Antworten von Pat Frank auf Tamino http://tamino.wordpress.com/2011/06/02/frankly-not/#comment-51439

Pat Frank | June 6, 2011 at 5:06 am | Reply

Tamino, in your criticism of my essay on the global anomaly trend at WUWT (originally at tAV), you noted that, "*It's easy to fit a waveform to data. It's not so easy to establish that the data are actually cyclic. … But Frank doesn't even attempt to do so,…*"

But I did do so. The first sentence of my essay said, "*In my recent "New Science of Climate Change" post here on Jeff's tAV, the cosine fits to differences among the various GISS surface air temperature anomaly data sets were intriguing*."

This was a reference to Figure 3 in the prior tAV essay, which showed the appearance of a difference sinusoid when the GISS 1999 anomalies were subtracted from the GISS 2010 anomalies. This oscillation was traced to a physical source.

I showed that this difference sinusoid arose from the inclusion, after 1999, of SSTs into the GISS global temperature anomaly data: J. Hansen, R. Ruedy, J. Glascoe, and M. Sato (1999) "GISS analysis of surface temperature change" JGR 104(D24), 30997-31022 (HRGS99). Up to 1999, the GISS global air temperature anomaly trend included only land station data.

HRGS99 also showed CRU (Land+SST) minus GISS (Land only) difference anomalies in Appendix Plate A1(b). Their difference anomalies exhibited an oscillation similar to my tAV Figure 3, but they didn't

remark on it. The oscillation shown in the HRGS99 Appendix is due to the presence of SST data in the CRU anomalies and its absence in the GISS land anomalies. My Figure 3 just extended the HRGS99 result to their own GISS data sets.

Since the oscillation appeared only after SSTs were added to the GISS global Land anomaly data set, a direct inference follows that the difference oscillation came from the SST data and reflects a global net cycle in ocean temperature that is either not present, or less present, in the land-only anomalies.

A cosine fit to the GISS 2010 minus GISS 1999 anomaly difference oscillation showed a crest-to-crest period of about 60 years.

The appearance of a difference sinusoidal period in the anomalies after inclusion of the SSTs justified looking for the parent oscillation in the global Land+SST anomaly trend. In the event, the cosine parts of the full fits to the entire GISS and CRU data sets showed about the same period as the cosine fit to the (SST+Land) minus (Land-only) difference anomalies.

So, I did show that the global air temperature anomaly trend included an oscillatory component, and I did show that it stemmed from global SST.

[Response: You're fooling yourself, but you're not fooling us. All you've done is substitute and unfounded claim of cyclic behavior in a "difference oscillation" for the unfounded claim of cyclic behavior in global temperature. You've gone from one claim of "It's cyclic" with no proof, to the claim of "It

includes other data which are cyclic" -- again with no proof.

And there can't be, because there aren't enough "cycles" to show cyclic behavior. In case you don't know (and apparently you don't), "cyclic" doesn't mean "it went up-and-down, then up-and-down again." Cyclic means that it has done so often enough, and with a similar enough pattern, that we can reliably predict it will do so again.]

You wrote, "*Such effort would be futile, because of the simple fact is that there aren't nearly enough "cycles" to show that global temperature is following a cyclic pattern. Even if there were, establishing cyclic behavior is very tricky.*"

If you look at Figure 3, or HRGS99 Figure A1(b), a little more than one full period of oscillation is in evidence; enough to show its presence through the 130 years prior to 2010.

[**Response: It seems you're determined to embarrass yourself, A little more than ONE full period is in evidence? You're becoming a self-parody.**

It's also revealing that your net "evidence" consists of "If you look at ..." No, you can't just "look at" a graph and draw conclusions about cyclic behavior -- especially when there aren't enough "cycles" to show such behavior.]

Also, it's not that, "*global temperature that is following a cyclic pattern*," but rather it's that the SSTs have apparently put a net oscillation into the global air temperature anomalies over the last 130

years or so.

[**Response: Have you lost your mind? Your whole "essay" is about your model of global temperature as a linear trend plus a cycle -- that's what a cosine (or sine) is! Denying your own model, doesn't make it look good.**

But now you don't want to call a cosine a "cyclic pattern", you want to refer to "net oscillation". That's just word games. And whether it's in global temperature, or SST, or both, you have still failed to provide any EVIDENCE other than "Looking at the graph," and there are still nowhere near enough "cycles" to show cyclicity.]

I wrote nothing to imply anything about the fits extending to times beyond that bound, though there is presently no reason to think something like a net global SST oscillation was (or will be) not present in global average temperatures from earlier (or later) times.

[**Response: SO -- you admit that your whole essay is nothing but an exercise in curve-fitting?**]

Concerning the fit residuals, you wrote "*if you fit a linear+sinusoid model to some data, then the residuals will have zero linear slope. Necessarily. Whether the model is any good or not.*"

That's not entirely true. A *fit* through the unfit residual of a linear fit will have zero linear slope. The unfit residual itself can have all sorts of positive and negative excursions away from zero, including trends and oscillations. The excursions need not have a zero *linear* slope, but they

will all average out to a zero net slope.

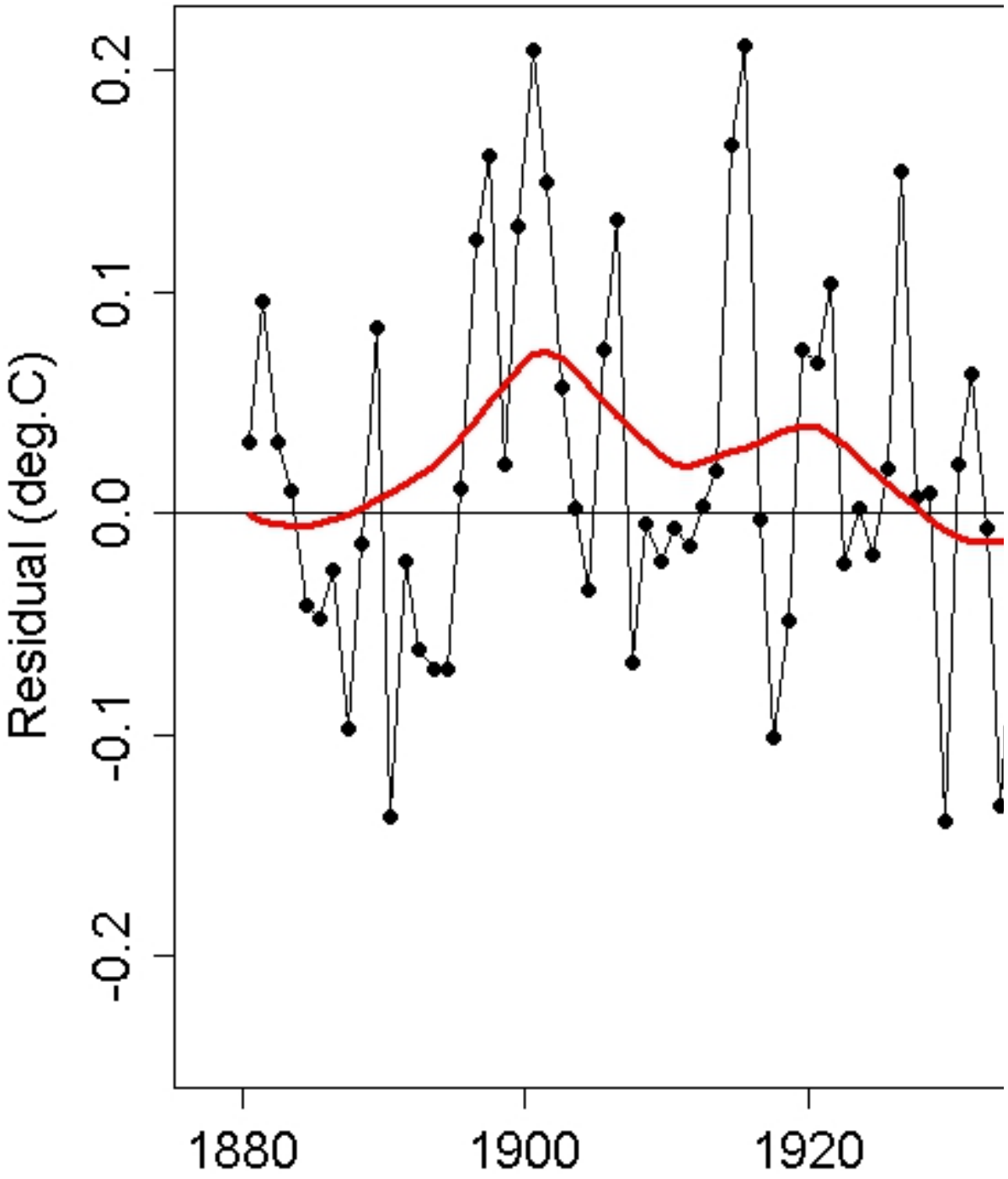
[Response: Mr. Frank, you are really embarrassing yourself here. You should just admit you're wrong because you don't know the proper use of the phrase "linear trend." Instead, you seem to feel entitled to claim that the absence of a "zero linear slope" is compatible with a "zero net slope."

Here's the truth, for those who are interested: you really (really!) don't know what you're talking about. You're just making up wordy phrases to justify your rather obvious incompetence.]

In my fits, the unfit residuals themselves trend along the zero line. There are no important excursions away from zero left in the unfit residuals. That's why I showed the residuals, and what the fits to the residuals were meant to show.

[Response: Malarkey. It just "looks" that way to you for three reasons: 1) you plotted it on such a small scale that it hides the visual changes, 2) you did no statistics (anywhere), and 3) you desperately want it to be so.

I plotted *your* residuals in better fashion:


Residual (deg.C)
0.2
0.1
0.0
-0.1
-0.2
1880
1900
1920

There are indeed "important excursions away from zero." Like, the last decade.]

You wrote, "*The "model" is that global temperature is following (and will follow!) a linear trend plus a sinusoid*"

Your parenthetical exclamation goes too far. I made no claim that the fits predict the trend in future anomalies. I claimed only that one could find an oscillation in the anomaly trend, over 1880-2010, of a period consistent with the oscillation that appeared when SSTs were added to the land station data.

[**Response: SO -- once again you admit that your model is just an exercise in curve-fitting. And you continue to misuse the term "oscillation," apparently thinking that down-and-up followed by another down-and-up justifies its use.**

I can indulge in curve-fitting too. And when I did, my model came out a helluva lot better than yours.]

So you began your analysis with an incorrect surmise.

You then fitted a different data range, produced different fit parameters, and then used them to criticize my fit. I duplicated your fit. The cosine fitted over your chosen range had a period 3 years shorter, and an intensity 15% lower, than mine showed over the full 1880-2010 range. Why is it a revelation that you achieved a poorer result?

[**Response: Why is it a revelation to you, that estimating parameters from most (but not all) of the data, then using**

that to predict the remainder, is a valid test?]

You began with a false premise (predictive model) and proceeded to a false test (improper bounds). The steep unfit residual at 2000-2010 in your second Figure only reveals the poor quality of your own extrapolation. It's no surprise that you came to a false conclusion.

[**Response: Your model utterly fails, both statistical comparison to the 4-line model, and fitting the most recent decade, even when it's constructed using the entire time span.**]

The bounds you chose [1880-1999 (end)] are not even appropriate to your premise. A proper test of my fit, granting your presumption of a predictive claim, would have required waiting for 10 years or so to see how the emerging anomalies trended. So your test was a scientific non-sequitur, apart from being an analytical malaprop.

[**Response: The test I did was pretty much standard faire in what we call "science." The bounds I chose were very favorable to you -- I only required a brief "prediction" and used the vast majority of data for training the model. But you insist a predictive test would have required waiting for 10 years. And you call MY test a "non-sequitur"?**]

By the way, the red line smooth in your Figure 2 evinces end-point padding, which you didn't mention using. It helped promote that steep final gradient, though, didn't it. End-point padding is evident in the smooths in your Figures 3, 5 and 7, as well. I consider end-point padding

tendentious and never use it.

[Response: Sigh … you REALLY don't know what you're talking about.

There is no end-point padding. The smooth curves are a modified lowess smooth, which doesn't use any endpoint padding.

I too dislike endpoint padding, and I never use it. But it's a recognized (and justified) procedure (I just think there are much better ways). You're free to avoid the practice, but clearly you are not qualified to pass judgement on it.]

I've already pointed out that the oscillations appeared in the anomalies after addition of the SSTs. In my essay I also pointed out, although you neglected to mention it, that the PDO and AMO include a period near 60 years, and provided a link to an article by Joe D'Aleo showing this (his Figure 11).

[Response: You've made it clear that you don't really know what periodic behavior is. The AMO and PDO do *not* show periodicity. Again, there aren't nearly enough "cycles" to show such. But you don't seem to get this simple fact.

As for linking to Joe D'Aleo, that doesn't help your credibility.]

Those observations together — the SST source data, the AMO and PDO periods — provided a physical meaning to the cosine part of the fit to the

130 year anomaly trend. Therefore your other tests, using arbitrarily chosen models (your Figures 3&4 and 7&8) as points of criticism, are irrelevant.

Roger Pielke Sr. has a recent guest post about work by Marcia Wyatt, Sergey Kravtsov, and Anastasios Tsonis, discussing their recent analysis of ocean thermal periods, which range around ~64 year cycles. These again allow inferring physical meaning to the cosine parts of the fits.

You wrote, "*We can also compare the models using the Akaike Information Criterion, or AIC … [and] … the 4-line model wins.*"

It shouldn't have to be mentioned that statistical criteria (AIC) applied to irrelevant models do not yield any insights into the validity of a physically justified analysis.

[**Response: What????????? You have lost your mind.**]

At the end, your conclusion that "*Frank's model has no physical basis.*" is wrong.

This comment, "*It ignores the known physics of climate including greenhouse gases, sulfate aerosols (both man-made and natural), solar variations.*" is an irrelevant diversion. (My analysis is an observation-based test of the claim of unusual late 20th century warming.)

And this one, "*It fails the simplest test of predictive skill,…*" merely turns upon your false premise.

Finally, this: "*His use of results to estimate climate sensitivity is, not to*

put too fine a point on it, laughable.," starting wrong and proceeding through the irrelevant, is unsurprisingly facile.

To reiterate my original point, the cosine fit was grounded in SSTs and closely corresponded to known ocean thermal cycles. Following removal of the oscillation, the remaining 130 year anomaly trend was linear to within its own noise. The rest of the analysis followed automatically, taking at face value the IPCC view of the 130 year global average surface air temperature anomaly trend.

[**Response: To reiterate the truth: the cosine model does NOT have a physical basis. There is NOT established periodic behavior in AMO or PDO. To imply that they are responsible for global temperature change is idiotic.**]

Pat Frank | June 6, 2011 at 5:34 am | Reply

"*There's a shitload of bizarreness in Frank's post: for example, after substracting the "cycles" he computes the 1880 – 1940 warming rate and then computes 1960 – 2010 rate: he then asumes that the effect of GHGs is the difference between them. Why? Because he asserts that the 1880 – 1940 rate is the rate corresponding to "LIA recovery" and GHGs effect "kicked in" around 1975!*"

Not correct, Kartoffel. If you look at SPM Figure 4 of the AR4, for example, the natural + GHG-driven model ensemble output begins to noticeably deviate from that driven by natural forcings alone only after about 1950-1960. That implies the 1880-1940 temperature rise reflects primarily natural forcings, with GHG contributions slowed by the thermal inertia of the climate system.

[**Response: First you said "climate of the first 40-50 years of the 20th century was unaffected by human-produced GHGs." Now you're backing off to "primarily natural forcings."**

By the way, the inertia of the climate system impacts all forcings, not just greenhouse gases.]

This leaves only the effect of natural forcings evident in the early 20th century global temperature anomalies. Since a rate of temperature rise by natural forcings is revealed by the early 20th century anomalies, a parsimonious interpretation of the late 20th century anomaly data is that it consists of natural plus GHG-enhanced warming. There is no obvious reason to suppose that the natural underlying rate of warming after 1950 should be different from the natural underlying rate of warming before 1950.

In an empirical analysis, one is required to be hypothetically conservative, which means using the known recent natural warming rate as the baseline beneath a recent artificially enhanced rate.

The rest of the analysis follows directly.

An empirical analysis of this sort is a test of theory. Making changes to the empirical warming rates to conform with theory injects the expected into the known. That makes the analysis tendentious.

[**Response: Do you not see the fault of your argument? You're trying to minimize the impact of early-20th-century greenhouse gases using IPCC model results. But those model results *demonstrate* exactly the consensus values of climate**

sensitivity that you're attempting to dispute!

I guess you'll accept IPCC models as evidence -- unless you don't want to.]

- **Pat Frank** | June 12, 2011 at 11:54 pm | Reply

  Rick, if you actually read my posts you'll have seen that the ~60 year sinusoid showed up in the anomaly trend when the marine temperatures were added to the land-only temperatures. That implies a physical cause; your objection has no basis.

  [**Response: No, it doesn't; the suggestion is ludicrous.**]

  Atmospheric CO2, by the way, traps energy not heat. How the climate works determines whether that energy appears as sensible heat. Your analysis is careless.

  [**Response: Keep digging that hole.**]

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- **Pat Frank** | June 12, 2011 at 11:57 pm | Reply

  This response is rather long, and so I've posted it in three parts.

  Tamino: I wrote, "So, I did show that the global air temperature anomaly trend included an oscillatory component, and I did show that it stemmed from global SST."

  You wrote in response: "*You're fooling yourself, but you're not fooling us. All you've done is substitute and unfounded claim of cyclic behavior in a "difference oscillation" for the unfounded*

claim of cyclic behavior in global temperature…“

Unfounded claim? Readers are invited to look at Figure 3 here. The difference oscillation shows nearly two full periods. It appears in the GISS difference data only after the ocean temperatures were added in. That’s a demonstration, which is rather different from an unfounded claim. Who should we believe, Tamino: you, or our lying eyes?

[**Response: Like most mathturbators, the sum total of your "evidence" consists of "look at."**]

You then wrote, “*You’ve gone from one claim of “It’s cyclic” with no proof, to the claim of “It includes other data which are cyclic” — again with no proof.*”

The sentence “*It includes other data which are cyclic*” appears nowhere in my essay. Why did you put it in quotes as though I wrote it?

The logic is very straightforward. An oscillation appears in the global air temperature difference anomalies only after the marine temperatures are added to the land air temperatures. That must mean an oscillation entered with the marine data. One is then led to look for the oscillation in the full data set. There’s nothing mysterious here.

You wrote: “*And there can’t be, because there aren’t enough “cycles” to show cyclic behavior.*”

Curious: there are two cycles showing cyclic behavior.

You wrote: “*In case you don’t know … Cyclic means that it has done so often enough, and with a similar enough pattern, that we*

can reliably predict it will do so again."

Not correct. A statement about cycles in some data implies nothing about future events. It implies something about the data. You are making an unwarranted inferential extension. Stating the data set 'shows cyclic behavior' means that cycles are observed in the data under examination. It says nothing about whether those particular cycles are extensive.

One can hypothesize that the cyclic behavior will continue. Whether cycles are observed in future data tests a hypothesis *about the future*. But none of that impacts whether cycles are observed in the data we have now. And when we look, there it is in Figure 3.

You wrote: "*It seems you're determined to embarrass yourself, A little more than ONE full period is in evidence? You're becoming a self-parody*."

Admitted, I misstated the case. There are nearly 2 full periods in evidence.

You wrote: "*It's also revealing that your net "evidence" consists of "If you look at …""*"

Figure 3 shows a cosine fit. The evidence is that a cosine fit goes right through the middle of oscillatory difference anomalies. Your criticism is a careless misrepresentation.

You wrote: "*No, you can't just "look at" a graph and draw conclusions about cyclic behavior*"

Isn't that what you're doing with your dismissal by visual judgment?

"*– especially when there aren't enough "cycles" to show such behavior*."

Almost two full cycles plus a good fit with a cosine function are enough to show that an oscillation is present.

In response to your original criticism, I wrote that, "Also, it's not that, "*global temperature that is following a cyclic pattern*," but rather it's that the SSTs have apparently put a net oscillation into the global air temperature anomalies over the last 130 years or so."

Your reply: "*Have you lost your mind? Your whole "essay" is about your model of global temperature as a linear trend plus a cycle — that's what a cosine (or sine) is! Denying your own model, doesn't make it look good*."

You have now shifted the ground of your argument. You initially wrote about "global temperature following a cyclic pattern." Those weren't my words, and that idea does not appear in my analysis. My analysis found a ~60-year cosine-like oscillation *within* the 130-year global air temperature anomaly trend. There's nothing in that about the global trend itself following a cyclic pattern. Your original description was incorrect, and your current description of the model as "*a linear trend plus a cycle*" itself contradicts your original "*cyclic pattern*" description.

You wrote: "*But now you don't want to call a cosine a "cyclic pattern", you want to refer to "net oscillation". That's just word games*."

No. "Net oscillation" refers to the fact that the oscillation initially showed up in a difference anomaly data set. I.e,. it's what emerges

after the two data sets have been differenced. This isn't as hard as you're making it.

You wrote: "*And whether it's in global temperature, or SST, or both, you have still failed to provide any EVIDENCE other than "Looking at the graph,""*

Well, except for the nearly two periods that show up in the difference anomalies, except for the cosine fit to the difference anomalies, and except for the two cosine + linear fits to two methodologically independent anomaly data sets. And, except for the fact that those two independently fitted cosines may as well be of the same frequency and phase. Except for all that, no evidence.

"*and there are still nowhere near enough "cycles" to show cyclicity*."

Nearly two periods in evidence plus a good cosine fit are enough to show cyclicity, but only if one examines the data itself without an artificial, unwarranted, and imposed condition that cyclicity observed now is not present now unless one has miraculous pre-knowledge of future nows.

After I explained to you that, "I wrote nothing to imply anything about the fits extending to times beyond that [130-year] bound…"

You responded: "*SO — you admit that your whole essay is nothing but an exercise in curve-fitting*"

You've made yet another untoward inference. My analysis was an exercise in physical phenomenology. That's when one takes real physical data (temperature anomalies) and, within a physical context (global thermal behavior), uses physical reasoning to

examine the data in terms of mathematical functions that reflect common natural phenomena (cycles).

As I noted above, one could extend the model as an empirical conjecture and suggest that *if* the thermal oscillation found in the global air temperature anomalies reflected a global net effect of on-going energy flux in the oceans, *then* one should observe it in the future trend of air temperature anomalies. At that point, one sits back and waits for the data to come in. Conjecture, test, refutation/verification — the method of science.

You wrote, "*you seem to feel entitled to claim that the absence of a "zero linear slope" is compatible with a "zero net slope*."

You continue to infer what is not in evidence. You claimed that the residuals of any linear fit "*will have zero linear slope. Necessarily*." I replied that is not entirely true, pointing out that the residual of a linear fit can have arbitrarily large excursions from linearity, but will always average out to zero trend. The zero average produces a fit with zero slope, even though the fit residual may excurse up, down, and all around.

Your explanation was sloppy and misleading; zero net slope is descriptively much closer than "zero linear slope" to the generalization of the sort of residual one can get from a linear fit.

Further: the context of my comment was the linearity of the Figure 1 fit residuals themselves. I.e., Figure 1 Legend: "The colored lines along the zero axis are linear fits to the respective residual. These show the unfit residuals have no net trend."

The zero slope of the residuals, not of the fits to the residuals,

shows that all of the signal is accounted for in the cosine+linear fits. The zero slope fits to the zero slope residuals just visually emphasizes this fact. No significant excursions. This is not very hard to understand.

At this point in your response, you wrote: "*Here's the truth, for those who are interested: you really (really!) don't know what you're talking about. You're just making up wordy phrases to justify your rather obvious incompetence.*"

Thank-you for that: it provides me an opportunity to summarize your argument thus far. You've dismissed the empirical demonstration of an oscillation in observational data on the grounds of needing a miraculous prescience of future cyclicity. In so doing, you displayed an apparent non-recognition of standard phenomenological analysis. You've improperly inferred meaning where it did not exist, and missed meaning where it was presented. Your argument has thus far rested upon continued misconstrual and thereafter you proposed that I'm incompetent. Good job.

[**Response: It's not worth my time to argue with an idiot. If readers want to do so, that's their choice.**]

Pat Frank | June 12, 2011 at 11:59 pm | Reply

Part 2:

Tamino, you wrote: "*Malarkey. It just "looks" that way to you for*

three reasons: 1) you plotted it on such a small scale that it hides the visual changes, 2) you did no statistics (anywhere), and 3) you desperately want it to be so."

I overlooked putting a scale on the residuals plots, it's true. Apologies. That was an oversight. The scale was 0.2 C per division. But in any case, comparative inspection of the noise readily shows that the residuals and the anomalies were plotted on a similar scale. That takes care of the substance of your "1)." Your "1)" also improperly implies a motive not in evidence. So does your "3)." For your "2)," the statistics of these fits are less important than showing the uniformly zero-slope of the residuals, which demonstrate that the fits account for the great majority of the anomaly signal.

You wrote: "*I plotted *your* residuals in better fashion: … There are indeed "important excursions away from zero." Like, the last decade.*"

How about the residual for 1890-1900 and 1940-1950? Those decades show excursions just as large as the last decade, except they're not conveniently at an end-point. How did you miss realizing that? They show the residual is noisy throughout, vitiating your entire point.

In fact, if our audience squints at your smoothed line, they'll see what looks like a weak cycle traversing the residual, which you'd say isn't there because we have no miraculous prescience about its future extension. A cosine fit the GISS or CRU residual does indicate a weak oscillation in each of them, about twice as intense in

the CRU residual [~(+/-)0.09 C) as in the GISS residual [~(+/-) 0.45 C). But, neither really emerges from the noise, so one shouldn't make too much of them.

You wrote: "*SO -- once again you admit that your model is just an exercise in curve-fitting*."

As already noted the last time you supposed this, you've inferred what was not in evidence, and have apparently not recognized a straight-forward phenomenological analysis.

"*And you continue to misuse the term "oscillation," apparently thinking that down-and-up followed by another down-and-up justifies its use.*"

Two cycles are two cycles. This simple fact escapes your gaze, fixated as it is, apparently, on needing to foresee the future so as to recognize what is in plain sight today.

"*I can indulge in curve-fitting too. And when I did, my model came out a helluva lot better than yours.*"

None of your models except the first one are justified by a signal that emerged after one variable was changed in one of two otherwise homologous physical data sets. That makes them irrelevant.

In response to the point that you fit your sinusoidal test model over a different data range, you wrote: "*Why is it a revelation to you, that estimating parameters from most (but not all) of the data, then using that to predict the remainder, is a valid test?*"

Fine, then let's put the models to a valid climate-science style test of goodness. I duplicated your 1880-1999 cosine+linear test fit as:

anomaly T = -0.0871*cos(6.581*YYYY)+0.00502*YYYY-9.817, where YYYY=4-digit year.

Then in proper climate science fashion I regressed it against the entire 1880-2009 data set. The correlation r^2 was 0.833 (p lt 0.0001); pretty darn good, even for climate science.

The same regression of the full 1880-2009 cosine+linear fit over the same entire data set: anomaly T = -0.103*cos(6.575*YYYY) +0.00575*YYYY-11.21, showed only a slightly improved correlation r^2 of 0.844 (p lt 0.0001). So, your more limited fit is hardly worse than the full fit.

Further, if one regresses the 2000-2009 residuals obtained by extrapolating your 1880-2000 fit, against the 2000-2009 residuals obtained from the full 1880-2009 fit, the correlation r^2=0.97 and the mean difference of the residuals is 0.1 C; equivalent to the noise of the unfit residual [(+/-)0.1 C, 1880-2009; (+/-)0.09 C, 1880-1999]. For anyone interested, that means they're the same to within about 1-sigma.

But we can go even farther. One should detrend these data sets when specifically looking for a periodic signal. So, I did that by separately fitting lines to the full GISS 1880-2009 and to the truncated GISS 1880-1999 data sets, and then subtracting its fitted line from each set over its entire range.

I then looked for an oscillation in the detrended 1880-1999 data, i.e., your truncated test data, and in the detrended 1880-2009 data, i.e., the full data set, using the same cosine function as before, a*cos(b*YYYY), where YYYY is again the 4-digit year.

The result:

1880-1999: -0.054*cos(5.01*YYYY); (detrending slope: 0.049 C/decade)

1880-2009: -0.060*cos(5.01*YYYY); (detrending slope: 0.057 C/decade)

That is, both time regions yielded fitted cosines with virtually identical phases and periods, but with slightly different (0.9:1) amplitudes.

A look at the two detrending slopes reveals the major cause of the poorer quality fit of your chosen test range. Removing the end-point warm years of 2000-2009 down-tilted the linear part of your fit. The problem of a poorer fit does not reside in the cosine part. We can notice this as well when looking at the coefficients of the above cosine+line fits over 1880-2009 and 1880-1999: the cosine periods are identical to within 0.01, but the slopes of the linear parts differ.

So, both data sets support the same oscillation and are within 0.1 C of the observed trend over the prediction range of 2000-2009. Indeed, the standard deviation of the 2000-2009 residuals from your truncated fit range is (+/-)0.09 C, relative to (+/-)0.08 C for the residuals of the full range fit, no matter the steep 2000-2009 residual line you chose to emphasize.

So, at the end, the cosine+linear model easily passes your truncated data set verification test.

- **Pat Frank** | June 13, 2011 at 12:01 am | Reply

Part 3:

Continuing: after I pointed out that you composed your original criticism with the false premise of a predictive model, you wrote: "*Your model utterly fails, both statistical comparison to the 4-line model, and fitting the most recent decade, even when it's constructed using the entire time span.*"

Your 4-line model stems from no physical inference and is irrelevant. To the contrary, the cosine+linear model does so stem and is therefore phenomenologically relevant. The model is now shown to succeed both over the full data range and over your own chosen truncated range.

Both produce the same underlying oscillation, which can then legitimately be subtracted from the full anomaly data set to yield the net linear 130-year warming trend. My original analysis is again justified and follows directly.

You wrote< "*The test I did was pretty much standard faire in what we call "science.""*

It's standard fare in proxy reconstructions of paleo temperatures. We can agree to disagree about whether that field is science. So, I'm glad you chose to enquote the word. However, typical in science is to predict the appearance of results not yet in hand; results to be obtained by further experiment or observation. In any case, as we've all now seen, the cosine+linear model passed your chosen

test to high climate-science verification standards.

When I suggested you used end-point padding in your smooth, you wrote: "*Sigh … you REALLY don't know what you're talking about*." and indicated having used a Lowess smooth. Great. We're all relieved. But you didn't mention your method in the original critique.

Given the widespread use of end-point padding in climate science, my inference is hardly surprising or extreme. So, your mannered sigh and pointed comment appear to be the opportunistic exploitation of an understandably mistaken inference stemming from your own methodological silence.

After I mentioned the analogous periodicity of the AMO and PDO, you wrote, "*You've made it clear that you don't really know what periodic behavior is*." By now everyone knows that recognition of cyclicity is empirical, and that your criterion of cyclicity by prescience is nonsense.

You then wrote, "*The AMO and PDO do *not* show periodicity. Again, there aren't nearly enough "cycles" to show such. But you don't seem to get this simple fact*."

Maybe you should read Marcia Wyatt?s comment here and the comments following, and take a look at the fact of this plot, obtained from the same comment thread.

Your disparagement of Joe D?Aleo is unworthy of reply.

When I mentioned that, "statistical criteria (AIC) applied to irrelevant models do not yield any insights into the validity of a physically justified analysis," you replied, "*What????????? You*

have lost your mind.”

So, as you clearly disagree, suppose you explain how statistical criteria applied to irrelevant models yield positive insights into the validity of a physically justified analysis. That should be fun.

And, finish line thankfully in sight, we reach your final words: “*To reiterate the truth: the cosine model does NOT have a physical basis.*”

Demonstrated wrong.

“*There is NOT established periodic behavior in AMO or PDO.*”

Demonstrated wrong.

“*To imply that they are responsible for global temperature change is idiotic.*”

Obviously irrelevant: I never wrote or implied they are so responsible.

To reiterate what I actually did write with respect to the PDO +AMO: they display about the same ~60 year periodicity as the fitted oscillation in the 130-year global air temperature anomaly trend. They were offered as a surrogate for a net global ocean thermal cycle that apparently puts a ~60-year thermal oscillation into the global air temperature anomalies.

- **Pat Frank** | June 13, 2011 at 12:04 am | Reply

In your defense of Kartoffel’s post, you wrote: “*First you said*

“climate of the first 40-50 years of the 20th century was unaffected by human-produced GHGs.” Now you’re backing off to “primarily natural forcings.”

Using the Myhre, 1998 [1] equation, the extra forcing due to increased CO2 (from 1900) produces an anomaly temperature of about 0.07 C by 1940. That anomaly is well within natural variation and is unobservable. To assume it was truly present is to assume what is to be demonstrated. You wouldn’t want to indulge circular thinking, would you.

In an empirical analysis, the only alternative to such circular thinking is to proceed on the basis that air temperature in the early part of the 20th century was driven by natural forcings, and unaffected by human-produced GHGs.

You also wrote, “ *By the way, the inertia of the climate system impacts all forcings, not just greenhouse gases.*”

Right. And during the early 20th century, the inertia of the climate system was derived from the natural forcings and feedbacks of prior centuries.

When I wrote, “An empirical analysis of this sort is a test of theory. Making changes to the empirical warming rates to conform with theory injects the expected into the known.”

Your reply: “*Do you not see the fault of your argument? You’re trying to minimize the impact of early-20th-century greenhouse gases using IPCC model results. But those model results *demonstrate* exactly the consensus values of climate sensitivity that you’re attempting to dispute!*”

Climate-system inertia due, for example, to the heat capacity of the oceans and the isothermal phase changes of water, will be part of any complete theory of climate. There's no acceptance of any particular formulation of climate theory in recognizing that.

More to the point, there is no empirical reason to assign anything unusual or unnatural to the surface air temperature trend of the early 20th century. Therefore to use this time as an empirical baseline is valid.

The point of noting GCM predictions is that their in-built water vapor feedback gives a maximal estimate of the effect of increased CO2, and they *predict* an unobservably small thermal effect for the earlier 20th century

Therefore, the proposition that the thermal history of the early 20th century reflects natural forcings and feedbacks is both justified empirically and predicted by your GCMs.

Since the unbiased empirical view and the GCM view agree on an unperturbed early trend, then it is entirely valid to use the history of early 20th century air temperatures to test the prediction of GCMs as regards the later 20th century surface air temperatures. During this later time only, do GCMs predict a detectable anthropogenic thermal effect.

There is no "fault" to my argument. Anyone who thinks GCMs actually predict surface air temperatures, in the scientific meaning of "predict," will have to agree that the comparative early/late test is valid.

And in the event, the test falsified the prediction.

Finally, by this statement, "*I guess you'll accept IPCC models as evidence — unless you don't want to.*" shows that you didn't see the very standard scientific reasoning I've described above, about how to use data to test a theory. I'm surprised you missed it.

Theories do not produce "evidence," by the way. Theories, falsifiable in science, produce predictions and provide explanations (meaning). Evidence in science is strictly the province of observation and experiment. Evidence tests theory, theory provides meaning and produces analytically testable predictions. Your supposition that GCMs provide evidence is to fundamentally misconstrue theory and result in science.

[1] G. Myhre, et al., (1998) "New estimates of radiative forcing due to well-mixed greenhouse gases" Geophys. Res. Lett. 25(14), 2715-2718, Table 3.

Pat Frank | June 13, 2011 at 12:11 am | Reply

Ken, your criticisms of my article in Skeptic rested on discredit by personal attack, just as does your criticism here.