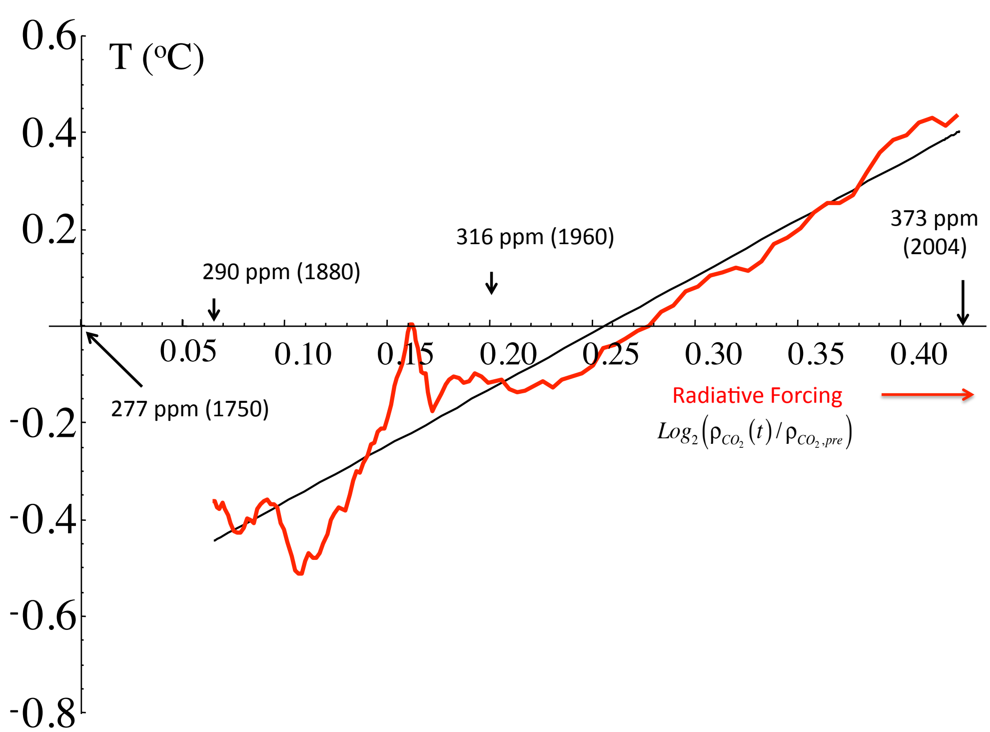
Global temperature variations, fluctuations, errors and probabilities

Questions and Answers about the Climate Dynamics paper

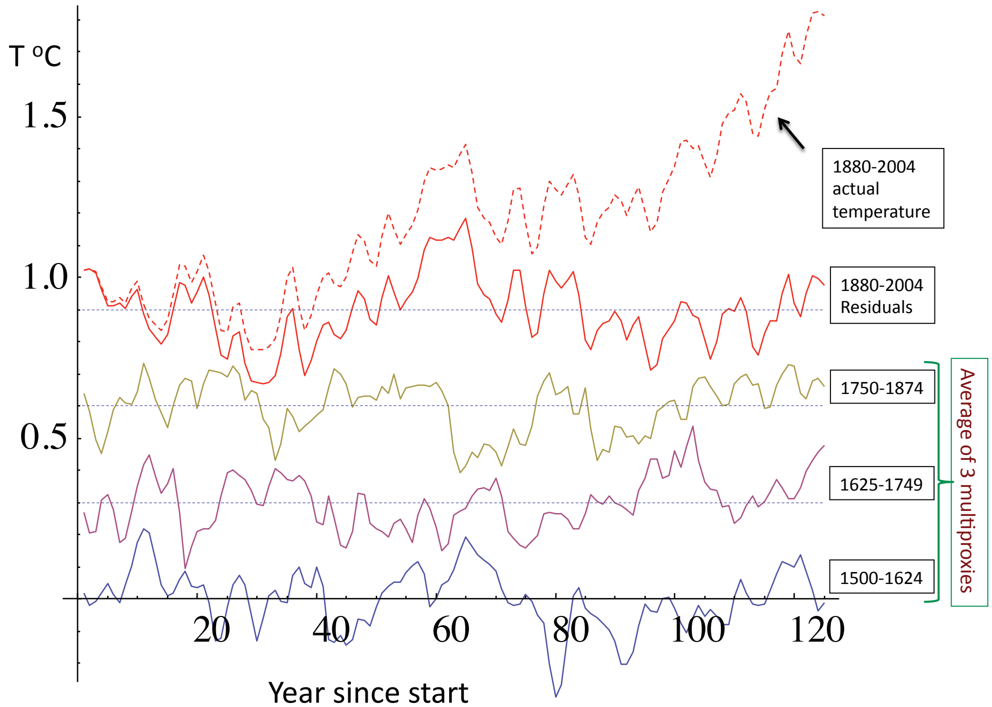
I have been flooded with questions and purported rebuttals of my paper and cannot answer them all individually. Here are some common misconceptions, misunderstandings and in some cases misrepresentations of my paper. A (legal not copyright protected) pre-proof version of the paper [*Lovejoy*, 2014a] can be found here:

http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/Anthro.climate.dynamics.13.3.14.pdf.

Key figures from the Climate Dynamics paper



This figure visually shows the strong linear relation between the radiative forcing and the global temperature response since 1880, it is a simplified version of fig. 3a of [*Lovejoy*, 2014a] showing the 5 year running average of global temperature (red) as a function of the CO_2 forcing surrogate from 1880 to 2004. The linearity is impressive; the deviations from linearity are due to natural variability. The slope of the regression line is 2.33±0.22 °C per CO_2 doubling (it is for the unlagged forcing/response relation).



The three lower curves are the means of three multiproxies over three consecutive 125 year periods starting in the year 1500. Each segment had its overall mean removed and was displaced by 0.3 °C in vertical for clarity. The fourth curve from the bottom is the estimate of the natural variability after removing the anthropogenic contribution determined by the straight line in the forcing versus temperature graph (above, from 1880-2004). The top (dashed) curve is the *actual* annual resolution mean global temperature. Whereas the curves from the three multiproxy epochs are quite similar to the residuals in the recent epoch, the actual recent epoch temperature shows a strong, systematic increase (Edited slightly from fig. 5 of the Climate Dynamics paper).

Questions in italics, responses regular font

Q. It is impossible to determine climate sensitivity either to the claimed precision of 0.01 Cº or to 99% confidence from the temperature data.

A. This is a misrepresentation: I never claimed that I could estimate the climate sensitivity to that accuracy – my value was 3.08±0.58 ^{o}C i.e. 95% of the time between 3.08-2x0.58 and 3.08+2x0.58 equivalently between 1.9 and 4.2 ^{o}C for a CO_2 doubling, and the confidence itself was not simply 99% but in a range of 99% to 99.99% with 99.9% the most likely.

Q. OK, so what about the ±0.03^{o}C global annual surface temperature error estimate just after eq. 1 in the paper?

A. The short answer is that the issue of accuracy of the surface measurements is overblown, misunderstood and – unless it is far larger than any estimates give – it is nearly irrelevant to the final conclusion (the number ±0.03 ^{o}C is from a paper a year old, [*Lovejoy et al.*, 2013], http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/ESDD.comment.14.1.13.pdf).

If you really want to know the details, see the section for the statistically inclined (below). The following Q and A also gives more details.

Q. Why did you ignore the temperature measurement errors?

A. Because the key error is the estimate of the anthropogenic component of the warming – which was estimated as ±0.11 ^{o}C. This estimate is mostly due to the uncertainty in the correct lag between the forcing and the response (zero to 20 years). The uncertainty (±0.11 ^{o}C) is so much larger than ±0.03 ^{o}C that the latter is irrelevant.

Q. I think eq. 1 is a con. You simply ***define*** *the natural variability as the residue of a regression with the CO_2 forcing, you can't separate out the anthropogenic and natural components in this way.*

A. Equation 1 is motivated by the fact that anthropogenic effects – what ever they are - don't affect the statistical *type of variability*, essentially they affect the atmospheric boundary conditions, so that the equation is at least plausible, it's actually a scientific hypothesis not just a definition. In science you're entitled to make any hypothesis you like and *a priori* they may be totally wrong. That's why you test them against the data. The reason that we can be confident that eq. 1 isn't wrong is because the residues have nearly the same probability distributions and generally same statistics as the pre-industrial multiproxies (see the Q and A below). Therefore the hypothesis that they really are natural is verified (just check with your eye fig. 5 (top of this Q+A), or do it properly, fig. 8).

Q. Your claim to be a nonlinear geophysicist makes me laugh. Your eq. 1 and hypothesis about the CO_2 RF surrogate is totally linear, you're a hypocrite!

A. You're the one missing a subtlety. I never said that in Equation 1, that T_{nat} would have been the same with or without the anthropogenic changes/forcings. That would indeed be a linearity assumption. I only hypothesize that the statistical *type* of variability is the same.

This means that if I had two identical planets and on one of them, we let humans loose while on the other, there were none, then the T_{nat} would be totally different in their details but that their statistics (such as their standard deviations) at all scales would be the same. In other words, eq. 1 does not contradict the fact that there can (and will) be nonlinear interactions between the anthropogenic forcings and the "internal" atmospheric dynamics.

Q. But the IPCC estimates the anthropogenic warming as 0.85 ± 0.20 ^oC (0.65 to 1.05 to ^oC) which has much larger range, larger uncertainty - how does that affect the results?

A. The research for the paper was done before last September when the AR5 came out. I therefore used the AR4 (2007) range of 0.74±0.18 ^oC i.e. 0.56 to 0.92 ^oC. The AR4 lower limit (0.56 ^oC) is a bit smaller than the AR5 lower limit (0.65 ^oC), so that the more recent AR5 estimate makes us even more confident that the warming is not natural. Anybody can use fig. 10 of the paper to check that with the AR5 values, the probability of the warming being natural goes down from 0.3 - 0.7% (AR4) to 0.1 - 0.3% (depending on how extreme we assume the tails to be). My lower limit 0.76 ^oC) gives a range of 0.05-0.2%.

Q. How big would the error have to be in order to make it plausible that the industrial epoch warming is a just a natural fluctuation?

A. It depends on where your probability cut-off is. Let's say that we're ready to accept that the warming was natural even if the probability was as low as 10% (some people would reject the hypothesis at this level, but let's see what it implies). Using fig. 10, we find that this implies a change of only 0.26 ^oC. Therefore, even if the temperature in 2013 was only 0.26 ^oC higher than it was 125 years ago, this would still be surprising since it would only happen 10% of the time, but it might nevertheless be a credible natural fluctuation.

Notice that the estimate 0.26 ^oC refers to an event that occurs over the *entire* 125 year period starting at its beginning, and whose probability of occurring is 10% or less. This means that if we took 1250 years of the pre-industrial temperature record and broke into ten 125 year segments, that only one of them would have a *change* of 0.26 ^oC (or more) over its duration, all the others would have smaller changes.

The actual value of the industrial period warming would therefore have to be about three times smaller than current estimates in order for the natural warming hypothesis to have any credibility.

Q. But the post-war cooling event was larger than 0.26 ^{o}C, surely there is a mistake?

A. No, it is correct. The post-war cooling (1944-1976) turns out to be in the range 0.42 – 0.47 ^{o}C (this includes an anthropogenic warming of 0.16 -0.21 ^{o}C, the observed cooling is actually 0.26 ^{o}C; these estimates use the same methodology as the *Climate Dynamics* paper and have been submitted to review elsewhere). But according to fig. 10, such a large event is expected to occur every 100 years or so (the "return period"). This can be also seen from the direct use of the 32 year curve in fig. 9). However this is the biggest 32 year event we would expect in any 125 year period of time - *no matter when it occurs within the 125 years* - not starting at a specific (given) point in time, here in 1880.

Thanks to natural variability, some event – with an amplitude equal to the post war cooling - is thus *expected* to occur every century or so. Ex post facto, we know that this event actually started in 1944. However, in 1880 we could only know that such an event would almost surely occur at some point during the next 125 years.

Q. I still don't understand how we can expect some 32 year event to be as big as 0.42 -0.47 ^{o}C yet we don't expect the 125 year temperature difference from 1880 – 2004 to be nearly so big (less than 0.26 ^{o}C). How is this possible?

A. The reason is that from about 10 days until about 125 years, temperature fluctuations tend to cancel each other out. This is the "macroweather regime" with the property that over a time period Δt, the fluctuations in temperature ΔT tend to decrease in a power law way: $\Delta T\ (\Delta t) \approx \Delta T^{H}$ with $H \approx$ -0.1 for global temperatures. ([*Lovejoy*, 2013], http://onlinelibrary.wiley.com/doi/10.1002/2013EO010001/pdf). Therefore if there is a large random/natural excursion of temperature up, it tends to be followed by one down that nearly cancels (and visa versa). For example, this is indeed the case of the post-war cooling. It is also the case of the recent so-called "pause" - but that's another story for another paper!

Q. This has to wrong. If fluctuations always tend to cancel, then there would be no medieval warming, no Little Ice Age and – for that matter - no Big Ice Age!

A. No, it correct. The reason is that the macroweather regime ends at about 100 – 125 year time scales and that the longer time scales have a lot more low frequencies; the exponent H changes from -0.1 to about +0.4 so that on the contrary, the temperature fluctuations tend to grow with increasing time interval, they tend to "wander"; this the true "climate" regime.

But of course this slow "wandering" doesn't effect the shorter time scale 125 year fluctuations that are important here.

Q. I still don't trust the global temperature series, what can I do?

A. The global series I picked are generally agreed to be the best available, although since the analysis was done, they have had updates/improvements.

If you still don't like them pick you own, then estimate the total change over a 125 year period and then use fig. 10 to work out the probability. You will find that the probability rapidly decreases for changes larger than about 0.4°C - so as discussed above - to avoid my conclusion, you'll need to argue that the change in temperature over the last 125 years is about a half to a third of what all the instrumental data, proxy data and models indicate.

Q. But what about the Medieval warming with vines growing in Britain - or the Little Ice Age and skating on the Thames? Surely the industrial epoch warming is just another large amplitude natural event?

A. Well no. My result concerns the probability of centennial scale temperature *changes*: large changes – if they occur slowly enough – are not excluded. So if you must, let the peons roast and the Thames freeze solid, the result stands.

The question indicates a misunderstanding about the role of time resolution/ time scale. Taking differences at 125 years essentially filters out the low frequencies.

Q. To estimate the probabilities you used the hockey stick and everyone knows that it has been discredited, why should I believe your probabilities?

A. The hockey stick is the result of using a large number of paleo data to reconstruct past global scale (usually annual resolution) temperatures. Starting with the [*Mann et al.*, 1998] series, there are now a dozen or so in the literature. They have been criticized and improved, but not discredited. For example, responding to early criticism, those published since 2003 have stronger low frequency variability, thus rectifying some of the earlier limitations. But in spite of some low frequency disagreements, as I have shown, even the pre and post 2003 multiproxies agree well with each other up to about 100-200 year scales which is all that I need here (see fig. 9, 10 of [*Lovejoy and Schertzer*, 2012a], http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/AGU.monograph.2011GM001087-SH-Lovejoy.pdf, and see also ch. 11 in [*Lovejoy and Schertzer*, 2013].

Thus, while it is true that the multiproxies disagree somewhat with each other at longer scales, this is irrelevant, only the statistics at 125 year scales are important and up to this time scale they agree quite well with each other. As mentioned before, it's therefore possible that several (even all!) have underestimated the medieval warming, but this is irrelevant to my conclusions.

Q. Why did you pick the multiproxies that you did: wouldn't you get different results with a different choice?

A. I picked a diverse group – one of the three (Huang) used boreholes (it needed no paleo calibrations), one used a wavelet based method that helped to ensure the low frequencies were realistic (Moberg), and one was an update of the original Mann series with various improvements.

But as indicated, for the *probability distribution of the differences of 125 years and less*, there isn't much difference between any of them (note that in principle the series could be totally different but still have identical probabilities). Readers can check the distributions in fig. 6 which show that the probabilities of differences are nearly identical – except for a small difference in the fluctuation amplitudes. Such differences are exactly what we expect if the amount of spatial averaging is a little different in each (due to different mix of paleodata).

Q. How can you estimate a probability of an extreme (one in a thousand) event that lasts a century? There isn't anywhere near enough data for that!

A. This is the subject of the key second part of my study that uses the multiproxy data from 1500 to estimate the probability that this temperature change is due to natural causes. Since we are interested in rare, extreme fluctuations, a direct estimate would indeed require far more pre-industrial measurements than are currently available. This type of problem is standard in statistics and is usually solved by applying the bell-curve. Using this, the chance of the fluctuation being natural would be in the range of one in a hundred thousand to one in ten million. Yet we know that climate fluctuations are much more extreme than those allowed by the bell curve. This is where nonlinear geophysics comes in.

Nonlinear geophysics confirms that the extremes should be far stronger than the usual "bell curve" allows. Indeed, I was able to show that giant century long fluctuations are more than 100 times more likely than the bell curve would predict, yet - at one in a thousand - their probability is still small enough that they can be confidently rejected.

Q. You're trying to con me with vague but sophisticated sounding gobbly-gook. Give me more details, I'm still not convinced.

A. One needs to make an assumption about the probability tails (extremes). I explicitly describe that assumption: that the distribution is asymptotically bounded by power laws - and I give a theoretical justification from nonlinear geophysics: power law probability tails ("fat tails", associated with "black swan" huge fluctuations) are generic features of scaling processes (such as the global temperature up to about 125 years). Scaling also comes in because I use it to extrapolate from the 64 year distributions to the 125 year distributions, but there is big literature on this scaling (some of it is cited). Finally there is specific evidence going back to 1985 (cited in the paper) for power law probabilities in climatological temperatures including with the exponent $q_D = 5$ (see [*Lovejoy and Schertzer*, 1986], http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/Annales.Geophys.all.pdf). Classical statistics assume Gaussian distributions (q_D = infinity) and the latter hypothesis would make the extremes 100 to 10000 even less likely. For my 99% confidence result to be invalid, the power law would have to be incredibly strong and start at probabilities below around 0.001. Even if you don't like power law tails - you might find them distastefully extreme and would feel more comfortable with familiar Gaussians - here they are only used as bounds. With Gaussians, the actual probabilities would thus much smaller (as indicated above).

Q. What about cycles? Lots of people have claimed that temperatures vary cyclically, couldn't a cycle of the right length explain the warming?

A. No. The reason is that almost all of the variability (technically, the variance) is in the "background" (continuum) part of the spectrum which as mentioned above, is scaling (power law). The cycles may exist, but their contribution to the variability (including overall changes) is tiny (with of course the exception of the annual cycle!). See: [*Lovejoy*, 2014b], http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/esubmissions/climate.not.climate.dynamics.7.2.14.pdf.

Q. It's obvious that it's the sun that's causing the temperature to change, think about the sunspots!

A. Solar forcings and responses are already taken care of in my analysis since T_{nat} was defined to including anything not anthropogenic. However, at climate scales (longer than 125 years), they might be important (e.g. for roasting the peons in 13th century Britain). However, this is not trivial since according to sunspot based solar reconstructions, we would need to find a mechanism that could nonlinearly amplify the fluctuations by a factor 15 – 20, and this factor must apply over a wide range of time scales (see [*Lovejoy and Schertzer*, 2012b], http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/Sensitivity.2012GL051871.proofs.SL.pdf). If we use the competing ^{10}Be based reconstructions, then the situation is hopeless since these reconstructions imply that rather than growing with scale ($H \approx 0.4$ as for the temperatures and sunspot based forcings), the ^{10}Be based reconstructions rapidly decrease with scale with $H \approx -0.4$.

Q. OK, forget the sun, it's got to be volcanoes! We know that when they go off there's a huge effect - think of Pinatubo or Krakatoa! Haven't the last hundred years been a bit calmer? (less volcanism, less volcanic cooling?).

A. The short answer is no, my analysis implicitly includes volcanism.

To make this more plausible, the beginning of my paper addresses this specifically and shows that the last century of volcanism was a little weak, but it was no big deal. More to the point, when a volcano blows it's a strong effect, but it doesn't last long. Over a century, you can forget it. According to fig. 1 (and see [*Lovejoy and Schertzer*, 2012b]), for volcanic forcing, $H \approx -0.4$ so effects from volcanism (i.e. the time series of volcanic forcings) rapidly cancel each other out.

Q. How can you ever prove that the warming was anthropogenic? As a scientist you know that you have to do an experiment to prove anything, and we can't rerun the last 125 years without fossil fuels or humans just to see what would have happened. You'll never con me into believing that it's our fault!

A. This is absolutely correct, but it is not what I claim!

I am not trying to *prove* that anthropogenic warming is correct, I'm trying to *disprove* that natural warming is correct! Of course disproving natural warming makes anthropogenic warming a lot more plausible, but it isn't "proof" in a mathematical sense. Maybe you'll come up with some other alternative for example, a one-off miracle warming type event would not contradict my analysis (of course it would likely contradict the laws of physics, but if you are to ready to suspend these…).

As I point out in the last sentence of my paper: "While students of statistics know that the statistical rejection of a hypothesis cannot be used to conclude the truth of any specific alternative, nevertheless - in many cases including this one – the rejection of one greatly enhances the credibility of the other." (This last part is because we're doing science, not statistics!).

For the statistically minded, more error analysis

In equation 1 in my paper I mention that the average global temperatures can be estimated to an accuracy of ±0.03 °C, and I refer to the source (fig. 1 bottom curve, [*Lovejoy et al.*, 2013] http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/neweprint/ESDD.comment.14.1.13.pdf). What does the figure show? I simply took four globally, annually averaged surface temperature series from 1880 (NOAA, NASA, HAdcrut3 and the Twentieth Century Reanalysis (20CR) series, [*Compo et al.*, 2011]). These series are all different, they use different data sets (although with overlap) and different methodologies. In particular, one of them (the 20CR series) used no station temperatures whatsoever, [*Compo et al.*, 2013] (only station pressure data and monthly averaged Sea Surface Temperatures are used) so for this one, there are no issues of urban warming or stations being moved around, or tweaked or even suffering from slow changes in day time –night time variability! They all agree with each other to ±0.03K up to ≈ 100 year scales. (This number was estimated from *the* root mean square of these differences which from the figure cited above is close to 0.05°C; if the corresponding variance is equally apportioned between the series, this leads to about ±0.03 °C each. Finally, we're interested in the differences at century scales, and this allows us to do even more averaging, potentially doing even better, see below for more details. Any biases from manipulation of temperature station data must be small.

The basic reason that the errors are so small is that we are averaging enormous numbers of measurements; if we take a large enough number we may expect errors to cancel out and become negligible. For example, using standard statistical assumptions about the independence of the errors, one expects that the overall error decreases as the square root of the number of measurements.

As explained above, the one year differences have errors of about ±0.03°C, but what about the systematic errors? How to quantify this?

I used a more sophisticated method of estimating the fluctuations between each series and the mean of the four series. Its called Haar fluctuations (see: [*Lovejoy and Schertzer*, 2012c]

http://www.physics.mcgill.ca/~gang/eprints/eprintLovejoy/Haar.npg-19-513-2012.final.pdf). Over a time interval Δt, it is defined as the average of the first half minus the average of the second half of the interval (i.e. for a century scale, this would be the difference of the average of the first 50 years minus the average of the next fifty years). Due to the averaging, this is quite robust, but also, since we can change Δt, we can examine how quickly the errors decrease with scale.

How does this work for the error estimates? I then took their average and computed the standard deviations of their differences from the average as a function of time scale. That means taking the series at one year resolutions and looking at the differences, 2 year averages, then the differences, 4 year, 10 year etc. averaging over longer and longer periods and taking differences. The root mean square of these differences is close to 0.05°C and – interestingly – it barely changes with scale. If the corresponding variance is equally apportioned between the series, this leads to about ±0.03 °C each. Since, we expect the overall error to decrease as the square root of the averaging period, the near constancy of the error implies that the errors and not independent of each other, this is an effect of the systematic errors: because of this, the overall century scale errors are much larger (about ten times larger), yet they are still very small!

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