Heat, Ocean, and Atmosphere

Origins of the Warming 'hiatus' Concept

Jan Galkowski

Westwood Statistical Studios

`empirical_bayesian@ieee.org`

Westwood, MA 02090

April 26, 2014

(revised draft 001)

When present, changes are indicated with red changebars, such as to the left of this paragraph.

1   How Heat Flows and Why It Matters

Heat is most often experienced as *energy density*, related to *temperature*. While technically temperature is only meaningful for a body in thermal equilibrium, temperature is the operational definition of heat content, both in daily life and as a scientific measurement, whether at a point or averaged. For the present discussion, it is taken as given that increasing atmospheric concentrations of carbon dioxide trap and re-radiate Earth blackbody radiation to its surface, resulting in a higher mean blackbody equilibration temperature for the planet, via *radiative forcing* [1, 2, 3, 4]. The question is, how does a given Joule of energy travel? Once entrained on Earth, does it remain in atmosphere? Warm the surface? Go into the oceans? And, especially, if it *does* go into the oceans, what is its *residence time* before released to atmosphere? These are important questions [5, 6]. Because of the miscibility of energy, questions of residence time are very difficult to answer. A Joule of energy can't be tagged with a radioisotope like matter sometimes can. In practice, energy content is estimated as a constant plus the time integral of energy flux across a well-defined boundary using a baseline moment.

Variability is a key aspect of natural systems, whether biological or large scale geophysical systems such as Earth's climate [7]. Variability is also a feature of statistical models used to describe behavior of natural systems, whether they be straightforward empirical models or models based upon *ab initio* physical calculations. Some of the variability in models captures the variability of the natural systems which they describe, but some variability is inherent in the mechanism of the models, an artificial variability which is not present in the phenomena they describe. (The nomenclature can be confusing. With respect to observations, variability arising due to choice of method is sometimes called *structural uncertainty* [37, 38].) No doubt, there is always some variability in natural phenomena which no model captures. This variability can be partitioned into parts, at the risk of specifying components which are not directly observable. Sometimes they can be inferred.

Models of planetary climate are both surprisingly robust and understood well enough that appreciable simplifications are possible, such as setting aside fluid dynamism, without damaging their utility [2, Preface]. Thus, long term or asymptotic and global predictions of what consequences arise when atmospheric carbon dioxide concentrations double or triple are known pretty well. What is less certain are the dissipation and diffusion mechanisms for this excess energy and its behavior in time [8, 9, 10, 11]. There is keen interest in these mechanisms because of the implications differing magnitudes have for regional climate forecasts and economies [12, 13, 14]. Moreover, there is a natural desire to obtain empirical confirmation of physical calculations, as difficult as that might be, and as subjective as judgments regarding quality of predictions might be [15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 36, 37, 26, 27, 28, 29, 30, 31].

Observed rates of surface temperatures in recent decades have shown a moderating slope compared with both long term statistical trends and climate model projections [32, 33, 15, 34, 36, 18, 21, 35, 16]. It's the purpose of this article to present this evidence, and report the research literature's consensus on where the heat resulting from radiative forcing is going, as well as sketch some implications of that containment.

2   On Surface Temperatures, Land and Ocean

Independently of climate change, monitoring surface temperatures globally is a useful geophysical project. They are accessible, can be measured in a number of ways, permitting calibration and cross-checking, are taken at convenient boundaries, land-and-atmosphere or ocean-and-atmosphere, and coincide with the living space about which we most care. Nevertheless, like any large observational effort in the field, such measurements need careful assessment and processing before they can be properly interpreted. The Berkeley Earth Surface Temperature ("BEST") Project represents the most comprehensive such effort, but it was not possible without many predecessors, such as HadCRUT4, and works by Kennedy, *et al* and Rohde [39, 37, 27, 40, 41].

Surface temperature is a manifestation of four interacting processes. First, there is warming of the surface by the atmosphere. Second, there is lateral heating by atmospheric convection and latent heat in water vapor. Third, during daytime, there is warming of the surface by the Sun or *insolation* which survives reflection. Last, there is warming of the surface from below, either latent heat stored subsurface, or geologic processes. Roughly speaking, these are ordered from most important to least. These are all manifestations of *energy flows*, a consequence of equalization of different contributions of energy to Earth.

Physically speaking, the total energy of the Earth climate system is a constant plus the time integral of energy of non-reflected insolation less the energy of the long wave radiation or *blackbody radiation* which passes from Earth out to space, plus geothermal energy ultimately due to radioisotope decay within Earth's aesthenosphere and mantle, plus thermal energy generated by solid Earth and ocean tides, plus waste heat from anthropogenic combustion and power sources. (There are tiny amounts of heating due to impinging ionizing radiation from space, and changes in Earth's magnetic field.) The amount of non-reflected insolation depends upon albedo, which itself slowly varies. The amount of long wave radiation leaving Earth for space depends upon the amount of water aloft, by amounts and types of greenhouse gases, and other factors. Our understanding of this has improved rapidly, as can be seen by contrasting Kiehl, *et al* in 1997 with Trenberth, *et al* in 2009 and the IPCC's 2013 WG1 Report [42, 43, 44]. Steve Easterbrook has given a nice summary of radiative forcing at his blog, as well as provided a succinct recap of the 2013 IPCC WG1 Report and its take on energy flows elsewhere at the *Azimuth* blog. I refer the reader to those references for information about energy budgets, what we know about them, and what we do not.

Some ask whether or not there is a physical science basis for the "moderation" in global surface temperatures and, if there is, how that might work. It is an interesting question, for such a conclusion is predicated upon observed temperature series being calibrated and used correctly, and, further, upon insufficient precision in climate model predictions, whether simply perceived or actual. Hypothetically, it could be that the temperature models are not being used correctly and the models *are* correct, and which evidence we choose to believe depends upon our short-term goals. Surely, from a scientific perspective, what's wanted

is a reconciliation of both, and that is where many climate scientists invest their efforts. This is also an interesting question because it is, at its root, a statistical one, namely, how do we know which model is better [46, 7, 25, 45, 47, 48, 49]?

A first graph, Figure 1.1, depicting evidence of warming is, to me, quite remarkable.   A similar graph
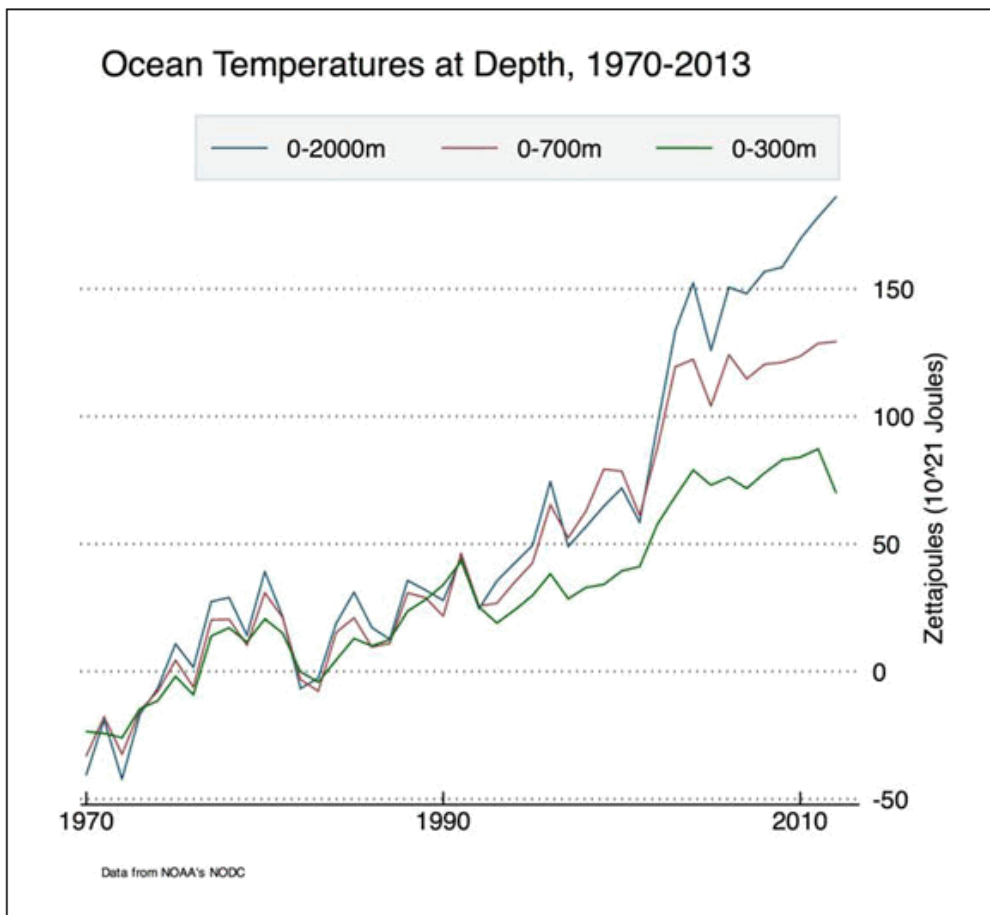
Total ocean heat content has increased by around 170 Zettajoules since 1970, and about 255 Zettajoules since 1955. This increased temperature has caused the oceans (0-2,000 meters) to warm about 0.09 C over this period. As the UK's Met Office points out, if the same amount of energy had gone into the lower atmosphere it would of caused about 36 C (nearly 65 degrees F) warming! The oceans are by far the largest heat sink for the Earth, absorbing the vast majority of extra heat trapped in the system by increasing concentrations of greenhouse gases.

It's important to point out that overall deep-ocean heating (0-2,000 meters) shows no sign of a slow down in recent years, though shallower layers (0-300 meters and 0-700 meters) do. That the slowdown in surface warming has been concentrated in the ocean-surface (and shallow-ocean) temperatures has led a number of scientists (including the Met Office) to posit that the pause in ocean surface warming may be driven in part by increased heat uptake in the deep ocean.

Figure   1.1:   Ocean   temperatures   at   depth,   from   `http://www.yaleclimatemediaforum.org/2013/09/` `examining-the-recent-slow-down-in-global-warming/`.

is shown in the important series recapping the recent IPCC Report by Steve Easterbrook [50]. A great deal excess heat is going into the oceans. In fact, most of it is. This can happen in many ways, but one dramatic way is due to a phase of the *El Niño Southern Oscillation* ("ENSO").

The trade winds along the Pacific equatorial region vary in strength. When they are weak, the phenomenon called *El Niño* is seen, affecting weather in the United States and in Asia. Evidence for *El*
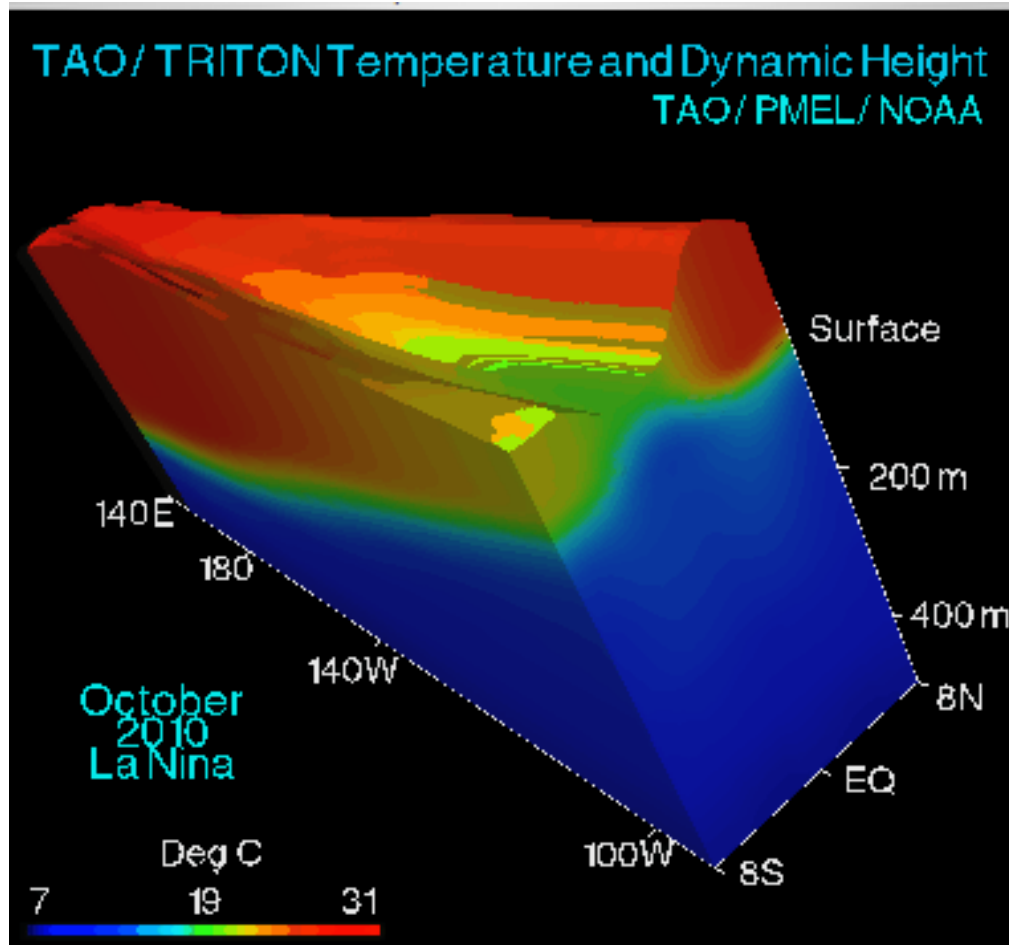
Figure 1.2: Oblique view of variability of Pacific equatorial region from *El Niño* to *La Niña* and back. Vertical heigh of ocean is exaggerated to show piling up of waters in the Pacific warm pool. **(Note that this will be replaced on the Web page with a GIF file.)**

Niño includes elevated sea-surface temperatures ("SSTs") in the eastern Pacific. This short-term climate variation brings increased rainfall to the southern United States and Peru, and drought to east Asia and Australia, often triggering large wildfires there. The reverse phenomenon, *La Niña*, is produced by strong trades, and results in cold SSTs in the eastern Pacific, and plentiful rainfall in east Asia and northern Australia. Strong trades actually pile ocean water up against Asia, and these warmer-than-average waters push surface waters there down, creating a cycle of returning cold waters back to the eastern Pacific. This process is depicted in Figures 1.2 and 1.3. At its peak, a *La Niña* causes waters to accumulate in the *Pacific warm pool*, and this results in surface heat being pushed into the deep ocean. To the degree to which heat goes into the deep ocean, it is not available in atmosphere. To the degree to which the trades do not pile waters into the Pacific warm pool and, ultimately, into the depths, that warm water is in contact with atmosphere [51]. There are suggestions warm waters at depth rise to the surface [52].

Documentation of land and ocean *surface* temperatures is done in variety of ways. There are several important sources, including Berkeley Earth, NASA GISS, and the Hadley Centre/Climatic Research Unit ("CRU") data sets [39, 53, 37]. The three, referenced here as BEST, GISS, and HadCRUT4, respectively have been compared by Rohde [41]. They differ in duration and extent of coverage, but allow comparable inferences. For example, a linear regression establishing a trend using July monthly average temperatures from 1880 to 2012 for Moscow from GISS and BEST agree that Moscow's July 2010 heat was 3.67
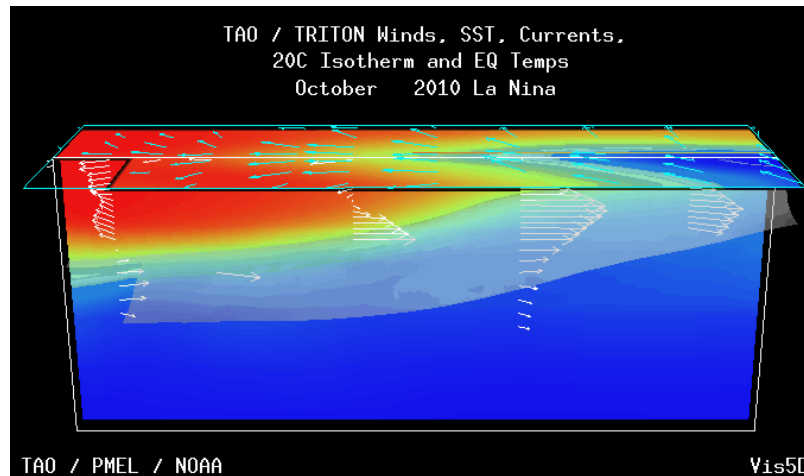
Figure 1.3: Trade winds vary in strength, having consequences for pooling and flow of Pacific waters and sea surface temperatures. (**Note that this will be replaced on the Web page with a GIF file.**)
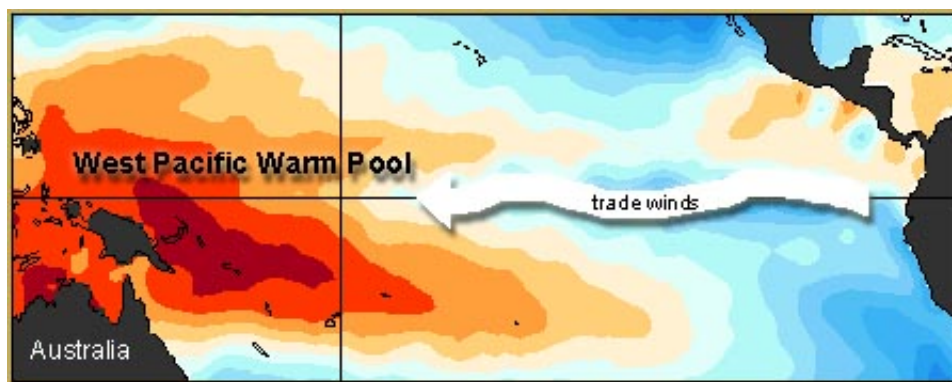

Figure 1.4: Strong trade winds cause the warm surface waters of the equatorial Pacific to pile up against Asia.

standard deviations from the long term trend. (3.667 (GISS) versus 3.670 (BEST).) Nevertheless, there is an important difference between BEST and GISS, on the one hand, and HadCRUT4.

BEST and GISS attempt to capture and convey a single best estimate of temperatures on Earth's surface, and attach an uncertainty measure to each number. Sometimes, because of absence of measurements or equipment failures, there are no measurements, and these are clearly marked in the series. HadCRUT4 is different. With HadCRUT4 the uncertainty in measurements is described by a hundred member *ensemble* of values, actually a 2592-by-1967 matrix. Rows correspond to observations from 2592 patches, 36 in latitude, and 72 in longitude, with which it represents the surface of Earth. Columns correspond to each month from January 1850 to November 2013. It is possible for any one of these cells to be coded as "missing". This detail is important because HadCRUT4 is the basis for a paper suggesting the pause in global warming is structurally inconsistent with climate models. That paper will be discussed later.
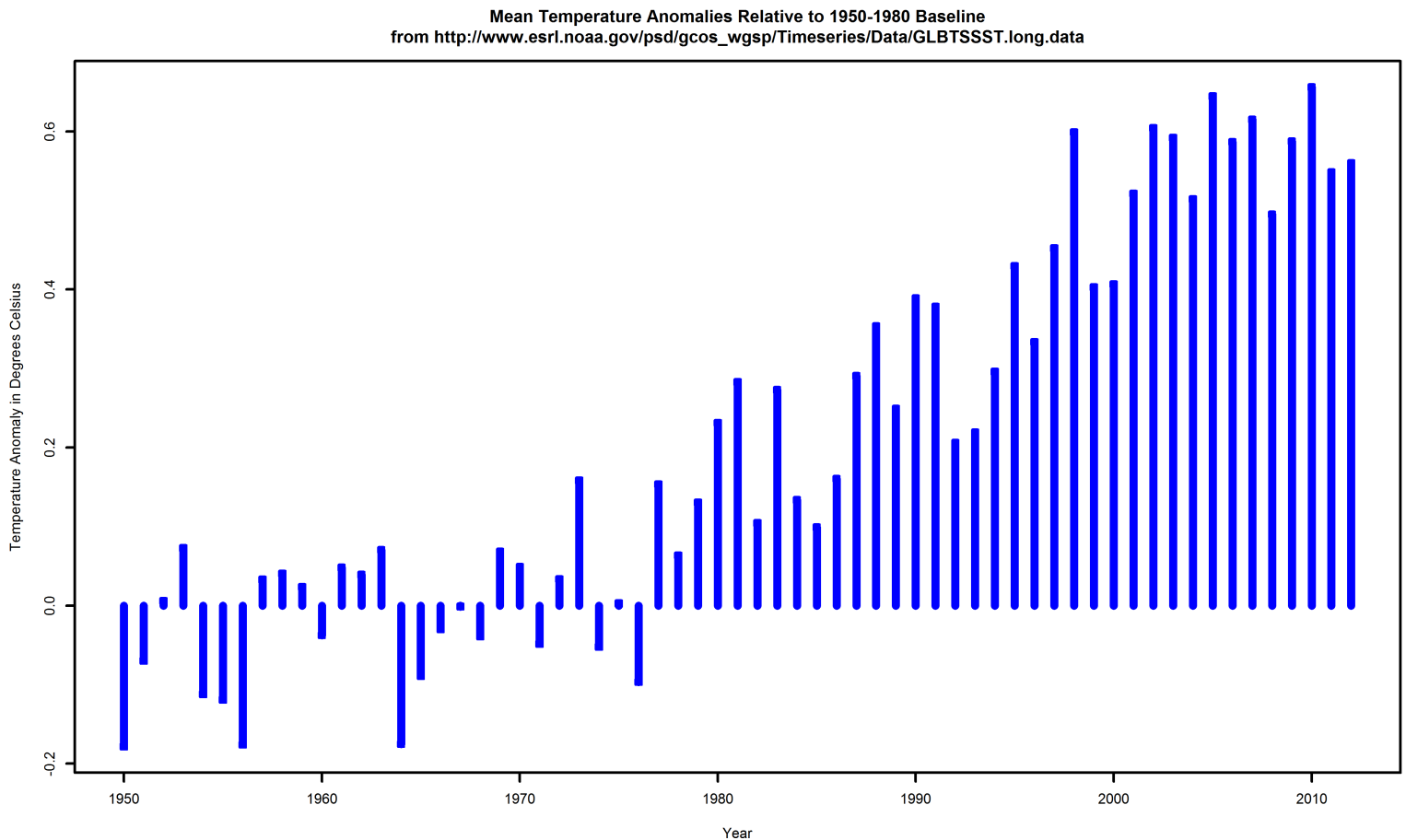
Mean Temperature Anomalies Relative to 1950-1980 Baseline
from http://www.esrl.noaa.gov/psd/gcos_wgsp/Timeseries/Data/GLBTSSST.long.data

Figure 1.5: Global surface temperature anomalies relative to a 1950-1980 baseline.

3   Rumors of Pause

Figure 1.5 shows the global mean surface temperature *anomalies* relative to a standard baseline, 1950-1980. Before going on, consider that figure. Study it. What can you see in it?

Figure 1.6 shows the same graph, but now with two trendlines obtained by applying a *smoothing spline*, one smoothing more than another. One of the two indicates an uninterrupted uptrend. The other shows a peak and a downtrend, along with wiggles around the other trendline. Note the smoothing algorithm is the same in both cases, differing only in the setting of a smoothing parameter[1]. Which is correct? What is "correct"?

Figure 1.1 shows trend curves for ocean heat content over roughly the same period. Figure 1.7 shows a time series of anomalies for Moscow, in Russia. Do these all show the same trends? These are difficult questions, but the changes seen in Figure 1.6 could be evidence of a warming "hiatus" [54, 32]. (The term *hiatus* has a formal meaning in climate science, as described by the IPCC itself [44, Box TS.3].) Note that, given Figure 1.6 the most which can be said about it is that there is a reduction in the rate of

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[1]Called "SPAR".

Mean Temperature Anomalies Relative to 1950-1980 Baseline
from http://www.esrl.noaa.gov/psd/gcos_wgsp/Timeseries/Data/GLBTSSST.long.data
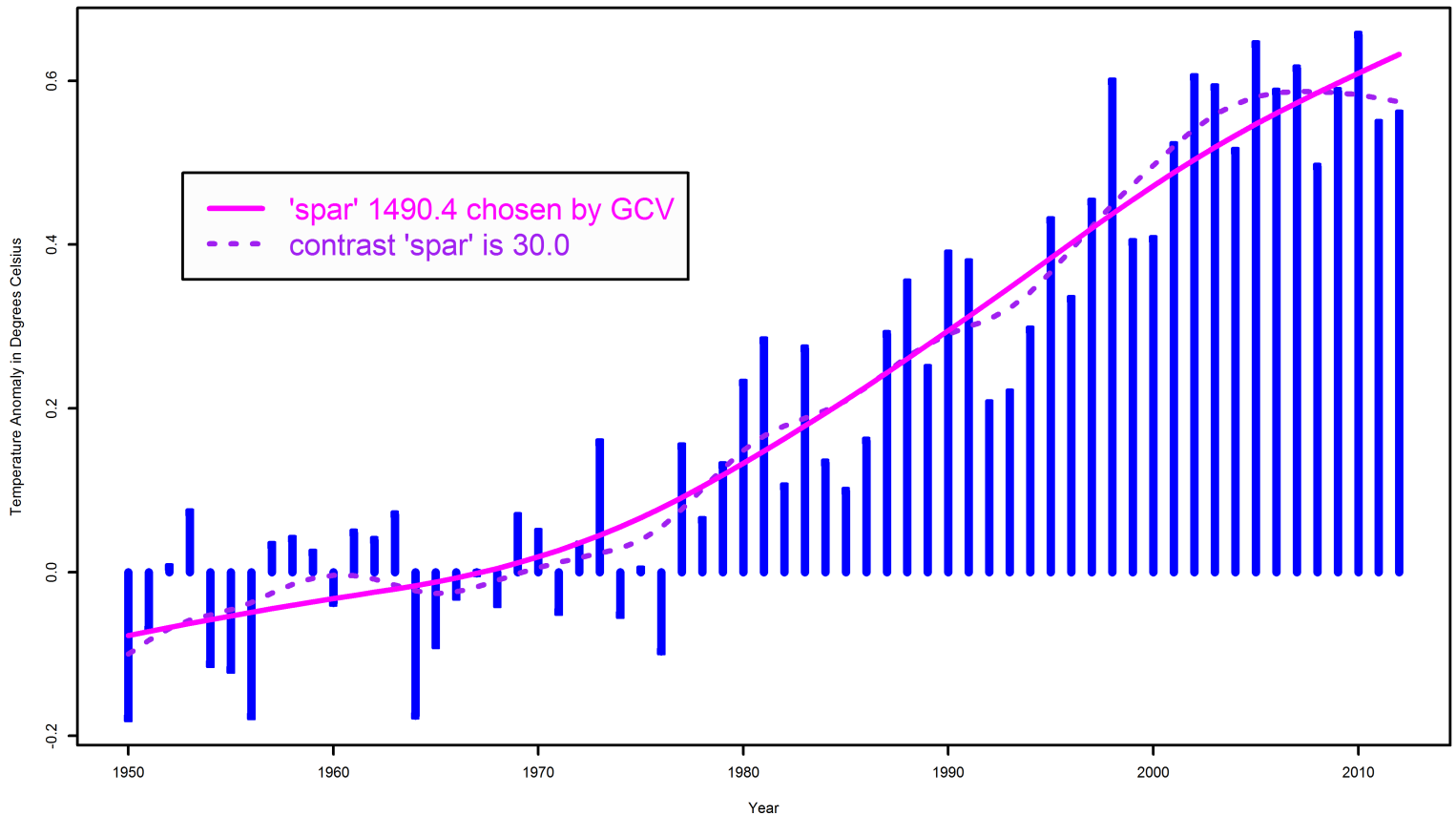

Figure 1.6: Global surface temperature anomalies relative to a 1950-1980 baseline, with two smoothing splines printed atop.

temperature increase. We'll have a more careful look at this in Section 4. With that said, people have sought reasons and assessments of how important this phenomenon is. The answers have ranged from the conclusive "Global warming has stopped" to "Perhaps the slowdown is due to 'natural variability'", to "Perhaps it's *all* due to 'natural variability'" to "There is no statistically significant change". Let's see what some of the perspectives are.

It is hard to find a scientific paper which advances the proposal that climate might be or might have been cooling in recent history. The earliest I can find are repeated presentations by a single geologist in the proceedings of the Geological Society of America, a conference which, like many, gives papers limited peer review [55, 55, 56, 57, 58, 59, 60, 61]. It is difficult to comment on this work since their full methods are not available for review. The content of the abstracts appear to ignore the possibility of lagged response in any physical system.

These claims were summarized by Easterling and Wehner in 2009, attributing claims of a "pause" to cherry-picking of sections of the temperature time series, such as 1998-2008, and what might be called *media amplification* [54]. Further, technical inconsistencies within the scientific enterprise, perfectly normal in its deployment and management of new methods of measurement, have been highlighted and abused to parlay claims of global cooling [62, 63, 64]. Based upon subsequent papers, climate science seemed to not only need to explain such variability is to be expected, but to provide a specific explanation

**Mean Temperature Anomalies in Moscow
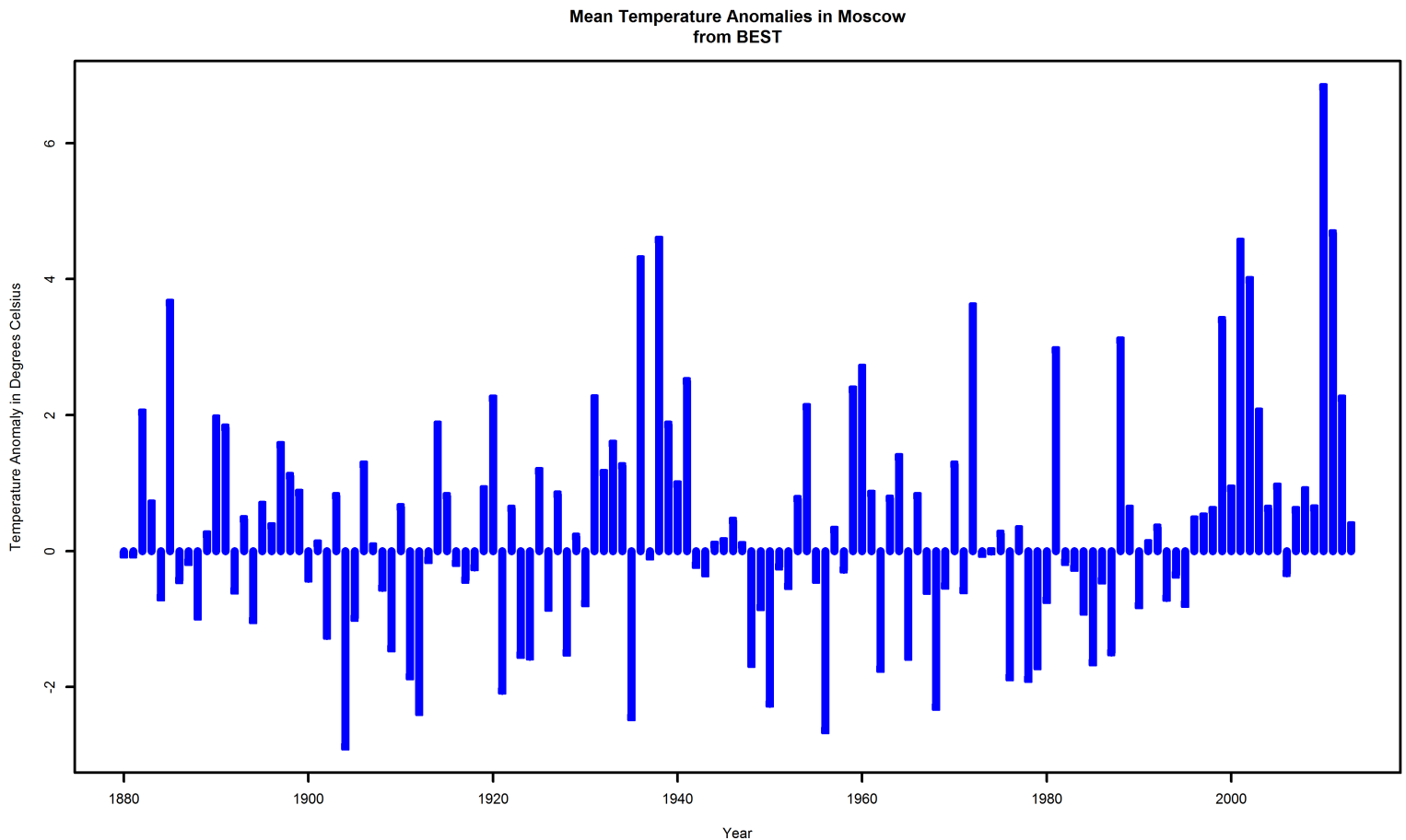from BEST**

Figure 1.7: Temperature anomalies for Moscow, Russia.

for what could be seen as a recent moderation in the abrupt warming of the mid-late 1990s, appealing to oceanic capture, as described in Section 2 [51, 36, 32]. The reader should note that, given the overall temperature anomaly series, such as Figure 1.6, and specific series, such as the one for Moscow in Figure 1.7, moderation in warming is not definitive. It is a statistical question, and, pretending for the moment we knew nothing of geophysics, a difficult one.

But there certainly is not any problem with accounting for the Earth's energy budget overall, even if one grants distribution of distribution cannot be specifically explained [42, 43, 2]. That should really not be a surprise, since, for example, the equipartition theorem of physics fails to apply to a system which has not achieved thermal equilibrium.

In my opinion, an interesting discrepancy is presented in a pair of papers in 2013 and 2014. The first, by Fyfe, Gillet, and Zwiers, has the (somewhat provocative) title "Overestimated global warming over the past 20 years" [21, 35]. It has been followed by another paper by Fyfe and Gillet applying the same methods to argue that even with the Pacific surface temperature anomalies and accommodating the coverage bias in the HadCRUT4 dataset there are discrepancies between the surface temperature record and climate model ensemble runs [65, 20, 33]. How this pair of papers presents that challenge and its possible significance is a story unto itself.
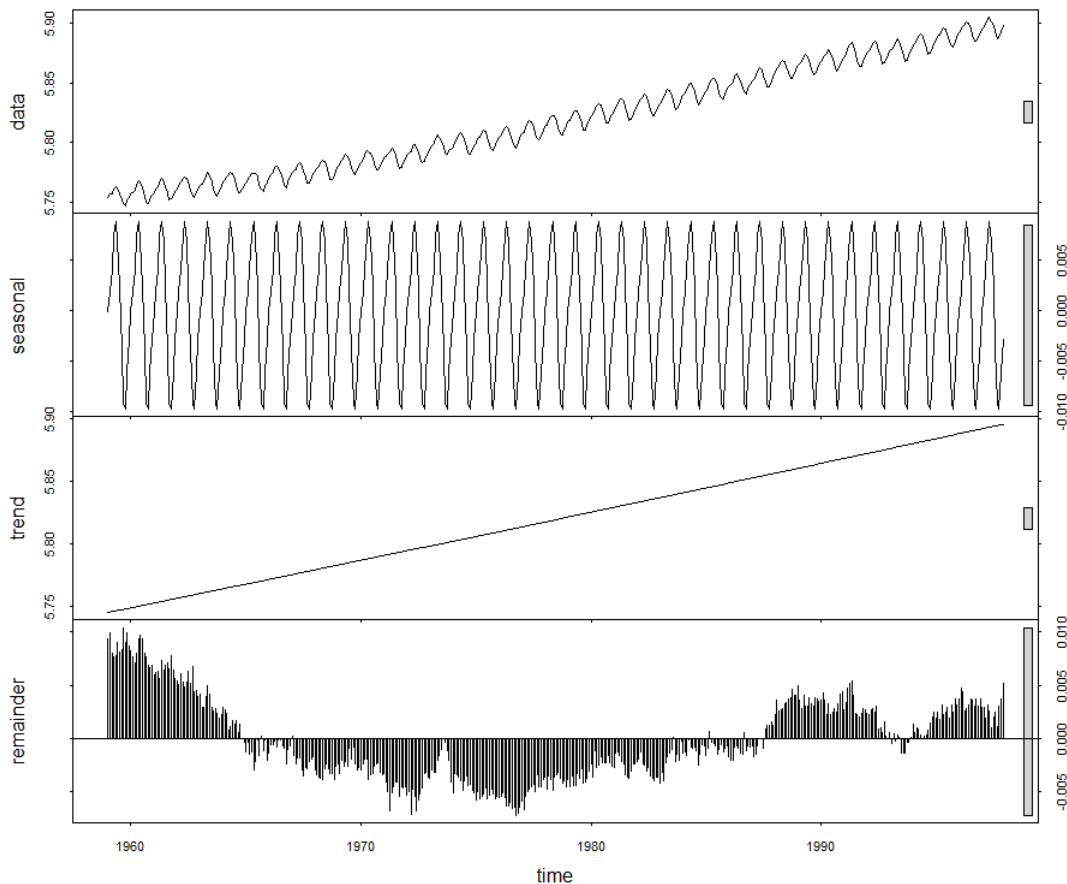
Figure 1.8: Keeling CO_2 concentration curve at Mauna Loa, Hawaii, showing original data and its decomposition into three parts, a sinusoidal annual variation, a linear trend, and a stochastic residual.

4   Trends Are Tricky

Trends as a concept are easy. But trends as objective measures are slippery. Consider the Keeling Curve, the record of atmospheric carbon dioxide concentration first begun by Charles Keeling in the 1950s and continued in the face of great obstacles [66]. This curve is reproduced in Figure 1.8, and there presented in its original, and then decomposed into three parts, an annual sinusoidal variation, a linear trend, and a stochastic remainder. The question is, which component represents the true trend, long term or otherwise? Are linear trends superior to all others? The importance of a trend is tied up with to what use it will be put. A pair of trends, like the sinusoidal and the random residual of the Keeling, might be more important for predicting its short term movements. On the other hand, explicating the long term behavior of the system being measured might feature the large scale linear trend, with the seasonal trend and random variations being but distractions.

Consider the global surface temperature anomalies of Figure 1.5 again. What are some ways of determining trends? Apart from a single long term trend, such as Figure 1.9, local linear trends can be estimated, depicted in Figure 1.11. These can be averaged, if you like, to obtain an overall trend. There is a question

Mean Temperature Anomalies Relative to 1950-1980 Baseline
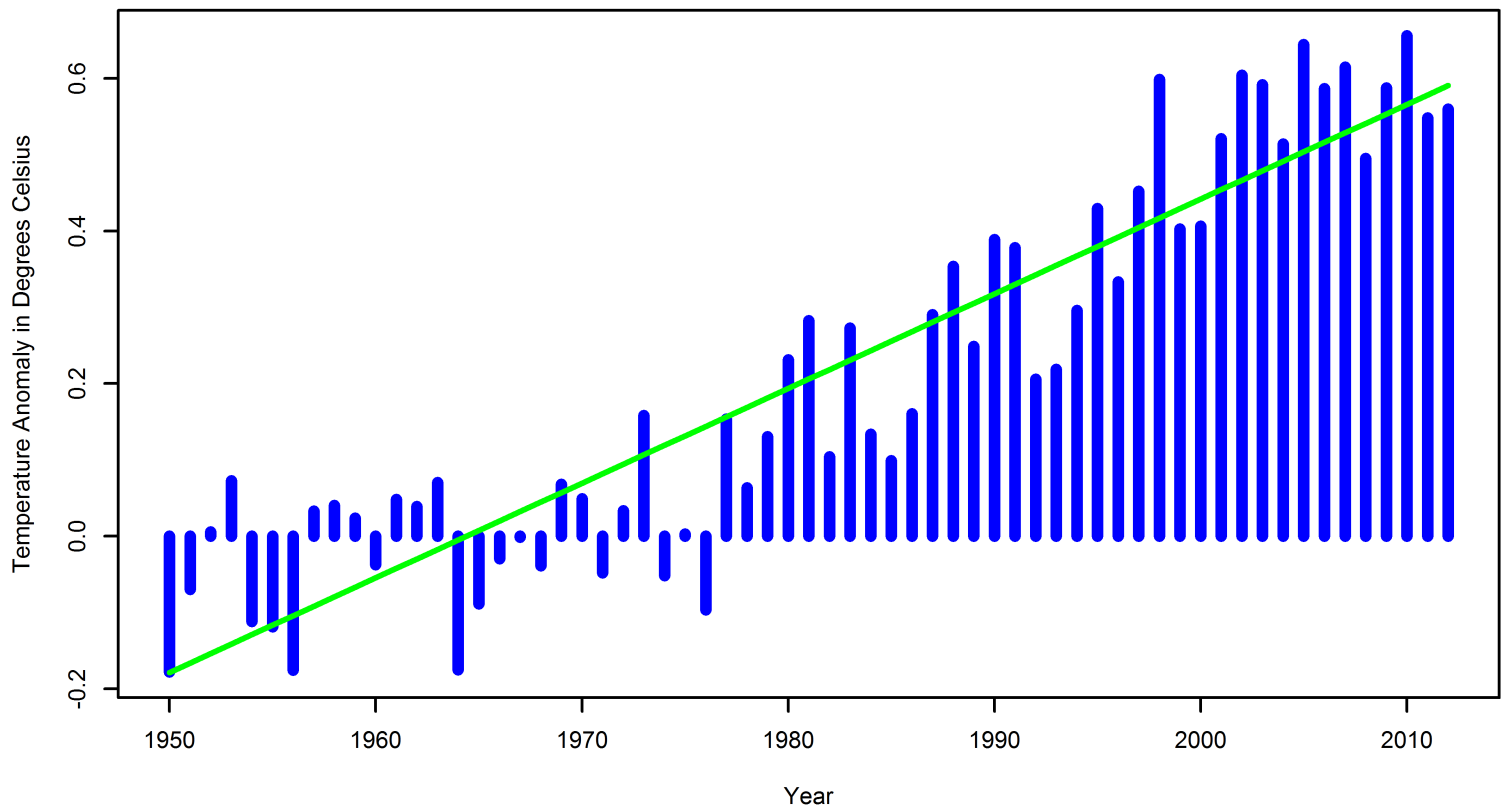## with overall line fit

Figure 1.9: Global surface temperature anomalies relative to a 1950-1980 baseline, with long term linear trend atop.

of what to do if local intervals for fitting the little lines overlap, since these are not independent of one another. There are a number of statistical devices for making them independent. One way is to do clever kinds of random sampling from a population of linear trends. Another way is to shrink the intervals until they are infinitesimally small, and, so, necessarily independent. That's just the point slope of a curve going through the data, or its *first derivative*. Numerical methods exist of estimating these, one involving a *smoothing spline* and estimating the derivative(s) of *that* [67]. Such an estimate is shown in Figure 1.12 where the instantaneous slope is plotted atop the data of Figure 1.5. The spline is a cubic spline and the smoothing parameter is determined by *generalized cross-validation* [68]. (The smoothing parameters specifies the weight of the penalty term for the second derivative of curvature.)

What else might we do? We could go after a *really good* approximation to the data of Figure 1.5. One possibility is to use the Bayesian Rauch-Tung-Striebel ("RTS") smoother to get a good approximation for the underlying curve and estimate the derivatives of *that* [69]. There is a problem in that the *prior variances* of the signal need to be estimated. The larger the ratio of the estimate of the *observations variance* to the estimate of the *process variance* is, the smoother the RTS solution. (Here, the process variance was taken here to be $\frac{1}{300}$ of the observations variance.) The RTS smoother result for two variance values of 0.156 and high 0.0.312 is shown in Figure 1.13. These are 4 and 8 times the decorrelated variance value for the series of 0.039, estimated separately.

**Mean Temperature Anomalies Relative to 1950-1980 Baseline
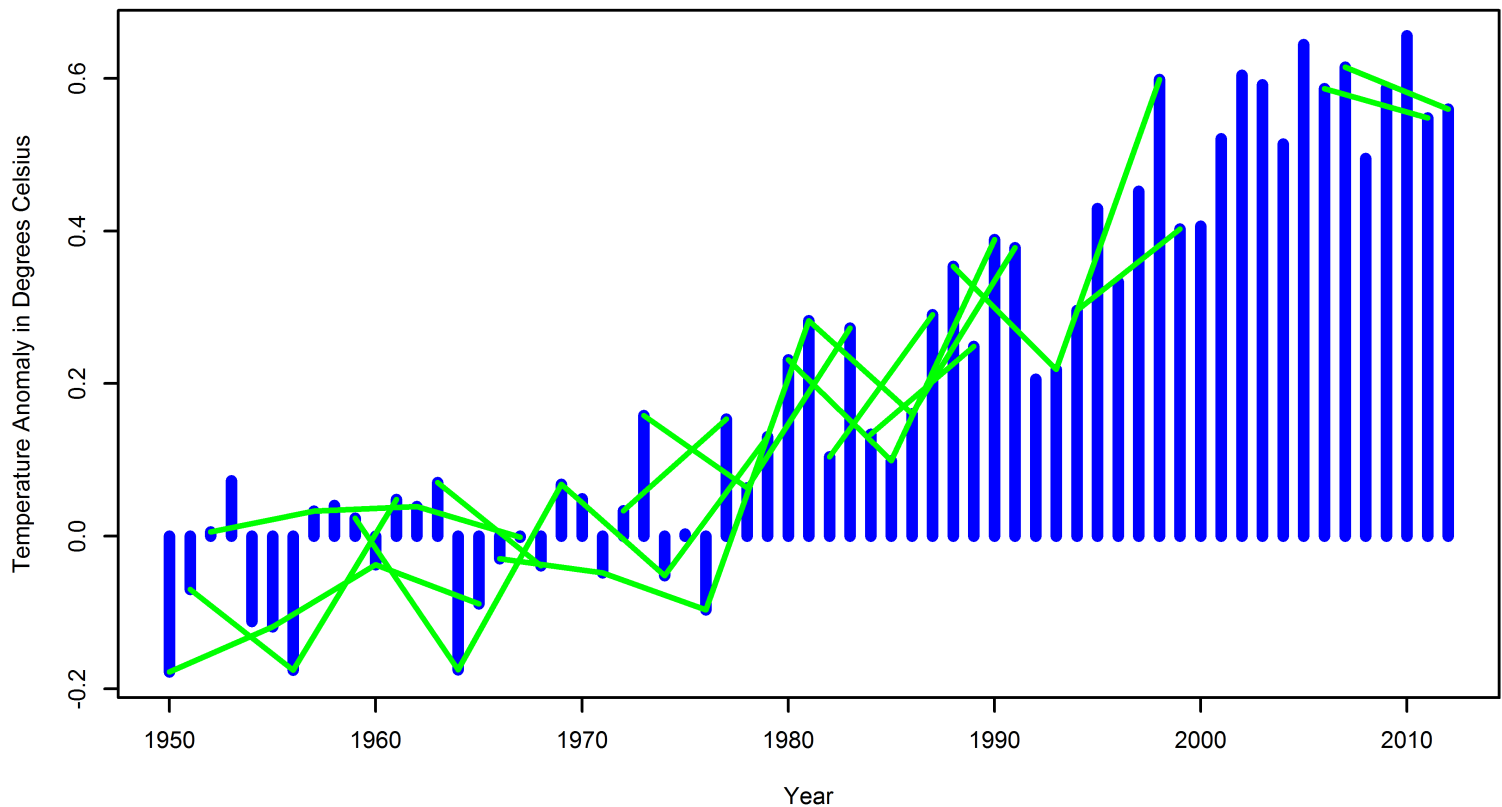with line fits from point to 5 years previous**

Figure 1.10: Global surface temperature anomalies relative to a 1950-1980 baseline, with randomly placed trends from local linear having 5 year support atop.

Combining all six methods of estimating trends results in Figure 1.14 which shows the overprinted densities of slopes. To match each of the methods by statistical weight, since, for instance, the spline and RTS methods produce many more estimates of trends than do the local linear fits, 30 randomly selected trends were chosen from each of the kinds of trends, and these are plotted. Note the spread of possibilities given by the local linear fits. Such fits to HadCRUT4 time series were used by Fyfe, Gillet, and Zwiers in their 2013 paper [21, 35].
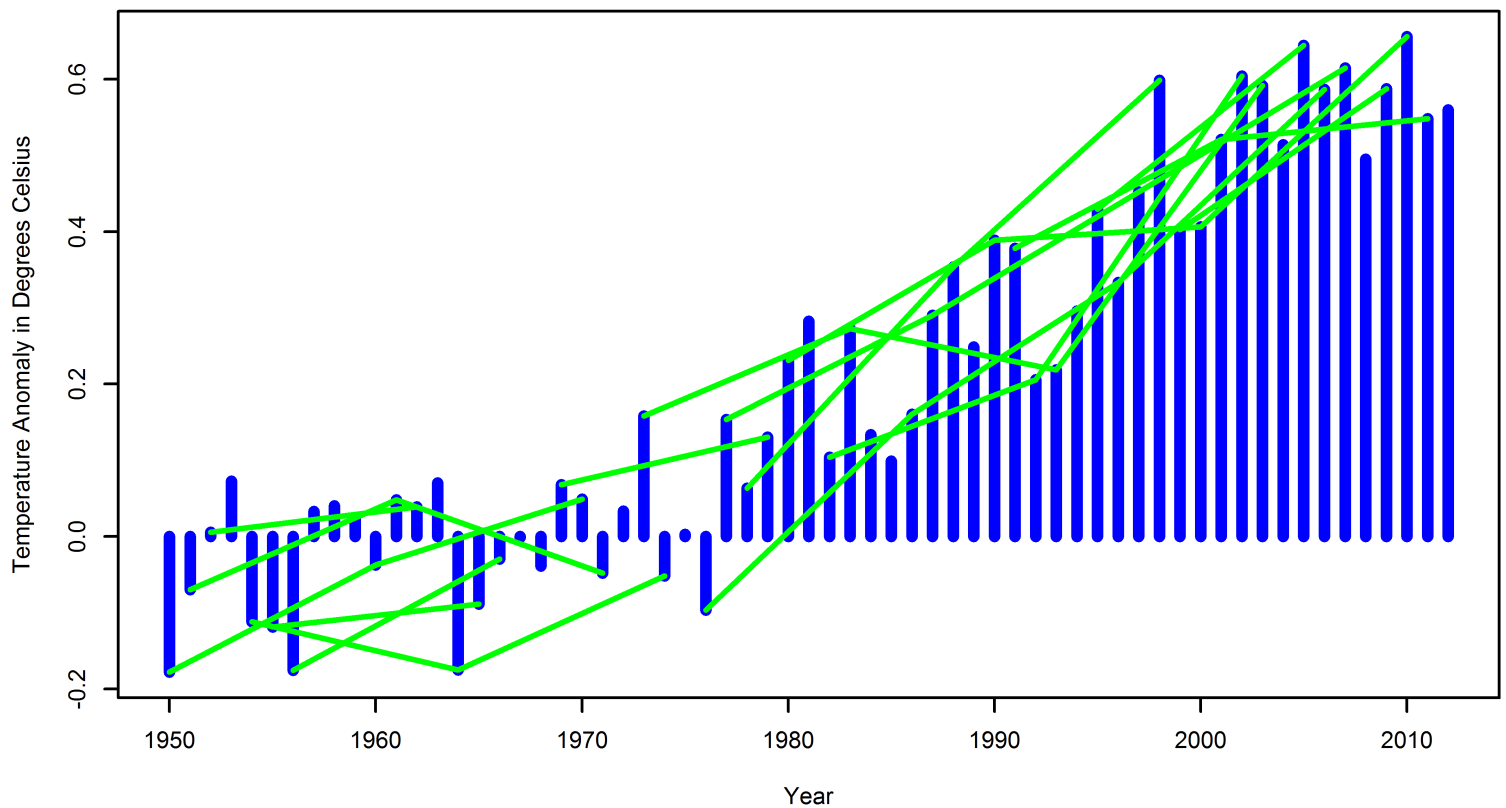
Figure 1.11: Global surface temperature anomalies relative to a 1950-1980 baseline, with randomly placed trends from local linear having 10 year support atop.

**Mean Temperature Anomalies Relative to 1950-1980 Baseline
using a smoothing spline to estimate point derivatives**
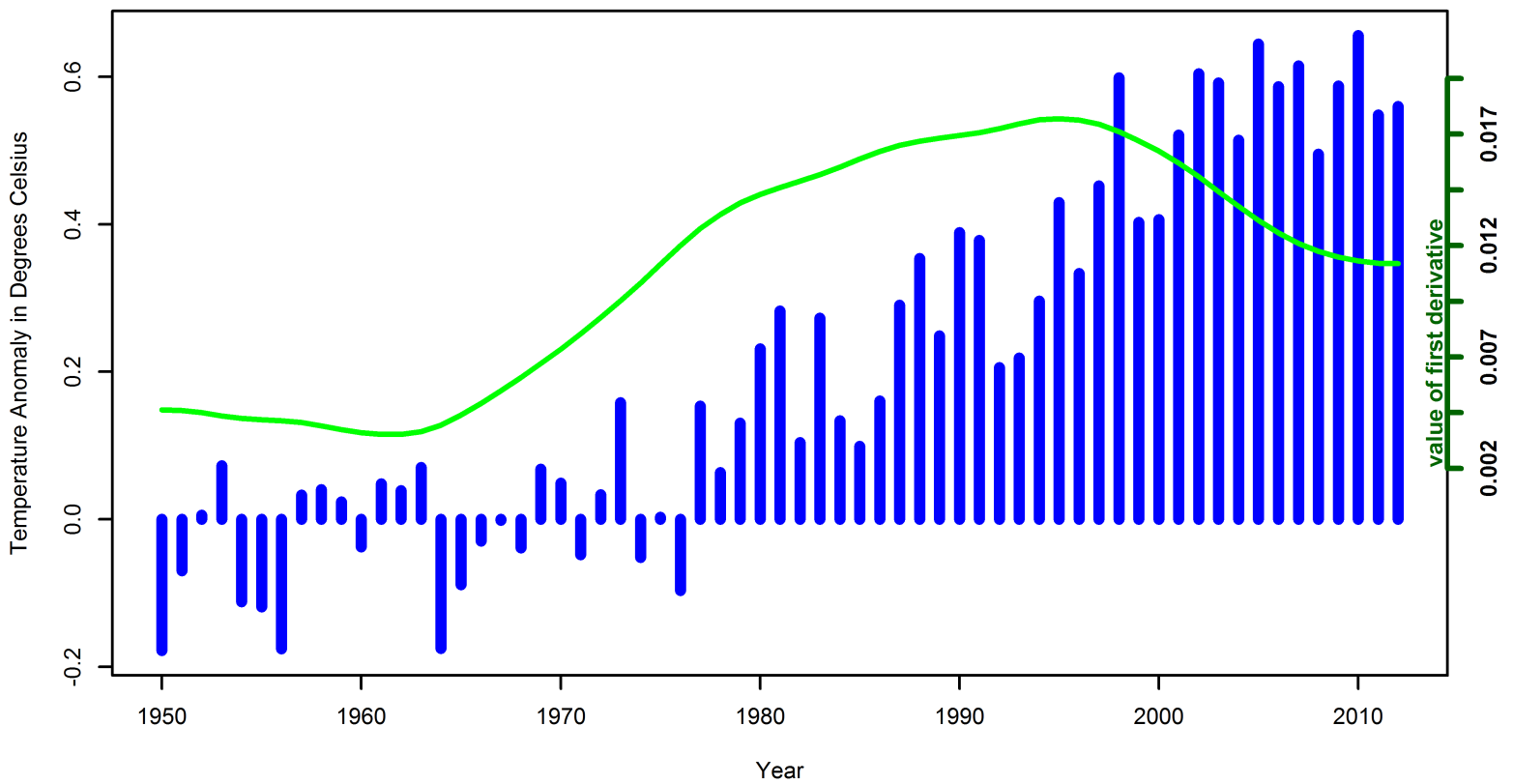

Figure 1.12: Global surface temperature anomalies relative to a 1950-1980 baseline, with instaneous numerical estimates of derivatives atop. Support for the smoothing spline used to calculate the derivatives is obtained using *generalized cross validation* [67]. Note how the value of the first derivative *never drops below zero* although its *magnitude* decreases as time approaches 2012.

Figure 1.13: Global surface temperature anomalies relative to a 1950-1980 baseline, with fits using the Rauch-Tung-Striebel smoother placed atop, in magenta and dark red. The former uses a prior variance of 4 times that of the Figure 1.5 data corrected for serial correlation. The latter uses a prior variance of 4 times that. The instaneous numerical estimates derived from the two solutions are shown in green and blue, respectively. Note the two solutions are essentially identical.

**Trend densities by 6 methods from
Mean Temperature Anomalies Relative to 1950-1980 Baseline**

N = 30   Bandwidth = 0.0009479
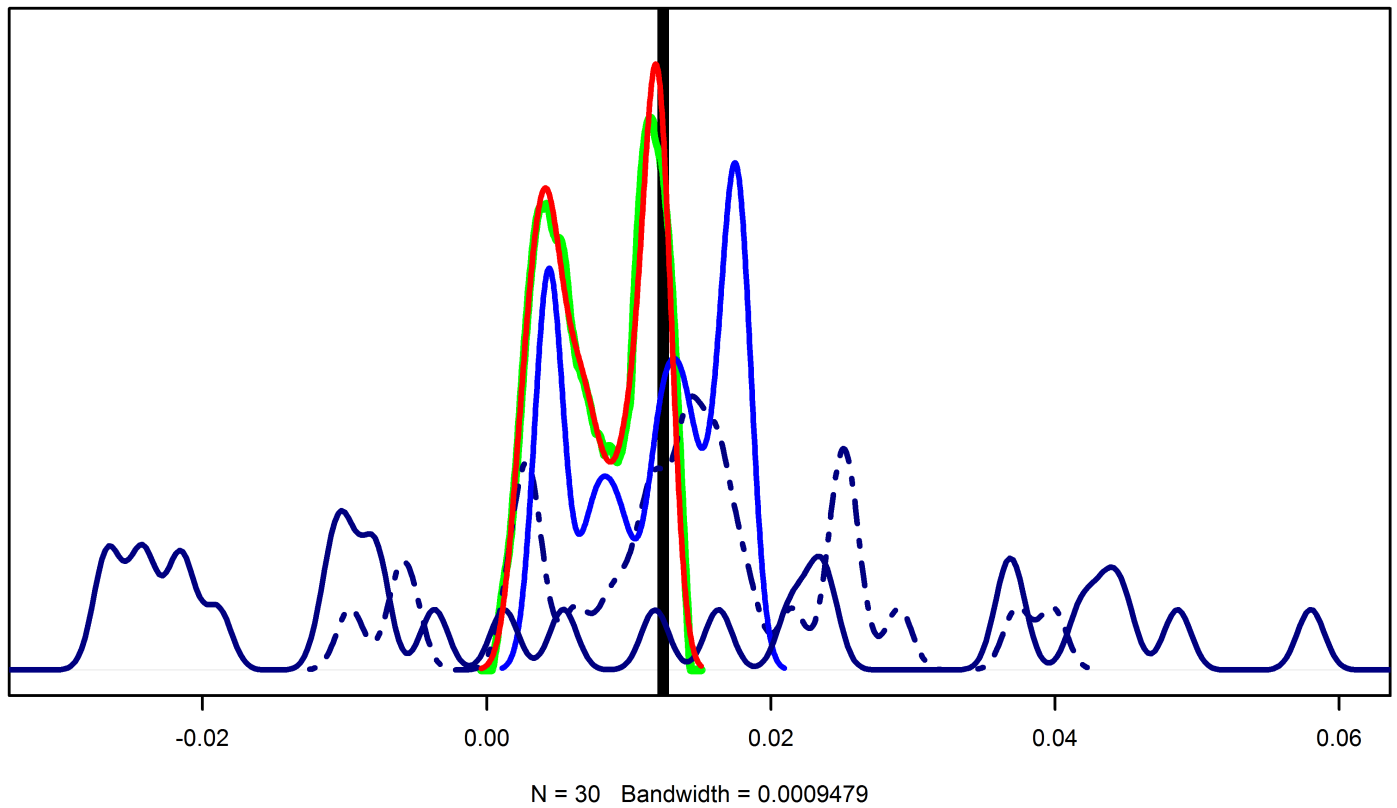
Figure 1.14: Composite of trends estimated using the long term linear fit (the vertical black line), the local linear fits with 5 years separation (navy blue trace), the local linear fits with 10 years separation (dashed navy blue trace), the smoothing spline (blue trace), the RTS smoother with variance 4 times the corrected estimate for the data as the prior variance (green trace), and the RTS smoother with eight times the corrected estimate for the data (red trace). 30 randomly chosen (without replacement) trends have densities estimated to construct this chart.

5   Internal Decadal Variability

The recent IPCC AR5 WG1 Report sets out the context [44, Box TS.3]:

Hiatus periods of 10 to 15 years can arise as a manifestation of internal decadal climate variability, which sometimes enhances and sometimes counteracts the long-term externally forced trend. Internal variability thus diminishes the relevance of trends over periods as short as 10 to 15 years for long-term climate change (Box 2.2, Section 2.4.3). Furthermore, the timing of internal decadal climate variability is not expected to be matched by the CMIP5 historical simulations, owing to the predictability horizon of at most 10 to 20 years (Section 11.2.2; CMIP5 historical simulations are typically started around nominally 1850 from a control run). However, climate models exhibit individual decades of GMST trend hiatus even during a prolonged phase of energy uptake of the climate system (e.g., Figure 9.8; Easterling and Wehner, 2009; Knight et al., 2009), in which case the energy budget would be balanced by increasing subsurface-ocean heat uptake (Meehl et al., 2011, 2013a; Guemas et al., 2013).

Owing to sampling limitations, it is uncertain whether an increase in the rate of subsurface-ocean heat uptake occurred during the past 15 years (Section 3.2.4). However, it is very likely[2] that the climate system, including the ocean below 700 m depth, has continued to accumulate energy over the period 1998-2010 (Section 3.2.4, Box 3.1). Consistent with this energy accumulation, global mean sea level has continued to rise during 1998-2012, at a rate only slightly and insignificantly lower than during 1993-2012 (Section 3.7). The consistency between observed heat-content and sea level changes yields high confidence in the assessment of continued ocean energy accumulation, which is in turn consistent with the positive radiative imbalance of the climate system (Section 8.5.1; Section 13.3, Box 13.1). By contrast, there is limited evidence that the hiatus in GMST trend has been accompanied by a slower rate of increase in ocean heat content over the depth range 0 to 700 m, when comparing the period 2003-2010 against 1971-2010. There is low agreement on this slowdown, since three of five analyses show a slowdown in the rate of increase while the other two show the increase continuing unabated (Section 3.2.3, Figure 3.2).

During the 15-year period beginning in 1998, the ensemble of HadCRUT4 GMST trends lies below almost all model-simulated trends (Box 9.2 Figure 1a), whereas during the 15-year period ending in 1998, it lies above 93 out of 114 modelled trends (Box 9.2 Figure 1b; HadCRUT4 ensemble-mean trend 0.26 °C per decade, CMIP5 ensemble-mean trend 0.16 °C per decade). Over the 62-year period 1951-2012, observed and CMIP5 ensemble-mean trends agree to within 0.02 °C per decade (Box 9.2 Figure 1c; CMIP5 ensemble-mean trend 0.13 °C per decade). There is hence very high confidence that the CMIP5 models show long-term

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[2]"In this Report, the following terms have been used to indicate the assessed likelihood of an outcome or a result: Virtually certain 99−100% probability, Very likely 90−100%, Likely 66−100%, About as likely as not 33−66%, Unlikely 0-33%, Very unlikely 0-10%, Exceptionally unlikely 0-1%. Additional terms (Extremely likely: 95 − 100%, More likely than not > 50 − 100%, and Extremely unlikely 0-5%) may also be used when appropriate. Assessed likelihood is typeset in italics, e.g., very likely (see Section 1.4 and Box TS.1 for more details)."

GMST trends consistent with observations, despite the disagreement over the most recent 15-year period. Due to internal climate variability, in any given 15-year period the observed GMST trend sometimes lies near one end of a model ensemble (Box 9.2, Figure 1a, b; Easterling and Wehner, 2009), an effect that is pronounced in Box 9.2, Figure 1a, because GMST was influenced by a very strong El Niño event in 1998.

The contributions of Fyfe, Gillet, and Zwiers ("FGZ") are to (a) pin down this behavior for a 20 year period using the HadCRUT4 data, and, to my mind, more importantly, (b) to develop techniques for evaluating runs of ensembles of climate models like the CMIP5 suite without commissioning specfic runs for the purpose [21, 33]. This, if it were to prove out, would be an important experimental advance, since climate models demand expensive and extensive hardware, and the number of people who know how to program and run them is very limited, possibly a more limiting practical constraint than the hardware [70]. ("""It's great there's a new initiative," says modeler Inez Fung of DOE's Lawrence Berkeley National Laboratory and the University of California, Berkeley. "But all the modeling efforts are very short-handed. More brains working on one set of code would be better than working separately"" [70].) FGZ try to explicitly model trends due to internal variability [35]. They begin with two equations:

$$(1.1) \qquad M_{ij}(t) = u^m(t) + \text{Eint}_{ij}(t) + \text{Emod}_i(t), i = 1, \ldots, N^m, j = 1, \ldots, N_i$$

$$(1.2) \qquad O_k(t) = u^o(t) + \text{Eint}^o(t) + \text{Esamp}_k(t), k = 1, \ldots, N^o$$

with time explicitly indicated, unlike FGZ [35, page 2]. i is the model membership index. j is the index of the ith model's jth ensemble. Here, $M_{ij}(t)$ and $O_k(t)$ are trends calculated using models or observations, respectively. $u^m(t)$ and $u^o(t)$ denote the "true, unknown, deterministic trends due to external forcing" common to models and observations, respectively [35]. $\text{Eint}_{ij}(t)$ and $\text{Eint}^o(t)$ are the perturbations to trends due to internal variability of models and observations. $\text{Emod}_i(t)$ denotes error in climate model trends for model i. $\text{Esamp}_k(t)$ denotes the sampling error in the kth sample. Notably FGZ assume $\text{Emod}_i(t)$ are exchangeable with each other as well, at least for the same time t. Note that while the internal variability of climate models $\text{Eint}_{ij}(t)$ varies from model-to-model, run-to-run, and time-to-time, the 'internal variability of observations', namely $\text{Eint}^o(t)$, is assumed to only vary with time.

The technical innovation FGZ use is to employ *bootstrap resampling* on the observations ensemble of HadCRUT4 and an ensemble of runs of 38 CMIP5 climate models to perform what is essentially a *two-sample comparison* [71, 72]. In doing so, they explicitly assume, in the framework above, *exchangeability of models*. (*Exchangeability* is a weaker assumption than *independence*. Random variables are *exchangeable* if their joint distribution only depends upon the set of variables, and not their order [73, 74, 75]. Note the caution in Coolen [76].) k runs over the bootstrap samples taken from HadCRUT4 observations.

So, what *is* a bootstrap? In its simplest form, a bootstrap is a nonparametric, often robust, frequentist technique for sampling the distribution of a function of a set of population parameters, generally irrespective of the nature or complexity of that function, or the number of parameters. Since estimates of the variance of that function are themselves functions of population parameters, assuming the variance exists, the bootstrap can also be used to estimate the precision of the first set of samples, where "precision"

is the reciprocal of variance. In the case in question here, with FGZ, the bootstrap is being used to determine if the distribution of surface temperature trends as calculated from observations and the distribution of surface temperature trends as calculated from climate models for the same period have in fact similar means. This is done by examining *differences of paired trends*, one coming from an observation sample, one coming from a model sample, and assessing the degree of discrepancy based upon the variances of the observations trends distribution and of the models trends distribution.

The equations (1.1) and (1.2) can be re-written:

$$(1.3) \qquad M_{ij}(t) - \text{Eint}_{ij}(t) = u^m(t) + \text{Emod}_i(t), i = 1, \ldots, N^m, j = 1, \ldots, N_i$$

$$(1.4) \qquad O_k(t) - \text{Eint}^o(t) = u^o(t) + \text{Esamp}_k(t), k = 1, \ldots, N^o$$

moving the trends in internal variability to the left, calculated side. Both $\text{Eint}_{ij}(t)$ and $\text{Eint}^o(t)$ are *not* directly observable. Without some additional assumptions, which are not explicitly given in the FGZ paper, such as

$$(1.5) \qquad \text{Eint}_{ij}(t) \sim \mathcal{N}(0, \Sigma_{\text{model int}})$$

$$(1.6) \qquad \text{Eint}^o(t) \sim \mathcal{N}(0, \Sigma_{\text{obs int}})$$

we can't really be sure we're seeing $O_k(t)$ or $O_k(t) - \text{Eint}^o(t)$, or at least $O_k(t)$ less the mean of $\text{Eint}^o(t)$. The same applies to $M_{ij}(t)$ and $\text{Eint}_{ij}(t)$. Here $\Sigma_{\text{model int}}$ and $\Sigma_{\text{obs int}}$ are covariances among models and among observations. FGZ essentially say these are diagonal with their statement "An implicit assumption is that sampling uncertainty in [observation trends] is independent of uncertainty due to internal variability and also independent of uncertainty in [model trends]" [35]. They might not be so, but it is reasonable to suppose their diagonals are strong, and that there is a row-column exchange operator on these covariances which can produce banded matrices.

6   On Reconciliation

The centerpiece of the FGZ result is their Figure 1, reproduced here as Figure 1.15. Their conclusion, that climate models do not properly capture surface temperature observations for the given periods, is based upon the significant separate of the red density from the grey density, even measuring that separation using pooled variances. But, surely, a remarkable feature of these graphs is not only the separation of the means of the two densities, but the marked difference between the variance of the two densities. Why are climate models so less precise than HadCRUT4 observations? Conceivably, why do climate models
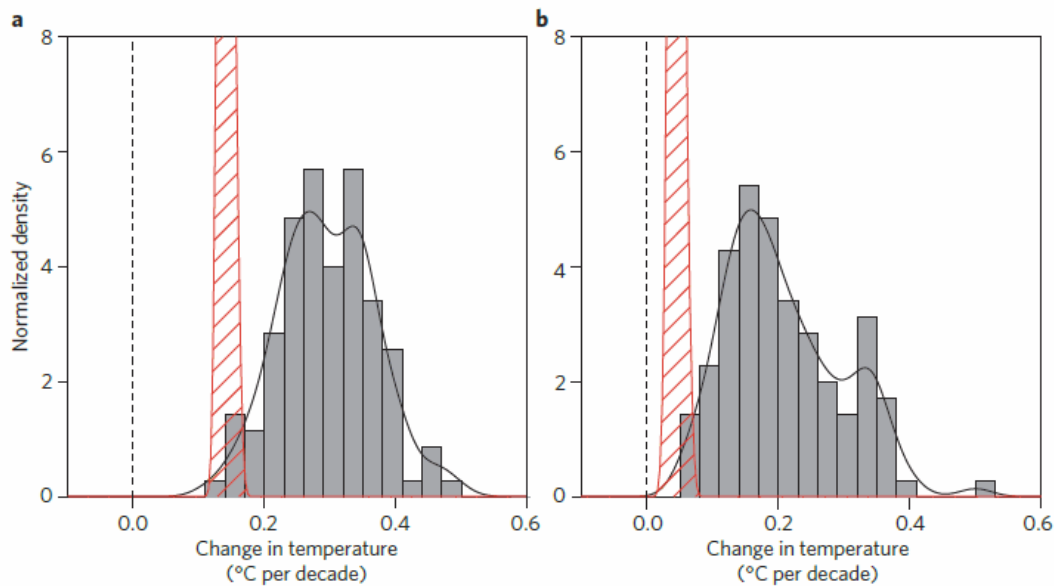
Figure 1 | Trends in global mean surface temperature. **a**, 1993–2012. **b**, 1998–2012. Histograms of observed trends (red hatching) are from 100 reconstructions of the HadCRUT4 dataset[1]. Histograms of model trends (grey bars) are based on 117 simulations of the models, and black curves are smoothed versions of the model trends. The ranges of observed trends reflect observational uncertainty, whereas the ranges of model trends reflect forcing uncertainty, as well as differences in individual model responses to external forcings and uncertainty arising from internal climate variability.

Figure 1.15: Figure 1 from Fyfe, Gillet, Zwiers [21].

not agree with one another so dramatically? We cannot tell without getting into CMIP5 details, but the same result could be obtained if the climate models came in three Gaussian populations, each with a variance 1.5x that of the observations, but mixed together. We could also obtain the same result if, for some reason, the variance of the HadCRUT4 was markedly understated.

That brings us back to the comments about HadCRUT4 made at the end of Section 2. HadCRUT4 is noted for "drop outs" in observations, where either the quality of an observation on a patch of Earth was poor or the observation was missing altogether for a certain month in history. It also has incomplete coverage [20]. Whether or not values for patches are *imputed* in some way, perhaps using *kriging*, or supports to calculate trends are adjusted to avoid these holes, is an important question. As seen in Section 4, what trends you get depends a lot on how they are done. FGZ did linear trends. These

are nice because means of trends have simple relationships with the trends themselves. On the other hand, confining trend estimation to local linear trends binds these estimates to being only supported by pairs of actual samples, however sparse they may be. Furthermore, the *sampling density* of such a trend estimator is equivalent to convolving with a box or *moving average* filter (also known as a *FIR filter*) with broadly spaced taps. The connection between Fourier representations and sampling densities is available through the density's *characteristic function* representation. Figure 1.14 can be interpreted as the result of convolving the RTS density with a *sinc function*. This has the unfortunate effect of producing a broadly spaced set of trends which, when averaged, appear to be a single, tight distribution, close to the vertical black line of Figure 1.14, and erasing all the detail available by estimating the density of trends with a robust function of the time series' first derivative. FGZ could be improved by using such, repairing this drawback and also making it more robust against HadCRUT4's inescapable data drops.

If Earth's climate is thought of as a *dynamical system*, and taking note of the suggestion of Kharin that "There is basically one observational record in climate research", we can do the following thought experiment [28]. Suppose the total state of the Earth's climate system can be captured at one moment in time, no matter how, and the climate can be reinitialized to that state at our whim, again no matter how. What happens if this is done several times, and then the climate is permitted to develop for, say, exactly 100 years on each "run"? What are the resulting states? Suppose the dynamical "inputs" from the Sun, as a function of time, are held identical during that 100 years, as are dynamical inputs from volcanic forcings, as are human emissions of greenhouse gases. Are the resulting states copies of one another? No. Stochastic variability in the operation of climate means these end states will be each somewhat different than one another. Then of what use is the "one observation record"? Well, it is arguably better than no observational record.

Setting aside the problems of using local linear trends, FGZ's bootstrap approach to the HadCRUT4 ensemble is an attempt to imitate these various runs of Earth's climate. The trouble is, the frequentist bootstrap can only replicate values of observations actually seen, in this case, those of the HadCRUT4 ensembles. It will never produce values in-between and, as the parameters of temperature anomalies are in general continuous measures, that seems a reasonable thing to do. It may be possible to remedy this using a different kind of bootstrap, such as a *Bayesian bootstrap*, but I think there's another way [72, Section 10.5]. . Suppose the $M_{ij}(t)$ are used to construct, for each time t, an *average model*, say, $\mathcal{M}(t)$ [77]. That construction also yields a time-varying variance, $\text{var}[\mathcal{M}](t)$ of this average model. And, while it can be done without the Gaussian assumption, suppose for example, deviations from that model are treated as Gaussian, so particular climate variable, like surface temperature, $\tau(t)$, abides a Gaussian density, per the usual:

$$(1.7) \qquad \mathbf{n}(t, \tau(t)) = \frac{1}{\sqrt{2\pi \, \text{var}[\mathcal{M}](t)}} \exp\left(-\frac{(\tau(t) - \mathcal{M}(t))^2}{2 \, \text{var}[\mathcal{M}](t)}\right)$$

Such an expression can be interpreted as a *likelihood function* of a particular $\tau(t)$ and therefore seen as the probability of having an excursion from the best known model average of $\tau(t) - \mathcal{M}(t)$. (It is possible to develop an *empirical likelihood function* as well. See Owen [78].) Such a reformulation would change

the FGZ title of "Overestimated global warming over the past 20 years" to something like *Warming over the past 20 years is unusual*, which, by what's known of the science, seems more consistent.

More work needs to be done to assess the proper virtues of the FGZ technique. By rights, while climate models and observations might have different mean values in their estimates of variability for any time interval in the record, the widths of their distributions should broadly overlap. In my opinion, it is less the difference of their means that is interesting than the remarkably narrow distribution attributed to HadCRUT4 after processing. A device like that Rohde used to compare BEST temperature observations with HadCRUT4 and GISS, one of supplying the FGZ procedure with synthetic data, would be perhaps the most informative regarding its character [41]. It would be worthwhile.

And regarding climate models, assessing *parametric uncertainty* hand-in-hand with the model builders seems to be a sensible route [79]. If the FGZ technique, or any other, can contribute to this process, it is most welcome. Lee reports how the GLOMAP model of aerosols was systematically improved using such careful statistical consideration [80]. It seems likely to be a more rewarding way than "black box" treatments. Incidently, Dr Lindsay Lee's article was runner-up in the *Significance*/Young Statisticians Section writers' competition. It's great to see bright young minds charging in to solve these problems!

7 Summary

Various geophysical datasets recording global surface temperature anomalies suggest a slowdown in anomalous global warming from historical baselines. Warming is increasing, but not as fast, and much of the media attention to this is reacting to *the second time derivative of temperature*, which is negative, not the first time derivative, its rate of increase. Explanations vary. In one important respect, 20 or 30 years is an insufficiently long time to assess the state of the climate system. In another, while the global *surface* temperature increase is slowing, oceanic temperatures continue to soar, at many depths. Warming might even decrease. None of these seem to pose a challenge to the geophysics of climate, which has substantial support both from experimental science and *ab initio* calculations. An interesting discrepancy is noted by Fyfe, Gillet, and Zwiers, although their calculation could be improved both by using a more robust estimator for trends, and by trying to integrate out anomalous temperatures due to internal variability in their models, because much of it is not separately observable.

In summary, working out these details is the process of science at its best, and many discplines, not least mathematics, statistics, and signal processing, have much to contribute to the methods and interpretations of these series data. It is possible too much is being asked of a limited data set, and we have not yet observed enough of climate system response to tell anything definitive [106]. But the urgency to act responsibly given scientific predictions remains.

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