**[http://wattsupwiththat.com/2014/10/02/a-rare-debate-on-the-settled-science-of-climate-change/#comment-1753069](http://wattsupwiththat.com/2014/10/02/a-rare-debate-on-the-settled-science-of-climate-change/" \l "comment-1753069)**

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# Post by Dr. Robert G. Brown, Lecturer in Physics at Duke University to a comment by X

*“…debates are rare because science is not a debate, or more specifically, science does not proceed or advance by verbal debates in front of audiences. You can win a debate and be wrong about the science. Debates prove one thing. Folks who engage in them don’t get it, folks who demand them don’t get it and folks who attend them don’t get it”.*

[Response to X] … it is pretty clear that you’ve never been to a major physics meeting that had a section presenting some unsettled science where the organizers had set up two or more scientists with entirely opposing views to give invited talks and participate in a panel just like the one presented. This isn’t “rare”, it is very nearly standard operating procedure to avoid giving the impression that the organizers are favoring one side or the other of the debate. I have not only attended meetings of this sort, I’ve been one of the two parties directly on the firing line (the topic of discussion was a bit esoteric — whether or not a particular expansion of the Green’s function for the Helmholtz or time-independent Schrodinger equation, which comes with a restriction that one argument must be strictly greater than the other in order for the expansion to converge, could be used to integrate over cells that de facto required the expansion to be used out of order). Sounds a bit, err, “mathy”, right, but would you believe that the debate grew so heated that we were almost (most cordially :-) shouting at each other by the end? And not just the primary participants — members of the packed-room audience were up, gesticulating, making pithy observations, validating parts of the math.

You’re right that you can “win the debate and be wrong about the science”, however, for two reasons. One is that in science, we profoundly believe that there is an independent objective standard of truth, and that is nature itself, the world around us. We attempt to build a mathematical-conceptual map to describe the real terrain, but (as any general semantician would tell you) the map is not the terrain, it is at best a representation of the terrain, almost certainly an imperfect one. Many of the maps developed in physics are truly excellent. Others are perhaps flawed, but are “good enough” — they might not lead someone to your cufflinks in the upstairs left dresser drawer, but they can at least get someone to your house. Others simply lead you to the wrong house, in the wrong neighborhood, or lead you out into the middle of the desert to die horribly (metaphorically speaking). In the end, scientific truth is determined by correspondence with real-world data — indeed, real world future data — nothing more, nothing less. There’s a pithy Einstein quote somewhere that makes the point more ably than I can (now there was a debate — one totally unknown patent clerk against an entire scientific establishment vested in Newtonian-Galilean physics :-) but I am too lazy to look it up.

Second, human language is often the language of debates and comes with all of the emotionalism and opportunity for logical fallacy inherent in an imprecise, multivalued symbol set. Science, however, ultimately is usually about mathematics, logic and requires a kind of logical-mathematical consistency to be a candidate for a possible scientific truth in the sense of correspondence with data. It may be that somebody armed with a dowsing rod can show an extraordinary ability to find your house and your cufflinks when tested some limited number of times with no map at all, but unless they can explain how the dowsing rod works and unless others can replicate their results it doesn’t become anything more than an anecdotal footnote that might — or might not — one day lead to a startling discovery of cuff-linked ley lines with a sound physical basis that fit consistently into a larger schema than we have today. Or it could be that the dowser is a con artist who secretly memorizes a map and whose wife covertly learned where you keep your cufflinks at the hairdresser. Either way, for a theory to be a candidate truth, it cannot contain logical or mathematical contradictions. And even though you would think that this is not really a matter for debate, as mathematics is cut and dried pure (axiomatically contingent) truth — like I said, a room full of theoretical physicists almost shouting over whether or not the Green’s function expansion could converge out of order — even after I presented both the absolutely clear mathematical argument and direct numerical evidence from a trivial computation that it does not.

Humans become both emotionally and financially attached to their theories, in other words. Emotionally because scientists don’t like being proven wrong any more than anybody else, and are no more noble than the average Joe at admitting it when they are wrong, even after they come to realize in their heart of hearts that it is so. That is, some do and apologize handsomely and actively change their public point of view, but plenty do not — many scientists went to their graves never accepting either the relativistic or quantum revolutions in physics. Financially, we’ve created a world of short-term public funding of science that rewards the short-run winners and punishes — badly — the short-run losers. Grants are typically from 1 to 3 years, and then you have to write all over again. I quit research in physics primarily because I was sick and tired of participating in this rat race — spending almost a quarter of your grant-funded time writing your next grant proposal, with your ass hanging out over a hollow because if you lose your funding your career is likely enough to be over — you have a very few years (tenure or not) to find new funding in a new field before you get moved into a broom closet and end up teaching junk classes (if tenured) or have to leave to proverbially work at Walmart (without tenure).

Since roughly six people in the room where I was presenting were actively using a broken theory to do computations of crystal band structure, my assertion that the theory they were using was broken was not met with the joy one might expect even though the theory I had developed permitted them to do almost the same computation and end up with a systematically and properly convergent result. I was threatening to pull the bread from the mouths of their children, metaphorically speaking (and vice versa!).

At this point, the forces that give rise to this sort of defensive science are thoroughly entrenched. The tenure system that was intended to prevent this sort of thing has been transformed into a money pump for Universities that can no longer survive without the constant influx of soft and indirect cost money farmed every year by their current tenured faculty, especially those in the sciences. Because in most cases that support comes from the federal government, that is to say our taxes, there is constant pressure to keep the research “relevant” to public interests. There is little money to fund research into (say) the formation of fractal crystal patterns by matter that is slowly condensing into a solid (like a snowflake) unless you can argue that your research will result in improved catalysis, or a way of building new nano-materials, or that condensed matter of this sort might form the basis for a new drug, or…

Or today, of course, that by studying this, you will help promote the understanding of the tiny ice crystals that make up clouds, and thereby promote our understanding of a critical part of the water cycle and albedo feedback in Climate Science and thereby do your bit to stave off the coming Climate Apocalypse.

I mean, seriously. Just go to any of the major search engines and enter “climate” along with anything you like as part of the search string. You would be literally amazed at how many disparate branches of utterly disconnected research manage to sneak some sort of climate connection into their proposals, and then (by necessity) into their abstracts and/or paper text. One cannot study poison dart frogs in the Amazon rainforest any more just because they are pretty, or pretty cool, or even because we might find therapeutically useful substances mixed into the chemical poisons that they generate (medical therapy being a Public Good even more powerful that Climate Science, quite frankly, and everything I say here goes double for dubious connections between biology research and medicine) — one has to argue somewhere that Climate Change might be dooming the poor frogs to extinction before we even have a chance to properly explore them for the next cure to cancer. Studying the frogs just because they are damn interesting, knowledge for its own sake? Forget it. Nobody’s buying.

In this sense, Climate Science is the ultimate save. Let’s face it, lots of poison dart frogs probably don’t produce anything we don’t already know about (if only from studying the first few species decades ago) and the odds of finding a really valuable therapy are slender, however much of a patent-producing home run it might be to succeed. The poor biologists who have made frogs their life work need a Plan B. And here Climate is absolutely perfect! Anybody can do an old fashioned data dredge and find some population of frogs that they are studying that is changing, because ecology and the environment is not static. One subpopulation of frogs is thriving — boo, hiss, cannot use you — but another is decreasing! Oh My Gosh! We’ve discovered a subpopulation of frogs that is succumbing to Climate Change! Their next grant is now a sure thing. They are socially relevant. Their grant reviewers will feel ennobled by renewing them, as they will be protecting Poison Dart Frogs from the ravages of a human-caused changing climate by funding further research into precisely how it is human activity that is causing this subpopulation to diminish.

This isn’t in any sense a metaphor, nor is it only poison dart frogs. Think polar bears — the total population is if anything rapidly rising, but one can always find some part of the Arctic where it is diminishing and blame it on the climate. Think coral reefs — many of them are thriving, some of them are not, those that are not may not be thriving for many reasons, some of those reasons may well be human (e.g. dumping vast amounts of sewage into the water that feeds them, agricultural silt overwashing them, or sure — maybe even climate change. But scientists seeking to write grants to study coral reefs have to have some reason in the public interest to be funded to travel all over the world to really amazing locations and spend their workdays doing what many a tourist pays big money to do once in a lifetime — scuba or snorkel over a tropical coral reef. Since there is literally no change to a coral reef that cannot somehow be attributed to a changing environment (because we refuse to believe that things can just change in and of themselves in a chaotic evolution too complex to linearize and reduce to simple causes), climate change is once again the ultimate save, one where they don’t even have to state that it is occurring now, they can just claim to be studying what will happen when eventually it does because everybody knows that the models have long since proven that climate change is inevitable. And Oh My! If they discover that a coral reef is bleaching, that some patch of coral, growing somewhere in a marginal environment somewhere in the world (as opposed to on one of the near infinity of perfectly healthy coral reefs) then their funding is once again ensured for decades, baby-sitting that particular reef and trying to find more like it so that they can assert that the danger to our reefs is growing.

I do not intend to imply by the above that all science is corrupt, or that scientists are in any sense ill-intentioned or evil. Not at all. Most scientists are quite honest, and most of them are reasonably fair in their assessment of facts and doubt. But scientists have to eat, and for better or worse we have created a world where they are in thrall to their funding. The human brain is a tricky thing, and it is not at all difficult to find a perfectly honest way to present one’s work that nevertheless contains nearly obligatory references to at least the possibility that it is relevant, and the more publicly important that relevance is, the better. I’ve been there myself, and done it myself. You have to. Otherwise you simply won’t get funded, unless you are a lucky recipient of a grant to do e.g. pure mathematics or win a no-strings fellowship or the Nobel Prize and are hence nearly guaranteed a lifetime of renewed grants no matter how they are written.

This is the really sad thing, …. Science is supposed to be a debate. What many don’t realize is that peer review is not about the debate. When I review a paper, I’m not passing a judgment as a participant on whether or not its conclusion is correct politically or otherwise (or I shouldn’t be — that is gatekeeping, unless my opinion is directly solicited by an editor as the paper is e.g. critical of my own previous work). I am supposed to be determining whether or not the paper is clear, whether its arguments contain any logical or mathematical inconsistencies, whether it is well enough done to pass muster as “reasonable”, if it is worthy of publication, ~~now~~ *not* whether or not it is right or even convincing beyond not being obviously wrong or in direct contradiction of known facts. I might even judge the writing and English to some extent, at least to the point where I make suggestions for the authors to fix.

In climate science, however, the ClimateGate letters openly revealed that it has long since become covertly corrupted, with most of the refereeing being done by a small, closed, cabal of researchers who accept one another’s papers and reject as referees (well, technically only “recommend” rejection as referees) any paper that seriously challenges their conclusions. Furthermore, they revealed that this group of researchers was perfectly willing to ruin academic careers and pressure journals to fire any editor that dared to cross them. They corrupted the peer review process itself — articles are no longer judged on the basis of whether or not the science is well presented and moderately sound, they have twisted it so that the very science being challenged by those papers is used as the basis for asserting that they are unsound.

Here’s the logic:

a) We know that human caused climate change is a fact. (We heard this repeatedly asserted in the “debate” above, did we not*?* It is a fact that CO2 is a radiatively coupled gas, completely ignoring the actual logarithmic curve Goreham presented, it is a fact that our models show that that more CO2 must lead to more warming, it is a fact that all sorts of climate changes are soundly observed, occurred when CO2 was rising so it is a fact that CO2 is the cause, count the logical and scientific fallacies at your leisure).

b) This paper that I’m reviewing asserts that human caused climate change is not a fact. It therefore contradicts “known science”, because human caused climate change is a fact. Indeed, I can cite hundreds of peer reviewed publications that conclude that it is a fact, so it must be so.

c) Therefore, I recommend rejecting this paper.

It is a good thing that Einstein’s results didn’t occur in Climate Science. He had a hard enough time getting published in physics journals, but physicists more often than not follow the rules and accept a properly written paper without judging whether or not its conclusions are true, with the clear understanding that debate in the literature is precisely where and how this sort of thing should be cleared up, and that if that debate is stifled by gatekeeping, one more or less guarantees that no great scientific revolutions can occur because radical new ideas even when correct are, well, radical. In one stroke they can render the conclusions of entire decades of learned publications by the world’s savants pointless and wrong. This means that physics is just a little bit tolerant of the (possible) crackpot. All too often the crackpot has proven not only to be right, but so right that their names are learned by each succeeding generation of physicist with great reverence.

Maybe that is what is missing in climate science — the lack of any sort of tradition of the maverick being righter than the entire body of established work, a tradition of big mistakes that work amazingly well — until they don’t and demand explanations that prove revolutionary. Once upon a time we celebrated this sort of thing throughout science, but now science itself is one vast bureaucracy, one that actively repels the very mavericks that we rely on to set things right when we go badly astray.

At the moment, I’m reading Gleick’s lovely book on Chaos [[*Chaos: The Making of a New Science*](http://www.goodreads.com/book/show/64582.Chaos)], which outlines both the science and early history of the concept. In it, he repeatedly points out that all of the things above are part of a well-known flaw in science and the scientific method. We (as scientists) are all too often literally blinded by our knowledge. We teach physics by idealizing it from day one, linearizing it on day two, and forcing students to solve problem after problem of linearized, idealized, contrived stuff literally engineered to teach basic principles. In the process we end up with students that are very well trained and skilled and knowledgeable about those principles, but the price we pay is that they all too often find phenomena that fall outside of their linearized and idealized understanding literally inconceivable. This was the barrier that Chaos theory (one of the latest in the long line of revolutions in physics) had to overcome.

And it still hasn’t fully succeeded. The climate is a highly nonlinear chaotic system. Worse, chaos was discovered by Lorenz [[*Edward Norton Lorenz*](http://en.wikipedia.org/wiki/Edward_Norton_Lorenz)] in the very first computational climate models. Chaos, right down to apparent period doubling, is clearly visible (IMO) in the 5 million year climate record. Chaotic systems, in a chaotic regime, are nearly uncomputable even for very, simple, toy problems — that is the essence of Lorenz’s discovery as his first weather model was crude in the extreme, little more than a toy. What nobody is acknowledging is that current climate models, for all of their computational complexity and enormous size and expense, are still no more than toys, countless orders of magnitude away from the integration scale where we might have some reasonable hope of success. They are being used with gay abandon to generate countless climate trajectories, none of which particularly resemble the climate, and then they are averaged in ways that are an absolute statistical obscenity as if the linearized average of a [Feigenbaum tree](http://en.wikipedia.org/wiki/File:LogisticMap_BifurcationDiagram.png) of chaotic behavior is somehow a good predictor of the behavior of a chaotic system! This isn’t just dumb, it is beyond dumb. It is literally betraying the roots of the entire discipline for manna.

One of the most interesting papers I have to date looked at that was posted on WUWT was the one a year or three ago in which four prominent climate models were applied to a toy “water world” planet, one with no continents, no axial tilt, literally “nothing interesting” happening, with fixed atmospheric chemistry.

The four models — not at all unsurprisingly — converged to four completely different steady state descriptions of the planetary weather.

And — trust me! — there isn’t any good reason to think that if those models were run a million times each that any one of them would generate the same probability distribution of outcomes as any other, or that any of those distributions are in any sense “correct” representations of the actual probability distribution of “planetary climates” or their time evolution trajectories. There are wonderful reasons to think exactly the opposite, since the models are solving the problem at a scale that we know is orders of magnitude~~to~~ [*too*]coarse to succeed in the general realm of integrating chaotic nonlinear coupled systems of PDEs in fluid dynamics.

Metaphor fails me. It’s not like we are ignorant (any more) about general properties of chaotic systems. There is a wealth of knowledge to draw on at this point. We know about period doubling, period three to chaos, we know about fractal dimension, we know about the dangers of projecting dynamics in a very high dimensional space into lower dimensions, linearizing it, and then solving it. It would be a miracle if climate models worked for even ten years, let alone thirty, or fifty, or a hundred.

Here’s the climate model argument in a nutshell. CO2 is a greenhouse gas. Increasing it will without any reasonable doubt cause some warming all things being equal (that is, linearizing the model in our minds before we even begin to write the computation!) The Earth’s climate is clearly at least locally pretty stable, so we’ll start by making this a fundamental principle (stated clearly in [the talk above](https://www.youtube.com/watch?feature=player_embedded&v=EFYZ9dKAuNc)) — The Earth’s Climate is Stable By Default. This requires minimizing or blinding ourselves to any evidence to the contrary, hence the MWP and LIA must go away. Check. This also removes the pesky problem of multiple attractors and the disappearance and appearance of old/new attractors (Lorenz, along with Poincaré [[*Jules Henri Poincaré*](http://en.wikipedia.org/wiki/Henri_Poincar%C3%A9)], coined the very notion of attractors). Hurst-Kolmogorov statistics, punctuated equilibrium, and all the rest is nonlinear and non-deterministic, it has to go away. Check. None of the models therefore exhibit it (but the climate does!). They have been carefully written so that they cannot exhibit it!

Fine, so now we’re down to a single attractor, and it has to both be stable when nothing changes and change, linearly, when underlying driving parameters change. This requires linearizing all of the forcings and trivially coupling all of the feedbacks and then searching hard — as pointed out in the talk, very hard indeed! — for some forlorn and non-robust combination of the forcing parameters, some balance of CO2forcing, aerosol anti-forcing, water vapor feedback, and luck that balances this teetering pen of a system on a metaphorical point and tracks a training set climate for at least some small but carefully selected reference period, naturally, the single period where the balance they discover actually works and one where the climate is actively warming. Since they know that CO2 is the cause, the parameter sets they search around are all centered on “CO2 is the cause” (fixed) plus tweaking the feedbacks until this sort of works.

Now they crank up CO2, and because CO2 is the cause of more warming, they have successfully built a linearized, single attractor system that does not easily admit nonlinear jumps or appearances and disappearances of attractors so that the attractor itself must move monotonically to warmer when CO2 is increasing. They run the model and — gasp! — increasing CO2 makes the whole system warmer!

Now, they haven’t really gotten rid of the pesky attractor problem. They discover when they run the models that in spite of their best efforts they are still chaotic! The models jump all over the place, started with only tiny changes in parametric settings or initial conditions. Sometimes a run just plain cools, in spite of all the additional CO2. Sometimes they heat up and boil over, making Venus Earth and melting the polar caps. The variance they obtain is utterly incorrect, because after all, they balanced the parameter space on a point with opposing forcings in order to reproduce the data in the reference period and one of many prices they have to pay is that the forcings in opposition have the wrong time constants and autocorrelation and the climate attractors are far too shallow, allowing for vast excursions around the old slowly varying attractor instead of selecting a new attractor from the near-infinity of possibilities (one that might well be more efficient at dissipating energy) and favoring its growth at the expense of a far narrower old attractor. But even so, new attractors appear and disappear and instead of getting a prediction of the Earth’s climate they get an irrelevantly wide shotgun blast of possible future climates (that is, as noted above, probably not even distributed correctly, or at least we haven’t the slightest reason to think that it would be). Anyone who looked at an actual computed trajectory would instantly reject it as being a reasonable approximation to the actual climate — variance as much as an order of magnitude too large, wrong time constants, oversensitive to small changes in forcings or discrete events like volcanoes.

So they bring on the final trick. They average over all of these climates. Say what? Each climate is the result of a physics computation. One with horrible and probably wrong approximations galore in the “physics” determining (for example) what clouds do in a cell from one timestep to the next, but at least one can argue that the computation is in fact modeling an actual climate trajectory in a Universe where that physics and scale turned out to be adequate. The average of the many climates is nothing at all. In the short run, this trick is useful in weather forecasting as long as one doesn’t try to use it much longer than the time required for the set of possible trajectories to smear out and cover the phase space to where the mean is no longer meaningful. This is governed by e.g. the Lyupanov exponents of the chaotic processes. For a while, the trajectories form a predictive bundle, and then they diverge and don’t. Bigger better computers, finer grained computations, can extend the time before divergence slowly, but we’re talking at most weeks, even with the best of modern tools.

In the long run, there isn’t the slightest reason — no, not even a fond hope — that this averaging will in any way be predictive of the weather or climate. There is indeed a near certainty that it will not be, as it isn’t in any other chaotic system studied so why should it be so in this one? But hey! The overlarge variance goes away! Now the variance of the average of the trajectories looks to the eye like it isn’t insanely out of scale with the observed variance of the climate, neatly hiding the fact that the individual trajectories are obviously wrong and that you aren’t comparing the output of your model to the real climate at all, you are comparing the average of the output of your model to the real climate when the two are not the same thing!

Incidentally, at this point the assertion that the results of the climate models are determined by physics becomes laughable. If I average over the trajectories observed in a chaotic oscillator, does the result converge to the actual trajectory? Seriously dudes, get a grip!

Oh, sorry, it isn’t quite the final trick. They actually average internally over climate runs, which at least is sort of justifiable as an almost certainly non-convergent sort of Monte Carlo computation of the set of accessible/probable trajectories, even though averaging over the set when the set doesn’t have the right probability distribution of outcomes or variance or internal autocorrelation is a bit pointless, but they end up finding that some of the models actually come out, after all of this, far too close to the actual climate, which sadly is not warming and hence which then makes it all too easy for the public to enquire why, exactly, we’re dropping a few trillion dollars per decade solving a problem that doesn’t exist.

So they then average over all of the average trajectories! That’s right folks, they take some 36 climate models (not the “twenty” erroneously cited in the presentation, I mean come on, get your facts right even if the estimate for the number of independent models in CMIP5 is more like seven). Some of these run absurdly hot, so hot that if you saw even the average model trajectory by itself you would ask why it is being included at all. Others as noted are dangerously close to a reality that — if proven — means that you lose your funding (and then, Walmart looms). So they average them together, and present the resulting line as if that is a “physics based” “projection” of the future climate. Because they keep the absurdly hot, they balance the nearly realistically cool and hide them under a safely rapidly warming “central estimate”, and get the double bonus that by forming the envelope of all of the models they can create a lower bound (and completely, utterly unfounded) “error estimate” that is barely large enough to reach the actual climate trajectory, so far.

Meh. Just Meh. This is actively insulting, an open abuse of the principles of science, logic, and computer modeling all three. The average of failed models is not a successful model. The average of deterministic microtrajectories is not a deterministic microtrajectory. A microtrajectory numerically generated at a scale inadequate to solve a nonlinear chaotic problem is most unlikely to represent anything like the actual microtrajectory of the actual system. And finally, the system itself realizes at most one of the possible future trajectories available to it from initial conditions subject to the butterfly effect that we cannot even accurately measure at the granularity needed to initialize the computation at the inadequate computational scale we can afford to use.

That’s what Goreham didn’t point out in his talk this time — but should. The GCMs are the ultimate shell game, hiding the pea under an avalanche of misapplied statistical reasoning that nobody but some mathematicians and maverick physicists understand well enough to challenge, and they just don’t seem to give a, uh, “flip”. With a few very notable exceptions, of course.

*Rgb*

**Postscript** (from a related [slashdot post](http://slashdot.org/comments.pl?sid=5790561&cid=48073849)):

1° C is what one expects from CO2 forcing at all, with no net feedbacks. It is what one expects as the null hypothesis from the very unbelievably simplest of linearized physical models — one where the current temperature is the result of a crossover in feedback so that any warming produces net cooling, any cooling produces net warming. This sort of crossover is key to stabilizing a linearized physical model (like a harmonic oscillator) — small perturbations have to push one back towards equilibrium, and the net displacement from equilibrium is strictly due to the linear response to the additional driving force. We use this all of the time in introductory physics to show how the only effect of solving a vertical harmonic oscillator in external, uniform gravitational field is to shift the equilibrium down by Δy = mg/k. Precisely the same sort of computation, applied to the climate, suggests that ΔT ≈ 1° C at 600 ppm relative to 300 ppm. The null hypothesis for the climate is that it is similarly locally linearly stable, so that perturbing the climate away from equilibrium either way causes negative feedbacks that push it back to equilibrium. We have no *empirical* foundation for assuming *positive* feedbacks in the vicinity of the local equilibrium — that’s what linearization is all about!

That’s right folks. Climate is what happens over 30+ years of weather, but Hansen and indeed the entire climate research establishment *never bothered to falsify the null hypothesis of simple linear response* before building enormously complex and unwieldy climate models, *building strong positive feedback into those models from the beginning*, working tirelessly to “explain” the single stretch of only ***20*** years in the second half of the 20th century, badly, by balancing the strong feedbacks with a term that was and remains poorly known (aerosols), and asserting that this would be a reliable predictor of future climate.

I personally would argue that historical climate data manifestly a) fail to falsify the null hypothesis; b) strongly support the assertion that the climate is highly *naturally* variable as a chaotic nonlinear highly multivariate system is *expected* to be; and c) that at this point, we have extremely excellent reason to believe that the climate problem is non-computable, quite probably non-computable with any reasonable allocation of computational resources the human species is likely to be able to engineer or afford, even with Moore’s Law, anytime in the next few decades, if Moore’s Law itself doesn’t fail in the meantime. 30 orders of magnitude is 100 doublings — at least half a century. Even then we will face the difficulty if initializing the computation as we are not going to be able to afford to measure the Earth’s microstate on this scale, and we will need theorems in the theory of nonlinear ODEs that I do not believe have yet been proven to have any good reason to think that we will succeed in the meantime with some sort of interpolatory approximation scheme.

rgb

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**Moderation and Author’s Replies Note:** This elevated comment has been posted at the request of several commenters here. It was edited by occasional WUWT contributor Kip Hansen with the author’s approval. Anything added to the comment was denoted in [*square brackets*]. There are only a few corrections of typos shown by ~~strikeout~~[*correction*]. When in doubt, refer to the [original comment here](http://wattsupwiththat.com/2014/10/02/a-rare-debate-on-the-settled-science-of-climate-change/#comment-1753775). RGB is currently teaching at Duke University with a very heavy teaching schedule and may not have time to interact or answer your questions.

# First Response by Brown: "Truth" vs "the best answer"

**rgbatduke**

[October 7, 2014 at 9:20 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756731)

*Science does not care about the truth, it only cares about “the answer that best describes observations.” I would appreciate any thoughts about “truth” versus “the best answer.”*

Curiously, I have written extensively about this It is one of my passions. You can read a draft of my musings at:

<http://www.phy.duke.edu/~rgb/axioms.pdf>

In a nutshell, the reason science doesn’t search for Aristotelian or Platonic **Truth**(note the capital T and boldface!) is because we can never find it. Seriously. This is a very, very serious problem in philosophy. How do we know what is **Truetm** with regard to assertions about the physical Universe? All we have to work with (in the end) is the aggregate information input through our senses into an internal, imperfect, “mental, semantic” Universe!

These sensory impressions of reality are not, themselves, reality. They are a map, a model. Everybody here loves to disrespect modelling, but everything you or I or anyone you know think that they “know” about the Universe that they appear to be living *in* as one tiny but highly organized temporally persistent structure is nothing but a model. Furthermore, that model is all built up by a complex process that mixes*mathematics and logic* (deductive stuff) with *inference* (inductive stuff).

Neither mathematics nor induction can be proven without assumptions, assumptions that are loosely called “axioms” (or “postulates”, or “physical laws” or… hence the title of my book) that cannot, themselves, be proven. Hence the foundation of all human knowledge is, in some sense, rotten in a very deep and profound way at the core. We literally cannot be certain of any of our beliefs but the one Descartes pointed out long ago — that as sentient beings we cannot reasonably doubt our own existences as the act and ability “to doubt” *empirically* contradicts the thing being doubted, until the day we die and it doesn’t. All of this was pointed out by the great Skeptic and empiricist, David Hume, who basically proved that in this deep sense, all of philosophy (but especially the formal philosophy of e.g. Plato, Aristotle, and all of the others who believed that Pure Reason was able to deduce Pure Truths that had to be true independent of any need to establish correspondence between the beliefs and observations of reality) was *bullshit*.

Thus matters stood for at least 200 years. Yes, Kant and many others attempted to overcome Hume’s objections, but they deeply failed, because his objections are*correct*. We have names for them — they are the logical fallacy *post hoc ergo propter hoc* (after a thing, therefore because of a thing), which we say in English as*correlation is not causality* (temporally ordered or not). In the meantime, his conclusions not only became stronger, but the *mathematical* community discovered that even *mathematical* “truths” — things like the theorems of Euclid in plane geometry, which were held up as a shining example of perfect truth that in no way depended on correspondence with reality — were not, actually, perfect truths, they were at best *contingent* truths. Sure, the sum of the angles of a triangle add up to \pi *in a plane*, but what about *curved* space geometries? Change the *axioms of the theory*and you *get a different, equally (contingently) valid theory!* Which one is “true”? Either? Both? Which set of *axioms* are “true”? How can we tell? They are*assumptions*, so at best mathematical and logical reason yields contingent truth predicated upon an unprovable base of assumptions.

Worse, the grand attempt of Hilbert to axiomatize mathematics came to a crashing halt when another paradigm-shifting upstart, a buddy of Einstein’s named Godel, derived a startling theorem in *number theory*, one of the branches of mathematics Hilbert was trying to reduce to the cut and dried. He proved, basically, that one*cannot fully axiomatize* any mathematical theory complex enough to include *simple arithmetic*. Geometry made the cut — one can axiomatize geometry. *Almost everything else, including number-based science, did not!* That is, not only can we not arrive at Perfect Truths about the Universe *practically*, as finite imperfect inference engines operating through a sensory filter with a limited perception and range and resolution, we cannot even arrive at them *in principle*. As soon as the Universe passes a certain critical threshold in its complexity, it becomes in some sense*unknowable*, which is quite relevant to the remarks above on chaos and nonlinearity arising from internal feedbacks within dynamical systems in general. All of this is described in lovely detail in Morris Kline’s book *Mathematics, the Failure of Certainty*.

Thus matters stood until the mid-1940s, when two people working on quite distinct problems made *another* startling discovery, or perhaps “invention” is a better word. Physicist Richard Cox was trying to come up with a clear axiomatic basis for the Gibbs prescription in statistical mechanics, which is basically the way we form a picture of not the “certain” evolution but the *most probable evolution* of systems of many, many particles in physics. This is, in fact, the problem climate science is trying to solve, but at the time the greater concern was simply explaining equations of states of liquids, solids, gases, understanding phase transitions and critical phenomena — the idea of computing something as complex as a turbulent fluid was foreign to nearly all physicists, and the few who attempted it got the answer horribly, horribly wrong (although it took decades to figure that out). Cox proposed three very simple, “common sense” axioms from which a *Theory of Probable Inference* followed — you can see how it works if you buy his lovely monograph on the subject. From this theory, statistical mechanics could be *axiomatically described*.

Almost immediately after this, a researcher named Claude Shannon working for Bell Labs invented a mathematical description of the way information degrades when being transmitted on a noisy transmission channel — a theory with profound and immediate practical benefit, but one that has obvious connections to our own ability to make inferences about the outside Universe via information received through the*noisy transmission channels* of our senses and internal brain structures. His invention of *Information Theory* turned out to fundamentally be equivalent to the axiomatic theory of Cox — one can be derived from the other, and both provide a sound basis for the concept of *entropy* as the *natural log of the missing information*, the information lost when averaging a system from the microscopic to the macroscopic level.

A third physicist, E. T. Jaynes, realized that these two theories were the same, and that they had a profound consequence. The “logic” of science, and indeed the *basis of all ontology and epistemology*, is not the Boolean/Aristotelian algebra of True/False dichotomy, it is *Bayesian probability theory* the algebraic theory of *soft logic*originally described by Laplace, written out by George Boole in his fundamental book on logic and reason but *without proof* (since he lacked the axioms to prove it from a common sense foundation), and (perhaps surprisingly) developed by a number of people include John Maynard Keynes who were writing treatises on probability theory from the point of view of *maximum likelihood estimates* that essentially embody *maximum entropy* as a guiding principle without ever naming it or making the connection to physics and science in general.

Cox and Jaynes also realized that the Cox axioms *finally resolved the problem of David Hume* and put the theory of knowledge on a sound basis. The proper basis of knowledge isn’t the logical positivism where meaning comes from the ability to be *empirically proven* (an assertion that is in fact inconsistent as it cannot be empirically proven) or the notion of *falsification* advanced by Popper and others to repair some of the inconsistencies — it is probability theory wedded to skepticism at a very deep level. We can never **prove** any non-contradictory proposition about the real world to be *unconditionally true* or *unconditionally false*. The best we can do is demonstrate a kind of *consistency* in a process of iterative recomputation of posterior probabilities of a model as it is compared to data, to evidence. Positive evidence increases our degree of belief. Negative evidence decreases our degree of belief. Over time agreement with positive evidence increases our degree of belief to*near* certainty, but the limit of “unquestionably true” cannot be reached by any amount of finite evidence. Over time negative evidence (which is very broadly interpretable!) can and should decrease our degree of belief to *near* certainty that the belief is false, but we can never be *absolutely* certain of that because our entire system of beliefs about the Universe Itself could be false — we cannot be certain of the Bayesian priors we are using to interpret the evidence, however consistently they appear to be working and however high their posterior probabilities as we add more data.

All of this can be boiled down (by me:-) into a pithy little English language sound bite rule that one can use as the basis of all epistemology and worldview building, an embodiment of the Cox axioms and Jaynes prescription suitable for use in everyday life:

*It is best to believe that which you can doubt the least, given the evidence and the entire network of coupled, evidence supported best beliefs, you have* ***so far****!*

Note well that this is a dynamic, iterative prescription. You should doubt *everything*. Somebody says “God exists!” You should doubt this! Give it a try. Somebody says “God exists and is male and His name is Allah and…(etc)!” You should doubt this more, mathematically, because the probability of a conjunction of probabilities is their product, so the probable truth of this statement is *strictly less than* the probable truth of a single one of its components. Ockham’s razor is thus automatic — more complex propositions are less probable and require more empirical support to raise to a high degree of belief. In all cases, the *sole* valid basis for increasing degree of belief is hard, unbiased, empirical evidence. You may think God is a lovely idea — just as Plato thought that Spheres and Forms were lovely ideas, as Einstein thought that certain geometries were lovely ideas — but the beauty of an idea is not the same thing as objective unbiased (e.g. double blind) empirical support. Even personal “experience” of God is not to be trusted, at least not very far, because most of us have lived long enough to mistrust our own brains and perceptions and this sort of experience is far too easily explainable by alternative hypotheses for the “evidence” to have much resolution, even more easily doubtable as we increase the specificity of our belief system.

This is even a moral system. It asserts pure common sense. Of course we should believe that which we can doubt the least. What is immoral, in some deep sense, is to promulgate easily doubtable, uncertain assertions as *probable truth* when they are in direct contradiction of existing knowledge and evidence, or when it is simply the case that the evidence isn’t sufficient to advance belief much *either way*.

So this is what your friend probably means — science isn’t a search for **Truth**. It is a search for **that which it is best to believe, given the evidence, so far**. Hope that this helps. It also will help you understand the way physicists temper their “beliefs” in even such well-demonstrated things as gravity, let along things like the Higgs boson or photons or magnetic monopoles. We believe in them quite strongly (well, not so much with monopoles:-) *when and as evidence supports the belief*, but until then we treat them as plausibly true *conjectures* and afterwards we consider them to be part of a *working model for the theory of everything* that itself is subject to any revision required by new evidence and lines of reasoning as they accrue. We know better than to get attached to things like Classical Mechanics or its functional replacement, Quantum Mechanics, or even to good old gravity. All too often in the past, these sorts of things have been resoundingly proven *almost certainly false* after being strongly believed for a very long time.

rgb

# Second response by Brown: Is mathematics a consistent tautology

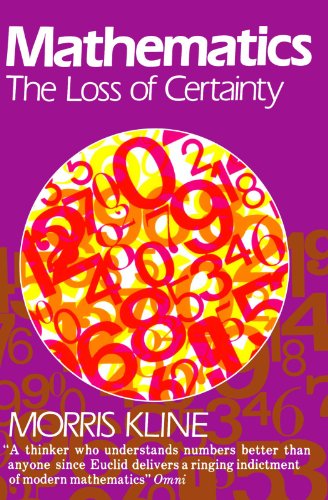
* **rgbatduke**

[October 9, 2014 at 6:17 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1758088)

*You gave a good description of scientific truth. Mathematical truth is a different beast. It has certainty constrained by well-defined conditions. Wittgenstein said mathematics is a consistent tautology. Unlike science, it is neither empirical nor probabilistic.*

Dear Dr. Strangelove,

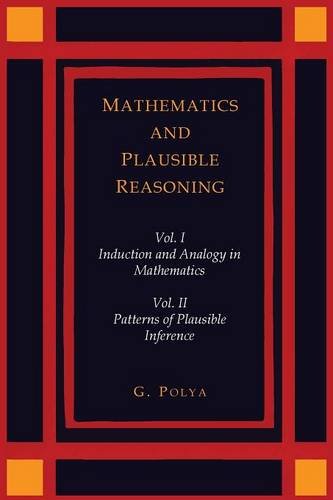
I would strongly recommend:

[](http://www.amazon.com/Mathematics-Loss-Certainty-Oxford-Paperbacks/dp/0195030850)

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The first reviews the history of mathematics to demonstrate the startling discovery — that Wittgenstein, Hilbert, Whitehead, and even Russell got *wrong*— is not, in fact certain. It is, as I said, at best a kind of *contingent* truth. Specifically, we can formulate any mathematical theory as a sort of *modus ponens*: **If** the following set of unprovable axioms/propositions are assigned a value of “True”, **then** the following set of contingent propositions (theorems) can be proven to also be “True”. The resulting set of definitions (another kind of proposition), axioms, and proven theorems constitutes a “theory”.

But who dictates the truth of the original propositions? How can they be proven? And for that matter, how did you end up making up *that* set of propositions and proving the *particular* set of interconnected theorems that make up your theory? How many theorems are there? Can the theory be shown to be *complete* (where all true theorems can be proven)? Can it be shown to be *consistent* (specifically, can it prove its own consistency *within the theory* to avoid the need for a recursive chain of reason with no end)?

The answer is unsurprisingly “nobody” for the first, hence mathematics is at best a kind of consistent fantasy, a **contingent** consistent tautology. The use of the term “true” in the above is also a bit odd, and we should really replace it in our minds with something different, as it *means* provable from axioms, not *true* as in *one to one correspondence with something possessing the existential property, something objectively real!* The invention/discovery of curved space geometry as a secondary consistent set of “perfect truth” to add to Euclidean geometry was a paradigm shift that was as philosophically crushing to the philosophers of its day as any revolution in physics (including Einsteins, which basically said not only are non-Euclidean geometries mathematically consistent theories, they are theories that are in a better correspondence with *observational reality than Eucliden geometries*, a point quantum mechanics drove home with a vengeance. I will skip a digression into Hilbert Space, especially Hilbert space supported on a manifold, or conformal topology.

The answer to the second set of questions is:

<http://en.wikipedia.org/wiki/G%C3%B6del%27s_incompleteness_theorems>

Basically, no you cannot ever, even in principle, prove all the theorems in a theory with anything but the most trivial set of axioms (e.g. plane geometry is OK, arithmetic is not). And no, you cannot prove a theory to be consistent within the theory. Wittgenstein’s assertion is simply incorrect. Any theory more complex than arithmetic is *not even* a set of (contingent) tautologies, because it cannot demonstrate its own consistency. Worse, if one *can* internally demonstrate the consistency of some theory within the theory, *it is certainly false!*

Mathematicians have invented an elaborate apparatus to deal with the requisite layering of reasoning these theorems have brought about — first and second order logic, for example, because one cannot prove the consistency of first order logic within first order logic, but one can prove first order logic with *second* order logic. Self-referentiality is a big part of the problem — as soon as a theory becomes complex enough to make assertions about itself, you are almost certainly in trouble.

The second book by Polya is a classic and timeless contribution to the theory of mathematical reasoning. In it, Polya points out, tirelessly and with countless examples, that *inference and induction are alive and well and at work in almost all non-trivial mathematical developments and discoveries over recorded history!* We tend to think that mathematics works by brilliant minds coming up with a set of axioms that somehow descend from the heavens already inscribed on stone tablets with some sort of certificate of truth, so much so that the very *word* axiom is often taught to the ignorant to mean “a proposition whose truth is so plain as ***not to need*** any further examination or explication”, that is, **self-evident truth**. The *empirical* truth is that nothing of the sort happens.

Mathematicians discover things the same way physicists do, but in a quasi-empirical playground. Number theories observe certain regularities by *playing with numbers*. For example, <http://en.wikipedia.org/wiki/Goldbach%27s_conjecture>. Or Fermat’s infamous last theorem. These conjectures are found to be *empirically* true long, long before they are proven. They may not be *provable* (Godel states that number theory contains an infinite number of true but unprovable theorems and Goldbach might be one of them!) Geometry is no better — the truths about triangles and straight lines were without any doubt *observed* by Euclid before he was able to invent the reasoning process by which he *proved* them, many of them observed and/or proven by the Pythagoreans. That is not to minimize the power of axiomatic reasoning once one *does* infer a set of axioms capable of deriving truths — the discovery of irrational numbers being an unexpected cautionary tale in that regard. Sometimes you get what you do *not* expect, because our ability to “see” things or even imagine them is so very limited.

Rgb

# Third comment by Brown –stupidity of spending billions on the the precautionary principle without assessing value for money

* **rgbatduke**

[October 7, 2014 at 9:42 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756748)

Which is not an answer to the *real* question: Would you bet $250 billion a year on it and condemn literally millions of people to a horrible death from preventable poverty and disease and starvation due to inflated energy prices and misdirected elective global national product on “the best we can do with the tools that we have” when we lack even a *theoretical* reason to believe that the tools are *capable* of solving the problem in a meaningful way?

Because that’s the way the bet is going down right now. More people have died, worldwide, as a direct consequence of our *response to the threat* of “catastrophic” future global warming than have died from global warming itself. Many orders of magnitude more people. It’s difficult to attribute a single specific death to “global warming”, especially given the tenuous basis used as the WHO projections. It is bone simple to attribute them to the consequent increases in energy costs and subsequent inflation in a society with utterly inelastic energy requirements.

rgb

# Fourth comment by Brown – Essex on non-linear systems and chaos

**EdA the New Yorker**

[October 7, 2014 at 9:03 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756709)

EdA the New.Yorker

Professor Brown has outdone his usual lucidity here, but may I suggest the alternative route of having your friends first spend a very pleasant hour with Prof. Christopher Essex at [http://m.YouTube.com/watch?v=hvhipLNeda4](http://m.youtube.com/watch?v=hvhipLNeda4) for a lighter treatment, then email them RGB’s article?

[Reply](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/?replytocom=1756709#respond)

* + **rgbatduke**

[October 7, 2014 at 9:48 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756757)

This is a brilliant treatment as well. Essex is spot on the money regarding nonlinear systems and chaos. Shame nobody pays the slightest attention.

rgb

# Fifth comment by Brown – data mining without introspection

**rgbatduke**

[October 7, 2014 at 1:39 pm](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756928)

Yeah, and as an incidental side effect you’d destroy the entire US system of higher education, top to bottom. The research money basically subsidizes *both* the research (which often does have some direct payback value to the taxpayer, a ton of indirect payback, and which does, without question probably need some rethinking) *and* the institution itself so that it can teach your kids for a lot less than it would be otherwise (and it’s already bad enough).

Bear in mind that *most* of science — even a lot of climate science — is done with integrity and so on. The climate scientists who publish forecasts/prophecies of doom and disaster truly do believe it, they really do think they are Saving the Worldtm one coal burning plant at a time. There only real sin is not allowing the idea that *they could be wrong* to take root in their mind. Without that, life even for an honest research scientist who really *does* appreciate data becomes one long bout of data dredging.

The exact same thing leads to the exact same thing, over and over again, in medical research, especially large scale, broad epidemiological studies. You’ve got a big database with (say) 100,000 people in it. You split it up into clusters (this is something I know a great deal about, BTW) — groups of people who all Ate Their Wheaties every morning *and* are over 70 *and* are female, etc, and look to see if the rates of “cancer” are higher in this sub-population. Perhaps you look, and find that Female Wheatie-Eaters Over 70 have a surplus rate of cancer of the big toe that is in the 99th percentile relative to the population as a whole. Oh My, you say, *Wheaties cause cancer!* Note that you can state this with tremendous confidence! 99th percentile!

Or *can* you? Not at all. After all, you weren’t looking specifically for cancer of the big toe, you were looking for *any* cancer. There are hundreds of them! Given hundreds to look for, not just one but *several* cancers are likely to be present in rates in both the top *and* the bottom 1%. Indeed, looking more closely, you discover that you could just as easily have stated that *Wheaties prevent cancer!*, of the thumb, because rates for *this* cancer are in the *bottom* 1%.

To correct for this one has to use an arcane correction that basically says that in a data dredge situation, all of the usually inferred percentiles and probabilities have to be so diluted as to become nearly meaningless, because it isn’t even *surprising*that one or two cancers would turn up somewhere in the top or bottom percent, it is nearly a sure thing, *even if there is absolutely no actual physical causal relationship between Wheaties and any sort of cancer!*

If there *is* one, so much the worse. You may or may not be able to resolve it in an epidemiological population study, ever!

That’s because of a second problem. Confounding variables. Even if you found a connection between Eating Wheaties and cancer of the big toe that survived the Bonferroni correction and still gave you a p-value of 0.001 or something, you cannot be *certain* that the cancer isn’t *really* caused by something else, some trait not revealed on the population study survey that is *shared* by Wheatie Eaters. For example, nobody eats their Wheaties dry, they pour on the milk. Milk comes from cows that have been force fed massive amounts of estrogen and antibiotics and that are pastured on a landfill covering an old toxic waste dump. The people who love Wheaties all live under high voltage transmission lines (remember that nonsense?) and talk on their cell phones a lot (that’s nonsense that is still going on). None of that, however is on the questionnaire. It could even be that *having cancer of the big toe makes one more inclined to eat Wheaties!* All you know is that there is a *possibly* statistically significant correlation between the two, not causality.

In medicine, this problem is so routine and expected that they have strict rules and procedures that almost always kill any claims with an exiguous foundation, or at least force anyone asserting such a claim to tone the rhetoric way the hell down. The gold standard there is the *double blind, placebo controlled study* with careful control for confounding variables and inverted causality (as best as one can with a probably now *inadequate population* — the one place where the broad population studies shine). Even so, which is better for you: butter or margarine? Canola oil or Olive oil? Oat bran or wheat bran? Aspirin or Acetaminophen? Sugar or fat in general? Atkins or the Mediterranean diet? Statins or diet and exercise?

In all cases it is really surprisingly difficult to say, because you can find historical periods where physicians and medical people would have been inclined to answer either one. Even now a lot would answer either way. And the answer in most cases would be conditional — aspirin in low doses is *good* for old people (so they say, with good data support) but it can *kill young people*. Tylenol is simply great — unless you overdose or your liver is boxed because you are an alcoholic or drug abuser or have had hepatitis. Saturated fats are the devil, butter bad margarine good (we all grew up to that one). Now it is *precisely reversed!* butter and ice cream in moderation are just fine, it is the *trans fats* in margarine that are the devil, and it is the sugar in the ice cream that is sketchy, not the fat per se.

In no case is it at all likely that the physicians who published the original stuff claiming the opposite were malicious or had stock in major margarine manufacturers. They simply had a theory, did a broad study, found a subpopulation where their theory was validated, and because they thought they *understood* it and *had data* that their conclusion was true.

Oops.

In climate science there simply isn’t any such safeguard. There is its opposite. There is a feeding frenzy. Climate science is the universally relevant addendum that can only improve nearly any proposal. Whether you are studying the mating habits of the rhinoceros beetle or the lifestyle of the elusive giant squid in the deep ocean, it can never hurt to add the phrase “to attempt to resolve the impact of expected*anthropogenic* climate changes on X because X is a potential human resource” where X is whatever you want to study for real. It hits the human benefit and human guilt button *at the same time!* What’s not to like?

And what is the basis for that expectation? Climate models. Well, climate models and the ~1 C warming expected from the equivalent of shining *a single flashlight onto the ground from a height of around 1 meter* 24×7. Good luck staying warm with nothing but your flashlight if you are lost in the north woods, of course, but from the way it is presented you could think we could use flashlights to sear our meat to keep in juices before grilling it.

Why is there supposed to be a 1-6 meter SLR by 2100 (even though the greatest rate of SLR in the entire tide gauge record is around 1.25 inches *per decade*, and that rate of rise occurred back in the *1930s!* before the advent of anthropogenic-attributed CO\_2? Because the *climate models say so!*

That’s why there is so much furor, so much sound and fury, in the climate science community at the moment. They are trying, rather desperately, to lock in an international commitment to their storyline *before it is too late* because even *they*have to secretly suspect that as we hit the serious downside of the current solar cycle, temperatures will *at best* remain rather flat, and if the currently anaemic Nino truly does fizzle, or worse, goes back to a strong *Nina* and we have a really cold northern hemisphere winter at or near the *peak* of the double bump solar cycle, and if northern polar ice continues its healthy recovery while Antarctic ice continues to set records, temperatures might even *gasp* drop! And if they drop *even a bit*, for (say) the five plus years of solar *minimum*, they will never, ever convince anyone to fund them ever again.

In fact, if the temperature actually drops at all, ever, there will probably be congressional investigations. If TCS continues to drop that could happen even without an actual drop in temperature — Congress might well want to know exactly*why* we’ve burned a few hundred billion dollars on ameliorating CO\_2 if TCS is going to end up being feedback neutral after all. And it’s not that the prediction, sorry, “projection” turned out wrong that is the problem — they would want to know *why they weren’t told of the substantial uncertainty in the unproven projections and the horrendous abuse of the principles of statistics that went into the assertions of “confidence” in those projections!*

I wouldn’t blame them. I’m kinda curious myself…

rgb

# Sixth and seventh comments by Brown – why the IPCC does not throw out the worst (30) of the 36 models it uses

* + **rgbatduke**

[October 9, 2014 at 6:44 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1758100)

*It is very sad that they get away with not disclosing the “under the hood” facts about their computer models.*

It’s worse than that. They openly disclose them — in chapter **9** of AR5, in a single three or four paragraph stretch that nobody who matters will ever read, or understand if they do read. Then they write *arrant nonsense* in the Summary for Policy Makers, disclosing *none* of the indefensible statistical inconsistency of their processing of the individually failing models before using them as the basis for the *personal opinion of a few, carefully selected writers, expressed as “confidence”* about many things that they could not quantitatively defend if their lives depended on it using the axioms and practice of statistics on behalf of the whole body of participants, including those that don’t agree *at all* with the stated conclusions and who would utterly reject the assertions of confidence as having a statistical/empirical foundation.

As in *high confidence my ass!* “Confidence” in statistics is a term with a fairly specific meaning in terms of *p-values!* You show me the *computations* of one, single p-value, and explain its theoretical justification in terms of (say) the Central Limit Theorem. *Be sure to list all of the Bayesian priors* so we can subject them to a posterior analysis! Be sure to *explain* to the policy makers that forming the mean of the models is pure voodoo, disguising their weakness by presenting the conclusions as the result of a *vote* (of uncritically selected idiots!) In the SPM — not in chapter 9.

rgb

* **rgbatduke**

[October 7, 2014 at 1:45 pm](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756940)

Yeah, Jimbo. Sheer common sense. And why don’t they just throw out the worst (say) thirty of the thirty six models when they are making their projections?

The answer is, of course, pretty obvious. But it is a sad, sad answer. It doesn’t even*have* to be malicious. These guys are all colleagues. Who wants to be the one who has to go to talk to Joe, the author of the 36th worst model (dead last, not even close!) and tell them that regretfully they’ve decided to drop it out of all of the projections and predictions in AR5, or AR6, or whatever? It’s a career ender for Joe.

I’m not sure *even* Wal Mart could employ the vast horde of future unemployed climate scientists that will be unleashed on the market if the climate starts to actively cool, or even remains flat for another decade before changing again up *or*down.

rgb

# Eight included comment – Central tendency is ice ball Earth

**rgbatduke**

[October 8, 2014 at 4:45 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1757289)

*Over the longer term the problem is that the paleo record indicates the most stable pattern of Earthly climate is an ice ball. If RGB is correct (and I’m inclined to suppose he is) then the direction of a linear forcing to our chaotic system provides absolutely no clue about which way the system will respond. We may reach a tipping point where radiative warming kicks off catastrophic cooling. Models can’t say. The precautionary principle then requires us to invest against the risk of either dramatic cooling or dramatic warmth, from ANY forcing, whether anthropogenic, astronomical, geological, or any combination. And such investments must bear in mind that cooling is vastly more dangerous and harder to revoke than warming.*

Precisely correct. For the last 600,000 to 1,000,000 years, the Earth has spent roughly 90% of its geo time as an iceball, 90,000 years in, 10,000 years out. During the out bit the interglacial temperatures have equalled or exceed the present — in the last (the Eemian IIRC) there was a peak *substantially* — 1 to 2 C — warmer. Those Atlanteans and their burning of fossil fuels, I guess.

But that’s the thing you have to watch a chaotic, or for that matter a merely bistable system do — switch attractors — to believe it, to see the ultimate downfall of linear thinking. The thing is, the motion is best described as an *orbit*, and orbits do not behave intuitively. For example, how do you catch up to somebody who is ahead of you in an orbit? If you simply speed up in their general direction, you move *out*, to a higher orbit, not forward in the orbit you are in. In a multiattractor model, increasing the orbit simply makes it more likely that you will jump attractors. But — and this is a point I’ve made repeatedly in discussions with people regarding the models currently being used — the current models don’t really *have* multiple attractors.

Here’s a simple challenge for the modellers. Run the models through with known orbital variations and demonstrate that they correctly track the actual climate observed/inferred from the paleo proxy record, that is, the precise track of the Pliestocene, with its gradually deepening and variable period of glaciation. This should be *easy* — and should be the *first* thing done with the models to ensure that they have the *principle* feedbacks, the ones responsible for critical instabilities, properly represented in their models. I say “easy” because hey, it isn’t even full blown chaos, its just quasi-periodicity, a bit of fractal stuff going on. Once the models can fairly precisely reproduce the last 4 million years of obviously structured climate variation, one might believe that they have the essential physics correct “enough” to predict the dynamics of the attractors themselves as underlying driving undergoes some very simple time evolution. They might then be trusted to predict (for example) how close we are to the next glacial transition, instead of this being yet another example of reading sheep entrails in climate science instead of developing a soundly predictive theory before making assertions in public.

Of course they have tried to do this, but they don’t have anything like an a priori model that can. What they have managed is to tweak the parameters and so on to where they can model some limited part of this, but things like why the glaciation is increasing are as mysterious as why the Ordovician-Silurian glaciation happened with CO\_2 over 10x today’s levels. The truly *paleo* climate record — over the last 600*million* years — is bizarre as all hell. Warmer, cooler, hothouse, icehouse — and *most of that time* the CO\_2 levels were ballpark 1000 ppm or higher. It is only in the Pliestocene that the levels have fallen as low as they where pre-industrially, or as critically low as they were in the last (Wisconsin) glaciation, when they went down to where mass extinction of many plant species was a real possibility.

In my opinion, for whatever that is worth, I suspect that the additions of CO\_2 to this point have been almost entirely beneficial, restoring a balance that has gradually been lost. But I can’t solve Navier-Stokes equations in *my* head either. The wisest course is clearly *not* to add CO\_2 to the atmosphere in huge quantities because we *don’t* really know what it will do, any more than we know how the climate would (have) evolve(d) if we hadn’t added any at all. I do not trust in a divine providence — the Earth could be critically unstable already for the glacial transition after 12,000 warm interglacial years.

What I do *not* see in the climate record is any obvious signs that — given the current configuration of continents and thermohaline circulation — the Earth is overtly *tri*-stable, with a substantially *warmer* stable phase lurking. That doesn’t mean one could not emerge. That’s what attractors do in turbulent systems — emerge as the driving changes.

rgb

# Ninth Comment – Milton Friedman on methodology

**kakatoa**

[October 7, 2014 at 10:13 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1756782)

It sounds like Dr. Brown would approve of Milton Friedman’s thoughts on theories and their usefulness:

“The relevant question to ask about the ‘assumptions’ of a theory is not whether they are descriptively ‘realistic,’ for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions.”  
-Milton Friedman, 1953

[Reply](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/?replytocom=1756782#respond)

* + **rgbatduke**

[October 9, 2014 at 7:47 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1758157)

Absolutely. Although this is hardly original to Friedman. I had the privilege of seeing Friedman give a talk oh, thirty or forty years ago at Duke. Brilliant man.

rgb

# 10th– Occam's razor, the null hypothesis

**rgbatduke**

[October 8, 2014 at 2:51 pm](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1757655)

Dear Steven,

As I work through below, here’s a skeptic’s model of global warming. The direct warming we can expect in a no-feedback model of forcing *only* from the increase in CO\_2 concentration itself is:

\Delta T = 1.9 \ln(P/P_0)

in degrees C. This corresponds to a TCS of 1.3 C over a doubling of atmospheric CO\_2, and is directly supported by physical analysis of the atmospheric radiative effect. If you feel uncomfortable with a no-feedback model, feel free to include an additional term, and/or a noise term, but beware of multiplication of parameters. My model is a *no* parameter model using actual estimations of CO\_2 driven forcing, although I admit that the number 1.9 is optimized (from the middle of the accepted range).

This model predicts the temperature anomaly from 1944 to 2014 to within about 20%, that is, within around 0.1 C. Well, actually it predicts the *total* anomaly to within *0%* — it is basically exactly dead on — but the curve itself deviates by as much as 20-25% on the way up. This sort of deviation is obviously not significant, of course — this is a no-parameter physical model based on pure physics and the principle of ignorance. Since we cannot reasonably compute, estimate, or measure the feedbacks, since the climate system is manifestly reasonably stable, the best a priori assumption for them is *to ignore them until* ***the data*** *forces us to consider them!*

We obviously aren’t there yet.

Note well, this is an honest climate model for the temperature anomaly — especially for the anomaly, as the exact same reasoning that makes statisticians focus on the anomaly instead of an absolute temperature they can’t measure or estimate to within a whole degree C — because one can look at the expected *change* without knowing the details affecting the interior as long as those details are essentially unbiased ignorance.

Where, exactly, is there room in this for substantial positive feedback? Where is there room for any feedback at all?

If you want to assert natural variability, well, I agree. We agree then that all climate models badly underestimate it, because my model then represents the *lower theoretical bound* of positive feedback CO\_2 only warming and it is spot on with the data. The only way you get to invoke strong positive feedback is by invoking equally strong, and cancelling, natural variation to *leave* us with this lower bound CO\_2 only warming. Obviously this is formally less likely as it requires a fortuitous cancellation and Bayes hates that, but feel free to play through.

This is the *null hypothesis model* for CO\_2-driven AGW. If we double CO\_2 to 600 ppm, we can expect 1.3 – 0.5 = 0.8 C of further AGW, plus or minus whatever non-resolvable “noise” contributed by natural variations of about the same century-long scale and any feedbacks the model is unable to resolve because of commensurate noise.

rgb

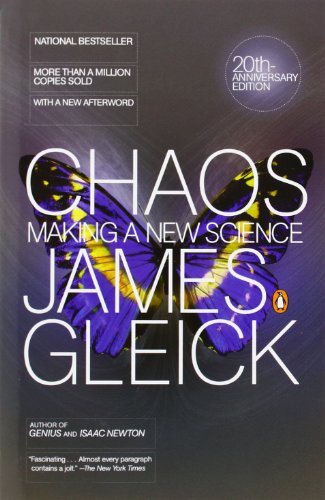
# 11th – Chaotic systems

**rgbatduke**

[October 8, 2014 at 2:20 pm](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1757616)

*I appreciate rgb’s critique of consensus science but have been puzzled why critics have deigned to not come up with better science. The chronic excuses are that processes are irreversible, nonlinear, chaotic … and beyond our abilities.*

Oh my! So from this point of view, physicists are offering *excuses* for being unable to integrate chaotic systems indefinitely into the future at infinite precision? Look, you really, really need to buy a copy of Gleick’s book:

[](http://www.amazon.com/Chaos-Making-Science-James-Gleick/dp/0143113453)

[Chaos: Making a New Science](http://www.amazon.com/Chaos-Making-Science-James-Gleick/dp/0143113453" \t "_blank)

[Buy from Amazon](http://www.amazon.com/Chaos-Making-Science-James-Gleick/dp/0143113453)

and read it. It’s perfectly accessible to a lay person. Maybe then you will know why your reply here is really amazingly funny. Sort of like saying “I have been puzzled why it is taking physicists so long to master FTL travel so we can all build starships and spread out across the Universe. The chronic excuses are that FTL travel violates a half dozen physical and mathematical principles .. and is beyond our abilities.” Or “I don’t know why those computer scientists haven’t managed to come up with P solutions to NP-complete problems. Their chronic excuse is that P is probably not equal to NP so that doing so is … beyond our abilities.”

Seriously, it isn’t that skeptics are *lazy*, as you seem to imply. It is that the solution that is being sold as trustworthy by the crowd of True Climate Model Believers almost certainly cannot be solved at all in the way they are trying to solve it. Other replies down below indicate why — it’s the bit in the top article about “30 orders of magnitude”. This breaks down as follows:

1 mm — approximate Kolmogorov scale for viscosity in air, the scale of the smallest eddies that are relevant in relaxation processes in turbulent air, the scale of the fluctuations that nucleate the self-organization of larger scale structures.

10^{-3}/340 \approx 3 \times 10^{-6} seconds — the time required for sound to travel 1 mm.

The spatiotemporal scale at which we might reasonably be able to solve an actual physics computation of the relevant microscopic dynamics:

Cover the Earth’s atmosphere in cubes roughly 1mm across (smaller than the K-scale is OK, larger not so much). Integrate in timesteps of roughly 1 microsecond. Don’t forget to do the same thing to the ocean. Somewhere along the way, work out how to do the radiative dynamics when the mean free path of LWIR photons is order of 1 meter, hence directly statistically integrable on this grid with minimal approximation.

The spatiotemporal scale being used:

100km x 100 km x 1 km. That is, each grid cell in the higher grid resolution models contains 10^8 x 10 ^8 x 10^6 = 10^22 cubic millimeters.

5 minutes = 300 seconds: The approximate time for sound to propagate 100 km (they simply ignore the vertical direction and hence the model is non-physical vertically and will not correctly describe vertical transport or relaxation processes).

Note that 300/3 x 10^{-6} = 10^8, and 10^8\*10^22 = 10^30.

I wasn’t kidding. The computations are being carried out at a scale 10^30 times away from the scale where we *might* actually expect the solution to *work*. Although with nonlinear dynamics, it might need to be finer than this.

Note further. In timesteps of 10^{-6} seconds, to advance the solution 30 years requires roughly 10^15 timesteps. If it took only a 1 microsecond or actual compute time to advance the solution on the entire grid by 1 timestep, it would still take 1 billion seconds to complete, which is (wait for it) 30 years! That is, to *beat the Earth*to the solution we’d have to complete a computational timestep in *less time* than the time the timestep represents in physical units and the integrated dynamics. And if you think a timestep would take a computational microsecond, I have some real estate you might like to look at in New York, a big bridge, y’know…

As for why you need this granularity, it is because there are *structures* that end up being *important* to the energy flow in the system that are simply erased by too coarse a computational scale. At best one has to make a blind guess for their integrated effect and hope that the resulting **mean field theory** works.

But a nonlinear chaotic mean field theory that works is very, very close to being a pure mathematical/physical oxymoron. The discovery of chaos was the discover that mean field theory *breaks down* in systems with sufficient complexity. It is the moral equivalent of the discovery of Godel’s theorem in mathematics and logic — it means that people (like Hilbert) who fail to axiomatize all of mathematics don’t fail because they are **lazy**, they fail because **it cannot be done**. It doesn’t matter how hard you try. You might as well try to solve NP-complete problems in P time, or invent a physical system that violates the second law of thermodynamics. At the*very least*, the onus of proof is *entirely* on anyone claiming to have discovered a way to integrate a mean-field climate model that works to prove it, both with some actual plausible mathematical arguments (which can begin by explaining why you succeeded in this insanely difficult problem when nobody else has succeeded with mere *toy* problems that are far simpler) and by — you bet — demonstrating predictive skill.

But let me **rise to the challenge**. Here it is, the moment you’ve been waiting for:

**The Skeptics’ Global Warming Model!**

(fanfare, please)

Start with any of the many, many papers that estimate the direct radiative warming one expects from CO\_2 only. There’s a nice paper in the American Journal of Physics — a teaching journal — in 2012 by Wilson that presents a nice summary of the literature and the results of simple Modtran computations. I can do no better than to quote the authors near the end of the paper:

*…gives a no-feedback climate sensitivity of about 0.9 K.*

We’ll use a value of 1 C as the no-feedback warming to be expected from doubling CO\_2 from 300 ppm to 600 ppm. Let us now build *the simplest possible model!* It is known

that the atmospheric warming this represents is logarithmic in the CO\_2 content. That is, we expect to get a temperature anomaly of \Delta T = A \ln(P/P_0) for any given partial pressure of CO\_2 relative to the reference partial pressure at the start. We can set the constant by using the no-feedback expected doubling:

1 C = A \ln(2)

which is really convenient because then I can write:

\Delta T = (1.0/\ln(2))*\ln(P/P_0) \approx 1.44 \ln(P/P_0)

in centigrade. This is my climate model. All of it. My basic assumption is that CO\_2 is a greenhouse gas. It is expected to cause a direct radiative warming of the earth as its concentration increases. The Earth climate system is *stable*, so I’m going to make a *linear response* hypothesis — on average, in the vicinity of equilibrium, the response to any *small* perturbation is to try to restore equilibrium. The additional forcing is over an order of magnitude smaller than the annual variation in forcing due to the Earth’s eccentric orbit, well over two orders of magnitude smaller than the TOA insolation, and far smaller than the *daily* or even *hourly* variations with e.g. cloud, snow and ice cover, or variations due to water vapor (the *important* GHG).

I cannot solve the Navier-Stokes equations for the planetary climate system in any believable way. Nobody can. The simplest assumption is therefore that the feedbacks from *the entire collective system* are neutral, neither strongly positive nor strongly negative. Any other result would cause the system to nonlinearly and cumulatively respond to the near infinity of *natural* forcings in a biased way, which would result in a biased random walk (or heck, a plain old random walk) and the system would be *unstable* to its own noise, like a loudspeaker turned up too high. We could hardly miss such a thing, were it to occur — it would look like a plunge into glaciation or “overnight” emergence from glaciation.

So let’s compare my climate model to the data. HADCRUT4 on WFT clearly shows 0.5C warming from the general decade 1940-1950 (to avoid cherrypicking an end point, and at appropriate precision) up to the general decade 2004-2014. Mauna Loa on WFT one has to extrapolate back a bit to reach the start decade, but in numbers of similar precision the ratio P/P_0 \approx 1.3 is reasonable. Thus:

\Delta T_{exp} = 0.5 C

\Delta T_{model} = 1.44\ln(1.3) = 0.4 C

My model is accurate over a span of roughly *seventy years* to within 0.1 C, less than the acknowledged error in HADCRUT4 even without worrying about its neglect of things like UHI that might very reasonably make HADCRUT4 and *upper bound* (and probably biased) estimate of the temperature anomaly.

With this absolutely bone simple model I outshoot all of the models computed in CMIP5 over the exact same span. It won’t work very well on a longer hindcast, of course — because all one learns from looking at data before 1945 is that there was almost as much warming in the span from 1910 to 1945 as there was from 1945 to the present, even without a commensurate increase in P/P_0. My model does not account for this sort of deterministic/natural “noise” in the climate, so of course it won’t do very well in tracking it.

**But neither do any of the CMIP5 models!**. In fact, they do *no better* than my model does — in figure 9.8a of AR5 you can see the multimodel mean skating straight over this early 20th century warming (and you can see how its *constituent*models don’t even come close to the measured temperature, being consistently high and having all sorts of obviously wrong internal dynamics and timescales and fluctuation scales.

Now, unlike the IPCC modelers, I’m perfectly willing to acknowledge that my model could be improved (and thereby brought into better agreement with the data). For example, the estimate of 1.0C per doubling of CO\_2 is pretty crude, and could just as easily have been 1.2 C or 1.5 C. A simple improvement is to solve for my single parameter, A, to get the best fit across the entire span. Without really doing a nonlinear regression, we can match the temperature change pretty precisely with:

\Delta T = 1.9 \ln(P/P_0)

which corresponds to a TCS of 1.3 C *from all sources of feedback and forcing!*.

My model is now dead on — how could it not be — but it is also dead on the midpoint of the most often quoted range for CO\_2-only forcing, 1 to 1.5 C. Indeed, it’s scary good.

I would never assert that the prediction *is* is that good, because I’m not an idiot. I happen to think that natural variation alone can easily produce temperature deltas of 0.4-0.5 C on precisely the same timescale without any help from CO\_2 at all. One such warming episode is clearly visible in the thermometric climate record from the first half of the 20th century. There is also a compelling correspondence between the late 20th century rise and e.g. ENSO events and PDO phase, further supporting an assertion that natural variation is probably so large that my linear response model could easily be off by 100% either way with the difference being natural. That is, the total warming from CO\_2 *including feedbacks* but not including natural variation and noise could be as little as 0 C — flat out neutral, insensitive altogether — to 2 to 2.5 C — the warming “should” have been twice as great including all feedbacks but natural variation cancelled it. To put it another way, almost all of the late 20th century warming could have nothing to do with CO\_2, or the warming we observe there could have been even greater if it weren’t for partial cancellation due to natural *cooling*.

But there is little evidence for either one of these — certainly no evidence so compelling that I should feel it necessary to make a choice between them. Until there is, the only rational thing to do is keep it simple, and assume that my simplest possible physics based model is correct until it is falsified by the passage of time. In the meantime, it stands as strong evidence ***against*** large positive feedbacks.

Suppose there were, as has so very often been asserted, a strong, positive, water vapor feedback.

*Then where the hell is it?*

The data is *precisely* fit by CO\_2 alone with no feedback, not over the paltry 15 to 20 years (that aren’t even “climate” according to climate scientists) in which the late 20th century actually *warmed*, but across the *entire range* of 70-odd years which is surely enough to represent a meaningful increasing CO\_2 climate trend. Indeed, if one smooths the temperature curve in a 20 or 30 year running average, the agreement with my model curve if anything *improves* — the irrelevant noise goes away.

There isn’t any *room* for positive feedback. If it occurred, surely we would have observed more than 0.5 C of warming, because that’s exactly what is predicted in a*no* feedback model.

rgb

# 12th – chaos theory

* **rgbatduke**

[October 10, 2014 at 6:53 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1758826)

Dear David A,

I suspect you won’t return to look for this answer, but I’ll give it anyway. I suppose that my reply would have to be a qualified — pretty highly qualified, weak, even wussie, “Yes”. Or in parlance, “sorta”, “maybe”, “it’s plausible”.

The reason I can give no better answer is that two principles are in conflict. One is the logical fallacy *ceteris paribus* — trying to connect past behavior (especially across geological time) to the present on the basis of proxy data being leveraged to the very limits of its probable information content is fraught with many perils — we *think*, for example, that the state of the sun itself was cooler, that CO\_2 was higher, that the land mass was different, that the moon was closer, that the tides were higher, that the axial tilt and eccentricity were different, that the mean distance of the Earth from the sun was different. We cannot solve a believable climate model given moderately accurate measurements and knowledge of all of these things in the *present*, and trying to use what we know or believe about the present in climate models to hindcast the past has so far met with notable failure (as has both recently and repeatedly been reported on WUWT pages).

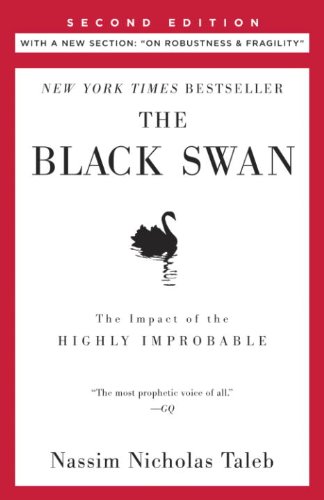
This makes it difficult to assess the implications of things like the Ordovician-Silurian glaciation (with CO\_2 likely over 10x higher than it is today). There are lots of ways one might try to explain this, but in the end they are all storytelling, not science, because the data to support one explanation over the other is truly lost. The sun was cooler. The continents were in a different shape. CO\_2 was much higher. The orbital parameters were (no doubt) completely different and we *don’t know what they were* and cannot *possibly* infer them from present data. We can do only a little bit better with the many other ice ages spread out over the paleo record, plenty of them occurring with CO\_2 over 1000 ppm, as *most* of the last 600 million years the Earth has had CO\_2 levels over 1000 ppm. The current super-low levels seem to be a relatively recent development. Even the recent paleo record — the Pleistocene — exhibits a stunning global bistability with equally stunning internal *instability* in both the glacial and interglacial phases.

However, the fundamental principle of statistical modeling, if there is such a thing *is ceteris paribus!* To be more precise, it is *maximum entropy*, selecting as a probable future the one that matches our *ignorance* most closely. We don’t *know*how to balance out the feedbacks in a nonlinear chaotic system as they probably aren’t computable by means of any sort of linearization including the sort that is casually done in every climate model ever written. Every such linearization is an assumption that short spatiotemporal scales can be treated in the mean field approximation in a chaotic system of nonlinear PDEs. That assumption is proven wrong in pretty much every numerical study of nonlinear PDEs that exhibit chaos ever conducted, but it leaves us in a state of near total ignorance about what we should expect for the sign and magnitude of the feedbacks.

In computational mathematics, all that this teaches us is that we cannot numerically solve nonlinear coupled ODEs as predictive instruments out past a certain number of timesteps before the solutions spread out to fill a large phase space of possibilities and are no longer predictively useful. It also teaches us that monkeying with the granularity of the solution or making mean field approximations leads us to a (and this is something the climate guys just don’t seem to get) **different phase space altogether** — quite possibly one with **no chaos at all**. In chaotic systems, the human adage “don’t sweat the small stuff” is precisely contradicted. You *have to sweat the small stuff*. It’s that damn butterfly, beating its wings and then everything changes.

When scientists deal with a chaotic system such as the climate in nature, then, what to do? **In my opinion** the best thing to do is construct a linearized statistical model that **maximizes our ignorance** (in context, admits that we do not know and cannot reasonably compute the feedbacks either in detail or with heuristic arguments that are anything more than “plausible”, not “probably true”) and then **beware the Black Swan!**

Since I’m having great fun posting fabulous books every engaged in this *debate*should read, let me throw this one in:

[](http://www.amazon.com/Black-Swan-Impact-Highly-Improbable-ebook/dp/B00139XTG4)

[The Black Swan: Second Edition: The Impact of the Highly Improbable Fragility" (Incerto)](http://www.amazon.com/Black-Swan-Impact-Highly-Improbable-ebook/dp/B00139XTG4" \t "_blank)

[Buy from Amazon](http://www.amazon.com/Black-Swan-Impact-Highly-Improbable-ebook/dp/B00139XTG4)

The author really needs to redo his cover art. The Black Swan on the cover should be depicted a la *Escher* as a *fractal image of chaos* in the form of a black swan, with a surface boundary with complexity, all the way down (instead of turtles!). Chaos theory is precisely the fundamental theoretical justification for not taking our linearized predictions *even if we knew all of the linear responses of the system to forcings in every independent dimension quite precisely* at all seriously. In ordinary linearized physics, knowing all of the partial derivatives gives you the gradient, and the gradient tells you what the system will do. Except of course when it doesn’t — for example at critical points. But chaotic systems can be viewed as one vast multicritical point, a multicritical *volume* of phase space. Knowing the gradient might — note well, might — give you some reason to believe that you can extrapolate/integrate a very short time into the future. Empirically, **two to three weeks** before one tiny, ignored feature on a single feather of the black swan grows to completely replace it with a swan of a startlingly different shape, only to be replaced in turn a few weeks later by a tiny feature on *its* beak, and so on. It’s like predicting a kaleidoscope, or the top card drawn from a deck of well-shuffled cards, only worse — the kaleidoscope can turn itself into a different toy altogether, the usual white swan can change color altogether, the nice warm interglacial can suddenly decide to *race* into glaciation, with no “explanation” needed beyond the chaotic nature of the dynamics themselves.

Again, this eludes climate scientists. They think that the Younger Dryas requires *explanation*. Well it might have one — *ex post facto*. It might even have several. But even armed with the explanatory events (if there are any!) beforehand, it was probably not *certain*, and even if there *were no explanatory events at all* — no giant asteroid impacts, no sudden drainings of lakes, or at least no more than usual with nothing particularly “special” about them — it might have still *just happened*. That’s the spooky thing about chaos. Try*explaining* the trajectory of (say) a double pendulum in the chaotic phase as it dances around like a mad thing in terms of its *normal modes*. (I have had students build these things a couple of times as extra credit projects in intro mechanics and have one sitting in my office — great fun to play with:

<http://video.mit.edu/watch/double-pendulum-6392/>

Only the last bit, at the very end, is the linearized principle mode oscillation where the two rods move in phase like a simple oscillator. Right before this mode established itself via damping, the oscillation could *probably* have been decomposed into a linear combination of this and the other mode where they oscillate in opposite directions.)

This is no more than a metaphor for the climate, of course. The climate is*breathtakingly more complex* and is a *driven* oscillator. Here is a video from 1982 demonstrating the time evolution of a van der Pol oscillator — an absolutely trivial nonlinear driven oscillator in only two dimensions. The thing to note is that tiny changes in the driving parameters don’t make small changes in an otherwise systematic “orbit” — they cause the system to *completely rearrange itself* in any regime where there are multiple attractors. If you looked at only (say) the y-projection of this motion, it would not look trivially like an oscillator.

All of this should make us mistrust any sort of “no Black Swan” conclusion based on the kind of linearization argument I give above and that you correctly point out could be used to argue more strongly for *negative* feedback. I offer the linearized no-feedback model not because I think it has strong predictive value into either the past or the future but because it is the *best we can do*, given our ignorance and inability to do better. It is quite literally a null hypothesis, and should be trusted for just as long as it *works*, while acknowledging that the climate system is fundamentally unpredictable and could go all postal on us *in any direction* even without *any* anthropogenic help.

The one thing the catastrophists have dead right is this. For better or worse, the CO\_2 we have added and will continue to add is *most likely* — in the statistically and physical defensible sense that this is predicted by a mix of physics and ignorance — going to produce a degree or so of warming by the time atmospheric CO\_2 reaches 600 ppm. This is not a confident prediction — the oceans could eat the heat. Water vapor could double this. Clouds could entirely cancel it. Volcanoes could erupt and alter aerosols in ways that completely change things. The sun could go quiet for a half century or more and have some occult (but nonlinearly amplified!) effect on the climate. Thermohaline circulation could suddenly discover a new mode and the Gulf Stream could suddenly divert 500 miles south and leave the entire northeast coast of the US and all of Europe in the deep freeze and *trigger an ice age* (seriously, that’s all that would be needed — 100 years, a blink of an eye in geological time, of diverted gulf stream and we’d enter the next glacial episode, IMO).

We can predict none of this. We cannot really measure any of it to any meaningful precision, and climate scientists are apparently *just realizing* that their assumptions about the ocean in all of the climate models are *dead wrong*. Oops.

That means that we do not know the effects of the extra degree of warming. We do not even know how to *estimate* the stability of the climate system. We do not know the partial derivative field for a large-scale convective eddy in the ocean in response to an irregularly distributed change in its surface thermal field and driving. We cannot calculate it. We do not know. We do not know. We do not know.

But it is quite possible that the result will be *bad*. Or good! Or a wash, neither bad nor good.

What catastrophe theorists *should* be doing is trying to guesstimate not the direct response which is already accurate enough in my toy model until nature says otherwise, but the *probability* of a *catastrophic* response in the *nonlinear*system. At a guess, we’re 30 to 50 years away from even a semi-phenomenological theory that could handle this, and that could be optimistic, as we aren’t even really trying and haven’t started looking, dicking around with simulations at a silly spatiotemporal length scale and mean field approximations galore that we shouldn’t expect to come *close* to the behavior of the actual system in any quantitative sense that matters.

Hence my own opinion. Up to now, I suspect that the additional CO\_2 has been on average a good thing. Still, if I had my druthers, we would not rocket it up to 600 ppm simply because it will almost certainly cause a rearrangement — possibly a dramatic rearrangement — of the underlying attractors as we do. We cannot count on past observations of climate to be particularly good predictors of future climate, and cannot rule out new hot attractors forming, and we *know*that there is a cold phase attractor lurking because the Earth is currently bistable on geological time and in a rather protracted excursion into the warm phase. I’m *hoping* that all we have done (and will do with the rest of the additional degree) is stabilize the system against a return to glaciation. Wouldn’t that be lucky! But even *without* the CO\_2, the *climate* varies by enough to cause massive drought and heat wave and cold wave and flood and hurricane and disasters a plenty.

For better or worse, we’ve jumped on the back of this particular shark. There is no safe way to get off, and trying to the extent that we are trying is killing millions of people a year because we’ve *panicked* and are doing it *stupidly*, trying to fling ourselves from its back and not worrying about whether or not we’ll land in a bed of broken glass. In our panic, we don’t even know that it is a shark — it could be a friendly dolphin, carrying us away to Atlantis and a world with a truly human-optimal climate — so far, the ride has if anything been mostly beneficial and we haven’t so much as caught a glimpse of teeth.

All things being equal, it is probably a good idea not to jab the putative shark with spurs, even if it might turn out to be a sadomasochistic dolphin instead, while at the same time not launching ourselves from the back altogether and damning several billion people to a continuation of a life of abject poverty. Including us, when the effort inevitably triggers a massive depression, because we *do not have the technology* yet to safely dismount.

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# 13th– Black box and the oceans

**rgbatduke**

[October 9, 2014 at 8:04 am](http://wattsupwiththat.com/2014/10/06/real-science-debates-are-not-rare/#comment-1758176)

Sure. But climate science is precisely about the state of what is inside the box, not about the fact that the surface integrals have to balance. And when what is inside of the box is a truly stupendously large thermal reservoir and what comes out of the box is governed by dynamic chaotic processes inside of the box that can obviously be at least fully bistable, making inferences about what is inside of the box and its probable response to *other* stuff happening *inside the box* on the basis of surface integrals is just silly.

Bear in mind that I absolutely love surface integrals and vector calculus and am teaching electrodynamics at this very moment, which is rife with precisely what you assert. But in electrodynamics we *also* have (usually) *linear response* assumptions that let us connect things like polarizability to field inside the box from a knowledge of the surface states, and we have other things like *uniqueness theorems* that work for the *linear* PDEs that describe the contents.

Open systems, especially open self-organizing nonlinear multicritical systems, are not quite so amenable to this sort of analysis. Oh, one can do it and arrive at *some*insight — detailed balance is still a condition on open systems in some sort of local equilibrium and not at or particularly near a critical instability — but to figure out the inside of the box one has to be able to go inside the box and look, and what one discovers can *easily* be non-computable, however much one can explain what one observes *ex post facto*.

A trivial example. I can easily understand how turbulent rolls form in a container heated in a certain way and cooled in another. I can absolutely understand that heat flows in on one side and flows out on the other when it is in a kind of “steady state”. However, in certain regimes what I might observe is a modulation of that heat flow indicative of a critical instability that *completely changes the pattern of turbulence and temperature inside of the container*. My surface measurements become nearly*irrelevant* to determining the state of the box, because the internal state is no longer uniquely determined by the surface observation.

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