

## *Hiding the Decline*



# HIDING THE DECLINE

*A.W. Montford*

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Set in Charter.

Cover design by the author. The chart shows the twentieth century divergence between some tree rings and the instrumental temperatures.

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For Lesley



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## PREFACE

The events at the University of East Anglia in November 2009 had a profound and transformative effect on my life. Coinciding with the publication of *The Hockey Stick Illusion*, Climategate catapulted me into the front line of the Hockey Stick wars and brought me a measure of fame (or notoriety depending on one's view) if not of fortune. The success of *The Hockey Stick Illusion* was always going to be a hard act to follow and by deciding to set out the story of Climategate I have made my life doubly difficult. Many of those criticised in this book have set out to cover their tracks with a web of deceit and obfuscation and in this they have been moderately successful. The result of these efforts has been that the tale has been extraordinarily difficult to tell. I hope my efforts mean that it is somewhat easier to follow.

I have set out to write this record of what has happened in the last two years more out of a sense of public duty than because I think that I have a bestseller on my hands. Nevertheless, the story is an important one and, I think, one that carries many insights into the way the public sector functions.

The roots of the Climategate story are intertwined with that of the Hockey Stick graph, a tale that I have therefore been forced to retell in potted form in the first chapter of this new book. This has given me a chance to focus on those details that appeared insignificant at the time but now take on a new importance. I would advise those who are familiar with my last book not to skip over the new chapter so that the all the threads of the tale can be grasped.

Uncovering these complications has inevitably involved a great deal of detective work, a task that would have been far too great for anyone to complete on their own. Apart from myself, that task has fallen chiefly to Steve McIntyre and David Holland, to whom I am grateful for tips and clues and many useful discussions on the interpretation of the evidence.

Many others have helped out along the way, either with the text of the book or providing support along the way. Names that spring to mind Ross McKittrick, Josh Gifford, Peter Gill, Chris Horner, Doug Keenan, Richard Thomas, Jonathan Jones, Matt Ridley, Benny Peiser, Tony Newbery, David Henderson, Anthony Watts, Steven Mosher, Tom Fuller and Don Keiller, as well as several others who have to remain anonymous. Thanks also to all those commenters at my blog who have suggested lines of inquiry, and also the many anonymous small donors who help support my blogging work and hence the investigations that have underpinned this book. If I have missed anyone out from this list, I apologise.

As with *The Hockey Stick Illusion*, Dr Lesley Montford and Dr Angela Montford both helped bring the book to fruition in their separate ways. To them, many thanks are due. Smaller members of the family kept me from my desk as much as possible, which was probably just as important in bringing the project to a successful conclusion.

AWM

Kinrossshire, 2012.

## NOTES ON USAGES

As it is one of the most important allegations arising from the Climategate emails, I return several times to Phil Jones' infamous truncation of one of Keith Briffa's tree ring series. Readers will often see this incident referred as 'Mike's Nature trick', but I have adopted the usage of 'the trick to hide the decline'. In the often absurdly heated atmosphere of the global warming debate, criticism is sometimes levelled at those who place the latter expression in quotation marks. It is said to be unreasonable to imply that this is a direct quotation since in Jones' original email the words 'trick' and 'hide the decline' are separated by a parenthetical clause. To avoid this kind of unpleasantness I will use the expression without quotation marks – the trick to hide the decline – or 'the Trick' for short.

UK freedom of information laws are set out in two main pieces of legislation: the Freedom of Information Act 2000 and the Environment Information Regulations 2000. I will refer to 'FOI legislation' when I mean to refer to both of these at the same time.

We still have no idea of the identity of the person who disclosed the Climategate emails (nor indeed if they were a hacker or a whistleblower). I refer to the person involved as 'he', but this should not be taken as any indication that the field of suspects has been narrowed in any way.

Finally, as in *The Hockey Stick Illusion*, this book includes many extracts from the email correspondence of the main characters. In general I have chosen to correct spelling and punctuation errors in these in order to avoid having to excuse myself of each one.



## THE HOCKEY STICK

The roots of this story are long and tangled and sometimes hard to grasp, but if one were to point to an element that is key to understanding the events at the University of East Anglia at the end of 2009, it is the story of the Hockey Stick graph. I related this extraordinary tale in my last book, *The Hockey Stick Illusion*, but it is nevertheless important to revisit at least some of the details here, to enable new readers to understand the background to the story and also to focus on those elements that turned out to be important in what followed.

The Hockey Stick is a graph of global temperatures for the last millennium, reconstructed from tree rings and other so-called proxy data, its name coming from its remarkable shape, a long flat ‘handle’ representing comparatively stable temperatures in earlier centuries, followed by a dramatic uptick – the ‘blade’ – representing the effect of industrialisation on temperatures in the twentieth century. It was, for a time, the most important graph in the world, its message of unprecedented warmth at the end of the twentieth century a vital part of the campaign to persuade a doubting public that mankind had changed the world’s climate. The graph was the work of a young American climatologist named Michael Mann, and it was controversial from the moment it appeared in the journal *Nature* in 1998, with its publication prompting a fierce debate over whether Mann and his co-authors, Raymond Bradley and Malcolm Hughes, had spliced two different datasets or had merely ‘overlaid’ them.

Within days of its publication the Hockey Stick graph was being widely cited and soon afterwards it was picked up by the Intergovernmental Panel on Climate Change (IPCC), the body that issues periodic assessments of the world’s climate. The IPCC viewed Mann’s findings as highly significant and gave the Hockey Stick top billing in its Third Assessment Report, showing it no less than six times across the various parts of the report. Despite, or perhaps because of, the importance

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attached to the graph, it quickly became a target for sceptics, who were suspicious of Mann's findings and the relentless way in which the graph was being promoted as evidence of manmade global warming.

SOON AND BALIUNAS

One of the first attempts to rebut the Hockey Stick paper was made by two Harvard astrophysicists, Willie Soon and Sallie Baliunas, who published a paper in 2003 in the journal *Climate Research*, concluding that Mann had been incorrect and that temperatures at the end of the twentieth century had been well within the normal range of climate variability.<sup>1</sup> In fact, they said, modern temperatures were likely to have been surpassed during the Medieval Warm Period. Soon and Baliunas's paper was therefore a direct challenge to both the Hockey Stick itself and to the IPCC report, which placed great emphasis upon its dramatic message of unprecedented warmth in the twentieth century. It was clear from the moment the Soon and Baliunas paper appeared that there would be a response.

One of the first mainstream climatologists to notice the new paper was Tim Osborn, a scientist at the Climatic Research Unit (CRU) at the University of East Anglia (UEA) in the UK. Realising that the paper had the potential to strike a serious blow against the 'consensus' position that modern temperatures were unprecedented, Osborn quickly brought it to the attention of the unit's director, Professor Phil Jones. Shortly afterwards, Jones forwarded the news to a group of senior colleagues. Although it is not clear exactly who received Jones' message, the recipients of later emails in the thread included Michael Mann as well as several other members of the tightly knit group of climatologists who would later become known as the 'Hockey Team': Jonathan Overpeck, a senior IPCC author, and Keith Briffa, a climatologist who worked alongside Jones at CRU. The message was as follows:

Tim Osborn has just come across this [paper]. 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 [Chris de Freitas,] a well-known



skeptic in [New Zealand]. He has let a few papers through by [sceptics] in the past.<sup>2</sup>

This first message has something of a tone of weary resignation: as Jones explained, he had already complained several times to *Climate Research* about its willingness to publish sceptic papers, but the editor, Hans von Storch, had been unimpressed. However, the Soon and Baliunas paper was clearly much more serious in effect than any of these earlier episodes, and Jones and his colleagues started to discuss taking much firmer steps to bring the journal to heel – steps that may have crossed an ethical line.

Jones' next message started out in innocent enough fashion, with a detailed description of his concerns over the new paper. In particular, he felt that Soon and Baliunas should have discussed the question of whether the Medieval Warm Period happened at the same time in different places around the world. At first sight, this would appear to be a fairly run-of-the-mill difference of scientific opinion – one that would, in the normal state of affairs, lead to the submission of a critical comment to the journal. However, as it continued, Jones' email started to take on an uglier tone and he told his colleagues that the paper was 'appalling':

Writing this I am becoming more convinced we should do something – even if this is just to state once and for all what we mean by the [Little Ice Age and Medieval Warm Period]. I think the skeptics will use this paper to their own ends and it will set [paleoclimatology] back a number of years if it goes unchallenged. 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 CRU person is on the editorial board, but papers get dealt with by the editor assigned by Hans von Storch.<sup>2</sup>

Remarkably then, Jones appears to have been suggesting a boycott of the journal if it published sceptic papers. Mann was in complete agreement and he expanded on Jones' hint that there might be something untoward going on at the journal:

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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.<sup>2</sup>

In fact, Mann was even more suspicious than Jones, pointing the finger of doubt at von Storch as well as de Freitas:

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)...<sup>2</sup>

As Mann went on to suggest, one of the main criticisms of the sceptics was that they did not publish their findings in the peer-reviewed journals, and he was determined that something should be done to stop Soon and Baliunas winning a public relations coup by actually doing so.

This was the danger of always criticising the skeptics for not publishing in the 'peer-reviewed literature'... they found a solution to that – take over a journal! 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...<sup>2</sup>

For a time the Hockey Team discussed the possibility of a more orthodox response – a published rebuttal in another journal. However, some members of the team were still keen on something more radical. Over the following weeks the conversation expanded to include Mike Hulme, the director of the Tyndall Centre, the UK's national centre for climate research, which was also sited on the UEA campus. Also involved were a number of scientists based in Australia and New Zealand. Hulme in particular was keen to make the response decisive – his idea was to prompt a mass resignation of the editors of *Climate Research*, and he noted that Jones had carried through on his threat

and was already refusing to peer review any papers for the journal. Jim Salinger, a scientist at New Zealand's National Institute of Water and Atmospheric Research, was also one of the hawks. In his emails he was alluding darkly to de Freitas's right-wing views and he offered to compile a dossier of his journalism as evidence. Jones said that he intended to put even more pressure on von Storch the following week:

...I'll be telling him in person what a disservice he's doing to the science and the status of *Climate Research*. I've already told Hans I want nothing more to do with the journal.<sup>3</sup>

Mann was grateful for his colleagues' support of his paper and said that he was encouraged at the prospect of some sort of action being taken, suggesting that a complete boycott of the journal would be in order.

I believe that a boycott against publishing, reviewing for, or even citing articles from *Climate Research* is certainly warranted.<sup>3</sup>

Shortly afterwards, Hulme sent a letter to a group of *Climate Research* editors, explaining the strength of feeling among mainstream paleoclimatologists about Soon and Baliunas's paper, which he said was 'just crap science that should never [have] passed peer review'.<sup>3</sup> He explained that communications managers at the Tyndall Centre had backed his idea of a mass resignation.

While Hulme was pursuing this project, other members of the Hockey Team were considering other possibilities. Barrie Pittock, a scientist at Australia's national research institute, CSIRO, circulated a long list of ideas, including branding dissenting editors as 'rogue editors', taking legal action – he noted that it might be possible to obtain financial support for this – and returning to the idea of a boycott to bring recalcitrant journals back into line.<sup>4</sup>

A few days later, Salinger wrote a long email to the rest of the Team, copying it to a larger group of colleagues in Australia as well as Rajendra Pachauri, the head of the IPCC. In it, he explained the still more dramatic steps he had in mind:

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I have had thoughts also on a further course of action. The present Vice Chancellor of the University of Auckland, Professor John Hood (comes from an engineering background) is very concerned that Auckland should be seen as New Zealand's premier research university. . . My suggestion is that a band of you review editors write directly to Professor Hood with your concerns. In it you should point out that you are all globally recognized top climate scientists. It is best that such a letter come from outside NZ and is signed by more than one person.<sup>5</sup>

Salinger helpfully provided a suggested text,

We write to you as the editorial board (review editors??) of the leading international journal *Climate Research* for climate scientists. . . We are very concerned at the poor standards and personal biases shown by a member of your staff. . .

When we originally appointed. . . to the editorial board we were under the impression that they would carry out their duties in an objective manner as is expected of scientists world wide. We were also given to understand that this person has been honoured with science communicator of the year award, several times by your. . . organisation.

Instead we have discovered that this person has been using his position to promote 'fringe' views of various groups with which they are associated around the world. It perhaps would have been less disturbing if the 'science' that was being passed through the system was sound. However, a recent incident has alerted us to the fact that poorly constructed and uncritical work has been allowed to enter the pages of the journal. A recent example has caused outrage amongst leading climate scientists around the world and has resulted in the journal dismissing (??). . . from the editorial board.

We bring this to your attention since we consider it brings the name of your university and New Zealand into some disrepute. We leave it to your discretion what use you make of this information.

The journal itself cannot be considered completely blameless in this situation and we clearly need to tighten some of our edito-

rial processes; however, up until now we have relied on the honour and professionalism of our editors. Sadly this incident has damaged our faith in some of our fellow scientists. Regrettably it will reflect on your institution as this person is a relatively senior staff member.<sup>5</sup>

Tom Wigley, a former head of CRU, appeared somewhat uncomfortable with what was being discussed. However, while he said that he was convinced that de Freitas was selecting reviewers sympathetic to sceptic views, he maintained that ‘a barrage of ad hominem attacks or letters’ was not the way forward,<sup>6</sup> and in fact his objections may have carried the day since there is no evidence that the Hockey Team ever acted on Salinger’s more extreme ideas. Wigley also felt that some of the blame should be apportioned to von Storch:

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.<sup>7</sup>

Disturbingly, Wigley’s message carries a hint that he wanted *Climate Research* closed to the sceptics whether the misdemeanours the Hockey Team saw in it were real or not, and he went on to reveal that von Storch was also in his sights:

[Mike Hulme’s] idea to get editorial board members to resign will probably not work – must get rid of von Storch too, otherwise holes will eventually fill up with [sceptics].<sup>7</sup>

Mann was particularly keen on this idea, emphasising the political importance of taking action against *Climate Research*.

This latest assault uses a compromised peer-review process as a vehicle for launching a scientific disinformation campaign (often vicious and ad hominem) under the guise of apparently

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legitimately reviewed science... 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... editorial board) seems, in my opinion, to have some potential merit.

This should be justified not on the basis of the publication of science we may not like of course, but based on the evidence... that a legitimate peer-review process has not been followed by at least one particular editor.<sup>8</sup>

Interestingly, Mann also revealed a belief that the scientific literature was largely closed to sceptic views, with only a handful of journals still permitting dissenting views to be aired:

While it was easy to make sure that the worst papers... didn't see the light of the day at [*Journal of Climate*], it was inevitable that such papers might slip through the cracks at e.g. [*Geophysical Research Letters* (GRL)] – there is probably little that can be done here, other than making sure that some qualified and responsible climate scientists step up to the plate and take on editorial positions at GRL.<sup>8</sup>

Shortly afterwards, Jones reported to the Team on his meeting with von Storch. Interestingly he wrote two separate emails – one to Hulme and one to Mann. To his UEA colleague he explained that von Storch was now onside and would be writing to the publisher to help the team identify the scientists de Freitas had chosen to peer review the Soon and Baliunas paper.<sup>9</sup> The same day, he revealed rather more to Mann, telling him that von Storch had actually gone much further and had indicated that de Freitas would be forced out of his position.<sup>10</sup> The news was soon spreading. Clare Goodess, one of Jones' colleagues at CRU and a member of the *Climate Research* editorial board, apparently told Mann that de Freitas would soon be gone.<sup>11</sup> This appears to represent an extraordinary breach of privacy, with de Freitas's position

at *Climate Research* being freely discussed by a variety of scientists, at least some of whom were unconnected with the journal.

What happened next is not entirely clear, but a partial record of events has been given by Goodess who, although she had been party to much of the correspondence among the members of the Hockey Team, made no mention of this in her account of events. According to her account, she and von Storch were sent ‘numerous unsolicited complaints and critiques of the paper from many leading members of the international [paleoclimate] and historical climatology community’.<sup>12</sup> Another complaint, which Goodess did not mention, was sent direct to the journal’s publisher, Otto Kinne, by Hulme.<sup>13</sup> Although the contents of this message have not been made public, much of its content can be determined from de Freitas’s defence of the accusations Hulme had made about him – that he was politically motivated and that he was giving an easy ride to sceptic papers.<sup>13</sup> In response, de Freitas argued that he had invited no fewer than five scientists to review the paper, although one had been too busy to take part. However, he had not selected the five names himself, but instead had passed this duty on to a scientist with expertise in the field. As Kinne noted in a subsequent letter to his editorial team this all appeared to be entirely normal – in fact he personally reviewed de Freitas’s files and could find nothing untoward – de Freitas had been targeted by critics in the past and was very careful to document his work thoroughly so as to be able to defend his decisions.<sup>14</sup>

Mann was unconvinced and, in what was later to emerge as a pattern in his behaviour, set about raising the temperature of the debate:

It seems clear we have to go above [Kinne]. 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.<sup>15</sup>

Wigley was in complete agreement with Mann’s provocative plan, and started to plan ahead, asking...

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... what would be our legal position if we were to openly and extensively tell people to avoid the journal?<sup>15</sup>

Jones' contribution to the conversation also adds some important details about the nature of de Freitas's review. Firstly Jones said that he thought that the paleoclimatologist who had selected the reviewers on de Freitas's behalf was Anthony Fowler – a researcher from New Zealand. Fowler is not known to be a sceptic, so Jones' news should have provided considerable reassurance to the Hockey Team members. But if this information was insufficient to convince them, they should certainly have had their concerns eased by the news that the scientist who had been too busy to get involved in the review was none other than Ray Bradley, Mann's co-author on the Hockey Stick papers – hardly someone who would be chosen by an editor looking to 'fix' the result of the peer review in favour of the sceptics. Remarkably, however, these revelations did nothing to change the views of the Hockey Team, and Jones even suggested that a negative review from Bradley would have made no difference to de Freitas' decision to publish. The reasons why Mann and Jones and their colleagues were so keen to proceed with action against the journal are unclear but, as we will see, there were hints of another, non-scientific agenda operating in the background.

At the same time as they were emailing Kinne and de Freitas, the Hockey Team scientists were busy preparing a more legitimate response to the Soon and Baliunas paper – a formal rebuttal, which they intended to submit to the journal *Eos*, where the editorial staff would be sympathetic to their position. The team of authors assembled for the rebuttal paper was a *Who's Who* of the Hockey Team: Mann and his co-authors Bradley and Hughes, Jones, Briffa and Osborn from CRU, and several others whose names will become familiar over the course of this story: Kevin Trenberth, Jonathan Overpeck and Caspar Ammann. Lastly, there was Michael Oppenheimer, a Princeton professor and long-time advisor to a green advocacy group, the Environmental Defense Fund. Towards the end of July Mann and Oppenheimer wrote to the rest of the authors of the *Eos* piece suggesting that they should



also write a joint letter to the US Senate because, as Mann put it, there was a 'continued assault on the science of climate change by some on Capitol Hill'.<sup>16</sup> As the email makes clear, Soon and Baliunas's work was having an impact on the political process in the USA, and Mann was determined to prevent this happening. In other words, it may not have mattered who had reviewed the Soon and Baliunas paper; it was necessary for it to be discredited.

Wigley agreed that politicians were making use of Soon and Baliunas' work and wondered if there was a possibility that the American Geophysical Union (AGU) and the American Meteorological Society might be recruited to help in discrediting it. Alternatively he wondered if Soon and Baliunas's university – Harvard – could be persuaded to distance themselves from the two sceptics in some way. Mann concurred and noted that the editor of *Eos* might be prevailed upon to get the AGU to make a statement. He said that Oppenheimer had also suggested that they might also be able to get the editor of *Science*, Donald Kennedy, to have the American Association for the Advancement of Science make a stand against the paper. However, most of the Hockey Team members were lukewarm about the idea of writing to the Senate, and a decision was taken to concentrate on the *Eos* article instead.

Meanwhile, in an attempt to put affairs at *Climate Research* back on an even keel, Kinne decided to promote Hans von Storch to the position of editor-in-chief, with a brief to oversee the review process and ensure its integrity. However, this idea quickly backfired. Just days before Soon and Baliunas were due to speak about their paper to legislators in the Senate, von Storch circulated the draft of an editorial that he said he would publish in the next issue of the journal.<sup>17</sup> He proposed telling readers that the Soon and Baliunas paper was flawed and that the peer review process at the journal had been inadequate to stop it from appearing.

The editorial board, however, were by no means united behind this idea and at least one of its members objected strongly, as he explained in an email to von Storch:

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A paper has been published that some people disagree with . . . the authors have responded. Isn't this the nature of the same scientific process that has worked just fine for centuries? Many papers have been published with which I have disagreed, but I never viewed the 'process' to be flawed. Honest scientists have differences of opinion. That is clearly the case here. You should know that I know the parties on BOTH sides of this particular issue and am not taking sides.

I cannot agree with your editorial since, in my view, there is no problem with the peer-review process.<sup>17</sup>

The result was inevitable. Shortly afterwards, and neatly coinciding with Soon and Baliunas's appearance on Capitol Hill, von Storch and several members of the editorial board at *Climate Research* resigned, no doubt to the horror of the politicians who had invited the two sceptics to speak, and to delight of their opponents.

Soon and Baliunas's critique of the Hockey Stick lapsed into obscurity, but in fact the Hockey Team was not quite finished with the two sceptics. A few months later, despite all the effort expended on discrediting them, an issue of the journal *Progress in Physical Geography* featured a review article on global warming written by Soon and Baliunas. This appearance, so soon after their humiliation on Capitol Hill appears to have been too much for Hulme, who wrote to the journal's editor in no uncertain terms:

I am writing to resign from my position as Editorial Adviser for the journal *Progress in Physical Geography*. . . I reached this decision after seeing the September 2003 issue of the journal in which I noticed that Willie Soon and Sallie Baliunas have been asked to provide the annual progress reports for 'global warming' for the journal and after reading their first contribution.<sup>18</sup>

And with that, the work of the Hockey Team appeared to be complete, as least as far as Soon and Baliunas were concerned.

MCINTYRE

Shortly after the Soon and Baliunas affair had ended in disappointment for the sceptic community, a new figure began to look at the Hockey Stick paper. Steve McIntyre was a semi-retired mining consultant from Toronto who had begun to investigate Mann's paper on a whim. Armed with formidable mathematical skills and a dogged determination, he quickly unearthed a raft of problems with Mann's data, findings which he published later in 2003 in a paper co-authored with his fellow Canadian, economist Ross McKittrick. The paper, which we will refer to as MM03, appeared in an obscure journal called *Energy and Environment*, which had a reputation as the sceptics' journal of choice, since its editorial board were sympathetic to dissident voices.

On the eve of the publication of MM03, Mann circulated an email to several of his colleagues, attaching a commentary about McIntyre and McKittrick's paper. This remarkable document, written by an unnamed author, shows that despite their outwardly aggressive stance towards anyone who questioned mainstream climate science, some of Mann's colleagues seem to have been privately impressed by MM03. Indeed the commentary suggested that their main conclusion – that the Hockey Stick graph's story of little medieval warmth was changed by minor corrections to the underlying database – was already well known to 'those who understand Mann's methodology'.<sup>19</sup> However, the author of the message was concerned that Mann's hot temper was going to get him into trouble once he saw the paper, and these concerns turned out to be well founded. Despite being aware that at least one of his associates agreed with McIntyre and McKittrick, Mann's immediate reaction was not to challenge MM03 through a response in the scientific literature, but instead to question the integrity of *Energy and Environment*. He suggested that the journal was 'a shill for industry', and on this basis dismissed the paper in its entirety rather than engaging with its criticisms of his work. As Mann told his colleagues:

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.<sup>19</sup>

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With the assistance of a sympathetic journalist, Mann proceeded to issue a strongly worded denunciation of McIntyre and McKittrick. A decision was also taken to issue a further informal rebuttal via CRU, since the UK wing of the Hockey Team could then be presented to the public as neutral arbiters in the dispute.<sup>19</sup> In due course a response was posted on Tim Osborn's website at CRU.<sup>20</sup>

The first skirmishes between Mann and the two Canadians were inconclusive, but Mann's hot-headed response to the publication of MM03 had led McIntyre to another extraordinary series of discoveries about the way the Hockey Stick paper had been put together. Mann's database included a number of hockey-stick-shaped tree-ring series derived from bristlecone pine trees, despite this species being widely recognised in the literature as being contaminated with a non-climatic signal. Then, to make it worse, it had been discovered that part of Mann's computer algorithm would pick hockey-stick shaped series out of the database and overweight them in the final result – in other words, even if there were only a few hockey-stick-shaped series in the database, the final reconstruction would look like a hockey stick. Together, McIntyre's damning findings looked as though they would break Mann's creation once and for all.

One of the main criticisms of MM03 had been the fact of its publication in *Energy and Environment*, which Hockey Team supporters claimed was not even a science journal – in fact, some had suggested that MM03 had not actually been peer reviewed at all.\* With this in mind, it had been important for McIntyre and McKittrick to get their new findings published in a more mainstream journal and they had initially submitted their manuscript to *Nature*, the journal that had published the Hockey Stick paper in the first place. However, after a protracted review process, the journal issued a rejection, apparently on the somewhat surprising grounds of lack of space.

Despite this, just as with MM03, some of Mann's closest colleagues appear to have been favourably impressed by the case put forward by

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\*Note, however, the email in which Osborn says that *Energy and Environment* had disputed this and that one scientist – Fred Singer – has identified himself as having reviewed the paper.<sup>21</sup>

the two sceptics. Wigley told Jones that McIntyre's findings appeared valid and that Mann's was 'a very sloppy piece of work'.<sup>22\*</sup> Jones however, was deep in the Mannian mindset by this time, and told Wigley that everything that McIntyre and McKitrick were saying was wrong, 'a complete distortion of the facts'.<sup>23</sup>

#### THE SAIERS AFFAIR

It must have been clear to Mann and his colleagues that McIntyre and McKitrick would not be put off by this setback and that they would try to publish their findings elsewhere. Soon afterwards their fears were proved correct when they learned that the Canadians' paper – which we will refer to as MM05 – had been accepted by GRL, one of the journals where the Hockey Team thought that that sceptic papers might 'slip through the cracks'.

Mann was furious, but he was not someone to give up easily and he made a desperate attempt to put a spanner in the works. Towards the end of January 2005 he telephoned Steve Mackwell, the editor-in-chief of GRL, complaining that he had not been allowed to comment on the paper and apparently trying to delay its publication. Although we cannot know precisely what Mann said, we can surmise many of these details from Mackwell's reply:

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.

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\*It is not entirely clear whether Wigley was discussing McIntyre's submission to *Nature* rather than MM03, although the date of the email makes the former the more likely. If so, then Mann's having circulated the manuscript to third parties would probably represent a breach of peer-review confidentiality.

## *Hiding the decline*

However, you are perfectly in your rights to write a comment, in which you challenge the authors' arguments and assertions.<sup>24</sup>

Mann's reaction to this gentle rebuff was not to do as Mackwell had suggested and argue his case in a comment submitted for publication, but once again to assume that sceptics had 'captured' the journal in some way. However, his attention was drawn not to Steve Mackwell, but to his deputy, James Saiers, who had been the editor responsible for handling the McIntyre and McKittrick paper. Saiers had formerly worked at the University of Virginia alongside a noted sceptic climatologist, Pat Michaels, and Mann found this association to be highly suspicious, as he explained in an email to several of his colleagues:

Just a heads up. Apparently, the contrarians now have an 'in' with GRL. This guy Saiers has a prior connection [with] the University of Virginia Dept. of Environmental Sciences that causes me some unease. I think we now know how [sceptic papers] have gotten published in GRL.<sup>24</sup>

Tom Wigley, the former head of CRU who had told Jones that McIntyre's new findings looked valid, was one of those who responded, but now presented a rather different take on the paper:

This is truly awful. GRL 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. . . 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 [AGU] channels to get him ousted. Even this would be difficult.<sup>24</sup>

Mann appeared to agree with Wigley that Saiers should be dealt with in some way and suggested a possible course of action:

It's one thing to lose *Climate Research*. We can't afford to lose GRL. 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 [in] what is going on here).<sup>24</sup>

With Mackwell refusing to accept Mann's protests, there was little that the Hockey Team could do about the new paper. But they were not going to suffer this setback in silence and when a journalist from the *New York Times* contacted Mann for a comment, he was told that the paper was 'pure scientific fraud'.<sup>25</sup>

When MM05 was published at the start of 2005, there was something of a media storm, with the finding that the Hockey Stick was flawed making headlines around the world. McIntyre was even profiled on the front page of the *Wall Street Journal*. The media interest was driven not just by the scientific importance of the Hockey Stick but by its prominence in the IPCC reports and in the resulting political debates. So if McIntyre and McKittrick's new paper was a problem for Mann it was potentially even worse for the IPCC. Having relentlessly promoted the Hockey Stick in the Third Assessment Report back in 2001, it would now suffer a hugely embarrassing loss of face if it was forced to admit that Mann's paper had been wrong. To make things even more difficult, work on the Fourth Assessment Report was already under way, so if anything was to be done about MM05 before the report appeared it would have to be happen in fairly short order. It was not long before the Hockey Team swung into action again.

#### WAHL AND AMMANN

In May 2005, on the same day that McIntyre was due to make a rare public appearance at a Washington think tank, the source of the Hockey Team's response became clear. Just hours before McIntyre was due to speak, a press release was issued by the US National Center for Atmospheric Research on behalf of two of its scientists. One of these – Caspar Ammann – we have already met as one of the co-authors to the proposed rebuttal to Soon and Baliunas in *Eos*; the other, Eugene Wahl, was also a core member of the Hockey Team. The press release announced the submission of two articles for publication. The first

was a paper for publication in the journal *Climatic Change* ('the paper'), which Wahl and Ammann claimed exactly replicated the Hockey Stick and demonstrated that Mann's findings were sound. The second was a comment on MM05 for publication in GRL ('the comment'), which they said would show that McIntyre and McKittrick's critique of the Hockey Stick was baseless. Perhaps there would be time to save the Hockey Stick for the Fourth Assessment Report after all.\*

However, while it was one thing for Wahl and Ammann to claim they had refuted McIntyre and McKittrick, it was quite another to actually do so in practice, for the simple reason that their two papers actually *supported* many of the Canadians' main criticisms of the Hockey Stick, in particular their claim that the Hockey Stick was statistically unreliable. At stake were the so-called verification statistics: numbers that give statisticians a feel for how much reliance they can place on a result. There were two main verification measures at issue, the  $R^2$  and the RE. McIntyre and McKittrick had shown that the Hockey Stick failed the  $R^2$ , which is a routine measure used by statisticians. Mann, however, had been arguing for the use of the more obscure RE test, which was unknown outside the field of climatology, but McIntyre had shown that the Hockey Stick actually fell at this hurdle too. Since Wahl and Ammann claimed to have exactly replicated Mann's paper, this could only mean that their version of the Hockey Stick must have failed its verification statistics as well. In other words, both Mann's graph and Wahl and Ammann's replication of it were statistically unreliable. What was worse for the Hockey Team, Wahl and Ammann had published all their data and computer code online, so the details and proof that McIntyre and McKittrick were correct were there to be seen by anyone who took the trouble to look. So, no matter what Wahl and Ammann told the press they had done, they would not be able to hide the facts for long. Shortly afterwards, their tricks were to be exposed.

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\*Interestingly, there is some evidence that at the time MM05 was published, Wahl and Ammann were in the process of completing a paper rebutting MM03.<sup>26</sup> This earlier rebuttal was presumably withdrawn before it could appear so that they could focus on the more serious findings in MM05.



Since Wahl and Ammann were criticising their work, GRL invited McIntyre and McKittrick to provide a written response. This would be sent out to the peer reviewers alongside the comment, to allow them to assess both sides of the argument. When the invitation to respond arrived, the two Canadians held nothing back and issued a strongly worded denunciation of Wahl and Ammann's work, demonstrating a series of flaws in the science and some highly questionable ethics as well. In fact, so damning was the rebuttal that when he saw it, the GRL editor, James Saiers, decided that he could not credibly send Wahl and Ammann's comment out for peer review and took an editorial decision to reject it entirely.

The rejection must have been a crushing blow to Wahl and Ammann. Not only was their purported refutation of McIntyre and McKittrick in tatters, but their replication of the Hockey Stick – their paper in *Climatic Change* – was potentially in ruins too because it relied for some of its key statistical arguments on the GRL comment. Even if they could get the paper through peer review in time, it would be simple for McIntyre to point out that it relied on the comment, which had already been rejected.

The Hockey Team's plan to save the graph for the IPCC looked as though it would come to nothing and, with the deadline for papers to be included in the Fourth Assessment Report looming just months away, there was almost no time left to do anything about it.

#### THE IPCC DEADLINE

The job of dealing with the Hockey Stick issue in the IPCC report was the responsibility of the coordinating lead authors on the paleoclimate chapter, Jonathan Overpeck and Eystein Jansen.\* The two men had the unenviable task of coordinating an international team of authors and producing a chapter that covered climate history over many different timescales. And if this were not difficult enough, they also had to deal with the fallout from the most heated dispute of the whole re-

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\*We have already met Overpeck as one of the Hockey Team members who was involved in the discussion about how to react to the Soon and Baliunas paper.

port. The two years of the IPCC review promised to be interesting ones for the author team.

Because of the complexity of the chapter they were supervising, Overpeck and Jansen had divided the work up between the members of the team. The author who was given responsibility for the millennial temperature reconstructions – including the Hockey Stick – was, like Overpeck, a core member of the Hockey Team and a name we have come across already: the CRU's Keith Briffa.<sup>27</sup>

Briffa can have been under few illusions about the importance of the task he had been given, but if he harboured any doubts the email he received from Overpeck would have swept them aside. As Overpeck put it, Briffa's section was to be 'the big one' and he went on to explain that, exceptionally among the author team, Briffa would be allowed to expand beyond his initial word allocation if it helped him to produce a compelling section that could be included in the Summary for Policymakers – the executive summary of the report.<sup>28</sup> However, it was also clear that there would be no hiding from the Hockey Stick controversy. Even while the first draft – known as the Zero Order Draft – was being prepared, Briffa had been sent an email by David Rind, one of his fellow lead authors:

[McIntyre and McKittrick] claim that when they used [the statistical procedure used by Mann, but with random data series], it always resulted in a 'hockey stick'. Is this true? If so, it constitutes a devastating criticism of the approach; if not, it should be refuted. While IPCC cannot be expected to respond to every criticism a priori, this one has gotten such publicity it would be foolhardy to avoid it.<sup>29</sup>

With the millennial temperature reconstructions intended to form the centrepiece of the paleoclimate chapter, Wahl and Ammann's attempt to save the Hockey Stick was clearly going to be a matter of intense interest to the team at the IPCC. At the start of July Overpeck emailed Ammann to ask after the paper's progress, saying that it was 'most important' that it be in press by the end of the month – the deadline for inclusion in the First Order Draft.<sup>30</sup> There are no records of the reaction of any of the scientists to the rejection of Wahl and Ammann's

comment on MM05 a few weeks earlier, but it is possible that none of them – Briffa and Overpeck included – were aware of how critical the arguments in the comment were to the case put forward in the paper. Wahl, however, must have realised just how difficult it was going to be to get both the paper and the comment through peer review at all, let alone before the IPCC report was issued. He wrote back to Overpeck, perhaps somewhat nervously, seeking clarification of exactly when the final deadline for submission of papers was to be.

Fortunately, an opportunity for Wahl to extricate himself from his predicament was soon to present itself. Shortly after his email exchange with Mann, Mackwell had come to the end of his term of office as editor-in-chief of GRL and was replaced by a new man – Professor Jay Famiglietti of the University of California. With the new regime came a new opportunity to save the Hockey Stick.

The first signs that something unusual was happening came when Famiglietti announced in a magazine interview that he had decided to take personal responsibility for MM05 and its responses, including Wahl and Ammann’s comment, apparently on the grounds that the McIntyre and McKitrick paper had been so controversial. Saiers – the man the Hockey Team had discussed ousting just a few months earlier – was somewhat ignominiously to be pushed aside.\* Shortly afterwards it emerged that Wahl and Ammann’s comment had been resubmitted to GRL and that now, only a few days later, it was ‘pending final acceptance’. This was extraordinary, since critical comments submitted to the journal were supposed to be sent out to reviewers accompanied by a response from the authors who were being criticised. In order to avoid another devastating critique from McIntyre and McKitrick, Famiglietti had been forced to break his journal’s own rules, simply failing to allow the two Canadians to have their say. Having already accepted the comment, Famiglietti added insult to injury by belatedly inviting McIntyre to submit a response.

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\*There has been some confusion over what happened to Saiers, with some commentators believing that he lost his position at the journal entirely. In fact he only lost responsibility for dealing with McIntyre’s paper, but remained at the journal for some time thereafter.

## *Hiding the decline*

The integrity of the peer review process had been shattered in the scramble to save the Hockey Stick, but as Mann later commented to Jones and Osborn, the ‘leak at GRL’ had been ‘plugged’.<sup>31</sup>

### THE TIMETABLE

The coup at GRL and Famiglietti’s acceptance of the comment that Saiers had rejected had brought all of Wahl and Ammann’s statistical arguments back into play.<sup>32</sup> However, as we have seen, Wahl and Ammann’s *paper* – the replication of the Hockey Stick – failed both of the main statistical tests used to assess its reliability and it was therefore unclear how the resurrection of the *comment* was going to help get it through its peer review. However, by accepting the comment without a response from McIntyre, Famiglietti had at least opened the way for the paper to finally recommence the peer review process.

The difficulties with the verification statistics were not the least of Wahl and Ammann’s problems either. There had been a delay of nearly six months between Saiers’ original rejection of the comment and Famiglietti’s ousting him from his position, so it was the end of November 2005 before the comment was back in play. Time was therefore running impossibly short for the paper to be included in the IPCC report: the first order draft had been issued and the review process was nearly complete. The Hockey Team therefore had only days left before they ran out of time, as the IPCC’s timetable made absolutely clear:

Third Lead Author meeting, December 13 to 15, Christchurch, New Zealand. This meeting considers comments on the first order draft and writing of the second order draft starts immediately afterwards. Meeting of the [Technical Summary and Summary for Policymakers] writing team [on] December 16, [in] Christchurch, New Zealand. Note. Literature to be cited will need to be published or in press by this time.\*

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\*Quoted in David Holland’s submission to the Russell review.<sup>33</sup> The document can no longer be seen in its original web location.

The Second Order Draft was the last time the official reviewers would see the report before it was published, so the deadline had presumably been imposed to ensure that they would see and assess at least once all the papers that the author team had cited. To emphasise the point, the deadline had been made still more explicit in another document – the ‘Deadlines for Literature Cited in the Working Group I Fourth Assessment Report’. This was written by Martin Manning, the head of the Technical Services Unit (TSU), the IPCC’s administrative arm, and in it Manning charged chapter authors with making sure that they only used final versions of any paper they had considered during the writing of the Second Order Draft, although he also made allowance for incorporating copyediting changes after this time. Crucially, however, Manning also explained what this would mean in practical terms:

In practice this means that by December 2005, papers cited need to be either published or ‘in press’.<sup>34</sup>

However, the ‘in press’ deadline was only half the story. As the ‘Deadlines’ document made clear, it was considered important that government and expert reviewers only use versions of the papers that were complete in all respects, and a second hurdle was therefore put in place:

When the second draft of the [report] is sent to governments and experts for the second round review, the TSU must hold final preprint copies of any unpublished papers that are cited in order that these can be made available to reviewers. This means that by late-February 2006 if [lead authors] can not assure us that a paper is in press and provide a preprint we will ask them to remove any reference to it.<sup>34</sup>

The stern warning that citations of papers that did not meet these deadlines would be removed shows that the IPCC was taking compliance with the timetable very seriously. Clearly there was no way that the *Climatic Change* paper was going to be ‘in press’ in two weeks’ time and the added hurdle of having a preprint available by the end

## *Hiding the decline*

of February must have made the problem look almost insurmountable. The Hockey Team were, however, nothing if not resourceful and by-passing a mere deadline was to prove well within their capabilities.

### BEATING THE CUT

The editor of *Climatic Change*, and therefore the man who was responsible for deciding the fate of Wahl and Ammann's paper, was Stephen Schneider, a climatologist from Stanford University in the USA. Schneider had been at the very centre of the global warming movement almost from the beginning and he was therefore completely trusted by the Hockey Team.\* Their confidence was not misplaced; when he had received the first draft of Wahl and Ammann's paper back in May, Schneider had quite properly sent it out for review to McIntyre, with Phil Jones providing an opinion from the opposite end of the spectrum of opinion.† However, with time now of the essence, it appears that Schneider decided to tip the balance in favour of the Hockey Team: for the second draft of the paper McIntyre was not included among the peer reviewers.

With McIntyre out of the way, there would be no awkward questions about the contents of the paper, but there was still the problem of the IPCC deadline to deal with and, with time so short, Schneider was forced to go one step further. Although it is not known who came up with the idea, on the eve of the lead author meeting in Christchurch he introduced a new status for *Climatic Change* papers of 'provisionally accepted'. There is no record of this status ever having been applied to other papers at the journal and it therefore appears that Schneider created it for the sole purpose of ensuring that the Wahl and Ammann paper could be cited in the IPCC report. However, he also appears to have been nervous about what he was getting himself into, and he picked up on some of McIntyre's criticisms of the paper, telling Wahl and Ammann that the verification statistics must be shown before he

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\*He had, for example, been copied in on much of the correspondence relating to the Soon and Baliunas affair.

†Jones' friendly review of the paper was among the Climategate files.

would clear it for publication. But ‘provisional acceptance’ was enough for Wahl, who sent an exultant email off to Overpeck and Briffa:

I want you to know that we heard from Steve Schneider today that our paper with *Climatic Change* has been provisionally accepted for publication. The provisions Steve outlined are ones we fully accept and will implement (extra statistics of merit and remaking of graphics), so this paper can be viewed as accepted, I should think.<sup>35</sup>

But Wahl had some less favourable news too – the GRL comment had run into further problems. When Famiglietti had announced that the comment had been accepted, he had received a bitter complaint from McIntyre, who noted that Famiglietti had broken his own journal’s rules in the way he had handled Wahl and Ammann’s comment and that he had then compounded the problem by appearing to criticise McIntyre and McKitrick in a magazine interview. McIntyre said that through these actions Famiglietti appeared to be left hopelessly compromised as the editor responsible and asked him to hand over to someone more neutral. Famiglietti’s position was difficult – his breach of the rules had been so transparent that it was likely to rebound badly on him, particularly as McIntyre would probably write an excoriating response to the comment. However, with Wahl and Ammann’s *Climatic Change* paper moving safely into the IPCC process, it was now possible for Famiglietti to backtrack somewhat and save face: just days after apparently accepting the comment outright, he wrote to Ammann to explain that his comment on MM05 would actually have to run the gauntlet of peer review once again.<sup>36</sup>

#### THE SECOND DEADLINE

At the start of 2006, and with work under way on the Second Order Draft, Wahl wrote once again to Schneider and Briffa, this time enclosing the revised version of the *Climatic Change* paper:

I’m not sure that I ever sent you the updated Wahl–Ammann paper that was the basis for [Schneider’s] provisional acceptance.

## *Hiding the decline*

Here it is. As is, it contains a long appendix... [on verification statistics], which was not in the version I had sent you earlier in the year. All the main results and conclusions are the same.<sup>36</sup>

Wahl went on to explain that Schneider and the peer reviewer of the second draft had requested further changes to the manuscript, involving the vexed question of the verification statistics, saying that he and Ammann were going to:

... address publishing [ $R^2$  and CE] calculations for verification, which Steve [Schneider] and the reviewer reason should be done to get the conversation off the topic of us choosing not to report these measures, and onto the science itself.<sup>36\*</sup>

Wahl's message shows clearly that the version of the paper considered by the author team at their meeting before Christmas had clearly not been complete in all material respects as demanded by the Deadlines document – it had not included the adverse verification statistics, which would have shown that the conclusions were unreliable.<sup>†</sup>

What was worse for the author team, Schneider's concoction of a 'provisionally accepted' status had been observed by McIntyre, who had written a long blog post with a detailed analysis of the timings involved. This was potentially very serious for the author team's credibility, as Eystein Jansen explained to Overpeck:

Hi Peck, I assume a provisional acceptance is OK by IPCC rules? The timing of these matters are being followed closely by McIntyre ... and we cannot afford to being caught doing anything that is not within the regulations.<sup>36</sup>

Overpeck was not optimistic, and appeared resigned to the Wahl and Ammann paper being non-compliant, at least on the first deadline. However, he seemed to be pinning his hopes on it getting full acceptance by the time of the second deadline at the end of February 2006:

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\*Note that the wording suggests that there was a single reviewer of the second draft of the paper.

<sup>†</sup>See p. 23.



if Wahl and Ammann could come up with a preprint by then, it would presumably allow Overpeck to argue that the paper should be included in the report regardless of it having missed the earlier deadline back in December:

I'm betting that 'provisional acceptance' is not good enough for inclusion in the Second Order Draft, but based on what Gene [Wahl] has said, he should have formal acceptance soon – we really need that. Can you give us a read on when you'll have it Gene? Best make this a top priority, or we'll have to leave your important work out of the chapter.<sup>36</sup>

At the start of February,\* Wahl wrote to update the IPCC team on progress. He and Ammann were still working on the changes to the verification statistics that Schneider had demanded, but he suggested that this work could be complete within days and that Schneider might then be able to provide unconditional acceptance for the paper in a similarly short time.

Overpeck's reply suggested, however, that he had almost given up hope of the paper meeting the deadline:

Based on your update (which is much appreciated), I'm not sure we'll be able to cite [the paper] in the [Second Order Draft]... The rule is that we can't cite any papers not in press by end of Feb.

From what you are saying, there isn't much chance for in press by the end of the month? If this is not true, please let me, Keith, Tim and Eystein know, and make sure you send the in press doc as soon as it is officially in press (as in you have written confirmation). We have to be careful on these issues.<sup>37</sup>

Overpeck's statement is interesting because it appears to contradict the 'Deadlines' document, which said that not only must the paper be in press, but that the authors must be able to provide a preprint too. However, Overpeck's email appears to have given Wahl an idea, as he explained in his reply:

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\*The timing is approximate since the date is unclear from the email thread. It may have been during late January.

## *Hiding the decline*

... as I have understood it in our communications with [Schneider], final acceptance is equivalent to being in press for *Climatic Change* because it is a 'journal of record'. However, this would need to be confirmed to be quite sure.<sup>37</sup>

Wahl then wrote to Schneider to seeking clarification:

Overpeck. . . says that the paper needs to be in press by the end of February to be acceptable to be cited in the [Second Order Draft]. (I had thought that we had passed all chance for citation in the next IPCC report back in December, but [Overpeck] has made it known to me this is not so).

He and I have communicated re: what 'in press' means for *Climatic Change*, and I agreed to contact you to have a clear definition. What I have understood from our conversations before is that if you receive the [manuscript] and move it from 'provisionally accepted' status to 'accepted', then this can be considered in press, in light of [*Climatic Change*] being a journal of record.<sup>38</sup>

This email appears twice in the CRU disclosures – once in the message sent to Schneider and once when the correspondence was copied to Overpeck later that month.<sup>39</sup> Intriguingly, on the latter occasion, Wahl appears to have deleted the parenthetical sentence in the first paragraph about his having understood that the deadline had been missed.

Shortly afterwards Schneider replied, telling Wahl that his interpretation was 'fine', but he also emphasised the need to complete the revisions quickly so that the paper could get through its peer review in time for the IPCC. Despite all the problems with Wahl's two papers, it looked as they might just beat the deadline after all.

Finally, at the end of the month, and just hours before the deadline expired, Wahl wrote triumphantly to Overpeck:

Good news this day. The Wahl–Ammann paper. . . has been given fully accepted status today by Stephen Schneider. I copy his affirmation of this below, and after that his remark from earlier this month regarding this status being equivalent to 'in press'.<sup>40</sup>

'Accepted' had been deemed to be equivalent to 'in press', the failure of Wahl and Ammann to provide a preprint of the paper had been overlooked, but at last the IPCC had got its rebuttal of McIntyre and McKittrick.

#### THE DRAFTS

With Wahl and Ammann's paper safely delivered into the IPCC process at the end of February 2006, Briffa could finally set about completing his drafting of the chapter. However, his task remained a difficult one; he was under considerable pressure to give a picture of global temperature history that he felt was misleading and that understated the uncertainties. As he told Overpeck and Jansen,

...we are having trouble to express the real message of the reconstructions – 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... allows the uncertainty to swamp the magnitude of the changes through time.

...you have to consider that since the [Third Assessment Report], there has been a lot of argument re [the] 'hockey stick' and the real independence of the inputs to most subsequent analyses is minimal. True, there have been many different techniques used...but the efficacy of these is still far from established. We should be careful not to push the conclusions beyond what we can securely justify – and this is not much other than a confirmation of the general conclusions of the [Third Assessment Report]. We must resist being pushed to present the results such that we will be accused of bias...<sup>41</sup>

Although Briffa's conscience appeared to be pushing him towards recognising the problems with Hockey Stick and the other millennial temperature reconstructions in the report, he felt that some other members of the IPCC team were much less scrupulous in their approach to the uncertainties; he was being pushed towards taking a much less even-handed approach. As he explained, his concerns were centred

around two people in particular: Mann and Susan Solomon, the scientist in charge of the whole scientific report:

Of course this discussion now needs to go to the wider chapter authorship, but do not let Susan (or Mike) push you (us) beyond where we know is right.<sup>41</sup>

To add to Briffa's problems, word arrived that the US National Academy of Sciences (NAS), which at the time was investigating the whole area of paleoclimate,<sup>42</sup> was going to provide little support for the idea that proxy-based temperature reconstructions were reliable. The news came via Richard Alley, a glaciologist from Penn State University, who had made a presentation to the NAS inquiry and who was on cordial terms with several members of the Hockey Team. According to Alley, the panel members had shown a great deal of interest in some of the doubts raised over the reliability of the reconstructions. Alley was therefore worried that if the IPCC produced a report that appeared to place too much confidence in the reconstructions, the contrast with the NAS panel's conclusions would be an embarrassment to both groups.

The pressure on Briffa must have been enormous, with Alley and his conscience suggesting caution and Mann and Solomon pushing him in the other direction. The Second Order Draft is therefore a remarkable document, partly because it had little of the caution that Alley had suggested, but also when read in the light of the statements Briffa had made to Overpeck and Jansen.

Briffa's new assessment of the millennial temperature reconstructions stated that Wahl and Ammann had produced a replication of the Hockey Stick, succeeding where McIntyre and McKittrick had failed in MM03. However, this was hardly a full and fair description of what had happened, since at that time the two Canadians had written their first paper, Mann had been withholding key details of his methodology. It is therefore not surprising that the two Canadians had found that an exact replication eluded them. Briffa then went on to note the details of the new critique made by the two Canadians in MM05 – the verifi-

cation statistics and the biased algorithm – but failed to make it clear where the argument now stood:

The latter may have some foundation, but it is unclear whether it has a marked impact upon the final reconstruction.<sup>43</sup>

In fact, McIntyre and McKittrick had issued detailed refutations to each of the critics who had suggested that Mann’s biased methodology had only a limited impact on the final reconstruction,<sup>44</sup> and indeed, just a few weeks later, Tim Osborn reported the opinions of a group of top paleoclimatologists on this very question. . .

In general, most people accepted that the MBH method could, in some situations, result in biased reconstructions with too little low-frequency.<sup>45</sup>

. . . in other words there *was* a significant impact. But while Briffa had at least made some kind of statement about the methodological problems with the Hockey Stick, he stayed resolutely silent on the subject of the verification statistics. Did McIntyre’s observation that Wahl and Ammann’s version of the Hockey Stick failed its verification  $R^2$  have any foundation? Briffa was not saying.

Lastly, and rather remarkably, Briffa said that because Mann had come up with a reconstruction that was broadly similar to those of other scientists working in the area, he had probably arrived at the correct answer regardless of any problems with his data and methods:

However, subsequent work using different methods to those of Mann et al. (1998, 1999), also provides evidence of rapid 20th century warming compared to reconstructed temperatures in the preceding millennium.<sup>43</sup>

So while he had told Overpeck and Jansen that the efficacy of these different methods was ‘far from proven’, he was now suggesting to the public that they provided assurance of the soundness of the Hockey Stick. Moreover, the new draft contained not a hint of the caveat Briffa had delivered to Overpeck about the ‘independent verifications’

## *Hiding the decline*

of Mann's work being nothing of the sort – they largely relied on the bristlecones, the same contaminated data that Mann had used. The message for public consumption was clearly going to be very different to the one communicated in private.

### MORE DEADLINE PROBLEMS

Wahl may have thought his problems were over once his paper was accepted for the Second Order Draft, but in fact the respite was only temporary. Just two weeks after the passing of the IPCC deadline, McIntyre forwarded his response to the revised Wahl and Ammann comment to Famiglietti, who shortly afterwards announced that he, like Saiers before him, had decided that the comment was unpublishable. This was extraordinary – the journal had first rejected the submission out of hand, then had ousted the editor responsible for doing so, then had resurrected the comment and finally had decided to reject it once again. Exactly what was going through Famiglietti's mind is not clear, but it may have been that he had decided that it was simply too embarrassing to publish a paper that was so full of errors and misrepresentations – McIntyre had not been gentle in his comments on what Wahl and Ammann had done. Moreover, with Wahl and Ammann's *Climatic Change* paper now safely in the IPCC review process, Famiglietti may have felt that he had done his bit. So although rejecting the comment again left the paper in trouble once more – after all, it relied upon the comment for its statistical arguments – in practice it turned out to present no difficulties for the IPCC team, who simply carried on as if nothing had happened.

Further difficulties were emerging too. The Second Order Draft had been completed and sent out to reviewers at the end of March 2006, with the new text indicating that Wahl and Ammann had been able to replicate the Hockey Stick in full. However, the use of the paper to support the Hockey Stick was about to backfire. During May, the expert reviewers started to submit comments on the Second Order Draft, and Overpeck and his team will surely have been horrified to see that several commenters had pointed out that the version of Wahl and Ammann's paper on the IPCC website was different to the final draft

that Ammann had posted on his own home page, demonstrating conclusively that the version considered by the author team was not the final one.<sup>46</sup> This could only mean one thing: that Wahl and Ammann's paper had failed to meet the deadline. For a while there was consternation among the author team as they tried to work out exactly which version of the paper they had been looking at when the deadline had been reached, although Overpeck declared that the differences were not material. However, it was becoming increasingly clear that the paper's inclusion was going to leave the author team exposed to a great deal of criticism.

As well as Wahl and Ammann's effort, there were several other papers supportive of the Hockey Team position that had come into print too late for the IPCC deadline, including one by Briffa himself. This was a problem for the author team: the IPCC's own rules were preventing them from incorporating these helpful findings. However, once again the Hockey Team proved to be more than capable of extricating themselves from their dilemma.

At the end of June 2006 the author teams were to meet to discuss the review comments on the Second Order Draft in Bergen, Norway. At that meeting Solomon and Manning, together with Trenberth, Jansen and the coordinating lead authors for the other chapters,\* discussed the issue of compliance with the deadlines and it appears that they simply decided to rewrite the rules, as Manning explained in an email to the expert reviewers shortly afterwards:

We are very grateful to the many reviewers of the second draft of the Working Group I contribution to the IPCC Fourth Assessment Report for suggestions received on issues of balance and citation of additional scientific literature. To ensure clarity and transparency in determining how such material might be included in the final Working Group I report, the following guidelines will be used by Lead Authors in considering such suggestions.

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\*Also present was Solomon's co-chair at the head of Working Group I. Daihe Qin. The identities of those involved in taking the decision to change the deadline were revealed in an appendix to Osborn and Briffa's submission to the Russell review.<sup>47</sup>

## *Hiding the decline*

In preparing the final draft of the IPCC Working Group I report, Lead Authors may include scientific papers published in 2006 where, in their judgment, doing so would advance the goal of achieving a balance of scientific views in addressing reviewer comments. However, new issues beyond those covered in the second order draft will not be introduced at this stage in the preparation of the report.

Reviewers are invited to submit copies of additional papers that are either in-press or published in 2006...<sup>48</sup>

Manning's email was extraordinary on a number of levels. Firstly there is almost no evidence that reviewers of the Second Order Draft had been making suggestions about citation of new papers. In fact, there appears have been only a single such request, and this had apparently been rejected, with the authors noting that they would only examine recent papers that met IPCC deadlines.<sup>49</sup> Moreover, because the Second Order Draft was the last time the official reviewers would see the report before its publication, the new citations and any new text involved would go completely unreviewed. The perception that Manning's email was an unofficial attempt to assist the author team in the battle over the Hockey Stick is hard to avoid.

This view is reinforced by a later discovery. The IPCC is an inter-governmental body and governments are therefore closely involved in its activities. In particular, the way the panel operates is determined by agreement among the governments, which means that any changes to the rules have to be agreed by the governments' representatives. As well as setting down the rules, governments provide their own review comments on the drafts of the report. The us government in particular had been one of those that had commented on the inconsistency of the different versions of the Wahl and Ammann paper – they at least were aware of the issues over the deadline and could therefore be expected to object to any changes to the rules to allow new citations to be added after the completion of the review. However, this eventuality had been foreseen and the possibility of one of the governments objecting was sidestepped by the simple expedient of sending the email advising of



the rule changes to the expert reviewers alone. The governments were kept in the dark.\*

#### SECRET COMMUNICATIONS

In the months that followed, Briffa's problems grew. As well being pressured by Mann and Solomon he was having to deal with his review editor, Professor John Mitchell of the UK's Meteorological Office. Mitchell, along with his colleague Jean Jouzel, was responsible for refereeing any disputes in the paleoclimate chapter and ensuring that both sides of any debate were represented. Having seen the review comments on the Second Order Draft Mitchell knew that the Hockey Stick debate was going to require his attention, and he wrote to Overpeck offering his advice, and copying Osborn and Briffa. It was important, he said, to give a 'clear answer to the skeptics' and he set out what he thought needed to be covered:

Our response should consider all the issues for both [the Hockey Stick] and the overall chapter conclusions:

a. The role of bristlecone pine data

Is it reliable?

Is it necessary to include this data to arrive at the conclusion that recent warmth is unprecedented?

b. Is the [principal components analysis] approach robust? Are the results statistically significant? It seems to me that in the case of [the Hockey Stick] the answer in each is no. It is not clear how robust and significant the more recent approaches are.<sup>50</sup>

So clearly, Mitchell was in full agreement with Briffa's position on the Hockey Stick and the 'independent confirmations':<sup>†</sup> none of them were reliable. In these circumstances, a clear response to the sceptics

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\*David Holland ascertained this information by making an FOI request for the email to the UK government department responsible for dealings with the IPCC. He was informed that they did not have it.

<sup>†</sup>See p. 29.

that also kept Solomon and Mann happy looked as though it would be almost impossible to achieve.

With the deadline for inclusion in the report redrawn, Wahl and Ammann's paper and all the papers that the author team thought would support the IPCC consensus could be incorporated into the text. However, this approach had an unfortunate downside in that sceptics among the reviewers would now be able to suggest their own new papers too. McIntyre was fully aware of this opportunity and decided to submit the reports by two separate inquiries into the temperature reconstructions: the NAS report\* into the field of paleoclimate and the Wegman report into the statistics of the Hockey Stick. These two inquiries had confirmed many of McIntyre and McKittrick's criticisms and the Wegman report, in particular, was quite unequivocal in its support for McIntyre's statistical criticisms of the Hockey Stick, describing them as 'valid and compelling'. However, the NAS panel had tempered its support with an observation that was similar to the text in Briffa's latest draft – namely that despite using inappropriate data and a biased algorithm, Mann's findings appeared to be supported by other, allegedly independent, temperature reconstructions.

The mention of the findings of either the Wegman or NAS panels in the final report could potentially have been disastrous for the IPCC. Once again, the steps taken to deal with the problem appear to have been unethical. Shortly after the new deadline expired, the Technical Services Unit (TSU) issued a spreadsheet to the author teams which listed the new papers that had been suggested by the authors and reviewers. It has been determined, however, that this did not mention either the Wegman or NAS reports.† Once again, the scales were being tipped decisively in favour of the Hockey Team's position. What is more, Briffa was about to tip them even further.

The previous summer, shortly after Wahl and Ammann had announced their two journal submissions, Wahl had asked Overpeck if he and the IPCC team would like a copy of the manuscript of the GRL comment to help them in their work. This suggestion was somewhat

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\*See p. 30.

†See below, p. 220.

unorthodox: there is an official register of IPCC reviewers and Wahl's name was not on it. On this occasion, Overpeck had replied enthusiastically, saying that Wahl should send it over if he was comfortable doing so.<sup>30</sup> However, Overpeck's reaction to the offer is remarkable when set against the way he dealt with Neil Roberts, a scientist from the University of Plymouth who some time later made a similar offer of informal assistance with the review. On that occasion, Overpeck had explained in no uncertain terms that it was necessary to be registered as an official reviewer before contributing:

Since the IPCC has very strict rules about all this, I'm going to ask them... 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.<sup>51</sup>

Of course, the inconsistent treatment of the two offers of informal help may have been prompted by the discovery that McIntyre was watching them so closely or perhaps by a directive from further up the IPCC organisation. However, since Overpeck had copied this last message to Briffa, it is clear that by May 2006 Briffa was fully aware that further correspondence with unregistered reviewers was forbidden. Despite this, the pressure of dealing with the Hockey Stick war appears to have been too much: just weeks later, he wrote to Wahl:

Gene

I am taking the liberty (confidentially) to send you a copy of the reviewers comments (please keep these to yourself) of the last IPCC draft chapter. I am concerned that I am not as objective as perhaps I should be and would appreciate your take on the comments... that relate to your reassessment of the Mann *et al.* work. I have to consider whether the current text is fair or whether I should change things in the light of the sceptic comments... I must put on record responses to these comments – any confidential help, opinions are appreciated.<sup>52</sup>

## *Hiding the decline*

The fact that Briffa requested confidentiality suggests that he knew that he was breaching the rules. What is more remarkable still is that while Wahl, who was not an official IPCC reviewer, was being sent a copy of the review comments, official reviewers, including McIntyre, were being told that they would have to visit the official IPCC archive in Harvard, Massachusetts if they wanted to get a look at them.<sup>53</sup>

Wahl was happy to help and a few days later he provided detailed input, sending over a copy of his GRL comment on MM05 to help Briffa's deliberations. But he too appeared to be quite clear that the course of action he and Briffa were embarking on was illegitimate, although his concern may have been more to do with the possibility of the journal discovering that he had breached the confidentiality of the peer review process than any breach of IPCC rules.

Please note that [the comment] text is sent strictly confidentially – it should not be cited or mentioned in any form, and MUST not be transmitted without permission. However, I am more than happy to send it for your use, because it succinctly summarizes what we have found on all the issues that have come up re [The Hockey Stick].<sup>52</sup>

Interestingly, along with the text of the paper Wahl enclosed a summary of the issues surrounding the Hockey Stick that he had written for the benefit of what he called 'a person in [Washington] DC' who was involved with the hearings about Mann's paper that were to be held in the wake of the Wegman and NAS reports. The political importance of the Hockey Stick was apparently undiminished.

Over the next few days emails shuttled back and forth between Wahl and Briffa, with Briffa seeking comments on what he had written in the text and in the responses to reviewers and Wahl putting forward his own suggestions. Throughout, the two men were focused on the way to deal with the Hockey Stick and how to handle McIntyre's criticisms. And throughout they were at pains to ensure that no hint of what had happened should reach the outside world:

What I am concerned about for the time being is that nothing in the [comment] shows up anywhere. . .

... Please do not pass these on to anyone at all. . .

... PLEASE REMEMBER that this is 'for your eyes only'. . .<sup>54</sup>

Wahl's ideas were apparently proving useful, and Briffa thanked him for his help and asked once again for reassurance that no details of their correspondence would be revealed:

I have 'borrowed (stolen)' from two of your responses in a significant degree – please assure me that this OK (and will not later be obvious) hopefully.<sup>54</sup>

Briffa spoke of the stress he was working under and Wahl only made things worse when he explained that publication of his paper might not even take place in 2006, which would put him in breach of even the new deadline.

I should note that [the paper] is still in 'in press' status,\* and its exact publication date will be affected by publication of an editorial designed to go with it that Caspar and I are submitting this weekend. Thus I cannot say it is certain this article will come out in 2006, but its final acceptance for publication as of [28 February 2006] remains completely solid.<sup>54</sup>

#### THE REPORT

The final version of the IPCC report appeared in early 2007 and it was very much as the sceptics had feared.<sup>55</sup> In the Second Order Draft, Briffa had said that the effect of McIntyre's criticisms was unclear. Now, however, he was more definite, claiming that Wahl and Ammann's paper had shown that the impact of the errors McIntyre had identified was 'very small'. Moreover, as expected, Briffa noted that there were other reconstructions around that provided support for Mann's general conclusions. The verification statistics were mentioned in passing but a definitive statement was sidestepped. Of the bristlecones there was not a word.

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\*Wahl refers to 'AW 2006', which might seem to suggest that he was talking about the comment rather than the paper. However his reference to the acceptance date in February makes it clear that he is referring to the paper.

Wahl and Ammann's paper was eventually published in September that same year, more than eighteen months after reaching 'in press' status, demonstrating fairly conclusively that it had failed to meet the deadline that had been moved for its benefit. It was accompanied by another paper: the comment on McIntyre and McKittrick's work, which Famiglietti had apparently refused to publish in *GRL*. Wahl and Amman had therefore rewritten it and submitted it to *Climatic Change*; having agreed to accept the paper despite it relying on the unpublished comment, editor-in-chief Stephen Schneider was put in a nearly impossible position by *GRL*'s rejection of the comment for a second time – he simply had to offer *Climatic Change* as an alternative outlet for the comment, despite all its flaws. The story of the scientific distortions that Schneider was forced to accept in the process has been told elsewhere.<sup>53</sup>

#### THE PURSUIT

The Hockey Team may have felt that they had won an important victory, but the sceptics were only just getting into their stride. As McIntyre unearthed each new detail of the manoeuvrings behind the scenes at the IPCC, he set out what he had discovered for all to read at his *Climate Audit* blog. Since he had set it up in 2005 the site had turned into a hive of activity, and McIntyre now had a loyal band of followers who were increasingly willing to help him out with his growing workload. One of the readers who took some of the strain was David Holland, a retired electrical engineer from England. Holland had been a regular commenter at *Climate Audit* for several years and had been heavily involved in sceptic efforts in the UK, although he still kept a relatively low profile. This, however, was soon to change.

Holland's first target was to understand how the IPCC review process had worked in practice. McIntyre and McKittrick had submitted many detailed comments on the paleoclimate chapter and had been repeatedly fobbed off with unresponsive replies from Briffa. This dismissal should have been picked up by the chapter review editors, Mitchell and Jouzel, whose job was to ensure that scientific disputes were correctly represented in the final report. However, despite the

IPCC's rules requiring all review comments to be made public, the contributions of Mitchell and Jouzel remained unpublished. Holland therefore decided to force the issue, requesting copies of Mitchell's comments under UK freedom of information (FOI) laws.

At first Mitchell resisted the request, claiming unconvincingly that he did not have a copy of the report that he had written. He suggested that Holland ask the TSU to send it to him, although he claimed, equally unconvincingly, that he did not have their email address. Fortunately, the IPCC appear to have overruled Mitchell and by the start of 2008 Holland was in possession of the full set of review editor comments. These, however, turned out to be a huge disappointment. Instead of a description of the disputes in each chapter and how they had been represented in the report, each one amounted to a simple sign-off by the review editor, affirming that they had reviewed their chapter and were happy with it. The sole exception to this pattern turned out to be from Mitchell, who had discussed the Hockey Stick:

As Review Editor of Chapter 6... I can confirm that the authors have in my view dealt with reviewers' comments to the extent that can reasonably be expected. There will inevitably remain some disagreement on how they have dealt with reconstructions of the last 1000 years and there is further work to be done here in the future, but in my judgment, the authors have made a reasonable assessment of the evidence they have to hand. The other possible area of contention (within the author team) is on some aspects of sea-level rise. This has gone some way towards reconciliation but I sense not everyone is entirely happy.

With these caveats I am happy to sign off the chapter...<sup>56</sup>

So instead of discussing how Briffa had achieved what was required of him – to fairly represent both sides of the Hockey Stick dispute in the report – Mitchell had accepted that Briffa could act as judge and jury,\* a decision that appeared to give the lead authors carte blanche to ignore the IPCC's procedures.

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\*Note that Mitchell seems to have signed the report with a date of 8 December 2007. This is nine months after the publication of the report. At some point subsequently, the date has been amended, apparently in another hand, to 2006.

## *Hiding the decline*

The brevity of the review looked very troubling, and Holland therefore decided to dig a little further – it occurred to him that there might be more to Mitchell’s review than simply the sign-off. Shortly afterwards he issued a further FOI request to the Met Office, asking for copies of any working papers produced by Mitchell during his work on the IPCC report. A few days later he extended his inquiry again, sending a similar request to Sir Brian Hoskins of the University of Reading, who had been a review editor on another chapter of the report.

Holland’s FOI requests seemed to have caused a measure of consternation among their recipients. Some days after receiving Holland’s request, Mitchell told Susan Solomon what was happening and asked how he should proceed, copying the message to Jouzel and Briffa, as well as to the TSU, whose email address he had told Holland he did not have just a few weeks earlier:

I have received the following letter from David Holland, who has links with Stephen McIntyre and his Climate Audit website, on the review process for chapter 6 of [the Fourth Assessment Report]. I have discussed this briefly with Jean [Jouzel] and we do not think there is an issue. However given the wider nature of the questions, I think it would be more appropriate for any response to come through IPCC rather than me as an individual. I will wait to hear from IPCC before I respond... I understand [Sir Brian Hoskins] has received a similar enquiry, hence I have included his name on the copy list.<sup>57</sup>

Solomon was adamant that nothing should be released, telling Mitchell that the proper sources for people who wanted to understand the review were the reviewers’ comments and the authors’ responses to them. She explained that it would be ‘inappropriate to provide more information’.<sup>57</sup> This was a remarkable position for her to take, since she must have been aware that that Holland had made a formal, legally binding request for information – she was in essence asking Mitchell to break UK FOI legislation. And it was not just Mitchell; her reply was copied to Briffa and to key figures in the IPCC, including Renate Christ, the organisation’s secretary, and all 27 review editors.



Everyone needed to know that the sceptics were hot on their trail and that no disclosures should be made.

When Mitchell replied to Holland's letter he was once again evasive, echoing almost to the letter Solomon's request to stonewall, and going on to claim that he had not kept any of his working papers.

There is no requirement to do so, given the extensive documentation already available from IPCC. The crux of the review editors' work is carried out at the lead authors meetings going through the chapters comment by comment with the lead authors.<sup>58</sup>

Mitchell's suggestion that he did not have to keep his working papers is not correct, since IPCC procedures require that all expert review comments are retained as part of the public record. His reply therefore seemed so unlikely that Holland decided to keep pressing, and at the start of April he issued another FOI request, this time asking for all of Mitchell's email correspondence in connection with the Fourth Assessment Report. Once again the Met Office's response was remarkable; although they released correspondence that post-dated Holland's first FOI request, they claimed that they held no relevant information from earlier dates. This implied one of two extraordinary scenarios – either Mitchell had not sent or received any emails in connection with the IPCC report or he had destroyed all this correspondence within months of the report's publication. And as we have seen, there *had* been some correspondence: Mitchell had emailed Overpeck and Jansen in the closing stages of the IPCC review to discuss how the Hockey Stick affair should be handled.\*

Although the Met Office may have felt that they had successfully complied with Solomon's request not to reveal any further details of the review process, the information they did release was very interesting to Holland. Among the disclosures were Mitchell's email to Solomon discussing his FOI request and Solomon's instruction to the scientists involved in the review process to reveal nothing. This

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\*See p. 35.

showed Holland clearly that the IPCC was mounting a stubborn resistance, and he determined to redouble his efforts.

The next step was a letter to Briffa, probing several of the outstanding questions about the Fourth Assessment Report. Like Mitchell, Briffa decided not to answer immediately, writing to tell Overpeck and Jansen that he would reply when he got round to it. Holland, however, was still considering ways to get a glimpse of what was happening behind the IPCC's facade, and he soon struck upon a different way of tackling the problem. He could also see that, as well as Mitchell and Briffa, several other UK-based scientists had received Solomon's email and it was possible that some of the institutions at which they worked might be more forthcoming with information. He therefore decided to extend his requests. First, he formalised the message to Briffa into an FOI request, asking for all of Briffa's IPCC-related correspondence. At the same time he sent further requests to DEFRA, the government department that co-ordinated the UK's involvement in the IPCC, and to the universities of Oxford and Reading, who had also provided review editors to the IPCC.\*

At CRU, Holland's request seems to have caused some concern and just four days after Holland had issued his request, Jones emailed Mann, Bradley and Ammann to tell them what was happening.

You can delete this attachment if you want. Keep this quiet also, but this is the person who is putting in FOI requests for all emails Keith and Tim [Osborn] have written and received re [the paleoclimate chapter of the Fourth Assessment Report]. We think we've found a way around this.<sup>59</sup>

#### THE DOG THAT DIDN'T BARK

While Holland was digging away, trying to discover what had happened behind the scenes in the final stages of the IPCC review, McIntyre had been pondering the responses given to his comments on the Second Order Draft and had stumbled across something rather remarkable. It had begun when he noticed that Ammann appeared to

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\*Myles Allen and Sir Brian Hoskins had been review editors on Chapters 10 and 3 of the report, respectively.

have failed to submit any comments to the IPCC review process, something that seemed unlikely given his close involvement in the Hockey Stick debate. Then, in mid-2007, the comment on MM05 that GRL had twice rejected was finally published in *Climatic Change*, and McIntyre quickly noticed something else that was very odd: some of his review comments on the Second Order Draft had been rejected by Briffa using arguments and turns of phrase that bore an uncanny resemblance to language in the new Wahl and Ammann comment. The problem with this was that the comment had not even been *submitted* to *Climatic Change* until well after the deadline for submission of papers to the IPCC review. McIntyre quickly surmised the truth: Briffa had been taking advice on how to deal with McIntyre's arguments outwith the IPCC process. At first he assumed, incorrectly, that this advice had been provided by Ammann, the lead author of the comment – as we have seen it was actually Wahl who had been the source. However, no matter who had provided the information, it was clear that Briffa had been guilty of multiple breaches of the IPCC's rules, which required reviews to be open and transparent, and the literature cited to be peer reviewed and in print. Neither Wahl nor Ammann were official IPCC reviewers, so all this chicanery had taken place entirely outwith official channels. McKittrick later explained why this mattered:

The problem, apparently, was that the actual publication record was either over [Briffa's] head or yielded a message he was disinclined to report, or both. So he went outside the structure of the IPCC report-writing process to recruit a highly partisan coach ("Gene") to provide him some text which would not be shown to the expert reviewers but which would go right into the final draft and be represented as the result of the official IPCC report-writing process.<sup>60</sup>

His trickery was now exposed, but things became even worse for Briffa the following day, when McIntyre revealed that he had also noticed that the IPCC had rewritten the timetable for submission of papers to the Fourth Assessment Report so as to allow time for Wahl and Ammann to finish their paper.<sup>61</sup> All the breaking and bending of rules looked as though it was going to come back to haunt the Hockey Team.

## *Hiding the decline*

The sceptic community was abuzz with these new findings and Holland wasted no time in acting on them, sending out further FOI requests to CRU, this time specifying that he wanted any correspondence relating to the Wahl and Ammann paper or to changes in the timetable for the IPCC report.

The pressure was building inexorably at CRU and shortly afterwards David Palmer, the university's FOI officer, emailed Jones, Briffa and Osborn, telling them about Holland's new request. He said that he wanted to respond 'by the book', and noted that a refusal was likely to end in Holland appealing to the Information Commissioner.\*

The scientists must have realised that their situation was perilous and they appear to have studied the legislation long and hard, looking for some way in which they could reject Holland's request. Before long they found what they thought was their get-out clause, in the shape of an exemption for information provided in confidence. There was, however, a potential problem in this line of reasoning in that the IPCC rules stated clearly that the organisation's overriding principles were of openness and transparency. But there were no other options on the table and just hours after Holland had sent his request, Osborn wrote to Caspar Ammann:

Our university has received a request, under the UK 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 IPCC assessment process.

... 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.<sup>62</sup>

If the hint to Ammann was not dubious enough, the following day, Jones emailed Palmer and a senior faculty manager at UEA named Michael McGarvie, to discuss how to resist disclosure.

Keith (or you Dave) could say that... Keith didn't get any additional comments in the drafts other than those supplied by

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\*The Information Commissioner is the official charged with enforcing FOI legislation in the UK.

IPCC... [he] should say that he didn't get any papers through the IPCC process either.

...What we did get were papers sent to us directly – so not through IPCC, asking us to refer to them in the IPCC chapters. If only Holland knew how the process really worked!!<sup>63</sup>

This is a remarkable document, which demonstrates that senior staff at UEA conspired to breach FOI laws. However, what followed was even more extraordinary. The next day, Jones started to take steps to ensure that there was no way Holland would be able to find the truth elsewhere, asking Mann to delete his correspondence with Briffa:

Can you delete any emails you may have had with Keith re [the Fourth Assessment Report]? Keith will do likewise... Can you also email Gene [Wahl] and get him to do the same? I don't have his new email address. We will be getting Caspar [Ammann] to do likewise.<sup>64</sup>

The message is particularly damning because, as its title 'IPCC & FOI' made clear, Jones was aware of the relevance of the FOI laws and knew that they had a direct bearing on what he was asking Mann to do.\* Mann's reply also made it quite clear that he intended to comply with Jones' request:

I'll contact Gene about this ASAP.<sup>64</sup>

Shortly afterwards, Mann forwarded Jones' request, apparently without comment, and Wahl dutifully deleted the emails.<sup>65</sup>

There was still the question of Ammann's correspondence, however, and it required a reminder from Osborn to prompt a response, with Ammann making a slightly more definite statement, saying that he might have written the emails differently had he known they were going to be made public. This, apparently, was good enough for the university and a few days later Holland's request was rejected on the

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\*The title of Jones' message is surmised from Mann's response, which has the subject, 'Re: IPCC & FOI'.

## *Hiding the decline*

grounds that the messages requested were confidential. Similar rejections followed from Oxford, Reading and DEFRA. The door had been slammed shut.

### PRECAUTIONARY PRINCIPLE

Holland may have been seen off, but the sceptic blogs were still alive with the stories of the shenanigans around the Fourth Assessment Report and it must have been clear to everyone that more FOI requests would soon be issued. And with so many hard-to-defend decisions taken in recent months, it was imperative for the IPCC scientists that any such requests not be successful: precautionary measures were required.

Shortly afterwards, Jones explained what these measures had entailed in an email to Jean Palutikof, who until 2004 had been his co-director at CRU and who had been a senior figure in the IPCC.

Jean

... What Keith and Tim did was to email all the [authors] on [the paleoclimate chapter], to ask if they would be happy for Keith/Tim to send emails relating to [their] discussions. They all refused, hence the refusal letter.

... John Mitchell did respond to a request from Holland. John had conveniently lost many emails, but he did reply with a few. Keith and Tim have moved all their emails from all the named people off their PCs and they are all on a memory stick.

So any thoughts on how to respond?

... As you and Tom know Keith and I are nowhere near the world's best for structured archiving – working as we do on sedimentary sequencing!

Cheers

Phil<sup>66</sup>

Jones may well have believed that by simply moving emails from computers to memory sticks he could simply refuse the next FOI request from McIntyre or Holland, telling them that the information was

not held. If so, he was almost certainly mistaken. When the FOI laws were framed, the possibility of public bodies trying to avoid compliance in this way was foreseen and the laws were written in such a way that the information on Jones' memory stick would be deemed to be held on behalf of the university. But more seriously, if a future FOI request had been refused, then a criminal breach of the legislation would almost certainly be committed in the process. The decision by the CRU scientists to hide their emails in this way was therefore a fateful one. And in time it would come back to haunt them.