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MODELS – GOOD, AND BAD:

AND THE (MIS-)USE OF ENGINEERING IDEAS IN THEM

-by Bernie Hutchins

During a career teaching engineering, I often was impressed with the fact that the exact "topics" we were teaching were of much less relative importance than the training to think like an engineer. At some point we encounter people who suppose that engineers do their work by looking up formulas in books, plugging in numbers, and if they do the math right, they succeed, else I guess, the bridge falls down. There is more to it than that – of course. We develop an intuition about whether or not an idea in itself works – and how to fix things when they don't work. Engineers, perhaps with a uniquely valuable perspective, can look at a model, just as they would an actual device, and decide of it really ought to work, or if it might have a fatal flaw.

MODELS OF CLOSE RELATIVES - ANALOGS

At some point in the past we began talking about doing something (like building a music synthesizer) by alternative analog or digital approaches. Somewhat strangely from today's perspective, initially the consideration seemed to be whether we used logic IC's (digital, like RTL or TTL) or perhaps op-amps (analog). Later the notion of "digital" was more properly taken to involve discrete levels (quantization) and discrete time (sampling), as we find with digital music recordings as a prime example. Soon "analog" became simply that which was NOT digital. Sometimes we talked about "linear circuits" (not quantized) – although linearity, as the term was manifested by superposition, was a property of digital systems we almost always embraced. So what if analog now meant "not digital"? That's okay – look up any word in the dictionary – most will have multiple meanings.

For myself, one of the first times I encountered the idea of "analog" was in a linear systems course (all examples in the text were "analog" circuits by today's terminology). The term "analog" was in a discussion of "electrical/mechanical analogs", and these were fascinating. Here was a grid or table of networks. One side had an electrical network of capacitors, inductors, and resistors. The other side had a mechanical network of (correspondingly) springs, masses, and "dashpots" (frictional dampers). The claim was that these were somehow analogs – analogous to each other. That they should be the same was itself fascinating. (The explanation that they obeyed the same mathematical differential equations didn't really explain the situation, it just moved it.)

We learned that the electrical network of the analog pair might well be easier to build and study, and the components more ideal in a particular investigation. On the other hand, while we had no real intuition with regard to the behavior of an electrical network, we usually had a pretty good intuitive feeling for what a mechanical network would do. A mass suspended from a spring attached to our finger behaved in a way with which we could identify by pure thought – based on memories from childhood with relevant toys – or we could always do the experiment with a paperweight and rubber bands. So these electrical/mechanical analogs were very useful. As some of us became electrical engineers, and we developed the usual mathematical tools of that trade, we recognized that we could very likely back-apply these to mechanical devices. Our built-in mechanical intuition and the mathematics of electrical engineering worked together. They "modeled" each other. Neat.

This was not exact. As stated, we did not expect that even our electrical components were completely "ideal" (inductors had some resistance for example). But mechanical components were likely worse (springs were linear "Hook's law" only for small displacements). But there was always that fundamental identity of the mathematical physics to rely on. Eugene Wigner [1], famously, and Richard Hamming [2], with more practical examples, commented on "the unreasonable effectiveness of mathematics". Both are worth reading, but neither really answers the "why" question.

The association of electrical and mechanical networks as I have described is a welcome surprise. The common mathematical basis, differential equations in the same form is some comfort (for example $F=m\, dv/dt$ and $E = L\, di/dt$). How disconcerting it would be if we needed a whole new math! So the question comes up, if two things can be described by the same mathematics, does this guarantee a common physical reality? If not, is one at least a good model for the other? The answers are: ***absolutely not*** to the first question, and ***possibly*** to the second.

The case of the electrical/mechanical analogs are an example of very close relatives. We feel comfortable with either one being a model of the other. If, for example, we had a concept of a "Q" of an electrical circuit, we would without hesitation look for the same concept in the mechanical system. For some time I remember seeing, and trying to ignore, a disturbing instability in the peak response of sharp bandpass filters. Sometimes while measuring the frequency response, the response would suddenly jump discretely in amplitude. This really spoiled my neat plots! Well, if I just turned down the signal level, it went away. Possibly the large signals caused some strange kind of ringing? At least I knew how to make them go away. First make it work – then make it pretty.

Somehow, I misremember the details of my discovery of the existing phenomenon of "jump resonance", finding it to be a rather common mechanical phenomenon. I am unable to relocate the exact item I remember seeing. [I am rather certain it was a tipped-over resonance curve in a book on mechanical vibrations. But it is not the book in my physics collection where I have believed it rested undisturbed, for many years. That book I found in a close-out bin in a Woolworth department store. It still has the 99₵ price tag. The store was not known for remaindered books, let alone college-level physics books. But I can't find the figure!] Some comments on jump resonance in the electrical bandpass filter are found in Electronotes issues EN#97 and EN#195. So the point relates to the fact that jump resonance is a result of a non-linearity (slew limiting in the case of an op-amp). Because non-linearities appear more readily in mechanical systems, we benefit by looking not just at electrical systems, but at the close-relative mechanical systems. Wonderful stuff.

FORCING A MODEL – DISTANT RELATIVES

Back as an undergraduate in a senior-level physics lab, I became overly enthusiastic with mathematical modeling, although I didn't think of it that way. I have no recollection of the particular experiment I was doing – I does not matter as that was not the lesson I needed to learn (as I later related above in my own teaching). What I had was data, and everyone knows data are meant to be plotted. The campus store had all kinds of graph paper. I plotted the data, and from the curves, calculated the equations. It never occurred to me (at first) that having ALL those various answers was not a good thing. As I say, I don't remember the physics, but I do remember the professor (Herbert Mahr) being very polite and telling me that while my presentation was very impressive, it didn't really MEAN anything. Indeed!

In the case of the electrical/mechanical analogs, we were on sound ground. We had well-established differential equations corresponding well to their respective physical realities. The equations in the electrical and mechanical cases were mathematically identical. Thus we had good, very good, models. Take your choice: (1) The math is a model for the physical realities, or (2) The mechanical and electrical analogs are models for each other. An embarrassment of riches.

In my physics experiment, it seemed at first that things could just be forced to fit a model. If I was able to fit a logarithmic curve, that's possibly interesting. Lots of physical relationships are logarithmic – for good reasons – they kind of had to be. Or perhaps, what if I seem to be able to fit a polynomial. That's possibly interesting too. But should I be able to do both? I really should consider if either one is possibly related to the physical reality? If not, maybe I am just playing with a paint-by-number game.

Famously a story about John von Neumann was passed down through Enrico Fermi and Freeman Dyson. Von Neumann was concerned about the ease with which people embraced free parameters. The quote is:

" With four parameters I can fit an elephant,
and with five I can make him wiggle his trunk. "

Of course, von Neumann was having a good joke while making a very valid point. We can force data to fit a model, at least locally. Sometimes there is good purpose in this – as long as we don't believe it "means anything" as Professor Mahr warned me.

For example, we used to get values of functions (such as logarithms) from tables printed in books, When we needed a value that was between tabulated values, we interpolated (usually linearly). That is, while we knew (by definition) that the function <u>was a logarithm</u>, we <u>used linear</u> interpolation (as being sufficiently simple, and generally quite good enough).

In the field of signal processing, we have many times worked with models that we knew full well were NOT close relatives. We just had convenient math. How often have we used a DFT (FFT) to analyze a signal we most assuredly knew was not periodic. Another good examples is the use of a polynomial to represent a signal. Polynomials and signals are unrelated. Fig. 1 shows why this is so.

Basically signals run in time (horizontally if you will) for considerable regions while staying between limits vertically. The vertical limits may well be enforced by power supply limits, wordlength, or our eardrums. Signals are horizontal. On the other hand, polynomials are vertical. Any polynomial you can write (other than a constant) runs away relatively quickly. It may wiggle around a bit, but then takes off. Fig. 1 shows a signal (two sinewave components) and a seventh-order polynomial which are similar in the region 9.25 to 11.25 or so. They were <u>made</u> to fit. [This was a matter of taking samples of the signal, using Matlab's ***polyfit***, and then ***polyval***. The details are not important.]

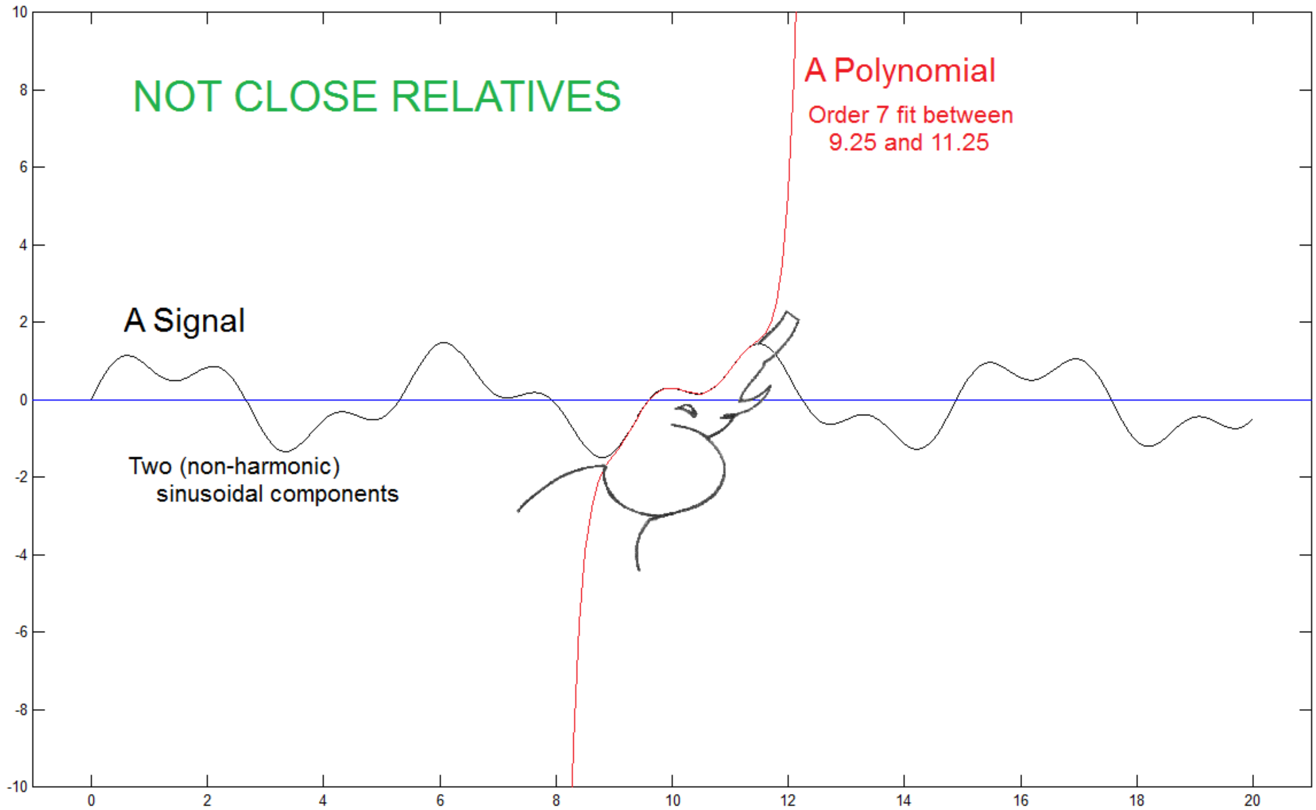

Fig. 1 Signals are Horizontal, Polynomials are Vertical

No one should ever suggest that the signal and the polynomial are close relatives. Perhaps in theory, we could choose a polynomial of high enough order to fit any finite-length signal we happen to have to some specified accuracy. In practice, even the example of Fig. 1 was starting to strain Matlab.

So why would we do it? Well hidden in the graph (at about the elephant's eye) is a very credible fit. That is, we can use polynomial interpolation [3] to good purpose. As we have seen, we can design good interpolation filters by this procedure. So we can do this – we just should not claim that the signal IS a polynomial.

BASIS FUNCTIONS, PERIODICITY, AND POLE MODELING

Our favorite mathematical analysis system is probably the Discrete Fourier Transform (DFT, as calculated by Fast Fourier Transform - FFT) [4]. It is fairly closely related to other Fourier and Laplace transform methods, but is strictly applicable exactly only to periodic functions with a fundamental frequency that is the reciprocal of the length of the time series involved, and which are also bandlimited to less than half the sampling rate. While the DFT (FFT) is pressed into duty when it comes to the actual calculation of most Fourier-like transforms, we often think of it as applying mainly to periodic signals that do not decay with time. For example, the familiar Fourier Series expansion.

If, on the other hand, we were describing a bell of the type we find at the top of a clock tower, one useful thing to note would be the resonant frequencies of the various "modes" of vibration. These modes are characterized much more by decaying amplitudes and non-harmonic frequencies, and much less by periodicity.

We have looked at mathematical analysis in such cases more in terms of pole modeling such as through the use of Prony's method and Kautz functions [5]. Even signals such as speech and some musical instruments have resonators, even with the resonators responding to a periodic excitation. The use of these mathematical tools is very often very useful, probably because the math mimics the actual physics (like acoustical filters).

We could well spend many pages reviewing these methods. Likely the readers here know of them and where to look, so this has been very brief.

COMPLEX SYSTEMS – SIMPLE MODLES (ABSOLUTELY NOT)

Here I want to be fairly specific about what I mean by a complex system. I could, if I had a purpose, consider at least trying to write down a system of many many interconnected equations to describe a system. We always have trouble finding a name for such a system. Probably "complicated" or "virtually incomprehensible" or "unfathomable" is what we are thinking of. Yet, while daunting, it can often, in theory, seem to be perfectly knowable and definable. We just don't really have the brainpower to get it cornered, and possibly lack the tools (computers – at least the software, are not immediately available). But the problem may be what is called "well posed" and solvable in theory. This is not really what I have in mind.

What I have in mind probably involves many unknowns (and the ever popular, "unknown unknowns") and sadly, we may have actual mathematical "chaos" about to emerge or in full-fledge eruption. The problem is ill-posed. The earth's climate is an obvious example. The behavior of animals and people is another. We can't solve the full problem, because we have neither the equations, the data, nor the computational power. Even in principle we may not have a suitable way to approach the problem. It may be that only a model that fully simulates the system itself will work, and only by actually iterating this do we get to do the simulation. Useful approximations, even for limited ranges of time may not be manageable, and let alone any prospect of a result in the form of simple formula (like a polynomial or known function – even for the crudest case). Garbage In – Garbage Out.

Yet the general public believes that computers are smart and some people are smart. Put the two together and we must have an answer we can all trust. We don't. We can't. Sit back and see what happens. Then, and only then, will you know.

So there may be little hope. And we do need to recognize the limitations. We may perhaps to able to do very limited simulations that are of some value. Perhaps most importantly we may be able to determine if the results are constrained in any way. For example, do the laws of thermodynamics tell us that while we can't know the answer very well, it has to be constrained by something fundamental and unavoidable? What results coming out of a mathematical model might be just silly?

SCREENING FALLACY – BAD PROXY DATA INTO MODELS

Likely everyone who is even remotely acquainted with the "Global Warming" issue knows about the "Hockey Stick Graph" [6]. In brief, if one wants to show (with whatever motivation) that any current warming of the climate is unprecedented, one needs to show two things (1) that it is currently warming, and (2) that for a period of history looking backward, it has never been this warm before.

But what, for example, does a term like "record high temperature" mean! It turns out that this is not by any means easy to discuss – but it does sound so good. Should be easy, you just look at a column of numbers. Is the newest entry the largest? But then: What time period? What location, here or ten feet to the left? We all think, automatically, that we know what it means. We don't.

In fact, record temperatures aside, it is quite silly to even talk about a "global temperature" like at 12:34 GMT today, let alone what it was 1000 years ago. To understand how impossible this idea is, just suppose that you were assigned the task of measuring it. How would you do it? You would likely tell us – well the way they do it. That's no answer.

Speaking in the most general sense, few people from either of the major camps on the global warming issue doubt there is some recent warming, from about 1970-2000. So, this warming, the “blade” of the hockey stick, is not really the issue. It is rather, the straight shaft, the supposition that the global temperature was flat for 1000-2000 years prior to 1970 that is at issue. In particular, was it, or was it not, as warm as today or warmer during the “medieval warm period” (MWP), very roughly 1000 years ago, and was it particularly cold during the “little ice age” (LIA), roughly 300 years ago. If we in fact had these substantial variations, than is any warming today unusual, let alone a cause for alarm and/or action.

About 1998, climate researchers led by Michael Mann and his team presented a summary of proxy data which is now called the “hockey stick” (HS). This was equivalent to getting rid of the MWP and the LIA (thereby achieving the flat shaft of the HS) and declaring the blade of the HS unprecedented, and further blaming it on manmade CO_2 emissions. There were many things wrong with the HS itself and the associated studies. A few years later, statistician Steve McIntyre and his colleagues (in particular, Ross McKitrick) found errors in the methods used by Mann et al, and found the HS graph to be a result of mathematical errors in their attempt to use what is known as Principal Component Analysis (PCA). Central to the critique is the claim that the method itself (with its erroneous implementation), if applied to random data, produced a HS shape. Indeed, this is devastating – there is no way around that. Nothing can be proven about the data if the method, not the data, causes the result!

I must of necessity limit references to this controversial topic. The interested reader will have no trouble using the internet to find endless papers and blogs. I just want to point out that the HS graph was found to be wanting on the basis of what was claimed to be a faulty normalization of PCA. In full disclosure, I agree, because of this faulty math, and because of much much more evidence (scientific and political, etc), that the global-warming/climate-change/sever-weather-event-increase, is bogus. What I want to discuss in a very simple demonstration is how a HS shape can come out of faulty procedure, rather than out of data.

While I have actually taught PCA, I don’t feel comfortable enough with it to try to use it in an example. (Not in the way I am very comfortable with Fourier analysis, in comparison.) I have tried PCA (from Matlab) on some red noise signals which I generated, and normalized in both the standard and non-standard way. Let’s just say there is a big difference between the results. However, as I said, I am not comfortable enough with the results to present them here. Perhaps more to the point, since PCA is not a household notion, or even a standard engineering course topic, by any means, I want to use something far simpler.

The notion of correlation is certainly more familiar than PCA for our readers. However, this too is not overly familiar to many, so I propose a much simpler correlation-like technique as given below. It’s a kind of “poor man’s correlation”. In addition, instead of suggesting a bogus PCA normalization, what I am discussing is what Steve McIntyre has called a “screening fallacy” – an unintentional “cherry-picking” that produces HS shapes. And I just want to do it with white noise. It is not clear if this is the same thing, exactly or approximately, that was the cause of the false hockey sticks with PCA (but seems a related “end effect”).

<u>**SCREENING SIGNALS WITH A "POOR MAN'S" CORRELATION**</u>

This is so obvious. The term "screening fallacy" was used by Steve McIntyre in his "Climate Audit" blog, but I actually think I encountered it from Lucia's "Blackboard" blog first. [Incidentally, the amazing contributions of "amateur" bloggers such as McIntyre, Lucia, Andrew Montford's "Bishop Hill", Anthony Watts "Watts Up WithThat", Jo Nova, and so many others will be a long appreciated effort as future historians will see it.] The screening fallacy is so obvious – who could have made that mistake?

Now, just exactly what are the things about the screening of proxies that necessarily produces a hockey stick (or any target feature) in a bogus manner, that I claim is so obvious. It is obvious in two senses. First, the argument why is should work seems, at first blush, straightforward enough. Secondly, even without careful analysis, something should click in your head to alert you that you are <u>about to fool yourself</u>. Here is the argument:

(1) We want to find temperatures going back 2000 years, lets us say. Clearly we don't have records for credible instruments going back more than perhaps 200 years at most. (All this ignores the problems of a lack of even modest coverage over a widespread area.)

(2) But perhaps we do have half-credible instrument data that suggest a 30 year warming, perhaps from 1970 to 2000. (All this ignores causes of any observed warming, natural or man-made, and possibly man-made in the Urban Heat Island sense, not a CO_2 greenhouse-gas sense.)

(3) We <u>postulate</u> that some surviving natural physical evidence (very often tree ring data) should correlate with temperature. That is, tree rings, properly handled, <u>may be</u> proxy thermometers. (All this ignores other causes or tree-ring variations, perhaps rainfall, perhaps animals browsing and/or dropping fertilizer, etc.)

(4) It <u>seems logical</u> that <u>not all</u> tree rings (or other proxies, ice cores, corals, etc.) will be good temperature proxies. Perhaps not all have "the temperature signal". (It seems like a human talent – you have it or not!) It should be possible to identify those that are good, by "calibrating" with the instrument record over the period 1970-2000 (or perhaps 1850-2012, whatever) when we think we have a credible notion of the "real" temperature. That is, we look at each proxy, or class of proxy, and see which of these (as a class, or individually) seem to have the "temperature signal". Keep those, discard the rest. Now just statistically average the "good" ones.

(5) <u>Here is where the alarm bell should sound</u>. What if, you say, any apparent temperature signal is accidental? You have kept only signals that show a recent warming, so these should all average upward (the hockey stick blade). Moreover, if there is in fact only accidental correlation with temperature, than for the period outside the calibration period, everything more or less cancels. We get the flat part of the stick. Trouble here!

(6) That is, in selecting what we believe to be only proxies with the "temperature signal", we are artificially "cherry picking" the input under self-imposed delusion of choosing (teasing out) reliable (good) signals. This is obvious, but let's look at the simplest possible example we can imagine. Here is the very simple experiment. It is no more than a simple demonstration. Nothing to do with real temperatures.

First we choose a large set of random signals. We often see these chosen from a population of "red noise" random-walk signals. But here for simplicity, let's just use white noise which is more familiar. In fact, we will use Matlab's ***rand*** function, a very standard random uncorrelated sequences with a target mean of 0.5. Next, we will check individually to see if these sequences seem to contain the signal we believe is there. We could use any feature we like to look for, but for simplicity, we will look for a hockey-stick blade. In particular, we will look for an increase in the last three values of the sequence. Thus if we use w=***rand***(1,20) we want to see if w(20)>w(19)>w(18)>w(17). If so, we judge this to be a signal-bearing proxy. If not, discard it. Fig. 2 shows the result. We found that about 4% of our signals passed the test. And we get an average (of these 853) that has an uptick at the end. Just what we ordered. Yet it is not quite a hockey stick.

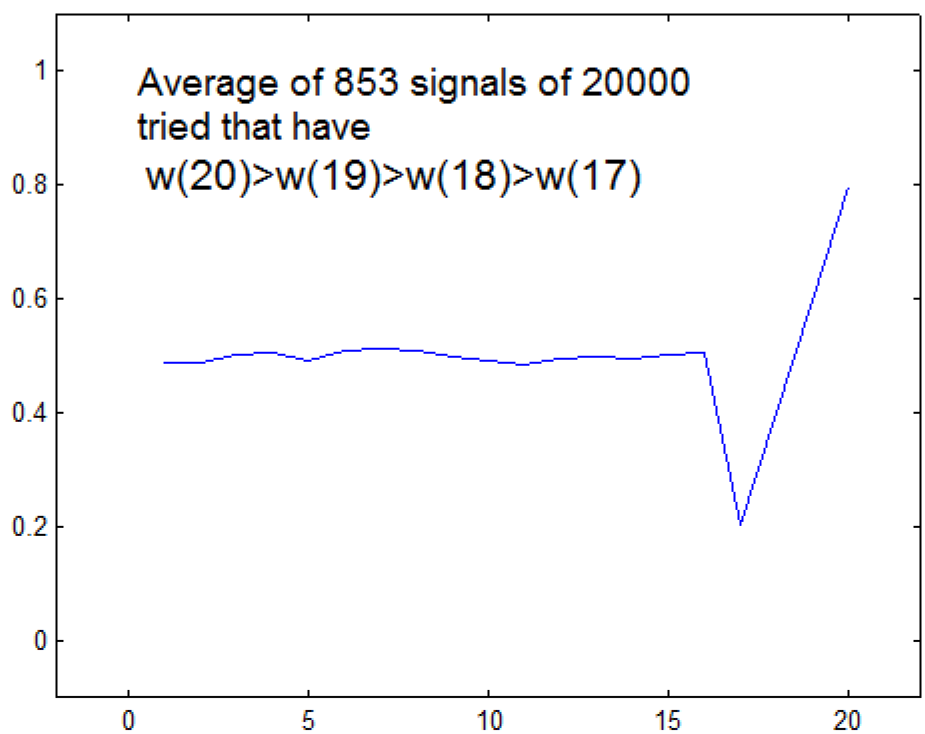

Fig. 2 Here we used a condition on the last three samples.
We get a scythe instead of a hickey stick.

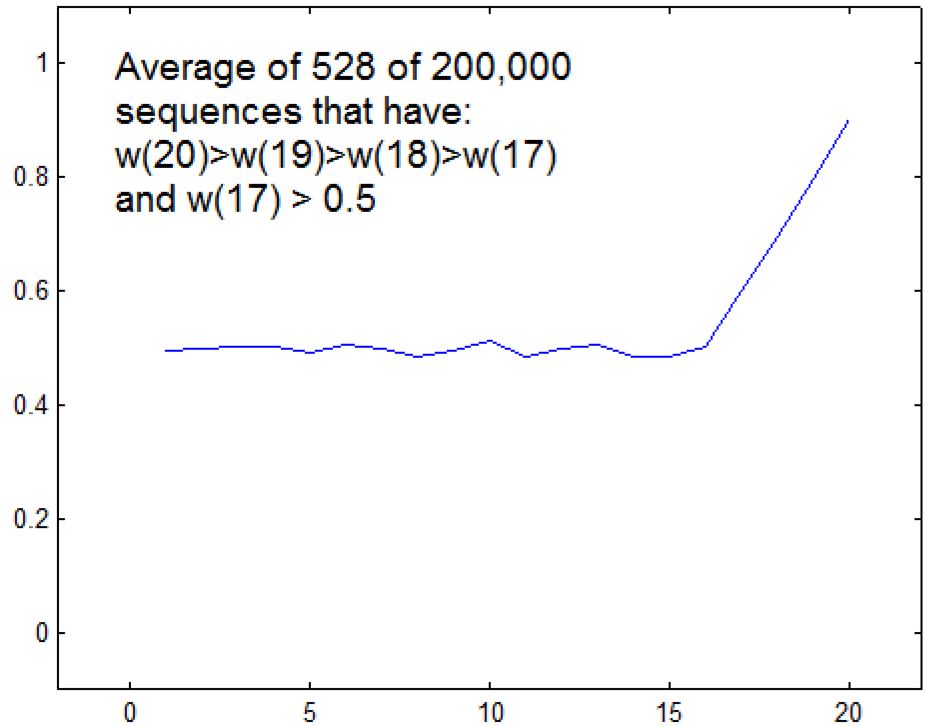

Fig. 3 It is a simple matter to add an additional condition to get the "blade"

In Fig. 3 we amend the program with an additional condition. We look for an upward increasing sequence above the mean of the first 16 samples. That is, we need to know that w(17) was greater than the average of the first 16 samples. We can do this by actually computing the average (see Program 1), but almost equally well, we simply want w(17) to be greater than the mean of the random generator, which is 0.5. The result is a traditional hockey-stick generated by a selection process on signals that have (by definition) no temperature component. The selection procedure, seemingly rationally conceived, did it. This is the point. We can however look at a few points more closely.

In Fig. 3 we looked at 200,000 signals and accepted 528 of them as fitting our criterion that the last three samples (18, 19, and 20) increased and the 17th was greater than 0.5. We said that we could also look for w(17) to be greater than the mean of w(1) to w(16) to much the same effect, and this was chosen (just as a more interesting picture) in Fig. 4 for four “accepted” or “screened” proxies. Note that the first 16 samples of these flail away randomly, pretty much using the range from 0 to 1, as we expect.   The last four samples seem to not be random. Well, they were generated by the same random process as the first 16. We have just chosen four signals that have the ending rising. That’s why they look different.

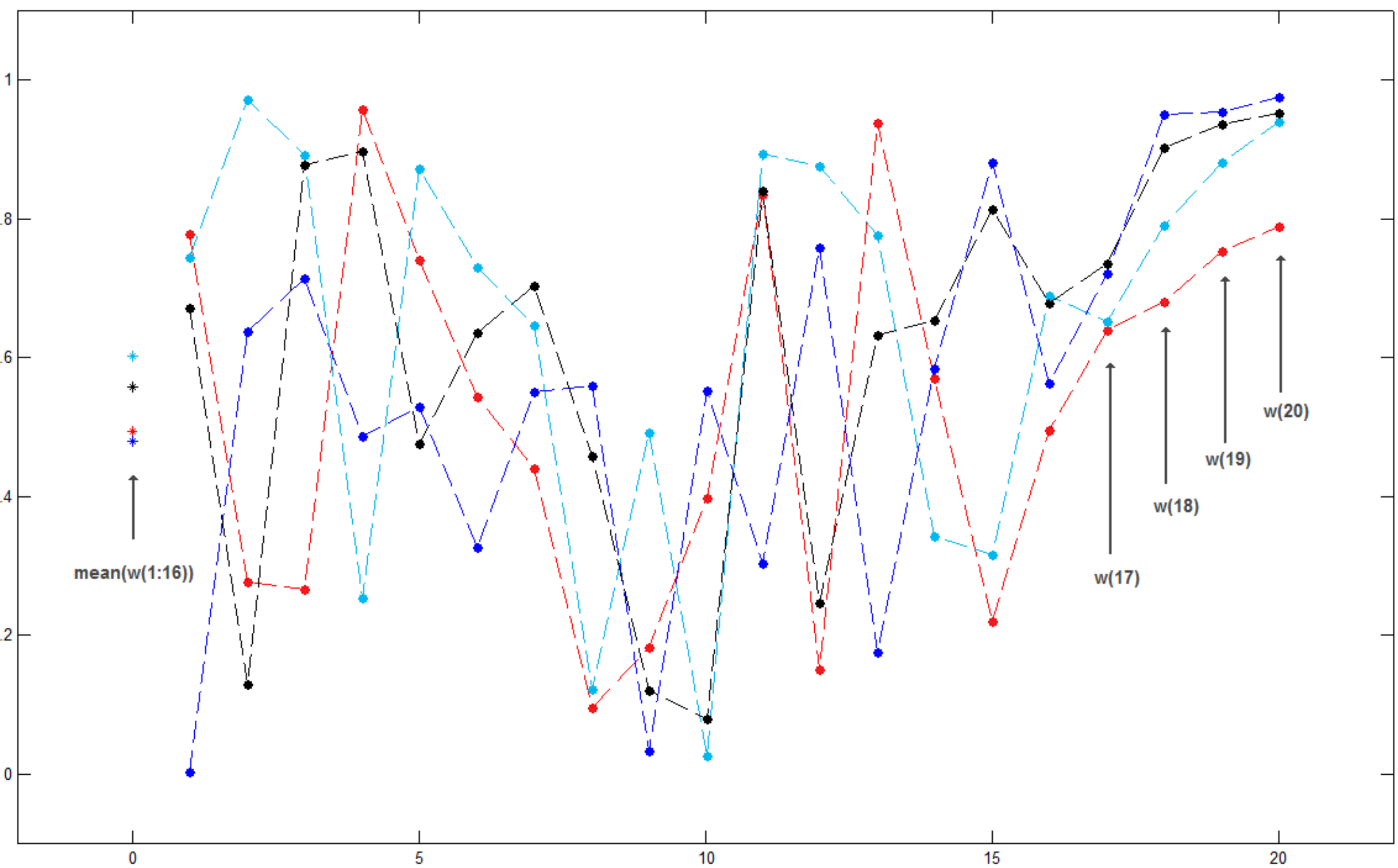

Fig. 4   A close look at four of the “accepted” proxy signals

We know that if we generate signals using rand and do a lot of averaging, they average right around 0.5.   This does happen for samples 1-16 here. But the last four samples were screened for a rise, and thus they average (quite nicely in Fig. 3) to the HS blade.

We can make three points. First, we have described a selection (or screening) process in terms of looking for certain properties, but what we have done is equivalent to an elementary or intuitive form of correlation analysis.   This we did for simplicity of description (to avoid mentioning correlation). Secondly – as we said, this is obvious.   At least it is obvious in the sense that a bell-goes-off - that pre-selecting (screening) is not simply a calibration or validation procedure, but a “cherry picking” procedure. To see ahead of time that it generates a hockey stick is not quite so obvious – but that probably takes only a minute or two. The third point is that this is not something that we can turn on or off. It happens – biasing the result - from the first sequence screened.

Well, perhaps a bit too dramatically offered here. It is rather beautiful in its simplicity, but it is clear that it had to happen here, and likely no one (except true zealots or the technically illiterate) would buy this in a real situation. The reason being that we found signals in only 528 of 200,000 (0.26%). And we know that these signals were there by chance (by definition it is by chance).

Suppose we used Fig. 3 not as a reconstructed output, but as a way of suggesting that the entire class of signals, the ones we kept and the ones we threw away, are valid proxies. We simply wanted to see if anything could be dug out. So we decide not to SCREEN OUT any signals. Does anyone doubt the result (as I write this, I have not yet run the test).

Okay - here it is (see Fig. 5):

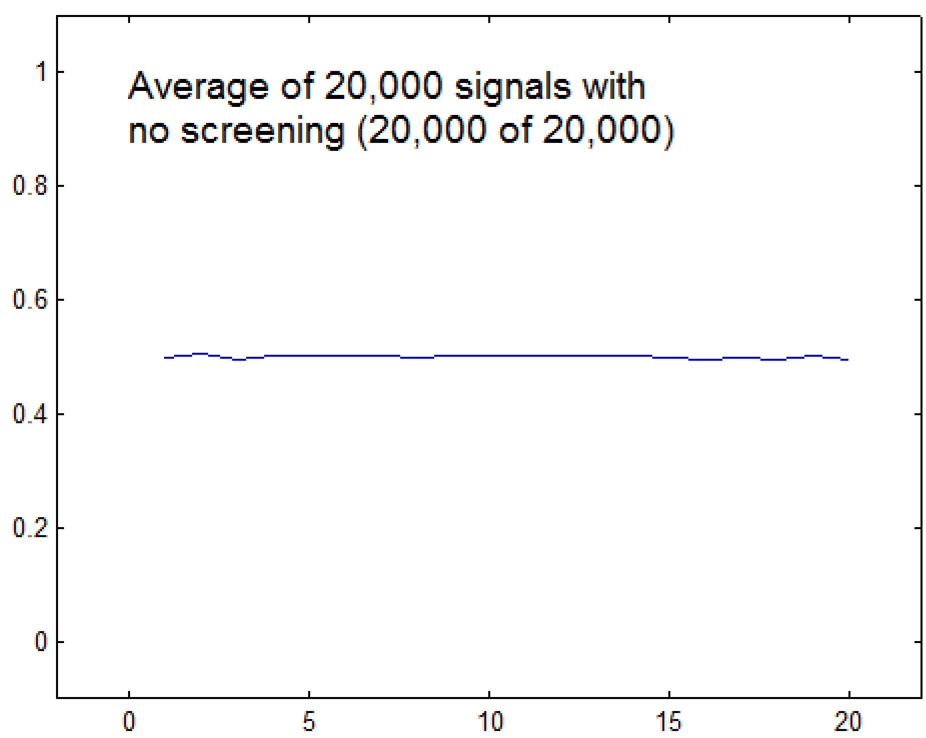

Fig. 5

Turning off the screening (keeping all the random signals) averages, or course, to be flat.

This probably makes the point.   If the process itself generates a hockey stick from random noise, any result showing a hockey stick from this process cannot be trusted, and has no meaning.   If you are satisfied, you need read no further.

But what, you may say, would happen if there were actually a hockey-stick signal in the data?   This is easy to do. We can, for example, add 0.05 to w(18), 0.10 to w(19), and 0.15 to w(20).   Fig 6 shows this with 20,000 signals thusly biased and averaged, with no screening here.   Of course, in this case we get the hockey stick shape. In fact, it is fairly easy to see from Fig. 6 the exact signal added by the time we have averaged 20,000 signals. Any individual signal would still look basically random. But here, all 20,000 signals are signal bearing. Fig. 6 is no surprise (any more than Fig. 5 was). We are not using screening, so there are no effects of screening here.

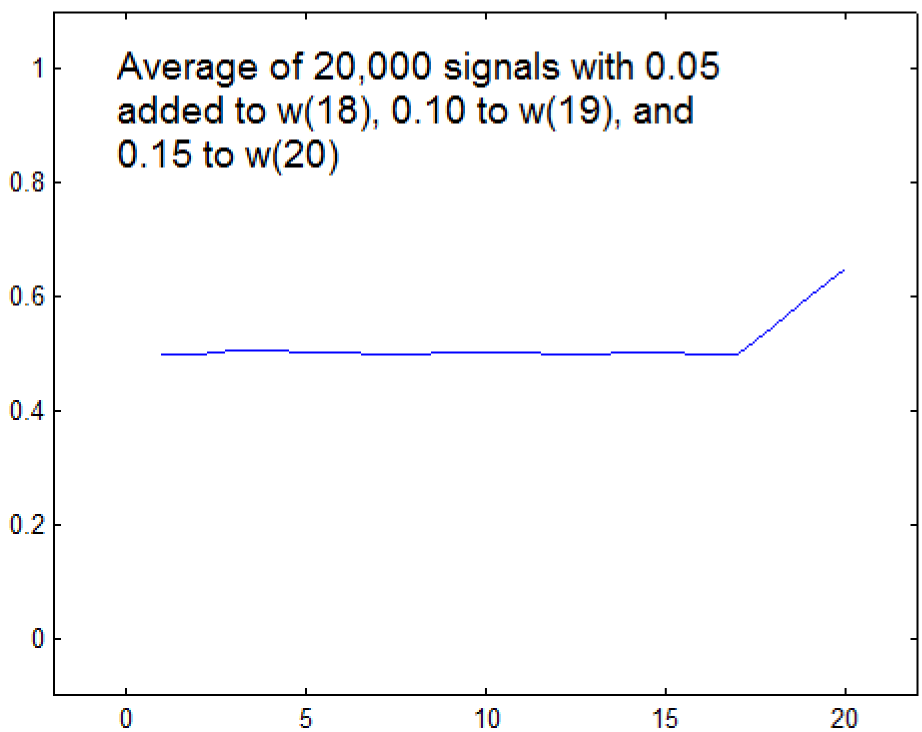

Fig. 6 Here we have added a signal to all 20,000 sequences. If we looked at any one, we would be unlikely to see the added signal, but it of course shows up very clearly in the average.

For Fig. 7 we have put the screening procedure back in. Note the very strong resemblance of Fig. 7 to Fig. 3.   The difference we note is that here a greater number of signals passed the test (1543 instead of 528), because we did raise the last three samples. That is, while all 20,000 sequences have the temperature signal, still most of them fail the screening test.

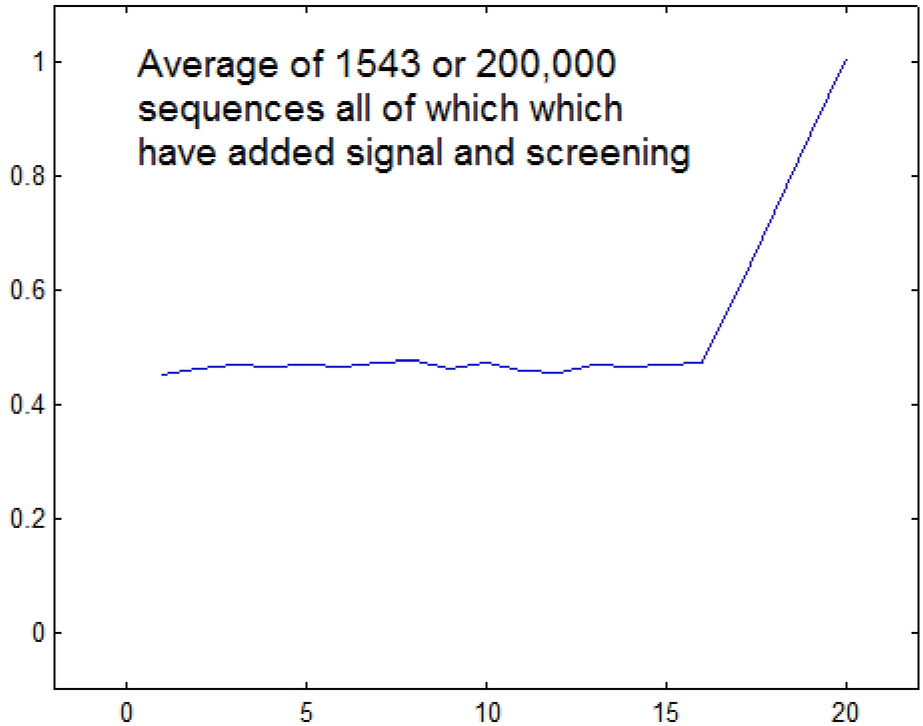

Fig. 7 Signal added and screening (using mean of first 16)

This strongly resembles Fig. 3, except we see that more signals passed the test.

We could easily think of more and more test to run, and continue this study for many more pages. Yet we would not have expectations of doing anything more definitive, as we already have little notion that this relates to an actual climate proxy situation. We promised that this was just a demonstration. The point was that you can't tell, from the results, if there is a real climate signal or if it is a product of the screening procedure.

In some sense, we have been devious here. We have not paid great attention to the fact that the "poor man's correlation" screening here only passed about ¼ of 1% (Fig. 3) of the possible sequences. When we think about it, we should be unlikely to believe that these matches mean anything (being random, they don't). Yet, the final result is a rather outstanding HS! It would be easy enough to get more sequences through by relaxing the criterion for a valid temperature signal, and accepting a HS shape of less beauty. For example, suppose we chose to require only that w(20) and w(19) have values greater than the mean of w(1:20)-0.25. That is, only the last two values needs to exceed the mean, minus 1/4. This seems so weak that it would be unlikely to produce a HS. Indeed, it passes about 58% of the sequences in the example of Fig. 8, which tests 1000 sequences. The HS is however still quite pronounced. And so on.

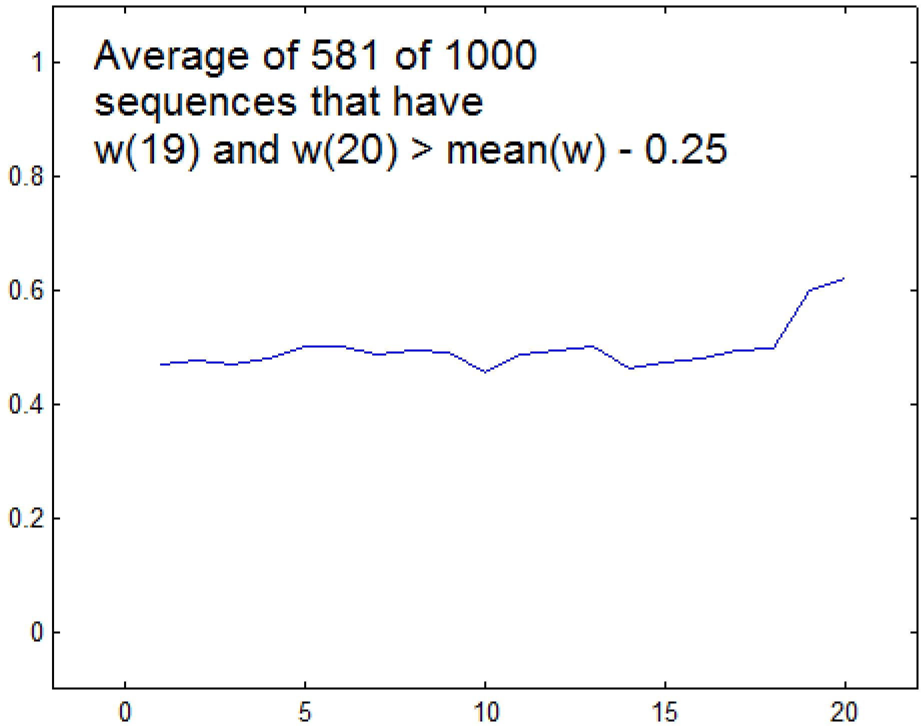

Fig. 8 Here we greatly weaken the screening criterion – yet the HS is still pronounced.

All this does suggest some fundamental cautions. You can, if you wish, use a correlation analysis to see if any indication of a temperature signal is present in data. But then, you must do one of two things: First, you could say that a certain class of proxies (pumpkin trees, for example, with apology to Mark Twain) may well contain a climate signal and we collect data on 1000 new, untested pumpkin trees. The second thing we could do would be to keep the proxies that passed the screening (perhaps data are rare) along with all those that failed. It is perhaps a very hard thing to keep data that we have reason to believe are going to weaken our results – pictures not so pretty. (You won't have to look hard to find people who will insist that it is only the ones that have passed screening that should be kept!) But then, we aren't supposed to know what our results are expected to be – ahead of time.

In the case of actual studies that have used screened proxies, it perhaps seems too late to work with only unscreened proxies. Of course, it's not too late unless data are destroyed or made unavailable. Honest scientific procedure would demand retention of all data.

PROGRAM 1

This really is all there is to the code.

```
% screening fallacy SF1.m
aw=zeros(1,20)
numgood=0
for k=1:200000
  w= rand(1,20);
  %  if rand>0.7   %  use to insert "signal"
  %      w = w+[0 0 0 0 0 0 0 0 0 0 0 0 0 0 0 0 0 .05 .1 .15];
  %  end
  if w(20)>w(19)
    if w(19)>w(18)
      if w(18)>w(17)
        %if w(17)>mean(w(1:16))  % Alternative to line below
          if w(17)>0.5

        aw=w + aw;
        numgood=numgood+1;

         end
      end
    end
  end

end
numgood
aw=aw/numgood
figure(1)
plot(aw)
axis([-2 22 -.1 1.1]);
axis
figure(1)
```

REFERENCES AND NOTES

Note: This reference list is a bit unusual, as in many cases here only brief listings offering the flavor is included, as a practical matter. Thus we also offer a few notes as well.

[1] Eugene Wigner,"The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Communications in Pure and Applied Mathematics*, Vol. 13, No. I (February 1960).

[2] Richard W. Hamming, “The Unreasonable Effectiveness of Mathematics”, *The American Mathematical Monthly*, Vol. 87 No. 2 February 1980

[3] For polynomial interpolation see, for example, B. Hutchins, “Interpolation Based Filter Design”, *Electronotes*, Vol. 20, No. 198, pp 44-50 (June 2001) (online electronotes.netfirms.com/EN198.pdf)

[4] For Fourier methods, see for example B. Hutchins “A Review of Fourier Methods in Signal Processing and Musical Engineering“, *Electronotes*, Special Issue “D” (155-160) Nov. 1983. Possibly also, 10% of what we have published is Fourier stuff.

[5] For Prony, see for example, B. Hutchins, “Prediction, Deconvolution, and System Identification: Part 1; Undriven Systems”, *Electronotes*, Vol. 17, No. 179, pp 14-29 (June 1992) and B. Hutchins, “Frequency Analysis/Resolution With Few Samples,” Application Note 365, April 2006 (online at electronotes.netfirms.com/AN365.pdf). For Kautz, see B. Hutchins, ”Tools for Investigating Kautz Functions,” *Electronotes,* Vol. 22, No. 207, December 2011, (online at electronotes.netfirms.com/EN207.pdf).

[6] Almost certainly the central book to read about the HS is *The Hockey Stick Illusion* by Andrew Montford, Stacey International, (2010). This is a remarkable book. Written pre-“Climategate”, this is largely a highly-organized presentation of the findings of Steve McIntyre and others, and as the title of course suggests, finds the HS notion highly suspect. Montford (blogging under “BishopHill”) and McIntyre, blogging under “Climate Audit” are central players. There are many other excellent books and blogs. Anthony Watts’ “WattsUpWithThat” is both essential on a day by day basis, and has a rather complete list of links to other blogs (both camps).

If one reads *Climate Audit*, we may well be blown away by the technical level of statistical expertise on display there. This observation on my part is from a person who is not at all comfortable with statistics. Certainly many of the people commenting there are far far more competent in statistics than the climate scientists are.