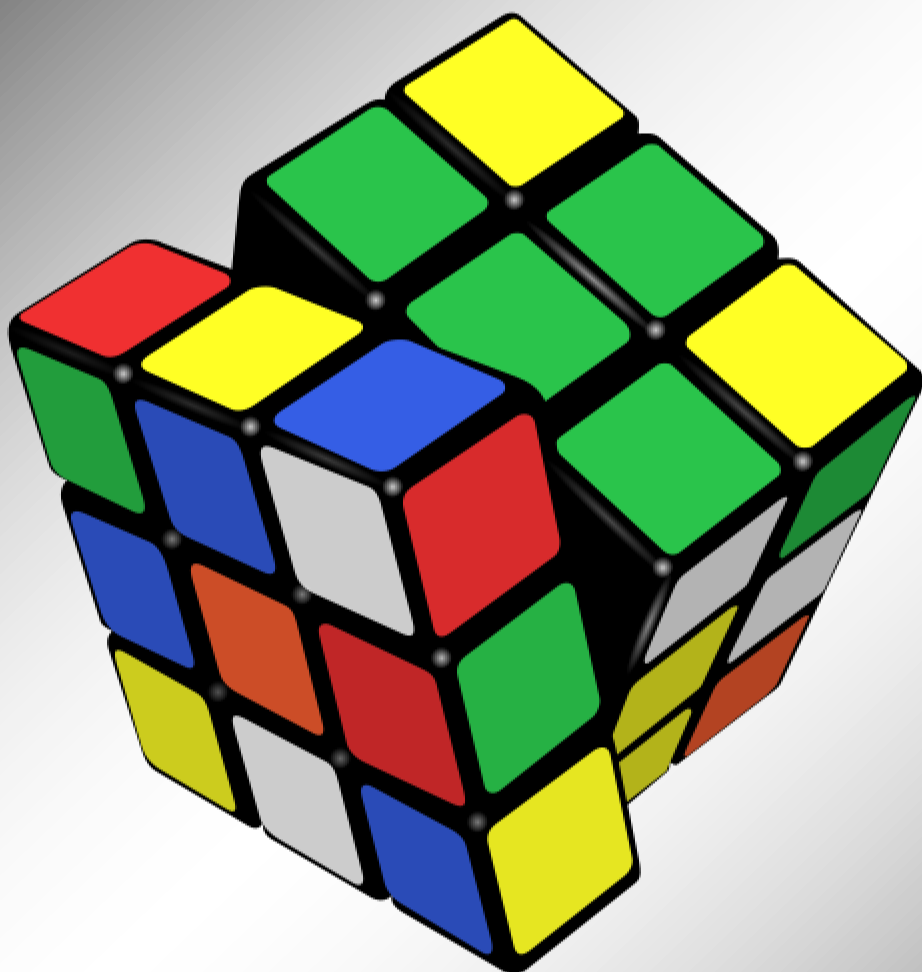
CLIMATEGATE ANALYSIS

by John P. Costella



SPPI

Science & Public Policy Institute

"science-based policy for a better world"

SPPI REPRINT SERIES ♦ **January 20, 2010**

CLIMATEGATE ANALYSIS

by John P. Costella | January 18, 2010

INTRODUCTORY ESSAY

Why Climategate is so distressing to scientists

by John P. Costella | December 10, 2009

The most difficult thing for a scientist in the era of Climategate is trying to explain to family and friends why it is so distressing to scientists. Most people don't know how science really works: there are no popular television shows, movies, or books that really depict the everyday lives of real scientists; it just isn't exciting enough. I'm not talking here about the major discoveries of science—which are well-described in documentaries, popular science series, and magazines—but rather how the *process* of science (often called the "scientific method") actually works.

The best analogy that I have been able to come up with, in recent weeks, is the criminal justice system—which *is* (rightly or wrongly) abundantly depicted in the popular media. Everyone knows what happens if police obtain evidence by illegal means: the evidence is ruled inadmissible; and, if a case rests on that tainted evidence, it is thrown out of court. The justice system is not saying that the accused is necessarily innocent; rather, that determining the truth is impossible if evidence is not protected from tampering or fabrication.

The same is true in science: scientists assume that the rules of the scientific method have been followed, at least in any discipline that publishes its results for public consumption. It is that trust in the process that allows me, for example, to believe that the human genome has been mapped—despite my knowing nothing about that field of science at all. That same trust has allowed scientists at large to similarly believe in the results of climate science.

Until now.

So what *are* the "rules" of the scientific method? Actually, they are not all that different from those of the justice system. Just as it is a fundamental right of every affected party to be heard and fairly considered by the court, it is of crucial importance to science that all points of view be given a chance to be heard, and fairly debated. But, of course, it would be impossible to allow an "open slather" type of arrangement, like discussion forums on the Internet; so how do we admit all points of view, without descending into anarchy?

This question touches on something of a dark secret within science one which most scientists, through the need for self-preservation, are scared to admit: most disciplines of science are, to a greater or lesser extent, controlled by fashions, biases, and dogma. Why is this so? Because the mechanism by which scientific debate has been "regulated" to avoid anarchy—at least since the second half of the twentieth century—has been the "peer review" process. The career of any professional scientist lives or dies on their success in achieving publication of their papers in "peer-reviewed" journals. So what, exactly, does "peer-reviewed" mean? Simply that other professional scientists in that discipline must agree that the paper is worthy of publication. And what is the criterion that determines who these "professional scientists" should be? Their success in achieving publication of their papers in peer-reviewed journals! Catch-22.

It may seem, on the surface, that this circular process is fundamentally flawed; but, borrowing the words of Winston Churchill, it is the worst form of government except for all those others that have been tried. Science is not, of course, alone in this respect; for example, in the justice system, judges are generally selected from the ranks of lawyers. So what is it that allows this form of system work, despite its evident circularity?

The justice system again provides a clue: judges are not the ones who ultimately decide what occurs in a courtroom: they simply implement the laws passed or imposed by the government—and politicians are not, in general, selected solely from the ranks of the legal profession. This is the ultimate "reality check" that prevents the legal system from spiraling into navel-gazing irrelevance.

Equivalent "escape valves" for science are not as explicitly obvious, but they exist nonetheless.

Firstly, a scientific discipline can maintain a "closed shop" mentality for a while, but eventually the institutions and funding agencies that provide the lifeblood of their work— the money that pays their wages and funds their research—will begin to question the relevance and usefulness of the discipline, particularly in relation to other disciplines that are competing for the same funds. This will generally be seen by the affected scientists as "political interference", but it is a reflection of their descent into arrogance and delusions of self-importance for them to believe that only they themselves are worthy of judging their own merits.

Secondly, scientists who are capable and worthy, but unfairly "locked out" of a given discipline, will generally migrate to other disciplines in which the scientific process is working as it should. Dysfunctional disciplines will, in time, atrophy, in favor of those that are healthy and dynamic.

The Climategate emails show that these self-regulating mechanisms simply failed to work in the case of climate science—perhaps because "climate science" is itself an aggregation of many different and disparate scientific disciplines. Those component disciplines are extremely challenging. For example, it would be wonderful if NASA were able to invent a time machine, and go back over the past hundred thousand years and set up temperature and carbon dioxide measurement probes across the breadth of the globe. Unfortunately, we don't have this. Instead, we need to infer these measurements, by counting tree rings, or digging up tubes of ice. The science of each of these disciplines is well-defined and rigorous, and there are many good scientists working in these fields. But the real difficulty is the "stitching together" of all of these results, in a way that allows answers to the fundamental questions: How much effect has mankind had on the temperature of the planet? And how much difference would it make if we did things differently?

It is at this "stitching together" layer of science—one could call it a "meta-discipline"— that the principles of the scientific method have broken down. Reading through the Climate-gate emails, one can see members of that community usually those with slightly different experience and wisdom than the power-brokers questioning (as they should) this "stitching together" process, particularly with regard to the extremely subtle mathematical methods that need to be used to try to extract answers. Now, these mathematical and statistical methods are completely within my own domain of expertise; and I can testify that the criticisms are sensible, carefully thought-out, and completely valid; these are good scientists, asking the right questions.

So what reception do they get? Instead of embracing this diversity of knowledge—thanking them for their experience (no one knows everything about everything) and using that knowledge to improve their own calculations—these power-brokers of climate science instead ignore, fob off, ridicule, threaten, and ultimately black-ball those who dare to question the methods that they—the power-brokers, the leaders—have used. And do not be confused: I am here talking about those scientists *within* their own camps, not the "skeptics" which they dismiss out of hand.

This is not "climate science", it is climate ideology; it is the Church of Climatology.

It is this betrayal of the principles of science—in what is arguably the most important public application of science in our lifetime—that most distresses scientists.

THE ANALYSIS: KEY EMAILS, INCLUDING EXCERPTS AND LINKS

Welcome to my analysis of Climategate, the climate science scandal that has already eclipsed Watergate in terms of its global political ramifications.

Climategate publicly began on November 19, 2009, when a whistle-blower leaked thousands of emails and documents central to a Freedom of Information request placed with the

Climatic Research Unit of the University of East Anglia in the United Kingdom. This institution had played a central role in the "climate change" debate: its scientists, together with their international colleagues, quite literally put the "warming" into Global Warming: they were responsible for analyzing and collating the various measurements of temperature from around the globe and going back into the depths of time, that collectively underpinned the entire scientific argument that mankind's liberation of "greenhouse" gases—such as carbon dioxide—was leading to a relentless, unprecedented, and ultimately catastrophic warming of the entire planet.

The key phrase here, from a scientific point of view, is that it is "unprecedented" warming. There is absolutely no doubt that mankind has liberated huge quantities of carbon dioxide into the atmosphere over the past two centuries. But mankind did not "create" this carbon dioxide out of nothing. It was released by the burning of "fossil fuels", created by the Earth over millions of years from the remains of plants and animals (who themselves ultimately obtained their nutrition from those plants). So where did those plants get their energy and carbon dioxide from? They absorbed the radiant energy of the Sun, and breathed in carbon dioxide from the atmosphere, as plants continue to do today. In other words, when we burn fossil fuels, we are utilizing a small part of the solar energy that had been collected and stored by plants over millions of years, and in the process we are liberating into the atmosphere the carbon dioxide that those plants had absorbed from the atmosphere in the first place.

This may sound like a fairly benign sort of natural cycle, until you realize that a couple of hundred years is a mere blink of an eye compared to the millions of years it took for the planet to build up those resources. It is right for scientists to worry about whether that massive and almost instantaneous "kick" to the planet may throw the equilibrium of the biota into complete chaos. It is a valid question, of ultimate global importance—one that most people would have thought would have demanded the most careful, exacting, and rigorous scientific analyses that mankind could muster.

Climategate has shattered that myth. It gives us a peephole into the work of the scientists investigating possibly the most important issue ever to face mankind. Instead of seeing large collaborations of meticulous, careful, critical scientists, we instead see a small team of incompetent cowboys, abusing almost every aspect of the framework of science to build a fortress around their "old boys' club", to prevent real scientists from seeing the shambles of their "research". Most people are aghast that this could have happened; and it is only because "climate science" exploded from a relatively tiny corner of academia into a hugely funded industry in a matter of mere years that the perpetrators were able to get away with it for so long.

But as wisely noted by P. T. Barnum, and quoted by Abraham Lincoln,

> *You may fool all the people some of the time, you can even fool some of the people all of the time, but you cannot fool all of the people all the time.*

As an increasing number of highly qualified scientists slowly began to realize that the "climate science" community was a facade—and that their vitriolic rebuffs of sensible arguments of mathematics, statistics, and indeed scientific common sense were not the product of scientific rigor at all, but merely self-protection at any cost—the veil began to drop on what has already become clear as the greatest scientific fraud in this history of mankind.

This is one of the darkest periods in the history of science. Those who love science, and all it stands for, will be pained by what they read below. However, the crisis is here, and cannot be avoided.

In the following, for simplicity, I have kept the numerical and chronological order of the emails, as they appear in the Climategate files. I considered reorganizing them by topic, but quickly realized that this would require the replication of large numbers of excerpts—which would lengthen what is already becoming a long document. Thus, the various issues involved in this scandal are explored chronologically, in parallel.

To assist the reader in getting "up to speed" with the various characters in this saga, I have color-coded their emails, as described below. To make the emails understandable to any normal person, I have edited out scientific jargon, expanded acronyms, and inserted explanatory comments where I thought it necessary. All of my comments, and the editing that I have done to the excerpts, is in black.

Unlike the Climategate perpetrators themselves, however, I have made all the raw data—the emails themselves, in unredacted raw text format—available on this site; and the heading for each email contains a link to that original email. Thus, if you believe that I have excerpted from the email unfairly, or out of context, then you can simply read the original email to determine if that is the case.

And so let us begin.

CAST OF COLORFUL CHARACTERS

- **MIKE MANN:** lead conspirator in the United States.
- **PHIL JONES:** lead conspirator in the United Kingdom.
- **TOM WIGLEY:** older conspirator who becomes increasingly worried about the unfolding scandal.
- **KEITH BRIFFA:** older conspirator whose blunders lead the others to all but abandon him.
- **BEN SANTER:** dangerously arrogant and naive young conspirator in the United States.
- **OTHER CONSPIRATORS:** of varying degrees of complicity and integrity.
- SKEPTICS: **and other unrelated parties.**

March 6, 1996: **email 0826209667**

This earliest email of note in the Climategate collection reminds us that—as with many things in life—money plays a key role in this saga. Let me emphasize that Climategate is not riddled with financial scandals—not explicit ones, of Madoff magnitude, in any case. Rather, we here are reminded of the fact that the entire industry of "climate science" was created out of virtually nothing, by means of a massive influx of funding that was almost universally one-sided in its requirement that its recipients find evidence for man-made climate change—not investigate whether or how much mankind had caused climate change.

In contrast to the literally trillions of dollars of global expenditure ultimately urged on world leaders by these scientists by the end of 2009, the amounts involved in funding their research appear trifling—typically measured in "mere" millions of dollars. (A trillion is a million millions!) But many "climate scientists" built their entire careers on this funding; and so it is not surprising that they became so completely reliant on this conditional lifeline, that they became single-mindedly focused on achieving the ends for which they were commissioned—and viciously attacking any intruders who may threaten that lifeline.

In this unfortunate case, a scientist in the former Soviet Union appears to descend to level of tax evasion, in order to maximize the amount of money available. As Stepan Shiyatov writes to Keith Briffa:

> Also, it is important for us if you can transfer the … money on the personal accounts which we gave you earlier and the sum for one occasion transfer (for example, during one day) will not be more than 10,000 United States Dollars. Only in this case we can avoid big taxes …

Unfortunately, all other emails relating to these cash transfers have either been lost, deleted, or withheld by the Climategate whistle-blower, so we don't know whether Keith Briffa complied with this request or not.

I believe that this level of financial impropriety would be a rare occurrence—although it does highlight the fact that some of the people involved in this research were prepared to "bend the rules" in order to achieve their goals. It also reminds us that scientists in general are often ignorant of the requirements of the law; but, most of the time, this does not lead to any significant ramifications. Therefore, although there are other examples of low-level financial impropriety and misappropriation sprinkled throughout the Climategate emails, I do not believe that they are of any significance over and above the general comments that I have made here, and I will not explicitly list them in the following.

July 11, 1996: **email 0837094033**

In the next email we are introduced to a number of key aspects of Climategate, which run throughout the saga. The writer is Phil Jones, head of the Climatic Research Unit at the University of East Anglia in England. The recipient is Alan Robock, a climate scientist who was, at the time, at the University of Maryland.

Phil Jones has apparently become aware of a climate skeptic in the United Kingdom—seemingly the first, from his words:

> Britain seems to have found its Pat Michaels / Fred Singer / Bob Balling / Dick Lindzen (American climate skeptics). Our population is only 25% of yours so we only get 1 for every 4 you have. His name in case you should come across him is Piers Corbyn. He is nowhere near as good as a couple of yours and he's an utter prat but he's getting a lot of air time at the moment.

Robock requires a translation into American English:

> Could you please define "utter prat" for me? Sometimes I think we speak the same language, and sometimes I'm not so sure.

We don't seem to have Jones's reply, but the translation would be something like "absolute trouble-maker and useless idiot". Note that Jones is immediately reporting the existence of this first British skeptic to climate scientists on the other side of the Atlantic, taking special note of the "air time" (exposure on television or radio) that the skeptic is apparently receiving. Already, we can start to appreciate that the politics and "spin doctoring" in this field outweighs the scientific issues. Continuing from Jones's email:

> For his day job he teaches physics and astronomy at a University and he predicts the weather from solar phenomena.

Jones's report is as efficient as that of an intelligence agent: the skeptic is dangerous because he is the British equivalent of a college professor—in the "hard sciences" of physics and astronomy, no less. However, he softens his attitude to the skeptic slightly:

> He's not all bad as he doesn't have much confidence in nuclear-power safety.

We here see clearly that Jones's assessment of a scientist's worth is influenced strongly by his assessment of their ideology—in scientific terms, nuclear power safety is completely unrelated to the science of climate change. This dangerous prejudice will prove to be one of the most persistent threads throughout the Climategate scandal.

September 17, 1996: **email 0842992948**

We now turn to Keith Briffa, one of the more curious University of East Anglia characters in the Climategate saga. Gary Funkhouser of the University of Arizona writes to Briffa about some data that was collected in the late 1980s. Briffa makes it clear that he is only interested in the data if it can be used to "sell" the climate change message to the general public:

> The data is of course interesting but I would have to see it and the board would want the larger implications of the statistics clearly phrased in general and widely understandable (by the ignorant masses) terms before they would consider it not too specialised.

September 19, 1996: **email 0843161829**

Two days after the previous exchange, Gary Funkhouser reports on his attempts to obtain anything from the data that could be used to sell the message of climate change:

> I really wish I could be more positive about the … material, but I swear I pulled every trick out of my sleeve trying to milk something out of that. … I don't think it'd be productive to try and juggle the chronology statistics any more than I

> already have—they just are what they are … I think I'll have to look for an option where I can let this little story go as it is.

His reluctance to report a "null result" (namely, that the data do not show anything significant) is extremely disturbing, as it flies in the face of standard scientific practice, which requires that all results be reported. The fundamental problem is that any censoring of results that do not lead to a predetermined conclusion will always—by design—bias the corpus of reported results towards that conclusion, in the same way that a gambler who always brags about his wins (but stays silent about his losses) will appear to be hugely successful, even if his losses have, in reality, far outweighed his winnings (as is generally the case, in the long run, except for the extremely skillful).

We will, sadly, see that this fundamental scientific flaw—which, in and of itself, is sufficient to render the evidence for climate change completely unreliable and scientifically worthless—is one that runs throughout the entire Climategate saga.

Note, also, the immense power wielded—albeit ever so subtly—by Briffa: he influenced the analysis that Funkhouser performed, simply by telling him that the results would need to politically "saleable". Scientists are not naive: they know that securing funding, publication of their papers, and interest from other institutions are the key factors determining their future.

November 22, 1996: **email 0848679780**

Geoff Jenkins was head of climate change prediction at the Hadley Centre for Climate Prediction and Research, part of the United Kingdom's Met(eorological) Office (national weather service). He writes to Phil Jones:

> Remember all the fun we had last year over 1995 global temperatures, with the early release of information (via Australia), "inventing" the December monthly value, letters to Nature, etc., etc.?
>
> I think we should have a cunning plan about what to do this year, simply to avoid a lot of wasted time.

Again, selling the public message—before the actual end of the calendar year—is of primary importance for these senior scientists. Jenkins goes on to explain how this "invented" data should be leaked:

> We feed this selectively to Nick Nuttall (of the United Nations Environment Program) (who has had this in the past and seems now to expect special treatment) so that he can write an article for the silly season. We could also give this to Neville Nicholls (climate scientist at the Bureau of Meteorology Research Centre in Melbourne, Australia)?

Lest it be thought that this may be standard public relations procedure for the Met(eorological) Office, Jenkins puts the issue beyond doubt:

> I know it sound a bit cloak-and-dagger but it's just meant to save time in the long run.

In other words, Jenkins was more interested in getting "headline" numbers out to the general public, than in ensuring an impartial release of information to all members of the press at the same time.

If repeated in 2009, Jenkins' actions could have rendered him liable to criminal prosecution for insider trading. However, given that we are here talking about 1996—before so many billions of dollars of decisions and market movements watched the unfolding climate change debate—we can put his actions down to mere expedience and naivete.

October 9, 1997: **email 0876437553**

We now encounter one of the most insidious red herrings in the climate debate: how many thousands of scientists "endorsed" the views of the Intergovernmental Panel on Climate Change.

With just months until the Kyoto Climate Conference, we find the germ of this idea fertilizing in an email from Joe Alcamo, Director of the Center for Environmental Systems Research in Germany, to Mike Hulme and Rob Swart:

> Sounds like you guys have been busy doing good things for the cause.
>
> I would like to weigh in on two important questions—
>
> Distribution for Endorsements—
>
> I am very strongly in favor of as wide and rapid a distribution as possible for endorsements. I think the only thing that counts is numbers. The media is going to say "1000 scientists signed" or "1500 signed". No one is going to check if it is 600 with PhDs versus 2000 without. They will mention the prominent ones, but that is a different story.
>
> Conclusion—Forget the screening, forget asking them about their last publication (most will ignore you).Get those names!

This statement alone shows how ridiculous the "endorsement" process was from the very beginning. Signing a petition in support of an opinion—regardless of whether the signer has a PhD or not—is as scientifically meaningless as if these same people had voted Albert Einstein's hairstyle as the most interesting in the history of science. It is nonsense, pure and simple.

Alcamo continues:

> Timing—I feel strongly that the week of 24 November is too late.
>
> 1. We wanted to announce the Statement in the period when there was a sag in related news, but in the week before Kyoto we should expect that we will have to crowd out many other articles about climate.
>
> 2. If the Statement comes out just a few days before Kyoto I am afraid that the delegates who we want to influence will not have any time to pay attention to it. We should give them a few weeks to hear about it.
>
> 3. If Greenpeace is having an event the week before, we should have it a week before them so that they and other Non-Governmental Organizations can further

> spread the word about the Statement. On the other hand, it wouldn't be so bad to release the Statement in the same week, but on a different day. The media might enjoy hearing the message from two very different directions.
>
> Conclusion I suggest the week of 10 November, or the week of 17 November at the latest.

Alcamo demonstrates that this is a carefully crafted piece of political and ideological activism, not related to the scientific process at all. Indeed, the optimization of the timing—allowing just enough time for delegates to absorb the message, but not enough time for the scientists signing on to this petition to actually examine or criticize its contents—will return with a vengeance below.

November 12, 1997: **email 0879365369**

Richard Tol to Mike Hulme and Timothy Mitchell:

> I am always worried about this sort of thing. Even if you have 1000 signatures, and appear to have a strong backup, how many of those asked did not sign?

Tol is absolutely correct: just as suppressing research results that do not support climate change inevitably biases the published record, so too does suppressing the number of scientists who declined to sign the petition.

Many similar lessons of history are related to undergraduate students of statistics every year the world over, which earn enormous laughter in the lecture theater, but are less humorous in real life: estimating war-time damage to planes by examining only those that return; completely wrong predictions of elections, due to conservative voters being less likely to respond to pollsters; and so on. That any faith at all was placed on climate petitions of this sort is almost unbelievable.

Tol continues:

> I think that the text of the Statement conveys the message that it is a scientific defense for the European Union's position. There is not any.

Indeed, as we have seen in the intervening years, it was used to justify far more.

November 25, 1997: **email 0880476729**

Tom Wigley roundly criticises the eleven scientists seeking endorsement of their Statement.

> Dear Eleven,
>
> I was very disturbed by your recent letter, and your attempt to get others to endorse it. Not only do I disagree with the content of this letter, but I also believe that you have severely distorted the Intergovernmental Panel on Climate Change (IPCC) "view" when you say that "the latest IPCC assessment makes a convincing economic case for immediate control of emissions." …
>
> This is a complex issue, and your misrepresentation of it does you a dis-service. To someone like me, who knows the science, it is apparent that you are presenting a personal view, not an informed, balanced scientific assessment. What is

> unfortunate is that this will not be apparent to the vast majority of scientists you have contacted. In issues like this, scientists have an added responsibility to keep their personal views separate from the science, and to make it clear to others when they diverge from the objectivity they (hopefully) adhere to in their scientific research. I think you have failed to do this.
>
> Your approach of trying to gain scientific credibility for your personal views by asking people to endorse your letter is reprehensible. No scientist who wishes to maintain respect in the community should ever endorse any statement unless they have examined the issue fully themselves. You are asking people to prostitute themselves by doing just this! I fear that some will endorse your letter, in the mistaken belief that you are making a balanced and knowledgeable assessment of the science—when, in fact, you are presenting a flawed view that neither accords with the IPCC nor with the bulk of the scientific and economic literature on the subject.
>
> …
>
> When scientists color the science with their own personal views or make categorical statements without presenting the evidence for such statements, they have a clear responsibility to state that that is what they are doing. You have failed to do so. Indeed, what you are doing is, in my view, a form of dishonesty more subtle but no less egregious than the statements made by the greenhouse skeptics …. I find this extremely disturbing.

I couldn't express it any better myself.

May 6, 1999: **email 0926026654**

Phil Jones writes to Mike Mann, copying in Keith Briffa and Tim Osborn (United Kingdom), and Malcolm Hughes and Ray Bradley (United States), regarding a tiff between the two continents:

> … you seem quite pissed off with us all in the Climatic Research Unit. I am somewhat at a loss to understand why. It is clear from the emails that this relates to the emphasis placed on a few words or phrases in Keith's and Tim's Science paper. These may not be fully resolved but the paper comes out tomorrow. I don't want to open more wounds but I might by the end of the email.

As we shall see, Mike Mann does not tolerate criticism at all—no matter how mild, nor whether it comes from his own colleagues; and he does everything within his power to prevent the publication of any such criticisms. In this case, it seems that he has failed.

In defense of his team, Jones raises an issue that recurs throughout the Climategate saga:

> You may think Keith or I have reviewed some of your papers but we haven't. I've reviewed Ray's and Malcolm's—constructively, I hope, where I thought something could have been done better. I also know you've reviewed my paper with Gabi Hegerl very constructively.

This is a remarkable discussion for two senior scientists to be having. The "peer review" process for papers submitted to academic journals is, in general, completely anonymous, for the same reason that voting at elections is anonymous: to prevent intimidation or bullying. For these scientists to be surreptitiously trying to determine who the reviewers of their papers are immediately tells us two things: that the practitioners have absolutely no respect for the principles of scientific integrity and objectivity; and that this "discipline of science" has such a small and exclusive membership that they are able to guess at the names of their reviewers by a simple process of elimination.

Jones tries to heal over the rift, but then proceeds to back up the statements of his colleagues:

> There are two things I'm going to say though:
>
> 1) Keith didn't mention in his Science piece but both of us think that you're on very dodgy ground with this long-term decline in temperatures on the 1000 year timescale. …
>
> 2) The errors don't include all the possible factors. …

Scientific disagreement is absolutely normal and healthy; that is not the point of this exchange. Rather, it is Jones's feeling the need to justify the criticisms being published by his staff—and to assert, unequivocally, that he agrees with and supports those criticisms—that is of real concern. Presumably, if Jones had not agreed with them, then Mann's attempt to have the criticisms suppressed might well have been successful.

In other words, these two men—Mike Mann and Phil Jones—essentially controlled what could and could not be published in the scientific literature relating to their field. This is an extremely dangerous concentration of power for any discipline, let alone one possessing such enormous political and financial ramifications.

May 6, 1999: **email 0926031061**

We don't have the intervening discussion, but it seems that Phil Jones and Mike Mann have called a truce:

> We'll differ a bit on a few points, but let's wipe the slate clean …
>
> I must admit to having little regard for the Web. Living over here makes that easier than in the United States—but I would ignore the so-called skeptics until they get to the peer-review arena. I know this is harder for you in the United States and it might become harder still at your new location. I guess it shows though that what we are doing is important. The skeptics are fighting a losing battle.

It might seem remarkable that a senior scientist in 1999 could be dismissive of the World Wide Web; but we must remember that this is not particle physics (where the web originated, in the early 1990s), but rather a sleepy corner of science that was relatively isolated and peaceful before the climate debate grabbed the attention of the world's population.

As he dismisses the impact of the web, Jones feels safe that he won't be hassled by skeptics, as he knows that they have no chance of penetrating the closed club of peer review; recall, the field of his "peers" is so small that he can determine who is anonymously reviewing his papers by a process of elimination.

I think it is fair to agree with Jones that, as of 1999, the skeptics were, indeed, fighting a losing battle.

May 19, 1999: **email 0927145311**

Tom Wigley writes to Mike Hulme and Mike MacCracken, regarding a chain of emails discussing climate models:

> I've just read the emails of May 14 onwards regarding carbon dioxide. I must say that I am stunned by the confusion that surrounds this issue. Basically, I and MacCracken are right and Felzer, Schimel and (to a lesser extent) Hulme are wrong. There is absolutely, categorically no doubt about this.

Mike Hulme responds:

> I still have a problem … making sense of what the Met(eorological) Office Hadley Centre have published …

Tom Wigley replies:

> Yes, I am aware of the confusion surrounding what the Met(eorological) Office Hadley Centre did and why. It is even messier than you realize. … The Hadley people have clearly screwed things up, but their "errors" don't really matter given all of the uncertainties. I didn't mention this because I thought that opening up that can of worms would confuse people even more.
>
> …
>
> The climate model output is also uncertain.

The fact that scientists are disagreeing here is not remarkable; rather, it is the degree of confusion that is alarming—and a seeming complete lack of concern for the consequences of publishing data that is agreed to be wrong. Wigley's argument is that the consequences of the mistake are insignificant compared to the uncertainties in the model itself; and so he deemed it better to "let sleeping dogs lie".

As we shall see, this form of disregard for accuracy and honesty was widespread in this small community of scientists, and sowed the seeds of their own destruction.

July 16, 1999: **email 0932158667**

In discussing a paper published in Science, Ed Cook asks Keith Briffa:

> Also, there is no evidence for a decline or loss of temperature response in your data in the post-1950s (I assume that you didn't apply a bodge here) …

This caveat by Cook implies that "applying a bodge"—i.e., a fudge, a fake-up; a manipulation of the data to obtain the property you wish to see in it—is something that he believes that

Briffa may well have done; and he wants to first make sure that Briffa has not done so, before encouraging him to publish a response that may be critical of the published work.

That this comment is not made in an inflammatory or accusational tone—but merely as a friendly check—is of extreme concern. Briffa's reply confirms that he took no offense; he doesn't even answer the question.

"Applying a bodge" is scientific fraud, pure and simple; that it was accepted by this small coterie of scientists as "standard practice" is damning.

Briffa's response, however, does continue a familiar theme:

> I really have not had time to fully digest their message but I can't see why either they or Nature did not ask my opinion of it. My instinctive first reaction is that I doubt it is the answer but we do get results that support… that may be consistent with their hypothesis … If you get any detailed thoughts on the Nature paper please let me know, as I don't know how to respond, if at all.

Briffa implicitly assumes that any paper that touches on his own work would automatically be sent to him for review, and he cannot understand why this "gentlemen's understanding" was not honored in this case—even though he admits that the published paper may well be correct! Again, this highlights how tiny, cosy, and scientifically dysfunctional this discipline of science really was—at a time that the world was being convinced that the science had been extensively corroborated and was rock-solid against any criticism.

July 29, 1999: **email 0933255789**

The World Wildlife Fund's Adam Markham writes to University of East Anglia climate scientists Mike Hulme and Nicola Sheard, about a paper that Hulme and Sheard had written about climate change in Australasia:

> Hi Mike,
>
> I'm sure you will get some comments direct from Mike Rae in World Wildlife Fund Australia, but I wanted to pass on the gist of what they've said to me so far.
>
> They are worried that this may present a slightly more conservative approach to the risks than they are hearing from Australian scientists. In particular, they would like to see the section on variability and extreme events beefed up if possible. …
>
> I guess the bottom line is that if they are going to go with a big public splash on this they need something that will get good support from Australian scientists (who will certainly be asked to comment by the press).

Climategate takes on a new dimension with this revelation: political activists from an environmental lobby group are telling East Anglia climate scientists to rewrite sections of their paper, as it is less alarming than the message that Australian scientists have already presented for public consumption!

September 22, 1999: **email 0938018124**

In this next email, Keith Briffa raises one of the issues that is central to the infamous "hide the decline" email (which is the next email to be dealt with, below). It is therefore worth spending some time understanding what this is all about, in at least a simplified form. (Scientists interested in a more thorough account of all the methods used to "hide the decline" should refer to Steve McIntyre's extensive discussion of these issues.)

To measure the temperature of the planet, we obviously need some thermometers. Now, it would be nice if someone was able to invent a time machine, so that we could go back over the past few thousand (or hundred thousand) years, and place accurate scientific thermometers all over the planet, to make these measurements for us. Of course, this isn't possible, so scientists need to use other things as substitutes—or "proxies"—for these thermometers.

A key "temperature proxy" used by climate scientists is tree-ring data, namely, measurements of the patterns of the rings of trees that were growing hundreds or even thousands of years ago.

Now, even my sons (in elementary school at the time of writing) can tell me a handful of different factors that might influence the growth of a tree in a particular year: the amount of sun shining on it; the amount of rain it gets; how hot the weather is when it is growing; the conditions of the soil it is growing in; and the amount of carbon dioxide available for it to breathe in. We should imagine that the growth of a tree should, at the very least, depend on these five things.

So is a tree really a good thermometer?

As a physicist, such a proposition seems fraught with danger from the outset. Let's pretend, for the moment, that the growth of a tree depends only on these five factors, and no others. An elementary fact of mathematics, that I used to teach to my 15-year-old high school students, is that if you have five unknowns (these five factors at any given instant of time in a particular tree's lifetime), then you need at least five pieces of independent information to disentangle them all—and you need to know these five quantities to a high accuracy.

So to make any use at all of tree ring data, climate scientists would need at least four other completely independent "proxies". Is this what they do?

They do not.

At best, they have a few other "proxies", which themselves introduce more unknown quantities into the equation—like the differences between sea temperatures and air temperatures, or the sizeable differences of temperature across the planet. And instead of using these other "proxies" to try to disentangle temperature from the other relevant physical quantities, these climate scientists told the world that each of them is an independent measure of temperature.

It may seem unbelievable that these "scientists" were so mathematically incompetent that they didn't realize the folly of this assumption; but it must be remembered that they drew their limited membership from the ranks of the "soft" sciences, where mathematical modeling expertise is, sadly, often lacking.

Of course, these researchers realized that all of their "independent temperature proxies" didn't always give the same answers; so most of their work was involved in either figuring out which pieces of data agreed with which others (and ignoring or suppressing those that didn't), or concocting mathematically invalid ways of "averaging out" the various discrepant pieces of data, to give an artificial appearance of consistency.

Unfortunately for them, the game fell apart when one of their colleagues did what any good scientist would have done in the first place: they went to check that their main "temperature proxy"—the tree ring data—agreed with absolutely reliable and rock-solid temperature measurements: those made in a certain area in the United States, over the previous forty years, using actual, genuine, scientific thermometers.

And what did they find?

That while the thermometers said that temperatures had gone up, the rings of the trees in those same locations indicated that temperatures had gone down.

In other words, tree rings had been proved to be completely unreliable thermometers.

It is with this scandal in mind that Keith Briffa writes to writes to Mike Mann, Phil Jones, Tom Karl, and Chris Folland, expressing severe reservations about their contribution to the next Report by the Intergovernmental Panel on Climate Change, at that time in the revision stages:

> I know there is pressure to present a nice tidy story as regards "apparent unprecedented warming in a thousand years or more in the temperature proxy data" but in reality the situation is not quite so simple. We don't have a lot of temperature proxies that come right up to today and those that do (at least a significant number of tree proxies) have some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.

That is an understatement! Indeed, Briffa states his key opinion even more clearly:

> I believe that the recent warmth was probably matched about 1000 years ago.

This is a remarkable statement, which undermines the entire argument propounded by Briffa and his colleagues that global warming was "unprecedented".

Mike Mann responds to this catastrophic development:

> I walked into this hornet's nest this morning! Keith and Phil Jones have both raised some very good points. And I should point out that Chris Folland, through no fault of his own, but probably through me not conveying my thoughts very clearly to the others, definitely overstates any singular confidence I have in my own (Mann and coworkers') results.

In other words, Mann has no confidence in his own results!

Mann now engineers what became the infamous "green graph"—the green tree-ring line in the graph in the Intergovernmental Panel on Climate Change Report that mysteriously passes behind the other lines at the year 1961—and never emerges on the other side. First, he needs to fiddle the data, to make sure that the lines all cross at right place:

> I am perfectly amenable to keeping Keith's series in the graph, and can ask Ian Macadam (Chris?) to add it to the graph he has been preparing (nobody liked my own color and graphing conventions so I've given up doing this myself). The key thing is making sure the lines are vertically aligned in a reasonable way. I had been using the entire 20th century, but in the case of Keith's, we need to align the first half of the 20th century with the corresponding average values of the other lines, due to the late 20th century decline.

Satisfied with that solution, he then turns to the problem of that bothersome "late 20th century decline":

> So if Chris and Tom (?) are ok with this, I would be happy to add Keith's line to the graph. That having been said, it does raise a conundrum: We demonstrate … that the major discrepancies between Phil's and our line can be explained in terms of (statistical excuses). But that explanation certainly can't rectify why Keith's data, which has similar properties to Phil's data, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on—everyone in the room at the Intergovernmental Panel on Climate Change was in agreement that this was a problem and a potential distraction/ detraction from the reasonably consensus viewpoint we'd like to show with the Jones and coworkers' and Mann and coworkers' results.

In other words, there was no consensus at all at the Intergovernmental Panel on Climate Change—other than the participants' universal agreement that there was a problem with what they wanted to show. Mann is here telling us, in his own words, that there was an agenda to present a "consensus viewpoint"—that simply didn't exist in reality because of the science.

Mann now buries himself, by explaining what they should have done:

> So, if we show Keith's line in this plot, we have to comment that "something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the data and the potential factors that might lead to it being "warmer" than the Jones and coworkers' and Mann and coworkers' results? We would need to put in a few words in this regard. Otherwise, the skeptics would have a field day casting doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith in the … estimates from paleological data. I don't think that doubt is scientifically justified, and I'd hate to be the one to have to give it fodder!

In other words, Mann believes that all the lines should agree, but the actual data says otherwise; and he is loathe to give that "fodder" to the critics. Mann is in denial of the obvious conclusion: that the science is in doubt. He tries to pressure Briffa to come up with excuses why his data might not agree with the others.

Of course, we know that, ultimately, he gave up on this impossible task, and the troublesome "decline" was simply removed!

Mann-made global warming, indeed.

November 16, 1999: **email 0942777075**

That background now paves the way to our understanding the historic email that generations of schoolchildren to come will study as the catchphrase of the greatest scientific fraud in the history of mankind:

Phil Jones to Ray Bradley, Mike Mann, Malcolm Hughes, Keith Briffa, and Tim Osborn, regarding a diagram for a World Meteorological Organization Statement:

> I've just completed Mike's Nature trick of adding in the real temperatures to each series for the last 20 years (i.e. from 1981 onwards) and from 1961 for Keith's to hide the decline.

Those thirty-three words summarize the hoax so magnificently succinctly that the Nobel Committee should consider retrieving their Peace Prize from the Intergovernmental Panel on Climate Change and Al Gore, and re-issuing it as a Literature Prize to Phil Jones.

This email was sent less than two months after the one analyzed above. Clearly, Mike Mann's problems with Keith Briffa's data—that it didn't agree with the real temperature measurements from 1961 onwards—had by this time spread to the data for the other "temperature proxies", albeit only from 1981 onwards. Jones reveals that Mann did not address this problem by making honest note of it in the paper that he and his co-authors published in Nature, but rather by fraudulently substituting the real temperature data into the graphs, for the past twenty or forty years as required.

That Mann did so would, in and of itself, disqualify him and all of his research from any future consideration in the annals of science; but here we have the other leader of the field, Phil Jones, bragging that he admired the "trick" so much that he adopted it himself. Moreover, his email was sent to the major players who dominated this field. It is the silence of these conspirators over the intervening decade that has forever damned the field of "climate science" to a state of irreversible ignominy, and will almost certainly lead to the incarceration of the principal perpetrators in the near future.

July 5, 2000: **email 0962818260**

Mike Kelly, of the Climatic Research Unit at the University of East Anglia, writes to Mike Hulme and Tim O'Riordan:

> Had a very good meeting with Shell yesterday. Only a minor part of the agenda, but I expect they will accept an invitation to act as a strategic partner, and will contribute to a studentship fund, though under certain conditions.

And they accuse skeptics of "being in the pockets" of Big Oil?

> I'm talking to Shell International's climate change team, but this approach will do equally for the new Foundation, as it's only one step or so off Shell's equivalent of a board level. I do know a little about the Foundation and what kind of projects they are looking for. It could be relevant for the new building, incidentally, though opinions are mixed as to whether it's within the remit.

Sounds lucrative. Buildings don't come cheap.

August 23, 2000: **email 0967041809**

In this email we get an insight into how the politics of propaganda completely overrode the rules of good scientific practice, when it came to publications on "climate science". Steve Schneider of the Department of Biological Sciences at Stanford University in the United States complains to a number of his international colleagues:

> … please get rid of the ridiculous "inconclusive" for the 34% to 66% subjective probability range. It will convey a completely different meaning to lay persons—read decision makers—since that probability range represents medium levels of confidence, not rare events. A phrase like "quite possible" is closer to popular lexicon, but "inconclusive" applies as well to very likely or very unlikely events and is undoubtedly going to be misinterpreted on the outside.

To anyone even vaguely familiar with probability and statistics, Schneider's suggestion is unforgiveable; and it doesn't take a Ph.D. to understand why. Forget about climate change, for the moment, and consider the simpler example of tossing a coin. If the coin is fair, and it is tossed fairly, then the likelihood of getting "heads" is 50%. Now, imagine that you had to describe how sure you are that you would get "heads" on the next toss, to your boss—or your spouse—without using any numbers. "It's inconclusive" would accurately convey the fact that it's just as likely that you would not get "heads" as it is that you would. "It's quite possible", on the other hand, conveys the impression that it's a possibility that is quite likely; it biases the language in one direction, without faithfully conveying equal likelihood that reality could go in the exact opposite direction.

Indeed, placing any emphasis at all on a 34% to 66% confidence interval is a complete misapplication of probability and statistics. Standard scientific practice is to only consider a result to be significant if the probability of it being true is estimated to be greater than some pre-determined threshold—typically 95%, for everyday analyses, or some more stringent threshold if the ramifications of getting it wrong are more grave.

Tom Karl, Director of the National Oceanic and Atmospheric Administration's National Climatic Data Center, compounds the comedy:

> Steve, I agree with your assessement of "inconclusive"—"quite possible" is much better and we use "possible" in the United States National Assessment. Surveys have shown that the term "possible" is interpreted in this range by the public.

Despite Karl completely agreeing with his butchering of the language, Schneider is concerned that Karl's term is still not alarmist enough. His response reminds one of Sir Humphrey in Yes, Minister:

> Great Tom, I think we are converging to much clearer meanings across various cultures here. Please get the "inconclusive" out! By the way, "possible" still has some logical issues as it is true for very large or very small probabilities in principle, but if you define it clearly it is probably OK—but "quite possible" conveys medium confidence better—but then why not use "medium confidence", as the 3 rounds of review over the guidance paper concluded after going through exactly the kinds of discussions were having now?

Indeed, if they continued this farce for long enough, they would eventually conclude that they may as well say that it is "overwhelmingly likely"! Remember, we are here talking about a scenario that—even according to their own calculations—was just as likely to be wrong as it was right!

September 11, 2000: **email 0968705882**

Filippo Giorgi, Senior Scientist and Head of the Physics of Weather and Climate Section of The Abdus Salam International Centre for Theoretical Physics in Trieste, Italy, writes to the other Lead Authors of Chapter 10 of the latest Intergovernmental Panel on Climate Change (IPCC) Report. In his first paragraph he makes a comment that I will return to below:

> We said that one thing to look at was the agreement with the old data and thus I noticed that relaxing the criteria determining what "agreement" means would yield a greater agreement.

He then details his serious concerns about how the IPCC Report is being drafted:

> First let me say that in general, as my own opinion, I feel rather uncomfortable about using not only unpublished but also un-reviewed material as the backbone of our conclusions (or any conclusions). I realize that Chapter 9 of the Report is including new stuff, and thus we can and need to do that too, but the fact is that in doing so the rules of the IPCC have been softened to the point that in this way the IPCC is not any more an assessment of published science (which is its proclaimed goal), but the production of results. The softened condition that the models themselves have to be published does not even apply, because the Japanese model, for example, is very different from the published one which gave results not even close to the actual … version … . Essentially, I feel that at this point there are very little rules and almost anything goes. I think this will set a dangerous precedent which might undermine the IPCC's credibility, and I am a bit uncomfortable that now nearly everybody seems to think that it is just ok to do this. Anyway, this is only my opinion, for what it is worth.

Further on in the email, he describes the criterion for determining that models "agree":

> 1) Do we soften our requirement, i.e. from "all the models except one need to agree with each other" to "all the models except two need to agree with each other" agreement? I do not feel strongly about it but am more in favor of not softening the criterion. We are looking for confidence and model agreement and should have stringent requirements on it.

In other words—to fill in the missing emails, that we do not have—what has happened is the following: The scientists previously decided that they would accept that all the models "agree" if either all of them agree with each other, or all but one of them agree with each other. But in preparing their Chapter for the Report, they found that two of the models did not agree with the others. Thus it has been suggested that they now "move the goalposts"—after the event—to redefine "agreement" so that two of the models be allowed disagree with the others!

In fact, this entire "criterion for agreement" is absolute nonsense in the first place, flying in the face of the most elementary principles of statistics, as I will discuss shortly. But even ignoring that, the idea that they can avoid the "inconvenient truth" of their results by moving the goalposts after the fact is, in and of itself, scientific fraud of the highest order.

To his great credit, Filippo is arguing against this form of subterfuge.

He continues:

> 2) Do we include the data that disagree in the analysis? I say yes, not having time for more detailed analysis as to why they should not be included. In Chapter 9 of the Report they are presented as bracketing the answers, not as being wrong. This is the problem of not having published research on this: perhaps a paper would have excluded them on scientific grounds, but can we, at this point? I am not sure we can have solid enough foundations to legitimate it. Besides, I have done the analysis without them as well, and things did not change almost at all.

To any scientist with even a rudimentary knowledge of statistics, this paragraph shows that the entire IPCC had absolutely no idea what they were doing. Data that disagree with the other data ("outliers" in the jargon of mathematics, although I will not use that term here) are of critical importance: understanding them is key to understanding what your data is telling you; they provide the ultimate "reality check" that you really know what you are doing. They are not "wrong", as these "scientists" are suggesting; they should not be simply omitted. Nor should they simply be presented as "bracketing" the data that do agree.

Again, Filippo's comments are a credit to his wisdom and integrity: he urges—correctly—that if there is no valid reason to exclude the data, then it must be presented as it stands.

September 12, 2000: **email 0968774000**

Following on from the previous email, Filippo Giorgi writes to the various Lead Authors, having obtained at least partial agreement with his arguments. He reiterates his opinion:

> I myself think that material for a document as important as the Intergovernmental Panel on Climate Change's Third Assessment Report cannot be drawn from last-minute barely quality-checked and un-peer-reviewed material (people have barely looked at the Max Planck Institute run that was completed last Friday!).

September 14, 2000: **email 0968941827**

Recall the discussion above about the criterion used to determine if a set of models "agreed" with each other. Hans von Storch argues against moving the goalposts:

> I have already indicated that I favour the "all models but one have to agree" version. Obviously, this choice of criterion is arbitrary, but it was made before we did the analysis. By changing the criterion after we have seen the data, we may be targeted by critics for biased rules. Using material which is unpublished and unreviewed is already a bit shaky (Hans Oerlemans is unwilling to participate in the Intergovernmental Panel on Climate Change process because of a similar incident in the 1995 report!).

Peter Whetton argues that the criterion is now too stringent, because it gives them less chance of getting "agreement" purely by luck! He points out that the criterion was previously only used for five models, for which

> … agreement … could be expected 37% of the time just by chance … . With nine models the equivalent figure for "all models but one have to agree" is only 3.5%, and it is still much lower for "all models but two have to agree" (18%)… (assuming that my somewhat rusty probability calculations are correct). It really depends on what we had understood the purpose of the criterion to be. I am not certain how much this was discussed.

As noted above, standard scientific practice is to ensure that the chances of getting agreement purely by luck is less than some percentage, often 5%. To argue that the criterion is too strong because the chance of such a "false (lucky) positive" is only 3.5%—and that the previous situation of allowing a 37% chance of false positive is far preferable—is simply astounding: it shows a poor understanding of the fundamental principles of statistics.

But more astonishing is Whetton's lack of confidence in performing an elementary calculation in probability theory, that 16-year-old high school students routinely calculate every day! It would be equivalent to Tiger Woods expressing a lack of confidence in his ability to decide which wood he should use for a particular hole …

September 22, 2000: **email 0969618170**

Tom Crowley of the Department of Oceanography at the Texas A&M university writes to Malcolm Hughes and Keith Briffa, about the huge problems involved in trying to figure out if the various "temperature proxies" are measuring temperatures, carbon dioxide levels, or some other complicated combination:

> As I discuss in my … paper the "anomalous" late 19th century warming also occurs in a … tree ring record from central Colorado, the Urals record of Keith Briffa, and the east China … temperature record of Zhu.
>
> Alpine glaciers also started to retreat in many regions around 1850, with one-third to one-half of their full retreat occurring before the warming that commenced about 1920.
>
> …
>
> So, are you sure that some carbon dioxide effect is responsible for this? May we not actually be seeing a warming?

Malcolm Hughes's response exemplifies the utter confusion of these researchers:

> I tried to imply in my e-mail, but will now say it directly, that although a direct carbon dioxide effect is still the best candidate to explain this effect, it is far from proven. In any case, the relevant point is that there is no meaningful correlation with local temperature.

Why should these topics be so dangerous to write explicitly that they must be implied?

In the mathematical jargon I have omitted from this email—and many others—these "scientists" explain that sometimes the "proxies" they are using (tree rings, etc.) seem to measure temperature, and sometimes they don't (the extremely blunt, simplistic, and naive mathematical test that they use to determine this—something so simple that it is used every day by high school students— is called "correlation").

What they do is "cherry pick" those proxies that seem to give the "right" answers, and ignore those that don't. That's not just bad science: it's completely wrong.

Hughes's next comment exemplifies this "cherry picking":

> I am confident that, before 1850, they do contain a record of temperatures changing over decades. I am equally confident that, after that date, they are recording something else.

And, at the end of the day, that's what this "cherry picking" is based on: the gut feeling of these scientists.

February 27, 2001: **email 0983286849**

Phil Jones is upset that Julia Uppenbrink, the editor at Science, did not send a piece to them to review, which would have allowed them to block it:

> Obviously this isn't great as none of us got to review it. Odd that she didn't send it to one of us here as she knew we were writing the article she asked us to!

It is the height of arrogance for these scientists to assume that every single article published relating to climate science in any way would automatically be sent to them, for them to be able to veto.

March 2, 2001: **email 0983566497**

Chick Keller, of the Institute of Geophysics and Planetary Physics at the University of California at San Diego, United States, writes to Mike Mann, Ray Bradley, Phil Jones, Keith Briffa, Tom Crowley, Jonathan Overpeck, Tom Wigley, and Mike MacCracken, pointing out problems in the historical temperature estimates obtained from individual "proxy" methods:

> Anyone looking at the records gets the impression that the temperature variation for many individual records or sites over the past 1000 years or so is often larger than 1°Celsius. … And they see this as evidence that the 0.8°Celsius or so temperature rise in the 20th century is not all that special.

He then makes note of a trick that they have used to mask this effect:

> The community of climate scientists, however, in making averages of different proxies gets a much smaller amplitude of about 0.5°Celsius, which they say shows that reasonable combinations of effects can indeed explain this and that the 20th century warming is unique.

Keller realizes the mistake inherent in this trick shortly. First, he provides an excellent summary of the debate:

> Thus, the impasse—one side the skeptics pointing to large temperature variations in many records around the globe, and the other side saying, "Yes, but not at the same time and so, if averaged out, is no big deal."

He then points out that this glib brush-off is simply not valid:

> But, just replying that events don't happen at the same time (sometimes by a few decades) is the reason might not be enough. It seems to me that we must go one step further. We must address the question: what effects can generate large … temperature variations over hundreds of years, regional though they may be (and, could these occur at different times in different regions due to shifting climate patterns)? If we can't do this, then there might be something wrong with our rationale that the average does not vary much even though many regions see large variations. This may be the nub of the disagreement, and until we answer it, many careful scientists will decide the issue is still unsettled, and that indeed climate in the past may well have varied as much or more than in the last hundred years.

This remarkable statement—mailed to all of the key players in this scandal—shows that they knew, clearly, more than eight years before the Climategate whistle-blower released these emails, that the entire basis of their claims was on shaky ground.

In his last paragraph, Keller points out the elementary mathematical error in the "averaging trick":

> Also, I note that most proxy temperature records claim timing errors of … 50 years ahead or behind the correct date or so. What is the possibility that records are cancelling each other out on variations in the hundred-year timeframe due simply to timing errors?

There are, in fact, many more mathematical reasons why the "averaging trick" is completely wrong; but Keller's observation is completely correct, and by itself discredits the entire discipline of work establishing these "multi-proxy" historical temperature estimates.

May 2, 2001: **email 0988831541.txt**

Mike Mann criticizes Ed Cook's work with colleague Jan Esper—not for poor methods or invalid conclusions, but rather because it was being used publicly, before being able to be blocked through the peer review process. Firstly, he applies the "peer group pressure" argument:

> We may have to let the peer-review process decide this, but I think you might benefit from knowing the consensus of the very able group we have assembled in this email list, on what Esper and you have done?

Cook parries admirably:

> Of course, I know everyone in this "very able group" and respect their opinions and scientific credentials. The same obviously goes for you. That is not to say that we can't disagree. After all, consensus science can impede progress as much as promote understanding.

Mann is taken aback, and tries a different tack:

> I don't in any way doubt yours and Jan's integrity here.
>
> I'm just a bit concerned that the result is getting used publicly, by some, before it has gone through the gauntlet of peer review. Especially because it is, whether you condone it or not, being used as we speak to discredit the work of us, and Phil and coworkers; this is dangerous. I think there are some legitimate issues that need to be sorted out … .
>
> I'd be interested to be kept posted on what the status of the manuscript is.

Cook responds with a level of integrity foreign to Mann's mind-set:

> Unfortunately, this global change stuff is so politicized by both sides of the issue that it is difficult to do the science in a dispassionate environment. I ran into the same problem in the acid rain/forest decline debate that raged in the 1980s. At one point, I was simultaneous accused of being a raving tree hugger and in the pocket of the coal industry. I have always said that I don't care what answer is found as long as it is the truth or at least bloody close to it.

May 17, 2001: **email 0990119702**

Ed Cook makes valid statistical and mathematical criticisms of the error estimation methods being used by Mike Mann and colleagues:

> I have growing doubts about the validity and use of error estimates that are being applied to reconstructions … . (mathematical reasons follow). But I really think that uncertainty bars on graphs, as often presented, may potentially distort and unfairly degrade the interpreted quality of reconstructions. So, are the uncertainty bars better than nothing? I'm not so sure.

Mike Mann responds by agreeing that the estimates of uncertainties are wrong, but that wrong estimates are better than nothing:

> What you say is of course true, but we have to start somewhere. …
>
> I firmly believe that a reconstruction without some reasonable estimate of uncertainty is almost useless! … I believe that this is absolutely essential to do, whether or not we can do a perfect job.

Cook is arguing that misleading estimates of uncertainties are worse than not presenting any estimates at all; Mann is arguing that graphs without error estimates would not look credible, which is more important than the estimates actually being meaningful.

Cook is correct.

May 23, 2001: **email 0990718382**

John Christy explains the events of the filming of an episode of "20/20" for the American Broadcasting Company, in which he fears he will be quoted out of context, but he includes the following comment:

> However, I do agree with the "20/20" host's premise … that the dose of climate change disasters that have been dumped on the average citizen is designed to be overly alarmist and could lead us to make some bad policy decisions. (I've got a good story about the writers of the TIME cover piece a couple of months ago that proves they were not out to discuss the issue but to ignore science and influence government.)

Mike Mann's response to this comment is only thinly veiled:

> Your comments below remain disturbingly selective and myopic, and we have dealt with similar comments many times over … If the American Broadcasting Company is looking to do a hatchet job on Intergovernmental Panel on Climate Change so be it (this doesn't surprise me—"20/20" co-anchor John Stossel has an abysmal record in his treatment of environmental issues, from what I have heard), but I'll be very disturbed if you turn out to have played into this in a way that is unfair to your co-authors on Chapter 2of the Intergovernmental Panel on Climate Change Report, and your colleagues in general. This wouldn't have surprised me coming from certain individuals, but I honestly expected more from you…

July 2, 2001: **email 0994083845**

Ian Harris of the Climatic Research Unit at the University of East Anglia writes to the Norwich Green Party mailing list, responding to a comment that natural events can cause climate changes that swamp any effects of mankind:

> We're looking at an unprecedented acceleration in temperature … Even if it turns out to be naturally-occurring, who's willing to take that chance? We should be trying to wean ourselves off of unsustainable energy generation and use anyway.

This is a remarkable admission: even if the scientists are completely wrong, we "should" force changes on mankind that could cost trillions of dollars, on simplistic ideological grounds?

December 17, 2001: **email 1008619994**

Keith Briffa, a referee of a paper submitted to Science by Ed Cook and Jan Esper, tells Cook:

> I simply would not like to see you write a paper that puts out a confused message with regard to the global warming debate, leaving ambiguity as to your opinion on the validity of the Mann curve ("hockey stick") … .

Briffa is abusing his position of power as a reviewer of the paper, making it clear to Cook that he will block its publication if they deviate from the "party line". He twists the knife, using personal intimidation:

> I would not like this affair to ruin my Christmas, as it surely will if it is the cause of our falling out.

In other words, change the paper, or you are no longer a friend and colleague.

Finally, he lays down his expectations:

> I am totally confident that after a day's rephrasing this paper can go back and be publishable to my satisfaction by Science.

March 22, 2002: **email 1018045075**

Keith Briffa and Tim Osborn issued a comment on the paper by Ed Cook and Jan Esper published in Science. Both papers question the work of Mike Mann and coworkers. Mike Mann admonishes all of them, copying the email to two staff of The American Association for the Advancement of Science:

> Sadly, your piece on the Esper and Cook paper is more flawed than even the paper itself. Ed, the Associated Press release that appeared in the papers was even worse. Apparently you allowed yourself to be quoted saying things that are inconsistent with what you told me you had said. You three all should have known better. … In the meantime, there is a lot of damage control that needs to be done and, in my opinion, you've done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence. Many of us are very concerned with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for the reasons I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here.

Keith Briffa replies, refuting Mann's insinuations and rebuffing his intimidations:

> Given the list of people to whom you have chosen to circulate your message(s), we thought we should make a short, somewhat formal, response here. I am happy to reserve my informal response until we are face to face!
>
> …
>
> Finally, we have to say that we do not feel constrained in what we say to the media or write in the scientific or popular press, by what the sceptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "sceptics" have their own dishonest agenda—we have no doubt of that. If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.

Mann is demonstrating his need to be the unchallenged leader of the team, and his annoyance with anyone who does not toe his line.

March 11, 2003: **email 1047388489**

A paper by astrophysicists Willie Soon and Sallie Baliunas was published by Climate Research, which concluded that "the 20th century is probably not the warmest nor a uniquely extreme climatic period of the last millennium." Phil Jones writes a number of emails to his colleagues. In the first:

> Tim Osborn has just come across this. Best to ignore probably, so don't let it spoil your day. I've not looked at it yet. It results from this journal having a number of

> editors. The responsible one for this is a well-known skeptic in New Zealand. He has let a few papers through by (skeptics) Michaels and Gray in the past. I've had words with Hans von Storch about this, but got nowhere.

His conclusions are remarkable, given that he admits that he hasn't even looked at the paper as yet. His next email is sent after having read a small amount:

> I looked briefly at the paper last night and it is appalling … I'll have time to read more at the weekend …
>
> The phrasing of the questions at the start of the paper determine the answer they get. They have no idea what multiproxy averaging does.

In other words, because these astrophysicists don't use the mathematically and statistically incorrect method of "averaging" the various temperature proxies to hide the variability of temperature in the past, they're not a member of the club!

He continues:

> Writing this I am becoming more convinced we should do something …
>
> I will be emailing the journal to tell them I'm having nothing more to do with it until they rid themselves of this troublesome editor. A Climatic Research Unit person is on the editorial board, but papers get dealt with by the editor assigned by Hans von Storch.

Recall, this action is being taken before he has even read the whole paper even a single time.

Mike Mann replies:

> The Soon and Baliunas paper couldn't have cleared a "legitimate" peer review process anywhere. That leaves only one possibility—that the peer-review process at Climate Research has been hijacked by a few skeptics on the editorial board. And it isn't just De Freitas; unfortunately, I think this group also includes a member of my own department… The skeptics appear to have staged a "coup" at Climate Research (it was a mediocre journal to begin with, but now it's a mediocre journal with a definite "purpose").

In other words, the publication of a single paper critical of their work—which is how any healthy discipline of science is supposed to work—is, automatically, evidence of a "hijacking" of an entire peer-reviewed journal.

Mann urges his colleagues to start a witch-hunt:

> Folks might want to check out the editors and review editors:
>
> link to a page on Climate Research's website listing the editors

Despite the paper having barely been looked at, Mann immediately starts to plan their retribution:

> I told Mike MacCracken that I believed our only choice was to ignore this paper. They've already achieved what they wanted—the claim of a peer-reviewed paper. There is nothing we can do about that now, but the last thing we want to

> do is bring attention to this paper, which will be ignored by the community on the whole…
>
> It is pretty clear that the skeptics here have staged a bit of a coup, even in the presence of a number of reasonable folks on the editorial board (Whetton, Goodess, …). My guess is that Von Storch is actually with them (frankly, he's an odd individual, and I'm not sure he isn't himself somewhat of a skeptic himself), and with Von Storch on their side, they would have a very forceful personality promoting their new vision.
>
> There have been several papers by Pat Michaels, as well as the Soon and Baliunas paper, that couldn't get published in a reputable journal.
>
> This was the danger of always criticising the skeptics for not publishing in the "peer-reviewed literature". Obviously, they found a solution to that—take over a journal!

We now see what Mann and colleagues are so upset about: they believed that their cosy club was safe from intruders, as the only way to challenge them was to be published in a "peer-reviewed" journal—which they themselves controlled. But now that the fortifications were breached, the entire house of cards was in danger of falling down.

Mann immediately suggests black-balling the journal that dared to challenge their authority:

> So what do we do about this? I think we have to stop considering Climate Research as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal. We would also need to consider what we tell or request of our more reasonable colleagues who currently sit on the editorial board…

So it's OK for their gang to control the "peer review" process, but not OK for sceptics to have any say?

March 11, 2003: **email 1047474776**

Mike Mann writes:

> I do … think there is a particular problem with Climate Research. This is where my colleague Pat Michaels now publishes exclusively, and his two closest colleagues are on the editorial board and review editor board. So I promise you, we'll see more of this there, and I personally think there is a bigger problem with the "messenger" in this case…

Phil Jones replies:

> Can we not address the misconceptions by finally coming up with definitive dates for the Little Ice Age and Medieval Warm Period and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.

Using their weight of numbers to "redefine" these historical periods? Is this the genesis of the Wikipedia censorship scandal?

March 12, 2003: **email 1047484387**

In trying to decide which journal they would submit their attack on the Climate Research paper to, Mike Mann illustrates that it is most definitely "not what you know, but who you know" in this field of science:

> Either journal would be good, but Eos (a journal) is an especially good idea. Both Ellen Mosley-Thompson and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.
>
> …
>
> If there is group interest in taking this tack, I'd be happy to contact Ellen or Keith about the potential interest in Eos, or I'd be happy to let Tom or Phil to take the lead too…

March 12, 2003: **email 1047489122**

Mike Mann discusses the difficulties in creating the Eos article after learning that the different sets of results are inconsistent:

> There are some notable differences …. The position of Crowley and Lowery, in particular, is quite inconsistent between our respective comparisons. …

Mann now again suggests that they "cherry-pick" and present only those results that support the message that they would like to portray:

> So, in short, let's see what we get, and then discuss any similarities or differences with your result, then make a decision as to what to show in the Eos piece. I'm sure we can come up with something we're all happy with…

April 23, 2003: **email 1051156418**

Tom Wigley writes to a large number of recipients, building on the idea that every critical or skeptical paper published in the peer-reviewed literature must be due to a "conspiracy of skeptics":

> Danny Harvey and I refereed a paper by skeptic Pat Michaels and coworkers and said it should be rejected. We questioned the editor (de Freitas again!) and he responded, saying:
>
> The manuscript was reviewed initially by five referees. … The other three referees, all reputable atmospheric scientists, agreed it should be published subject to minor revision. Even then I used a sixth person to help me decide. I took his advice and that of the three other referees and sent the manuscript back for revision. It was later accepted for publication. The refereeing process was more rigorous than usual.
>
> On the surface this looks to be above board—although, as referees who advised rejection, it is clear that Danny and I should have been kept in the loop and seen how our criticisms were responded to.

Again, Wigley perpetuates the arrogant myth that this small club of scientists should have the right to interfere with, and ultimately veto, the review and publication process for each and every paper published in their field. Such censorship is not how a healthy discipline of science operates; indeed, any discipline that operates in this manner is not "science" at all, but mere religious dogma.

Wigley continues:

> I suspect that de Freitas deliberately chose other referees who are members of the skeptics camp. I also suspect that he has done this on other occasions. How to deal with this is unclear, since there are a number of individuals with genuine scientific credentials who could be used by an unscrupulous editor to ensure that "anti-greenhouse" science can get through the peer review process (Legates, Balling, Lindzen, Baliunas, Soon, and so on). The peer review process is being abused, but proving this would be difficult.

This is a damning admission by Wigley: he acknowledges that these skeptics have impeccable scientific credentials; the only reason that they should be banned from reviewing papers for journal publication is that they don't buy into their dogma of global warming! This email dispels any doubt that this cosy club redefined "peers" to mean "scientists who agree with us"—which makes a mockery of the entire idea of "peer review".

The ultimate irony in all this, of course, is that skepticism is not a scientific insult, but rather an essential tenet of the scientific method. Only fundamentalist theological debates brand skepticism a heresy.

April 24, 2003: **email 1051190249**

Tim Carter, research professor at the Finnish Environment Institute, suggests to Tom Wigley a way of ensuring that no papers get published without their ability to veto:

> On the Climate Research issue … I wonder if a review of the refereeing policy is in order. The only way I can think of would be for all papers to go through two Editors rather than one, the former to have overall responsibility, the latter to provide a second opinion on a paper and reviewers' comments prior to publication. A General Editor would be needed to adjudicate in the event of disagreement. Of course, this could then slow down the review process enormously. However, without an editorial board to vote someone off, how can suspect Editors be removed except by the Publisher (in this case, Inter-Research, the publishers of Climate Research).

Tom Wigley replies:

> Re Climate Research, I do not know the best way to handle the specifics of the editoring. Hans von Storch is partly to blame—he encourages the publication of crap science "in order to stimulate debate". One approach is to go direct to the publishers and point out the fact that their journal is perceived as being a medium for disseminating misinformation under the guise of refereed work. I use the word "perceived" here, since whether it is true or not is not what the publishers care about—it is how the journal is seen by the community that counts.

In other words, Wigley is unambiguously advocating a "smear campaign" against the journal. I have no doubt that the key phrase, "whether it is true or not", will be a key piece of evidence in Wigley's trial.

Wigley continues:

> I think we could get a large group of highly credentialed scientists to sign such a letter—50+ people. Note that I am copying this view only to Mike Hulme and Phil Jones. Mike's idea to get the editorial board members to resign will probably not work—we must get rid of von Storch too, otherwise the holes will eventually fill up with people (skeptics) like Legates, Balling, Lindzen, Michaels, Singer, etc. I have heard that the publishers are not happy with von Storch, so the above approach might remove that hurdle too.

Wonderful!

April 24, 2003: **email 1051202354**

Mike Mann responds to Tom Wigley's suggestions, again highlighting the fact that the politics is more important than the science:

> This might all seem laughable, if it weren't the case that they've gotten the (Bush) White House Office of Science & Technology taking it as a serious matter (fortunately, Dave Halpern is in charge of this project, and he is likely to handle this appropriately, but not without some external pressure).

So, the conspirators are fortunate to even have a man on the ground in the White House itself.

Mann continues:

> Here, I tend to concur at least in spirit … that other approaches may be necessary. I would emphasize that there are indeed, as Tom notes, some unique aspects of this latest assault by the skeptics which are cause for special concern. This latest assault uses a compromised peer-review process as a vehicle for launching a scientific disinformation campaign (often vicious and personal) under the guise of apparently legitimately reviewed science, allowing them to make use of the "Harvard" moniker in the process.

Mann's inferiority complex is palpable: he cannot bear to be criticized by an astrophysicist—from Harvard, no less.

However, his relief at being able to control almost all of the media is equally evident:

> Fortunately, the mainstream media never touched the story (mostly it has appeared in papers owned by Murdoch and his crowd, and dubious fringe on-line outlets). Much like a server which has been compromised as a launching point for computer viruses, I fear that Climate Research has become a hopelessly compromised vehicle in the skeptics' (can we find a better word?) disinformation campaign, and some of the discussion that I've seen (e.g. a potential threat of mass resignation among the legitimate members of the Climate Research editorial board) seems, in my opinion, to have some potential merit.

Remember, this retaliation is in response to the publication of a single critical paper—which in any healthy discipline of science is an absolutely vital, every-day occurrence.

Mann continues to engineer the "spin-doctoring" of this retaliation:

> This should be justified not on the basis of the publication of science we may not like, of course, but based on the evidence (e.g. as provided by Tom and Danny Harvey, and I'm sure there is much more) that a legitimate peer-review process has not been followed by at least one particular editor.

Mark Eakin adds:

> Since the White House has shown interest in this paper, the Office of Science & Technology Policy really does need to receive a measured, critical discussion of flaws in Soon and Baliunas's methods. I agree with Tom that a noted group … such as Mann, Crowley, Briffa, Bradley, Jones and Hughes should spearhead such a letter. Many others of us could sign on in support. This would provide Dave Halpern with the ammunition he needs to provide the White House with the needed documentation that hopefully will dismiss this paper for the slipshod work that it is.

"Ammunition" it is, indeed—for an attempted character assassination. Mike Mann confirms that he has supplied this "ammunition" to their man in the White House:

> Indeed, I have provided David Halpern with a written set of comments on the offending paper(s) for internal use, so that he was armed with specifics as he confronts the issue within the Office of Science & Technology Policy. He may have gotten additional comments from other individuals as well—I'm not sure. I believe that the matter is in good hands with Dave, but we have to wait and see what happens.

June 4, 2003: **email 1054756929**

Ed Cook writes to Keith Briffa:

> Now something to ask from you. Actually somewhat important too. I got a paper to review (submitted to the Journal of Agricultural, Biological, and Environmental Sciences), written by a Korean guy and someone from Berkeley, that claims that the method of mathematics that we use in our field (reverse regression) is wrong, biased, lousy, horrible, etc. They use your … reconstruction as the main whipping boy.

We now get another glimpse into the impeccable data storage and record-keeping procedures of these "scientists":

> I have a file that you gave me in 1993 that comes from your 1992 paper. Below is part of that file. Is this the right one? Also, is it possible to resurrect the column headings? I would like to play with it in an effort to refute their claims.

Cook continues:

> If published as is, this paper could really do some damage. It is also an ugly paper to review because it is rather mathematical, with a lot of filter theory stuff in it. It

> won't be easy to dismiss out of hand as the mathematics appears to be correct theoretically, but it suffers from the classic problem of pointing out theoretical deficiencies, without showing that their improved inverse regression method is actually better in a practical sense. So they do lots of computer stuff that shows the superiority of their method and the deficiencies of our way of doing things, but never actually show how their method would change your reconstruction from what you produced. Your assistance here is greatly appreciated.

This is a remarkable email: Cook is admitting that the paper that he has been entrusted to review "won't be easy to dismiss out of hand as the mathematics appears to be correct"! He opines that it suffers the "classic problem" of not hand-holding these amateurs through the process of doing things correctly!

If there was ever any doubt that the fundamental principles of science had been lost in this discipline, this email absolutely demolishes it. Cook should never have been entrusted with the task of reviewing this paper.

June 4, 2003: **email 1054757526**

Mike Mann writes to his many co-authors about the "shock and awe" paper that they are preparing for publication in Eos. We see the "War on the Medieval Warm Period" germinating:

> I think that trying to adopt a timeframe of 2000 years, rather than the usual 1000 years, addresses a good earlier point that Jonathan Overpeck made … that it would be nice to try to "contain" the putative "Medieval Warm Period", even if we don't yet have data available that far back.

That the goal of discrediting the existence of the Medieval Warm Period was decided upon before having any definitive data one way or the other (Mann describes some preliminary results, but says that they are not "kosher") provides irrefutable evidence that this was an ideological crusade, not a scientific investigation.

July 3, 2003: **email 1057941657**

The Director of Climate Research, Otto Kinne, investigated the complaints about the editorial and refereeing process, and wrote:

> Dear colleagues,
>
> In my 20 June 2003 email to you I stated, among other things, that I would ask Climate Research editor Chris de Freitas to present to me copies of the reviewers' evaluations for the two Soon and coworker papers.
>
> I have received and studied the material requested.
>
> Conclusions:
>
> 1) The reviewers consulted (four for each manuscript) by the editor presented detailed, critical and helpful evaluations.

2) The editor properly analyzed the evaluations and requested appropriate revisions.

3) The authors revised their manuscripts accordingly.

Summary:

Chris de Freitas has done a good and correct job as editor.

Mike Hulme forwards this email to Phil Jones, Tom Wigley, and Mike Mann:

So, this would seem to be the end of the matter as far as Climate Research is concerned.

Mike Mann is not willing to let it go at that:

It seems to me that this "Kinne" character's words are disingenuous, and probably supports what de Freitas is trying to do. It seems clear we have to go above him. I think that the community should, as Mike Hulme has previously suggested in this eventuality, terminate its involvement with this journal at all levels—reviewing, editing, and submitting, and leave it to wither way into oblivion and disrepute.

Tom Wigley realizes that such tactics amount to scientific blackmail:

I agree that Kinne seems like he could be a de Freitas clone. However, what would be our legal position if we were to openly and extensively tell people to avoid the journal?

Ben Santer has no such qualms:

Based on Kinne's editorial, I see little hope for more enlightened editorial decision-making at Climate Research. Tom, Richard Smith and I will eventually publish a rebuttal to the Douglass and coworkers paper. We'll publish this rebuttal in the Journal of Geophysical Research—not in Climate Research.

July 22, 2003: **email 1058906971**

Mike Mann exerts "peer group pressure" on the co-authors of the Eos article to start yet another petition—this time to be sent to the United States Congress:

Dear fellow Eos co-authors,

Given the continued assault on the science of climate change by some on Capitol Hill, Michael Oppenheimer and I thought it would be worthwhile to send this letter to various members of the United States Senate, accompanied by a copy of our Eos article.

Can we ask you to consider signing on with Michael and me (providing your preferred title and affiliation). We would like to get this out as soon as possible.

Jonathan Overpeck realizes the danger in signing on to such a petition, without carefully considering the huge ramifications:

> I'm not too comfortable with this, and would rather not sign—at least not without some real time to think it through and debate the issue. It is unprecedented and political, and that worries me.
>
> My vote would be that we don't do this without a careful discussion first.

To his great credit, Overpeck understands the ramifications of crossing the line between honest scientific research, and pure political activism:

> What are the precedents and outcomes of similar actions? I can imagine a special-interest organization or group doing this, like all sorts of other political actions, but is it something for scientists to do as individuals?
>
> It just seems strange, and for that reason I'd advise against doing anything with out real thought, and certainly a strong majority of co-authors in support.

Is it acceptable for taxpayer-funded scientists to advocate political issues?

July 31, 2003: **email 1059664704**

Tim Osborn writes to Mike Mann, trying to make sense of some of Mann's data, which appears to have simplistic estimates of uncertainties. After an exchange in which Mann attempts to explain what he has done, he adds:

> Tim,
>
> Attached are the calculations requested …
>
> p.s. I know I probably don't need to mention this, but just to ensure absolutely clarity on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking with me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things…

In other words, Mann lacks so much confidence in his own calculations that he refers to them as his "dirty laundry", that is to be hidden from scrutiny at all costs.

This is the basis of global climate policy?

August 19, 2003: **email 1061298033**

Tom Wigley to many, reiterating the naive arrogance that they should have absolute veto power over any publication in any journal:

> I have been closely involved in the Climate Research fiasco. I have had papers that I refereed (and soundly rejected), under de Freitas's editorship, appear later in the journal—without me seeing any response from the authors. As I have said before to others, his strategy is first to use mainly referees that are in the anti-greenhouse community, and second, if a paper is rejected, to ignore that review and seek another more "sympathic" reviewer. In the second case he can then (with enough reviews) claim that the honest review was an anomalous data point that can be ignored.

Again, Wigley's view is so myopic that any dissenting opinion must be "dishonest".

He then has the gall to suggest formalizing this closed-shop mentality:

> I agree that an ethics committee is needed and I would be happy to serve on such a committee. It would have to have endorsement by international societies, like the Royal Society of London for the Improvement of Natural Knowledge, the United States National Academy of Sciences, the Academy of Europe, plus the Royal Meteorological Society, the American Meteorological Society, the American Geophysical Union, etc.

Now that would really provide fertile ground for institutionalized bullying and ideological exclusion!

Wigley continues:

> Jim Titus mentioned to me that in the legal profession here people are disbarred for behavior like that of de Freitas (and even John Christy—although this is a more subtle case). We cannot do that of course, but we can alert the community of honest scientists to such behavior and formally discredit these people.

How ironic that he would invoke the workings of the legal profession—to which he and his associates will shortly be subject, for many years to come.

> The Danish Academy did something like this recently, but were not entirely successful.

Wigley joins the chorus of conspirators urging that the journal be black-balled:

> In the meantime, I urge people to dissociate themselves from Climate Research. The residual "editorial" (a word I use almost tongue in cheek) board is looking like a rogues' gallery of skeptics. Those remaining who are credible scientists should resign.

August 19, 2003: **email 1061300885**

Tom Crowley realizes that the gang needs more ammunition against the astrophysicists Soon and Baliunas, who seem to avoid the worst of their "peer group pressure" at in-bred scientific meetings:

> We need some data on Soon and Baliunas. One of my concerns is that they only publish in low-impact journals, and completely bypass the normal give-and-take of presentations at open scientific meetings (for example, I think I have probably heard 100 presentations overall from the people on this mailing list).

His implication is that, if you repeat something enough times—to a sympathetic audience—it somehow becomes more credible. From this hypothesis, he develops an entire line of attack on these interlopers:

> It is therefore very important to inquire, for the sake of our exchanges with reporters, legislators, etc, as to how often any of you may have heard Soon or Baliunas give a talk in an open meeting, where they could defend their analyses.

> Please respond to me as to whether you have heard either of them present something on their climate analyses (I think I heard Baliunas speak once on her astrophysics work, but that doesn't count).
>
> I will let you know the results of the poll so that we may all be on the same grounds with respect to the data, and reporting such information to press inquiries, legislators, etc.

Tom Wigley proposes a tactic that is pure disinformation:

> Might be interesting to see how frequently Soon and Baliunas, individually, are cited (as astronomers).
>
> Are they any good in their own fields? Perhaps we could start referring to them as "astrologers" (excusable as … "oops, just a typo").

Mike Mann recommends counting citations ("my count is bigger than yours")—a practice that is meaningless when the members of a small discipline repeatedly cite each other's papers:

> I checked this out prior to my United States Senate hearing. Their science citations in the climate literature are poor, as one would hope and expect. Interestingly, they both drop their second initials when publishing in the climate literature so that their names don't turn in up in the citation index if you do a search on their publications in the astronomy literature (which use the full initials)—apparently, they don't want their astronomy colleagues to be aware that they're moonlighting as supposed climatologists…

What a bizarre theory!

Mann is forced to acknowledge that his research into their publication record is disheartening:

> Their numbers are better in the astronomy literature, though Soon's numbers even here are mediocre.
>
> Baliunas had some well-cited publications more than a decade ago. This is her work on the use of sun-like stars as a model for solar variability, etc., which is well referenced in the astrophysics community. However, most of these appear to be her Ph.D. work, and appear to have been published with her Ph.D. adviser.

Which is, of course, absolutely standard practice—and indicates that her Ph.D. work was both original and useful to the astrophysics community. Mann continues:

> Not much evidence however that she has made any useful, independent contribution since then. There are some additional papers she's published on time series analysis of solar signals—looks like the kind of stuff you might expect to see from a graduate student first-year research project.…

This is the ultimate irony, given that Mann and his colleagues demonstrated their absolute ineptitude in this very area of mathematics—called "time series analysis"—that is needed to properly understand their temperature proxy data.

Mann now suggests that they "cherry-pick" their citation record to give the misleading impression of a low citation count, by ignoring their publications in astrophysics:

> In my opinion, it would be a mistake to evaluate these on their citations numbers in astronomy. We should focus on their numbers in the climate literature, which are the only ones relevant when discussing the issue of how their work on climate is received by their fellow scientists.

September 3, 2003: **email 1062592331**

Ed Cook writes to Keith Briffa, describing his experiences with Ray Bradley at a conference in Norway:

> After the meeting in Norway, … hearing Bradley's follow-up talk on how everybody but him has fucked up in reconstructing past Northern Hemisphere temperatures over the past 1000 years (this is a bit of an overstatement on my part, I must admit, but his air of papal infallibility is really quite nauseating at times), I have come up with an idea that I want you to be involved in.

Cook describes his idea of publishing a paper, with a large author list—possibly including Bradley, Phil Jones, and Mike Mann—but notes the problems with the idea:

> I am afraid that Mike Mann and Phil Jones are too personally invested in things now (i.e. the 2003Geophysical Research Letters paper that is probably the worst paper Phil has ever been involved in—Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work—this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that any reconstruction is better than any other.

This is testament to the parlous state of this field: that an established member of this group is reduced to suggesting that a paper be written in which past mistakes are no longer covered up.

Cook's suggestions end with comments that are only half-humorous:

> 7) Publish, retire, and don't leave a forwarding address
>
> Without trying to prejudice this work, but also because of what I almost think I know to be the case, the results of this study will show that we can probably say a fair bit about … temperature variability within a century (at least as far as we believe the temperature proxy estimates), but that we honestly know fuck-all about what the … variability was like on timescales greater than a century with any certainty (i.e. we know with certainty that we know fuck-all).

Cook's "calling a spade a spade" immediately endears him to my heart, and gives us confidence that he is expressing his genuine opinion. And while that opinion agrees completely with my own assessment of this field of science, it is astounding to hear it so explicitly (and colorfully), directly from the mouth of one intimately involved in this case:

temperature variations within a century can probably be reliably estimated, but we can conclude absolutely nothing about temperature variations over longer time-scales.

That, ladies and gentlemen, is the absolute crux of the global warming question: whether current temperature changes are "unprecedented" over historical time periods. Here we have, in no uncertain terms, a definitive statement that we have no idea if this is the case.

The jury is dismissed. Mankind has been found innocent of all charges.

October 2, 2003: **email 1065125462**

Robert Matthews, Science Correspondent for The Sunday Telegraph writes to Mike Mann:

> Dear Professor Mann
>
> I'm putting together a piece on global warming, and I'll be making reference to your paper in Geophysical Research Letters with Prof Jones on "Global surface temperatures over the past two millennia".
>
> When the paper came out, some critics argued that the paper actually showed that there have been three periods in the last 2000 years which were warmer than today (one just prior to AD 700, one just after, and one just prior to AD 1000). They also claimed that the paper could only conclude that current temperatures were warmer if one compared the proxy data with other data sets. (For an example of these arguments, see: link to paper)
>
> I'd be very interested to include your rebuttals to these arguments in the piece I'm doing. I must admit to being confused by why proxy data should be compared to instrumental data for the last part of the data-set. Shouldn't the comparison be a consistent one throughout?
>
> With many thanks for your patience with this
>
> Robert Matthews

A reasonable request, one would think. Here is Mike Mann's response:

> Dear Mr. Matthews,
>
> Unfortunately Phil Jones is travelling and will probably be unable to offer a separate reply. Since your comments involve work that is his as well, I have therefore taken the liberty of copying your inquiry and this reply to several of his British colleagues.
>
> The comparisons made in our paper are well explained therein, and your statements belie the clearly-stated qualifications in our conclusions with regard to separate analyses of the Northern Hemisphere, Southern Hemisphere, and globe.
>
> An objective reading of our manuscript would readily reveal that the comments you refer to are scurrilous. These comments have not been made by scientists in the peer-reviewed literature, but rather, on a website that, according to published accounts, is run by individuals sponsored by ExxonMobil Corporation, hardly an objective source of information.

Owing to pressures on my time, I will not be able to respond to any further inquiries from you. Given your extremely poor past record of reporting on climate change issues, however, I will leave you with some final words. Professional journalists I am used to dealing with do not rely upon un-peer-reviewed claims off internet sites for their sources of information. They rely instead on peer-reviewed scientific research, and mainstream, rather than fringe, scientific opinion.

Sincerely,

Michael E. Mann

I don't think the message gets much clearer than that—Matthews is black-balled on both sides of the Atlantic!

Note the common threads being weaved by Mann here. Firstly, he hides behind the pretence of "peer-reviewed literature", safe in the knowledge that he and his cronies have that avenue under their control. Secondly, he bullies Matthews by forwarding his email to as many collaborators as possible, so that they all know that they are not to speak to him under any circumstances. Thirdly, he condescendingly tells the journalist that the answers to his questions are self-evident from their paper, if only he had the intelligence to understand it.

These are nothing short of academic bullying tactics, pure and simple. Genuine scientists don't need to hide behind such intimidatory techniques.

October 13, 2003: **email 1066337021**

John Holdren of the John F. Kennedy School of Government and the Department of Earth and Planetary Sciences at Harvard University, United States, responds to a request from a John Shulz, editor of TCSDaily:

As you no doubt have anticipated, I do not put Mann and coworkers in the same category with Soon and Baliunas.

If you seriously want to know "Why not?", here are three ways one might arrive at what I regard as the right conclusion:

(1) For those with the background and patience to penetrate the scientific arguments, the conclusion that Mann and coworkers are right and that Soon and Baliunas are wrong follows from reading carefully the relevant Soon and Baliunas paper and the Mann and coworkers response to it:

(cites the papers)

This is the approach I took. Soon and Baliunas are demolished in this comparison.

In other words, Holdren is gratuitously arguing that it is "self-evident", if you are intelligent enough to read the papers. Next:

(2) Those lacking the background and/or patience to penetrate the two papers, and seriously wanting to know who is more likely to be right, have the option of asking somebody who does possess these characteristics—preferably somebody outside the handful of ideologically committed and/or oil-industry-linked professional climate-change skeptics—to evaluate the controversy for them.

> Better yet, one could poll a number of such people. They can easily be found by checking the web pages of earth sciences, atmospheric sciences, and environmental sciences departments at any number of major universities.

In other words, Holdren is implying that if you're too dumb or too lazy to read the papers, then simply ask the members of the "club"!

His last alternative:

> (3) The least satisfactory approach for those not qualified for (1) and lacking the time or initiative for (2), would be to learn what one can about the qualifications (including publications records) and reputations, in the field in question, of the authors on the two sides. Doing this would reveal that Soon and Baliunas are, essentially, amateurs in the interpretation of historical and paleoclimatological records of climate change, while the Mann and coworkers' authors include several of the most published and most distinguished people in the world in this field. Such an investigation would also reveal that Dr. Baliunas's reputation in this field suffered considerable damage a few years back, when she put her name on an incompetent critique of mainstream climate science that was never published anywhere respectable but was circulated by the tens of thousands, in a format mimicking that of a reprint from the Proceedings of the National Academy of Sciences, in pursuit of signatures on a petition claiming that the mainstream findings were wrong.

This text could have been taken straight out of a Central Intelligence Agency manual on "disinformation", character assassination, and dishonest techniques for discrediting opposition!

Holdren now displays his flair as an intelligence asset, by pointing out the obvious flaws in his last suggestion—and then covering them up with yet more insinuations of laziness and inability, and a bogus "probability" argument:

> Of course, the third approach is the least satisfactory because it can be dangerous to assume that the more distinguished people are always right. Occasionally, it turns out that the opposite is true. That is one of several good reasons that it pays to try to penetrate the arguments, if one can, or to poll others who have tried to do so. But in cases where one is not able or willing to do either of these things—and where one is able to discover that the imbalance of experience and reputation on the two sides of the issue is as lopsided as here—one ought at least to recognize that the odds strongly favor the proposition that the more experienced and reputable people are right. If one were a policy maker, to bet the public welfare on the long odds of the opposite being true would be foolhardy.

Are you convinced yet?

October 26, 2003: **email 1067194064**

Mike Mann receives secret information about the forthcoming McIntyre and McKitrick paper, which marks the start of the debunking of the "hockey stick":

> Two people have a forthcoming Energy and Environment paper that's being unveiled tomorrow (Monday) that—in the words of one Cato Institute / Marshall Institute / Competitive Enterprise Institute type—
>
> … will claim that Mann arbitrarily ignored paleo data within his own record and substituted other data for missing values that dramatically affected his results.
>
> When his exact analysis is rerun with all the data and with no data substitutions, two very large warming spikes will appear that are greater than the 20th century.
>
> Personally, I'd offer that this was known by most people who understand Mann's methodology: it can be quite sensitive to the input data in the early centuries.

In other words, most of Mann's colleagues were fully aware of the problems.

> Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin skin I am afraid he will react strongly, unless he has learned (as I hope he has) from the past….

Mike Mann passes this on to a large number of colleagues:

> Dear All,
>
> This has been passed along to me by someone whose identity will remain in confidence. Who knows what trickery has been pulled or selective use of data made. It's clear that Energy and Environment is being run by the baddies—only a shill for industry would have republished the original Soon and Baliunas paper as submitted to Climate Research without even editing it. Now apparently they're at it again…

A remarkable conclusion, given that he hasn't read the paper yet!

He continues:

> My suggested response is:
>
> 1) to dismiss this as a stunt, appearing in a so-called "journal" which is already known to have defied standard practices of peer-review. It is clear, for example, that nobody we know has been asked to "review" this so-called paper;

Again, Mann displays unbelievable arrogance in assuming that each and every paper submitted for publication should automatically be passed to one of his gang, so that it can be vetoed.

He continues:

> Who knows what sleight of hand the authors of this thing have pulled. Of course, the usual suspects are going to try to peddle this crap. The important thing is to deny that this has any intellectual credibility whatsoever and, if contacted by any media, to dismiss this for the stunt that it is.
>
> Thanks for your help.

How on Earth can Mann tell others to discredit this paper, before anyone has actually read it? Simply because it disagrees with him?

October 30, 2003: **email 1067532918**

Ray Bradley writes to Tim Osborn, Phil Jones, Keith Briffa, Mike Mann, and Malcolm Hughes, offering a novel definition of the term "independent":

> Tim, Phil, Keith:
>
> I suggest a way out of this mess. Because of the complexity of the arguments involved, to an uninformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at the Climatic Research Unit could make a statement as to whether the McIntyre and McKitrick effort is truly an "audit", and whether they did it right, I think that would go a long way to defusing the issue.

This one statement alone is sufficient to see through the repeated bogus claims of "independent" verification of results by other groups. "Independence", for these cowboys, means asking a group from another institution (preferably an overseas one) to rubber-stamp their findings.

He continues:

> If you are willing, a quick and forceful statement from The Distinguished Climatic Research Unit Boys would help quash further arguments, although here, at least, it is already quite out of control…

Indeed.

November 12, 2003: **email 1068652882**

Tim Osborn writes to Keith Briffa and Phil Jones, discussing a request by Steve McIntyre (not included, but we can infer the nature of the request shortly):

> You will have seen Stephen McIntyre's request to us. We need to talk about it, though my initial feeling is that we should turn it down (with carefully worded/explained reasons) as another interim stage and prefer to make our input at the peer-review stage.

Osborn then forwards an email that McIntyre wrote to Mike Mann and Osborn, asking for data that is in dispute, and asks that erroneous statements be publicly withdrawn:

> In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out preliminary and incorrect early responses—the waters are really muddied now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early, or now deleted, is really confusing things!

Osborn is describing a flurry of activity by Mann and colleagues, whereby data files were hastily cobbled together using Microsoft Excel (which should not have been necessary: the data should have been available for scrutiny or distribution at any time), posted to the download site, then quickly withdrawn as elementary errors were evident.

This is clear evidence that it was only the increasingly vocal demands of McIntyre and others that caused this gang of cowboys to start cleaning up their data, into what was only a superficial semblance of acceptability.

Osborn then expresses relief that they are now "off the hook" regarding McIntyre's earlier request:

> Anyway, because McIntyre has now asked Mann directly for his data and programs, his request that we send McIntyre's request to Mann has been dropped (I would have said "no" anyway).

Is it any wonder that getting the data and computer programs from these "scientists" was more difficult than pulling teeth? Perhaps dictionary pages for the word "obstruction" should be redirected here…

January 16, 2004: **email 1074277559**

The journal Climatic Change requests from Phil Jones that Mike Mann's data and computer programs be made available, to check that the calculations are reproducible by other scientists. Jones writes to a large number of climate scientists, hosing down the need for such action:

> 1. The papers that McIntyre and McKitrick refer to came out in Nature in 1998 and to a lesser extent in Geophysical Research Letters in 1999. These reviewers did not request the data (all the temperature proxy series) or the computer programs. So, acceding to the request for this to do the review is setting a very dangerous precedent. Mike has made all the data … available and this is all anyone should need. Making computer programs available is something else.

Jones is arguing for despicable double standards: he and his colleagues continue to cite these papers, by the dozen, as the "gold standard" of the global warming debate; but when asked to substantiate the claims made in them, he effectively argues that it is "past history"—and if they got away without providing the programs to the peer reviewers in 1998 or 1999, then they should be scot-free forever!

He continues:

> 2. The computer programs are basically irrelevant in this whole issue. In the Geophysical Research Letters paper (in 2003 by Mann and Jones), we simply average all the data sets we use together. The result is pretty much the same as for Mann, Bradley, and Hughes in 1998 in Nature, and for Mann, Bradley, and Hughes in 1999 in Geophysical Research Letters.

More misdirection. "Averaging the data sets together" is not "simple"—or, rather, if they did do it "simply", i.e., naively, then not only it is statistically invalid and completely meaningless, but the computer program should be so simple that there should be no reason to not release it. Even Jones is forced to use the qualifier "pretty much".

Jones's next misdirection:

> 3. As many of you know, I calculate temperature data each month. Groups at the National Climatic Data Center and the National Aeronautics and Space

> Administration Goddard Institute for Space Studies do this as well. We don't exchange computer programs—we do, occasionally, though, exchange the data. The computer programs here are trivial as they are in the paleoclimatology work.

Again, if the computer programs were trivial, then surely they could be distributed without any qualms at all.

Note that Jones is here admitting that the various groups do not even check each other's programs, let alone make them available for independent scrutiny. In other words, they have not been checked outside their own lab at all. He furthermore admits that the data are only checked "occasionally".

Jones now widens the crack of self-contradiction:

> Mann, Bradley, and Hughes get geographical patterns, but the bottom line (the 1000-year series of global temperatures) is almost the same if you simply average.

Ah-ha! "Almost" the same. And it is the panoply of subtleties that come into that "almost" that necessitates careful checking and validation.

He continues to explain why none of this is in the least bit "trivial":

> The geographical patterns give more, though, when it comes to trying to understand what has caused the changes — e.g. by comparison with models. McIntyre and McKitrick are only interested in the Northern Hemisphere and Global 1000-year data sets — in fact only in the Mann, Bradley, and Hughes work from 1400.

Perhaps realizing that he is arguing against his own thesis, Jones now tries to argue that Mann is being victimized:

> 4. What has always intrigued me in this whole debate, is why the skeptics (for want of a better term) always pick on Mike. There are several other data sets that I've produced, as has Keith Briffa ... and Tom Crowley. Jan Esper's work has produced a slightly different data set but we don't get bombarded by McIntyre and McKitrick. Mike's paper wasn't the first. It was in Nature and is well-used by the Intergovernmental Panel on Climate Change (IPCC). I suspect the skeptics wish to concentrate their effort onto one person as they did with Ben Santer after the second IPCC report.

Apart from answering his own question—Mann's "hockey stock" work is held up by all of them, including in their role as the voice of the IPCC, as the gold standard—Jones's argument is ridiculous. Mann's data and programs should not be scrutinized, simply because other people's data hasn't yet been scrutinized? That sounds like a good Catch-22 argument for preventing the process from starting at all!

Jones now displays the ultimate in hypocrisy:

> 5. ... I found out later that the (skeptic) authors of a paper were in contact with the reviewers up to a week before the article appeared. So there is peer review and peer review!! Here the peer review was done by like-minded colleagues.

As the Climategate emails show, Mann, Jones, and their colleagues were not only in contact with their reviewers, but regularly chose them—or applied detective work to determine who they were—as a matter of course! It is unbelievable that they seem unable to recognize that they themselves do precisely what they accuse others of doing—and they openly discuss it!

Now, in contrast to the above carefully-constructed defense, consider the following email: Jones frantically leaks the journal's request to Mike Mann:

> Subject: Climatic Change needs your advice—YOUR EYES ONLY !!!!!
>
> Mike,
>
> This is for YOUR EYES ONLY. Delete after reading—please! I'm trying to redress the balance. One reply from Christian Pfister said you should make all available!! Pot calling the kettle black—Christian doesn't make his methods available. … I told Steve separately, and told him to get more advice from a few others, as well as Kluwer (publishers), and the legal department.
>
> PLEASE DELETE—just for you, not even for Ray Bradley and Malcolm Hughes.

Jones's blind panic—in private to Mann—speaks volumes. He is so scared of the ramifications that he even asks that Mann destroy the email immediately.

Are these the actions of scientists with nothing to hide?

January 29, 2004: **email 1075403821**

Phil Jones forwards to Mike Mann an email advising the sudden death of skeptic John Daly:

> In an odd way this is cheering news! One other thing about the Climatic Change paper — just found another email — is that McKittrick says it is standard practice in Econometrics journals to provide all the data and computer programs!! According to legal advice, Intellectual Property Rights overrides this.

Ignore Jones's insensitive comments regarding an opponent's death, if you can. What is remarkable here is that Jones apparently finds completely bizarre and foreign the idea that the data and methods used to arrive at a scientific conclusion should be made available for independent scrutiny! This is astounding: these requirements are fundamental to the entire scientific method, through its demands of reproducibility: any scientist, anywhere in the world, must in principle be able to reproduce and verify a scientific result, before it is even considered to be a result at all.

Of course, in the context of the climate debate, Jones's arrogance is far more damning: these results, central to their call on world leaders to enact treaties and legislation that would have truly astounding ramifications for the planet, should have been audited, scrutinized, validated, and verified with greater thoroughness than possibly any other results of modern science. To have Jones and Mann argue that the data and programs central to these recommendations are "private property"—protected by patent and copyright laws—is not just obnoxious: it is criminal.

February 2, 2004: **email 1075750656**

Keith Briffa makes an astounding comment to Rashit Hantemirov, regarding a request made of Hantemirov:

> Dear Rashit
>
> Thanks for this—these people ask many questions as they try constantly to attack the global warming proponents. I answer sometimes, but it usually means they come back with many more questions. All part of science, I suppose.

Indeed!

It is remarkable, firstly, that Briffa describes himself and his colleagues as "global warming proponents", rather than "researchers", "investigators", or even just "scientists". Surely they are not meant to be "proponents" of a predetermined view? A Freudian slip on Briffa's part, perhaps?

Secondly, Briffa's bewilderment that anyone would question them over their work—and that an answer would not simply provide a brush-off, but may well stimulate follow-up questions—is astounding.

February 4, 2004: **email 1076083097**

A large number of collaborators are discussing ways to avoid providing Steve McIntyre with enough of the computer programs to actually check their results. Linda Mearns, Senior Scientist at the Institute for the Study of Society and Environment at the National Center for Atmospheric Research, writes:

> My point about the computer programs is still that "providing the programs" can be interpreted a lot of ways. I have thought about this, and imagined if in one of my larger and more complex projects, I was asked to provide all the programs. I could do that just by sending the pieces with a summary file explaining what each piece was used for. It still theoretically allows someone to see how the programming was done. And I do think that is a far sight easier than providing stuff that can be run, etc. I am suggesting that one could do the minimum. Then the point is, one isn't faced with garish headlines about "refusal to provide programs". I think it is harder to come up with a garish headline about "refusal to provide completely documented programs with appropriate instructions files and hand-holding for running it".

Mearns' overwhelming concern with newspaper headlines, rather than scientific corroboration and validation, speaks volumes.

Mearns' argument is effectively this: if we are forced to provide the computer programs, then let's break them up into the smallest possible pieces, so that McIntyre can see roughly what we have done, but would have an almost impossible task putting the pieces back together again so that it could be used—sort of a "Humpty Dumpty" version of transparency and full disclosure.

Phil Jones realizes that this won't fool many: if they had done the science properly, then the computer programs and supporting documentation would be readily available for anyone to use, without any further work:

> So now it seems that we're separating "providing the programs" from "running the programs". I can't see the purpose of one without the other. Even if Mike Mann complies, I suspect there will need to be several sessions of interaction to explain how to run the programs, which neither side will be very keen on.

Jones is savvy enough to understand that providing un-runnable programs will lead to an immediate request or demand for assistance in actually getting them to run.

He now admits that, even with possession of the programs and the data, a lot of "fiddling" is needed to get to their claimed results:

> As I said before, I know that running the programs will involve lots of combinations (for different time periods with different temperature proxies).

He further realizes that validating their programs would require validating their mathematical "number-crunching" programs—often shared between different programs, and hence called "library routines":

> Also I would expect, knowing the nature of the mathematical approach that we use, that there will be library routines. We don't want McIntyre (and McKitrick) to come out and say that he can't get it to work after a few days.

At least Jones understands the realities of the situation—although it is surprising that he doesn't know for certain whether they use library routines or not. One must wonder about the environment which the more junior scientists are accustomed to, for them to be seriously considering withholding parts of the programs to prevent them from being usable.

Jones continues:

> So, it is far from simple. I'm still against the computer programs being given out. Mike has made the data available. That is all they should need. The method of calculations is detailed in the original paper … and also in several other papers Mike has written.

In other words, the skeptics have a description of what was done which should be enough.

Then this bombshell:

> As an aside, Mike Mann is now using a different method from the paper of Mann, Bradley, and Hughes of 1998.

So even if McIntyre and colleagues follow the method described in the 1998 paper, they still won't obtain agreement with what Mann is now doing!

Could there be any clearer argument for providing the exact computer programs and methodology used for each and every published paper? Jones apparently can't fathom the ridiculousness of his own words.

He continues:

> It might appear that they want the programs to check whether their version works properly. If this is the case, then there are issues of Intellectual Property Rights. So, if they get the programs, how do we stop them using it for anything other than this review?

God forbid that any other scientists should be given assistance in researching this issue of critical importance to humanity! Jones's treatment of their data and research as "private property", for them to exploit and profit from—to the exclusion of all other scientists—is obnoxious, particularly as it is paid for by taxpayers!

February 9, 2004: **email 1076336623**

Steve McIntyre has been trying to get raw data, and writes to Australian Antarctic scientist Tas van Ommen:

> Dear Dr van Ommen,
>
> Some time ago I inquired as to the availability of the … data set which was used in the paper of Mann and Jones in 2003. Is this the same data as was used in Jones and coworkers in 1998 (in the journal The Holocene)? Do you plan to make available a public archive of this data? Otherwise, I would appreciate an email copy of the data.
>
> Thanks for your consideration.
>
> Stephen McIntyre.

Van Ommen forwards the ensuing email exchange to Phil Jones:

> What you will find below is … an email interchange between Steve McIntyre and myself. He has been asking for Antarctic data for a while (since your Geophysical Research Letters paper came out) and to my chagrin; I have put him off once already, for reasons I spell out below. …
>
> Anyway, I am aware of McIntyre's controversial history and am trying to handle things in a non-inflammatory way. He seems not to be troubling me over my own delay, but has asked for data that was used in your Holocene paper of 1998. For this, I have referred him to you. I expect he wants to replicate your calculations, and so he should use the identical data set, and I give you permission to pass on whatever it was I gave you for that work—with the caveat that it is representative of where the Antarctic proxy record was in 1997, not 2004. I leave it to you to decide how to deal with this—you may prefer to ignore the issue, and I would understand.

Van Ommen clearly understands that it is crucial for McIntyre to be given the identical data set in order to replicate Jones's calculations—but then goes on to condone what he guesses will be Jones's likely response: to ignore the issue completely.

Phil Jones replies, copying in Mike Mann:

> Thanks for the email. Steve McIntyre hasn't contacted me directly about the Antarctic data (yet), nor about any of the data used in the 1998 Holocene paper or the 2003 Geophysical Research Letters one with Mike. I suspect (hope) that he

won't. I had some emails with him a few years ago when he wanted to get all the station temperature data we use here in Climatic Research Unit. At that time, I hid behind the fact that some of the data had been received from individuals and not directly from Met(eorological) Services through the Global Telecommunications Service (GTS) or through the Global Climate Observing System.

We here start to learn about the tricks that Jones and colleagues have used to thwart attempts to get access to the data that their published claims are based on. In this case, Jones is trying to argue that data provided by individuals does not need to be provided for independent scrutiny—yet the mathematical results obtained from that very data can be published in leading journals, which then makes it eligible to be used support their statements in the Intergovernmental Panel on Climate Change Reports!

He continues:

> Emails have also been sent to some other paleoclimatology people asking for data sets used in 1998 or 2003. Keith Briffa here got a request, for example. Here, they have also been in contact with some of Keith's Russian contacts. All seem to relate to trying to get data that we've used. In the Russian case, issues relate to the Russian (Rashit Hantemirov) having a paper out with the same data that Keith used ….The data are different for two reasons. One reason is that Keith used (a mathematical method on the data);and, secondly, Rashit has added some data since Keith got the data a couple of years ago.

Jones is here giving yet more reasons why the original data should be be made available. So what will he do?

> I'll just sit tight here and do nothing. Mike will likely do the same, but we'll expect another publication in the nearish future.

So not only will they ignore all requests for the data—and hide behind dubious loopholes to do so—but they are moreover planning to continue publishing papers based on all this "private" data, adjusted by their own private mathematical methods!

February 9, 2004: **email 1076359809**

Steve McIntyre follows the trail from Tas van Ommen to Phil Jones:

> Dear Phil,
>
> Tas van Ommen has refered me to you for the version of his data set that you used in the paper of Jonesand coworkers in The Holocene in 1998, and I would appreciate a copy. I would also appreciate a copy of the Lenca data used in this study. Regards, Steve McIntyre

Phil Jones forwards this to Mike Mann:

> For your information. I sent him the two data sets—the as-received versions. I wonder what he's up to? Why these two data sets? We used a lot more in the 1998 paper. He didn't want the Alerce data. He must already have the Tassy series from Ed. I know that Ed has a more recent series than we used in 1998. He got this for the 2003 work.

Why is Jones so concerned at what McIntyre is "up to"? Honest scientists welcome every chance for independent researchers to check and (hopefully) confirm their results: it gives them extra credibility. Instead, Jones seems to be worrying about which "skeleton in the closet" McIntyre may be onto.

Mike Mann replies:

> Personally, I wouldn't send him anything. I have no idea what he's up to, but you can be sure it falls into the "no good" category.

Mann is acting even guiltier than Jones. He seems to revel in the fact that McIntyre will still be missing some of the data:

> There are a few data sets from our 2003 paper that he won't have—these include the latest data from Jacoby and D'Arrigo, which I scanned in from their publication (they haven't made it publicly available), and the extended western North American series, which they wouldn't be able to reproduce without following exactly the procedure described in our 1999 Geophysical Research Letters paper to remove the estimated non-climatic component.

In other words, unless McIntyre and colleagues were able to follow the procedure described in Mann's and Jones's 1999 paper—without the aid of the computer programs used to apply those methods, which they are refusing to supply—then they will be unable to even get hold of the fundamental data that Mann's and Jones's research is based on! No wonder Mann is confident that their secrets are safe.

He continues, admonishing Jones for his weakness:

> I would not give them anything. I would not respond or even acknowledge receipt of their emails. There is no reason to give them any data, in my opinion, and I think we do so at our own peril!

Peril? That is not a word that an innocent man would use.

Jones is now forced to defend his act of sending McIntyre two data sets:

> These were two simple data sets to provide. Also, Tas told him that I had one of them. I guess that these are the ones that aren't available on web sites.
>
> Anyway, it is done now. If he starts asking for them in dribs and drabs, I'll baulk at that.
>
> Ben waded in with very positive comments regarding the Climatic Change issue. Steve McIntyre's going to find it very hard to ask you to send the computer programs. Those … on the Climatic Change board that say that you should send the programs have little idea what is involved. Most are on the social science side.

One set of "soft" scientists belittling another set of "soft" scientists? Are there any real scientists doing climate science?

Yet again, we find that Jones sees the issue of accountability as a series of battles, rather than a looming war.

February 26, 2004: **email 1077829152**

Phil Jones to Mike Mann:

> Can I ask you something in confidence—don't email around, especially not to Keith and Tim here. Have you reviewed any papers recently for Science that say that the paper by Mann, Bradley, and Hughes in 1998 and the paper by Mann and Jones in 2003 have underestimated variability in the thousand-year record—from models or from some slowly varying temperature proxy data? Just a yes or no will do. Tim is reviewing them—I want to make sure he takes my comments on board, but he wants to be squeaky clean with discussing them with others. So forget this email when you reply.

An interesting way to manipulate the peer-review process, and a novel definition of "squeaky clean"!

May 7, 2004: **email 1083962601**

Phil Jones to Tas van Ommen and Caspar Ammann:

> Many of us in the paleoclimatology field get requests from skeptics (mainly a guy called Steve McIntyre in Canada) asking us for data. Mike Mann and I are not sending anything, partly because we don't have some of the data he wants, also partly as we've got the data through contacts like you, but mostly because he'll distort and misuse them.

Again, Jones writes with crystal clarity on the big issues. The three reasons for hiding the data: the skeptics will check their work; some of the data was destroyed or lost; and the data is "private property" in any case!

July 8, 2004: **email 1089318616**

Phil Jones to Mike Mann:

> Subject: HIGHLY CONFIDENTIAL
>
> For your interest, there is a report coming out soon, which shows that Eugenia Kalnay and Ming Cai are wrong. It isn't that strongly worded, as the first author is a personal friend of Eugenia. The result is rather hidden in the middle of the report.

He sends a follow-up email:

> For your information only—don't pass on. … As I said it is worded carefully due to Adrian knowing Eugenia for years. He knows they're wrong, but he succumbed to her almost pleading with him to tone it down as it might affect her proposals in the future! I didn't say any of this, so be careful how you use it—if at all.

This pair of emails demonstrates most clearly that one was either "in the club" or not, as far as these conspirators were concerned. The paper of Kalnay and Cai was skeptical of man-made global warming. Here, Phil Jones is telling Mike Mann that the paper has been shown to be wrong—in a written report, no less. Yet Jones condones the fact that the criticism is being buried: he is accepting that "peer review" has been distorted into whatever the reviewer wants it to be, rather than its intended mechanism: of ensuring that published papers are correct.

Contrast this to their actions on hearing of skeptical papers being published by those not "in the club": they organize letters of protest to journals, and to the White House, even before they have read the papers at all! Indeed, Jones exemplifies their approach, just one paragraph on:

> The other paper by McKitrick and Michaels is just garbage—as you knew. De Freitas is the Editor again. Pielke is also losing all credibility as well by replying to the mad Finn as well—frequently, as I see it. I can't see either of these papers being in the next Intergovernmental Panel on Climate Change Report. Kevin and I will keep them out somehow—even if we have to redefine what the "peer-review literature" is!

This pervasive inculcation of double standards—not just between the proponents and skeptics (their terms) of global warming, but between different "classes" of skeptics, no less—destroys the very fabric of science.

August 6, 2004: **email 1091798809**

Phil Jones replies to a favorable comment by Australian climate scientist, Janice Lough:

> Janice,
>
> Most of the data for most of the graphs have just appeared on the Climatic Research Unit web site. Go to "data", then to "paleoclimate". We did this to stop getting hassled by the skeptics for the data sets. Mike Mann refuses to talk to these people and I can understand why. They are just trying to find if we've done anything wrong.

Strange: I thought that that's what science is all about!

He continues:

> I sent one of them loads of data sets and he barely said a thank you.

Jones's imperious comment shows that he considers that he is doing the skeptics a huge favor by providing the data that is central to his claims. In reality, the "onus of proof" is on him and his colleagues.

August 10, 2004: **email 1092167224**

Mike Mann writes to Phil Jones, Gabi Hegerl, and Tom Crowley:

> Dear Phil and Gabi,
>
> I've attached a cleaned-up and documented version of the computer programs that I wrote for doing the Mann and Jones (2003) calculations. I did this knowing that Phil and I are likely to have to respond to more crap criticisms from the idiots in the near future, so it is best to clean up the programs and provide them to some of my close colleagues in case they want to test it, etc.

> Please feel free to use these programs for your own internal purposes, but don't pass them along where they may get into the hands of the wrong people.

So here we are, in late 2004, before Mike Mann finally feels the need to bring his computer programs up to the standard that would be required of any high-school student—and not because of any feelings of guilt about their parlous state, but simply because "the heat was on" from the skeptics, with it becoming increasingly likely that he would be forced to provide these programs for independent scrutiny in the near future.

To anyone who has spent their career performing numerical computations, Mann's email is simply astounding. Firstly, by "cleaning up" his programs, he is not, in fact, providing the programs that generated the results that his publications were based on; he is providing an altered version. It would be like the police prosecutor "cleaning up" the evidence before showing it to the jury.

Secondly, Mann's admission that his programs were previously undocumented—an admission that he will repeat shortly—destroys any residual credibility that any of his scientific work may have otherwise retained—period. Masses of formulas, without any explanation of what they are doing or why they are being applied, are worse than useless: they show Mann to have the scientific maturity of a teenager (and that is an insult to many conscientious teenagers).

Thirdly, it is unfathomable that it is only at this late date that Mann even suggests that his "trusted colleagues" check that his programs produce the results he claims—let alone that what has been programmed is even mathematically or statistically correct. In other words, none of the results of any of these "scientists" are ever checked by anyone prior to publication. That is simply stupefying.

Fourthly, Mann again damns himself by expressing his fear that his programs—even after being cleaned up and documented—will get into the hands of the "wrong people".

One might wonder whether all of this astounding incompetence might cast doubts on Mann's results. But wait! There is no need to speculate: Mann himself provides the first answer, in the very same paragraph, with what must make him eligible for an honorary role in Monty Python:

> In the process of trying to clean the programs up, I realized I had something a bit odd, not necessarily wrong, but it makes a small difference. … It looks like I had two similarly-named data sets floating around in the programs, and used perhaps the less preferable one … .
>
> This may explain part of what perplexed Gabi when she was comparing my results with the real temperatures. I've attached the version of the analysis where the correct data is used instead, as well as the computer programs, which you're welcome to try to use yourself and play around with. Basically, this increases everything everywhere by the factor 1.29. Perhaps this is more in line with what Gabi was estimating (Gabi?).
>
> Anyway, it doesn't make a major difference, but you might want to take this into account in any further use of the Mann and Jones data…

Yes, the world will take this into account: Don't trust the Mann and Jones data at all.

Mann's lack of honesty is manifest in his own words: he himself discovers, in his own bird's nest of "spaghetti programming", that he made a careless error; but rather than declare it as such—to even his closest colleagues—he whitewashes it as "not necessarily wrong, but it makes a small difference".

A "scientist" that can never admit that he is wrong? I think we all know where his "science" belongs.

Hilariously, Mann then suggests that his comedy of errors might provide a good opportunity for publishing another of his illustrious publications:

> Phil: is this worth a follow-up paper to Geophysical Research Letters, with a link to the computer programs?

Roll credits.

September 28, 2004: **email 1096382684**

Andy Revkin, Environment Reporter for The New York Times, writes to Tim Osborn:

> Again, the take-away message is that Mann's method can only work if past variability is the same as the variability during the period used to calibrate your method.
>
> So it could be correct, but it could be very wrong as well.
>
> By the way, von Storch doesn't agree with Osborn and Briffa on the idea that higher past variability would mean there'd likely be high future variability as well (bigger response to greenhouse gases). He simply says it's time to toss the "hockey-stick graph" and start again; he doesn't take it further than that.
>
> Is that right?

So The New York Times should have headlined, "Climate Change Scientists Could Be Very Wrong," and sub-headlined, "Time to Toss the Hockey-Stick Graph."

No?

Or is that not Fit to Print?

December 10, 2004: **email 1102687002**

Gavin Schmidt, of the Goddard Institute for Space Studies of the United States National Aeronautics and Space Administration (NASA GISS), writes to many:

> Colleagues,
>
> No doubt some of you share our frustration with the current state of media reporting on the climate change issue. Far too often we see agenda-driven "commentary" on the Internet and in the opinion columns of newspapers crowding out careful analysis. Many of us work hard on educating the public and journalists through lectures, interviews and letters to the editor, but this is often a thankless task.

> In order to be a little bit more pro-active, a group of us (see below) have recently got together to build a new "climate blog" website, RealClimate.org, which will be launched over the next few days at:
>
> http://www.realclimate.org
>
> The idea is that we working climate scientists should have a place where we can mount a rapid response to supposedly "bombshell" papers that are doing the rounds, and give more context to climate-related stories or events.
>
> …
>
> Gavin Schmidt
>
> on behalf of the RealClimate.org team:
> - Gavin Schmidt
> - Mike Mann
> - Eric Steig
> - William Connolley
> - Stefan Rahmstorf
> - Ray Bradley
> - Amy Clement
> - Rasmus Benestad
> - William Connolley
> - Caspar Ammann

The propaganda war goes digital!

Somewhat hilariously, the "RealClimate.org team" in the signature block contains the name William Connolley twice. Did this infamous "Wikipedia terrorist" hack into their own announcement email?

January 6, 2005: **email 1105019698**

David Parker, of the United Kingdom Met(eorological) Office, writes to Neil Plummer, Senior Climatologist at the National Climate Centre of the Bureau of Meteorology, Melbourne, Australia:

> There is a preference in the atmospheric observations chapter of the Intergovernmental Panel on Climate Change Fourth Assessment Report to stay with the 1961–1990 baseline. This is partly because a change of baseline confuses users, e.g. anomalies will seem less positive than before if we change to a newer baseline, so the impression of global warming will be muted.

Can't give that impression, eh?

January 20, 2005: **email 1106322460**

Steve Mackwell, Editor in Chief of Geophysical Research Letters, writes to Mike Mann, who evidently complained because he was not able to "look over" a manuscript, critical of his own work, prior to its publication:

Dear Prof. Mann

In your recent email to Chris Reason, you laid out your concerns that I presume were the reason for your phone call to me last week. I have reviewed the manuscript by McIntyre, as well as the reviews. The editor in this case was Prof. James Saiers. He did note initially that the manuscript did challenge published work, and so felt the need for an extensive and thorough review. For that reason, he requested reviews from three knowledgeable scientists. All three reviews recommended publication.

While I do agree that this manuscript does challenge (somewhat aggressively) some of your past work, I do not feel that it takes a particularly harsh tone. On the other hand, I can understand your reaction. As this manuscript was not written as a Comment, but rather as a full-up scientific manuscript, you would not in general be asked to look it over. And I am satisfied by the credentials of the reviewers. Thus, I do not feel that we have sufficient reason to interfere in the timely publication of this work.

Mike Mann forwards this response to a number of his colleagues:

Dear All,

Just a heads-up (warning). Apparently, the contrarians now have an "in" with Geophysical Research Letters. This guy Saiers has a prior connection with the University of Virginia Department of Environmental Sciences that causes me some unease.

I think we now know how the various Douglass and coworkers papers with Michaels and Singer, the Soon and coworkers paper, and now this one have gotten published in Geophysical Research Letters.

Tom Wigley writes:

This is truly awful. Geophysical Research Letters has gone downhill rapidly in recent years. I think the decline began before Saiers. I have had some unhelpful dealings with him recently with regard to a paper Sarah Raper and I have on glaciers—it was well received by the referees, and so is in the publication pipeline. However, I got the impression that Saiers was trying to keep it from being published.

Proving bad behavior here is very difficult. If you think that Saiers is in the greenhouse skeptics camp, then, if we can find documentary evidence of this, we could go through official American Geophysical Union channels to get him ousted. Even this would be difficult.

Mike Mann responds in terms more reminiscent of a political election than of scientific journals:

Yeah, basically this is just a heads-up to people that something might be up here. What a shame that would be. It's one thing to lose Climate Research. We can't afford to lose Geophysical Research Letters. I think it would be useful if people

> begin to record their experiences with both Saiers and potentially Mackwell (I don't know him—he would seem to be complicit with what is going on here).
>
> If there is a clear body of evidence that something is amiss, it could be taken through the proper channels. I don't think that the entire American Geophysical Union hierarchy has yet been compromised!

Malcolm Hughes suggests using the Editor-in-Chief's words as a loophole:

> Does it not … follow that if you were to challenge their "work" in "full-up scientific manuscript", but not as a "Comment", then it, too, should be reviewed without reference to McIntyre and McKitrick?

But Mann is adamant that Geophysical Research Letters is to be black-balled:

> I'm not sure that Geophysical Research Letters can be seen as an honest broker in these debates any more, and it is probably best to do an "end run" around Geophysical Research Letters now where possible. They have published far too many deeply flawed contrarian papers in the past year or so. There is no possible excuse for them publishing all three Douglass papers and the Soon and coworkers paper. These were all pure crap.
>
> There appears to be a more fundamental problem with Geophysical Research Letters now, unfortunately…

Four "contrarian" peer-reviewed papers? Intolerable!

January 21, 2005: **email 1106338806**

Tom Wigley writes to Phil Jones, primarily about a Review Panel by the VTT Technical Research Centre in Finland:

> Tom Karl told me you will be on the VTT Review Panel. This is very good news.

However, he brings up a new concern:

> I got a brochure on the Freedom Of Information Act from the University of East Anglia. Does this mean that, if someone asks for a computer program we have to give it out?? Can you check this for me (and Sarah Raper)?

Phil Jones is confident that it won't be a problem:

> On the Freedom Of Information Act, there is a little leaflet we have all been sent. It doesn't really clarify what we might have to do regarding programs or data. Like all things in Britain, we will only find out when the first person or organization asks. I wouldn't tell anybody about the Freedom Of Information Act in Britain. I don't think the University of East Anglia really knows what's involved.

However, he also starts the process of finding loopholes in the legislation:

> As you're no longer an employee, I would use this argument if anything comes along.

Tom Wigley replies:

> Thanks for the quick reply.

> The leaflet appeared so general, but it was prepared by the University of East Anglia so they may have simplified things. From their wording, computer programs would be covered by the Freedom Of Information Act. My concern was if Sarah is/was still employed by the University of East Anglia. I guess she could claim that she had only written one tenth of the programs, and therefore only release every tenth line of the programs.

Another interesting attempt at finding a loophole, albeit unlikely to succeed.

Wigley returns to the original topic:

> Let me fill you in a bit (confidentially) (on the VTT Review Panel). You probably know the panel members.... As token skeptic there is Dick Lindzen—but at least he is a smart guy and he does listen.

Glad to know that the "token skeptic" has been appointed! I guess we don't need to wonder what conclusion that Panel will come to!

Phil Jones replies, refining the loophole even further:

> As for the Freedom Of Information Act, Sarah isn't technically employed by the University of East Anglia and she will likely be paid by Manchester Metropolitan University.

Not that she wouldn't be covered by the Act: merely that she would be paid by a different University!

He continues:

> I wouldn't worry about the computer programs. If the Freedom Of Information Act does ever get used by anyone, there is also Intellectual Property Rights to consider as well. Data is covered by all the agreements we sign with people, so I will be hiding behind them. I'll be passing any requests onto the person at the University of East Anglia who has been given a post to deal with them.

So, yet again, Phil "Hide Me" Jones has found another cubby hole: this time behind the various dubious agreements that they have signed with individuals and institutions, giving them legal assurance that the data would remain private, despite the publications and policy recommendations derived from it being most definitely public.

So that's three potential loopholes: no longer employed by us; intellectual property rights; and the data is not ours to give.

February 2, 2005: **email 1107454306**

Phil Jones writes to Mike Mann:

> Just sent loads of ... data to Scott Rutherford. Make sure he documents everything better this time!

So it isn't until 2005 that they decide it is time to document what they are doing?

> And don't leave stuff lying around on anonymous download sites—you never know who is trawling them. McIntyre and McKitrick have been after the

> Climatic Research Unit … data for years. If they ever hear there is a Freedom of Information Act now in the United Kingdom, I think I'll delete the file rather than send it to anyone.

Wonderful.

> Does your similar Act in the United States force you to respond to enquiries within 20 days?—our does! The United Kingdom works on precedents, so the first request will test it.

How uncivilized: actually being forced to respond to enquiries!

> We also have a Data Protection Act, which I will hide behind.

Ah, we were wondering how long it would take him to find a loophole to hide behind.

> Tom Wigley has sent me a worried email when he heard about it—he thought people could ask him for his computer programs. He has retired officially from the University of East Anglia so he can hide behind that.

Every civilized man should have something to hide behind.

> Intellectual Property Rights should be relevant here, but I can see me getting into an argument with someone at the University of East Anglia who'll say we must adhere to the Freedom of Information Act!

God forbid: someone there will insist on them abiding by the law?

Mike Mann responds:

> Yes, we've learned our lesson about anonymous download sites. We're going to be very careful in the future what gets put there. Scott really screwed up big time when he established that directory so that Tim could access the data.

Unforgiveable, giving independent scientists access to the data!

> Yeah, there is a Freedom Of Information Act in the United States, and the contrarians are going to try to use it for all it's worth. But there are also intellectual property rights issues, so it isn't clear how these sorts of things will play out ultimately in the United States.

Ah, similar hiding places on the other side of the Atlantic, too.

February 21, 2005: **email 1109021312**

Phil Jones writes to Mike Mann, Ray Badley, and Malcolm Hughes, regarding news reports that Mann will be forced to release his data:

> The skeptics seem to be building up a head of steam here!
>
> …
>
> Leave it to you to delete as appropriate!
>
> …

> PS I'm getting hassled by a couple of people to release the Climatic Research Unit … temperature data. Don't any of you three tell anybody that the United Kingdom has a Freedom of Information Act!

One would think that, eventually, people would realize this without having to be told …

March 17, 2005: **email 1111085657**

Ray Bradley writes to Phil Jones and Mike Mann, alerting them to a report by the British Broadcasting Corporation (BBC) on the controversy raging over the infamous "hockey stick" graph. Jones replies to Bradley:

> I tried to convince the reporter here that there wasn't a story, but he went with it anyway. At least he put in a quote from me …

Mike Mann responds:

> Yes, the BBC has been disappointing in the way they've dealt with this—almost seems to be a contrarian element there.

Terrible! The British national broadcaster does not simply parrot their words?

They had better find the culprit:

> Do you remember the name of the reporter you spoke to?

Jones:

> The reporter was Paul Rincon.

Mann:

> I've got a call in from a different BBC reporter today, Ben Dempsey, who seems much better. He's doing something for Horizon on climate change. Do you know anything about this?

Phil Jones:

> On Horizon, I'm supposed to be called in a few minutes by someone. I'm not sure who yet. This program is generally good. They did something on global dimming a few months ago and now want to do something on the truth about global warming, the Intergovernmental Panel on Climate Change and skeptics. That's all I know so far. The person's name is Paul Olding. He should be calling at 2:00 pm, so in five minutes' time.

In other words, it's acceptable for the BBC to be biased—as long as it is in their direction.

April 27, 2005: **email 1114607213**

Steve McIntyre writes to Phil Jones:

> Dear Phil,
>
> In keeping with the spirit of your suggestions to look at some of the other multiproxy temperature publications, I've been looking at Jones and coworkers paper of 1998. The methodology here is obviously more straightforward than for

> the Mann, Bradley, and Hughes paper of 1998. However, while I have been able to substantially emulate your calculations, I have been unable to do so exactly. The differences are larger in the early time periods.
>
> Since I have been unable to replicate your results exactly based on available materials, I would appreciate a copy of the actual data set used in the Jones and coworkers paper of 1998 as well as the computer programs used in these calculations.
>
> There is an interesting article on replication of results by independent scientists by … some distinguished economists (gives link), discussing the issue of replication in applied economics and referring favorably to our attempts to replicate results in respect to the paper of Mann, Bradley, and Hughes from 1998.
>
> Regards, Steve McIntyre

Phil Jones forwards it to Mike Mann:

> I got this email from McIntyre a few days ago. As far as I'm concerned he has the data — sent ages ago. I'll tell him this, but that's all — no computer program. If I can find the program, it is likely to be hundreds of lines of undocumented FORTRAN!

Any computer programmer would know that FORTRAN—a computer language so old that its name is spelt in uppercase, because computers did not have lowercase letters back then—is very efficient at performing mathematical calculations, but very obscure to understand if extensive documentation is not provided throughout the program, and very easy to make mistakes in if the program is not well-structured and well-documented.

So we now know that the Climatic Research Unit had no policies covering the checking of results, data archiving, or anything to control the writing and archiving of computer programs!

That the claims of climate change could rest on such a parlous state of affairs would be hilarious, if it were not so serious.

Jones continues to reminisce about his FORTRAN program:

> I recall the program did a lot more that just average the series. I know why he can't replicate the results early on—it is because there was a mathematical adjustment when there were fewer data sets.

In other words, McIntyre was exactly correct: the data did not match in the earlier time periods, because Jones's program—the one that he refuses to hand over—fiddled with the data.

It is remarkable that the only thing that Jones can remember about this work from seven years previously is that he had to adjust the data.

June 27, 2005: **email 1119901360**

Jonathan Overpeck writes to Keith Briffa, Tim Osborn, and Eystein Jansen, concerned about highly influential early diagrams first "created" by Hubert Lamb, pioneer of the Climatic Research unit at the University of East Anglia:

> I'm sure you saw the recent (to be infamous) Wall Street Journal editorial—they showed what I think was a Intergovernmental Panel on Climate Change First Assessment Report curve — with the good old Medieval Warm Period and Little Ice Age, etc. (Lamb view? — I don't have the First Assessment Report with me). The way to handle the hockey stick might best be to put it in an historical perspective along with the older Intergovernmental Panel on Climate Change views. First, show your great figures, discuss them and what went into them, and then—after showing the state-of-the-art, discuss how much our understanding and view have changed. In this, simply compare each of the historical views (First Assessment Report, Second Assessment Report, Third Assessment Report) to the current view, and while doing so, play down the controversy(s)—especially the hockey stick. The smart folks will realize that that the fluff in the news is just that, but those with a real stake in that debate will hopefully get the point that it doesn't matter…

This is a remarkable admission. It was the work of Lamb and others (to be discussed at greater length shortly) that sparked the fears of climate change in the first place. Just as these scientists "re-branded" their claims—from the "Greenhouse Effect" to "Global Warming" to "Climate Change"—so too did they change their apparently "rock solid" results, through the First, Second, and Third Assessment Reports of the Intergovernmental Panel on Climate Change.

The Medieval Warm Period and subsequent Little Ice Age—so well established in both history and the scientific evidence that these very scientists showed them clearly on their graphs—gradually became "undesirable", as it was realized that it would not simply be sufficient to show the planet warming, but essential to argue that it was unprecedented warming.

But by this time—mid-2005—the mainstream media had begun to take note of the increasing number of scientists crying foul over this subtle but systematic form of scientific revisionism—more than four years before the exposure of the "Wikipedia Terrorist", William Connolley.

Overpeck is here effectively telling his colleagues that "the evidence doesn't matter"—that all that is important is that, at any point in time, they had some evidence that apparently substantiated their claims. That they subsequently discredited their own evidence is to be swept under the rug!

If there was any lingering doubt that these "scientists" were simply working towards justifying a predetermined conclusion, rather than honestly seeking the scientific truth, then these comments—and those to follow—eliminate them.

June 27, 2005: **email 1119924849**

Jonathan Overpeck writes again to Keith Briffa, Tim Osborn, and Eystein Jansen, over the brewing storm:

> The recent Wall Street Journal editorial that is creating all the crap in the United States actually showed a graph from the Intergovernmental Panel on Climate Change (IPCC) First Assessment Report—if you don't have it, or Eystein can't send it, I can scan it in (my Republican Dad sends me these things, although he's an increasingly rare breed of moderate Republican). My thought is that it might be worth adding a couple lines of text documenting how the IPCC view of the Medieval Warm Period changed with each of its Assessment Reports and new knowledge. In doing so, it could be made very clear that there is a reason that scientists don't show those old graphs anymore. We need to move the debate beyond the IPCC's First Assessment Report, Second Assessment Report, and Third Assessment Report on this issue!

In other words, these scientists themselves have discredited their own IPCC claims; their only problem is achieving their revisionism of history—even to the point of "moving beyond" their most recent IPCC Report!

Overpeck's assumption that one's opinion on climate science is dictated by partisan political persuasion is concerning.

June 28, 2005: **email 1119957715**

A Subcommittee of the United States House of Representatives is investigating Mike Mann's scientific claims. Mann writes:

> This was predicted—they're of course trying to make things impossible for me. I need immediate help regarding recourse for free legal advice, etc.

Why would an innocent man need legal advice? Surely he could simply testify truthfully?

Michael Oppenheimer responds:

> This is outrageous. I'll contact some people who may be able to help right away.

Oppenheimer knows he is guilty.

Tom Wigley responds:

> I would not advise a legal route. I think you need to consider this as just another set of referees' comments and respond simply, clearly and directly.

In contrast to Mann and Oppenheimer, Wigley obviously believes, at this point in time, that Mann has nothing to hide. Rather, he thinks it is a plot:

> Although this may be difficult, remember that this is not really a criticism of you personally, but one aspect of a criticism of the foundations of global warming science by people both inside and outside of Congress who have ulterior motives.

However, his true colors quickly shine through: this is an opportunity to weed out those who are not "on the team":

> There may, in fact, be an opportunity here. As you know, we suspect that there has been an abuse of the scientific review process at the journal editor level. The method is to choose reviewers who are sympathetic to the anti-greenhouse view. Recent papers in Geophysical Research Letters (including the McIntyre and McKitrick paper) have clearly not been reviewed by appropriate people. We have a strong suspicion that this is the case, but, of course, no proof because we do not know who the reviewers of these papers have been. Perhaps now is the time to make this a direct accusation and request (or demand) that this information be made available. In order to properly defend the good science it is essential that the reasons for bad science appearing in the literature be investigated.

We need not wonder from which ranks the "appropriate people" should be chosen.

Further on, Wigley uses a curiously appropriate turn of phrase:

> The others who could be added to this email list at this early stage are Ray Bradley and Malcolm Hughes, your "co-conspirators"—and perhaps Phil Jones, Keith Briffa and Tim Osborn.

Well that answers the question of whether it is fair to call them "(co-)conspirators": it is their own term!

One would imagine that Wigley would suggest that Mann use these other colleagues to support his scientific claims. But the reality is the exact opposite:

> A word of warning. I would be careful about using other, independent paleoclimatology … work as supporting your work. I am attaching my version of a comparison of the bulk of these other results. Although these all show the "hockey stick" shape, the differences between them prior to 1850 make me very nervous. If I were on the greenhouse deniers' side, I would be inclined to focus on the wide range of paleoclimatology results and the differences between them as an argument for dismissing them all.

And that is the final nail in the coffin of the greatest scientific fraud in the history of mankind: Wigley has personally proven that **all** of the temperature estimates for before 1850—the period needed to show that the warming is "unprecedented"—are so discrepant as to be inconclusive.

Mike Mann, as always, cares only about himself:

> Thanks—yes, we seem to back in the days of McCarthyism in the States. Fortunately, we have some good people who will represent us legally without charge; and in the best case scenario, this backfires on these thugs…
>
> The response of the wording is likely to change dramatically after consultation with lawyers …

It is remarkable that Mann he ignores completely Wigley's dire warnings that his results are wrong; his only concern is obtaining free legal advice to ensure that he does not have to testify before Congress. We note that he is now drawing in his co-conspirators: his terminology has shifted from "me" to "us".

June 28, 2005: **email 1120014836**

Jonathan Overpeck indicts his own Intergovernmental Panel on Climate Change (IPCC) co-authors:

> Also, please note that, in the United States, … Congress is questioning whether it is ethical for IPCC authors to be using the IPCC to champion their own work and opinions. Obviously, this questioning is wrong and scary, but if our goal is to get policy-makers (liberal and conservative alike) to take our Chapter of the IPCC Report seriously, it will only hurt our effort if we cite too many of our own papers (perception is often reality). Please do not cite anything that is not absolutely needed, and please do not cite your own papers unless they are absolutely needed. This is common sense, but it isn't happening. Please be more critical with your citations so we save needed space, and also so we don't get perceived as self-serving, or worse. Again, we can debate this if anyone thinks I've gone off the deep end.

Overpeck is absolutely correct; to even feel the need to make these comments to his co-authors is astonishing. That he thinks his co-authors may believe him to have "gone off the deep end" is simply astounding—yet Eystein Jansen does disagree:

> Having the fortune of not being that close to the darker sides of United States politics, I have the feeling that Peck's comment concerning referencing perhaps is a bit too "paranoic".

July 5, 2005: **email 1120593115**

Phil Jones sends an article and a blog entry to climate scientist John Christy:

> This quote is from an Australian at the Bureau of Meteorology Research Centre, Melbourne (not Neville Nicholls). It began from the attached article. What an idiot. The scientific community would come down on me in no uncertain terms if I said the world had cooled from 1998. OK, it has, but it is only 7 years of data and it isn't statistically significant.

Again, Jones's ability to concisely summarize the key facets of this scandal are remarkable. That this leader is scared of his own "scientific community"—to the point of his not being allowed to state something publicly which he acknowledges is actually true, is telling. Could you imagine how intimidated the more junior scientists would be?

It is also extremely telling that Jones excuses his silence on the grounds of statistical insignificance only for facts, such as these, that go against the "relentless warming" message. In fact, his own colleagues have shown that all of their temperature estimates lack statistical significance. The correct course of action would be to be silent altogether.

Later in the same email:

> As you know, I'm not political. If anything, I would like to see the climate change happen, so the science could be proved right, regardless of the consequences. This isn't being political, it is being selfish.

So Jones would prefer catastrophic global warming to actually occur, just so that he could bask in the accolades of being "proved" right! Ignoring the fact that catastrophic global warming—if it occurred—would not prove whether mankind's liberation of carbon dioxide was a causative factor at all, Jones's "ego trip" death-wish offers us yet another insight into the character of this man.

July 6, 2005: **email 1120676865**

Neville Nicholls, of the Bureau of Meteorology Research Centre in Melbourne, Australia, asks Phil Jones:

> Do you expect to get a call from Congress?

Jones replies:

> I hope I don't get a call from Congress! I'm hoping that no-one there realizes I have a United States Department of Energy grant, and have had this (with Tom Wigley) for the last 25 years.

The fact that Jones received these grant moneys from a foreign government department is not an issue; it is a normal and healthy part of scientific research. What is astounding is his hiding of the fact. It is standard scientific practice to acknowledge all sources of funding, however indirect. For example, in the Acknowledgments section of a paper that my Ph.D. supervisor and I had published in the International Journal of Modern Physics in 1992, we include the following sentence:

> We warmly thank the Institute for Nuclear Theory at the University of Washington for its hospitality and the United States Department of Energy Grant #DOE/ER40561 for partial support during the completion of this work.

This is absolutely standard practice—and the Department of Energy Grant acknowledged here was not even funding us directly, but rather the Institute for Nuclear Theory itself, which hosted both of us for some months in early 1992. That Jones not only did not acknowledge his sources of public funding, but moreover hoped that he could keep the fact hidden to avoid proper scrutiny by the United States Congress, is dismaying.

July 25, 2005: **email 1122300990**

Tom Crowley to Jonathan Overpeck, Keith Briffa, and Eystein Jansen:

> Hi all, there is another reason why I should not be formally listed as a Lead Author — it is my understanding that Intergovernmental Panel on Climate Change contributors have to be a little careful about getting involved in political matters that could be used to impugn the integrity of the process—well, I am starting to do just that, with the attached comment in Eos, plus some radio interviews where I have been somewhat pointed in my thoughts.
>
> I suppose it's still ok to be a reviewer, but even then you might keep these comments in mind.

Crowley's realization, and offer to recuse himself, is admirable. It is a pity that more of his colleagues did not share his sense of integrity and impartiality.

July 26, 2005: **email 1122422429**

Tim Osborn writes to Keith Briffa, Jonathan Overpeck, and Eystein Jansen:

> As you'll have seen from Tom Crowley's replies to my fairly direct requests for the data that went into his Medieval Warm Period graph, he seems somehow reluctant to send it to me and prefers me to find it myself (including spending a week re-assembling a Mongolian data set). I have no time to do this, so have instead reverted to using the very similar data that we already had.

It is astounding that even Crowley's own colleagues were rebuffed in their requests for the data that went into this important graph, and basically told to "figure it out yourself". It is therefore not surprising that independent auditors were met by outright hostility!

August 4, 2005: **email 1123163394**

Phil Jones and Mike Mann again demonstrate their real forte: intelligence gathering. Phil Jones:

> If you've not gone to China yet—you'll meet someone called Martin Dukes (?). He's giving a talk at your session. He knows about mathematics, etc., but not much about paleoclimatology! He might need some education, but is probably OK. I have not met him, but Tim has. He is doing some worked funded by the Dutch government on the "hockey stick" graph.

Mann:

> Thanks, yes I'm in China now. …
>
> Martin Juckes has an invited talk in my session. I invited him, because he was working with Stott and coworkers, and so I assumed that he was legitimate, and not associated with the contrarians. But if he's associated with the Dutch group, he may actually be a problem. Do you have additional information about him and what he has been up to?

Jones:

> He's been working with Myles Allen. Tim went to the first meeting of this Dutch-funded project near Oxford last week.
>
> Tim said they were doing some odd things …
>
> The meeting wasn't that productive, according to Tim. There was a belief amongst those there that all the trees you used have lost their low-frequency information (the information needed to estimate long-term trends). …
>
> Tim got the impression that they wanted to find that Mann, Bradley, and Hughes (the "hockey stick" paper)is wrong. …
>
> Martin isn't associated with the contrarians, but he's not in possession of … all the facts.

Mike Mann:

> Thanks for the heads-up (warning). I will be prepared for this, then. I thought that Gabi Hegerl was involved with this guy? Doesn't she know better? It is disturbing that she hasn't set them straight on this.

Phil Jones:

> Gabi was supposed to be there, but wasn't either. I think Gabi isn't being as objective as she might, because of Tom Crowley. …

What more needs to be said?

Jones now makes a remarkable comment, about something that is elementary to even high-school science experiments:

> There is an issue coming up in the Intergovernmental Panel on Climate Change. Every graph needs uncertainty bars, and having them is all that matters. It seems irrelevant whether they are right or how they are used.

In other words, he is only concerned that they give the appearance of estimating the uncertainties in their predictions, rather than actually getting those (subtle and difficult) vital calculations right.

August 5, 2005: **email 1123268256**

Jonathan Overpeck writes to Tim Osborn, Eystein Jansen, Keith Briffa, and Oyvind Paasche, regarding increasing problems around the Medieval Warm Period:

> I hope you're not going to kill me, but I was talking with Susan Solomon today, and she impressed me with the need to make several points if we can.
>
> One issue … is whether we can extend the Medieval Warm Period graph to include the 15th century. I don't read the blogs that regularly, but I guess the skeptics are making hay of there being a global warm event around 1450. I agree with Susan that it is our obligation to weigh in on issues like this, so… can we extend the graph to extend up to 1500?

August 8, 2005: **email 1123513957**

Tim Osborn responds to Jonathan Overpeck:

> There is a period around 1400 when the temperature proxy records we used in this Medieval Warm Period graph do indicate a warm period—and all records show higher temperatures at the same time. Thus it couldn't/shouldn't be dismissed in the same way as the Medieval Warm Period…

That the goal should be to "dismiss" anything is in itself disturbing. However, Overpeck's response is astounding:

> This means that the Medieval Warm Period graph needs to talk about the period around 1400—can you make sure that's on Keith's radar screen. I believe that historians talk about the Medieval Period going to at least 1450, so what the heck…

One would have thought that, for these scientists to "dismiss" the Medieval Warm Period, their knowledge of it should have been encyclopedic (but not, of course, wikipedic). Instead, they are being educated by their critics—even resorting to the historian's definition of "Medieval" to limit the scope of their investigations.

We get the picture of a group of scientists running around putting out fires, rather than performing a careful, comprehensive investigation before making their public pronouncements.

August 25, 2005: **email 1124994521**

Mike Mann writes to Christoph Kull, Phil Jones, Heinz Wanner, and others:

> In our discussion of possible participants in Bern, I think (someone correct me if I'm wrong) we concluded that the last two on the list (with question marks) would be unwise choices because they are likely to cause conflict than to contribute to consensus and progress.

Phil Jones to Christoph Kull:

> I agree with Mike that the last two names on the list should be removed.

Debate and disagreement is crucial to the healthy functioning of science. Weeding out those who may prevent a predetermined "consensus" is abhorrent.

August 26, 2005: **email 1125067952**

Heinz Wanner to Christoph Kull:

> Concerning the participants:
>
> …
>
> - If Phil and Mike do not support von Storch it does not make sense to invite him (or Eduardo Zorita?).

Mike Mann concurs:

> I'm afraid I don't agree on Zorita. He has engaged in some very nasty, and in my opinion unprofessional email exchanges with some close colleagues of mine who have established some fundamental undisclosed errors in work he co-published with von Storch. Given this, I don't believe he can be involved in constructive dialogue of the sort we're looking for at this workshop.

Again, a "constructive" dialog appears to be one that leads to their predetermined "consensus".

He continues:

> There are some similarly problematic issues with Cubasch, who, like von Storch, … has engaged in inflammatory and personal public commentary. There is no room for that on any side of the debate.
>
> If the Germans need to be represented here, I would suggest instead someone from the Potsdam group, such as Eva Bauer …

Our attention is here drawn to an undercurrent in this entire saga: the need to give the perception of international agreement—which translates into the notion of the need for a "quota" of representation from the key countries involved, rather than true international debate.

September 19, 2005: **email 1127614205**

Steve McIntyre to the Intergovernmental Panel on Climate Change Working Group I:

> For the unpublished articles referenced in the draft Intergovernmental Panel on Climate Change Working Group I Report, could you also provide locations of download sites where the underlying data may be reviewed.

In a follow-up email:

> I have been unable to locate supplementary information or data archives for several of the articles posted… and would appreciate assistance in this regard.

Martin Manning to Jonathan Overpeck and Eystein Jansen:

> Following the release of the first draft of the Report we have had a response from Steve McIntyre (a name that should ring a bell) regarding unpublished literature in our Chapter. He also asks about access to data sets but that is not an Intergovernmental Panel on Climate Change function so is easily dealt with.

Again, a technicality to "hide behind".

Manning continues:

> I am attaching the correspondence with McIntyre below for your information, but the only issues you need to consider are those above, and we will handle any further interactions with McIntyre from here.

In other words, McIntyre will be "handled", obviating the need for the scientists who authored the papers to deal with him.

September 27, 2005: **email 1128000000**

Steve McIntyre writes to Colin O'Dowd, Editor of Journal of Geophysical Research:

> Dear Dr O'Dowd,
>
> I am a reviewer for the Intergovernmental Panel on Climate Change (IPCC) Fourth Assessment Report …and am writing in respect to a submission to your journal by D'Arrigo and coworkers … This article was referenced in Chapter 6 of the Draft Report and made available to IPCC reviewers. In the course of my review, I contacted the senior author, Dr. D'Arrigo, for the download location of the data used in this article, or for alternative access to the data. Dr. D'Arrigo categorically refused, and I was referred to the journal editor if I desired recourse.
>
> Data Citation and Archiving
>
> I point out that American Geophysical Union (AGU) policies for data citation and data archiving (provides link) specifically require that authors provide data

> citation according to AGU standards, and require that contributors archive data in permanent archives, such as the World Data Center for Paleoclimatology.
>
> …
>
> In cases where the data has been archived, it has not been cited according to AGU policies. …
>
> In order that this submission comply with AGU policies on data archiving, I request that you require D'Arrigo and coworkers do (1) provide accurate data citations complying with AGU policies for all data sets presently archived at the World Data Center for Paleoclimatology; (2) archive all "grey" data used in the article.

A reasonable request, one would think?

Rob Wilson forwards the request to Tim Osborn and Keith Briffa:

> Please see the e-mail (attached) from Steve McIntyre to the Editor of the Journal of Geophysical Research. This seems a major abuse of his position as reviewer for the IPCC?

Tim Osborn replies to Rob Wilson and Keith Briffa, including Rosanne D'Arrigo in his response:

> Dear Rob and Rosanne,
>
> I strongly agree that this is an abuse of his position as an IPCC reviewer! The data archiving issues are a separate issue because I think there's no need for the data you used to be publicly available until the paper is actually published …

In other words, they should be able to use the pending publication in their IPCC Chapter, but at the same time block McIntyre's ability to review the paper on the technicality that it has not actually been published at the time that he wishes to review it! This is a remarkably duplicitous tactic.

Osborn finishes with a call to round up the troops:

> I will take this issue up with the Chapter Lead Authors and the Working Group 1 technical support unit—unless you prefer that I didn't. Please let me know.

Osborn then keeps the process rolling, writing to Phil Jones, Eystein Jansen, Jonathan Overpeck, and Keith Briffa:

> Dear Phil, Eystein and Peck,
>
> I've already talked about this to Phil and Keith, but for Eystein's and Peck's benefit the emails copied below relate to McIntyre downloading a copy of a manuscript cited by the IPCC paleoclimatology chapter …
>
> Rosanne replied to my email below, to say that they do want this taken further. So…
>
> Phil has agreed to forward these messages to Susan Solomon and Michael Manning.

> Eystein and Peck: do you want to add anything too?

It almost goes without saying that actually providing McIntyre with the data would take far less collective effort than this rearguard action.

November 15, 2005: **email 1132094873**

Mike Mann writes to Tim Osborn, Phil Jones, and Keith Briffa:

> I' not sure if you guys are aware: McIntyre presented this poster at the Climate Change Science Program meeting. Apparently, they gave him a very prominent location, so that everyone entering the meeting would have seen the poster…

Even a poster at a conference is enough to get the intelligence chatter going!

Tim Osborn replies:

> Thanks for this, Mike. We'd spotted an earlier draft of his poster and were a bit concerned about this receiving prominence at the meeting. Did it arouse much discussion, do you know?

Mann:

> He almost had a point with a mathematical issue, but as we all know, that doesn't matter at all in the end. The issue isn't whether or not he's right, as we all well know by now, but whether his false assertions have enough superficial plausibility to get traction. In this case, they might, so it's probably good to at least be prepared.

It is astounding to see Mann acknowledging that McIntyre's criticisms of their mathematical methods are correct, and nevertheless believing that it doesn't really matter to the "big picture". This is a common theme: it doesn't matter if each and every piece of evidence is systematically shown to be flawed; all that matters is that the "proponents" can jump to a new claim to justify their predetermined conclusions.

That mode of operation is as false in science as it is in the law.

Mann continues:

> I was told by a journalist Paul Thacker that his poster got prominent placement; probably not an accident (see forwarded email). I believe that Mike Schlesinger and David Karoly were there in the same session, so it might be worth checking with them. I think Connie Woodhouse and Tom Wigley were also at the meeting, but not sure…

If the science was rock-solid, why would they be obsessed with the finding out the identity of the person who was responsible for the prominent placement of a dissenting poster?

Mann reports victory in at least one battle:

> The Geophysical Research Letters leak may have been plugged up now with new editorial leadership there, but these guys always have Climate Research and

> Energy and Environment, and will go there if necessary. They are telegraphing quite clearly where they are going with all of this…

So they have "taken control back" of at least one journal.

However, Keith Briffa remarkably acknowledges that, despite all this political maneuvering and interference, the scientific arguments of McIntyre are actually valid:

> … so they are sort of right that the emphasis on 1032 is probably overdone.

Isn't that what these scientists should be concentrating on?

December 2, 2005: **email 1133532909**

Mike Mann writes to many:

> I thought you all would be interested in this. Esper and coworkers have played right into the hands of the contrarians:
>
> (FOX News story link)
>
> The wording of their Abstract is frankly just irresponsible…

In other words, they are to be condemned for being honest and for failing to dress up their statements in sufficiently alarmist tones.

February 3, 2006: **email 1138995069**

Keith Briffa is still struggling with the fact—already highlighted above by Tom Wigley—that, when the uncertainties in their temperature estimates are reported fairly, they completely "swamp" the variations that they are trying to use to bolster the conclusion of man-made global warming. He writes to Jonathan Overpeck and Eystein Jansen:

> We are having trouble … expressing the real message of the results—being scientifically sound in representing uncertainty, while still getting the crux of the information across clearly. It is not right to ignore uncertainty, but expressing this merely in an arbitrary way (and as a total range as before) allows the uncertainty to swamp the magnitude of the changes through time.

But that is the truth of the matter! Perhaps "the penny is starting to drop" for Briffa.

February 13, 2006: **email 1139835663**

The National Research Council of The National Academies of the United States invites Keith Briffa to appear before its enquiry in Washington, D.C. Keith Briffa writes to Mike Mann:

> IN STRICT CONFIDENCE I am sending this for your opinion. To be frank, I am inclined to decline. What do think? Presumably you and others are already in the frame?

Mann remains keen for his co-conspirators to be in the frame. He responds:

> I think you really should do this if you possibly can. The panel is entirely legitimate, and the report was requested by Sherwood Boehlert, who as you

> probably know has been very supportive of us in the whole Barton affair. … Especially, with the new Science article by you and Tim I think its really important that one of you attend, if at all possible.

If one is wondering about Mann's definition of "legitimate", he quickly erases any doubt:

> The panel is solid. Gerry North should do a good job in chairing this, and the other members are all solid. Christy is the token skeptic, but there are many others to keep him in check:
>
> (link to list of members)
>
> So I would encourage you to strongly reconsider!

Ah! In other words, the panel is already "stacked"; it is friendly; and so there is no danger in Briffa appearing before it.

However, Briffa's lack of confidence is manifest:

> Thanks for this, but after a lot of soul-searching this weekend, I have decided to decline the invitation. Pressure of stuff here is intense—but the real reason is that I really think it could be politic to retreat into "neutral" mode, at least until after the Intergovernmental Panel on Climate Change Report is out. I know you can argue this various ways, but the sceptics are starting to attack on this "non neutral" stance, and the less public I am at the moment, the better, I think. I hope you do not think I am a wimp here—just trying to go the way I think best.

Mann, who no doubt understands the message that non-appearance by one of their key scientists would send, tries to get Briffa to reconsider:

> I'm pretty sure they're just asking for a neutral discussion of the science that you've done that is relevant to the issues being reviewed by the Committee (after all, this is the United States National Academy of Sciences, not the United States Senate, etc.). But I understand where you're coming from, nonetheless. Perhaps you could suggest an alternative speaker? Is there any possibility that Tim could do this instead? My greatest fear is that McIntyre dominates the discussion. It's important that they hear from the legitimate scientists.

Again, we know what Mann's definition of "legitimate" is.

March 8, 2006: **email 1141398437**

Richard Alley writes to Jonathan Overpeck:

> Do you know anything about the "divergence problem" in tree rings? Rosanne D'Arrigo talked to the National Research Council yesterday. I didn't get to talk to her afterward, but it looked to me that they have redrilled a bunch of the high-latitude tree rings that underlie almost all of the high-resolution estimates, and the tree rings are simply missing the post-1970s warming, with reasonably high confidence. She didn't seem too worried, but she apparently has a paper just out in the Journal of Geophysical Research. It looked to me like she had pretty well killed the "hockey stick" graph in public forum—they go out and look for the most-sensitive trees at the edge of the treeline,

> flying over lots and lots of trees that are less sensitive but quite nearby, and when things get a little warmer, the most-sensitive trees aren't sensitive any more; and so the trees miss the extreme warming of the recent times, and can't reliably be counted as catching the extreme warmth of the Medieval Warm Period if there was extreme warmth then.
>
> Because, as far as I can tell, the "hockey stick" really was a tree-ring record, regardless of how it was labelled as "multiproxy", this looks to me to be a really big deal. And, a big deal that may bite your Chapter of the Intergovernmental Panel on Climate Change Report …

Overpeck responds:

> Hi Richard—this issue is one that we refer to in our key uncertainty table. I believe Keith Briffa was one of the first to write about it, and it is an important issue. I haven't seen Rosanne's paper or results myself, but I bet Keith has. I'm cc'ing this to him to see what he thinks.

Keith Briffa responds:

> We do need to say something, but as I said in an earlier message, not without more consideration. We should not write something curt on this—ditto the possible direct effects of carbon dioxide. In the push to do all this other stuff, we have had to leave it—to discuss later how to include an "uncertainty issues" bit about recent environmental mess-ups. The D'Arrigo paper is not convincing, but we have to do some work to show why, instead of just saying this.

Indeed! Briffa finally realizes that brief public assertions without any scientific backing will no longer be credible.

He continues:

> The divergence issue is not universal, and not unrelated to very recent period bias arising from processing methods.

In other words, the problem is real—and its extent unknown. That "processing methods" can completely bias their results completely undermines their stated public confidence that "the science is settled". Indeed, Briffa explains how little they really know—and this is 2006, not 1986:

> It is very likely not the threshold problem that D'Arrigo thinks it is. We need money here to work on this, and losing our last application to Europe has messed us up. For now we cannot include anything. I will work on text for the next Intergovernmental Panel on Climate Change Report.

In other words, as of 2006, they needed funding to begin new research to even determine how reliable any of their previous results were. Is this "settled science"?

March 7, 2006: **email 1141737742**

Steve McIntyre is trying to get the data needed to verify a paper published in Science. Jesse Smith of Science writes to Tim Osborn:

> We have just received an email from Steve McIntyre (pasted below), with a long and very specific list of alleged deficiencies in the availability of data by which to evaluate your recent paper … We would like to have your confidential response to this request, keeping in mind the stated policy of Science that "Any reasonable request for materials, methods, or data necessary to verify the conclusions of the experiments reported must be honored."

Osborn replies to Smith:

> Before responding to the specific data requests, we would like to say that it is our view that we should provide sufficient data to enable all the main elements of our analysis to be checked, but that we are not obliged to provide the data that would enable the research reported in other papers to be checked, even if we cite those other papers or use results reported in those other papers. You will see how this view has determined our response to some of the requests.

A continuation of the same cunning tactics previously employed: we are allowed to cite results published by others; but if you want their data, that's your problem, not ours. Science seems satisfied:

> Thank you for your clear and careful response to the requests made by Mr. McIntyre, which we forwarded to you: it was quite satisfactory, we believe, and will greatly help Brooks (Hanson) in crafting his reply to Mr. McIntyre. I hope that this will be the end of this episode, but if it is not, we will be in touch again.

The relief at Science is almost palpable!

Osborn seems to realize that the victory is only temporary:

> Keith—see below. I bet it won't be the end of the episode!

March 8, 2006: **email 1141849134**

Richard Alley to Jonathan Overpeck, on the growing crisis:

> The big issue may be that you don't just have to convince me now; if the National Research Council (NRC) committee comes out as being strongly negative on the hockey stick owing to Rosanne D'Arrigo's talk, then the divergence between the Intergovernmental Panel on Climate Change (IPCC) and the NRC will be a big deal in the future regardless. The NRC committee is accepting comments now (I don't know for how long)… As I noted, my observations of the NRC committee members suggest rather strongly to me that they now have serious doubts about tree-rings as thermometers (and I do, too … at least until someone shows me why this divergence problem really doesn't matter).

Overpeck responds, copying his response to many colleagues:

> Hi gang—Richard is raising important issues, and Keith is going to respond in some detail on Friday when he gets back. I am cc'ing this to a broader group of IPCC Chapter 6 folks so that we make sure we (Chapter 6) deal with the issues correctly. I'm hoping that Keith will cc to us all, and we'll go from there.

For those just in on the issue raised by Richard. There is a paper written by Rosanne D'Arrigo that apparently casts serious doubt on the ability of tree ring data to reconstruct the full range of past temperature change—particularly temperatures above mid-20th century levels. Chapter 6 obviously has to deal with this more in the next draft, so Eystein and I would like to get on top of it starting this week.

Keith or Richard—do you have a copy of this paper? Is it accepted?

Yet again, the problem is being handed to Keith Briffa, the one person who has most doubts about the validity and uncertainty of the reconstructions.

March 11, 2006: **email 1142108839**

Richard Alley continues on the crisis. In his summary:

These considerations do somewhat affect the confidence that can be attached to the best estimate of recent warmth versus that of a millennium ago. … By demonstrating that some tree-ring series chosen for temperature sensitivity are not fully reflecting temperature changes, the divergence issue widens the error bars and so reduces confidence in the comparison between recent and earlier warmth.

This is the message that Keith Briffa has been trying to get across, apparently with greater success.

April 26, 2006: **email 1146062963**

Mike Mann to Tim Osborn, Scott Rutherford, Keith Briffa, and Phil Jones, regarding Steve McIntyre's request for data:

I'm saddened to hear that this bozo is bothering you too, in addition to the National Center for Atmospheric Research, the National Science Foundation, the National Academy of Sciences, the Intergovernmental Panel on Climate Change and everyone else. Rest assured that I won't ever respond to McIntyre should he ever contact me, but I will forward you any email he sends related to this. I assume Scott feels the same way…

May 12, 2006: **email 1147435800**

Mike Mann to Tim Osborn, on Steve McIntyre:

Personally, I don't see why you should make any concessions for this moron.

So Mann is prepared to describe McIntyre in private as a "bozo" and "moron", and yet also has said that McIntyre "… has engaged in inflammatory and personal public commentary. There is no room for that on any side of the debate." It's OK to say it in private, but not in public?

May 18, 2006: **email 1147982305**

Neil Roberts writes to Jonathan Overpeck:

> Please excuse me for writing direct, but Keith Briffa suggested it would be simplest. I have looked through the draft Chapter 6 of the Intergovernmental Panel on Climate Change (IPCC) Report … However, bullet 4 on page 6.2, starting "global mean cooling and warming… .." strikes me as incorrect and misleading.

Roberts outlines his objections. Overpeck replies:

> Hi Neil—Thanks for your interest in providing feedback on the draft … Since the IPCC has very strict rules about all this, I'm going to ask them (the IPCC) to send you an official invitation to review, along with the process—formal, but highly efficient—to follow. If you could send your comments in that way it would be a great help. We've been asked to keep everything squeaky clean, and not to get comments informally.

So, "squeaky clean" only when criticized?

June 21, 2006: **email 1150923423**

John Mitchell, Director of the United Kingdom's Met(eorological) Office, to Jonathan Overpeck, Eystein Jansen, Jean Jouzel, Keith Briffa, and Tim Osborn:

> The issue of why we don't show the temperature proxy data for the last few decades (they don't show continued warming) but assume that they are valid for early warm periods needs to be explained.
>
> …
>
> Is the mathematical approach robust? Are the results statistically significant? It seems to me that in the case of Mann, Bradley, and Hughes (the "hockey stick" paper) the answer to each question is no. It is not clear how robust and significant the more recent approaches are.
>
> …
>
> … the comments give the impression that the recent 50-year warming is unprecedented over the last 500 years (seems reasonable) and elsewhere over the last 1000 years (less clear).

So the "hockey stick" is acknowledged to be dead. Even the recent warming is only a "reasonable" impression for the past 500 years—not surprising, because most of the past 500 years was the Little Ice Age.

August 1, 2006: **email 1154484340**

Keith Briffa to Jonathan Overpeck:

> The Intergovernmental Panel on Climate Change's Third Assessment Report was, in my opinion, wrong to say anything about the precedence (or lack thereof) of the warmth of the individual year 1998.

> The reason is that all reconstructions have very wide uncertainty ranges bracketing individual-year estimates of part temperature. Given this, it is hard to dismiss the possibility that individual years in the past did exceed the measured 1998 value. These errors on the individual years are so wide as to make any comparison with the 1998 measured value very problematic, especially when you consider that most reconstructions do not include it in their calibration range … and the usual estimates of uncertainty calculated … would not provide a good estimate of the likely error associated with it even if data did exist.

Again, Keith Briffa is reiterating the impact of uncertainties in the calculations—and coming to the sinking conclusion that the public pronouncements are scientifically indefensible.

January 2, 2007: **email 1167752455**

Ray Bradley writes to Mike Mann and others, about the embarrassing graph discovered in the Reports of the Intergovernmental Panel on Climate Change itself:

> I believe this graph in the 1995 IPCC Report originated in a (literally) grey piece of literature that Jack Eddy used to publish called "Earth Quest". It was designed for, and distributed to, high school teachers. …
>
> I may have inadvertently had a hand in this millennium graph! I recall getting a fax from Jack with a hand-drawn graph, that he asked me to review. Where he got his version from, I don't know. I think I scribbled out part of the line and amended it in some way, but have no recollection of exactly what I did to it. And whether he edited it further, I don't know. But as it was purely schematic (and appears to go through around 1950) perhaps it's not so bad. … In any case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what happened, like something we might sketch on a napkin at a party for some overly persistent inquisitor… (so make sure you don' leave such things on the table…). What made the last millennium graph famous (notorious!) was that Chris Folland must have seen it and reproduced it in the 1995 IPCC Chapter he was editing.

Mike Mann responds:

> Ray, happy holidays and thanks for the (quite fascinating) background on this. It would be good material for an … article for our website. It would be even better if someone could get Chris on record confirming that this is indeed the history of this graphic…

Mann seems completely unperturbed that the IPCC published a completely bogus graph, and that there is no record of where it came from.

The saga continues below…

January 5, 2007: **email 1168022320**

Phil Jones to many:

> I've added a few extra names in the cc of this email list to see if we can definitively determine where Figure 7.1c from the 1990 Intergovernmental Panel

> on Climate Change Report comes from. The background is that the skeptics keep referring back to it and I'd like to prove that it is a schematic and it isn't based on real data, but on presumed knowledge at some point around the late 1980s.

Wonderful! Fake graphs presented in the Intergovernmental Panel on Climate Change Report—but only disclose that once the skeptics take note of it?

January 6, 2007: **email 1168124326**

Stefan Rahmstorf to many, on the embarrassing Intergovernmental Panel on Climate Change (IPCC) Report graph:

> The point is not to blame anyone at all—at least my point was to track down the source in order to be able to show the skeptics (or in my special case, the school authorities) that this old graph is completely superseded and should not be used any more in teaching! And I also see your problem: what we are finding out now makes the IPCC process look somewhat unsophisticated back in 1990, so it is a diplomatic conundrum how to be completely truthful in reporting this, as we need to be as scientists, without providing the skeptics undue fodder for attacking the IPCC. But maybe we're too concerned—the skeptics can't really attack the IPCC easily in this case without shooting themselves in the foot.

Rasmus Benestad:

> I think that this story could possible catch on and make headlines, so I agree that we should be careful. … The sceptics may argue that the IPCC Reports are political after all, and this is also what it sounds like if governments "hoisted the national flag" by having their own graphs inserted at the last minute. However, by providing an account of the "evolution of the IPCC report writing", we could possibly give the story a softer landing. E.g. how many times of review the First Report underwent as compared to the present Report. … There are sometimes a few rotten apples in a good batch, unfortunately.

The unmistakeable message is that the only way to salvage any credibility for the IPCC Reports of the 2000s is to reveal that the IPCC Reports of the 1990s were deeply flawed. But they were the reports that the entire climate charge argument was based on!

January 9, 2007: **email 1168356704**

Tom Wigley, former head of the Climatic Research Unit (CRU), writes to Phil Jones, its then head, on the continuing Intergovernmental Panel on Climate Change (IPCC) graph scandal:

> Subject: Re: That darned diagram.
>
> I see the problems with this in terms of history, IPCC image, skeptics, etc. I'm sure you can handle it. In doing so, you might consider (or not) some of these points.
>
> (1) I think Chris Folland is to blame for this. The issue is not our collective ignorance of paleoclimatology in 1989–90, but Chris's ignorance. The text that was in the 1990 Report (thanks for reminding us of this, Caspar) ameliorates the problem considerably.

> (2) Nevertheless, "we" (the IPCC) could have done better even then. The Rothlisberger data were available then—and could/should have been used.
>
> (3) We also already knew that … Hubert Lamb's United Kingdom record was flawed. We published a revision of this—but never in a mainstream journal because we did not want to offend Hubert. I don't have the paper to hand, but I think it is …
>
> (cites a 1981 paper, of which he himself is first author)
>
> It could be …
>
> (cites a 1986 paper, again with himself as first author)
>
> The point of this paper (whichever one it is) is that it covers only the decade-scale variation—but it shows that Hubert Lamb was out to lunch even on these time scales. As you know, this arose from his uncritical use of historical sources—a problem exposed in a number of CRU papers in the 1980s, staring with Bell and Ogilvie in Climatic Change.
>
> So part of the issue is: where did Hubert get the century time scale changes in that diagram? The answer is, mainly from his own fertile imagination. For this he tried to synthesize both his flawed historical record for England (and records for Europe, equally flawed) and proxy data from many sources, again accepted uncritically. Still, there almost certainly was a Little Ice Age in Europe in the 17th and 18th centuries (but not in Iceland—at least not in the 17th century). Whether or not there was a significant centuries-long Medieval Warm Event is doubtful in my view.
>
> On another historical note, Hubert got many of his ideas from C.E.P. Brooks—possibly Brooks's work is what inspired Hubert to pursue his climate interests. Of course, he went a lot further (too far) because he had a lot more information to work with. However, it is interesting that Fig. 33 in Brooks (1928) looks a lot like the IPCC 1990/Lamb Figure—in Brooks the record goes back further, and there is a very warm period from about 500 to 950.

In other words, even though Wigley admits that his views of historical climate are doubtful and uncertain, he accuses his predecessor Hubert Lamb of producing completely fabricated historical climate results.

Phil Jones replies to Wigley and Caspar Ammann:

> Keep the attached to yourself. I wrote this yesterday, but still need to do a lot more. …
>
> So your point (3) needs to document that we knew the diagram wasn't any good, as well as how far back it goes. Knowing Hubert on some of his other "breakthroughs!" it is clearly possible it goes back to Brooks!
>
> On the post-Lamb work in the CRU, I recall talking to Graham (maybe mid-1980s) when he was comparing recent CRU work with Lamb's results—agreement, etc. Did that ever see the light of day in these publications or elsewhere? I will look. It isn't in the chapter that Astrid and he wrote in the CRU book from 1997. I recall

some very low agreement between Lamb's results and later results—for periods from 1100 to 1500.

> This is all getting quite complex. It clearly isn't something that should be discussed online on our website— at least till we know all the detail and have got the history right as best we can. A lot of this history is likely best left buried, but I hope to summarise enough to avoid all the skeptics wanting copies of these non-mainstream papers. Finding them in the CRU may be difficult!
>
> As for who put the graph in the IPCC Reports—I think I know who did it. Chris may be ignorant of the subject, but I think all he did was use the Department of Energy graph. This is likely bad enough.
>
> I don't think it is going to help getting the real culprit to admit putting it together, so I reckon Chris is going to get the blame. I have a long email from him—just arrived. Just read that and he seems to changing his story from last December, but I still think he just used the diagram. Something else happened on Friday—that I think put me onto a different track. This is all like a mystery whodunit.

It is strange that there are no records of who wrote the relevant part of the IPCC 1990 Report and put in the graph. Never mind—Chris Folland will be blamed anyway.

The scientific fraud is explicit, and covers the entire history of the CRU. But it is best left buried—too many skeletons in that cupboard—if the cupboard could even be found in the CRU archives!

The University of East Anglia should be forced, as an ironic penalty, to name a building after their criminal fraudster, Hubert Lamb.

Oh—that's right: they already did. It houses the CRU.

February 5, 2007: **email 1170724434**

Mike Mann to Curt Covey and many others, regarding Covey sending him an email exchange with leading skeptics Professor Fred Singer and Viscount Monckton of Brenchley regarding the latest Intergovernmental Panel on Climate Change Report:

> Curt, I can't believe the nonsense you are spouting, and I furthermore cannot imagine why you would be so presumptuous as to entrain me into an exchange with these charlatans. What on earth are you thinking? ... You are speaking from ignorance here, and you must further know how your statements are going to be used. You could have sought some feedback from others who would have told you that you are speaking out of your depth on this. By instead simply blurting all of this nonsense out in an email to these sorts of charlatans you've done some irreversible damage. Shame on you for such irresponsible behavior!

Mann is showing who is the leader.

March 8, 2007: **email 1173420319**

Piers Forster to Eystein Jansen, Ken Denman, and others:

> Also please could people approve the attachment of their name to such a letter. Non-highlighted names are people who appear to have already given approval for their name to be used. If you are a yellow highlighted name I think you are likely (or very likely) to sign!
>
> If we could have a relaxed attitude and sign a letter that is still in the process of being drafted it would save someone (me) a bunch of work at the end collecting approvals.

Yes, much easier not having to go through the inconvenience of actually reading what you're signing!

April 21, 2007: **email 1177158252**

Doug Keenan questions a paper of Phil Jones and Wei-Chyung Wang from 1990. Phil Jones writes to Kevin Trenberth, Mike Mann, and Ben Santer:

> It is all malicious. I've cc'd this to Ben and Mike as well, to get any thoughts from their experiences.
>
> If it gets worse I will bring Susan in as well, but I'm talking to some people at the University of East Anglia first. …
>
> … All the language is about me not being able to send them the … data … (as used in 1990!). I don't have this information, as we have much more data now (much more in Australia and China than then) and probably more stations in western USSR … as well.
>
> As for the other request, I don't have the information on the sources of all the sites used in the … database. We are adding in new data sets regularly (all of New Zealand from Jim Renwick recently), but we don't keep a source code for each station. Almost all sites have multiple sources and only a few sites have single sources. I know things roughly by country and could reconstruct it, but it would take a while.
>
> The Global Historical Climatology Network and the National Center for Atmospheric Research don't have source codes either. It does all come from the National Meteorological Services—well mostly, but some from scientists.
>
> … the Keenan letter knocked me back a bit. I seem to be the marked man now!

In other words, the raw data was never properly documented at all, and in any case is now gone.

Mike Mann:

> This is all too predictable. This crowd of charlatans is always looking for one thing they can harp on, where people with little knowledge of the facts might be able to be convinced that there is a controversy. They can't take on the whole of the science, so they look for one little thing they can say is wrong, and thus generalize that the science is entirely compromised.

That is how science is done: claims are analyzed, one by one.

> So they are simply hoping to blow this up to something that looks like a legitimate controversy. The last thing you want to do is help them by feeding the fire. The best thing to do is to ignore them completely. They no longer have their friends in power here in the United States, and the media has become entirely unsympathetic to the rants of the contrarians, at least in the United States—the Wall Street Journal editorial page is about the only place they can broadcast their disinformation. So in other words, for contrarians the environment appears to have become very unfavorable for development. I would advise Wang the same way. Keenan may or may not be bluffing, but if he tries this I believe that British law would make it easy for Wang to win a defamation suit against him (the burden is much tougher in the States).

In other words, forget about defending the science—use legal bullying tactics instead.

Kevin Trenberth:

> I am sure you know that this is not about the science. It is an attack to undermine the science in some way. In that regard I don't think you can ignore it all, as Mike suggests as one option, but the response should try to somehow label these guys as lazy and incompetent and unable to do the huge amount of work it takes to construct such a database. Indeed technology and data handling capabilities have evolved and not everything was saved. So my feeble suggestion is to indeed cast aspersions on their motives and throw in some counter-rhetoric. Labeling them as lazy with nothing better to do seems like a good thing to do.

In other words, only the people who constructed the adjusted data sets should have the right to see them; everyone else should obtain their own data! How absurd.

> How about "I tried to get some data from McIntyre from his 1990 paper, but I was unable because he doesn't have such a paper because he has not done any constructive work!"

We were here first; no one else has the right to question us.

> There is no basis for retracting a paper given in Keenan's message. One may have to offer a correction that a particular sentence was not correct if it claimed something that indeed was not so. But some old instrumental data are like paleoclimatology data, and can only be used with caution as information about the data does not exist. It doesn't mean they are worthless and cannot be used. Offering to make a correction to a few words in a paper in a trivial manner will undermine his case.

Again, the old data was so poorly documented that no information about what it refers to survives.

April 25, 2007: **email 1177534709**

Phil Jones to Ben Santer, regarding the data that Keenan wishes to see:

> Possibly I'll get the raw data from the Global Historical Climatology Network and do some work to replace our adjusted data with these, then make the

> Raw data (i.e. as transmitted by the National Meteorological Services). This will annoy them more, so may inflame the situation.

Again, Jones does not have the raw data, only his adjusted data, and is looking for ways of reconstructing the raw data from other sources.

Ben Santer:

> I looked at some of the stuff on the Climate Audit web site. I'd really like to talk to a few of these "Auditors" in a dark alley.

June 19, 2007: **email 1177890796**

Keith Briffa writes to Mike Mann, about the latest Intergovernmental Panel on Climate Change (IPCC) Report:

> I tried hard to balance the needs of the science and the IPCC, which were not always the same. I worried that you might think I gave the impression of not supporting you well enough while trying to report on the issues and uncertainties.

Such is Mann's overbearing and intolerant demeanor, that Briffa feels the need to apologize for actually examining the uncertainties in a scientific manner!

June 19, 2007: **email 1182255717**

Wei-Chyung Wang to Doug Keenan, regarding the missing data:

> Ms. Zeng told me when I was in Beijing in April 2007 that she no longer has … access to this data because it has been a long time (since 1990) and also because the Institute of Atmospheric Physics of the Chinese Academy of Sciences has moved office. But if you are interested, you can make an inquiry to the China Meteorological Administration …

More run-around—more missing data.

Phil Jones to Wei-Chyung Wang and Tom Karl:

> 1. I think I've managed to persuade the University of East Anglia to ignore all further Freedom Of Information Act requests if the people have anything to do with Climate Audit.
>
> 2. I had an email from David Jones of the Bureau of Meteorology Research Centre, Melbourne, Australia. He said they are ignoring anybody who has dealings with Climate Audit, as there are threads on it about Australian sites.

Freedom of Information evasion has begun in earnest, particularly appertaining to Doug Keenan and Steve McIntyre.

June 20, 2007: **email 1182342470**

Phil Jones to Wei-Chyung Wang and Tom Karl:

> I won't be replying to either of the emails below from Doug Keenan and Steve McIntyre, nor to any of the accusations on the Climate Audit website.
>
> I've sent them on to someone here at the University of East Anglia to see if we should be discussing anything with our legal staff.
>
> The second letter seems an attempt to be nice to me, and somehow split up the original author team.
>
> I do now wish I'd never sent them the data after their Freedom Of Information Act request!

Phil Jones to Tom Peterson:

> There are a few interesting comments on the Climate Audit web site. One says it is up to me to prove the paper from 1990 was correct, not for Keenan to prove we're wrong. Interesting logic.

No, it's scientific integrity.

June 20, 2007: **email 1182346299**

Kevin Trenberth to Phil Jones:

> It is nasty. It is also very inappropriate. Even were some problems to emerge over time, those should be addressed in a new paper by these guys. Unfortunately all they do is criticise.

Oh, no—not criticism!

June 20, 2007: **email 1182361058**

Eugene Wahl writes to Phil Jones, unable to bear the pressure of independent scrutiny:

> I was wondering if there is any way we as the scientific community can seek some kind of "cease and desist" action with these people. They are making all kinds of claims, all over the community, and we act in relatively disempowered ways. Note that the University Corporation for Atmospheric Research did send the response letter to the presidents of the two academic institutions with which McIntyre and McKitrick are associated, although this seems to have had no impact. Seeking the help of the attorneys you speak about would be useful, I should think. I know that Mike has said he looked into slander action with the attorneys with whom he spoke, but they said it is hard to do since Mike is, in effect, a "public" person—and to do so would take a lot of his time (assuming that the legal time could somehow be supported financially). If I might ask, if you do get legal advice, could you inquire into the possibility of acting proactively in response via the British system? Maybe the "public" person situation does not hold there, or less so. I only ask you to consider this question on my part; obviously, please do what you deem best for your situation.

The law is indeed the last refuge of the scoundrel.

August 29, 2007: **email 1188412866**

Benny Peiser, guest editor of Energy and Environment, sends a copy of the Keenan paper alleging the scientific fraud of Wei-Chyung Wang to Phil Jones for review. Jones forwards it to Kevin Trenberth and Mike Mann:

> The Appendix of this attachment has gone to the State University of New York at Albany and is being dealt with by them. Not sure when, but Wei-Chyung has nothing to worry about.

Again, an abuse of the peer review process.

August 30, 2007: **email 1188478901**

Phil Jones to Kevin Trenberth and Mike Mann:

> I've been in touch with Wei-Chyung Wang, who's in China at the moment. He forwarded the "paper!" to the people dealing with Keenan' allegations at the State University of New York. He got a reply to say that Keenan has now violated the confidentiality agreement related to the allegation. So, it isn't right to respond whilst this is ongoing.

Confidentiality is obviously something that only the "contrarians" must abide by.

Mike Mann replies, copying in Kevin Trenberth:

> I did take the liberty of discussing this with Gavin, who can of course be trusted to maintain the confidentiality of this. We're in agreement that Keenan has wandered his way into dangerous territory here, and that in its current form this is clearly libellous; there is not even a pretense that he is only investigating the evidence. Furthermore, while many of us fall under the category of "limited public figures" and therefore the threshold for proving libel is quite high, this is not the case for Wei-Chyung. He is not a public figure. I believe they have made a major miscalculation here in treating him as if he is. In the United Kingdom, where Energy and Environment is published, the threshold is even lower than it is in the United States for proving libel. We both think he should seek legal advice on this, as soon as possible.
>
> With respect to Peiser's guest editing of Energy and Environment and your review, following up on Kevin's suggestions, we think there are two key points. First, if there are factual errors (other than the fraud allegation) it is very important that you point them out now. If not, Keenan could later allege that he made the claims in good faith, as he provided you an opportunity to respond and you did not. Secondly, we think you need to also focus on the legal implications. In particular, you should mention that the publisher of a libel is also liable for damages—that might make Sonja Boehmer-Christiansen be a little wary. Of course, if it does get published, maybe the resulting settlement would shut down Energy and Environment and Benny and Sonja all together! We can only hope, anyway. So maybe in an odd way it's actually win-win for us, not them. Let's see how this plays out…

The legal standover strategy has extended to threatening the viability of entire journals.

> August 30, 2007: **email 1188508827**

Phil Jones to Wei-Chyung Wang and Tom Karl:

> I just received this. I won't be responding.
>
> Knowing this journal, there is no point, not even if I said I ought to review the paper. Peiser is a well-known skeptic in the UK. Not sure what to do. I guess you (Wei-Chyung Wang) should forward this to whoever needs to see it at Albany.
>
> If you think I should respond then I can. I will forward this to someone here, but mainly for their file.
>
> I did say the quote on page 3 about two to three years ago. I am still not releasing the Climatic Research Unit … data collected over … the last 25 years.

Wei-Chyung Wang:

> I have forwarded the file to the Vice-President of Research and she wrote back to me that Keenan has violated the confidentiality agreement, as I told her in the very beginning. In any case, I am letting the university … handle this. Send me whatever you have and I will forward it to Sunya. Keenan does not follow… any rules at all; reasoning with him is useless, but this will come back to badly hurt him.

The Editor of Energy and Environment writes to Phil Jones, clarifying Jones's query about its review:

> The paper has been sent to three reviewers. Of course I will take your comments and assessment into consideration. Indeed, if the claims are unsubstantiated, I would certainly reject the paper.

Phil Jones to Wei-Chyung Wang:

> I won't do anything then until the State University of New York (SUNY) process has run its course. Can you clarify what you mean by violated confidentiality? I presume you mean that Keenan agreed to do nothing on the issue until the SUNY process has run its course. I presume this will conclude sometime this autumn. Keep me informed of when the final decision might be, as after this we ought to do something about the paper in Energy and Environment. I checked with their guest editor and got this amazing reply! See above. So, if we didn't already think this was the worst journal in the world, now we know for certain it is, and have clear information from them to prove it.
>
> When I mean doing something, I don't mean sending anything to Energy and Environment, as that will be useless. Our blog site is a possibility, but there are other avenues.

Wang:

> The confidentiality agreement means that Keenan needs to keep the "inquiry" confidential during the process of the SUNY Albany "inquiry".

Jones:

1. Libel is quite easy to prove in the United Kingdom as you're not a public figure. Perhaps when you're back you ought to consider taking some legal advice from the SUNY. Assuming the paper is published, that is.

2. More important. I think I should send a short email to the editor Peiser and inform him that Keenan has broken his agreement with the SUNY over this issue. If I don't, they could say I had the chance and didn't. Can you check with the SUNY whether the folks there think I should?

Wang:

We should be thinking, after the whole ordeal is over, to take legal (or other) actions against Keenan. This is a time I regret not been a rich person, otherwise I could throw a million dollar lawsuit against him.

Let me know what you want to do. I have also asked the SUNY at Albany's opinion about what you should do within the SUNY framework. But be careful that you do not know much about the SUNY action.

Instead of intimidatory law suits, did they ever consider defending the science?

August 31, 2007: **email 1188557698**

Phil Jones to Tom Wigley:

Tom, Just for interest! Keep quiet about both issues.

I have been in touch with Wei-Chyung Wang. I just agreed with him that I will send a brief response to Peiser. The allegation by Keenan has gone to the State University of New York (SUNY). Keenan's about to be told by SUNY that submitting this has violated a confidentiality agreement he entered into with SUNY when he sent the complaint. Wei-Chyung Wang has nothing to worry about, but it is still unsettling!

All related to a paper in Nature from 1990! Keenan ought to look at the temperature data (which he has) rather than going on and on about site moves. See the end of this email and the response about Energy and Environment and the three reviewers. Amazing! We all knew the journal was awful.

On something completely different—just agreed to review another crappy paper by Chappell and Agnew on Sahel Rainfall. Chappell is out of a job—and still he tries to write papers saying the Sahel drought might not have happened! Both are just time wasters—but the review is necessary to do unfortunately.

Tom Wigley's remarkable reply:

It seems to me that Keenan has a valid point. The statements in the papers that he quotes seem to be incorrect statements, and that someone (Wei-Chyung Wang at the very least) must have known at the time that they were incorrect.

Whether or not this makes a difference is not the issue here.

Wigley, again, realizes that they have all missed the point: Keenan's allegations are actually valid!

September 11, 2007: **email 1189515774**

Phil Jones to Mike Mann and Gavin Schmidt:

> Don't pass this on; it's just for interest. It seems as though Energy and Environment will likely publish this paper. …
>
> … The fraud allegation against you, Mike, is only in passing!
>
> Wei-Chyung is in Vienna. Have forwarded this to him to pass onto the State University of New York. I wish they would conclude their assessment of malpractice.
>
> …
>
> PS to Gavin—been following (sporadically) the Climate Audit stuff about the Goddard Institute for Space Studies data and release of the computer programs, etc., by Jim. I may take some of the pressure off you soon, by releasing a list of the stations we use—just a list, no programs and no data. I have agreed to do this under the Freedom Of Information Act here in the United Kingdom.

Again, Jones is doing the bare minimum possible.

Mike Mann:

> It may be difficult for me to sue them over a footnote, and in fact he is very careful only to intimate accusations against me in a response to your comments. Note that he does not do so in the paper. I'm sure they know that I would sue them for that, and that I have a top lawyer already representing me.
>
> Wei Chyung needs to sue them, or at the least threaten a lawsuit. If he doesn't, this will set a dangerous new precedent. I could put him in touch with a leading attorney who would do this free of charge. Of course, this has to be done quickly. The threat of a lawsuit alone may prevent them from publishing this paper, so time is of the essence. Please feel free to mention this directly to Wei Chyung, in particular that I think he needs to pursue a legal course here … independent of whatever his university is doing. He cannot wait for Stony Brook to complete its internal investigations! If he does so, it will be too late to stop this.

September 11, 2007: **email 1189536059**

Phil Jones to Jacquie Burgess and Michael McGarvie:

> I've been in discussion with Michael over the past several months about a number of Freedom of Information (FOI) requests for Climatic Research Unit data. …

Michael McGarvie:

> I would like to suggest that we ask Dave Palmer to comment on the events on the Freedom Of Information Act request—I don't think I fully agree with the story presented here. Do you agree?
>
> I also think we should alert the Press Office in due course.

Jacquie Burgess:

> I will keep your email and hope we don't have to mobilise. This is very close to harassment, isn't it?

December 4, 2007: **email 1196872660**

Despite the growing controversy, Mike Mann feels the need to find an award for his mate, Phil Jones. This episode—and its hilarious aftermath—offers amazing insights into the characters of these two ring-leaders; but it also demonstrates the fallacy of relying on fancy-sounding awards and memberships to denigrate the criticisms of all those who are not "in the club". First, the menu:

> By the way, I am still looking into nominating you for an American Geophysical Union award; I've been told that the Ewing medal wouldn't be the right one. Let me know if you have any particular options you'd like me to investigate…

Jones selects his own award:

> As for the American Geophysical Union — just getting one of their Fellowships would be fine. I take it you've seen the attached in Energy and Environment.

Mann:

> I will look into the American Geophysical Union Fellowship situation as soon as possible. I don't read Energy and Environment; it gives me indigestion—I don't even consider it peer-reviewed science, and in my view we should treat it that way, i.e., don't cite anything appearing in it, and if journalists ask us about a paper, simply explain that it's not peer-reviewed science, and Sonja Boehmer-Christiansen, the editor, has even admitted to an anti-Kyoto agenda!

Mann has his own definition of what peer-review is!

> I do hope that Wei-Chyung pursues legal action here.

January 10, 2008: **email 1199999668**

Phil Jones to Ben Santer:

> Tim has let me into part of the secret. Glenn said the paper had two reviews—one positive; the other said it wasn't great, but would leave it up to the editor's discretion. This is why Glenn knows he made the wrong choice.
>
> The problem! The person who said they would leave it to the editor's discretion is on your email list! I don't know who it is—Tim does—maybe they have told you? I don't want to put pressure on Tim. He doesn't know I'm sending this. The second reviewer isn't me, by the way—nor Tim! Tim said it was someone who hasn't contributed to the discussion—which does narrow the possibilities down!

This is a deplorable corruption of the peer-review system; "peer group pressure" is a more accurate term.

March 26, 2008: **email 1206549942**

Mike Mann writes to Chris Folland, Phil Jones, and Tom Karl:

> Just wanted to give you a heads-up (warning) on something. Have you seen this?
>
> (link to graphic on the Met(eorological) Office site)
>
> Apparently the contrarians are having a field day with this graph. My understanding that it is based on using only January and February 2008 and padding filling the remaining values with that final value.
>
> Surely this can't be?? Is (skeptic) Fred Singer now running the United Kingdom Met(eorological) Office website?
>
> I would appreciate any info you can provide.

David Parker of the United Kingdom Met(eorological) Office responds, including John Kennedy on the cc list:

> Yes, it was based on only January and February 2008 and padding with that final value, but John Kennedy has changed / shortly will change this misleading plot!

This episode will be continued shortly.

March 27, 2008: **email 1206628118**

We are at the point where Climategate starts to get slightly self-referential: the noose is tightening, and the main characters begin to worry about past actions, statements, and emails.

In this episode, Jonathan Overpeck has been sent an email about a quote attributed to him "getting rid" of the Medieval Warm Period. Overpeck writes to his colleagues for advice. Phil Jones responds, copying in Mike Mann, and Susan Solomon:

> The person who sent you this is likely far worse. This is David Holland. He is a United Kingdom citizen who send countless letters to his Member of Parliament in the United Kingdom, writes in Energy and Environment about the biased Intergovernmental Panel on Climate Change (IPCC), and has also been hassling John Mitchell about his role as Review Editor for Chapter 6 of the IPCC Report. You might want to talk to John about how he's responding. Holland has been making requests under our Freedom Of Information Act about the letters Review Editors sent when signing off. I'm sure Susan is aware of this. He's also made requests for similar letters regarding Working Group 2, and maybe Working Group 3. Keith has been in contact with John about this.

It is good to see Jones hard at work gathering intelligence! But it is a pity he didn't take as much effort documenting his data as he did his opponents.

April 2, 2008: **email 1207158227**

Chris Folland of the Met(eorological) Office writes to Mike Mann, Phil Jones, Tom Karl, and Richard Reynolds, regarding the incorrect temperature graph on the Met(eorological) Office website:

> First, thanks very much, Mike, for noticing this and preventing greater problems. The error arose from a pre-existing hidden software bug that the person updating the data had not realised was there. The software is a mixture of languages which makes it less than transparent. The bug is now fixed on all the smoothed graphs. It was made worse because the last point was not an average of several preceding years as it should have been but was just January 2008. So many apologies for any excitement this may have created in the hearts of the more ardent sceptics. Some are much on the warpath at present over the lack of recent global warming, fired in some cases by visions of a new solar Dalton Minimum.

It is remarkable that, as recently as 2008, the Met(eorological) Office's quality management processes are in the same parlous state as that of the researchers—their computer programs a mess of different languages, full of bugs and hacks.

May 9, 2008: **email 1210341221**

Phil Jones writes to Mike Mann, Ray Bradley, and Caspar Ammann:

> A couple of things—don't pass on either.
>
> …
>
> 2. You can delete this attachment if you want. Keep this quiet also, but this is the person who is putting in Freedom Of Information requests for all the emails that Keith and Tim have written and received regarding Chapter 6 of the Intergovernmental Panel on Climate Change Report. We think we've found a way around this.

Intelligence work again—and yet something else to "hide behind"!

But wait—am I being too melodramatic? Let's see how Jones ends his email:

> This message will self destruct in 10 seconds!

No, Jones understands precisely what he is doing.

May 21, 2008: **email 1211462932**

Mike Mann continues in his quest to control all publications that relate to climate science. He writes to Phil Jones:

> Gavin and I have been discussing: we think it will be important for us to do something on the Thompson and coworkers paper as soon as it appears, since it's likely that naysayers are going to do their best to put a contrarian slant on this in the blogosphere. Would you mind giving us an advance copy? We promise to fully respect Nature's embargo (i.e., we wouldn't post any article until the paper goes

public), and we don't expect to in any way be critical of the paper. We simply want to do our best to help make sure that the right message is emphasized.

Thanks in advance for any help!

May 27, 2008: **email 1211911286**

David Douglass, Professor of Physics at the University of Rochester, makes a reasonable request to Ben Santer:

> In a recent paper by Peter Thorne in Nature Geoscience he references a paper that you and he (and others) have written.
>
> I cannot understand some parts of the Thorne paper without reading the Santer and Thorne reference.
>
> Would you please send me a copy?

Santer's reply introduces his astounding arrogance and pettiness:

> Dr. Douglass:
>
> I assume that you are referring to the Santer and coworkers paper which has been submitted to the International Journal of Climatology (IJoc). Despite your claims to the contrary, the Santer and coworkers IJoC paper is not essential reading material in order to understand the arguments advanced by Peter Thorne (in his "News and Views" piece on the Allen and Sherwood Nature Geosciences article).
>
> I note that you did not have the professional courtesy to provide me with any advance information about your 2007 IJoC paper, which was basically a commentary on previously-published work by myself and my colleagues. Neither I nor any of the authors of those previously-published works (the 2005 Santer and coworkers Science paper and the 2006 Karl and coworkers Climate Change Science Program Report) had the opportunity to review your 2007 IJoC paper prior to its publication—presumably because you specifically requested that we should be excluded from consideration as possible reviewers.
>
> I see no conceivable reason why I should now send you an advance copy of my IJoC paper. Collegiality is not a one-way street, Professor Douglass.

May 27, 2008: **email 1211924186**

Tim Osborn writes to Casper Ammann, copying in Keith Briffa and Phil Jones:

> Our university has received a request, under the United Kingdom Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the Intergovernmental Panel on Climate Change (IPCC) assessment process.
>
> We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.

> However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.
>
> Sorry to bother you with this.

Osborn's tactics in evading the Freedom of Information request are multi-layered. Firstly, he is hoping that the University of East Anglia will block the request outright. Secondly, he is giving an invitation to Ammann to declare that any emails that he did send did not relate to the IPCC process. Thirdly, he is hopeful that any emails not covered by such a denial were not "retained" (demonstrating his naivety with regard to email archiving processes). Fourthly, he is inviting Ammann to declare any emails that slip through the first three filters to be "confidential", which he is obviously hoping will be a sufficient excuse to prevent them from being released.

Ammann responds:

> Oh man! Will this crap ever end?
>
> Well, I will have to properly answer in a couple days when I get a chance digging through emails. I don't recall from the top of my head any specifics about IPCC.
>
> I'm also sorry that you guys have to go through this bullshit.

We will come to Osborn's response to this email shortly.

May 27, 2008: **email 1212009215**

Related to the above email exchange, the following email from the University of East Anglia's Dave Palmer (a librarian allocated to dealing with Freedom of Information requests) to Phil Jones, Tim Osborn, Keith Briffa, and Michael McGarvie clearly precedes Osborn's email to Ammann:

> Please note the response received today from Mr. Holland regarding his Freedom of Information request. Could you provide input as to his additional questions 1, and 2, and check with Mr. Ammann in question 3 as to whether he believes his correspondence with us to be confidential?

Palmer now raises what the others would see as a potential loophole:

> Although I fear/anticipate the response, I believe that I should inform the requester that his request will be over the appropriate limit and ask him to limit it …
>
> (lists guidelines)
>
> In effect, we have to help the requester phrase the request in such a way as to bring it within the appropriate limit …

In other words, it is not a loophole: they must do everything in their power to assist Holland to adjust his request to fall within the allowable limits.

Palmer clearly understands the gravity of the situation:

> I just wish to ensure that we do as much as possible "by the book" in this instance as I am certain that this will end up in an appeal, with the statutory potential to end up with the Information Commissioner's Office.

Tim Osborn begins the process of "divide and conquer":

> These follow-up questions appear directed more towards Keith than to me. But Keith may be unavailable for a few days due to family illness, so I'll attempt a brief response in case Keith doesn't get a chance to.
>
> Items (1) and (2) concern requests that were made by the Intergovernmental Panel on Climate Change (IPCC) Technical Support Unit (hosted by the University Corporation for Atmospheric Research in the United States) and any responses would have been sent direct to the IPCC Technical Support Unit, to the email address specified in the quote included in item (2). These requests are, therefore, irrelevant to the University of East Anglia.

So they can handball this one to the IPCC. (We will see shortly why this is a cunning tactic.)

> Item (3): we'll send the same enquiry to Ammann as we sent to our other colleagues, and let you know his response.

This prompted the email discussed above.

> Item (3) also asks for emails from "the journal Climatic Change that discuss any matters in relation to the IPCC assessment process". I can confirm that I have not received any such emails or other documents. I expect that a similar answer will hold for Keith, since I cannot imagine that the editor of a journal would be contacting us about the IPCC process.

Osborn here believes that simply asserting that none of the communications relate to the IPCC assessment process will suffice—ignoring completely the reality that these very scientists refer to the IPCC as "us", namely, that drawing an arbitrary distinction between their climate work and that of the IPCC is meaningless.

Phil Jones, the master of finding loopholes to hide behind, enters the fray:

> Although requests (1) and (2) are for the IPCC, so irrelevant to the University of East Anglia, Keith (or you Dave) could say that for (1) that Keith didn't get any additional comments in the drafts other than those supplied by the IPCC. On (2), Keith should say that he didn't get any papers through the IPCC process either. I was doing a different chapter from Keith and I didn't get any. What we did get were papers sent to us directly—so not through the IPCC, asking us to refer to them in the IPCC Chapters.

Jones is trying to argue that when they received papers, with explicit requests that the papers be referred to in the IPCC Report, then this was not part of the "IPCC process"! This is ludicrous.

Jones now laments the fact that Holland does not understand how the "old boys' club" works:

> If only Holland knew how the process really worked! Every faculty member in the School of Environmental Sciences at the University of East Anglia and all the post-doctoral research fellows and most PhD students do, but seemingly not Holland.

So Jones is confessing to indoctrinating the entire faculty!

He returns to the task at hand, telling everyone what they should say, even though he does not know whether it is correct or not, rather than actually asking anyone what the truth is:

> So the answers to both (1) and (2) should be directed to the IPCC, but Keith should say that he didn't get anything extra that wasn't in the IPCC comments.

Jones now proceeds to concoct answers on behalf of others:

> As for (3), Tim has asked Caspar Ammann, but Caspar is one of the worst responders to emails known. I doubt that either he emailed Keith or Keith emailed him related to the IPCC.

Ironically, we have already seen, above, that Ammann responded immediately!

Jones now tries to infer a motive for the requests:

> I think this will be quite easy to respond to once Keith is back. From looking at these questions and the Climate Audit web site, this all relates to two papers in the journal Climatic Change. I know how Keith and Tim got access to these papers and it was nothing to do with the IPCC.

So Jones is admitting that Briffa and Osborn had premature access to the papers in question, but that their method of doing so could not be explicitly traced to the IPCC process. A loophole!

May 27, 2008: **email 1212009927**

Ben Santer's arrogant and immature response to David Douglass's request for a paper that he referenced is coming back to bite him. He writes to many:

> Dear folks,
>
> I just wanted to alert you to an issue that has arisen in the last few days. As you probably know, a paper by Robert Allen and Steve Sherwood was published last week in Nature Geoscience. Peter Thorne was asked to asked to write a "News and Views" piece on the Allen and Sherwood paper. Peter's commentary on Allen and Sherwood briefly referenced our joint International Journal of Climatology (IJoC) paper. Peter discussed this with me about a month ago, and I saw no problem with including a reference to our IJoC paper. The reference in Peter's "News and Views" contribution is very general, and gives absolutely no information on the substance of our IJoC paper.

But it was cited as an authoritative reference nevertheless.

> At the time Peter I discussed this issue, I had high hopes that our IJoC manuscript would now be very close to publication. I saw no reason why publication of Peter's "News and Views" piece should cause us any concern. Now, however, it is obvious that David Douglass has read the "News and Views" piece and wants a

> copy of our IJoC paper in advance of its publication—in fact, before a final editorial decision on the paper has been reached. Dr. Douglass has written to me and to Peter, requesting a copy of our IJoC paper. In his letter to Peter, Dr. Douglass has claimed that failure to provide him (Douglass) with a copy of our IJoC paper would contravene the ethics policies of the journal Nature.

Rightly so: if one cites an unpublished paper, then a "preprint" (pre-publication draft) should always be provided on request. This is an absolutely standard procedure throughout science.

> As you can see from my reply to Dr. Douglass, I feel strongly that we should not give him an advance copy of our paper. However, I think we should resubmit our revised manuscript to IJoC as soon as possible.

This is remarkable: the paper is not even in the publication process! It was returned to the authors for revision. In other words, it was not accepted for publication at the time it was cited.

With proper caveats, such a premature citation is acceptable. However, under no circumstances would it be acceptable to then refuse to supply a preprint of the paper on request.

> The sooner we receive a final editorial decision on our paper, the less likely that it is that Dr. Douglass will be able to cause problems. With your permission, therefore, I'd like to resubmit our revised manuscript by no later than close of business tomorrow. I've incorporated most of the suggested changes I've received from you in the past few days. My personal feeling is that we've now reached the point of diminishing returns, and that it's more important to get the manuscript resubmitted than to engage in further iterations about relatively minor details. I will circulate a final version of the revised paper and the response to the reviewers later this evening. Please let me know if resubmission by Close Of Business tomorrow is not acceptable to you.

Santer is so desperate to hide the fact that the paper in question is not in publication that he wants to railroad his co-authors into resubmitting it immediately—before they have completed the process of properly correcting it.

Steven Sherwood responds:

> I wouldn't feel too threatened by the likes of Douglass. This paper will likely be accepted as is upon resubmission, given the reviews, so why not just send him a copy too once it is ready and final.

Tom Wigley disagrees:

> Sorry, but I agree with quick submission, but not with giving anything to Douglass until the paper appears in print.

Phil Jones:

> This is definitely the right response—so I agree with Tom. I have been known to disagree with him, and he's not always right. Submit as soon as possible!!

One would gather, from these recommendations, that the co-authors' confidence in their paper is rock-solid; that the science is rock-solid; that the paper was already ready to publish, aside perhaps from some minor cosmetic repairs. Right?

Read on.

May 28, 2008: **email 1212026314**

Ben Santer now drops a bombshell, to a large number of recipients:

> Dear folks,
>
> I just wanted to let you know that I did not submit our paper to IJoC. After some discussions that I've had with Tom Wigley and Peter Thorne, I applied our mathematical tests … The results are shown in the attached graph.

The rock-solid paper was not submitted? Why not?

> The worrying thing about the appended Figure is the behavior of one of the tests. This is the test which we thought Reviewers 1 and 2 were advocating. As you can see, the test produces unexpected results. We do not wish to be accused by Douglass and coworkers of devising a test that is unfair.

So the results are flawed.

> So the question is, did we misinterpret the intentions of the Reviewers?

Or the real questions are: Did the Reviewers understand the mathematics and statistics better than the authors? Did the authors even understand what was being told to them? Did they even listen?

> I will try to clarify this point tomorrow with Francis Zwiers (our Reviewer 2).
>
> …
>
> The bottom line here is that we need to clarify with Francis the exact form of the test he was requesting.

This lack of comprehension is astounding.

> I'm sorry about the delay in submission of our manuscript, but this is an important point, and I'd like to understand it fully. I'm still hopeful that we'll be able to submit the paper in the next few days. Many thanks to Tom and Peter for persuading me to pay attention to this issue. It often took a lot of persuasion…

Even Santer realizes that they will not be able to "bulldoze through" a paper that is patently wrong.

May 29, 2008: **email 1212063122**

Phil Jones writes to Mike Mann the email that will provide his prosecutors with their easiest conviction:

> Mike,

> Can you delete any emails you may have had with Keith regarding the latest Intergovernmental Panel on Climate Change report? Keith will do likewise. He's not in at the moment—minor family crisis.
>
> Can you also email Gene and get him to do the same? I don't have his new email address.
>
> We will be getting Caspar to do likewise.

So the primary co-conspirators in the Intergovernmental Panel on Climate Change are Phil Jones, Mike Mann, Keith Briffa, Eugene Wahl, and Caspar Ammann.

Mike Mann's response provides similar assistance to his prosecutors:

> I'll contact Gene about this as soon as possible.

May 29, 2008: **email 1212067640**

Peter Thorne of the Met(eorological) Office writes about the problematic International Journal of Climatology paper:

> We still need to be aware that this ignores two sources of uncertainty that will exist in the real world that are not included …
>
> …
>
> One approach, that I would advocate here because I'm lazy / because it's more intuitive* (* = delete as appropriate) is that we can (mathematical suggestion). However, the alternative approach would be to take the range of data set estimates, make the necessary poor-man's assumption that this is (one of two mathematical possibilities) depending upon how far you think they span the range of possible answers, and then incorporate this as an extra part of the equation. …
>
> Anyway, this is just a methodological quirk that logically follows if we are worried about ensuring universal applicability of the approach, which with the increasingly frequent use of the data for these types of applications is something we maybe should be considering. I don't expect us to spend very much time, if any, on this issue as I agree that key is submitting the paper as soon as possible.

Even though these scientists are being rapidly educated in the correct way to use mathematics and statistics to analyze their results, their most urgent goal is to get the cited paper back into the publication process—regardless of whether or not it is correct.

May 30, 2008: **email 1212156886**

Despite Phil Jones's assertion above that he does not respond to emails, Caspar Ammann writes to Tim Osborn, Keith Briffa, and Phil Jones, regarding Osborn's earlier hope that Ammann would consider his emails to be "confidential":

> In response to your inquiry about my take on the confidentiality of my email communications with you, Keith or Phil, I have to say that the intent of these emails is to reply or communicate with the individuals on the distribution list, and

they are not intended for general "publication". If I would consider my texts to potentially get wider dissemination then I would probably have written them in a different style. Having said that, as far as I can remember (and I haven't checked in the records, if they even still exist) I have never written an explicit statement on these messages that would label them strictly confidential.

Not sure if this is of any help, but it seems to me that it reflects our standard way of interaction in the scientific community.

Ammann's answers are exactly what one would expect any professional scientist to give, with regard to their workplace emails: they are written in the style of informal communication, rather than for publication, but there is nothing to hide.

May 30, 2008: **email 1212166714**

Tim Osborn replies to Caspar Ammann's previous response:

Hi again Caspar,

I don't think it is necessary for you to dig through any emails you may have sent us to determine your answer.

Our question is a more general one, which is whether you generally consider emails that you sent us to have been sent in confidence. If you do, then we will use this as a reason to decline the Freedom Of Information request.

Osborn is frustrated that Ammann is handling his first request honestly, promising to check his emails for anything that may be confidential. Osborn here makes explicit to Ammann that this is not what he wants, but rather a blanket statement that he can use as a loophole to hide behind.

June 2, 2008: **email 1212435868**

Mike Mann writes to Phil Jones, reporting his progress in nominating Jones for the award that Jones himself selected:

Hi Phil,

This is coming along nicely. I've got five very strong supporting letter writers lined up to support your American Geophysical Union Fellowship nomination (confidentially: Ben Santer, Tom Karl, Jean Jouzel, and Lonnie Thompson have all agreed; I'm waiting to hear back from one more individual; the maximum is six letters, including mine as nominator).

Meanwhile, if you can pass along the following information that is needed for the nomination package, that would be very helpful. Thanks in advance!

June 4, 2008: **email 1212587222**

Steve McIntyre writes to the Climatic Research Unit (CRU) at the University of East Anglia:

Dear Sir, Can you please send me a copy of the Farmer and coworkers 1989 paper, cited in Folland and Parker's paper of 1995, which, in turn, is cited in

> the Intergovernmental Panel on Climate Change Fourth Assessment Report. Thanks, Steve McIntyre

Phil Jones forwards the request to Mike Mann, and Gavin Schmidt of the Goddard Institute for Space Studies:

> This email came to the CRU last night.
>
> (quotes above email)
>
> The CRU has just the one copy of this paper! We've just got a new scanner for a project, so someone here is going to try this out—and scan the roughly 150 pages. I'm doing this as this is one of the project reports that I wished I'd written up.

Jones's admission is astounding: we learn that, as of June 2008, the CRU had no comprehensive electronic archive of its own reports; and he himself takes the blame for failing to write up a number of required project reports.

Mike Mann replies:

> It seems to me that the CRU should charge him a fee for the service. He shouldn't be under the assumption that he has the right to demand that reports be scanned in for him on a whim. The CRU should require reasonable monetary compensation for the labor, effort (and postage!).

Mann's stance is astonishing: McIntyre should pay for the labor of scanning in a report that should have been electronically archived decades earlier?

He continues:

> If this were a colleague acting in good faith, I'd say do it at no cost. But of, course, he's not. He's not interested in the truth here; he's just looking for another way to try to undermine confidence in our science.

So the real issue is not the labor involved, but the fact that this is yet another loophole to hide behind.

> I guess you're going to get your money's worth out of your scanner.

That Mann is unsurprised that the CRU is only now catching up with twenty-year-old technology is incredible.

June 8, 2008: **email 1212924720**

Mike Mann writes to Phil Jones, on the issue most dear to his heart:

> Hi Phil,
>
> I'm continuing to work on your nomination package to be awarded a Fellowship of the American Geophysical Union (here in my hotel room in Trieste—the weather isn't any good!). If it's possible for a case to be too strong, we may have that here! Lonnie is also confirmed as supporting letter writer, along with Kevin, Ben, Tom K, and Jean J. (Four of the five are already American Geophysical Union Fellows, which I'm told is important! Surprisingly, Ben is not yet, nor am I.

> But David Thompson is (quite young for one of these). I'm guessing that Mike Wallace and Susan Solomon might have had something to do with that(wink).

Jones should take the hint: Mann will be wanting the favor to be paid back!

> Anyway, I wanted to check with you on two things:
>
> 1. One thing that people sometimes like to know is the maximum value of "N", where "N" is the number of papers an individual authored or co-authored that have more than N citations. A level of N = 40 (i.e., an individual has published at least 40 papers that have each been cited at least 40 times) is supposedly an important threshold for admission in the United States National Academy of Sciences. I'm guessing your N is significantly greater than that, and it would be nice to cite that if possible. Would you mind figuring out that number and sending it to me—I think it would be useful in really sealing the case.

Mann is not wrong: such dubious measures of "worth" really are used for such purposes. Of course, in the corrupted field of climate science, such citations are not just of dubious value, but completely meaningless, as Mann and his colleagues had complete control over what was published (and hence cited) and what was not, and repeatedly cited each other's papers.

> 2. Would you mind considering a minor revision of your two-page bibliography? In my nomination letter, I'm trying to underscore the diverse areas where you've made major contributions … For example, your early Nature papers with Wigley … in 1980 and 1981 seem to be among the earliest efforts to try to do this (though I don't have copies of the papers, so can't read them!), and that seems very much worth highlighting to me.

Mann wants to highlight "contributions" of Jones that he himself has never read!

Or is that an incorrect interpretation of his words?

> Also, if you happen to have copies of the two early Wigley papers, or even just the text for the Abstracts, it would be great to have a little more detail about those papers so I can appropriately work them into the narrative of my letter.

No, it's not: he has no idea what is in the papers he wants to cite.

June 11, 2008: **email 1213201481**

Phil Jones replies to Mike Mann, on Mann's nomination of Jones:

> On point 1 (what Mann called "N"), this is what people call the H index. I've tried working this out, and there is software for it on the Web of Science website.
>
> The problem is my surname. I get a number of 62 if I just use the software, but I have too many papers. I then waded through and deleted those in journals I'd never heard of and got 52. I think this got rid of some biologist from the 1970s and 1980s, so go with 52.
>
> I don't have soft copies of the early papers. I won't be able to do anything for a few days either. When do you want this in, by the way?

Again, Jones reveals that there is no electronic archiving system at the Climatic Research Unit. Mike Mann:

> OK—thanks, I'll just go with the H = 62. That is an impressive number and almost certainly higher than the vast majority of American Geophysical Union Fellows.

Mann dishonestly ignores Jones's own disclaimer that the figure of 62 is completely wrong, and decides to use it regardless. Is there any greater insight into the absolute lack of integrity of this man?

In a later email:

> I'll … send you a copy of my nominating letter for comment and suggestions when I am done.
>
> Also — can you provide one or two sentences about the 1980 and 1981 Nature articles with Wigley so that I might be able to work this briefly into the narrative of my letter?

So he doesn't even feel the need to have a broad understanding of the papers, but will let Jones write his own accolades of himself. Jones replies:

> The 1980 and 1981 papers: I don't have soft copies.
>
> (summarises each paper in one paragraph)
>
> I did look a while ago to see if Nature had back-scanned these papers, but they hadn't.
>
> Is the above enough? I have hard copies of these two papers—in Norwich.

Note that Jones does not take the opportunity of asking Mann to use the correct figure of H = 52 rather than 62. Jones is implicitly going along with Mann's deception of the American Geophysical Union.

Mann:

> Thanks, Phil—yes, that's perfect. I just wanted to have some idea of the paper; that's more than enough information. I wouldn't bother worrying about scanning in, etc.
>
> I should have a draft letter for you to comment on within a few days or so, after I return from Trieste.

Mann assumes that Jones would have scanned in the papers, simply for the purpose of his own nomination for an award—but previously argued against scanning in a paper for the purposes of critical review by a skeptic. It is good to understand the priorities of these "scientists".

June 13, 2008: **email 1213387146**

Ben Santer writes to the Editor of American Liberty Publishers:

> Dear Sir,

> Your website (link) was recently brought to my attention. On this site, you make the following claims:
>
> In the Second Assessment Report, Benjamin Santer, lead author of a crucial study, falsified a chart to make it appear to support global warming—a conclusion not supported at all by the original data. But two climatologists, Knappenberger and Michaels, looked up the data and exposed the fraud. Santer said he adjusted the data to make it agree with political policy.
>
> These claims have no factual basis whatsoever, and are demonstrably libelous. I did not falsify data. I did not commit fraud. I did not—nor have I ever—"adjusted" scientific data "to make it agree with political policy." Nor did I ever state that I had made data adjustments in order to conform to political policy.
>
> I request that you retract these claims immediately. They are completely fictitious, and are harmful to my scientific reputation. If you do not retract these claims immediately, I will transfer this matter to the attention of legal staff at Lawrence Livermore National Laboratory.
>
> Sincerely,
>
> Dr. Benjamin Santer
> United States Department of Energy Distinguished Scientist (2006)
> Ernest Orlando Lawrence Award (2002)
> John D. and Catherine T. MacArthur Fellow (1998)

It is somewhat hilarious for Santer to be quoting his awards to add weight to his bullying tactics, at the same time that Mann and Jones are demonstrating that such awards are—at least within the small circle of climate science—completely meaningless.

June 14, 2008: **email 1213882741**

Mike Mann is still hard at work, getting Phil Jones his award. He writes to Jones:

> Hi Phil,
>
> I've attached a copy of my nomination letter. I just want to make sure I've got all my facts right—please let me know if there is anything I've gotten wrong or should be changed. I would be shocked is this doesn't go through—you're a no-brainer, and long overdue for this.
>
> I've got letters from three of the five other letter writers now; I am waiting on the two last ones, and then will submit the package.

Jones replies:

> This is fine. …
>
> Another thanks for putting this all together.

Then Jones sends an addendum:

> Mike,

> There is one typo in your nomination letter. I missed it the first time I read it. In the second paragraph, second line, remove the first "surface". You have two, one before and one after "CRU". Just the one after is needed.

Hilariously, Jones is correcting typos in his own nomination letter—but presumably letting the false citation number of H = 62 stand!!

Mann:

> Thanks Phil—fixed!
>
> I am waiting on two more letters, then I'll send in the package to the American Geophysical Union. Should be a no-brainer!

June 21, 2008: **email 1214228874**

Brian Lynch writes to Caspar Ammann, regarding the loopholes used to avoid the Freedom Of Information (FOI) request for the Intergovernmental Panel on Climate Change (IPCC) emails:

> Subject: IPCC FOI Request
>
> Dear Sir,
>
> I have read correspondence on the web about your letter … in relation to expert comments on IPCC's Chapter 6 sent directly by you to Keith Briffa, … outside the formal review process.
>
> The refusal to provide these documents tends to discredit you and the IPCC in the eyes of the public.
>
> Could I suggest that you make your letter and documents public. I would be very glad if you gave me a copy and oblige.

Ammann forwards the request to Keith Briffa, Tim Osborn, and Phil Jones. Jones writes:

> It doesn't discredit the IPCC!

Osborn writes:

> I've just had a quick look at Climate Audit. They seem to think that somehow it is an advantage to send material outside the formal review process. But anybody could have emailed us directly. It is in fact a disadvantage! If it is outside the formal process then we could simply ignore it, whereas formal comments had to be formally considered. Strange that they don't realise this and instead argue for some secret conspiracy that they are excluded from!

It is remarkable that he assumes that anything sent outside the formal process would be ignored, rather than actually considered! The implication is that even things that had to be formally considered would ultimately be rejected.

He continues:

> I'm not even sure if you sent me or Keith anything, despite McIntyre's conviction! But I'd ignore this guy's request anyway. If we aren't consistent in keeping our

> discussions out of the public domain, then it might be argued that none of them can be kept private. Apparently, consistency of our actions is important.

This is an intriguing comment, and perhaps suggests the possibility that whistle-blowing sentiments were already circulating in mid-2008.

Keith Briffa:

> I have been of the opinion, right from the start of these FOI requests, that our private, inter-collegial discussion is just that—private. Your communication with individual colleagues was on the same basis as that for any other person and it discredits the IPCC process not one iota not to reveal the details. On the contrary, submitting to these "demands" undermines the wider scientific expectation of personal confidentiality. It is for this reason, and not because we have or have not got anything to hide, that I believe none of us should submit to these "requests".

An interesting choice of words by Briffa: he argues that the refusals should be based on his opinion of the privacy of email communications—even work emails—"and not because we have or have not got anything to hide". In other words, he is not denying that they have things to hide.

June 21, 2008: **email 1214229243**

Phil Jones writes to Tim Osborn, Keith Briffa, and Caspar Ammann, about David Holland's Freedom of Information requests regarding Met(eorological) Office director John Mitchell's involvement as a Review Editor for the Intergovernmental Panel on Climate Change:

> This is a confidential email.
>
> Have a look at Climate Audit. Holland has put all the responses and letters up.
>
> There are three threads—two beginning with "Fortress" and a third later one.
>
> It is worth saving the comments on a Jim Edwards—can you do this, Tim?

Tim Osborn:

> So is this (confidential).
>
> I've saved all three threads as they now stand. I have no time to read all the comments, but I did note in the topic "Fortress Met(eorological) Office" that someone has provided a link to a website that helps you to submit Freedom Of Information requests to United Kingdom public institutions, and subsequently someone has made a further Freedom Of Information request to the Met(eorological) Office and someone else made one to the Department for Environment, Food and Rural Affairs. If it turns into an organised campaign designed more to inconvenience us than to obtain useful information, then we may be able to decline all related requests without spending ages on considering them. It is worth looking out for evidence of such an organised campaign.

The search for loopholes continues!

July 10, 2008: **email 1215712600**

Ben Santer writes to Phil Jones and others, regarding their response to the Douglass paper:

> Reviewer 2 was somewhat crankier. The good news is that the editor (Glenn McGregor) will not send the paper back to Reviewer 2, and is requesting only minor changes in response to the Reviewer's comments.
>
> Once again, Reviewer 2 gets hung up on the issue of fitting better mathematical models to the temperature data used in our paper. As noted in our response to the Reviewer, this is a relatively minor technical point.…
>
> The Reviewer does not want to "see the method proposed in this paper become established as the default method of estimating uncertainties in climatological results". We do not claim universal applicability of our approach. There may well be circumstances in which it is more appropriate to use the better models in estimating uncertainties. …
>
> I have to confess that I was a little ticked off by Reviewer 2's comments. The bit about "wilfully ignoring" expert mathematical literature was uncalled for. Together with my former Max Planck Institute colleague Wolfgang Brueggemann, I've fooled around with a lot of different methods of estimating uncertainties …. One could write a whole paper on this subject alone.

Phil Jones:

> From a quick scan below, Myles does seem to be a pain! As we both know he can be.

Ben Santer:

> Myles (if it is Myles) was a bit pedantic in his second review. Karl (who is a very-mild-mannered guy) described the tone of the review as "whining". It seems like the Reviewer was saying, "I'm a lot smarter than you, and I could do all of this stuff much better than you've done". I was very unhappy about the "wilfully ignoring" bit. That was completely uncalled for.

It sounds like Myles Allen (if it was Myles) is a lot smarter!

"Fooling around" with different methods is no substitute for actually understanding what you should be doing.

The continuation of Jones's technique of surreptitiously determining the identity of anonymous reviewers—and spreading this news widely, so that the "culprit" can be bastardized—is abhorrent.

August 20, 2008: **email 1219239172**

Phil Jones writes to Gavin Schmidt and Mike Mann:

> Keith and Tim are still getting Freedom Of Information (FOI) requests, as are the Meteorological Office Hadley Centre and the University of Reading. All our FOI officers have been in discussions and are now using the same exceptions not to respond—advice they got from the Information Commissioner.

Yet another loophole!

> As an aside, and just between us, it seems that Brian Hoskins has withdrawn himself from the Intergovernmental Panel on Climate Change (IPCC) Working Group 1 Lead nominations. It seems he doesn't want to have to deal with this hassle.

It is intriguing that outside scrutiny should cause such drastic changes of heart.

> The FOI line we're all using is this: The IPCC is exempt from any country's FOI Act—the skeptics have been told this. Even though we (the Meteorological Office Hadley Centre, the Climatic Research Unit and University of East Anglia) possibly hold relevant information, the IPCC is not part our remit (mission statement, aims etc.); therefore, we don't have an obligation to pass it on.

It is this obnoxious attempt at evading national laws that provides the strongest basis for charges of treason against the perpetrators.

October 26, 2008: **email 1225026120**

Mick Kelly writes to Phil Jones:

> Hi Phil
>
> I just updated my global temperature trend graph for a public talk, and noticed that the level has really been quite stable since 2000 or so, and 2008 doesn't look too hot.
>
> Anticipating the sceptics latching on to this soon, if they haven't done so already …
>
> It would be awkward if we went through another early-1940s-type swing!

Phil Jones:

> Mick, They have noticed for years—mostly with respect to the warm year of 1998. The recent coolish years we put down to La Nina. When I get this question I have 1991–2000 and 2001–2007/8 averages to hand. Last time I did this they were about 0.2 degrees different, which is what you'd expect.

Kelly:

> Yeah, it wasn't so much 1998 and all that that I was concerned about, I'm used to dealing with that, but the possibility that we might be going through a longer—10-year—period of relatively stable temperatures beyond what you might expect from La Nina, etc.
>
> This is speculation, but if I see this as a possibility then others might also. Anyway, I'll maybe cut the last few points off the graph before I give the talk again, as that's trending down as a result of the end effects and the recent cold-ish years.

In private, they admit that there could be significant cooling; in public, they hide it. Again: when the results don't fit your preconceptions, fraudulently alter them so that the public doesn't get the wrong idea!

October 31, 2008: **email 1225462391**

Steve McIntyre writes to Ben Santer:

> Dear Dr Santer,
>
> Could you please provide me either with the monthly model data … used for the statistical analysis in the Santer and coworkers 2008 paper, or a link to a website containing the data. I understand that your version has been collated from the Program for Climate Model Diagnosis and Intercomparison; my interest is in a file of the data as you used it (I presume that the monthly data used for statistics is about 1–2 megabytes).
>
> Thank you for your attention,
>
> Steve McIntyre

Ben Santer forwards this request to a large number of colleagues:

> Dear folks,
>
> While on travel in Hawaii, I received a request from Steven McIntyre for all of the model data used in our International Journal of Climatology paper (see forwarded email). After some conversation with my Program for Climate Model Diagnosis and Intercomparison colleagues, I have decided not to respond to McIntyre's request. If McIntyre repeats his request, I will provide him with the same answer that I gave to David Douglass …

November 10, 2008: **email 1226337052**

Following a second request by Steve McIntyre, Ben Santer writes his promised response, copying in his many colleagues:

> Dear Mr. McIntyre,
>
> I gather that your intent is to "audit" the findings of our recently-published paper in the International Journal of Climatology (IJoC). … You should have no problem in accessing exactly the same model and observational data sets that we employed. You will need to do a little work in order to calculate synthetic Microwave Sounding Unit (MSU) temperatures from climate model atmospheric temperature information. This should not pose any difficulties for you. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You will also need to calculate spatially-averaged temperature changes from the gridded model and observational data. Again, that should not be too taxing.
>
> In summary, you have access to all the raw information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound. I see no reason why I should do your work for you, and provide you with derived quantities (zonal means, synthetic MSU temperatures, etc.) which you can easily compute yourself.

Santer is placing as many impediments in McIntyre's way as possible.

> I am copying this email to all co-authors of the 2008 Santer and coworkers IJoC paper, as well as to Professor Glenn McGregor at IJoC. I gather that you have appointed yourself as an independent arbiter of the appropriate use of statistical tools in climate research.

Just in case McIntyre didn't get the message that he is black-balled.

> Please do not communicate with me in the future.

This last sentence seems to be a standard statement of Santer's. We will see it again.

November 11, 2008: **email 1226451442**

Tom Karl writes to Ben Santer, regarding Steve McIntyre's Freedom Of Information (FOI) request:

> FYI—Jolene can you set up a conference call with all the parties listed below, including Ben.

Ben Santer replies to Tom Karl, copying in many colleagues:

> My personal opinion is that both of McIntyre's FOI requests … are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc.

Santer is trying to offer the ludicrous excuse that he does not have time to prepare this material for release to McIntyre. In reality, the necessary work in carefully documenting and archiving the material should already have been done, as a routine part of his job.

> Quite frankly, Tom, … I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre.

Santer shares the view of his colleagues that their research is "personal" and immune from "intrusion". If so, it should never have been published in the professional scientific literature. Is review and replication of scientific research now a "frivolous" activity?

> I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation.

Sorry—doesn't that precisely describe Santer's diatribe to McIntyre, copied to an extensive list of colleagues? And where, exactly, are the threats and intimidation from McIntyre in his requests for information?

> We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh every word we write in every email we send to our scientific colleagues.

Again, their results should be taken on their say-so, without any chance of independent verification. This is not how science works.

> In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research. As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc. I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.

Again, "a playground bully" is a concise description of himself.

> I will be consulting Lawrence Livermore National Laboratory's Legal Affairs Office in order to determine how the Department Of Energy and Lawrence Livermore National Laboratory should respond to any Freedom Of Information requests that we receive from McIntyre. I assume that such requests will be forthcoming.

We will see shortly the results of this consultation.

> I am copying this email to all co-authors of our 2008 IJoC paper, to my immediate superior at the Program for Climate Model Diagnosis and Intercomparison (Dave Bader), to Anjuli Bamzai at the Department Of Energy headquarters, and to Professor Glenn McGregor (the editor who was in charge of our paper at IJoC).

That seems to be as intimidatory as it gets.

> I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's Climate Audit website.

Is Santer perhaps finally beginning to understand?

December 2, 2008: **email 1228249747**

Ben Santer's bluff has been called. He writes to many:

> Dear folks,
>
> There has been some additional fallout from the publication of our paper in the International Journal of Climatology. After reading Steven McIntyre's discussion of our paper on climateaudit.com (and reading about my failure to provide McIntyre with the data he requested), an official at Department Of Energy headquarters has written to Cherry Murray at Lawrence Livermore National Laboratory (LLNL), claiming that my behavior is bringing LLNL's good name into disrepute. Cherry is the Principal Associate Director for Science and Technology at LLNL, and reports to LLNL's Director (George Miller).

The Lawrence Livermore National Laboratory is a national scientific establishment established in the 1950s with a high reputation for science and engineering. At last, someone in an oversight position has realized that the climate scientists are running amok and bringing this prestigious organization into disrepute.

> I'm getting sick of this kind of stuff, and am tired of simply taking it on the chin.

I think the phrase is "glass-jawed".

> Accordingly, I have been trying to evaluate my options. I believe that one option is to write a letter to Nature, briefly outlining some of the events that have transpired subsequent to the publication of our IJoC paper. …
>
> …
>
> Since it was my decision not to provide McIntyre with derived data, I'm perfectly happy to be the sole author of such a letter to Nature.

Tom Wigley realizes that independent verification is a crucial part of the scientific process, and counsels Santer:

> I support you on this. However, there is more to be said than what you give below. For instance, it would be useful to note that, in principle, an audit scheme could be a good thing if done properly. But an audit must start at square one (your point). So, one can appear to applaud McIntyre at first, but then go on to note that his method of operation seems to be flawed.

Wigley then points out that they themselves have already provided their own form of "auditing":

> The issue of auditing is a tricky one. The auditors must, themselves, be able to demonstrate that they have no ulterior motives. One way to do this would be to audit papers on both sides of an issue. In other words, both us and Douglass should be audited together. In a sense, our paper is an audit of Douglass—and we found his work to be flawed. A second opinion on this already exists, through the refereeing of our paper. I suppose a third opinion from the likes of McIntyre might be of value in a controversial area like this. But then, is McIntyre the right person to do this? Is he unbiased? Does he have the right credentials (as a statistician)?

This is the ultimate example of the pot calling the kettle black: when superior statisticians criticize their results, they try to lock them out as not being part of their soft-science club.

December 3, 2008: **email 1228330629**

Ben Santer writes to Tom Wigley and others:

> … had I acceded to McIntyre's initial request for climate model data, I'm convinced (based on the past experiences of Mike Mann, Phil, and Gavin) that I would have spent years of my scientific career dealing with demands for further explanations, additional data, computer programs, etc. (Phil has been complying with Freedom Of Information (FOI) Act requests from McIntyre and his cronies for over two years).

Santer is admitting that his lack of proper documentation and archiving of his data and computer programs is so deficient that it would take years of work to rectify.

> For the remainder of my scientific career, I'd like to dictate my own research agenda. I don't want that agenda driven by the constant need to respond to Christy, Douglass, and Singer. And I certainly don't want to spend years of my life interacting with the likes of Steven McIntyre.

Santer no longer wants to work in science.

> I hope Lawrence Livermore National Laboratory management will provide me with their full support. If they do not, I'm fully prepared to seek employment elsewhere.

I am glad to hear it. Santer is completely unsuited to science research.

Phil Jones describes the con job that he has apparently successfully sold within his own institution:

> When the FOI requests began here, the FOI person said we had to abide by the requests. It took a couple of half-hour sessions—one at a computer screen, to convince them otherwise, showing them what Climate Audit was all about. Once they became aware of the types of people we were dealing with, everyone at the University of East Anglia (in the Registry (administration) and in the Environmental Sciences School — the Head of School and a few others) became very supportive. I've got to know the FOI person quite well, and the Chief Librarian—who deals with appeals. The Vice-Chancellor is also aware of what is going on—at least for one of the requests, but probably doesn't know the number we're dealing with. We are in double figures.

Jones must be extremely convincing, to get all of these officials to be complicit in flouting the law.

One of his tactics is to, perversely, use the sheer number of requests to argue against their validity:

> One issue is that these requests aren't that widely known within the School. So I don't know who else at the University of East Anglia may be getting them. The Climatic Research Unit is moving up the ladder of requests at the University of East Anglia though. We're away of requests going to others in the United Kingdom — Meteorological Office Hadley Centre, the University of Reading, the Department for Environment, Food and Rural Affairs, and Imperial College.

Jones now describes how he evaded the latest request:

> The inadvertent email I sent last month has led to a Data Protection Act request sent by a certain Canadian, saying that the email maligned his scientific credibility with his peers!
>
> If he pays 10 pounds (which he hasn't yet) I am supposed to go through my emails and he can get anything I've written about him. About 2 months ago I deleted loads of emails, so have very little—if anything at all.

More easy pickings for his prosecutors.

> In response to FOI and Environmental Information Regulations requests, we've put up some data—mainly paleoclimatology data. Each request generally leads to more—to explain what we've put up. Every time, so far, that hasn't led to anything being added by us—instead we just put up statements saying "Read what is in the papers and what is on the web site!" Tim Osborn sent one such response (via the FOI person) earlier this week. We've never sent computer programs … or manuals.

December 9, 2008: **email 1228922050**

Ben Santer catches onto Phil Jones's strategy of arguing that a greater number of requests implies lower credibility and validity, rather than the opposite:

> I had a quick question for you: What is the total number of Freedom Of Information (FOI) Act requests that you've received from Steven McIntyre?

Jones replies:

> I haven't got a reply from the FOI person here at the University of East Anglia. So I'm not entirely confident the numbers are correct. … I did get an email from the FOI person here early yesterday to tell me I shouldn't be deleting emails—unless this was "normal" deleting to keep emails manageable!

Unless Jones volunteered this information to their FOI officer, this intriguing email may indicate that the FOI officer—who will generally have system-level rights to read all emails, as part of their job—may have been monitoring Jones's emails, including the one above where he admitted deleting emails. This may lead to the identity of the Climategate whistle-blower, for those who continue to doubt their existence.

> McIntyre hasn't paid his 10 pounds, so nothing looks likely to happen regarding his Data Protection Act email.
>
> Anyway, requests have been of three types — observational data, paleo climatology data, and who made Intergovernmental Panel on Climate Change (IPCC) changes and why. … According to the FOI Commissioner's Office, the IPCC is an international organization, so is above any national FOI Act. Even if the University of East Anglia holds anything about the IPCC, we are not obliged to pass it on, unless it has anything to do with our core business—and it doesn't! I'm sounding like Sir Humphrey here!

At least Jones recognizes that his arguments are as ridiculous as the fictional public servant in the famous television series. The Climatic Research Unit's "core business" has nothing to do with the IPCC?

> Finally, I know that the Department for Environment, Food and Rural Affairs (DEFRA) receive Parliamentary Questions from Members of Parliament to answer. One of these two months ago was from a Conservative Member of Parliament asking how much money DEFRA has given to the Climatic Research Unit over the last five years. DEFRA replied that they don't give money

> — they award grants based on open competition. DEFRA's computer system also told them there were no awards to the Climatic Research Unit, as when we do get something it is written down as going to the University of East Anglia!

More loopholes! Sir Humphrey would indeed be proud.

> I've occasionally checked DEFRA responses to FOI requests — all from David Holland.

It is remarkable that Jones should have access to this level of information.

December 16, 2008: **email 1229468467**

Ben Santer writes to many:

> I just wanted to alert you to the fact that Steven McIntyre has now made a request to United States Department Of Energy (DOE) Headquarters under the Freedom of Information Act (FOIA). … I was made aware of the FOIA request earlier this morning.

McIntyre clearly realized that, if he went high enough, someone would take note of Santer's recalcitrant attitude.

> McIntyre's request eventually reached the United States DOE National Nuclear Security Administration (NNSA), Livermore Site Office. The requested records are to be provided to the "FOIA Point of Contact" (presumably at NNSA) by December 22, 2008.

In other words, he has been ordered to provide the data as requested—within six days.

> Over the past several weeks, I've had a number of discussions about the "FOIA issue" with the Program for Climate Model Diagnosis and Intercomparison (PCMDI)'s Director (Dave Bader), with other Lawrence Livermore National Laboratory (LLNL) colleagues, and with colleagues outside of the Lab. Based on these discussions, I have decided to "publish" all of the climate model data that we used in our International Journal of Climatology (IJoC) paper.

In other words, he didn't "decide" to provide the data; rather, his colleagues and superiors told him that his refusal to provide the data was wrong.

> This will involve putting these data sets through an internal "Review and Release" procedure, and then placing the data sets on the PCMDI's publicly-accessible website. The website will also provide information on how synthetic Microwave Sounding Unit (MSU) temperatures were calculated, anomaly definition, analysis periods, etc.

This procedure should already have been carried out for such internationally crucial data.

> After publication of the model data, we will inform the "FOIA Point of Contact" that the information requested by McIntyre is publicly available for genuine scientific research. Unfortunately, we cannot guard against intentional or unintentional misuse of these data sets by McIntyre or others.

It is neither Santer's responsibility nor prerogative to determine who uses the data, or for what purposes.

> I hope that "publication" of the synthetic MSU temperatures resolves this matter to the satisfaction of the NNSA, DOE Headquarters, and LLNL.

In other words, all of these levels of management told him that his refusal to provide **all** the data was wrong.

Tom Wigley replies, showing that his support is not of Santer, but of the principles of scientific accountability:

> This is a good idea. … To have these numbers on line would be of great benefit to the community. In other words, although prompted by McIntyre's request, you will actually be giving something to everyone.

January 5, 2009: **email 1231190304**

Phil Jones writes to Tim Johns, Chris Folland, and Doug Smith, regarding temperature predictions:

> I hope you're not right about the lack of warming lasting till about 2020. I'd rather hoped to see the earlier Met(eorological) Office press release with Doug's paper that said something like—"half the years to 2014 would exceed the warmest year currently on record, 1998"!
>
> Still a way to go before 2014.
>
> I seem to be getting an email a week from skeptics saying "where's the warming gone"? I know the warming is on the decades scale, but it would be nice to wear their smug grins away.

Jones's next complaint is simply hilarious:

> Chris—I presume the Meteorology Office continually monitor the weather forecasts. Maybe it's because I'm in my 50s, but the language used in the forecasts seems a bit over the top regarding the cold. Where I've been for the last 20 days (in Norfolk) it doesn't seem to have been as cold as the forecasts.

Jones wants the United Kingdom's national weather service to use more "warmist" language?

January 6, 2009: **email 1231257056**

Ben Santer writes to many:

> I am forwarding an email I received this morning from a Mr. Geoff Smith.
>
> The email concerns the climate model data used in our recently-published International Journal of Climatology (IJoC) paper. Mr. Smith has requested that I provide him with these climate model data sets. This request has been made to Dr. Anna Palmisano at Department Of Energy (DOE) Headquarters and to Dr. George Miller, the Director of Lawrence Livermore National Laboratory (LLNL).

Another request for data. One would think that, by now, Santer has his data in order.

> I have spent the last two months of my scientific career dealing with multiple requests for these model data sets under the United States Freedom of Information Act (FOIA). I have been able to do little or no productive research during this time. This is of deep concern to me.

Santer still seems unable to comprehend that proper documentation and archiving of data is a crucial part of his job.

He seems unwilling to learn from his last dressing-down:

> I would like a clear ruling from DOE lawyers—ideally from both the National Nuclear Security Administration and DOE Office of Science branches—on the legality of such data requests. They are troubling, for a number of reasons.
>
> 1. In my considered opinion, a very dangerous precedent is set if any derived quantity that we have calculated from primary data is subject to FOIA requests. At LLNL's Program for Climate Model Diagnosis and Intercomparison (PCMDI), we have devoted years of effort to the calculation of derived quantities from climate model output. … The intellectual investment in such calculations is substantial.

Santer wants "exclusive rights" to the publicly-funded data. Even if he had managed to secure such a lucrative arrangement, he should not have publicly published papers based on the data.

> 2. Mr. Smith asserts that "there is no valid intellectual property justification for withholding this data". I believe this argument is incorrect. The data used in our IJoC paper—and the other examples of derived data sets mentioned above—are integral components of both PCMDI's ongoing research, and of proposals we have submitted to funding agencies (DOE, National Oceanic and Atmospheric Administration (NOAA), and National Aeronautics and Space Administration).

So, this is about money.

> Can any competitor simply request such data sets via the United States FOIA, before we have completed full scientific analysis of these data sets?

Santer's characterization of independent researchers as "competitors" is disturbing. Climate scientists should not seek to profit from their research—particularly not when their public pronouncements on the issue are used to lobby for political policy changes.

> 3. There is a real danger that such FOIA requests could (and are already) being used as a tool for harassing scientists rather than for valid scientific discovery. Mr. McIntyre's FOIA requests to the DOE and the NOAA are but the latest in a series of such requests. In the past, Mr. McIntyre has targeted scientists at Penn State University, the United Kingdom's Climatic Research Unit, and the National Climatic Data Center in Asheville. Now he is focusing his attention on me. The common denominator is that Mr. McIntyre's attention is directed towards studies claiming to show evidence of large-scale surface warming, and/or a prominent

> human "fingerprint" in that warming. These serial FOIA requests interfere with our ability to do our job.

That would sound like a reasonable set of studies for McIntyre to target. Again, Santer misunderstands that "doing his job" properly in the first place would have obviated the need to clean up his mess at this late date.

> As many of you may know, I have decided to publicly release the data that were the subject of Mr. McIntyre's FOIA request … These data sets have been through internal review and release procedures, and will be published shortly on PCDMI's website, together with a technical document which describes how they were calculated. I agreed to this publication process primarily because I want to spend the next few years of my career doing research. I have no desire to be "taken out" as scientist, and to be involved in years of litigation.

Santer brought upon himself the years of prosecution and incarceration now facing him.

> If Mr. McIntyre's past performance is a guide to the future, further FOIA requests will follow. I would like to know that I have the full support of LLNL management and the United States Department of Energy in dealing with these unwarranted and intrusive requests.
>
> I do not intend to reply to Mr. Smith's email.

Santer has learnt nothing.

Stephen Schneider:

> I had a similar experience—but not FOIA since we at Climatic Change are a private institution—with Stephen McIntyre demanding that I have the Mann and coworkers cohort publish all their computer programs for papers published in Climatic Change. I put the question to the editorial board who debated it for weeks. The vast majority opinion was that scientists should give enough information on their data sources and methods so others who are scientifically capable can do their own brand of replication work, but that this does not extend to personal computer programs with all their undocumented parts, etc. It would be an odious requirement to have scientists document every line of code so outsiders could then just apply them instantly. Not only is this an intellectual property issue, but it would dramatically reduce our productivity since we are not in the business of producing software products for general consumption and have no resources to do so. The National Science Foundation, which funded the studies I published, concurred—so that ended that issue with Climatic Change at the time a few years ago.

This is a startling admission on the part of Schneider: that computer programs throughout climate science are so shoddily written and poorly documented—or even completely undocumented—that they do not even reach the minimal standards required of high school students. His allegation that his funding agency, the National Science Foundation, supported this stance is, no doubt, under investigation by that agency.

> This continuing pattern of harassment, as Ben rightly puts it in my opinion, in the name of due diligence is in my view an attempt to create a fishing expedition to

> find minor glitches or unexplained bits of computer programs—which exist in nearly all our kinds of complex work—and then assert that the entire result is thus suspect.

Again, this demonstrates a hopelessly amateurish attitude to computer programming. Glitches could indeed render the entire result false: that is why good documentation, verification, and replication are vital parts of science.

> Our best way to deal with this issue of replication is to have multiple independent author teams, with their own programs and data sets, publishing independent work on the same topics—like has been done on the "hockey stick". That is how credible scientific replication should proceed.

It is ironic that Schneider quotes the discredited "hockey stick" in support of his suggestion—which is one possible arm of a validation and verification process, but most certainly not a comprehensive blueprint.

> Let the lawyers figure this out, but be sure that, like Ben is doing now, you disclose the maximum reasonable amount of information so that competent scientists can do replication work, but short of publishing undocumented personalized programs etc. The end of the email Ben attached shows their intent — to discredit papers so they have no "evidentiary value in public policy"— what you resort to when you can't win the intellectual battle scientifically at the Intergovernmental Panel on Climate Change or the National Academy of Sciences.

The most disturbing aspect of this commentary is that Schneider completely understands the enormous public policy ramifications of this research—yet still expresses such remarkably naive sentiments. He still thinks enormous public policy decisions should be based on the results from undocumented and unchecked personal computer programs.

> Good luck with this, and expect more of it as we get closer to international climate policy actions. We are witnessing the "contrarian Battle of the Bulge" now, and expect that all weapons will be used.
>
> PS Please do not copy or forward this email.

The need for confidentiality is becoming more apparent to the co-conspirators. Do they sense a dissenter or a whistle-blower in the ranks?

January 29, 2009: **email 1233249393**

Phil Jones writes to Ben Santer about some delightfully unexpected news:

> I heard during the International Detection and Attribution Group meeting that I've been made an American Geophysical Union Fellow. I will likely have to go to Toronto to the Spring American Geophysical Union meeting to collect it. I hope I don't see a certain person (McIntyre) there! I have to get out of a keynote talk I'm due to give in Finland the same day!

It is remarkable that Mike Mann has not already booked Jones's flights and accommodation!

January 30, 2009: **email 1233326033**

Geoff Smith writes to Ben Santer:

Dear Dr. Santer,

I'm pleased to see that the requested data is now available on line. Thank you for your efforts to make these materials available.

My "dog in this fight" is good science and replicability. I note the following references:

The American Physical Society on line statement reads (in part):

"The success and credibility of science are anchored in the willingness of scientists to:

1. Expose their ideas and results to independent testing and replication by others. This requires the open exchange of data, procedures and materials.

2. Abandon or modify previously accepted conclusions when confronted with more complete or reliable experimental or observational evidence."

Also I note the National Academy of Sciences booklet "On Being a Scientist: Responsible Conduct in Research" (2nd edition) states "After publication, scientists expect that data and other research materials will be shared with qualified colleagues upon request. Indeed, a number of federal agencies, journals, and professional societies have established policies requiring the sharing of research materials. Sometimes these materials are too voluminous, unwieldy, or costly to share freely and quickly. But in those fields in which sharing is possible, a scientist who is unwilling to share research materials with qualified colleagues runs the risk of not being trusted or respected. In a profession where so much depends on interpersonal interactions, the professional isolation that can follow a loss of trust can damage a scientist's work." I know that the 3rd edition is expected soon, but I cannot imagine this position will be weakened. Indeed, with electronic storage of data increasing dramatically, I expect that most of the exceptions are likely to be dropped.

I understand that science is considered by some to be a "blood sport" and that there are serious rivalries and disputes. Nevertheless, the principles above are vital to the continuation of good science, wherever the results may lead.

Again, I thank you for making the data available, and I wish you success in your future research.

Kind regards,

Geoff Smith

I couldn't express it better myself.

Ben Santer's reply:

Dear Mr. Smith,

> Please do not lecture me on "good science and replicability". Mr. McIntyre had access to all of the primary model and observational data necessary to replicate our results. Full replication of our results would have required Mr. McIntyre to invest time and effort. He was unwilling to do that.

Santer is still laboring under the misunderstanding that his research remains "private".

> Mr. McIntyre could easily have examined the appropriateness of the Douglass and coworkers statistical test and our statistical test with randomly-generated data (as we did in our paper). Mr. McIntyre chose not to do that.

Santer's arrogance and narrow-mindedness extend to dictating that McIntyre must do only as Santer and his coworkers did. It does not seem to occur to him that the principles of statistics are not the exclusive domain of his small group of colleagues.

> He preferred to portray himself as a victim of evil Government-funded scientists. A good conspiracy theory always sells well.

Ironic, given that Tom Wigley has described themselves in precisely those terms.

> Mr. Smith, you chose to take the extreme step of writing to Lawrence Livermore National Laboratory and the Department Of Energy management to complain about my "unresponsiveness" and my failure to provide data to Mr. McIntyre.

Let us see if Santer has decided to become more "responsive".

> Your email to George Miller and Anna Palmisano was highly critical of my behavior in this matter. Your criticism was entirely unjustified, and damaging to my professional reputation. I therefore see no point in establishing a dialogue with you. Please do not communicate with me in the future. I do not give you permission to distribute this email or post it on Mr. McIntyre's blog.

Sadly, he has not.

Now where have we seen the phrase "Please do not communicate with me in the future" before? In his reply to Steve McIntyre on November 10, 2008. There's not much chance of "the open exchange of data, procedures and materials" with Ben Santer.

February 2, 2009: **email 1233586975**

Geoff Smith writes to Phil Jones, trying to clarify the situation:

> Dear Prof. Jones,
>
> (provides reference to the paper in question)
>
> As you are a co-author of the referenced paper, you may be interested to know of developments (in case you have not heard already).
>
> You will be aware that intermediate data … had been requested from the first author, Dr. Santer. A refusal has been posted online, but in the mean time the data is now available at (link).
>
> Perhaps you had this data already, but other co-authors have reportedly claimed (earlier) they did not have the data. A typical reported response to a Freedom Of

> Information Act (FOIA) request was, "I have examined my files and have no data from climate models used in the paper referred to, and no correspondence regarding said data."
>
> No one disputes Dr. Santer's claim that the "primary model data" is publicly available, but there is a strong case to be made that intermediate results, e.g., collation of such data and the relevant computer programs should be made available in studies such as this one, since there is an important possibility of errors in trying to replicate such a collation. The archiving of such intermediate results is required for econometrics journals, among others.
>
> It is further reported online that the posting of the data was not pursuant to an FOIA order, but posted voluntarily (although likely at the request of the funding agency, the Department of Energy, Office of Science). I hope other scientists will take this type of voluntary action. You may have heard that Professor Hardaker, the Chief Executive Officer of the Royal Meteorological Society, which publishes the International Journal of Climatology, has confirmed that the issue of data archiving will be on the agenda for the next meeting of the Society's Scientific Publishing Committee. There is a need for journals as well as funding agencies, and publishing scientists themselves, to establish and enforce good data and computer program archiving policies. A more precise definition of "recorded factual material commonly accepted in the scientific community as necessary to validate research findings" is probably overdue.
>
> I hope the Hadley Centre will take a lead in this issue. From time to time I'll look at the progress on archiving, but in the mean time, no reply is necessary.
>
> Kind regards,
>
> Geoff Smith

Jones writes to Ben Santer:

> Is this the Smith who has emailed? …
>
> I'm not on a Royal Meteorological Society committee at the moment, but I could try and contact Paul Hardaker if you think it might be useful. I possibly need to explain what is "raw" and what is "intermediate".
>
> I wasn't going to give this guy Smith the satisfaction of a reply!

Instead of improving their data and computer program archiving standards, Jones is only interested in influencing the committee that will revise the minimum requirements.

Santer replies:

> Yes, this is the same Geoff Smith who wrote to me. Do you know who he is? From his comments about the Royal Meteorological Society, he seems to be a Briton.
>
> …
>
> I think it would be useful to raise these issues with Paul Hardaker.

Agreement has been reached that the best way forward is to influence the Royal Meteorological Society.

March 19, 2009: **email 1237496573**

Phil Jones writes to Paul Hardaker, the Chief Executive Officer of the Royal Meteorological Society (RMS):

> This email came overnight — from Tom Peterson, who works at the National Climatic Data Center in Asheville, North Carolina.
>
> (link to article)
>
> "Phil Jones, the director of the Hadley Climate Center in the United Kingdom."
>
> We all know that this is not my job. The paper being referred to appeared in the Journal of Geophysical Research last year. The paper is (cites reference). The paper clearly states where I work — the Climatic Research Unit at the University of East Anglia. There is no mention of the Hadley Centre!
>
> There is also no about-face as stated on the web page.
>
> I am sending you this as it gives a good example of the sort of people you are dealing with when you might be considering changes to data policies at the RMS.

So the RMS should refrain from improving its policies because someone in the United States erroneously associated Jones with the wrong United Kingdom climate science institution? It is hardly surprising that people mistakenly confuse the two organizations, because they work closely together, as evidenced by the joint development of climatic data sets such as "HADCRUT3".

Bizarre.

> There are probably wider issues due to climate change becoming more mainstream in the more popular media that the RMS might like to consider. I just think you should be aware of some of the background. The Climatic Research Unit has had numerous Freedom Of Information requests since the beginning of 2007. The Met(eorological) Office, the University of Reading, the National Climatic Data Center and the Goddard Institute for Space Studies have had as well—many related to Intergovernmental Panel on Climate Change involvement. I know the world changes and the way we do things changes, but these requests and these sorts of simple mistakes should not have an influence on the way things have been adequately dealt with for over a century.

Ah, reminiscing to the "good old days" when the old boys' club wasn't accountable to anyone.

Ben Santer is still not going to let anyone have access to his data and methods:

> If the RMS is going to require authors to make all data available—raw data plus results from all intermediate calculations—I will not submit any further papers to RMS journals.

Phil Jones:

> I don't know whether they even had a meeting yet—but I did say I would send something to their Chief Executive.

Jones clearly believes that he holds some sway over Hardaker.

> I'm having a dispute with the new editor of Weather. I've complained about him to the RMS Chief Executive. If I don't get him to back down, I won't be sending any more papers to any RMS journals and I'll be resigning from the RMS.

The same tactics, yet again.

It is quite possible that Jones would have resigned from enough societies and black-balled enough journals that he would have effectively put himself into early retirement, even if Climategate hadn't ended his career eight months after this email!

May 4, 2009: **email 1241415427**

Tom Wigley writes to Phil Jones and Ben Santer, attaching an extensive report from the internet, "Climate science fraud at Albany University?" This continues the saga that we first encountered over two years previously (April 21, 2007) in which Doug Keenan had raised questions about a paper of Phil Jones and Wei-Chyung Wang from 1990. The report included the following comment:

> Wang had a co-worker in Britain. In Britain, the Freedom of Information Act requires that data from publicly-funded research be made available. I was able to get the data by requiring Wang's co-worker to release it, under British law. It was only then that I was able to confirm that Wang had committed fraud.

Wigley writes:

> You are the co-worker, so you must have done something like provide Keenan with the Department Of Energy report that shows that there are no station records for 49 of the 84 stations. I presume Keenan therefore thinks that it was not possible to select stations on the basis of …
>
> "… station histories: selected stations have relatively few, if any, changes in instrumentation, location, or observation times"
>
> (THIS IS ITEM "X")
>
> (Discusses two possibilities, both problematical for Wang)
>
> Now my views. (1) I have always thought Wei-Chyung Wang was a rather sloppy scientist. I therefore would not be surprised if he screwed up here. But ITEM X is in both the Wei-Chyung Wang and Jones and coworker papers—so where does it come from first? Were you taking Wei-Chyung Wang on trust?

Wigley has not only passed judgment on Wang—he furthermore fears that Jones and colleagues didn't even check the data that they used.

> (2) It also seems to me that the University at Albany has screwed up. To accept a complaint from Keenan and not refer directly to the complaint and the complainant in its report really is asking for trouble.

Such actions eclipse their allegations that Keenan breached a confidentiality agreement with the State University of New York; Wigley's damning observation reveals their "investigation" to be nothing more than a whitewash.

> (3) At the very start it seems this could have been easily dispatched. ITEM X really should have been …
>
> (modified version of ITEM X)
>
> … but this is not what the statement says.
>
> Why, why, why did you and Wei-Chyung Wang not simply say this right at the start? Perhaps it's not too late?

Wigley's lamentations suggest that this is simply wishful thinking.

> I realise that Keenan is just a trouble-maker and out to waste time, so I apologize for continuing to waste your time on this, Phil. However, I am concerned because all this happened under my watch as Director of the Climatic Research Unit and, although this is unlikely, the buck eventually should stop with me.

Despite labelling Keenan as a trouble-maker rather than a seeker after truth, Wigley is facing squarely up to the realization of this scandal—and accepting responsibility for it as their leader, despite being unaware of it at the time. This speaks volumes about his fundamental integrity. However, where is the concern that this fraudulent data has been used for 19 years, and where are the suggestions for dealing with the research that has used this false data for 19 years?

May 16, 2009: **email 1242749575**

Let us get now some further insight into the fundamental character of Mike Mann. He writes to Phil Jones:

> On a completely unrelated note, I was wondering if you, perhaps in tandem with some of the other usual suspects, might be interested in returning the favor (of being awarded a Fellowship of the American Geophysical Union) this year (wink)?

Now we know why he was so adamant about securing Jones's award!

> I've looked over the current list of American Geophysical Union Fellows, and it seems to me that there are quite a few who have gotten in (e.g. Kurt Cuffey, Amy Clement, and many others) who aren't as far along as me in their careers, so I think I ought to be a strong candidate.

If he does say so himself.

> Anyway, I don't want to pressure you in any way, but if you think you'd be willing to help organize, I would naturally be much obliged. Perhaps you could convince Ray or Malcolm to take the lead? The deadline looks as if it is again July 1 this year.
>
> I'm looking forward to catching up with you some time soon, probably at some exotic location of Henry's choosing (wink).

Does any remnant of doubt remain that awards in this field are absolutely and completely meaningless? Mann may as well pin a gold star on his own chest!

Jones understands the obligation:

> I'll email Ray and Malcolm. I'd be happy to contribute.

Mann:

> Thanks much, Phil.

Later, Jones sends an update:

> Mike,
>
> Have gotten replies—they're both happy to write supporting letters, but both are too busy to take it on this year. One suggested waiting till next year. Malcolm is supporting one other person this year. I'd be happy to do it next year, so I can pace it over a longer period. Malcolm also said that (skeptic Fred) Singer had an American Geophysical Union Fellowship!

But that would be impossible!

What with all the work that these fine fellows (and Fellows) were busy with—lining up to award each other in every conceivable combination, with all the paper-work involved—it is no surprise that that they didn't have enough time to properly document or archive their data or computer programs!

> Apart from my meetings, I have skeptics on my back—still; I can't seem to get rid of them. Also the new United Kingdom climate scenarios are giving government ministers the jitters, as they don't want to appear stupid when they introduce them (late June?).

So even government ministers realise that they will look stupid trying to forecast the climate in 50 and 100 years' time, when they can't even forecast next week's weather.

Mann:

> Thanks much, Phil,
>
> That sounds good. So why don't we wait until next round (June 2010) on this then. That will give everyone an opportunity to get their ducks in a row. Plus I'll have one more Nature and one more Science paper on my resume by then (more about that soon!). I'll be sure to send you a reminder sometime next May or so!

Well that's one onerous task that can be struck off the schedule!

> The contrarians' attacks certainly have not abated. The only hope is that they'll increasingly be ignored.

He hopes scientific skepticism will be ignored by the media and governments. Given that the enormously important Copenhagen summit was looming just seven months away, Mann's confidence in their ability to avoid scrutiny is astounding.

May 26, 2009: **email 1243369385**

Darrell Kaufman, of Northern Arizona University, to many:

> Co-authors:

> I just received the reviewers' comments and editor's decision on our Science manuscript (attached). …
>
> …
>
> (2) The reviewer suggested that, if we are concerned about (a standard issue in statistics), we should attempt a so-called "robust" regression procedure, such as median absolute deviation regression. Does anyone have experience with this?

You've got to be kidding! They don't even know what this is? Do any of these climate scientists understand statistics?

July 19, 2009: **email 1248902393.txt**

Phil Jones writes to Tom Peterson, of the National Climatic Data Center (NCDC) in the United States:

> I have a question for you. I'm going to write a small document for our web site to satisfy (probably the wrong word) the 50 or so Freedom Of Information / Environmental Information Regulations requests we've had over the weekend. I will put up the various agreements we have with Met(eorological) Services.

That he so nicely "hides behind".

> The question—I think you told me one time that you had a file containing all the data you couldn't release (i.e. it's not in the Global Historical Climatology Network). … Do you know off-hand how much data is in this category? Would the NCDC mind if I mentioned that you have such data—not the amount or locations or anything, just that there is some?

More restrictions to hide behind! And Jones is explicitly trying to not put specific bounds on which data is restricted in this way—so that he can apply the excuse to anything and everything!

Peterson replies:

> Data that we can't release is a tricky thing here at NCDC. Periodically, Tom Karl will twist my arm to release data that would violate agreements and therefore hurt us in the long run, so I would prefer that you don't specifically cite me or NCDC in this.

In other words, they do release data, against these very agreements that Jones wants to hide behind. Importantly, Peterson does not want to be dragged in to supporting Jones. Are cracks beginning to appear?

> But I can give you a good alternative. You can point to the Peterson–Manton article on regional climate change workshops. All those workshops resulted in data being provided to the author of the peer-reviewed paper with a strict promise that none of the data would be released. So far as far as I know, we have all lived up to that agreement—myself with the Caribbean data (so that is one example of data I have that are not released by NCDC), Lucie and Malcolm for South America, Enric for Central America, Xuebin for Middle Eastern data, Albert for south/central Asian data, John Ceasar for South-East Asia, Enric again for

> central Africa, etc. The point being that such agreements are common and are the only way that we have access to quantitative insights into climate change in many parts of the world. Many countries don't mind the release of derived data, but very much object to the release of actual data (which they might sell to potential users). Does that help?

Again, restrictions on access to data on the basis of financial profit rears its ugly head.

July 30, 2009: **email 1249007192**

Kevin Trenberth to Mike Mann and others, regarding their submission to the Journal of Geophysical Research:

> I think you should argue that it should be expedited for the reasons of interest by the press. The key question is who was the editor who handled the original, because this is an implicit criticism of that person. We may need to point this out, and ensure that someone else handles it.

Is there any journal left in their field that they are not threatening in one form or another? Should papers be published expeditiously because of press interest? Such an attitude is anathema to any semblance of worth left in the "peer review" process in this field.

Mike Mann:

> Folks, I was thinking exactly the same thing ... it does immediately call into suspicion the integrity of the review process.
>
> We probably need to take this directly to the Chief Editor at the Journal of Geophysical Research, asking that this not be handled by the editor who presided over the original paper, as this would represent a conflict of interest. If we are told that is not possible, then we would at least want the Chief Editor himself to closely monitor the handling of the paper.
>
> I too am happy to sign off at this point.

August 5, 2009: **email 1249503274**

The Journal of Geophysical Research's standard request:

> Please list the names of 5 experts who are knowledgeable in your area and could give an unbiased review of your work. Please do not list colleagues who are close associates, collaborators, or family members.

Phil Jones flouts the requirements, explicitly, by considering people that are close associates and collaborators:

> I agree with Kevin that Tom Karl has too much to do. Tom Wigley is semi-retired, and, like Mike Wallace, may not be responsive to requests from the Journal of Geophysical Research.
>
> We have Ben Santer in common! Dave Thompson is a good suggestion. I'd go for one of Tom Peterson or Dave Easterling.

> To get a spread, I'd go with three in the United States, one Australian, and one in Europe. So I suggest Neville Nicholls and David Parker.
>
> All of them know the sorts of things to say—about our Comment and the awful original, without any prompting.

To be "prompting" the reviewers of their Comment would, in itself, already be a serious violation of professional ethics; but to propose reviewers who already "know the sorts of things to say" is outright corruption.

September 3, 2009: **email 1252154659**

Nick McKay writes to many, including Darrell Kaufman and Jonathan Overpeck, over a criticism by Steve McIntyre that the conspirators had flipped a data set upside down when they used it:

> I haven't checked the original reference for its interpretation, but I checked our computer program and we did use the data in the orientation that McIntyre stated. He's also right that flipping the data to the correct orientation doesn't affect any of the conclusions. Actually, flipping it makes it fit in better with the 1900-year trend.

Wonderful!

> I've attached a plot of the original, and another with the data flipped.

The next day:

> The data was oriented in the reconstruction in the way that McIntyre said. I took a look at the original reference—the temperature proxy we looked at is X-ray density, which the author of the data interprets to be inversely related to temperature. We had higher values of X-ray density as warmer in the reconstruction, so it looks to me like we got it wrong, unless we decided to reinterpret the record, which I don't remember. Darrell, does this sound right to you?

Again, it is absolutely astounding that they did not meticulously document where the data came from, what assumptions were made, how it was interpreted and applied, and so on. They are relying on their memories!

In Darrell Kaufman's reply:

> Regarding the "upside down man", as Nick's plot shows, when flipped, the data has little impact on the overall graphs. Also, the data was not included in the calibration.

According to Nick McKay, these points were already made by McIntyre.

> Nonetheless, it's unfortunate that I flipped the … data. … I should have used the inverse of density as the temperature proxy. I probably got confused by the fact that the 20th century shows very high density values and I inadvertently equated that directly with temperature.

Is there any clearer sign that they were working towards predetermined conclusions? To decide which way to orient the data just by "how it looks for the 20th century" demonstrates an extremely poor methodology.

> This is new territory for me, but not acknowledging an error might come back to bite us. I suggest that we nip it in the bud and write a brief update showing the corrected composite (Nick's graph) and post it to our website. Do you all agree?

To Kaufman's credit, he acknowledges his mistake, and suggests a reasonable path of action to correct it. What is astounding is the behind-the-scenes anarchy.

Kaufman is also concerned that they have stretched the truth:

> McIntyre wrote to me to request the yearly data that we used to calculate the 10-year averages. The only "non-published" data are yearly data from the ice cores … We stated this in the footnote, but it does stretch our assertion that all of the data are available publicly. Bo (Vinther): How do you want to proceed? Should I forward the yearly data to McIntyre?

Again, an admirable admission—but one that further confirms that the difficulties faced by McIntyre and other skeptics were absolutely real, and not fabricated or frivolous.

> I'm also thinking that I should write to the authors of the data directly to apologize for inadvertently reversing their data.

Another good suggestion. Perhaps they should also write to Steve McIntyre, thanking him for pointing out the error?

September 5, 2009: **email 1252164302**

Jonathan Overpeck responds to Darrell Kaufman's email to the co-authors:

> Darrell and others—Please write all emails as though they will be made public.

Overpeck realizes that "the lid will be blown" imminently.

> I would not rush and I would not respond to any of them until the best strategy is developed—I don't want to waste anyone's time, including yours or McIntyre's. Since the graph in Science has an error, I think you do need to publish a correction in Science. … I don't think you have a choice here.

Overpeck is on the defensive: he understands the crisis and its ramifications.

> Kaufman's question about data is tricky. Giving McIntyre the data would be good, but only if it is yours to give. You can't give him data that you got from others and are not allowed to share. But it would be nice if he could have access to all the data that we used—that's the way science is supposed to work. See what Mike and Ray say…

Overpeck acknowledges "the way science is supposed to work", but he also knows the mantra of their leaders that the data is "private property", and that they should "hide behind" every agreement that they have signed with its providers.

> Be careful, very careful. But now you know why I advocated redoing all the analyses a few months ago—to make sure we got it all right. We knew we'd get this scrutiny.

Again, Overpeck senses the impending catastrophe. The mind-set is now to get it right—not because it is important to get it right, but simply because they know that others can now check it. Clearly, all previous work has never been thoroughly checked.

September 6, 2009: **email 1252233095**

Bo Vinther responds to Darrell Kaufman's email to the co-authors:

> I will suggest that we release the 1860–2000 section of the yearly ice core data, as these are the data that go into Figure 2 in the paper. Such a limited release I can permit immediately. Releasing everything is something different, and I can't see the need—as far as I remember we are not presenting or using the 1–1859 part of the data anywhere in the paper—or am I wrong?

Vinther clearly believes that data relating to the paper must be released—but only that much, no more.

Kaufman:

> Regarding the yearly data: You're correct that we only use 10-year averages throughout our calculations (Figure 2 shows yearly values, but they are not used in any calculation or conclusion). In his e-mail to me, McIntyre requested the yearly data that we say are not publicly available as a footnote to Table S1.
>
> Unless anyone has another suggestion, I will reply and send him the 10-year data (which is already posted at the National Oceanic and Atmospheric Administration Paleoclimate website) and explain that they were the basis for all of the calculations. He might want the annual data that the averages were based on. I suppose we'll cross that bridge when we get to it.

Kaufman is now proposing that McIntyre should only be given the data that would allow him to replicate some of their calculations, not the data he requested—and would not allow him to replicate their Figure 2 at all. Such hair-splitting is a farce.

September 28, 2009: **email 1254147614**

Tom Wigley writes to Phil Jones:

> Here are some speculations on correcting sea temperatures to partly explain the 1940s warming blip.
>
> If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know). So, if we could reduce the ocean blip by, say, 0.15 degrees Celsius, then this would be significant for the global average—but we'd still have to explain the land blip.
>
> I've chosen 0.15 degrees Celsius here deliberately. This still leaves an ocean blip, and I think one needs to have some form of ocean blip to explain the land blip …

> It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip?"

Why not just leave the data alone, and not try to fudge it to support a preconceived conclusion? Why not actually explain the blip?

September 29, 2009: **email 1254230232**

Phil Jones writes to Tim Osborn, Mike Mann, and Gavin Schmidt, regarding McIntyre's analysis of Keith Briffa's Russian tree-ring results:

> I totally agree that these attacks (for want of a better word) are getting worse. Comments on the thread are snide in the extreme, with many saying they see no need to submit the results to a journal. They have proved Keith has manipulated the data, so job done.

Given the control that Jones and his colleagues had over the journals (and their stated intention to regard as essentially nonexistent those that publish skeptical papers), perhaps it is more than reasonable to say "job done" once they have proved that Briffa manipulated the data!

September 29, 2009: **email 1254258663**

Mike Mann, responding to Andy Revkin of the New York Times:

> So, even if there were a problem with Briffa's data, it wouldn't matter as far as the key conclusions regarding past warmth are concerned. But I don't think there is any problem with these data, rather it appears that McIntyre has greatly distorted the actual information content of these data. It will take folks a few days to get to the bottom of this, in Keith's absence.

We return to the familiar tune: even if this result is wrong, there are other results that say the same thing. In some contexts, such an argument would hold weight. But we know that, in this field, Mann and his colleagues tortured all data that came their way until it appeared to support their predetermined conclusions; and, moreover, McIntyre almost had to perform a miracle to squeeze enough data and computer programs out of the gang to show that there was a problem. In such an environment—and with the insight we now have into their methods and practices—the inescapable conclusion is that all of their results are suspect.

> If McIntyre had a legitimate point, he would submit a comment to the journal in question. Of course, the last time he tried that (with our 1998 article in Nature), his comment was rejected. For all of the noise and bluster about … Antarctic warming, it's now nearing a year and nothing has been submitted. So it's more likely he won't submit a paper for peer-reviewed scrutiny, or if he does get his criticism "published" it will be in the discredited contrarian home journal Energy and Environment. I'm sure you are aware that McIntyre and his ilk realize they no longer need to get their crap published in legitimate journals.

The peer-review Catch-22 is now all too familiar to us.

> And based on what? Some guy with no credentials, dubious connections with the energy industry, and who hasn't submitted his claims to the scrutiny of peer review.
>
> Fortunately, the prestige press doesn't fall for this sort of stuff, right?

Compliments get you everywhere, in this business.

> I'm sure you're aware that you will see dozens of bogus, manufactured distortions of the science in the weeks leading up to the vote on Cap and Trade legislation in the United States Senate.

He will if Mann and colleagues get their many prognostications into the press!

September 29, 2009: **email 1254259645**

Andy Revkin of the New York Times writes to Mike Mann and Tim Osborn:

> Tom Crowley has sent me a direct challenge to McIntyre to start contributing to the reviewed literature or shut up. I'm going to post that soon.
>
> I just want to be sure that what is spliced below is from you … a little unclear?

It's remarkable that Mike Mann gets to check what's Fit to Print!

> I'm going to blog on this as it relates to the value of the peer-review process, and not on the merits of the McIntyre and coworkers' attacks.

I thought that these fellows didn't believe in blogs?

> Peer review, for all its imperfections, is where the herky-jerky process of knowledge building happens, would you agree?

Mann has shown that, in his field, it's certainly a herky-jerky process—but more of the empire-building nature, than knowledge-building.

Mann replies:

> Yep, what was written below is all me, but it was purely on background; please don't quote anything I said or attribute to me without checking specifically—thanks.

So he can put words in Revkin's mouth, but they aren't to be attributed to him without his permission. Remarkable.

> Regarding your point at the end—you've taken the words out of my mouth.

Yes.

> Skepticism is essential for the functioning of science. It yields an erratic path towards eventual truth.

Sounds fine so far.

> But legitimate scientific skepticism is exercised through formal scientific circles, in particular the peer review process. A necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through

> the legitimate scientific peer review process. Those such as McIntyre who operate almost entirely outside of this system are not to be trusted.

Ironically, Mann's "delegitimization" of the peer-review process will be his most devastating legacy.

September 30, 2009: **email 1254323180**

Phil Jones writes to Mike Mann and Gavin Schmidt about the Briffa Russian tree ring controversy:

> Another issue is science by blog sites—and the then immediate response mode. Science ought to work through the peer-review system.

Again—unless it's Revkin's blog site, or their own website.

> Even though I've had loads of Freedom Of Information requests and nasty emails, a few in the last two days have been the worst yet. I'm realizing more what those working on animal experiments must have gone through.

He's likening his work to experimentation on animals?

Mike Mann:

> It's part of the attack of the corporate-funded attack machine, i.e. it's a direct and highly intended outcome of a highly orchestrated, heavily-funded corporate attack campaign.

Mann and colleagues are the heavily-funded ones—who claim to "own" the climate data, no less.

> We saw it over the summer with the health insurance industry trying to defeat Obama's health plan, and we'll see it now as the United States Senate moves on to focus on the Cap and Trade bill that passed Congress this summer.

Isn't it great to see the author of the emblematic "hockey stick" treating the climate change debate as just another partisan political battle?

> It isn't coincidental that the original McIntyre and McKitrick Energy and Environment paper with associated press release came out the day before the United States Senate was considering the McCain–Lieberman Climate Bill in 2005.

What about Mann and colleagues' repeated rushing of papers into print barely hours before the deadline to appear in the Intergovernmental Panel on Climate Change Report?

> We're doing the best we can to expose this. I hope our website post goes some ways to exposing the campaign and pre-emptively deal with the continued onslaught we can expect over the next month.

Ah, pre-emptive strikes are fine, as long as you're the "good guys".

October 2, 2009: **email 1254505571**

Malcolm Hughes writes to Keith Briffa:

> What's going on? On 21st September I got an email from Tom Melvin (a colleague of Briffa's) that contained the following paragraph, among other more general discussion:
>
> "Keith has been complained at by Climate Audit for cherry-picking and not using your long Russian data set. Not used because we did not have the data. Please, could we have the data? We will make proper acknowledgement or coauthorship if we use the data."
>
> I replied pretty much straight away thus: "Hi Tom … The Russian data set is not yet available because it has not been published. …"
>
> So far, I have had no direct response to this email from Tom.
>
> This morning I get an email from Anders Moberg, telling me that you had asked him for the Russian data. …

In other words, Briffa has been unable to get the data from Hughes—a colleague.

> Once again, the actual data are unpublished, in spite of having been discussed in the Russian literature by Siderova and coworkers. A large proportion of the raw data are not yet in the public domain, and so you would not be able to critically evaluate the data as a possible climate proxy.

So the data has already been discussed in the literature, but is not available even to Briffa (let alone skeptics).

> As you know, it is my intention to be friendly, cooperative and open, but I'm determined to get some scientific value from all the years of work I've invested in the Russian … work, and in cooperation with Russia in general. Releasing these data now would be too much.

Hughes wants to have the exclusive right to exploit the Russian data. That would be fine—if he didn't publicly publish papers based on that "private" data.

In his reply, Keith Briffa admits that he didn't actually understand where the data sets were coming from:

> I fully accept and would never go behind your back to ask for the data. … I could do without all this now—I don't really understand what Climate Audit are getting so hysterical about, but I feel that I cannot ignore it this time—but I don't feel up to getting involved. I fully admit to being out of the loop as regards all this and having trouble getting back to it.
>
> …—to be honest also—I actually was not really aware that the data you were producing and that used by Sidorova were one and the same.

This is unbelievable! The conspirators are so possessive of their own data that even their own colleagues couldn't work out whose data were from where. How could anyone be expected to peer-review the work? Can anyone have any faith in any of the published papers?

October 5, 2009: **email 1254746802**

Phil Jones to Mike Mann and Gavin Schmidt:

> I assume you are both aware of this prat—Neil Craig, see below. Keith won't be responding.

Mike Mann:

> I never acknowledge emails from people I don't know, about topics that are in any way sensitive. This is a perfect example of something that goes right into the trash bin.

October 5, 2009: **email 1254756944**

Tom Wigley writes to Phil Jones, over the growing controversy surrounding Keith Briffa's research:

> … Keith does seem to have got himself into a mess. …
>
> But, more generally, … how does Keith explain the McIntyre graph that compares data sets? And how does he explain the apparent "selection" of the less well-replicated data rather that the later (better replicated)data? Of course, I don't know how often some of the data has really been used in recent, post-1995, work. I suspect from what you say it is much less often that McIntyre and McKitrick say—but where did they get their information? I presume they went through papers to see if the data was cited, a pretty foolproof method if you ask me. Perhaps these things can be explained clearly and concisely—but I am not sure Keith is able to do this as he is too close to the issue and probably quite pissed off.

Wigley's criticisms of Briffa have unquestioned credibility: he still supports the man, just not his science.

> And the issue of withholding data is still a hot potato, one that affects both you and Keith (and Mann). Yes, there are reasons—but many good scientists appear to be unsympathetic to these. The trouble here is that withholding data looks like hiding something, and hiding means (in some eyes) that it is bogus science that is being hidden.
>
> I think Keith needs to be very, very careful in how he handles this.

Wigley comprehends that their "reasons" are merely excuses, and that refusing transparency is fundamentally indefensible.

October 6, 2009: **email 1254832684**

Martin Lutyens, of the British CO2morrow project, writes to the Climatic Research Unit's Andrew Manning, the scientific consultant to CO2morrow:

> I just came across an article in The Week, called "The case of the vanishing data". It writes in a rather wry and sceptical way about your University of East Anglia colleagues Phil Jones and Tom Wigley, saying that only their "homogenised" or

> "adjusted" historical data is available, and the original, raw data has gone missing. Apparently some other environmental gurus now want to look at the original data and were "fobbed off".
>
> According to the article, the adjusted data forms the basis for much of the climate change debate and, because others now want to look at the source data, it is "at the centre of an academic spat that could have major implications for the climate change debate". The author of the original article is Patrick Michaels in The National Review, who may just be stirring it.
>
> The article concludes, "In short, the data invoked to verify the most significant forecasts about the world's future, have simply vanished." Could you comment on this please, as someone (e.g. Siemens Corporation) may pick this up and I think we should all be forearmed by knowing what really happened and what to say if asked.

The reality of the good ship Global Warming hitting an iceberg has started to sink in, and the crew are looking for the lifeboats. We are here just six days away from the Climategate whistle-blower leaking the first tranche of emails to Paul Hudson of the British Broadcasting Corporation.

Manning forwards the query to Phil Jones:

> Is this another witch hunt …? How should I respond to the email below? (I'm in the process of trying to persuade Siemens Corporation (a company with half a million employees in 190 countries!) to donate me a little cash to do some carbon dioxide measurements here in the United Kingdom—it is looking promising, so the last thing I need is news articles calling into question (again) observed temperature increases—I thought we'd moved the debate beyond this, but it seems that these sceptics are real die-hards!).

Phil Jones:

> McIntyre has no interest in publishing his results in the peer-review literature. The Intergovernmental Panel on Climate Change won't be able to assess any of it unless he does.

What? Jones seems to think that even exposing the missing data is something that has to run through the gauntlet of their "peer review".

> Your dad (Martin Manning, Director of Climate Change at the New Zealand Climate Change Research Institute) and Susan Solomon have had run-ins with him and others.

So this is now a generational feud?

> So other groups around the world have also entered into agreements restricting access to data. I know this doesn't make it right, but it is the way of the world with both instrumental and paleoclimatology data. I frequently try and get data from other people without success, sometimes from people who send me a soft copy of their paper, then tell me they can't send me the data that generated their plots.

At last, Jones admits that all this "hiding" is not right—but he tries to deflect the blame onto others.

> It is the right-wing web sites doing all this, presumably in the build up to Copenhagen.

As with Mann in the United States, so with Jones in the United Kingdom: it is all a partisan political battle.

October 8, 2009: **email 1255095172**

Rick Piltz, founder of Climate Science Watch in the United States, writes to Ben Santer, copying in Tom Wigley, Tom Karl, Jim Hansen, Bob Watson, Mike MacCracken, and John Mitchell:

> Gentlemen—
>
> I expect that you have already been made aware of the petition to the Environmental Protection Agency(EPA) from the Competitive Enterprise Institute (CEI) (and Pat Michaels) calling for a re-opening of public comment on EPA's prospective "endangerment" finding on greenhouse gases. CEI is charging that the Climatic Research Unit at the University of East Anglia has destroyed the raw data for a portion of the global temperature record, thus destroying the integrity of the Intergovernmental Panel on Climate Change(IPCC) assessments and any other work that treats the United Kingdom Jones–Wigley global temperature data record as scientifically legitimate. I have attached the petition in soft copy, with the statements by CEI and Michaels.
>
> The story was reported in Environment & Energy Daily yesterday (below). They called me for it, presumably because I am on their call list as someone who gets in the face of the global warming disinformation campaign, among other things. I hit the CEI, but I don't have a technical response to their allegations.

So he attacked the CEI in lieu of having any valid response? And it would seem the EPA will also have no valid response:

> Who is responding to this charge on behalf of the science community? Surely someone will have to, if only because the EPA will need to know exactly what to say. And really, I believe that all of you, as the authoritative experts, should be prepared to do that in a way that has some collective coherence.
>
> I am going to be writing about this on my Climate Science Watch Website as soon as I think I can do so appropriately. I am most interested in what you have to say to set the record straight and put things in perspective—either on or off the record, whichever you wish. Will someone please explain this to me?

Santer responds:

> First, there was no intentional destruction of the primary source data. I am sure that, over 20 years ago, Phil Jones could not have foreseen that the raw temperature data might be the subject of legal proceedings by the Competitive Enterprise Institute and Pat Michaels.

Just as Santer would not have predicted receiving an email from Rick Piltz on October 8, 2009. But they most definitely should have known that they could be asked to justify their claims, at any time—especially given the extreme public policy implications.

> The critics are applying impossible legal standards to science.

Santer supports critical legislation being enacted on the basis of the science, but denies the applicability of legal standards? His naivety and ignorance is unbelievable.

> They are essentially claiming that if we do not retain—and make available to self-appointed auditors—every piece of information about every scientific paper we have ever published, we are perpetrating some vast deception on the American public.

I support that claim: All information should be archived.

> I think most ordinary citizens understand that few among us have preserved every bank statement and every utility bill we've received in the last 20 years.

Santer's comparing what is arguably the most important scientific data in the history of mankind to the electricity bill of a laborer in outback Wyoming must go down as one of the more hilarious and ridiculous arguments of the Climategate perpetrators.

> Michaels should and does know better. I can only conclude from his behavior—and from his participation in this legal action—that he is being intentionally dishonest. His intervention seems to be timed to influence opinion in the run-up to the Copenhagen meeting, and to garner publicity for himself.

Remember: only the "good guys" are permitted to influence opinion or receive publicity.

> In my personal opinion, Michaels should be kicked out of the American Meteorological Society, the University of Virginia, and the scientific community as a whole.

Santer's childish over-reaction, yet again. Why not simply deport him from planet Earth altogether?

> He cannot on the one hand engage in vicious public attacks on the reputations of individual scientists (in the past he has attacked Tom Karl, Tom Wigley, Jim Hansen, Mike Mann, myself, and numerous others), and on the other hand expect to be treated as a valued member of our professional societies.

You're either for us, or against us. "Our" professional societies are for our team only.

> The sad thing here is that Phil Jones is one of the true gentlemen of our field. I have known Phil for most of my scientific career.

And now we all know Phil.

> They deserve medals as big as soup plates—not the kind of crap they are receiving from Pat Michaels and the CEI.

Are American Geophysical Union Fellows awarded soup plates? Maybe that should be added to the privileges of the award.

October 9, 2009: **email 1255095172**

Ben Santer to Phil Jones:

> I'm really sorry that you have to go through all this stuff, Phil. Next time I see Pat Michaels at a scientific meeting, I'll be tempted to beat the crap out of him. Very tempted.

Wonderful.

October 11, 2009: **email 1255298593**

Phil Jones to Rick Piltz and Ben Santer:

> The original raw data are not lost either. I could reconstruct what we had from some United States Department of Energy reports we published in the mid-1980s. I would start with the Global Historical Climatology Network data.

Jones is admitting that he does not have the raw data, and that he would need to work backwards to "reconstruct" it—or some of it, at any rate—from printed reports. This is patently unacceptable: there is clearly no way of verifying that this "reconstruction" is correct!

> I know that the effort would be a complete waste of time, though. I may get around to it some time. As you've said, the documentation of what we've done is all in the literature.

Even as Climategate is about to break, Phil Jones still believes that the world will accept explanations of what they did, rather than the actual data itself.

October 11, 2009: **email 1255352257**

Narasimha Rao, a Ph.D. student at Stanford University in the United States, writes to Stanford's Stephen Schneider:

> Steve,
>
> You may be aware of this already. Paul Hudson, the British Broadcasting Corporation (BBC)'s reporter on climate change, on Friday (October 9) wrote that there's been no warming since 1998, and that Pacific oscillations will force cooling for the next 20–30 years. It is not outrageously biased in presentation as are other skeptics' views.
>
> (includes links)
>
> The BBC has significant influence on public opinion outside the United States.
>
> Do you think this merits an opinion–editorial response in the BBC from a scientist?

Intriguingly, Hudson claims that he received the first tranche of emails from the Climategate whistle-blower on October 12.

Schneider sends the email on to many of his colleagues. Mike Mann responds:

> It is extremely disappointing to see something like this appear on the BBC. It's particularly odd, since climate is usually Richard Black's beat at the BBC (and he does a great job). From what I can tell, this guy (Hudson) was formerly a weather person at the Met(eorological) Office.

It seems that their "man on the ground" at the BBC (Richard Black, an environmental correspondent) has been displaced by a person with a scientific background who worked for the Met(eorological) Office for ten years.

Usually, we would expect a chorus of agreement with Mann. But something has changed. Kevin Trenberth, of the University Corporation for Atmospheric Research:

> Well I have my own article on "where the heck is global warming?" We are asking that here in Boulder where we have broken records the past two days for the coldest days on record. …
>
> The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't. The … data published in the August … 2009 supplement on 2008 shows there should be even more warming: but the data are surely wrong. Our observing system is inadequate.

The belief system of these scientists is undergoing crisis. For decades, they have predicted catastrophic, accelerated warming—but someone forgot to tell the Earth about it.

Rather than draw the obvious conclusions—that their predictions are wrong; that the models that their predictions come from are inadequate—they instead start to question the measured temperatures themselves!

It is not clear whether Trenberth realizes that, if true, his assertions would absolutely destroy climate science, not save it; for the measured temperature data is the very best and most direct data that we have (albeit almost impossibly intractable to analyze); and if he throws out all of that data, then all that remains is a hopelessly anemic and ragtag collection of rotting tree stumps and melting ice tubes, without any hope at all of calibrating these souvenirs against real-world temperature measurements.

October 14, 2009: **email 1255523796**

Kevin Trenberth, responding to Tom Wigley's criticism of his comments, is beginning to sound like a skeptic:

> How come you do not agree with a statement that says we are nowhere close to knowing where energy is going or whether clouds are changing to make the planet brighter?

The most fundamental law of physics is that energy cannot be created or destroyed; it can only be transformed or transferred. If their climate models do not even satisfy this elementary law, then it is questionable whether they are useful for anything at all.

> We are not close to balancing the energy budget. The fact that we cannot account for what is happening in the climate system makes any consideration of geoengineering quite hopeless, as we will never be able to tell if it is successful or not! It is a travesty!

Mike Mann responds:

> Kevin, that's an interesting point. … But this raises the interesting question: is there something going on here with the energy and radiation budget which is inconsistent with the … models? I'm not sure that this has been addressed—has it?

Rather than dispute Trenberth's remarkable statements, Mann acknowledges that there may be something fundamentally wrong with their climate models. Trenberth:

> Here are some of the issues as I see them:
>
> Saying it is natural variability is not an explanation. What are the physical processes? Where did the heat go? …

As a physicist, these are questions that I would have been asking thirty years ago—not stumbling across in October 2009. But I suppose that's the difference here: these guys are simply not physicists; and they ensured that any physicists who did wander into their field were suitably chased off.

Trenberth admits that there are three sets of vital data that are "wanting" before they can understand how the climate functions:

> But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with sky temperature data. That data is unfortunately wanting, and so too are the cloud data. The ocean data are also lacking, although some of that may be related to the ocean current changes, and burying heat at depth, where it is not picked up. If it is sequestered at depth then it comes back to haunt us later, and so we should know about it.

In other words, even the direct temperature measurements are indeed suspect—there are plausible reasons why they are giving an incomplete picture.

October 14, 2009: **email 1255532032**

Mike Mann responds to Kevin Trenberth:

> Thanks Kevin, yes, it's a matter of what question one is asking. To argue that the observed global average temperatures of the past decade falsify the model projections … , as the contrarians have been fond of claiming, is clearly wrong. But that doesn't mean we can explain exactly what's going on.

Mann is almost right, but his logic is slightly muddled. Not being able to "explain exactly what's going on" does invalidate their model projections, without any doubt. What it doesn't do is "prove" any opposing view, either.

The simple fact of the matter is that the incompetence of these "scientists"—covered up with decades of manipulation and "stacking the deck" of peer review—has left us with absolutely no idea whether the Earth's climate has been affected to any appreciable degree by mankind.

That is the real travesty.

October 14, 2009: **email 1255550975**

Tom Wigley weighs in:

> Kevin,
>
> I didn't mean to offend you. But what you said was "we can't account for the lack of warming at the moment". Now you say "we are nowhere close to knowing where energy is going". In my eyes these are two different things—the second relates to our level of understanding, and I agree that this is still lacking.

We are now debating how quickly the ship is sinking. But why didn't any of these scientists speak up when their paymasters said to the world, "the science is settled"?

October 27, 2009: **email 1256735067**

It is appropriate that Mike Mann's last words in the Climategate repository explain what it's all about. To Phil Jones and Gavin Schmidt:

> As we all know, this isn't about truth at all; it's about plausibly deniable accusations.

And again: it's tough when even your allies are starting to turn:

> Be a bit careful about what information you send to Andy Revkin of The New York Times and what emails you copy him in on. He's not as predictable as we'd like.

October 28, 2009: **email 1256765544**

And, finally, we turn to Phil Jones's final actions in the Climategate repository, still attempting to silence his critics by bullying tactics. He writes to Sonja Boehmer-Christiansen's head of department:

> Subject: Dr Sonja BOEHMER-CHRISTIANSEN
>
> Dear Professor Haughton,
>
> The email below was brought to my attention … It was sent by the person named in the header of this email. I regard this email as very malicious. Dr Boehmer-Christiansen states that it is beyond her expertise to assess the claims made. If this is the case then she shouldn't be sending malicious emails like this. The two Canadians she refers to have never developed a tree-ring chronology in their lives and McIntyre has stated several times on his blog site that he has no aim to write up his results for publication in the peer-review literature.
>
> I'm sure you will be of the same opinion as me that science should be undertaken through the peer-review literature as it has been for over 300 years. The peer-review system is the safeguard science has developed to stop bad science being published.

And what is to safeguard us against Jones's corruption of the peer-review process?

Haughton rejects Jones's tactics:

> Dear Phil, sorry to hear this. I don't see much of her these days, but when I do see Sonja next I'll try and have a quiet word with her about the way the affiliation to us is used, but at the moment in fairness she is entitled to use it in the way she does. Fortunately I don't get to see many of these email exchanges but I do occasionally hear about them or see them and frankly am rarely convinced by what I read. But as with all academics, I'd want to protect another academic's freedom to be contrary and critical, even if I personally believe she is probably wrong. I agree with you that it'd be better for these exchanges to be conducted through the peer review process but these forms of e-communication are now part of the public debate and it's difficult to do much about it other than to defend your position in this and other fora, or just ignore it as being, in your words, malicious.

Jones accepts defeat:

> You are probably aware of this, but the journal Sonja edits is at the very bottom of almost all climate scientists lists of journals to read. It is the journal of choice of climate change skeptics and even here they don't seem to be bothering with journals at all recently.
>
> I don't think there is anything more you can do. I have vented my frustration and have had a considered reply from you.

And lest it be thought that Haughton's rightful defense of academic freedom is due to his being a "skeptic", his final reply dispels any doubt:

> I know, I feel for you being in that position. If it's any consolation we've had it here for years, very pointed commentary at all external seminars and elsewhere, always coming back to the same theme. Since Sonja retired I am a lot more free to push my environmental interests without ongoing critique of my motives and supposed misguidedness—I've signed my department up to the "10:10 campaign" (cutting 10% of emissions in 2010) and have a taskforce of staff and students involved in it.... Every now and then people say to me under their breath with some bemusement, "and when Sonja finds out, how will you explain it to her...!"

Free to push environmental interests without ongoing critique?

Thank you, Climategate whistle-blower, for saving us from such a fate.

John P. Costella *– B.E.(Elec.)(Hons.) B.Sc.(Hons.) Ph.D.(Physics) Grad.Dip.Ed.*

SOURCE MATERIAL

- All files (62 MB, zipped)
- Emails (browsable folder)
- Documents (browsable folder tree)