 at 10:24 am

#127

This is one part of a self-consistent chain of reasoning in the Parker paper.

The Parker paper is based on the hypothesis that UHI should affect temperature trends more on a quiet day than on a windy day.

Evidence from Barrow is one piece of proof for this hypothesis.

After much careful analysis from Parker, he takes the view that the vast majority of station measurements do not show evidence of the UHI effect as the temperature trend from the windiest days is mostly the same as the temperature trend from the quietest days.

However, 13 out of 290 sites do show UHI. So the question should be is what is the *increase in the UHI effect over time*. For example, today Barrow has a 0.5-3C bias (average 1.75C say) perhaps 50 years ago the bias was much smaller, say 0.3-1.5 (average 0.9C). So the increase is 0.85C in 50 years (or 0.13C per decade). Assuming that all 13 problem sites were similarly affected, multiply this figure by 13/290 and you get 0.006C per decade increase in the global-measured amount, which is small compared to the total increase.

[129](#)

*Steve Sadlov* says:

June 13th, 2007 at 10:38 am

Neil, get off the philosophy. I get really annoyed by fancy foot work, I am a simple guy. If the "rural" data points against which "urban" ones are compared, looking for UHI effects on windy nights, are themselves neither truly rural, nor, free of micosite issues, nor, free of anthropogenic heat flow and other anthropogenically induced error and bias factors, than

how can Parker's stance be defended?

[130](#)

*jae* says:

June 13th, 2007 at 10:46 am

128, thanks for the response. I don't have the time to read the paper, so I guess I'll just trust that only 13 sites showed UHI.

[131](#)

*Dave Dardinger* says:

June 13th, 2007 at 12:33 pm

re: #128

The Parker paper is based on the hypothesis that UHI should affect temperature trends more on a quiet day than on a windy day.

The Problem is that this hypothesis is likely wrong or nearly so. The problem with urban landscapes in the first place isn't that air in them sits around waiting to warm up, but that there are lots of sources of heat storage and IR absorption / emission in cities which aren't present in rural areas. Further if heat in the air gets high enough, it will set up convection cells which can either carry away excess heat or will create / increase winds.

Now perhaps we could discuss to what extent the above hypothesis might hold, but until it's accepted, any discussion of the Parker paper is moot.

[132](#)

*Steve Sadlov* says:

June 13th, 2007 at 1:17 pm

The Parker paper is one of the most common red herrings thrown out there to try and discredit concerns about anthropogenic energy dissipation and surface modification influences on the surface temperature record. It's considered sacred ground, how dare anyone question or critique it .....

[133](#)

*Mark T.* says:

June 13th, 2007 at 2:24 pm

- isotope ratios of oxygen in trapped gas samples in Vostock, recording a 650,000 year period - unaffected by this station.

Extremely coarse time resolution with huge error bars.

- tree-ring studies - unaffected

Already flawed by any objective scientific measure, so “unaffected” translates to “still worthless.”

- studies of corals - unaffected

Immaterial since these aren't good measures of any global trend.

- satellite studies over decades - unaffected

Which currently do not provide results consistent with the ground trends.

You also forget that these temperature recordings are what are used to calibrate, and validate, all the above. Should the recordings be off significantly, the calibrations/validations are as well. I.e., the ice cream cone will no longer lick itself.

Mark

[134](#)

*Neal J. King* says:

June 13th, 2007 at 3:12 pm

#124: MarkW

No, the hypothesis is that a windy night will vitiate the UHI effect. And by what reasoning would you conclude that direction is important?

Experiments generally test the factors that the designer of the experiment has in mind. People with other factors in mind should design their own experiments.

#126: MarkW

What is implausible about thinking that a windy day will cool locally generated heat? Isn't that why people turn on fans to blow away hot air?

[135](#)

*Neal J. King* says:

June 13th, 2007 at 3:40 pm

#127: jae

The effect in Barrow involves a city located in an extremely cold environment. Since the people in Barrow would attempt to maintain something approximating normal temperatures indoors, I would think that the UHI effect is much bigger for Barrow than for Dallas or San Francisco.

So the 0.5-degree effect for Barrow would translate to a much lower effect for those two cities.

#130: jae

My reading of Parker's paper is that most of the sites showed a UHI: but there was no evidence of a trend in the UHI, that would imply the GW to be actually an artefact.

[136](#)

*Neal J. King* says:

June 13th, 2007 at 3:47 pm

#129: Steve Sadlov:

If there is nothing special about rural sites, then my conclusion would have to be that the whole countryside, rural & urban, is indeed heating up.

If the sites are affected by microsite or heat flow, why don't they show up when the wind should blow these effects away?

The matter is indeed simple: If there is no difference in trend when these proposed factors are present or when they are blown away, they cannot be important.

No philosophy or fancy foot-work needed. It's really straightforward.

[137](#)

*Jim Johnson* says:

June 13th, 2007 at 3:47 pm

Neal King,

“But unless you think that the asphalt will get hotter over time, or the electricity from the lamp will get stronger over time, these location peculiarities will only produce a constant temperature offset.”

Not necessarily true. One has to remember that what is being measured is temperature, not energy, and that temperature is driven by disparate inputs. Consider two primary groupings of those inputs: radiative and convective. A microclimate driver like asphalt is far more affected by the former than the latter. At an unpaved site, the temp may stay the same during a year when the balance of radiation vs convection shifts toward the former. At a paved site, temp will rise under those conditions.

Do we have any indication that there is an upward trend in radiation? Yes we do. Heard about it on NPR yesterday. The ‘protective aerosol layer’ is going away! Its been keeping us safe from the ‘global warming’! WE ARE ALL GOING TO DIE! LOL. Is that increase in radiation at the earths surface causing a net increase in temp? Cant tell if you are measuring a site that overstates radiative input vs the surrounding region it is supposed to represent.

“Even in the case of the burning can, you have to consider: Do you expect that they would have been doing more burning, or hotter burning, in order to generate an upward

temperature trend?â€?

And the answer to that is likely 'yes'. As more units are built, the incinerator gets more use.

“If there is no actual trend in temperatures, these circumstances will not produce one.”

Not true.

You are also overlooking the fact that it is not necessary for there to be a trend at any particular location for it to induce an apparent trend in the aggregate. When combined with a mess of other stations that experience a 'one time increase' at different times - 'poof' - a trend.

And that is apart from the effect that even a 'one time increase' may have, if that station is used to 'adjust' out some of the trend seen in a 'nearby' station.

And those other 'temp reconstructions'? Unaffected by the 'surface record' that they are calibrated to, and that is often grafted onto their tails in place of truncated divergences? LOL.

“What is implausible about thinking that a windy day will cool locally generated heat? Isn't that why people turn on fans to blow away hot air?”

Typically, they turn on fans to increase air flow and thus increase evaporative cooling of their perspiring bodies. Anthony hasnt found a temp sensor with a sprinkler on it yet, but I wouldnt discount the possibility.

[138](#)

*Steve Sadlov* says:

June 13th, 2007 at 3:52 pm

RE: #136 - But back to methodology. Parker claims there is not a difference between windy and non windy nights. What is the basis for his claim? I contend that the basis for his claim is flawed. He expected UHI to be limited to major urban areas. His "datum" was supposed rural areas. But he was wrong. It's not a datum when the datum tracks along with what you are trying to contrast against the datum, at least the extent that is makes the measurement you are trying to obtain similar in maginitude the the sum of errors.

[139](#)

*Neal J. King* says:

June 13th, 2007 at 4:07 pm

#131: Dave Dardinger:

- Even if the urban areas have hot areas that "store heat", blowing wind will reduce the difference in temperature between the hot spots and the cooler countryside. That is, indeed, why people turn on fans. Reducing this difference would reduce any trend that was caused

as an artefact of misinterpretation of this difference.

Since no change in trend was observed, Parker's deduction was that there was no artefact.

- If hotter urban temperatures were leading to windy nights, the UHI would be higher on windy nights. But that is in fact the opposite of what Parker found.

So, inasmuch as I see no support for your hypotheses, Parker's paper is very much on the table.

[140](#)

*Neal J. King* says:

June 13th, 2007 at 4:10 pm

#132: Steve Sadlov:

Yes, I know you don't like Parker's study. But you haven't demonstrated any problems with it.

[141](#)

*Steve Sadlov* says:

June 13th, 2007 at 4:25 pm

RE: #140 - You don't think that a faux "rural" datum is a problem? What about the overall enthalpy picture. He sort of arm waved around that. Look, I've got real work to do. Good bye.

[142](#)

*Steve Sadlov* says:

June 13th, 2007 at 4:27 pm

BTW your #139 shows you would have flunked heat flow.

[143](#)

*jae* says:

June 13th, 2007 at 4:28 pm

“What is implausible about thinking that a windy day will cool locally generated heat? Isn't that why people turn on fans to blow away hot air?”

The wind does not affect the radiation component, which I submit is very large when the station is located in an area covered by asphalt. Since asphalt is black, it absorbs almost all visible light, and becomes very hot on sunlit days (even on a windy day). Much hotter than most other surfaces. This heat is continually radiated as IR, continuing through the night. I just can't see any way that temperature measurements are not affected significantly by the presence of asphalt. The same issue applies, to a lesser extent, to locations near structures, especially stone and concrete structures. There simply HAS to be a significant "UHI" effect

for stations so affected. It doesn't matter whether it is in a "rural" location or an "urban" location.

[144](#)

*Neal J. King* says:

June 13th, 2007 at 4:40 pm

#133: Mark T.:

- Vostock: I don't see any evidence of error bars so huge that they threaten the conclusion. Can you give a visible reference?

- Tree rings: One would have to subtract out the effect of C-O2. Could be difficult...

- Studies of coral: Every temperature measurement is local. The question is, What does it imply? Look at them over the world.

- Satellite temperatures: As far as I know, the discrepancy between surface and satellite measurements was resolved in 2005.

As far as I know, these methods are not calibrated against ground station measurements. How would you attempt to calibrate temperatures derived from corals against those from weather stations, for example?

[145](#)

*Neal J. King* says:

June 13th, 2007 at 4:48 pm

#138: Steve Sadlov:

The basis for Parker's claim that there is not a difference in measured trend between windy and non-windy nights is that he measured them - and there was no difference in trend.

Except in northern Europe, where he did indeed find an increasing UHI trend. But that didn't do much for the global average non-trend.

[146](#)

*Neal J. King* says:

June 13th, 2007 at 5:01 pm

#137: Jim Johnson:

- It's very common to disaggregate the nighttime from the daytime measurements by looking at daily minimum as well as daily maximum and daily average temperatures. Part of the reason is that the greenhouse effect is expected to be more effective at increasing the minimum temperature than affecting the maximum. That would sidestep any questions concerning heating by radiation, since the sun is not noted to shine much at night.

- What temperature reconstructions and calibrations are you thinking about?

- wrt to windy vs. non-windy nights: Nonetheless, if you are in a hot building (heated by a dark roof) and turn on the fan, you are likely to mix air from outside the building (and thus cooler) with the air inside the building. The result is that some of the hot air leaves and some of the colder air enters. Viewing this from a perspective not confined to the building, the fan forces a mixing of hotter and cooler air, resulting in spreading the heat about a larger volume and reducing the temperature in the house.

Surprise! There is nothing surprising in this. A windy night will cool a hot town more than a non-windy night.

[147](#)

*Neal J. King* says:

June 13th, 2007 at 5:05 pm

#143: jae:

In his measurements, Parker separated minimum daily temperatures from maximum and average temperatures. So this sidesteps all possible interference from sunlight.

[148](#)

*Douglas Hoyt* says:

June 13th, 2007 at 5:30 pm

All that Parker's study showed is that the urban heat island is so strong and persistent that winds are not sufficient to dilute it. Or put another way, the fan is too small to cool the room to any measureable extent.

Barrow is a special case. I have been there. It is on the ocean and the surrounding land is very flat with no trees or large buildings to impede air flow. The roads are not paved. Only the airport, which is a ways away from the town, is paved. So there is no material with large heat capacity in town to store heat. Thus, in this case, the weak UHI can be blown away.

[149](#)

*Jim Johnson* says:

June 13th, 2007 at 5:43 pm

Jae,

“The same issue applies, to a lesser extent, to locations near structures, especially stone and concrete structures.”

I would argue to a larger extent, especially wrt non-stone-and-concrete structures, when those structures are heated. And/or, when they are cooled and the temp sensor is placed in the exhaust stream of the cooling unit.

Neal,

“It’s very common to disaggregate the nighttime from the daytime measurements by looking at daily minimum as well as daily maximum and daily average temperatures.”

It is also very common not to.

“Part of the reason is that the greenhouse effect is expected to be more effective at increasing the minimum temperature than affecting the maximum. That would sidestep any questions concerning heating by radiation, since the sun is not noted to shine much at night.”

Asphalt, pavement and masonry, on the other hand, are noted to ‘shine’ at all hours. As are heated structures.

When I walk home, I pass a brick building that is sensibly warm at a distance of several feet, well after dark. When I park my truck in my driveway next to my frame house, on a cold morning I will find frost or snow on the windows on the side away from the house, and clear windows next to the house. For you to argue from a standpoint that is uninformed by these common, easy to understand phenomena indicates that you probably shouldn’t be.

The colder it gets outside, the more energy my house radiates. The warmer it gets outside, the more my air conditioner runs. An inappropriately sited sensor would see clipped lows and exaggerated highs. And over the last twenty years, the BTU rating on both my heating and cooling units has increased.

You can not dismiss these effects with ignorance.

“As far as I know, these methods are not calibrated against ground station measurements.”

I’m often inclined to agree with that assessment. Nevertheless, they purport to be. LOL.

[150](#)

*jae* says:

June 13th, 2007 at 5:44 pm

146, Neal:

That would sidestep any questions concerning heating by radiation, since the sun is not noted to shine much at night.

But the asphalt continues to “shine” at night.

[151](#)

*Mark T.* says:

June 13th, 2007 at 6:03 pm

- Vostock: I don’t see any evidence of error bars so huge that they threaten the

conclusion. Can you give a visible reference?

Two points... 1) 650k years of cores, the minimum resolution is very large, not yearly. 2) Exactly what are they calibrated against over the 650k year period? As far as I know, there are no "error bars" in the Vostok cores published (I've never seen any, UC/bender/others may know of some). I cannot imagine any way in which they could even be calculated. They're used as rough estimates.

The temperature resolution of a Vostok core can't be very low. If it were anyway, why would we need all the other methods? Also, Vostok is Antarctica, right? We already know that Antarctica is not following the current NH trend (it is mostly cooling), so exactly how accurate is a core for predicting global, or even NH temps, when it does not follow the currently measured trend?

- Tree rings: One would have to subtract out the effect of C-O2. Could be difficult

It's going to take a little more than just that. We'll also have to subtract out the effect of other known forcings, such as soil fertilization, soil moisture, total solar irradiance. We're also going to need to understand how a summer growth measurement can accurately portray yearly average temperatures. This does not even get into the problems of non-linearities and the fact that many of the forcings are correlated (by hypothesis CO2 and temp are correlated).

- Studies of coral: Every temperature measurement is local. The question is, What does it imply? Look at them over the world.

Last time I checked, coral only grows in the ocean, and it is affected by many things non-temperature related.

- Satellite temperatures: As far as I know, the discrepancy between surface and satellite measurements was resolved in 2005.

Uh, no. The errors in the original calibration were resolved, but the satellite measurements still do not indicate warming that is predicted by "the theory." The graphic in the other thread clearly indicates that temps have flattened, and even cooled over the last 5 years as well.

Mark

[152](#)

*Neal J. King* says:

June 13th, 2007 at 6:06 pm

#148: Douglas Hoyt:

You have suggested:

“All that Parker’s study showed is that the urban heat island is so strong and persistent that winds are not sufficient to dilute it. Or put another way, the fan is too small to cool the room to any measureable extent.”

In fact, Parker even noted some local differences in trend, but they did not affect the global average:

“The global annual result conceals a relative warming of windy nights in winter in the extratropical Northern Hemisphere (Fig. 1b), mainly in western Eurasia. The observed tendency to an increased positive phase of the North Atlantic Oscillation implies that the windier days in western Eurasia had increased warm advection from the ocean, yielding greater warming. In summer in the extratropical Northern Hemisphere (Fig. 1c), there was no relative change in Tmin on windy nights. At that time of year, atmospheric circulation changes are less influential, but an urban warming signal is still absent. In the tropics, calm nights warmed relative to windy nights on an annual average, but only by  $0.02 \pm 0.01 \text{ }^\circ\text{C}$  per decade, which is much less than the overall tropical warming in Tmin ( $0.16 \pm 0.03 \text{ }^\circ\text{C}$ ).”

So it would seem strange is so weak that it doesn’t show the effect you would normally expect, but does show what might be expected to be a rarer effect.

[153](#)

*steven mosher* says:

June 13th, 2007 at 6:06 pm

Neil,

Parker’ study sounds fascinating. Is there a link? Does he have a list of stations studied? Did he take Photo’s of the station taken according to CRN standards? It would make a good addition to the database.

[154](#)

*Mark T.* says:

June 13th, 2007 at 6:07 pm

Oh, I forgot to add, the point of all this has nothing to do with other measurement methods (need to maintain an on-topic vein). The point of the thread (and others that are similar) is whether the network is now biased more than it was during, say, the 1930s when all these confounding factors did not exist. That you would interject the “but all the other methods are not invalidated because of some bad sites” is really a strawman. No kidding. The other methods will stand or fall based on their own merits. The problem arises when the other methods are used to validate the theory by comparison to measurement, yet both the measurement and other methods are fraught with errors. As I said, the ice cream cone ceases to lick itself and thus “the theory” needs some revision.

Mark

[155](#)

*Neal J. King* says:

June 13th, 2007 at 6:08 pm

#152: Clarification

So it would seem strange that the wind is so weak that it doesn't show the effect you would normally expect (cooling), but does sometimes show what might be expected to be a rarer effect.

[156](#)

*Steve Sadlov* says:

June 13th, 2007 at 6:15 pm

Disruptor! One of the "white blood cells" noted in a recent critique of the IPCC orthodoxy.

[157](#)

*Neal J. King* says:

June 13th, 2007 at 6:17 pm

#149: Jim Johnson:

a) Parker distinguished between Tmin and Tmax in his analysis.

b) I think you (and Douglas Hoyt) are missing a very important point: Parker DID see that windy nights show cooler than non-windy nights. You CAN blow away the UHI.

c) What DIDN'T show up was any tendency for the non-windy nights to warm up more than the windy nights. Which suggests very strongly that the warming trend had nothing to do with UHI.

What b) shows is that the wind measurably affects the UHI: it shows up, buildings or no buildings, asphalt or no asphalt.

What c) shows is that

[158](#)

*Neal J. King* says:

June 13th, 2007 at 6:21 pm

#149: Jim Johnson:

Please explain (with references) how it is claimed that they are calibrated wrt weather-station measurements.

Specifically, my understanding of isotope measurements of all kinds is that they are based upon calculations of chemical kinetics, from first principles of molecular dynamics: reaction-rate calculations. No need to check what some weather station was doing.

[159](#)

*Steve Milesworthy* says:

June 13th, 2007 at 6:31 pm

#148

I'm a little gobsmacked by this statement - so apparently the UHI can only be detected when it is very weak. And somehow every other station has suddenly been infected by such a vast amount of urban renewal that no wind can affect it?

Now it seems quite clear from information on this site that not every station is perfect. But thousands of stations are pretty good, being situated well away from immediate radiative impacts of asphalt and concrete. Surely you must understand that most observers are equally as anally-retentive as you lot and will notice the impacts of any very close-by developments.

[160](#)

*steven mosher* says:

June 13th, 2007 at 6:34 pm

Neil,

I found the Link to Parkers study. Shame that it costs 30 bucks. Anyways, if you have the study could you list the sites he used. Or provide a link to the data he archived for the study? Oh, did he post Source code as well for is analysis routines? That would be most helpful in repeating the analysis.

Anyways, if you know Mr Parker do you think he would be so kind as to forward me a free copy.

Best regards

[161](#)

*Neal J. King* says:

June 13th, 2007 at 6:34 pm

#151: Mark T.

- Not being intimately involved with all these techniques, I can't give a full response. (I do plan to learn a bit more about the Vostock cores; there is a book by Alley on this topic that is supposed to arrive next week.) However, it is interesting that all the evidence we have, however limited it may (or may not) be, points in the same direction.

- Satellite temperatures: That discrepancy is probably the stablest issue I've heard about. But there was that other radiosonde issue that lasted 10 years before someone found a mistake in their algebra. I'll take "preponderance of the evidence".

- I don't take seriously temperature trends without a 5-year running average. Looking at the general behavior of the 5-year curve, it's wiggly enough that I wouldn't worry about it unless it continues flat (or declines) for another 5 years.

[162](#)

*Douglas Hoyt* says:

June 13th, 2007 at 6:38 pm

I stand by my earlier statement. The heat capacity of cities is so large that winds will only cause a minor cooling. These same winds will also cool the nearby rural stations by just about the same amount that cities are cooled.

[163](#)

*Stan Palmer* says:

June 13th, 2007 at 6:39 pm

But thousands of stations are pretty good, being situated well away from immediate radiative impacts of asphalt and concrete.

What evidence do you have or this?

[164](#)

*Neal J. King* says:

June 13th, 2007 at 6:49 pm

#153: steven mosher:

Here is the original article:

[David E. Parker, NATURE, p. 290, VOL. 432, 18 NOVEMBER 2004: "Large-scale warming is not urban"](#)

Here is a follow-up. Unfortunately, I could only access the abstract:

[David e. Parker, Journal of Climate, pp. 2882-2895, Vol. 19, Issue 12 \(June 2006\): "A Demonstration That Large-Scale Warming Is Not Urban"](#)

Finally, there is [a recording of a talk](#) he gave at an AMS meeting: "Large scale warming confirmed by temperatures in windy weather"

[165](#)

*Neal J. King* says:

June 13th, 2007 at 6:55 pm

#160: steve mosher:

I've posted what I've got in #164. I'm trying to talk someone into snagging a free copy for myself, but so far no luck!

[166](#)

*Neal J. King* says:

June 13th, 2007 at 6:58 pm

#162: Douglas Hoyt:

As I pointed out in #157 (and actually in #119) there is a very clear difference between temperatures on windy and non-windy nights. What doesn't change between them is the trend of temperature increase over the years.

Look at #164 for the original work. It's really not very long to read.

[167](#)

*Neal J. King* says:

June 13th, 2007 at 7:04 pm

I'm going to repost #157, since my text got messed up:

////////////////////////////////////

#149: Jim Johnson:

- a) Parker distinguished between Tmin and Tmax in his analysis.
- b) I think you (and Douglas Hoyt) are missing a very important point: Parker DID see that windy nights show cooler than non-windy nights. You CAN blow away the UHI. This cooling shows up, buildings or no buildings, asphalt or no asphalt.
- c) What DIDN'T show up was any tendency for the non-windy nights to warm up more than the windy nights. Which suggests very strongly that the warming trend had nothing to do with UHI, but is a real behavior of the temperature over the country-side.

[168](#)

*Steve Milesworthy* says:

June 13th, 2007 at 7:09 pm

#163

What evidence do you have that they were all systematically paved in asphalt or concrete in the last 30 years?

Actually, somebody linked to a site of a reasonable number here or somewhere else recently, but I can't find the link at the moment.

#162

Standing by statements doesn't seem to cut the mustard here if you are sceptical of the CA orthodoxy. Is every rural station more urban than Barrow given that you wrote:

Barrow is a special case. I have been there. It is on the ocean and the surrounding land is very flat with no trees or large buildings to impede air flow. The roads are not paved. Only the airport, which is a ways away from the town, is paved. So there is no material with large heat capacity in town to store heat. Thus, in this case, the weak UHI can be blown away.

[169](#)

*Steve Milesworthy* says:

June 13th, 2007 at 7:14 pm

PS. I have access to the Parker paper at work, but I don't have anywhere to post it. If anyone can tell me how to do this I'll have a go tomorrow.

[170](#)

*Steve Sadlov* says:

June 13th, 2007 at 7:29 pm

The fatal flaw in Parker is that he assumes wind (especially at night) removed UHI. It does not.

[171](#)

*Steve Milesworthy* says:

June 13th, 2007 at 7:47 pm

#170

Up till now, all the stations in which the UHI has been detected (Fairbanks, Barrow etc.) have used as evidence that UHI is important.

Now, the fact that the UHI *can* be detected at these stations (due to differing temperature trends on windy and calm day) but **not** detected in the other 95% of stations is given as evidence that they are the *stations least affected by UHI!*

Steve S. and Douglas - I respect you for your commitment to your faith.

[172](#)

*Mark T.* says:

June 13th, 2007 at 7:59 pm

Neal, you're missing the point. None of your comments about "other methods" or "other evidence" are material to this thread. Not even whether or not GW is even happening. If the sites are providing unreliable data, than any conclusions from those sites is unreliable, pro or con GW. If the sites are now sitting in the middle of asphalt and we're comparing their data to times in which they were sitting in the middle of a field, there's an adjustment that

\_must\_ be made to account for this, or a valid scientific reason why not. This is just simple science. That nobody wants to take the task head on other than Steve M. (and some others more recently) is a complete travesty.

Mark

[173](#)

*Steve Sadlov* says:

June 13th, 2007 at 8:00 pm

Hey [self snipped word] - it has nothing to do with faith, you [self snipped word].

It's a legitimate scientific consideration. Boy, this topic of surface network quality has all you "Killer AGW" advocates in a real snit. Here's something more to stir the pot:

<http://climatesci.colorado.edu/2006/11/13/tv-interview-global-warming-week-issue-politicized/>

[174](#)

*steven mosher* says:

June 13th, 2007 at 8:01 pm

Kneel King

Sorry chap. You should go by N.J. King.. And bone up on a jersey accent.

That said, be a dear, and pass me the parker study. My spidey senses are tingling.

[175](#)

*Steve McIntyre* says:

June 13th, 2007 at 8:30 pm

#169. [esnips.com](http://esnips.com) permits you to upload material. It's free. You can link from there.

[176](#)

*Steve McIntyre* says:

June 13th, 2007 at 8:35 pm

Here's something to think about on UHI: as we parse the USHCN network with its small-town oddities, we see that the USHCN have to be vigilant for station moves as even the slightest change seems to be able to generate a half-degree bias. There is hardly a station in the entire network which doesn't have large adjustments. But we're told that the landscape changes in cities (collectively UHI) don't "matter".

[177](#)

*steven mosher* says:

June 13th, 2007 at 8:35 pm

NJ king

Thanks for the link to the little blurb. Was Tmin the only temp measure ?  
Open question. He mentioned 26 Northamerican/Siberian sites? listing?

Does he have a web site with the data and code open? can you give me a link?

Here is my deal. Some folks look at a guys funding and reject his work.  
I simply ask, show me the data, show me the code, If you cant show the data and show the code,  
then the busted credibility door is over there. Follow the shell oil shills out the door.

[178](#)

*Jim Johnson* says:

June 13th, 2007 at 9:43 pm

Neal,

“a) Parker distinguished between Tmin and Tmax in his analysis.”

Good for him.

Did Parker account for local anthropogenic temperature tainting effects (LATTEs) at scales smaller than ‘UHI’, and in places that are not ‘urban’? How?

Did Parker quantify the spatial component of wind, or just its instantaneous local velocity? How?

Did Parker compare non-UHI sites vs UHI sites in his analysis? What were those results?

Did Parker explain how hard the wind has to blow in order to move IR radiation out of the city? Did he reference Einstein when he did?

For that matter, what did Parker say? Exaclty? What, exactly, were his methods and results? From the ‘brief communications’ linked to (is that all he published of this study?) it is not possible to define his all of his methods explicitly. And what is pretty well defined does not inspire confidence.

“b) I think you (and Douglas Hoyt) are missing a very important point: Parker DID see that windy nights show cooler than non-windy nights.”

LOL. No he did not.

First, he did not look at ‘windy nights’ or ‘non-windy-nights’. Not at all in his main study, and very inadequately in his attempt to ‘reanalyze’ for winds and temps together (why wasnt that a criterion for the whole study, given that it is one of the base assumptions of his hypothesis? And what does it mean that the results were nearly identical between the two,

given that said base assumption was violated in the main study?).

Second, (and try not to be \*too\* disappointed) his 'windy' show warmer, not cooler, than his 'non-windy'.

And he did not show that this difference (windy nights being warmer than calm nights by 1C or 2C) represents the elimination of UHI or other anthropogenic warming.

“You CAN blow away the UHI.”

How would you know when you had? Evidently, you know that you have blown away the UHI when the nighttime temp doesnt drop quite so much 😊

“c) What DIDN'T show up was any tendency for the non-windy nights to warm up more than the windy nights.”

What DIDNT show up was a quantification of how much windier the 'windy' nights were than the 'non-windy' nights, and a demonstration that the 'windy' periods were sufficiently to remove UHI and other anthropogenic warming.

What did show up was an admission that there was no correspondence between the measured 'wind' period and the temp measurement. Even in his 'reanalysis' that attempted to do better than pairing a 'reanalysed' (what does that mean?) DAILY AVERAGE wind category with an instantaneous temp measurement at an unknown time is off by some amount up to six hours, distribution unknown. I'm not sure that that is even close enough to use the 'windy night' nomenclature loosely - when it is the daily average thing that term is just plain misrepresentation.

Where were his wind measurments taken? Co-located at the same X,Y,Z as the temps, or from some other place judged 'sufficiently close' by a person who is not looking at micro-scale issues?

Incidentally, Parker found +0.2C per decade trend. That is what, ~ twice what the 'global warming' is supposed to be based on the whole barbecue infested mess of stations? Indicates that there is something not representative about his stations. Why are they getting warm twice as fast as the rest of the planet? Must be all that wind warming them up?

“Which suggests very strongly that the warming trend had nothing to do with UHI.”

Nope. What this suggests is that his study was a creative attempt, but not up to snuff.

If you want to demonstrate that there is a non-anthropogenic trend, then get data from stations that have no anthropogenic influence and plot the trend. Sparing that, quantify the UHI for a particular station, subtract it out, and plot the trend. Rinse repeat for all stations. Just assuming that the wind blows all the UHI away? When there is a significant fraction (radiation) that really isnt blowable? When your ... uh... 'windy' ... uh ... 'nights' ... are

warmer than the obverse?

I'm not seeing it.

“Specifically, my understanding of isotope measurements of all kinds is that they are based upon calculations of chemical kinetics, from first principles of molecular dynamics: reaction-rate calculations. No need to check what some weather station was doing.”

I'll grant you that. How's your understanding of tree rings? How are those turned into temps?

[179](#)

[Steve McIntyre](#) says:

June 13th, 2007 at 10:25 pm

A question: why one would think that windy-nonwindy is a relevant test of UHI as opposed to direct measurements? Parker cites Johnston et al 1991 for the connection of windy-nonwindy to UHI. The Johnston et al articles are simulation studies in which *simulations* indicated that this effect should exist. I haven't located the Johnston articles yet, but surely another possibility is simply that the posited connection in Johnston et al 1991 doesn't exist and the simulations erred in some aspect.

[180](#)

[Neal J. King](#) says:

June 14th, 2007 at 1:46 am

steven mosher:

Currently, you have the same access to Parker's material that I have. Maybe Steve Milesworthy can use [esnips.com](#), as suggested by Steve McIntyre, to upload the article of 2006?

steve sadlov: Both the Parker and the Barrow studies show that wind does, indeed remove the UHI. Parker's own data show a consistent difference between the temperatures on windy days vs. non-windy days. The problem is, that this difference is not increasing over time, as would be expected if UHI were responsible for the upward trend in temperatures. Steve McIntyre: This is also a response to your question.

[This whole thread seems to be dominated by Steves!]

[181](#)

[Steve Milesworthy](#) says:

June 14th, 2007 at 4:27 am

I have access to the paper here at work, but I don't have access to esnips because of restrictions to file-sharing sites. I can email it to someone, alternatively I can email it home

and try to upload it tonight (7 hours from now).

#179

According to the abstract Johnson et al compared their simulations with observations.

[182](#)

*MarkW* says:

June 14th, 2007 at 4:55 am

#134,

Wind direction is important because temperature sensors are rarely in the exact center of the city.

No people turn on fans because air that is moving feels cooler than air that isn't. The moving air IS NOT actually cooler, it just feels that way.

[183](#)

*MarkW* says:

June 14th, 2007 at 4:57 am

#135,

The fact that Dallas has 10's of thousands of acres of concrete and asphalt is of no importance?

The fact that Dallas has several million people while Barrow only as a few ten's of thousands is of no importance?

[184](#)

*Earle Williams* says:

June 14th, 2007 at 7:17 am

The UHI effect was observed in Barrow, Alaska, a village of less than 5,000 people. See *Hinkel et al, 2003, THE URBAN HEAT ISLAND IN WINTER AT BARROW, ALASKA: Int. J. Climatol. 23: 1889-1905* which is available [here](#). The conclusions from this report on the observed UHI from a village of under 5,000 people are:

1. Based on spatial averages for the period 1 December 2001 to 31 March 2002, the urban area is 2.2  $^{\circ}\text{C}$  warmer than the rural area.
2. In winter, the daily UHIM (Delta Td,  $^{\circ}\text{C}$ ) increases with decreasing temperature, reaching a peak value of around 6  $^{\circ}\text{C}$  in January February. This likely reflects higher energy usage for residential and commercial space heating.
3. The daily UHIM decreases with increasing wind velocity. Under calm conditions (

I don't know what the population sizes were for Parker's work, but wind does not make heat go away, it mixes it. For a rural village of 5,000 the surrounding cooler air readily swamps the localized UHI. Any bona fide urban location will have a substantial suburban halo with its own UHI. Does Parker account for the UHI gradient one would see over more developed areas?

[185](#)

*Earle Williams* says:

June 14th, 2007 at 7:20 am

Oh crud, didn't notice the 'less than' symbol. Oh well, better to just read it for yourself! 😊

[186](#)

*Steve McIntyre* says:

June 14th, 2007 at 8:09 am

[Parker 2006](#)

[187](#)

*Douglas Hoyt* says:

June 14th, 2007 at 8:23 am

Suppose that windy days caused a cooling of dT now compared to calm days and it also caused a cooling of dT 30 years ago. The net effect would be to lower the temperature trend line of windy days compared to calm days with no change in the slope of the warming of the two trend lines. I don't see how using windy and calm days will tell you anything about the effect of UHIs on measured trends.

[188](#)

*steven mosher* says:

June 14th, 2007 at 9:23 am

After reading the Hinkle article ( I haven't read Parker) I'm generally happy with their methodology. They did not rely on the existing climate network. They installed 54 sensors/stations/dataloggers around Barrow with the intention of measuring the difference between sensors located "urban" and sensor located "rural".

They made continuous measurements and did a spatial analysis.

Now, as we have seen, sometimes, a sensor located in a area that people would consider "RURAL" actually has Urban characteristics, so I'm somewhat curious about parker.

From reading the Parker blurbs, I get the impression that he merely went to the databank ( 290 stations) assumed those stations were properly characterized ( urban/rural). Got some wind data.. and crunched numbers. My sense is he did not actually visit the 290 sites to verify them. My sense is he trusted the characterization

of those sites that others provided. This does not invalidate his study. It just raises audit issues. What did he use? what did he know about what he used?

We will see, but The Hinkle study is an actual experiment, as opposed to a data mining exercise.

[189](#)

*Steve Milesworthy* says:

June 14th, 2007 at 9:27 am

#187 Douglas

The hypothesis is that  $dT$  is dependent on the amount of urban influence ( $U$ ). The bigger  $U$  is, the bigger  $dT$  is.

If  $dT$  remains constant for 30 years it means  $U$  has remained constant, and the trend is valid.

If  $dT$  has gone up a lot in 30 years, it means that the station has been affected by increases in  $U$ , and the trend cannot be trusted.

[190](#)

*Steve Milesworthy* says:

June 14th, 2007 at 9:37 am

#188 Steven

Parker is a validation of the methodology which also does not characterise stations.

If his hypothesis is correct, stations whose warming is partially or entirely due to UHI will be *identified* by having an increase in  $dT$  (#189). 13 out of the 290 stations are characterised as such, and are listed in Table B1 on page 2892.

To put it very simply, it doesn't matter how bad the site is as long as it was equally bad 50 years ago. That's why it is complementary to other surveys that do try to address station characteristics.

[191](#)

*Douglas Hoyt* says:

June 14th, 2007 at 9:50 am

The hypothesis is that  $dT$  is dependent on the amount of urban influence ( $U$ ). The bigger  $U$  is, the bigger  $dT$  is.

This is a poor hypothesis. The wind will more likely carry away a constant amount of heat independent of the city size. In addition, the larger the city, the rougher the landscape is (more buildings blocking wind flow), so it even possible that winds will cause smaller  $dT$ 's as cities grow in size. Parker's study is based upon a lot of guesswork.

A valid approach to determining UHI effects is to do the study the same way the Barrow study was done, namely set up a network of temperature stations. Then one can see where the present USCHN stations are within the UHI and how much heating has occurred due to increased urbanization. The heating is unlikely to be small.

[192](#)

*jae* says:

June 14th, 2007 at 10:01 am

Good grief, how could anyone deny that the “UHI” effect is not a problem with temperature measurements. And there is a trend with time, so it wasn’t “equally bad 50 years ago.” Look at this [EPA study](#) of New York.

[193](#)

*Jim Johnson* says:

June 14th, 2007 at 10:05 am

Neal,

“Both the Parker and the Barrow studies show that wind does, indeed remove the UHI.”

No, Parker shows nothing wrt the wind removing UHI.

Barrow shows that a tiny town in the midst of a sea of pack ice and permafrost maintains a UHI greater in magnitude than the alleged ‘global warming’ effect - even into its third tercile of local wind speeds. The implications for a city a few orders of magnitude larger, surrounded by suburbs, that is not at the high arctic frontier? Not going your way.

“Parker’s own data show a consistent difference between the temperatures on windy days vs. non-windy days.”

Yeah. Warmer on windy days. So warmer on windy days (Parker) shows that wind removes UHI, and cooler on windy days (Barrow) shows that wind removes UHI. My, what stringent criteria for proof you have, grandma.

Steve M and Doug,

You two seem to be using DT for two different deltas: UHI temp increase (Steve) and UHI temp decrease due to wind (Doug).

[194](#)

*Steve Sadlov* says:

June 14th, 2007 at 10:11 am

RE: #187 - Parker talked himself into a major logical fallacy. And since his paper represents a convenient talking point for the “white blood cells” they now defend it almost as heartily as they defend the IPCC.

[195](#)

*steven mosher* says:

June 14th, 2007 at 10:24 am

RE 190.

Yes, I realize that. I'm looking at Parker now. Several issues.

1. how were the 290 sites selected? The spatial distribution seems somewhat skewed?

2. where is the list of the 290 sites so the study can be checked.

3. Why select 290 sites? seemed odd. Was pre analysis conducted? Essentially, when an analyst is merely looking a huge data set, it's a very simple matter to mine for an answer. And then present the subsample as somehow random.

4. Wind data. Wind data was not collected at the site ( contrast this with Hinkle) I've heard, any structure ( tree building) in the vicinity of the station changes the velocity of the wind in that area to a height of 20-30 times the height of the structure. Very simply. The wind sensor can see one wind profile, the temp sensor , if not collocated, sees another.

This reinforces the need to survey sites before merely crunching on a data stream. It also highlights the superior experimental design of Hinkle study.

Interesting thought would be to redo this analysis for the entire USCHN network.

[196](#)

*MarkW* says:

June 14th, 2007 at 11:15 am

Parker suffers from the same malady that so many others suffer from. He assumes that a station listed as rural will have no urbanization.

A very bad, and frequently disproven assumption.

[197](#)

*Boris* says:

June 14th, 2007 at 12:15 pm

“Even in the case of the burning can, you have to consider: Do you expect that they would have been doing more burning, or hotter burning, in order to generate an upward temperature trend?”

And the answer to that is likely ‘yes’. As more units are built, the incinerator gets more use.

A prime example of seeing what we want to see. Of course, the first reaction to seeing a

trash burning can next to a thermometer is “that’s pretty stupid.” Some people never leave this stage because they want the temp record to be wrong so bad.

If you’re interested, you might try, deliberately, to contaminate a temp record with an incinerator a few feet away. I’d wager you would be found out.

As an illustration, I remember seeing E.T. when I was young. In the film the protagonist places a thermometer against his light bulb so that it (the thermometer) will show a fever and he can stay home from school. Well, I thought I’d give it a try, so I did. And when my mother saw that I had a temp of 107 she promptly sent me off to school with promise of punishment to come.

Something to think about before throwing out data because of BBQs at a ground station.

[198](#)

*Boris* says:

June 14th, 2007 at 12:18 pm

My double blockquote failed. The first sentence under the quoted matter in 197 should also be quoted.

[199](#)

[Steve McIntyre](#) says:

June 14th, 2007 at 12:23 pm

If there is a “robust” signal (using the term “robust” as it is used in statistics not as it is used in Mann-speak, then the signal should be perceptible in USHCN stations that meet WMO standards for screens. That’s an objective ex ante standard that has nothing to do with ex post decisions made on the basis of the temperature results themselves. Marysville does not meet WMO standards; the barbecue is an easy way of showing this, but it’s not just the barbecue. How about you, Boris, do you like your data well-done, medium or rare?

[200](#)

*steven mosher* says:

June 14th, 2007 at 12:41 pm

Spidey senses about Parkers study get more tingly.

Parker, our super anti urban warming hero writes:

“Furthermore, Peterson (2003) found no statistically significant impact of urbanization in an analysis of 289 stations in 40 clusters in the contiguous United States, after the influences of elevation, latitude, time of observation, and instrumentation had been accounted for. One possible reason for this finding was that many

“urban” observations are likely to be made in cool parks, to conform to standards for siting of stations”

SLAPS FOREHEAD! duh, why didn't we see that the urban sites were really “rural” All the asphalt challenged sites are RURAL, and the urban sites, are in parks.

SLAPS FOREHEAD! Duh, why are we trying to deploy a new network ( CRN) when the old network is just fine. In fact, siting criteria was SO GOOD, that we have found Rural microclimates in urban centers! Parks! Just like the country! Zoo's are good too.

You know, I am dumbfounded at these brilliant idiots. LOOK AT THE SITES. OBSERVE.

Parker Hypothesis: urban observations are Likely to be made in cool parks. That's testable. Did he look at it?

Hmm,with GPS and google maps, is there anyone here you wants to take the bet that URBAN sites are Likely to be located in PARKS?

How many stations have we found in PARKS? lets see. Anthony heads out across California, nevada, etc and looks at every station in sight. did he find any in a park? has anyone anywhere found a climate station IN A PARK? Did parker look at station histories? Did he look at site descriptions? ANY LOCATED IN PARKS? I'm sure some are. That is not the claim. The claim is sites ae LIKELY to located in parks. IPCC likely?

Hmm. Lets say that urban stations are LIKELY to be located in a park. If you visited every site in Hadcru 40N 120W grid and found NONE in a park, what would you conclude about the global probablity of finding any site in a urban park?

That's an open question. Did parker look? or just pass off an excuse.

[201](#)

*Jim Johnson* says:

June 14th, 2007 at 1:31 pm

Boris,

“A prime example of seeing what we want to see.”

You are referring, of course, to your own post - correct? The one wherein you see spurious data bering accurately identified and thrown out, simply because that is what you want to see?

“Of course, the first reaction to seeing a trash burning can next to a thermometer is ‘that's pretty stupid.’”

Yes it is. And it is also against the siting standards. And the reason it is against the siting standards is that such things are a problem, and not necessarily a problem that can be winnowed from the data. And even if you can - you have still lost data.

“If you’re interested, you might try, deliberately, to contaminate a temp record with an incinerator a few feet away. I’d wager you would be found out.”

I’m sorry, but I do not accept your personal level of betting acumen to be a reliable scientific measure.

“As an illustration, I remember seeing E.T. when I was young.”

Priceless.

I remember seeing a film wherein the protagonist covered himself in mud to fool a sophisticated IR sensor. And he was not found out. So there! 😊

“Something to think about before throwing out data because of BBQs at a ground station.”

Something to think about is whether or not you can quantify the effect of the non-standard siting condition, accurately identify it in the record, and correct the spurious results (and only the spurious results) within the exacting tolerances necessary to ferret out a 0.1C per decade trend.

You probably can't, and the reality is that it would be too costly to do it for every permutation and combination of every foible, at every site, even if you could. And that is why there are siting standards. And that there are siting standards is of no use if there is no QC to ensure they are being followed, and that a friggin incinerator is found sitting five feet from a temp station is prima facie evidence that said QC does not exist, calling into question the validity of every related enterprise.

[202](#)

*Dave B* says:

June 14th, 2007 at 1:46 pm

boris said:

“If you’re interested, you might try, deliberately, to contaminate a temp record with an incinerator a few feet away. I’d wager you would be found out.”

I have an even simpler experiment for you to try yourself, “if you’re interested”.

get two thermometers. put one in the middle of a blacktop parking lot. put the other less than 100 yards away in the grass. read both thru the day and record the results. Switch thermometers the next day and repeat. let us know what you find.

you don’t seem to understand that barbecues, incinerators, and AC units are SYMPTOMS of

poor QC. it doesnt mean AGW doesn't exist. it just means the data isn't very good.

[203](#)

*Steve Milesworthy* says:

June 14th, 2007 at 2:04 pm

#191 Douglas

It's not a poor hypothesis. However, if you think that urban changes are as likely to reduce as to increase  $dT$ 's then presumably there is a way to reanalyse the data. For example, to see if there is more variation (up and down) in  $dT$  changes in "urban" settings than in "rural" settings.

A valid approach is to do both the Barrow and the Parker study. The Parker study is an attempt to understand the historical changes to stations through a novel method. More Barrow-like studies will improve the Parker methodology by showing the different effects of urban changes to  $dT$  that could refine the historical analysis.

#193 Jim

I agree with you that Parker is dependent on the wind theory being correct. He cites references that he thinks demonstrate it, but his study is not a demonstration of it. Both Doug and I are using the same measure of  $dT$  -  $dT$  is the UHI temperature decrease due to wind. Changes in the UHI effect in time would be  $d(dT)$  😊

#196 MarkW

Again all Parker requires is that his stations are a realistic sample of urban and rural. His technique will fail if all stations suffered from an equal increase in urbanisation over time.

[204](#)

*steven mosher* says:

June 14th, 2007 at 2:12 pm

Dave B.

That's funny. I thought the same thing. Visiting a pool at the park.  
Nice green grass. Then a slab o concrete. Then a pool.

I never once saw anyone run off of the hot grass onto the cool concrete.

[205](#)

*Jim Johnson* says:

June 14th, 2007 at 2:23 pm

Steve Mileslongnametotypetodifferentiateitfromalltheothersteves,



“Both Doug and I are using the same measure of  $dT$  -  $dT$  is the UHI temperature decrease

due to wind.”

So when you said:

“The hypothesis is that  $dT$  is dependent on the amount of urban influence ( $U$ ). The bigger  $U$  is, the bigger  $dT$  is.”

You meant:

\*The hypothesis is that the UHI temperature decrease due to wind is dependent on the amount of urban influence ( $U$ ). The bigger  $U$  is, the bigger the UHI temperature decrease due to wind is.\*

etc?

[206](#)

*steven mosher* says:

June 14th, 2007 at 2:24 pm

RE # 197.

Forget the incinerator. Forget that NOAAs CRN would fail a site collocated with an incinerator.

Here is a simple challenge Boris.

You pick 100 sites located by buildings, by asphalt, by heating sources.  
( all in violation of QC standards)

We will pick 100 sites located away from buildings asphalt and heating sources.

Now we bet the test: Your mean temp will be greater than our mean temp. Even odds. any amount.

Oh wait, we would actually have to find 100 sites located away from heating sources. DOH!

[207](#)

*Steve Milesworthy* says:

June 14th, 2007 at 2:52 pm

#205 Jim

Call me Steve M 😊

Yes, that is what I meant - I reread #189 to make doubly sure.

For example, the fact that Fairbanks is one of the 13 out of 290 that show up in the Parker

study is evidence that U, the urbanness of Fairbanks, has *increased* in the last few decades.

[208](#)

*MarkW* says:

June 14th, 2007 at 2:55 pm

I love the way Boris declares that the AGW crowd is incapable of error.

According to Boris, if there actually was a problem with the data, it would have been found. Therefore there is no problem with the data.

[209](#)

*MarkW* says:

June 14th, 2007 at 2:58 pm

Boris,

In the world that I inhabit, it's not at all unusual for the high temperatures to jump by 5 to 10 degrees from one day to the next, from purely natural causes. If once a week, that 5 degree jump was caused by burning trash instead of fewer clouds, just how are we supposed to figure that out after the fact?

Remember, until recently, all that was recorded was the daily high and low. Even with modern sensors that record more frequently, how do we tell the difference between the incinerator being fired up, and the sun coming out from behind a cloud?

[210](#)

*MarkW* says:

June 14th, 2007 at 3:02 pm

#203,

Since Parker made no effort to ensure that the stations that he labeled urban and rural, actually were urban and rural, then by your own definition, Parker is a useless study.

[211](#)

*jae* says:

June 14th, 2007 at 3:08 pm

200:

Parker Hypothesis: urban observations are Likely to be made in cool parks.  
That's testable. Did he look at it?

The EPA study I cited in #192 involves temperatures in Central Park in NY. Even THERE, where you have a "rural" setting in an urban area, there is a very prominent "UHI" effect, including an increasing dT.

[212](#)

*Earle Williams* says:

June 14th, 2007 at 3:17 pm

Re #207

Steve Milesworthy,

I disagree. The fact that Fairbanks has increased in both population and per capita energy consumption is evidence of its increase in urban-ness over the last few decades. One need not consult Parker's paper to verify that. However, the 277 other sites, do you posit that they have not increased in urban-ness over the same period of time?

[213](#)

*steven mosher* says:

June 14th, 2007 at 3:38 pm

211.

I think we agree. My point is Parker makes assumptions about URBAN SITES ( they are likely located in Parks) that are easily tested. But he didnt test it.

Fundamentally Parker has a pile of data. Some of that data is marked "rural" some marked "urban" he trusts these markings and crunches data.

First off, he is working with OPD. Other people's data. Second the OPD has been "validated"

So, one is inclined ( reclined actually) to accept it.

So, I did a spot check. an anecdotal random check of one US site that he got data from.

Olympa Washington:

46 58 -122 54

A climate station at an airport. Rural? or urban?

Now consider the wind profiles measured? Where were they measured? at the airport site? at what height? ground level, in the jet wash? or at a higher altitude?

Want to check another US site? Pick a number between 1 and 33. i'll look up the US site used in Parkers study

If its Urban, do you think its in a park?

[214](#)

*Steve Milesworthy* says:

June 14th, 2007 at 3:55 pm

#212 Earle

That was the point I was trying to make. The Parker hypothesis suggests that the 13 sites have significantly increased in urban-ness (ie. that there is a large change in dT) and the other 277 have not.

#210 MarkW

I don't think Parker labels any stations as either urban or rural (apart from one or two examples). That is the point.

#211 Jae

I don't understand this complaint. I thought the Peterson study did not find a UHI. Parker proposes a hypothesis that the Peterson study may have been flawed and you're complaining about it! Parker's "cool parks" hypothesis is an aside and has no bearing on the rest of the paper.

Is New York one of the sites used in Parker's study?

[215](#)

*jae* says:

June 14th, 2007 at 4:01 pm

214: Complaint?? No complaints; I was just trying to add something interesting to the discussion here.

[216](#)

*Steve Milesworthy* says:

June 14th, 2007 at 4:18 pm

#215 Sorry Jae, it was others who were doing the complaining.

[217](#)

*Jim Johnson* says:

June 14th, 2007 at 5:33 pm

OK, all of you Steve Ms and others ... I'm taking my further comment on Parker and related over to the Parker thread.

[218](#)

*Neal J. King* says:

June 14th, 2007 at 5:44 pm

Steve McIntyre,

Thanks for making available David E. Parker's "A Demonstration that Large-Scale Warming is Not Urban" (2006).

I would like to present a more formal rendition of Parker's argument. I think it might help to clarify some points in dispute, such as:

- to what extent windiness affects the degree of UHI
- local site issues
- the simultaneity of wind and temperature measurements

I define two indices:

- $j$ : The number of the weather-station site.  $j$  runs from 1 to  $N$ .
- $n$ : The number of the day. Since the dates of study run from 1950 to 2000,  $n$  runs from 1 to about 18,263.

Let  $A(j,n)$  be the minimum temperature measured at site  $j$  on day  $n$ .

Let  $B(j,n)$  be the maximum temperature measured at site  $j$  on day  $n$ .

It can be presumed that  $A(j,n)$  will be a night-time measurement, and can thus be affected by near-surface temperature inversions (when the ground cools by IR radiation, but the air above does not cool as rapidly).  $B(j,n)$  will be a day-time measurement, and will usually not be affected by a near-surface temperature inversion, except during very calm weather conditions in the winter, in which case there can be a *persistent* near-surface temperature inversion.

We assume that the measurements at site  $j$  will be affected by an urban-heat island effect  $UHI(j, n)$ . This means that

Eqn.(1):

$$A(j,n) = T_{\max}(j,n) + UHI(j,n)$$

where  $T_{\max}(j,n)$  is the maximum temperature you would have measured at site  $j$  and day  $n$ , **had all the urbanization, asphalt, burning cans, etc. not been present; and  $UHI(j,n)$  represents the impact of all that stuff.**

Likewise,

Eqn.(2):

$$B(j,n) = T_{\min}(j,n) + UHI(j,n) + NSTI(j,n)$$

where  $T_{\min}(j,n)$  is the minimum temperature you would have measured at site  $j$  and day  $n$ , without all the stuff;  $UHI(j,n)$  is the stuff again; and  $NSTI(j,n)$  is the near-surface temperature inversion. Since  $NSTI$  is cooling at the level appropriate to weather stations (near the ground), it is generally a positive quantity.

$A(j,n)$  and  $B(j,n)$  are simple time series. However, following Parker, I am going to do an experiment in data analysis. I am going to divide the days into two classes: windy days (set  $W$ ), and calm days (set  $C$ ). (These apply to one site only, of course.) Using the language of sets:  $\{1, 2, \dots, N\} = W \cup C$ ; or if you don't like my use of the Roman letter  $U$  for the set-

union symbol,  $\{1, 2, \dots, N\} = W + C$ .

On calm days, I expect both the UHI effect and the NSTI to be operating in full force: whatever heating is provided by the urbanization will be operating without being blown away; and whatever cooling is provided by the NSTI will also be protected by the stratification of the boundary layer.

However, on windy days, I expect the UHI effect to be vitiated by mixing of air from outside the region of the city with the relatively warmed air; and I expect the windiness to reduce the stratification of the boundary layer (to mix it up) and thus reduce the cooling effect of the NSTI.

Therefore, whereas on calm days, I still get the original equations, on windy days, the impact of the UHI and the NSTI are both reduced. For simplicity, let's assume that they are reduced by the same factor  $r$ . (This isn't true; but it will simplify the discussion; and you can go back later and verify that giving them each their own reduction factor won't change anything.). In that case, what we should really say is that;

for **calm days**:

Eqn.(3):

$$UHI(j,n) = UHI^*(j,n)$$

and

$$NSTI(j,n) = NSTI^*(j,n)$$

For **windy days**:

Eqn.(4):

$$UHI(j,n) = r UHI^*(j,n)$$

and

$$NSTI(j,n) = r NSTI^*(j,n)$$

where the  $*$ 'd functions are the slowly-varying seasonal values. In other words,  $UHI^*$  is the value of the urban-heat-island effect if wind were not reducing it by replacing warmer air with colder; and  $NSTI^*$  is the effect of the near-surface temperature inversion if the wind were not mixing up the air near the ground with the air a little higher up. Both these starred values should be more slowly varying, because the jitteriness of the windiness factor has been taken out. They should vary on more of a seasonal basis, rather than on a day-to-day timescale.

So now we have:

on calm days/nights (set C):

Eqn.(5):

$$A(j,n) = T_{max}(j,n) + UHI^*(j,n)$$

$$B(j,n) = T_{min}(j,n) + UHI^*(j,n) - NSTI^*(j,n)$$

on windy days/nights (set W):

Eqn.(6):

$$A(j,n) = T_{\max}(j,n) + r \text{UHI}^*(j,n)$$

$$B(j,n) = T_{\min}(j,n) + r \text{UHI}^*(j,n) \hat{\epsilon}'' r \text{NSTI}^*(j,n)$$

Each of these two sets of equations applies to different sets of days. In order to compare the behavior of these two sets of functions, I am going to define two sets of extended functions:

Eqn.(7):

$$A_c(j,n) = A(j,n) \text{ when } j \text{ is in set } C$$

$$A_c(j,n) = \text{the interpolation of } A(j,n) \text{ when } j \text{ is in set } W$$

$$B_c(j,n) = B(j,n) \text{ when } j \text{ is in set } C$$

$$B_c(j,n) = \text{the interpolation of } B(j,n) \text{ when } j \text{ is in set } W$$

$$A_w(j,n) = A(j,n) \text{ when } j \text{ is in set } W$$

$$A_w(j,n) = \text{the interpolation of } A(j,n) \text{ when } j \text{ is in set } C$$

$$B_w(j,n) = B(j,n) \text{ when } j \text{ is in set } W$$

$$B_w(j,n) = \text{the interpolation of } B(j,n) \text{ when } j \text{ is in set } C$$

In other  $A_w$  and  $B_w$  are the maximum and minimum temperatures on windy days, and are the interpolations of these values on calm days; and  $A_c$  and  $B_c$  are the maximum and minimum temperatures on calm days, and are the interpolations of these values on windy days. All 4 functions are defined on all  $N$  days.

**What we would then expect is that:**

Eqn.(8):

$$A_c(j,n) = T_{\max}(j,n) + \text{UHI}^*(j,n)$$

$$B_c(j,n) = T_{\min}(j,n) + \text{UHI}^*(j,n) \hat{\epsilon}'' \text{NSTI}^*(j,n)$$

$$A_w(j,n) = T_{\max}(j,n) + r \text{UHI}^*(j,n)$$

$$B_w(j,n) = T_{\min}(j,n) + r \text{UHI}^*(j,n) \hat{\epsilon}'' r \text{NSTI}^*(j,n)$$

In terms of Parker's graphs, the  $\hat{\epsilon}''c$  functions correspond to what Parker called "calm day" plots, and the  $\hat{\epsilon}''w$  functions correspond to what Parker called "windy day" plots: the dotted and dashed curves, respectively.

The point is this: If you take the trend of the  $A$  curves from 1950 to 2000, you should get:

Eqn.(9):

$$\text{Trend} [A_c(j,n)] = \text{Trend} [T_{\max}(j,n)] + \text{Trend} [\text{UHI}^*(j,n)]$$

$$\text{Trend} [A_w(j,n)] = \text{Trend} [T_{\max}(j,n)] + r \text{Trend} [\text{UHI}^*(j,n)]$$

and the trend of the  $B$  curves should be:

Eqn.(10):

$$\text{Trend} [B_c(j,n)] = \text{Trend} [T_{\max}(j,n)] + \text{Trend} [\text{UHI}^*(j,n)] \hat{\epsilon}'' \text{Trend} [\text{NSTI}^*(j,n)]$$

$$\text{Trend} [B_w(j,n)] = \text{Trend} [T_{\max}(j,n)] + r \text{Trend} [\text{UHI}^*(j,n)] \hat{\epsilon}'' r \text{Trend} [\text{NSTI}^*(j,n)]$$

Note, however, that there should not be any trend in NSTI\*: It should not be affected by climate change. Therefore,

Eqn.(11):

$$\text{Trend [NSTI*(j,n)]} = 0$$

and

**Eqn.(12):**

$$\text{Trend [Ac(j,n)]} - \text{Trend [Aw(j,n)]} = (1-r) \text{Trend [UHI*(j,n)]}$$

$$\text{Trend [Bc(j,n)]} - \text{Trend [Bw(j,n)]} = (1-r) \text{Trend [UHI*(j,n)]}$$

What does all this add up to?

- a) Under this analysis, we see that the difference in calm-day trends and windy-day trends reflects directly whatever is happening with the UHI trend. If there is no difference in these two trends, either  $(1-r) = 0$ , or else there is no UHI\* trend. (Eqn(12)).
- b) But if  $(1-r) = 0$ ,  $Ac = Aw$  and  $Bc = Bw$  (Eqn.(8)). That mean that the calm-day interpolated temperatures equalled the windy-day interpolated temperatures. And that is just not true: All of Parker's graphs show a gap between them. (This is what I meant by saying that "the wind blows the UHI away":  $r$  is less than 1.)
- c) a) and b) together imply that there is no trend in the UHI\*, if there is no difference in the windy-day and calm-day trends.

Since, in the majority of cases, Parker's analysis showed there was no difference in the calm-day and windy-day trends, there can be no significant UHI\* trend in these cases.

As noted in his papers, there were a few cases when he did detect a UHI\* trend.

As mentioned previously, the UHI (and thus the UHI\*) incorporates all the site-specific stuff: hot buildings, asphalt, incinerators, etc.

I have divided the days up into windy and calm. You could also subdivide them into 3 or more sets (as Parker did), as long as you have enough members in each set to do sensible interpolation, and thus get reasonable curves.

Appendix A details Parker's efforts to make sure that the wind measurements and the temperature measurements were as properly tied together as possible, taking into account different time-stamping procedures in different countries. Given that it doesn't seem to be possible to get absolutely simultaneous measurements over such an extended period in the past (1950-2000), what will this inaccuracy do? It will cause a degree of misallocation: Some days that were windy will be called calm, and vice versa, so the  $Ac$  &  $Aw$  and the  $Bc$  &  $Bw$  curves will be brought closer together, reducing the estimate of  $(1-r)$  UHI\*. So Parker's gap should represent a lower estimate for the amount of  $(1-r)$  UHI\*. It's reasonable to expect that the estimate is not too bad: there has to be some reasonable degree of auto-correlation in time for wind measurements, and the Parker gap hasn't disappeared.

Is there a way to get an upper estimate of the UHI\* ?

[219](#)

*Anthony Watts* says:

June 14th, 2007 at 9:39 pm

RE197, If you're interested, you might try, deliberately, to contaminate a temp record with an incenerator a few feet away. I'd wager you would be found out.

Oh goody, another experiment to try.

[220](#)

*Earle Williams* says:

June 15th, 2007 at 6:57 am

Re #214

Steve Milesworthy,

That was the point I was trying to make. The Parker hypothesis suggests that the 13 sites have significantly increased in urban-ness (ie. that there is a large change in dT) and the other 277 have not.

It seems to me that some very basic tools are available to falsify the conclusion that those 277 sites have not urbanized over 5 decades. The data are right in front of our noses and will either explicitly endorse Parker's hypothesis and conclusions or they will leave Parker's paper and your arguments in shambles.

You up for trying to falsify the hypothesis?

U.S. Census data is a start. Also energy consumption over time. I expect you will find that there is no correlation between Parker's conclusions on urbanization and actual urbanization for all 290 sites. Let me correct that. My prediction without having even read the Parker paper is that he was able to detect urbanization in frontier locales, ie Barrow Alaska, but that no conclusions can be made whatsoever to the degree of urbanization in large metropolitan areas.

[221](#)

*Steve Milesworthy* says:

June 15th, 2007 at 8:36 am

#220 Earle

You up for trying to falsify the hypothesis?

Yes of course!

If urbanisation is found where Parker doesn't detect it, it means either that the urbanisation change is unimportant (which backs up the temperature record), or that the windy/calm

hypothesis is wrong.

That's why more transects would be of interest to test the latter possibility. The windy/calm hypothesis has been demonstrated in some places, but maybe these places were exceptional for some reason.

[222](#)

*Steve McIntyre* says:

June 15th, 2007 at 9:39 am

#221. Please consider the discussion in the PArker thread.

[223](#)

*Boris* says:

June 15th, 2007 at 2:10 pm

Jim hurt my feelings and missed my point. The "gotcha moment" is, upon further inspection, neither "gotcha" nor, probably, a moment. But gosh the rheotrical power!

219: I await your publication. Should be a good read. Also, please don't burn your trash on a wooden palate.

[224](#)

*MarkW* says:

June 15th, 2007 at 2:18 pm

that's spelled pallet

[225](#)

*Boris* says:

June 15th, 2007 at 2:48 pm

224: You forgot my sentence fragments, my questionable use of nonrestrictive elements, and my inconsistency in using quotation marks around "gotcha" but not "moment."

[226](#)

*MarkW* says:

June 15th, 2007 at 4:24 pm

you aren't worth a full analysis

[227](#)

*Neal J. King* says:

June 15th, 2007 at 5:36 pm

#220: Earle Williams:

Where there is no calm-windy trend, Parker is not claiming a lack of urbanization, but a lack of UHI.

It is conceivable that there could be increasing urbanization w/o increasing UHI. This brings us back into the regime of urban cool spots & parks.

So if you want to falsify Parker's hypothesis, it seems to me that you have to:

- a) Measure directly the UHI, by comparing neighboring (but differently urbanized) points;
- b) Establish a trend in the UHI over time; and
- c) Demonstrate that this trend DOES NOT show up in the calm-windy temperature measurement signal.

This would be very welcome. However, I point out that it will take:

- Space: to do a direct UHI measurement; and
- Time: to see how the trends develop.

You cannot do this by simply looking at proxies such as census figures and electricity bills, or you will still be left with the question of how good the proxy is. Parker's methodology is aimed squarely at UHI and UHI trends, not at "urbanization" as such.

[228](#)

*Neal J. King* says:

June 15th, 2007 at 5:38 pm

#227: Correction:

"Where there is no calm-windy trend, Parker is not claiming a lack of urbanization, but a lack of UHI."

=>

"Where there is no calm-windy trend, Parker is not claiming a lack of urbanization, but a lack of UHI **trend**."

[229](#)

*David Smith* says:

June 30th, 2007 at 7:08 am

In the old days of burning trash in barrels, people would elevate the bottom with 2x4s or small limbs or bricks (if available), in order to keep the bottom of the barrel dry. Otherwise moisture is trapped beneath the barrel and the bottom rusts out. This is a big problem. The barrels also often had small holes poked in the bottom for internal drainage.

Temperature is not a concern because of wood's high ignition point, the lack of flame

impingement and the accumulation of ash in the bottom.

(Yes, when I was a young kid we had a trash-burning barrel and I was a fire bug.)

[230](#)

*John F. Pittman* says:

June 30th, 2007 at 3:40 pm

#227 you said

You cannot do this by simply looking at proxies such as census figures and electricity bills, or you will still be left with the question of how good the proxy is. Parker's methodology is aimed squarely at UHI and UHI trends, not at "urbanization" as such.

But this is the nature of proxies as not only the AGW authors have done but as Parker has done. He did not use actual wind data but used proxy data. I agree it is suspect, but to take Earle Williams to task is to take Parker to task. That Parker explained his assumption. proxies has nothing to do with whether or not he used a proxy or made a bad assumption. He simply looked at, used, assumed a proxy, why can't Williams? By any sane reasoning someone who asked why can't Parker discern a UHI when it has been documented as well or better than GW? After all continued UHI should be assumed more correct than Parker's proxy. paper, or assumptions. Translation is either his proxy or assumptions are incorrect, or perhaps all these proxies have a fundamental problem when extrapolated beyond verifiable constraints..such as Briffa, Parker, MBH98, IPCC TAR, etc.

[231](#)

*Weber* says:

November 12th, 2007 at 5:16 am

Some weather stations like this one

<ftp://tgftp.nws.noaa.gov/data/observations/metar/decoded/> from 10. November 2007 doesn't allow FTP connection by using FTP but only HTTP. Does anybody know for the stations that allow using FTP end that are free?

[232](#)

*JerryB* says:

November 12th, 2007 at 6:38 am

Weber,

That url does accept ftp connections; I just tried it and it connected fine.

BTW, that is not a station; it is a collection site for data of many stations.

Leave a Reply

Name (required)

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