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## Climate Reconstruction and the Making of Authoritative Scientific Knowledge

Hampel, Mathis

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Climate Reconstruction  
and  
the Making of Authoritative Scientific Knowledge

Mathis Lauren Hampel

A thesis submitted to King's College London in fulfilment of the requirements governing the award of the degree of Doctor of Philosophy.

King's College London  
University of London

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## *Abstract*

Because the authority of science is thought to legitimise governmental regulations to restrict the emission of so-called greenhouse gases (GHGs), in this thesis I study the making of authoritative scientific knowledge through the lens of a controversy about climate reconstruction. While controversies in climate science are typically explained with vested interests that have turned an innocent form of knowledge into the victim of the political opponent's misuse, I draw on insights from science studies to illuminate a more nuanced and symmetrical critique on climate science, the theory of anthropogenic global warming (AGW) and climate reconstruction in particular.

To that end the thesis focuses on three interconnected ideas which dominate the controversy: the idea of an objective scientific method, which places emphasis on the empirical testing of theory, the idea of an unbiased expert, which shifts my analytical focus onto norms and markers of expertise, and the overarching idea of science legitimising political programmes of action, which all of the protagonists subscribe to. First, climate reconstruction promises to be an empirical test for the scientific theory of AGW, but in the controversy over an iconic reconstruction so-called climate sceptics accuse scientists of having violated the scientific method. Second, in public investigations examining these allegations, the scientists and their critics draw on scientific norms to contest respective claims to expertise. Third, in consequence of these inquiries and the so-called 'Climategate' affair, which corroborated the critics, independent scientists re-analyse climate reconstruction: if climate science legitimises policies aiming at the restriction of GHG emissions, its authority qua science will have to be re-established. This dependence on science in difficult political decision-making puts a heavy burden on the former and obstructs the latter, and it characterises the climate change debate in the United States. Further research on the role of science in the politics of climate change would benefit from taking more explicitly political cultures into account.

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## *List of Acronyms*

AGW	Anthropogenic global warming
AMS	American Meteorological Society
APS	American Physical Society
AR4	Fourth assessment report
AR5	Fifth assessment report
BEST	Berkeley Earth Surface Temperature
BSE	Bovine spongiform encephalopathy
CCSP	Climate Change Science Program
CEI	Competitive Enterprise Institute
CFC	Chloro-fluoro-carbon
COP	Conference of Parties
CO2	Carbon dioxide
CPS	Centre for Policy Studies
CRU	Climate Research Unit
DDT	Dichloro-diphenyl-trichloroethane
EIA	Environmental impact assessment
EPA	Environmental Protection Agency
FAR	First assessment report
FoIA	Freedom of Information Act
GCM	General circulation model
GCRP	Global Change Research Program
GHG	Greenhouse gas
GISS	Goddard Institute for Space Studies
GMO	Genetically modified organism
GRL	Geophysical Research Letters
IAC	Inter Academy Council
IPCC	Intergovernmental Panel on Climate Change
IPR	Intellectual property right
ISI	Institute for Scientific Information
LIA	Little ice age
MBH98	Mann Bradley Hughes 1998
MBH99	Mann Bradley Hughes 1999
MMR	Measles-mumps-rubella
MWP	Medieval warm period
NAS	National Academy of Science
NASA	National Aeronautics and Space Administration

NCAR	National Center of Atmospheric Research
NCDC	National Climatic Data Center
NOAA	National Oceanic and Atmospheric Administration
NOX	Nitrogen oxide
NRC	National Research Council
NYT	New York Times
PCA	Principle component analysis
PNS	Post-normal science
SAR	Second assessment report
SPM	Summary for policy-makers
SO2	Sulphur dioxide
SSK	Sociology of scientific knowledge
STR	Surface temperature reconstructions
TAR	Third assessment report
UEA	University of East Anglia
UHI	Urban heat island
UNEP	United Nations Environmental Programme
UNFCCC	United Nations Framework Convention on Climate Change
USHCN	United States Historical Climatology Network
WMO	World Meteorological Organization
WSJ	Wall Street Journal



*Declaration*

I declare that this thesis is my own work and has not been submitted in any previous application for a degree.

Mathis Lauren Hampel

Date

## *Chapter One*

### *Introduction: Science and the search for political closure*

#### *1.1 'Climategate'*

In December 2009 the Parties to the 1992 United Nations Framework Convention on Climate Change (UNFCCC) gathered in Copenhagen for the 15th Conference of the Parties (COP15). Their aim was to agree on a successor to the 1997 Kyoto Protocol, which has set out binding targets for reducing GHG emissions. In many ways the UNFCCC was following the precedent set out by the 1987 Montreal Protocol on Substances that Deplete the Ozone Layer. Like the Montreal Protocol, which restricted the emission of a handful of synthetic chemicals, most notably chlorofluorocarbons (CFCs), the UNFCCC was also aiming to regulate atmospheric emissions of carbon dioxide (CO<sub>2</sub>) and a few other GHGs affecting the global climate. But the volume, variety and economic significance of these GHG emissions makes their control substantially more challenging and politically contentious than the regulation of CFCs, and so the UNFCCC followed the path set out by the Montreal Protocol in looking to base its inevitably contentious political decision-making on scientific evidence. While signatories to the Montreal Protocol were informed by its Scientific Assessment Panel (SAP), the signatories to the UNFCCC and the Kyoto Protocol invoked the authority of scientific assessments produced by the Intergovernmental Panel on Climate Change (IPCC) under the auspices of the United Nations Environmental Programme (UNEP) and the World Meteorological Organization (WMO).

For the IPCC reports (IPCC, 1990; 1996; 2001a; 2007a; 2013a) climate scientists review and assess the latest scientific climate literature at intervals of 5 to 6 years. Together with decision-makers, they then synthesise their findings in a summary for policy-makers (SPM). According to the IPCC (2001b), its SPM “provides a policy-relevant, but not policy-prescriptive, synthesis and integration of information contained within the [...] Assessment Reports.” For example, the Third Assessment Report (TAR), published in 2001, provides an assessment of new scientific information and evidence as an input for policy-makers in their deliberations about what might

constitute the “dangerous anthropogenic interference with the climate system” to which signatory states to the UNFCCC have pledged themselves to preventing (Ibid.). Based on this information politicians are encouraged to act on climate change, for example, by regulating the emission of GHGs from the burning of fossil fuels. And in the run up to Copenhagen, advocates of aggressive action to curtail global GHG emissions were pointing anxiously to the findings of the IPCC as a rationale for action. After two decades and four consensus reports the science was settled; it would demand immediate action – time was running out.

Yet a few weeks ahead of the Copenhagen Climate Conference, in November 2009 thousands of private emails between leading climate scientists were published online (EAE, 2009), and within days embarrassing excerpts from them were circulating among climate bloggers, who concluded that climate scientists had conspired in what a journalist of the British *Daily Telegraph* described as “the greatest scandal in modern science” (Delingpole, 2009). The emails were stolen from a server at the UK’s renowned Climate Research Unit (CRU) at the University of East Anglia (UEA). Covering the period 1996-2009, the emails, attributed to climate scientists who have co-authored chapters of the IPCC reports, document how they lobbied the IPCC to promote the well-known story of GHG driven climate change as described in the scientific theory of AGW and embraced by the UNFCCC Parties. In dozens of exchanges they discuss how to highlight the political relevance of their science, thereby revealing a political agenda behind the authoritative, supposedly strictly scientific reports. Was the Kyoto Protocol based on fraudulent science?

Among the many thousand emails a few conversations between ‘IPCC scientists’ suggest that for the TAR in 2001, its lead-authors ignored scientific critique of a key piece of research, unpinning its infamous conclusion that “there is new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities” (IPCC, 2001a). To make their case for AGW, the lead-authors of the IPCC chapter “Observed Climate Variability and Change” simply copied into the report the concluding remarks of a study by the US climatologist Michael Mann, who also happened to be one of the chapter’s lead-authors (IPCC, 2001a: 101):

New palaeoclimate analyses for the last 1,000 years over the Northern Hemisphere [Mann *et al* 1998, 1999] indicate that the magnitude of 20th century warming is likely to have been the largest of any century during this period. In addition, the 1990s are likely to have been the warmest decade of the millennium.

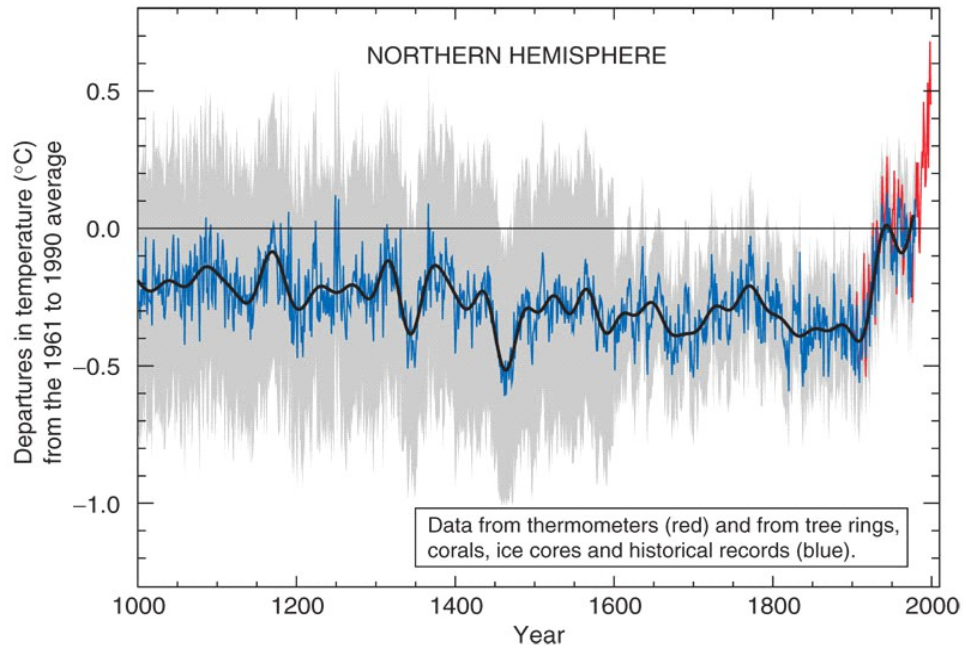
This palaeoclimate analysis or climate reconstruction, better known as the hockey stick graph owing to its distinctive shape (figure 1.1, page 12), was reproduced in the SPM and subsequently became an icon for climate-concerned campaigners.

Because the TAR's SPM (2001b) featured the hockey stick graph, politicians across the Atlantic were tempted to use it in their avowal of regulatory climate policy. In the US, the former Vice President Al Gore presented the reconstruction as part of his campaign for a political programme of action (Gore, 2006). In the UK, New Labour Prime Minister Tony Blair (2004) made reference to the graph in his climate change speech:

Over the last century average global temperatures have risen by 0.6 degrees Celsius: the most drastic temperature rise for over 1,000 years in the Northern hemisphere [...] That is the evidence [...] through the science we are aware of the problem and, with the necessary political and collective will, we have the ability to address it effectively [...] The 1987 Montreal Protocol - addressing the challenge posed by the discovery of the hole in the ozone layer - has shown how quickly a global environmental problem can be reversed once targets are agreed.

Quite evidently, these political authorities, along with many others, have interpreted the IPCC reports as policy-prescriptive. Their authoritative assessments should legitimise a target-oriented approach to the problem of climate change: in 2008 the UK Parliament passed a bill which requires the state to enforce the reduction of GHG emissions of 80% by 2050 (Crown, 2008).

Hacked and released several years after the 2001 report and the politicians' endorsements, the information from CRU suggests that "the most drastic temperature rise for over 1,000 years in the Northern hemisphere" was an artefact of politicised science. Moreover, other published research suggested that the climate was warmer in medieval times (Soon & Baliunas, 2003). Thus, opponents of emission regulations and so-called climate sceptics zeroed in on the hockey stick graph – if 20th century temperature rise had a precedent before the industrial revolution, GHGs would and could not be the culprit. And the scientists' evasion of inconvenient critique, as revealed in the emails, certainly fuelled allegations of a cover-up – it would delegitimise the Kyoto Protocol and with it decisions worth many billions of dollars.



**Figure 1.1:** The hockey stick climate reconstruction. “The Millennial Northern Hemisphere (NH) temperature reconstruction (blue) and instrumental data (red) from AD 1000 to 1999, adapted from Mann et al. (1999). Smoother version of NH series (black), linear trend from AD 1000 to 1850 (purple-dashed) and two standard error limits (grey shaded) are shown.” Source IPCC (2001a).

‘Scientific fraud exposed!’ read the headlines of an unruly media just days before politicians gathered in Copenhagen. First bloggers, then newspapers and TV stations reported on what the British journalist James Delingpole dubbed ‘Climategate’ – after the Watergate scandal over the cover-up of the misuse of governmental power under US president Nixon. Agitated climate sceptics picked a few racy emails which insinuated that climate scientists had used a “trick” to deliberately “hide a decline” (EAE, 2009: 16 November 1999) in 20th century temperatures. Though the accused scientists have always denied any wrong-doing, these revelations would be the final straw to break the climate establish, andment’s back – the emails would once and for all prove that climate scientists and their political protégées had invented a global problem so as to gain power, secure their careers and fill their wallets (cf. Avery, 2010).

Showing how scientists closed ranks and bullied their critics in order to affirm the orthodox story of human induced global warming (cf. Beck, 2011), the emails also marked a moment of public moral outcry and a feeling that trust had been breached (cf.

Leiserowitz *et al* 2013). In such a “moment of dislocation,” argues the sociologist Maarten Hajer (2009), the authority of both scientific and governmental institutions is called into question. Indeed, in the US and the UK, government officials quickly launched inquiries into ‘Climategate’. In a rare move, a scientist at CRU was ordered to testify in the British House of Commons (HC, 2010a), and in the US lead-authors of the original hockey stick reconstruction faced a series of lawsuits and even criminal investigations for fraud. For these experienced researchers and their prestigious institutions ‘Climategate’ was a truly serious matter – death threats against some of them even circulated on the internet. But was it the the greatest scandal in modern science? And did it confirm a political plot to put our blue planet in green shackles as the Czech president Vaclav Klaus (2006) once remarked? If the Kyoto Protocol and the success of the Copenhagen conference depended on science, ‘Climategate’ would surely diminish the authority of climate science in the political process.

While the emails gave compelling evidence to the politicisation of climate science, it was not the first event to have provoked this kind of accusation. Neither is climate change the first time politicians have resorted to the authority of science in order to close down a political debate – climate change exemplifies the latest in a line of high profile controversies in which political closure has proven extremely difficult. **It is the overall aim of this thesis to show that this deference to science invites the politicisation of climate science, in turn provoking a debate over its authority whose resolution the adversaries believe to be instrumental for the difficult decision-making process to continue.** And because they too envision the permeation of society by scientific rationality, several academic science students engage in that debate by prescribing the foundations of authoritative scientific knowledge. In this thesis, I apply their theories of expertise to the scientific controversy over climate reconstruction. I reflect on their usefulness as analytic tools and on their value in political debate on climate change.

This chapter continues with an introduction to the wider class of cases in which the promise of science to make one’s decisions seem incontrovertibly right has had unintended consequences.

## ***1.2 Science in the modern order***

In Western liberal nation states political closure is formally achieved by means of representative democracy: in respective national governments, the elected representatives formulate and pass legislation upon which regulations of, for instance, GHG emissions are based. And in their arguments for a particular legislation, like the UK Climate Change Act, state representatives typically invoke the authority of scientific knowledge in the hope that the electorate accepts science as an arbiter of truth. In the modern world, which is characterised by the success of the scientific enterprise, they generally do so, not least since science has helped to identify and solve problems such as the depletion of ozone.

By leaning on science as a referee in difficult political decisions, Western societies build on a centuries-long tradition. At least since Machiavelli, philosophers have promoted the idea of an objective and universally valid knowledge that transcends ideological differences between political rivals (Brown, 2009). Machiavelli claimed to speak the objective truth to the Prince precisely because he was not interested in grabbing his power. Whilst previously power had equalled truth, Machiavelli merely ‘spoke truth to power’ (cf. Wildavsky, 1979) – *ipsa scientia potestas est*, wrote Bacon. The idea that the authority of science, the quintessence of an objective and universal truth, legitimises what would otherwise be subjective political decisions is thus very modern indeed (Ezrahi, 1990). It has been invoked in innumerable political rows in which the existence of conflicting value systems has made it difficult for politicians to authoritatively close down the debate.

In arguably the first controversy involving a modern scientific element, in the Northern French town of Rouen in around 1800, aristocratic views of nature and society came into conflict with those of an emerging industrial bourgeoisie (Pepper, 1984: 127-129). The landed gentry represented a pre-revolutionary political class who saw their traditional ways of life threatened by the revolutionary capitalists and their swaths of poor workers that changed the face of good old Rouen. Their dirty factories were planted in what once was a verdant countryside, and the fumes emitted from the chimneys darkened the skies, further vexing the aristocrats. More than that, as the new classes sought political representation, the gentry found its long-held authority diminished.

In view of their declining political power, the landed class claimed to be the

victims of air pollution caused by the new chemical industries – their gases would be toxic to plants and humans – and therefore demanded the capitalists shut down their industries. Because the capitalists refused to leave the city, the Rouen city council sought a new, very modern way to mediate between them: instead of confronting the political tension that had been building up between the classes since at least the French revolution of 1789, it ordered experts from the new chemical sciences to assess the case. In what science student Bruno Latour (1993) calls an act of purification – facts (about nature) and values (about we ought to do) are separated –, the experts took samples of the emitted gases back to their laboratories in Paris where they studied their chemical properties and determined their toxicity. With these objective results in hand, they then returned to the city council to set out the facts of the matter, so that the council could base its political decision on the chemists' expert determination. The industries could stay, the council ruled, since according to the latest scientific theory their gases were found to be harmless.

Because the city council found the gentry's claims unwarranted, the latter called upon their own experts for a second opinion. Yet the city council insisted on the new chemical scientists' theory-based expertise as it represented the universal knowledge from which also the modern French nation state would draw its legitimacy. Associated with pre-modern times, when both political and epistemic authorities were united in one and the same institution or persona such as the Prince, the claims forwarded by the 'aristocrat' experts were deemed less authoritative. In the new modern order, represented by the Rouen city council and the independent experts, political authorities invoke the authority of scientific theory, which is true because it is not attributed to a person or place. Scientific knowledge is disembodied, disinterested, and "part of the public domain, shared by all and owned by none," writes the sociologist of science Robert K. Merton (1968: 588). Quite ironically, for this very reason the aristocrats refused to accept the council's decision; because chemical scientists were frequently employed by industrialists, the gentry denied the purity of their 'industrial' expertise. The chemists' conflict of interest would have surely impinged on their objectivity, or so they argued.

In this way the landed class' opposition to the money-grabbing capitalists and their dirty labour force got filtered into a purely factual question about whether fumes are damaging, i.e., they hoped that by authoritatively answering this technical question



the political one too would be resolved allowing them to return to their traditional ways of life. But since the council ruled otherwise, the political discussion became side-tracked into a yet narrower debate about the political economy of chemical experts and the purity of their knowledge. By arguing that the experts' conflict of interest and bourgeois economic being biased their knowledge claims, the aristocrats presaged a Marxist critique of capitalist science in which the cognitive value is contested via the infrastructure of scientific knowledge production (Hessen, 1971; Bernal, 2010; cf. Werskey, 2007).

Once the focus shifts to the political economy of science and the production of scientific facts, the institutions of science, including the scientists, their financial interests and wider social and economic being, as well as those involved in publishing and benefiting from scientific research, are also called into question. There is no scientific academy which does not have links to private industry or lobby groups; today scientists are on the payroll of the chemical, pharmaceutical and (bio)technology industries as well as governmental and non-governmental organisations (cf. Shapin, 2008). There is no scientific journal which has not once published science of questionable quality, and no scientist who has not, in one way or another, engaged in a broader cultural discourse about topics related to their areas of interest, whether it is toxins or carcinogens, vaccines, genetically modified organisms (GMOs) or GHG emissions. And because both the landed class and the capitalists, the left and their opponents from the right, Labour and Tories, Democrats and Republicans want science to speak truth to power, they deny each other's authority with a nod to the impurity of respective scientific knowledge. Any value judgement potentially distorts the supposedly rational process by which science informs politics.

With the success of science over the decades ever more policy-makers have sought to invoke the authority of science to legitimate their decisions and make them seem incontrovertibly right. In what sociologists have dubbed 'the linear model of science and society', in a sequential process, basic or fundamental, pure or undirected research would result in technical innovation, industrial production and public policies (Bush, 1945). The 'linear model' implies a straightforward transfer from scientific knowledge to political decision making. The 'linear model', summarises sociologist Reiner Grundmann (2009),

has an inbuilt assumption that decision making is rational. In essence, it is assumed that

decision makers will alter their practices on the basis of new information. Or at least, it is assumed that decision making should be rational, i.e. the ‘barriers’ to this mechanism should be removed. Examples of such barriers are manifold, including ignorance of practitioners, vested interests, unavailability of data, or ideological factors [...] According to the linear model, it would seem that ‘getting the science right’ is precondition for good policies. Sound science will help to put an end to ongoing political and ideological debates. Reducing scientific uncertainty leads to political consensus and ‘good policy decisions’.

Whether practicable or not, the modern idea behind the ‘linear model’ has been highly influential. In late modern times science has become a regular feature on the front pages of newspapers and makes headlines on TV and the internet. Hardly any topic of public interest passes without scientific experts having taken centre stage: they have been invited to advise politicians on the application of petrochemical pesticides and carcinogens such as DDT and benzene, the risks of measles-mumps-rubella (MMR) vaccination (cf. Leach, 2005), the health effects of tobacco smoke (cf. HSE, 1994), drugs classification (cf. Nutt, 2012), GMOs (cf. Mayer & Hill, 1996), the mysterious bee die-off, fiscal policy, even gay rights. In all of these political decisions with some (however puny) scientific element – is homosexuality genetically predetermined or nurtured? – the protagonists want to rely on the authority of science to settle their value disputes, i.e., because the various parties are committed to different ethical and ideological positions they are united in their focus on science and claim to derive the legitimacy of their policy position in one way or another from scientific evidence. The authority of science, it is understood, trumps the opponent’s ‘irrational’ value judgements.

Compared to early 19th century France, societies today have developed much more elaborate institutions and tools to facilitate decision-making. For example, in the US the Environmental Protection Agency (EPA) serves to bridge the gap between the environmental sciences and politics. Using the numerical, mechanistic, analytic language of so-called environmental impact assessments (EIAs), the regulatory agency informs politicians as to the most rational, such as the economically most viable, course of action – for example, the calculated costs of environmental pollution caused by the spraying of DDT outweigh the benefits from protecting cash crops from pests. Such an approach to decision-making has great normative force as “[i]t allows governing bodies to claim the cognitive high ground, a place from which they can be seen to be acting for the benefit of all without bowing to any particular interests of the governed,” summarises science student Sheila Jasanoff (2005: 265). By declaring a ban on the

petrochemical benzene a rational decision (cf. Jasanoff, 1987), the US EPA eventually managed to circumvent a value-laden discussion about the capitalist political economy that has allowed a few industries to benefit at the cost of the general public (cf. Hardin, 1968). In the Rouen of today, the controversy between the landed class and capitalists would surely have resulted in an EIA by the French EPA.

Working at the interface of science and policy, these regulatory agencies perform a precarious balancing act. On the one hand they must ensure that the boundary between science and politics is porous enough to allow for policy relevant interpretation of fundamental scientific knowledge. On the other hand they must insulate science from overtly political interests so as to avoid the impression of partiality – they keep science and politics apart *and* in tension with each other (cf. Jasanoff, 1990). The EPA, for instance, “has taken major steps to ensure that it carries out a program of sound science to inform Agency decisions without allowing regulatory objectives to distort scientific findings or analyses” (EPA, undated). “Major steps” refers to the mechanisms by which the products of scientific advisory processes, dubbed ‘regulatory science’, are certified. In the US, this includes open, transparent research planning, independent review of science publications, audits and site visits, public comment, judicial review and legislative oversight of the science used (Jasanoff, 1995ab). The principal requirement for openness and transparency is diagnostically American; in the US, more so than in any comparable Western liberal democracy, public accountability of scientific expertise is based on the assumption of distrust (cf. Jasanoff, 2005).

In other liberal democracies there exist similar tools (EIAs) and institutions (EPAs). Within the European Commission, for example, regulatory impact assessments are now carried out to assess the costs and benefits of all EU regulations (Torrity, 2007), and they are now used across a number of EU member states (Radaelli, 2005). Like EIAs, regulatory impact assessments also rely on the universal, objective image of science. Yet the processes by which scientific findings and policy options are linked are neither universal nor objective. They differ across (cf. Jasanoff, 2005) and within (cf. Keller, 2009) nation states resting upon factors that have more to do with accountability in terms of national democratic politics than with the quality of a universal science. Public reasoning, writes Jasanoff (2005: 249),

achieves its standing by meeting entrenched expectations about what authoritative claims

should look like and how they ought to be articulated, represented, and defended. Science, no less than politics, must fit itself into established ways of public knowing in order to gain political support—and these ways of knowing vary across well-defined cultural domains such as nation states.

In order to gain political support in the US, scientists are frequently asked to articulate, represent and defend their knowledge at the legislative stage of decision making that is in Congressional hearings. To this end, and much like in Rouen where the city council and aristocrats chose their own experts, Democrats and Republicans choose experts independently. They then put the scientists under oath and start their cross-examination, which is considered the most effective way to exercise and probe transparency. This adversarial procedure – a Congressional hearing is modelled on adversarial legal proceedings in which the majority side in Congress usually sets the agenda – is typical for how the litigious US society warrants scientific knowledge for policy-making, finds Jasanoff (2005: 263):

The U.S. accountability system owes its special flavor to the extraordinary prevalence of litigation as a means of achieving repose. In scientific as in other areas of policy disputation, the adversary process remains the dominant approach to establishing credibility. Truth, according to this template, emerges only from aggressive testing in an adversarial forum.

By contrast, in the UK the assumption of trust and mutual respect guides the relationship between scientific advisers and government. Also the demand for openness and transparency is less pronounced in the UK. And the option of subpoenaing testimony from scientists is rarely exercised. Instead, in a consensual decision, Parliament typically invites and asks for advice representatives of the Royal Society, which is recognised as the authoritative and trusted voice on scientific matters of fact. Trust is a precious resource, knows the UK Science and Technology Committee (2009: Ev21):

[T]he overarching requirement for smooth operation of the system for scientific advice to government is trust. [...] trust is earned, needs to be constantly reinforced and is a two way process. Committees have an obligation not to ambush ministers when influences other than scientific evidence become the prime drivers for decisions, just as committees do not want to be ambushed by Government criticism of their advice.

Jasanoff calls these different styles of reasoning about decisions involving science and technology ‘civic epistemologies’ (Jasanoff, 2005). These “culturally specific, historically and politically grounded, public knowledge-ways” evolve with

experience and experimentation (Jasanoff, 2005: 249; cf. Doubleday & Wynne, 2011).

And with the prominence of science in all spheres of social life inevitably come bad experiences and resistance towards decisions involving science and technology. Echoing those in the US, in the UK the mad-cow-disease (BSE) scandal has provoked calls for more openness and transparency. As a consequence, in the contentious politics of GMOs, the UK government invited both scientists and members of the public to deliberate solutions to the fears the prospect of genetically modified food in British supermarkets has triggered. During the 2003 “GMNation?” exercise, experts explained their science and its uncertainties to more or less deeply concerned citizens. While a for the UK untypical openness and public exposure to scientific uncertainty calls into question the hope modernists have invested in the boundary drawn between science and politics, it would be a promising mechanism to “[liberate] politics, law and the public sphere from their patronization by technocracy,” believes the sociologist Ulrich Beck (1992: 109). To be sure, different liberal democracies have traditionally approached decision-making involving a scientific element differently (cf. Löfsted & Vogel, 2001).

It so turns out that the modern separation of science and politics, and the reliance on the former to achieve administrative closure on political programmes of action, has had unintended consequences. Science has been transformed “from a relatively minor institution encapsulated from social influence to a major institution that influences and is influenced by other social spheres,” observe Etzkowitz and Webster (1994: 488; cf. Frank & Meyer, 2007). In all important decisions science has “[become] more and more necessary, but at the same time less and less sufficient for the socially binding definition of truth,” concludes Ulrich Beck (1992: 156). The more we want to insulate facts from values in order to make rational decisions, the more they become bound up with each other (cf. Latour, 1993).

And yet the modern idea of a universal scientific, by definition a-political authority dies hard: the promise of reducing practical questions about the good life to technical problems for experts, thereby eliminating the need for democratic deliberation and discussion of values, is all but appealing. What is more, scientists have been highly successful in shaping public discourse by bringing topics onto the political agenda: because of new scientific knowledge concerning climate change, carbon-consuming behaviours that were once tolerated and applauded are increasingly perceived as undesirable or immoral. It so seems as if need science to inform our decisions. But how

should this be done, and what do these decisions look like?

In the next section, I show how the idea behind the ‘linear model’ has been formally, at the legislation stage of decision-making, applied to the problem of climate change. **Nowhere has the model been more influential and do politicians put a higher premium on science to resolve their ideological disputes than in the US, and the controversies in climate science give further evidence to the Americanisation of the climate debate, the thesis will show.** But even though the climate controversy thrives in the US it has now taken on an almost global significance. Facilitated by the English language and internet media in which it is conducted, the debate over climate science gives support to those in other countries where US climate sceptics’ talking points have become part of the political rhetoric (e.g., Klaus, 2006 in the Czech Republic; Lawson, 2009 in the UK; Plimer, 2009 in Australia; Vahrenholt & Lüning, 2012 in Germany; Ball, 2014 in Canada). Moreover, climate change is global in nature (cf. Edwards, 2009) and a global agreement is not possible without getting the US as the world’s second largest gross emitter and largest economy on board.

### ***1.3 Climate policy-making in modern liberal nation states***

The phenomenon of human induced climate change has been well-known for at least a century (Weart, 2008) – even before the scientific theory AGW was described in the early 1900s, people were concerned, or thrilled indeed, about humanity’s impact on climate – and the engineering of climate was often hailed as yet another feat of human ingenuity (Fleming, 2010). Only from the 1960s and 1970s on has climate change been understood in predominately negative terms as potentially having an adverse impact on agricultural production and water and energy supply (NRC, 1976, 1977ab). It took another two decades for climate change to be recognised as something to be managed within the context of the modern nation state in an economically globalised world (cf. Elzinga, 1996; Miller, 2001, 2004). By no means was climate change destined to become a global issue to be tackled in the ways described in this section (cf. Howe, 2014: chapters 3 and 4).

With the end of the Cold War (and the war on terror yet to emerge) and the new internationalism of the 1990s, political and business leaders across the Atlantic saw an opportunity to make capital out of the climate crisis (cf. Boehmer-Christiansen, 1994).

The idea of a concerted effort to tackle global warming gained political momentum and bi-partisan support when the UK's conservative Prime Minister Margaret Thatcher and the Republican US president George H. Bush urged international co-operative action. "It may be cheaper or more cost-effective to take action now than to wait and find we have to pay much more later," argued Thatcher (1990). Speaking at the Rio Earth Summit, Bush (1992) noted that "[e]conomic growth provides the resources for environmental protection, and environmental protection ensures that growth is sustainable." At this Summit the UNFCCC (1992) was adopted and ratified by 195 countries. Its ultimate objective, the document stated,

is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system.

A few years later in 1997, parties to the UNFCCC agreed to tackle climate change head-on via a set of binding targets for industrial countries to reduce their GHG emissions: in the Kyoto Protocol the industrialised world agreed to reduce their emissions by specified amounts, called 'common but differentiated responsibilities', by 2012, relative to the 1990 baseline. The success of the 1987 Montreal Protocol encouraged optimism, while the example of US EPA's experiments with the trading of sulphur dioxide (SO<sub>2</sub>) and nitrogen oxide (NO<sub>x</sub>) emissions to provide a least cost solution to acid rain provided the idea of a carbon emission trading scheme.<sup>1</sup> Climate change too would be dealt with the policy tools of the neoliberal nation state: the taxation of GHGs and the so-called cap-and-trade scheme should function as incentives for nations to invest in climate-friendly energy, i.e., to modernise on an ecologically sustainable path (cf. Hajer, 1995; Bäckstrand, 2004). Seeing environmental protection and economic development as compatible, this so-called ecological modernisation "indicates the possibility of overcoming the environmental crisis without leaving the

<sup>1</sup> In the cases of acid rain and the hole in the earth's ozone layer political decisions are explicitly based on science: scientists showed how SO<sub>2</sub> and NO<sub>x</sub> emitted from factories or cars could mix well with rain and travel long distances in the atmosphere to affect rivers, soils, and wildlife far from the source of the pollution. This also holds true for ozone depletion, which scientists found to have been caused by the emission of CFCs stemming mostly from refrigerators, freezers and air conditioning systems. A depletion of the ozone layer in the lower portion of the earth's stratosphere, which absorbs most of the sun's ultra violet radiation, has been linked to an increase in skin cancer caused by that radiation. Here too the affected often lived far away from the source of the emissions. Thus, both acid rain and ozone depletion became a topic of international environmental policy and regulatory schemes which include market-based approaches, whose intention is to give polluters economic incentives to install pollution controls: every polluter is allocated or may purchase an emissions allowance for each unit of the pollutant it emits. Operators then install pollution control equipment to measure and sell portions of their emissions allowances they no longer need for their own operations.

path of modernization” (Hannigan, 1995: 183). Scientific assessments such as the IPCC’s provide the epistemic authority ecological modernisers among others would refer to. An optimistic Sir John Houghton noted in his preface to the IPCC’s First Assessment Report (FAR) in 1990 (IPCC, 1990: v):

I am confident that the [IPCC] Assessment [...] will provide the necessary firm scientific foundation for the forthcoming [...] negotiations on the appropriate strategy for response and action regarding the issue of climate change.

All the politicians had to do was to pass legislation upon which regulations and cap-and-trade policies were executed.

However enormous the task at hand, several national governments have since attempted to give legal expression to the Kyoto Protocol. In the UK, amidst heated public debate over its involvement in the war in Iraq, in 2004 Tony Blair announced national initiatives to tackle climate change.<sup>2</sup> To this end the administration ordered a cost-benefit analysis and risk assessment upon which the most rational course of action would be decided (Stern, 2006). The so-called Stern Report essentially corroborated Thatcher and Bush: it is more cost-effective to take action now than to wait and find we have to pay much more later. As a result, in 2008 the Labour government passed the Climate Change Act, which makes it the duty of the Secretary of State to ensure that by 2050 the UK will have reduced its GHG emissions 80% under the baseline of 1990. Four years later in Australia the Labour administration under Prime Minister Julia Gillard formally enforced a carbon tax that requires large businesses to purchase emissions permits. And according to Klein (2012), Germany, where the implementation of ambitious climate change policies has been strongly supported by public opinion (UBA, 2010) and is facilitated by the collapse of inefficient firms in East Germany after 1990 (OECD, 2001), is on the way to meeting its Kyoto targets.

These success stories overlook that there is ongoing political debate about new taxes and regulations. First, in Australia only a year and a half later the succeeding administration under Prime Minister Tony Abbott from the Liberal Party announced the introduction of a package of bills that would repeal the carbon tax as of 2014. Because carbon taxes are particularly unpopular in times of austerity, Abbott’s promise may well have won him the national elections. And climate sceptics celebrated about what they

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<sup>2</sup> Tony Blair’s announcement of radical climate policies came at a time when discontent over his war in Iraq grew within the population and in particular among the left. It can be argued that his climate change initiative, quite opportunistically, helped New Labour to regain popularity.



see as a confirmation of their arguments against climate science and the theory of AGW.<sup>3</sup> Second, in the UK it is by no means certain that the policies survive further administrative changes – what has been formally legitimated can be repealed or even ignored by any elected administration. And here too a handful of high-profile climate sceptics including Lord Christopher Monckton and Lord Nigel Lawson (2009) decided to leave no stone unturned so as to sink the Climate Change Act. But the 2050 target the British government set itself may well be too herculean to be feasible in the first place.<sup>4</sup> Third, in Germany the so-called *Energiewende*, whose costs are estimated to top €1 trillion, is highly contested and “full of contradictions,” writes John Rhys (2013):

[A] fundamental weakness of German energy policy [is] its inability to confront adequately the biggest single global challenges of this century – securing low carbon sources of energy to fuel modern economies, while reducing CO<sub>2</sub> emissions with urgency. The harsh reality is that in this respect Germany underscores the failure of the EU as a whole.

Also German climate sceptics sharpen their knives: In *Die kalte Sonne: Warum die Klimakatastrophe nicht stattfindet*, the German politician Fritz Vahrenholt and the geologist Sebastian Lüning (2012) asserted that climate change is driven by variations in solar activity rather than human activity.

In the US, the historically largest emitter of GHGs, the 2003 Climate Stewardship Act was introduced to Congress by John McCain (R-AZ) and Joe Lieberman (ID-CT) and reintroduced on modified terms in 2005 and 2007. If passed, the legislation would have required the EPA authority to enforce the capping of 2010 GHG emissions at the 2000 level. However, that bill never passed Congress and under the Bush Administration the EPA refused to regulate CO<sub>2</sub> as a pollutant under the Clean Air Act. This refusal prompted a number of states to file suits in federal court to force the hand of the EPA. In *Massachusetts v. Environmental Protection Agency*, 549 US 497 (2007), the Supreme Court ordered the Bush Administration to begin the

3 <http://www.climate-sceptics.com.au/> (Accessed on 12 May 2014)

4 Writes political scientist Roger Pielke, Jr., (2009): “Given the magnitude of the challenge and the pace of action, it would not be too strong a conclusion to suggest that the Climate Change Act has failed even before it has gotten started. The Climate Change Act does have a provision for the relevant minister to amend the targets and timetable, but only for certain conditions. Failure to meet the targets is not among those conditions. It seems likely that the Climate Change Act will have to be revisited by Parliament or simply ignored by policy makers. Achievement of its targets does not appear to be a realistic option”. In an update Pielke, Jr., affirms his critique: “If the UK is to hit its 2022 emissions target, then assuming a 2 percent annual GDP growth implies a rate of decarbonization of the economy of 4.4 percent per year over the next 9 years (for 1 percent annual GDP growth it is 3.3 percent and for 3 percent GDP growth it is 5.4 percent). Since the Climate Change Act was passed in 2008 the UK economy has actually decarbonized at a rate of 1.1 percent per year.” (<http://thebreakthrough.org/index.php/voices/roger-pielke-jr/can-the-british-decarbonize/>; Accessed on 11 January 2014)

administrative process to assess whether or not to regulate CO<sub>2</sub> as a pollutant under the Clean Air Act. In making that determination the EPA is required to consider the scientific evidence of the danger these emissions pose to public health and the costs of controlling those risks. Science, wrote the Administrator (EPA, 2009),

must be the backbone for EPA programs. The public health and environmental laws that Congress has enacted depend on rigorous adherence to the best available science. The President [Barack Obama] believes that when EPA addresses scientific issues, it should rely on the expert judgement of the Agency's career scientists and independent advisors. [...] EPA will stand ready to help Congress craft strong, science-based climate legislation that fulfils the vision of the President.

As the primary scientific and technical sources of the so called Endangerment Finding (EPA, 2009a), the Administrator relies on the IPCC, the US Climate Change Science Program (CCSP), the US Global Change Research Program (GCRP) and the National Research Council (NRC) (EPA, 2009c).

Unsurprisingly, the fossil fuel industry and their political protégées have been opposing emission controls. What is more, carbon taxes are hardly welcome by the large swaths of US Americans whose prosperity is built on the burning of fossil fuels. According to its opponents, the failure to consider these societal welfare benefits created by the energy sources that produce GHG emissions would undermine the rationality of the Climate Stewardship Act (see HSCCT, 2011: 83; cf. Adler, 1997). And because science forms the backbone of these sorts of calculations and risk assessments, they contest the veracity of scientific evidence presented by the IPCC, CCSP, GCRP and NRC. Congressman Michael Burgess (R-MN) exemplified this argumentation in a Congressional hearing dedicated solely to the hockey stick climate reconstruction (HCEC, 2006: 23):

We have already taken out the VOX, the POX, NOX, the SOX, the TOX. Now it is the carbon dioxide and water that are coming out of those smokestacks that has to be stopped [...] It is false to presume that a consensus exists today or that human activity has been proven to cause global warming, and that is the crux of this hearing. What we are here today to discuss is the [...] the hockey stick temperature studies [...] As the U.S. Congress and even the international policymaking bodies look to the scientific community to provide information and analysis, it is especially important to make certain that the processes are in place to ensure that we are using sound and unbiased science.

For Congressman Burgess, several fellow Republican party members and other opponents of regulations to restrict GHGs, the 2009 'Climategate' scandal was further evidence of a bias in the climate science being used to justify government action on

climate change. “The CRU information undermines [...] particularly the work of the IPCC. [...] ‘Climategate’ destroyed EPA’s basis for concluding that it could rely on the procedures for ensuring the quality, integrity and transparency of the information on which the IPCC relied [...],” read a petition to the EPA presented by a Republican-chosen expert witness in Congress (HSCCT, 2011: 91). ‘Climategate’ “appears to have contributed to an increasing disavowal of climate change and rejection of climate policy in the Republican Party, at the state and federal levels and among 2012 presidential candidates,” summarise communications experts (Maibach *et al* 2012: 289).

Ever since the science of climate change has been used to justify state regulation, opponents of that regulation have responded by trying to criticise the theory of AGW as scientifically unsubstantiated fear-mongering. In the eyes of many climate sceptics, global warming is a fear Democrats, environmentalists, leftists and ‘mobbed-up’ experts spread in order to gain political influence and power. Climate concerned commentators on the other hand blame the political impasse in the US and globally on these sceptics. “This virulent strain of anti-science infects the halls of Congress, the pages of leading newspapers and what we see on TV, leading to the appearance of a debate where none should exist,” stated the lead author of the hockey stick study Michael Mann (2014) in the *New York Times* (NYT). “Our present crisis over the rise of anti-science has been coming for a long time and we should have seen it coming,” complained historian Naomi Oreskes about climate sceptics among the Republican candidates in the presidential elections (*The Observer*, 2012). “We are sliding back into a dark era [...] and there seems little we can do about it. I am profoundly depressed at just how difficult it has become merely to get a realistic conversation started on issues such as climate change or genetically modified organisms,” added the president of the American Association for the Advancement of Science (AAAS), Nina Federoff (Ibid).

Climate policy is not the first time such accusations have been exchanged. Similar arguments dominate the politics of GMOs in which anti-GMO activists resist big capitalists controlling the world market of seeds (cf. Arjó *et al* 2013). The accusations of anti-science or scientific fraud also prevail in the politics of MMR vaccinations (cf. Flaherty, 2011), the smoking ban, DDT and so forth. In 2004 the conservative, climate sceptical Heartland Institute published “DDT: A Case Study in Scientific Fraud” in which the late entomologist Gordon Edwards (2004, cf. Kinketa,

2011) concluded:

The chemical compound that has saved more human lives than any other in history, DDT, was banned by order of one man, the head of the U.S. Environmental Protection Agency (EPA). Public pressure was generated by one popular book and sustained by faulty or fraudulent research. Widely believed claims of carcinogenicity, toxicity to birds, anti-androgenic properties, and prolonged environmental persistence are false or grossly exaggerated. The worldwide effect of the U.S. ban has been millions of preventable deaths.

In all of these controversies the political opponents claim to speak in the name of science and dispute each others' pretensions to expert authority. The overall objective of this thesis' empirical part is to introduce analytic rigour to the debate over climate science, not least because quite a few academic commentators, too, believe it to precede if not legitimise the difficult and contentious political decisions. To achieve this objective I systematically examine the adversaries' claims for and against its authority qua science.

In a first step (Chapters 3 and 4), I situate their arguments within the philosophy of science literature: here my aim is to see whether, and if so how, agreement on a universally valid and logically consistent foundation of scientific authority can help in the resolution of the so-called hockey stick controversy, which culminated in 'Climategate'. If the adversaries can agree on the principles by which authoritative knowledge is arrived at, they must – if one takes their arguments seriously – also agree on how to act upon it.

In a second step (Chapters 5 and 6), I analyse my protagonists' arguments using certain concepts from the sociology of science literature that deal with the problem of expert legitimation: here my aim is to see whether, and if so how, theoretical sociological claims about the nature of scientific knowledge qua expertise can settle the hockey stick controversy. If the adversaries can agree on the markers of expertise, they must – if one takes their arguments seriously – also agree on how to act upon what the experts say.

With the results of my empirical investigation, I return to the thesis' overall aim, which is to show that the scientisation of climate politics has provoked the politicisation of science along with a debate over its authority, whose resolution the adversaries believe to be instrumental for the difficult decision-making process to continue, i.e., because government bodies in the US more than anywhere else hand over their responsibility in difficult political decisions to 'science', thereby claiming the

cognitive high ground, political opponents question the nature of its authority.

In the next section, I rehearse important concepts in the outline of the thesis' respective analysis chapters. In the analysis chapters, I will return to that theoretical framework in more detail. I so hope to make the story flow.

#### ***1.4 The foundations of scientific authority***

When faced with difficult political decisions, modern societies habitually look to science as an arbiter of truth. As explained above, science promises a placeless and disembodied form of knowledge: it describes an objective world external to the minds of all humans; society and nature, mind and matter, subject and object are clearly divided realms, and scientific knowledge refers directly to this natural, material and in this sense objective reality. Thus, its authority qua science rests in a scientific method which allows scientists to bridge the gap between reality and representation by means of inductive reasoning. Here the scientist first observes the natural world and induces from these observations a scientific theory. The information with which the scientist assesses the truth value of the induced theory must then be derived from reports of empirical observation, called a posteriori sentences, and clearly demarcated logical statements, called a priori sentences. By following this procedure, a scientist verifies a scientific theory qua truth. Its authority is foundational, given by God or Nature, as it were. In a nutshell this is how the so-called logical empiricists of the early 20th century viewed science.

But at least since Karl Popper's 1934 (1980) treatise on *The Logic of Scientific Discovery* philosophers of science have noted that the scientific method based on inductive reasoning is not as logical as it seems. Because there is no logical end to testing – how many observations suffice to consider a theory verified? –, Popper famously replaced induction and associated verificationism with deduction and associated falsificationism. In deductive logical reasoning a theory which fails to predict a certain observation is falsified by the latter. And according to Popper, a theory that cannot potentially be proven wrong is dogma and as such no different from poetry or religion; it is not scientific. While the principle of falsification poses obvious problems for realism – if theories are scientific only if they can be proven wrong, no theory has ever described the world as it really is – it does so too for the authority of

science. In fact, science has no authority whatsoever, argues Popper (1980: 259-260):

It is all guesswork, doxa rather than epistēmē [...] Science, one might be tempted to say at times, is nothing but enlightened and responsible common sense – common sense broadened by imaginative critical thinking. But it is more. It represents our wish to know, our hope of emancipating ourselves from ignorance and narrow-mindedness, from fear and superstition. And this includes the [...] the fear of being proven wrong, or of being proved ‘inexact’, or of having failed to prove or justify our case. And it includes the superstitious belief in the authority of science itself (or in the authority of ‘inductive procedures’ or ‘skills’).

Popper’s insight must be ignored by those who insist on the indisputable authority of the scientific method. But since inductive reasoning requires scientists to consent on the terms of verification (such as what constitutes a sufficient number of observations), the authority of science can hardly be indisputable or foundational either.

The first analysis chapter of the thesis (Chapter 3) deals with the search for that foundational authority of climate science. It is an exercise in empirical philosophy of science in which I examine my protagonists’ ideas of the scientific method. Although the debates in US Congress about climate change are pitched as political debates, they are in fact drawing on these philosophical ideas about the scientific method. By putting the long and distinguished history of philosophy of science into conversation with these political debates, my aim is to clarify whether, and how, different sides actually disagree about the fundamentals of the scientific method. The chapter concludes with a characterisation of the philosophy of ‘sound science’ that has served sceptics to criticise climate science.

In the second analysis chapter of the thesis (Chapter 4), I revisit the scientific controversy on climate reconstruction. The chapter both provides the reader with necessary background information and assesses the idea of ‘sound’ climate science against the practice of climate reconstruction. In the so-called hockey stick controversy the disagreement between scientists and sceptics over the practical implementation of sound scientific principles has come to the fore: although they agree that science is the sober representation of a complex material reality, scientific closure on climate reconstruction is difficult to achieve by means of the scientific method alone; it seems as if closure requires consent and agreement among scientists, rather than strict logic. At least since Thomas Kuhn’s seminal *The Structure of Scientific Revolutions* (1962), that question of how to achieve closure has interested scholars in the sociology of scientific knowledge (SSK).

Presaged by Popper's attack on verificationism, scholars in the SSK decided not to study methodological questions but the scientists as "people with bodies, situated in time, space, culture and society, and struggling for credibility and authority" (Shapin, 2010). Methodological questions would still play a role in science, but they do not sufficiently explain how the institution of science and its experts have achieved an authoritative standing in modern societies. In the SSK, analytical focus shifted from those philosophical debates about the foundational authority of the scientific method onto the practitioners and the construction of scientific expertise, i.e., the SSK showed that scientific closure was a social as much as a cognitive achievement; scientific authority was relative, it was embodied.

The SSK may be divided into at least two traditions or what the sociologists Harry Collins and Robert Evans (2002) dubbed 'Wave One' and 'Wave Two' science studies. Taking the truth of science for granted, the aim of 'Wave One' studies (alternatively called the 'social institutional constructivist' tradition, see Sismondo, 1993) is to trace the social pressures that mould science, and to explain the falsity of theories through social bias. Representative of 'Wave One' scholarship is Robert K. Merton's *The Sociology of Science: Theoretical and Empirical Investigations* (Merton, 1968; also Mannheim, 1954). In his attempt to save science from the onslaught of totalitarian regimes, Merton was not interested in philosophical questions over the method but in how science remained independent of political pressures. Only if science was free as an institution, Merton held, would its practitioners remain authoritative and their advice valuable to decision-makers. Failures of scientific advice can thus be explained by reference to vested interests and political ideology that have turned an in principle innocent form of knowledge into a chaste victim of misuse.<sup>5</sup> Such ideology of science and its concomitant belief in science-led social progress has been highly influential in the 1950s and 1960s (Habermas, 1971); it continues to appeal to decision-makers.

But if social bias precipitated the demise of an established but ultimately false theory, would social factors not also account for a theory's truth? And how is it possible

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5 Arguably the most famous example is that of Soviet biologist Trofim Lysenko who rejected Mendelian genetics that described Darwinian competition between individuals as the natural law. That idea conflicted with the staunch Communist belief in the superiority of mutual assistance between individuals. Accordingly, Lysenko's research was heavily biased towards the idea of co-operation. Communist rulers then used his supposedly objective science as convenient legitimation for political intervention in the Soviet agricultural sector. And because this application led to mass famine and starvation, Lysenko's name still acts as a deterrent in many a scientific controversy, including that over AGW.

that people in all times and places, including our own, have believed in scientific theories that later turned out to be false? These are the fundamental questions asked by the then new SSK or ‘Wave Two’ science studies (e.g., Barnes & Bloor, 1982). In the late 1970s the new SSK criticised structuralist sociology like Merton’s for its historicism – as if science inevitably converged to truth, destined to progress. Truth, argued David Bloor (1991), cannot be the cause of any belief’s credibility – truth formation and scientific consensus too would need a historical and sociological explanation. Their response to this methodological dilemma was to adopt what they termed the ‘Strong Programme’ in the SSK. Instead of treating scientifically accepted truth as self-evident and only falsehood as amenable to sociological explanation, the ‘Strong Programme’ treated them symmetrically; it assumed an agnostic stance about the truth and falsehood of science. The sociologists’ task is to describe how scientists qua experts demarcate themselves from the non-expert (cf. Gieryn, 1983; Gieryn, 1999; Shapin & Schaffer, 1985). In this way they seek to avoid the trappings involved in previous accounts of science by revealing that “there is no guaranteed path from the material world to scientific truths, and no method identifies truths with certainty” (Sismondo, 2004: 43).

Through the lens of the SSK, in the third analysis chapter of the thesis (Chapter 5) I examine how the protagonists in the hockey stick controversy demarcate their proclaimed expertise from that lay knowledge ascribed to others. I describe the various mechanisms and discourses by which they demarcate science from politics or pseudo-science so as to secure their privileged status among ways-of-knowing. Once again we find these discussions at the legislative stage of decision-making in US Congressional hearings where the scientists have been cross-examined and scrutinised to an extent that makes the basis of their claims to expert knowledge particularly clear. The aim of this chapter is to put these debates into conversation with ‘Wave One’ and ‘Wave Two’ science studies as well as a theory (‘Wave Three’) recently proposed by Collins & Evans (2002).

In the fourth and last analysis chapter of the thesis (Chapter 6), I describe and analyse new, open and transparent, attempts to close the scientific debate on climate reconstruction against a certain theory of expertise, which the philosophers of science Funtowicz and Ravetz (1993) dubbed ‘post-normal science’ (PNS). Other than the ‘Strong Programme’, which argues that the authority of science rests on boundaries and



the affirmation of exclusion, and Collins & Evans, 2002, who want to prescribe boundaries to save its authority in decision-making, PNS negates the existence of boundaries. Funtowicz and Ravetz insist that anyone with a stake in political decisions with a scientific element should be allowed to participate in the making and reviewing of scientific knowledge. Because science has become so pervasive, decisions have become so urgent, and publics have a stake – they will have to pay carbon taxes, – the demand for openness seems only reasonable. What is more, an inscrutable climate science, as revealed in the ‘Climategate’ email disclosures, has resulted in a loss of trust in institutions and decisions whose authority would depend on science’ authority qua science.

Will openness and transparency help in the resolution of the controversy and the authorisation of contentious climate policy? With the answer to this question, in the conclusion of the thesis (Chapter 7), I shall also be able to answer whether it is the violation of the scientific method, the violation of certain scientific norms, or the implications of accepting the science as legitimation of public policy which provoked ‘Climategate’. In other words, does the problem rest within the science or within the politics of climate change? If it rests within science, philosophers should engage with open questions surrounding the scientific method, and sociologists of scientific knowledge should elaborate more deeply on a theory of expertise or else demand openness and transparency in every aspect of climate science. If, however, the problem rests within the politics of climate change, politicians need to search for an alternative course of climate action. In that case, we sociologists of science need to ask different questions and use different conceptual frameworks to answer them.

## *Chapter Two*

### *Methodology*

#### *2.1 How to study scientific controversies?*

At least since Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), scientific controversies have been an important part of the sociology of science; they offer opportunities to study the making (Knorr-Cetina, 1995:140) and unmaking (Yearley 1995, 465) of authoritative scientific knowledge. Using historical examples such as the Copernican Revolution – initiated by Copernicus, Newton unified the advances of Kepler and Galileo, – Kuhn showed how inconsistencies in an established theory have led to significant changes in the basic assumptions within a ruling theory. Inconsistencies within the geocentric “paradigm” led Kepler on the path of the heliocentric “paradigm”; a scientific revolution unfolded with Galileo's observations, and the scientists after him, including Newton, could not ignore the new theory since it explained more observations than the previous one. A new “paradigm” had been firmly established and all scientists after Newton worked within its theory of gravity – until Einstein came along and offered a more comprehensive explanation in the form of relativity.

In the transition period from one accepted theory to a more comprehensive one, scientists engage in controversies about the elements constituting the paradigm. In these scientific revolutions Kuhn observed changes in both the cognitive content and the social fabric of science: since older scientists are reluctant to change their minds it is usually younger scientists who challenge the established theory. A scientific revolution, as Kuhn described it, is at the same time a social revolution.

In line with Kuhn, the ‘Strong Programme’ has focused quite explicitly on the role of social factors in scientific controversies. Since people are prone to believe in what may turn out to be false theories, they have been interested in “the socially contingent manner in which the objects of science are constructed and knowledge about them is socially validated,” summarises Demeritt (2006: 455). They found that scientific theories are often either welcomed or rejected based on the onlookers' world views. For example, whilst Darwinian natural selection rang true in imperialist Britain,

it was denounced by communist onlookers, who would highlight the role of cooperation, not competition, between species. Quite evidently, wider socio-political controversies are reflected in scientific ones (cf. Shapin, 1975).

In this thesis the scientific controversy surrounding climate reconstruction provides an excellent opportunity to study the making of scientific truth. If the latter is as much a cognitive as a social achievement, we can expect to see conflicts over both the cognitive content of climate reconstruction and its social fabric, such as the cultural authority of its practitioners. Before I describe my data and methods of analysis, in the next section I review the existing body of scholarship on the climate controversy and locate gaps in the literature.

## ***2.2 Sociologies of climate science***

In the SSK, the ‘Strong Programme’'s social constructivism has become a methodological principle for the empirical study of knowledge production. In the analysis of climate science, however, few have followed David Bloor's advice to remain agnostic about scientific truth. Arguably because an agnostic stance on climate science would serve the coalition of climate sceptics, “[s]ocial scientists are [...] reluctant to turn their deconstructive frameworks loose on IPCC-sanctioned climate science, scientists, and assessment processes,” suspects Myanna Lahsen (2013). Without much scrutiny sociologists have, by and large, accepted or even endorsed the consensus presented by mainstream scientists (Lever-Tracy, 2008; Giddens, 2009; Oreskes & Conway, 2010; McCright, 2011; McCright & Dunlap, 2010; 2011; Dunlap & McCright, 2011; Elsasser & Dunlap, 2013; Brulle, 2013; Lewandowsky *et al* 2013). As Grundmann and Stehr (2010: 3) aptly note,

[t]he inherent alarmism in many social science contributions on climate change merely repeats the central message provided by mainstream media. It is curious that little guidance is provided in terms of what could be done to deal with climate change, or at least pointing out what options we have. Can sociology deliver on this? One would expect substantial proposals besides the exhortation to listen more to what the natural sciences have to tell us.

A popular vantage point for such an alarmist and partisan academic scholarship is the sceptics' political economy and the multi-million dollar campaign of climate denial. These analysts, many climate scientists and their political protégées have dismissed the sceptics' scientific critique with a nod to its smoothly oiled anti-climate

change PR machine led by the fossil fuel industry and its conservative political servants (cf. Hoggan & Littlemore, 2009; Brulle, 2013). Because some sceptics have indeed given in to the temptation of industry money (*The Guardian*, 2011), the historians Naomi Oreskes and Erik Conway (2010) dismiss their critique on mainstream climate science. Serving as the fossil fuel industry's expert front group, these sceptical scientists would vocally distort the truth as it suits their free-market ideology and fills their wallets (cf. McCright & Dunlap, 2010; 2011; Dunlap & McCright, 2011).

As important as this critique is, it does not satisfactorily explain why a quite heterodox contrarian movement stubbornly resists the putative authority of mainstream climate science. Not each and every sceptic is in denial for personal profit (cf. Hulme, 2009; chapter 6; Norgaard, 2011; Kahan *et al* 2011). And why should climate scientists be resistant to such ulterior motives. That argument can easily be turned around as sceptics have spotted behind climate science an ideology powered by the rising renewable energy industries. And the emails certainly reveal the scientists' attempts to produce facts that fit and serve this environmentalist agenda.

Therefore, in a second line of critique, climate-concerned scholars dismiss the sceptics' epistemic markers of expertise. Lacking what Oreskes and Conway (2010) deem appropriate scientific qualifications, sceptics are said to know little about climate science. And indeed, many prominent sceptics are physicists who have no formal training in climatology but later in their career became interested in climate science (cf. Lahsen 2008). Equally revealing, at least in Oreskes and Conway's view, is the contrarians' failure to publish in the peer-reviewed climate scientific literature. In an essay published in the journal *Nature*, Oreskes (2004) argues that none of the 928 scientific articles she surveyed disagrees with the IPCC consensus position that "[m]ost of the observed warming over the last 50 years is likely to have been due to the increase in greenhouse gas concentrations" (IPCC, 2001c: 21).

Yet these too may prove rather weak-kneed arguments against the authority of climate sceptics. When investigative journalist Donna LaFramboise (2011) speaks of the IPCC as *Delinquent Teenager Mistaken for the World's Top Climate Expert*, she employs many of the same arguments against mainstream climate scientists. She notes that quite a few IPCC authors are yet little credentialed PhD students, and that a substantial part of its referenced literature is not peer-reviewed. Bjurström and Polk (2010) too found that out of 14,000 references in the IPCC 2001 report, 38% referred to

books, conference proceedings and grey literature. As Tora Skovdin (2000: 414) presciently remarked, “[u]sing information from non-published sources may compromise the scientific authority the IPCC has gained over the years it has been in operation” (cf. Hulme & Mahony, 2010).

As representatives of the partisan divide on climate change Naomi Oreskes’ and Donna LaFramboise’ analyses exemplify the problems social institutional constructivist or ‘Wave One’-type critique can entail. If used to deny the other side’s epistemic authority, it quickly loses its punch, leading to an irresolvable stand-off between convinced and sceptics in which the analysts preach to their own choirs (cf. Kahan *et al* 2011).<sup>6</sup> In particular after the revelations of ‘Climategate’ the arguments against biased sceptics can be easily returned: In his take on ‘Climategate’, the sociologist Reiner Grundmann (2013) concludes that the involved scientists had acted out of self-interest; his analysis also serves as a reminder or warning to scientists who have a reputation to defend: by virtue of their office they are ethically obliged to preserve the authority of science.

With a few exceptions (e.g., Demeritt, 2006; Ryghaug & Skølvold, 2010; Grundmann & Stehr, 2010; Beck, 2012; Grundmann, 2013) analysts have studied the climate science controversy from a barely disguised partisan point of view. To date (2014) only science scholars Marianne Ryghaug and Tomas Moe Skølvold have followed the ‘Strong Programme’ taking an agnostic stance on scientific truth. In the tradition of so-called artefactual constructivism (cf. Callon, 1986; Latour, 1987; 1988; 1999; Pinch & Bijker, 1987), Ryghaug and Skølvold (2010) describe how climate scientists tried to convince their colleagues of their proposed facts’ truthfulness. Using the scientists’ emails (EAE, 2009) as a data source, they show how the experts of what Harry Collins (1981) called the “core-set” of scientists struggled to persuade critical colleagues of the methodological choices in the hockey stick climate reconstruction. Ryghaug & Skølvold conclude that climate scientists must have felt “significant

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6 Taken from there the analysis of climate scepticism becomes a phenomenon to be studied in the public engagement process rather than in the ‘republic of science’ (Krosnick *et al* 2006; Boykoff, 2007; Boykoff & Boykoff, 2007; Carvalho, 2007; Nisbet, 2009, Nerlich *et al* 2010, Gavin & Marshall, 2011). Communication scholars thus stress a need for improved communication and public engagement efforts (Maibach *et al* 2012). Others again argue that the public engagement process via communication efforts have a minor influence, but are “dwarfed by the effect of the divide on environmental issues in the political elite” (Brulle *et al* 2012). Sociologist Robert Brulle and colleagues conclude that “any communications strategy that holds out the promise of effectiveness must be linked to a broader political strategy.” For the purpose of all these studies climate science is understood as a no-brainer (see Lahsen, 2008 for a rare exception).

political pressure” to express the “voice of climate science” as unified voice, or as a “nice tidy story about unprecedented warmth” (p. 302, see Grundmann, 2012 for a critique). Apparently that pressure influenced the scientists’ methodological choices.

A typical critique on any analyses of the climate controversy, including on Ryghaug & Skølsvold’s ‘Wave Two’ account, concerns the disregard for the role of the scientific method. Whilst ‘Wave One’ analyses take its authority for granted, the ‘Wave Two’ generally ignores or merely mentions in passing the scientific method and its concomitant common-sense realist stance on truth (cf. Sismondo, 1993). This conventional image of science people refer to when they seek its authority is viewed as outdated and little worthy of discussion: “In the long run,” suggest Ryghaug and Skølsvold, “scientists may be better served by greater openness with respect to the actual practice of science, rather than upholding the conventional image of cool, restricted display of instrumental rationality” (2010: 304). In a similar vein, Brigitte Nerlich (2010) concludes her study of ‘Climategate’ arguing for a “...public understanding of modern science [...] based [...] on appreciating the nature of uncertainty and the impossibility, even undesirability, of establishing universal truths.” To no surprise, climate scientists have received such statements with puzzlement and discontent. Most of the scientists who venture into the world of constructivist social science “come away at best confused and at worst shaking their heads at what they often see as self-cited social theories devoid of real world knowledge of what scientists do,” remarked the late climate scientist Stephen Schneider (2001: 338). If scientists did not believe at least in the possibility of establishing universal truths via instrumental rationality, they would hardly practice science.

This ‘ignorance’ has failed analysts in as far as they have not explained why sociological studies of climate science may be important. Thus, they have been criticised by both mainstream climate scientists and climate sceptics, as neither of them wants their (dis)belief in AGW explained as social construction. Scientists are not happy with accounts describing them as a ‘bullying tribe’ but seek to illuminate the context which provided a fertile ground for ‘Climategate’ to happen (cf. Skrydstrup, 2013). And from the sceptics’ point of view, the climate scientists have not merely violated an ethos by excluding them, or misrepresented scientific practice, but meddled with the scientific method, to say the least. Other than the existing sociological accounts, in this thesis I take the arguments against mainstream climate science, and

climate reconstruction in particular, seriously – without the sceptics’ critique we would not have known of the scientific controversy – but remain agnostic about their truth value. Before we delve into the scientific debate in Chapters 3 and 4, in the remaining sections of this chapter, I describe the sources from which I gathered data as well as the methods by which I processed and analysed them.

### **2.3 Data sources**

In controversy studies the sociologist usually sifts through the scientists’ notebooks, holds interviews and follows the actors into their laboratories, where they closely observe how they reason, negotiate, persuade and convince, reject or confirm their colleagues’ criticism (Latour & Woolgar, 1979).

In the hockey stick controversy this has proven to be quite a difficult task. As the waves after ‘Climategate’ had not calmed enough by then, my request for interviews with whom I consider the main protagonists, notably Michael Mann, were almost unanimously ignored or turned down. Due to the subject’s sensitivity, it is also highly unlikely that interviewees would have answered complaisantly. Some principals I approached at side-events such as conferences were indeed reluctant to go on the record and insisted on informal conversations preferably not to be quoted from. Unsurprisingly, climate sceptics were more willing to engage with me. But these conversations, mostly held on Skype and via email, offered little information different from the one available on easily accessible sources such as blogs. I eventually decided against using interview material; only twice, in Chapter 5 on pages 112 and 122, do I use excerpts from interviews since I could not find comparable information on publicly accessible sources.

For two reasons I do not see the lack of any such data as a shortcoming. First, other sources such as the infamous emails (EAE, 2009) offer much better and most of all un(self)censored data. Showing actual comments in real time, rather than ex-post interview reflections offered to justify their actions, the email conversations are arguably much closer to the revealed preferences of the climate scientists and their critics. As such they are less prone to self-justifying rationalisation in front of a critical sociologist of science (or the news media). In fact, with the emails in hand, the sociologist can participate in the scientific controversy as the proverbial fly on the wall

with access to ‘oral’ communication not made for public consumption (cf. Traweek, 1988: 117).

The material hacked from CRU consists of 1,073 text files containing e-mails as well as nearly 3,500 other documents, which I have not dealt with. The first e-mails date back to March 1996; the last was sent on November 12th, 2009. Many more emails were stolen but it is thought that the perpetrator chose to release only those which contained certain key words. In these emails are recorded conversations between dozens of climate scientists most of who have been involved in the production of IPCC assessment reports. Therefore many conversations show scientists discussing (their) scientific research and its representation in IPCC assessment reports. They are shown to struggle with the task of both agreeing on the basic facts and turning them into policy-relevant findings, that is, to insulate science from political influences. Often they failed in this most precarious task, giving credence to claims from sceptics about a politicised scientific environment. However, most emails do not directly deal with the scientific controversy, nor with the IPCC, but show scientists and administrators discussing grant applications, media relations and administrative work. In comparably few conversations the scientists actually deal with climate sceptics and the question of how to respond to their often openly hostile critique.

The authenticity of the stolen CRU material is difficult to verify; they do not exist in a formally published version, nor can they be accessed from officially certified sources; they can only be downloaded from non-governmental websites. A similar qualification applies to other data stemming from non-official publicly available sources. But the fact that none of the involved scientists, nor their institutions, have denied the veracity of the emails strongly suggests that these are indeed my protagonists’ opinions and not simply made up. Bearing this lack of official certification in mind, I consider these data verified. I accessed the e-mails online via a blog run by the climate sceptic Jeff Condon. On November 19th, 2009, his blog *noconsensus.wordpress.com* first published excerpts from a 62 megabyte file that had anonymously been sent to him.

Second, I do not see the lack of interview data as a shortcoming because in the making of authoritative scientific knowledge the scientists’ public rationalisation is at least as important as the construction of scientific objects behind closed doors. In *Science on Stage: Expert Advice as Public Drama* the sociologist Stephen Hilgartner



(2000) makes this point by distinguishing between backstage and front-stage processes. Backstage, scientists engage in debates about data and methodological questions, whilst front-stage their aim is to make the construction of the objects of science look like mere discoveries. In front of a sceptical audience, this public rationalisation is arguably more important than esoteric methodological questions. Therefore, the public record where scientists and sceptics give an account of themselves provides another valuable source of data.

The formal public record stems primarily from US Congressional but also from a UK Parliamentary hearing on 'Climategate'. As gestured at in the introduction, these are important venues on which scientific knowledge is warranted as public truth fit for decision-making. And because politicians look at science to authorise their political programmes, which meet greatest opposition at that legislative stage, the supposedly legitimating knowledge is meticulously scrutinised. For practical reasons I identified and chose only hearings that in one way or another addressed the science of climate reconstruction. Whilst at least five hearings in the US covered the scientific debate (SCEPW, 2003; 2005; HCEC, 2006; SCCST, 2007; HCSS, 2010; HSCCT, 2011), in the UK only one hearing dealt explicitly with the issue of climate reconstruction (HC, 2010a; see table 2.1).

Further publicly available material came from policy and technical documents, floor speeches, notes submitted to Congress and Parliament, published accounts of 'Climategate' by both climate sceptics (e.g., Mosher & Fuller, 2010; Montford, 2010; 2012) and climate scientists (e.g., Mann, 2012), as well as interviews in newspapers, climate blogs and other social media platforms; in principle any published statement related to the controversy is data (cf. Charmaz, 2006). To this end I did an extended but unstructured media review sifting through online newspapers and blogs; both climate sceptics and mainstream scientists run their own blogs on which they authorise and contest each other's knowledge claims, front-stage as it were. And because on the internet the principals appear much more relaxed than on formal venues, where they have to swear an oath and there is a physically present audience that needs convincing, these blogs have proven a most valuable source of data (see table 2.2). On blogs scientists and their critics engage with commentators in interview-like situations during which they are more likely to offer a private opinion than in formal settings; they are in control of their audience and can decide whose arguments they want to deal with (and

how) and whose to exclude.

A last important source of information was the scientific literature on climate reconstructions. Through the course of my research, readings and interactions, I have developed enough of what Collins and Evans (2007: 14) call “interactional expertise,” that is, “the ability to master the language of a specialist domain in the absence of its practice” so as to engage with experts intelligently without necessarily being able to do the specialist work myself. This has enabled me to understand the contentious scientific issues debated in the controversy, despite not possessing the “core-set” expertise actually needed to practice in or contribute to the advancement of those specialist fields.

The time period covered by the thesis is ca. 18 years starting with the first of the unlawfully released emails (March 1996), and ending with the publication of the IPCC fifth assessment report (AR5) in April 2014 (IPCC, 2013a). The deadline of the thesis, April 2014, meant that later developments were not included.

**Table 2.1: Public hearings on climate reconstruction in the US and the UK**

<i>Title</i>	<i>Date</i>	<i>Location</i>	<i>Committee</i>	<i>Chairs</i>	<i>Key Witnesses</i>
Climate History and the Science Underlying Fate, Transport, and Health Effects of Mercury Emissions	July 17, 2003; 108th Congress	US Senate	Committee of Environment and Public Works	Senator James Inhofe (R-OK), Republican Majority	Dr. David R. Legates <sup>1</sup> Dr. Leonard Levin <sup>2</sup> Dr. Michael Mann <sup>3</sup> Dr. Gary Myers <sup>4</sup> Dr. Deborah Rice <sup>5</sup> Dr. Willie Soon <sup>6</sup>
The Role of Science in Environmental Policy Making	Sept. 28, 2005; 109th Congress	US Senate	Committee of Environment and Public Works	Senator James Inhofe (R-OK), Republican Majority	Dr. Richard Benedick <sup>7</sup> Dr. Michael Crichton <sup>8</sup> Dr. William Gray <sup>9</sup> Dr. Donald Roberts <sup>10</sup> Dr. David Sandalow <sup>11</sup>
Questions Surrounding the ‘Hockey Stick’ Temperature Studies: Implications for Climate Change Assessments	July 19 and 27, 2006; 109th Congress	US House of Representatives	Committee of Energy and Commerce, hearing before the Subcommittee on Oversight and Investigations	Committee Chair: Joe Barton (R-TX) Subcommittee Chair: Ed Whitfield, (R-KY), Republican Majority	Dr. Edward Wegman <sup>12</sup> Dr. Gerald North <sup>13</sup> Dr. Thomas Karl <sup>14</sup> Dr. Thomas Crowley <sup>15</sup> Dr. Hans von Storch <sup>16</sup> Mr. Steven McIntyre <sup>17</sup> Dr. Michael Mann <sup>18</sup> Dr. John Christy <sup>19</sup> Dr. Ralph Cicerone <sup>20</sup> Dr. Jay Gulledge <sup>21</sup>
Climate Change Research and Scientific Integrity	Feb. 7, 2007; 110th Congress	US Senate	Committee of Commerce, Science, and Transportation	Daniel K Inouye (D-HI), Democrat Majority	Dr. Richard Anthes <sup>22</sup> Dr. William Brennan <sup>23</sup> Dr. Thomas Knutson <sup>24</sup> Dr. James Mahoney <sup>25</sup> Dr. Rick Piltz <sup>26</sup> Dr. Sherwood Rowland <sup>27</sup>

A Rational Discussion of Climate Change: The Science, the Evidence, the Response	Nov. 17, 2010; 110th Congress	US House of Representatives	Committee of Science and Technology, hearing held before the Subcommittee on Energy and Environment	Committee Chair: Bart Gordon (D-TN) Subcommittee Chair: Brian Baird (D-WA), Democrat Majority	Dr. Ralph Cicerone <sup>28</sup> Dr. Richard Lindzen <sup>29</sup> Dr. Gerald Meehl <sup>30</sup> Dr. Heidi Cullen <sup>31</sup> Dr. Patrick Michaels <sup>32</sup> Dr. Benjamin Santer <sup>33</sup> Dr. Richard Alley <sup>34</sup> Dr. Richard Feely <sup>35</sup> Rear Admiral David Titley <sup>36</sup> Mr. James Lopez <sup>37</sup> Mr. William Geer <sup>38</sup> Dr. Judith Curry <sup>39</sup>
Climate Change: Examining the Processes Used to Create Science and Policy	March 31, 2011; 110th Congress	House of Representatives	Committee on Science, Space, and Technology	Ralph M. Hall (D-TX), Democrat Majority	Dr. Scott Armstrong <sup>40</sup> Dr. Richard Muller <sup>41</sup> Dr. John Christy <sup>42</sup> Mr. Peter Glaser <sup>43</sup> Dr. Kerry Emanuel <sup>44</sup> Dr. David Montgomery <sup>45</sup>
The disclosure of climate data from the Climatic Research Unit at the University of East Anglia	March 1, 2010; 89th Cabinet of the UK	UK House of Commons	Science and Technology Committee	Phil Willis, (Liberal Democrats), Labour Government	Lord Lawson of Blaby <sup>46</sup> Dr. Benny Peiser <sup>47</sup> Mr. Richard Thomas <sup>48</sup> Dr. Edward Acton <sup>49</sup> Dr. Phil Jones <sup>50</sup> Sir Muir Russell <sup>51</sup> Sir John Beddington <sup>52</sup> Dr. Julia Slingo <sup>53</sup> Dr. Bob Watson <sup>54</sup>

- 1 Center for Climatic Research, University of Delaware
- 2 Electric Power Research Institute
- 3 Department of Environmental Sciences, University of Virginia
- 4 Department of Neurology, University of Rochester Medical Center
- 5 Maine Department of Environmental Protection
- 6 Smithsonian Center for Astrophysics, Harvard University
- 7 US National Council for Science and the Environment
- 8 Author, medical doctor
- 9 Department of Atmospheric Science, Colorado State University
- 10 Division of Tropical Public Health, US Department of Preventive Medicine and Biometrics,
- 11 Environment and Energy Project, The Brookings Institution
- 12 Center for Computational Statistics, George Mason University
- 13 Department of Atmospheric Sciences, Texas A&M University
- 14 National Climatic Data Center, NOAA
- 15 Department of Earth Science, Duke University
- 16 Institute for Coastal Research, GKSS Research Center, Germany
- 17 Mining consultant, Toronto, Ontario, Canada
- 18 Earth System Science Center, The Pennsylvania State University
- 19 Earth System Science Center, University of Alabama in Huntsville
- 20 National Academy of Sciences (NAS)
- 21 Pew Center on Global Climate Change
- 22 University Corporation for Atmospheric Research (UCAR)
- 23 US Climate Change Science Program, NOAA
- 24 Geophysical Fluid Dynamics Laboratory, NOAA
- 25 Environmental Consultant
- 26 Climate Science Watch, Government Accountability Project
- 27 Department of Chemistry and Earth System Science, University of California, Irvine
- 28 National Academy of Sciences (NAS)
- 29 Department of Earth, Atmospheric and Planetary Science, MIT
- 30 National Center for Atmospheric Research (NCAR)
- 31 Climate Central
- 32 Cato Institute

- 33 Lawrence Livermore National Laboratory, University of California Berkeley  
34 Department of Geosciences and Earth and Environmental Systems Institute, Penn State University  
35 Pacific Marine Environmental Laboratory, NOAA  
36 Oceanographer and Navigator of the U.S. Navy  
37 US Department of Housing and Urban Development  
38 Center for Western Lands, Theodore Roosevelt Conservation Partnership  
39 Department of Earth and Atmospheric Sciences, Georgia Institute of Technology  
40 The Wharton School of Economics, University of Pennsylvania.  
41 Lawrence Livermore Laboratory, University of California Berkeley  
42 Earth System Science Center, University of Alabama in Huntsville  
43 Troutman Sanders Limited Liability Partnership (LLP)  
44 Department of Earth, Atmospheric and Planetary Science, MIT  
45 Independent economist and consultant  
46 Global Warming Policy Foundation  
47 Global Warming Policy Foundation  
48 UK Information Commissioner  
49 University of East Anglia, Vice-Chancellor  
50 Climatic Research Unit (CRU)  
51 Independent Climate Change E-Mails Review  
52 Royal Society, Government Chief Scientific Adviser  
53 Met Office  
54 UK Department of Environment, Food and Rural Affairs (DEFRA)

**Table 2.2: Selected climate blogs**

<i>Blog</i>	<i>Operators</i>	<i>url</i>
Air Vent	Jeff Condon	<a href="http://noconsensus.wordpress.com">http://noconsensus.wordpress.com</a>
Climate Audit	Steven McIntyre	<a href="http://climateaudit.org">http://climateaudit.org</a>
Wattsupwiththat	Anthony Watts	<a href="http://wattsupwiththat.com">http://wattsupwiththat.com</a>
Real Climate	G. Schmidt, E. Steig, S. Rahmstorf, M. Mann, R. Benestad, R. Bradley, R. Pierrehumbert, D. Archer	<a href="http://www.realclimate.org">http://www.realclimate.org</a>
Bishophill	Andrew Montford	<a href="http://bishophill.squarespace.com">http://bishophill.squarespace.com</a>
Steven Mosher's blog	Steven Mosher	<a href="http://stevemosher.wordpress.com">http://stevemosher.wordpress.com</a>
Skeptical Science	John Cook	<a href="http://www.skepticalscience.com">http://www.skepticalscience.com</a>
Climate Depot	Marc Morano	<a href="http://www.climatedepot.com">http://www.climatedepot.com</a>
Desmog Blog	James Hoggan	<a href="http://www.desmogblog.com">http://www.desmogblog.com</a>
Climate Progress	Joe Romm	<a href="http://thinkprogress.org/climate/issue">http://thinkprogress.org/climate/issue</a>
Dot Earth	Andy Revkin	<a href="http://dotearth.blogs.nytimes.com/author/andrew-c-revkin/">http://dotearth.blogs.nytimes.com/author/andrew-c-revkin/</a>
Climate Etc.	Judith Curry	<a href="http://judithcurry.com/">http://judithcurry.com/</a>
Die Klimazwiebel	H. von Storch, R. Bray, R. Grundmann, W. Krauss, E. Zorita, B. Hünicke	<a href="http://klimazwiebel.blogspot.com">http://klimazwiebel.blogspot.com</a>
Bob Tisdale – Climate Observations	Bob Tisdale	<a href="http://bobtisdale.wordpress.com">http://bobtisdale.wordpress.com</a>
Climate Skeptic	Warren Meyer	<a href="http://www.climate-skeptic.com">http://www.climate-skeptic.com</a>
Junk Science	Steve Milloy	<a href="http://junkscience.com">http://junkscience.com</a>
Climate Science: Roger Pielke Sr.	Roger Pielke Sr.	<a href="http://pielkeclimatesci.wordpress.co">http://pielkeclimatesci.wordpress.co</a>

Stoat	William Connolley	<a href="http://scienceblogs.com/stoat">http://scienceblogs.com/stoat</a>
Talbloke's Talkshop	Tim Channon	<a href="http://talbloke.wordpress.com/about-tim">http://talbloke.wordpress.com/about-tim</a>

## ***2.4 Data processing and data analysis***

Although the scientists have not denied the emails' veracity, they complain that climate sceptics have cut, copied and pasted certain statements in order to accuse them of fabricating evidence for AGW. Indeed, certain statements and notably the “trick” to “hide a decline” (EAE, 2009: 16 November 1999) in 20th century temperatures were immediately singled out, circulated widely, and even became the topic of a 268 page long treatise (Montford, 2012). The selection of a few among a very heterodox set of conversations by what has been dubbed the ‘hockey team’ has also been taken up by a sensationalist and opportunist (and mostly anglophone) news media. In the US, *Fox News* cited the most racy e-mails to insinuate “a possible conspiracy to distort science for political gain” (*Fox News*, 2009). In the UK the yellow press too cited from ‘the damning emails’, and though much less extensively, the story has also been covered in its quality press. Shocked by the revelations, *The Guardian's* environmentalist writer George Monbiot demanded the resignation of CRU's director, Phil Jones.

The “trick” to “hide the decline” in particular fuelled allegations of scientific fraud. But like many other excerpts it too has to be understood in context, which can hardly emerge from a single email alone. In fact, the scientists argue that the emails themselves have to be understood in the wider context of climate denial, which they have tried to illuminate ever since. In their view, the ‘dirty laundry’ has been de-contextualised and re-contextualised to fit the sceptics' unsubstantiated theory of scientific fraud.

Because I had to start somewhere, I too began my investigation with the emails. But other than the agitated commentators, I collected, processed, interpreted and analysed the data in a way that counters the marginalisation efforts of the news media (see also Ryghaug & Skølvold, 2010: 292). As a first step I read the 1,073 text files in chronological order, highlighting passages I deemed important for my research questions. I then jointly coded, grouped, and analysed those emails in ways that would speak to my theoretical framework. For example, one group of emails contained ‘questions on methodology’, another showed ‘scientists discussing the presentation of

their findings in the IPCC’, yet another one was about ‘the certification of scientific knowledge’, ‘conversations with or about sceptics’, and so forth. These central themes emerged in the course of reading the emails. And once I understood which themes would speak to which of my chapters, and whether they were indeed helpful in answering respective research question, I used them accordingly. But I also identified and used search terms, which I then applied to other sources of data, and vice versa.

The public hearings on climate science provided a source that allowed for relatively easy data compilation. I first listened to the audio files available from governmental web-sites to get a feeling for the ‘climate’ of public hearings. I then downloaded the transcripts and highlighted important passages. Once again I jointly coded and analysed the data. And since my understanding of the scientific debate together with my analytical sensibility improved in the course of researching, I read through all transcripts time and again looking for different themes. If new search terms seemed suitable, or proved more effective, I would return to the emails and other sources so as to counter-check observations. This allowed me to search for regularities in the research data and to cross-check data from multiple sources. Such triangulation ensures the quality and credibility of my findings (cf. O’Donoghue & Punch, 2003).

In other publicly available sources a systematic data compilation proved much more difficult. Because there are dozens of climate blogs, I had to draw a somewhat arbitrary boundary around the ones I consider useful; many more blogs could have made it onto the list presented in table 2.2. I made my choice after I had spent many dozens of hours familiarising myself with the often esoteric disputes, many of which are not published elsewhere, and the characters populating the ‘blogosphere’. Once again I applied the search terms I had previously used across other sources.

Overall, the compilation and processing of data turned out to be an instructive process in which I had to go back and forth within and between different materials, to revise or discard search terms. I spent significant time reading across different sources so as to develop a feeling for the venues and characters under study. Having compiled data from various sources, I can map out, or explain more fully, the richness and complexity of the protagonists’ behaviour in the scientific controversy; I can provide a more inclusive picture than the one presented by climate sceptics, the accused scientists, the news media, but also the few academic studies on ‘Climategate’. Lamented a reader of Ryghaug & Skølsvold’s study:

Amazing that a stack of hacked emails is now apparently enough to form the scientific basis for an analysis on how climate scientists tick. In the past, scholars of the philosophy of science had to interview the pertinent actors or (especially if already deceased) had to sift through lab books, notebooks, published and unpublished work or reminiscences from contemporary colleagues. Nowadays a few emails suffice to make a story that can be published. Sheds a sober light on the standards employed in the field of the history or the philosophy of science.<sup>7</sup>

On all of these venues, including the blog on which I found this comment, the commentators, analysts, scientists, sceptics, and politicians are actively engaged in the making of authoritative scientific knowledge.

In my interpretative analysis I followed Lasswell (1948) and asked: Who says what, to whom, why, to what extent, and with what effect? For example, who do scientists address in the various forums, via the scientific literature, the emails, public hearings and blogs? In the scientific literature and public hearings one can expect much more carefully phrased arguments than in the emails in which scientists quite carelessly expressed their opinion. In the informal but publicly accessible records, scientists and sceptics too are purposeful and adapt their behaviour and language according to the audience they want to convince. In terms of hermeneutics, the people I studied adapted their behaviour and language to the ‘interviewer’ – in order to maximise their goal.

To translate the interpretative approach into an analytical structure I eventually drew upon the concepts introduced in 1.4, which are described in detail in each of the analysis chapters. To be sure, the data can be interpreted within different theoretical frameworks, the marker of quality being intellectual rigour.

Although the study is largely driven by the available data, it is important to note that there are always theoretical considerations which are never clearly demarcated from the empirical work of data collection. No less than in any other human inquiry, the researcher has to have some idea about what they are looking for and where to find it; they make use of predetermined ideas, hypotheses, concepts and categories which are used to inform the development of other categories. For example, in this thesis the concept of ‘climate sceptic’, however ambivalent it may be (Hulme, 2009), signifies people who oppose climate policies via arguments over the reality of climate change as described in the scientific theory of AGW, i.e., by using the term sceptic, opponents of climate policies invoke the motif of scepticism as scientific virtue.

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<sup>7</sup> <http://klimazwiebel.blogspot.co.uk/2012/03/marianne-ryghaug-and-tomas-moe.html> (Accessed on 19 March 2012)

Like any other researcher, I am “necessarily engaged in understanding the human world from within a specific situation. The situation is always and at once historical, moral and political,” state Rabinow & Sullivan (1988: 21). And to some degree, one could add, it is also personal since for many years I have had an interest in environmental and climate history. As undergraduate student I learned how to reconstruct past environments from dead plant material (pollen) stored in a bog, and since 2007 I have been casually following the scientific debate on climate reconstruction on blogs. In autumn 2009 I spent a term at the UEA and frequented the CRU building from which the emails were leaked. This is where and when the idea to write this thesis was eventually born.

Finally, any generalisation of the findings herein or from any account of the controversy is difficult. The drive towards overarching generalisations in sociological analysis would not only distort the subject of study to fit a mode of analysis intent on developing universal laws, but also ignore meaning in favour of prediction, i.e., although I sometimes draw comparisons with other scientific and political debates, “Climate Reconstruction and the Making of Authoritative Scientific Knowledge” must be understood in its historical and geographical context. The authority of the thesis then rests on its uniqueness rather than on a kind of universal relevance scientific facts aim to achieve.



## *Chapter Three*

### *Philosophies of ‘sound science’*

The impersonal rigor of the method has produced enormously powerful results for 400 years. The scientific method is utterly apolitical. A truth in science is verifiable whether [...] you know the experimenter, or whether you don't. It's verifiable whether you like the results of a study, or you don't [...] It is not a matter of honesty or good intentions.

Michael Crichton in Congress (SCEPW, 2005: 52)

#### **3.1 Introduction**

In the thesis' introductory chapter I argued that climate sceptics' talking points have become part of the rhetoric whenever policy-makers want to legitimise their programmes of action with the universal authority of scientific knowledge. Arguably more so than anywhere else, this strategy is prevalent in the US. Political scientists, sociologists and historians of science typically ascribe climate scepticism in the US to party-political and ideological commitments (Hoggan & Littlemore, 2009; Oreskes & Conway, 2010; McCright, 2011; Leiserowitz *et al* 2011; McCright & Dunlap 2011; Maibach *et al* 2013; Elsasser & Dunlap, 2013). Whilst Democrats have always favoured regulatory policies to manage the adverse effects of industrialisation, the Republicans support small government, an enabling state, and free-market solutions. They typically oppose governmental interference in private affairs. Thus Democrats are frequently portrayed as pro-environment, pro-science and pro-climate policy, whereas Republicans are belittled for their 'irrational' stance on the environment, science and climate policy (cf. Mooney, 2006; 2012). And the scientific experts that the conservatives invite to testify in Congress are denounced as a handful of bribed scientists who "obscured the truth on issues from tobacco smoke to global warming" (Oreskes & Conway, 2010).

The analytical focus on ideological commitments and the underlying political economy of funding that drives climate scepticism provides a constant stream of research material, yet it ignores the fact that in their opposition to climate policy conservative sceptics also invoke the authority of science. Quite often they turn the argument around: "We have concluded that [forecasting global warming] is basically an

anti-scientific political movement,” announced professor of marketing Scott Armstrong (HSCCT, 2011: 14). What is more, the analysts have generously overlooked that protagonists across the political divide share the same idea of science. Both Republicans and Democrats put a high premium on ‘sound science’ to minimise controversy, help close down political debates, and defend the impartiality of decision making. In fact, many requirements for ‘sound science’ have been written into US legislation on scientific risk assessment (Herrick, 2004).

Because the ideas of science have remained unexamined in those a-symmetrical accounts of the climate controversy, as if they were not worthy of a discussion, this chapter aims to fill this gap. Using Congressional hearings as its main source of information, in this chapter I excavate the philosophies of science underlying the demands from policy-makers on both sides of the climate debate for sound scientific knowledge. To that end the chapter is structured as follows. First I present the bi-partisan idea of ‘sound science’. Second I parse its underlying philosophy. Third I subject the oft-repeated criticisms climate sceptics hold against the theory of AGW to a practical test informed by the philosophy of science and science studies literature. I conclude with a characterisation of the climate sceptics’ philosophy of science.

### ***3.2 The bi-partisan idea of ‘sound science’***

Although climate policy debates have stalled over the question of whether the theory of AGW is ‘sound science’, the underlying ideal of ‘sound science’ as a foundation for rational policy-making has remained largely unexamined. The climate establishment posits a scientific consensus on climate change (Oreskes, 2004; Anderegg *et al* 2010; Rosenberg, 2010) and climate sceptics counter their claims with charges about its ‘unsound’ knowledge base. In recent years much of their criticism has focused on potentially ‘unsound’ reconstructions of Northern hemispheric temperatures (Mann *et al* 1998; 1999) which the climate establishment counts as evidence for the theory of AGW. In the US, Republicans with a Congress majority have amplified the scientific debate on climate reconstruction by holding Congressional investigations into charges from sceptics about the scientific flaws in the reconstruction (SCEPW, 2003, 2005; HCEC, 2006).

Here and elsewhere Congressional Republicans have loudly endorsed the view

that sound policy should be based on ‘sound science’. Senator James Inhofe (R-OK) reasoned in Congress (SCEPW, 2003: 1):

One of my primary objectives as chairman of the committee is to improve the way in which science is used. I think that when I became chairman of this committee, I announced [...] we are going to try to base our decisions, things that we do, on sound science [...] When science is debated openly and honestly, public policy can be debated on firmer grounds. Scientific inquiry cannot be censored. Scientific debate must be open. It must be unbiased. It must stress facts rather than political agendas.

On the possibility of climate legislation, Senator Wayne Allard (R-CO) noted (SCEPW, 2003: 12):

This is why it is so important to me that we be cautious when dealing with situations such as these and why we should place strong emphasis on the use of sound science. Our regulations must be thoughtful reflections of what we know – they should not be reflexive or reactive attempts to legislate a cure before we know what the disease is.

This same view was articulated three years later by Congressman Ed Whitfield (R-KY) in a House hearing dedicated solely to climate reconstruction (HCEC, 2006: 5):

At the end of the day, the issues of climate change require open and objective discussion. Some of the work we’ll consider today points to the value of policy decisions that are informed by sound science and objective advice.

The use of ‘unsound’ science may even have disastrous consequences, warned Senator Kit Bond (R-MO): “[U]nsound scientific guesses have led to the possibility of flooding and risking lives” (SCEPW, 2005: 10). Not only would ‘sound science’ help decrease the vulnerability of populations to natural disasters, it would speak the truth against political extremism, suggested Congressman Dana Rohrabacher (R-CA): “The propaganda campaign of the man-made global warming alarmists has much more in common with Stalinism than does insisting that both sides of the issue be heard at a Congressional hearing” (Rohrabacher, 2008: 5039); assuming the mantle of Robert Merton (1968), Rohrabacher casts himself in the role of defender of rational science against Lysenkoist ideology. Fellow party member George Voinovich (R-OH) wrapped up (SCEPW, 2005: 17):

I have long believed that sound science, not politics, should drive our Nation’s environmental policy. In fact, I believe that in harmonizing our Nation’s economic, environmental, and energy policies, that sound science should be the unifying factor.

Though it is associated with the Republicans and sympathetic think tanks (Jaques *et al* 2008), the idea of ‘sound science’ is not solely mobilised by them. Because the prefix ‘sound’ indicates a scientific ideal insulated from political interference, politicians from across the partisan divide, and in particular from the regulatory agencies charged with making contentious administrative rules in the face of partisan scrutiny, make ample use of its rhetorical force. The EPA (undated) remarks in its Science Policy: “The EPA has taken major steps to ensure that it carries out a program of sound science to inform Agency decisions without allowing regulatory objectives to distort scientific findings or analyses.” Testifying before Senate, David B. Sandalow of the US Department of Energy agreed with Voinovich: “[S]ound science should guide all government policy” (SCEPW, 2005: 81). As regards the Republican critique of climate reconstruction, Democrat Senator Tammy Baldwin of Wisconsin warned that “[p]oliticians ignore sound science showing evidence that the Earth is warming at an unprecedented rate, that carbon dioxide levels are rising, and that human activities are largely the cause” (HCEC, 2006: 32). Senator John McCain (R-AZ), who introduced the Climate Stewardship Act with former Democrat Joe Lieberman (ID-CT) against the wishes of his party members, urged that “[e]mission reductions must be feasible and based on sound science” (SCCST, 2007: 82).

By and large, scientists and scientific bodies seem to agree. In a pledge to restore scientific integrity in policy making, Nobel Prize winning scientists lamented the “advocating of policies that are not scientifically sound” (UCS, 2004). In a review on scientific procedures, the Inter-Academy Council (IAC) opened by recognising that “[s]ound scientific, technological, and medical knowledge is fundamental to addressing critical issues facing the world today” (IAC 2010: i). NASA scientist Gavin Schmidt remarked on the science of climate reconstruction: “[T]he conclusion, the conclusion about the conclusion, and the conclusion about the conclusion of the conclusion, is that [...] the Hockey Stick is sound science and portrays the temperature record in a robust manner” (Schmidt, 2010). Last but not least, the climate scientist Jay Gulledge from the Pew Centre on Climate Change invoked ‘sound science’ to confirm that “[t]he warming over the past 5 decades has been attributed (through sound science) to human activities associated with greenhouse gases” (HCEC, 2006: 693). Only when they discuss scientific issues among themselves, scientists do not see a need for using the prefix ‘sound’ (EAE, 2009).

Because it creates an incentive to demonstrate that science supports their positions, none of the protagonists is willing to give up the ideal of ‘sound science’. For politicians, its function as a symbolic aspiration is invaluable, and for scientists, in front of a critical audience, it reinforces their status as objective experts. Thus, protagonists across the climate divide continue to press for the soundest science. And even if their demand is provoked by and directed towards opposite ends, does a shared ideal not foster rather than stifle the policy process? After all, it may indeed reflect a genuine belief in the objectivity of science, acknowledges the sociologist John Turnpenny (2012: 402). The next section describes the philosophy of science climate sceptics appeal to when they speak about ‘sound science’.

### ***3.3 ‘Sound science’ in theory***

#### *Verification and falsification*

In their critiques of climate science as ‘unsound’, sceptics frequently invoke the principle of induction as the grounds for scientific truth. Famously championed by Francis Bacon, in inductive reasoning a general theory is arrived at by the accumulation of unique observations. In the textbook example, the observation of a white swan verifies the theory that “all swans are white.” For Senator John Thune (R-SD) “scientific truth has to be verifiable, that is a key thing” (SCEPW, 2005: 38). “The basic rule of science is that hypotheses must be validated by observed data before they can be regarded as facts,” notes the Competitive Enterprise Institute (CEI, cited in Herrick, 2004: 426). In a floor speech, Dana Rohrabacher (R-CA) (2008) explained that a scientist “doubts, tests, verifies, and repeats.”

Alternatively, climate sceptics invoke the principle of deduction in which a conclusion is reached reductively. The scientist starts out with a general rule or theory and evaluates it against observations to reach their conclusion. Thus, deduction lends itself to falsification (Popper, 1980). In the textbook example, the observation of a single black swan falsifies the theory induced from x-observations of white swans only. When a theory’s prediction (“all swans are white”) is falsified by a single instance (a black swan) it has to be rejected. Dana Rohrabacher (2008) explained:

A scientist does not “believe” something to be true. Nor does he negotiate a solution with his colleagues. He does not reach consensus. A scientist engages in a search for answers by

forming a theory and trying to tear it apart. He invites his colleagues to prove him wrong [...] A scientist will do everything he can to prove a theory wrong. Only then, when he and his colleagues are unsuccessful at disproving a concept, will he accept it.

To be sure, mainstream climate scientists too allude to the principle of falsification: “Science [...] means objectivity, transparency, repeatability, and in principle the possibility of falsification,” wrote the geophysicist Rasmus Benestad on the blog *realclimate.org* (2007).

Verification and falsification are cornerstones of the scientific method, which is at the heart of ‘sound science’; it demands of scientists to either verify or falsify a scientific theory with observations. These observations can be relatively simple measurements of natural phenomena or the results of more or less complicated experiments. Observation and experimentation are central activities of scientists and their importance cannot be stressed enough: testifying in the House of Representatives, Jay Gullede explained that “[t]he primary evidence [of AGW] is based on physical principles and observational and experimental analysis of contemporary climate dynamics (HCEC, 2006: 177).

### *Observation*

In the classical experimental sciences, there is theory on the one hand, and experiment or observation on the other. The theorists produce hypotheses and calculations, and the experimentalists produce measured results or data from observations. These are indeed two different and quite distinct communities in pursuit of different objectives (cf. Galison, 1987; Sundberg, 2005). At some point the calculations are confronted with the experimentalists’ data to either verify or falsify certain hypotheses that are unified in a scientific theory. In order to do that the scientific method demands of the scientists to strictly demarcate observation from theory, and the question philosophers of science have struggled with is how to do that: how to eliminate the observational bias in which the scientist’s theoretical predisposition influences what and how they observe.

The so-called logical empiricists have found a compelling answer to this question. Adapted from Kantian empiricism, in logical empiricism observational statements are strictly demarcated from logically reasoned, theoretical sentences. The

latter cannot be derived from observation but are based solely on the rules of logic and language – they are true as such. Scientists can then make both analytic a priori and synthetic a posteriori statements: in the textbook example, the sentence ‘All unmarried men are bachelors’ is analytic because the idea of a ‘bachelor’ is already contained in ‘unmarried’. It is analytic *a priori* since logical reasoning exists in the mind independent of observation or experience. By contrast, the statement ‘Some bachelors are very unhappy’ is a synthetic statement because the idea of ‘unhappy’ is not contained in ‘bachelor’. This statement can only be justified a posteriori. The sentence is a synthetic *a posteriori* statement since the observation of unhappy bachelors does not exist in the mind independent of observation or experience. And because the truth of synthetic statements can only be derived from observation, and the truth of analytic statements is based on the rules of the language, from a logical empiricist view there cannot be synthetic a priori statements. For the testing of a scientific theory this means that the making and selecting of observations must be based on analytic a priori terms.

All scientists implicitly subscribe to the logical empiricist distinction between analytic a priori and synthetic a posteriori sentences. They often employ ‘a priori language’ to justify the selection of observations, but these terms are rarely described in the scientific literature as it is generally assumed that fellow scientists know what is meant by a priori and a posteriori. In scientific controversies the opponents would typically check and challenge what could be an analytic a posteriori selection of data: “To ignore aspects that don’t fit the hypothesis is definitely not science. Neither is adjusting data so to provide a good fit without a solid and convincing justification,” avered Rasmus Benestad (2007) in a rebuttal of scientific claims made by the sceptics. And about the selection of observational data for the hockey stick climate reconstruction, complained the ‘climate auditor’ Steven McIntyre (2005): “Without a statement of these “clear a priori” criteria, it is obviously impossible to replicate the proxy selections of MBH98.” In his statement McIntyre both agrees with the tenets of logical empiricism and points to a last important principle of ‘sound science’: the replicability of observations or experiments is a key motor of scientific progress and success.

### *Replication*

Replication is a universally accepted principle of the scientific method.

According to the principle of replication, scientific observations and experiments are credible if and only if anyone with the right skills and material resources is able to reproduce them. As the American Physical Society (APS) explains, “[t]he success and credibility of science are anchored in the willingness of scientists to [...] expose their ideas and results to independent testing and replication by others [...]” (cited in HSCCT, 2011: 2). “Replication of results is a key component of science,” affirmed the Director of the National Oceanic and Atmospheric Administration (NOAA)’s National Climatic Data Center (NCDC) Tom Karl in his testimony to US Representatives in the House (HCEC, 2006: 130). Likewise, novelist and climate sceptic Michael Crichton explained in his Congressional testimony (SCEPW, 2005: 18):

In essence, science is nothing more than a method of inquiry. The method says an assertion is valid – and will be universally accepted – only if it can be reproduced by others, and thereby independently verified. The impersonal rigor of the method means that it is utterly apolitical.

Any failure of replication raises important questions about the veracity of the original experiment. “If the work cannot be replicated and verified by independent experts, then that work’s conclusions become more speculation and possibly some will say it should be open to classification as outright scientific dishonesty,” warned the Republican Congresswoman Marsha Blackburn of Tennessee against the premature acceptance of climate reconstructions by Mann, Bradley and Hughes (HCEC, 2006: 32). Mann had repeatedly claimed the soundness of his reconstruction, arguing “[t]he precise details of our early work have been independently reproduced and confirmed by climate scientists” (HCEC, 2006: 641). Climate sceptics in turn question the independence of these scientists, notably Mann’s PhD student Scott Rutherford’s. But for the purpose of this chapter it suffices to know that both sides agree on the principle of replication as an essential component to the scientific method.

In summary, in the philosophy of ‘sound science’ a scientist has to follow the scientific method in which observations serve to verify or falsify a theory. Only if observational sentences are logically demarcated from theoretical sentences can the theory these statements describe be considered true. Finally, any observations and experiments have to be independently replicated. Thus, in broad terms the philosophy of ‘sound science’ describes the common-sense-realist attitude to truth, stressing the importance of observation over theory and denying the scientists any agency in the



discovery of truth. Both climate sceptics and mainstream climate scientists subscribe to this common-sense realism, and the more binding they consider the ideals of ‘sound science’, the more critical they will be of any practice in which they are violated.

In the next section I subject the ideals of ‘sound science’ to a practical test. In the second half of the 20th century, philosophers of science have increasingly argued that agreement about the scientific method and knowing quite whether its principles apply in practice is conventional. If it is indeed conventional, not logical, we can expect to see disputes over these conventions, for example, over the terms of verification, falsification, and replication.

### ***3.4 Theory testing, validation and replication in climate science***

#### *The role of observation in inductive logic*

A scientific theory is a well-substantiated, unifying explanation for a set of hypotheses that lend themselves to testing, though as we have seen there is some ambiguity among advocates of ‘sound science’ about whether the aim of such testing is verification or falsification. Climate sceptics have certainly argued that there are not enough observations of climate change for AGW to be considered verified. In this vein, the former president of the National Academy of Science (NAS), Frederick Seitz, held that AGW has not been substantiated by observations (in Singer, 2008: iii):

It is foolish to [impose drastic measures and harsh economic penalties] when the [climate] problem is largely hypothetical and not substantiated by observations [...] we do not currently have any convincing evidence or observations of significant climate change from other than natural cause

Alluding to the principle of induction, in Seitz’ opinion there have not been enough, if any, observations that would verify AGW. And the existing observations have not convinced him of the explanation of significant climate change. Also climate sceptic and geographer David Legates professed in Congress “not [to be] entirely convinced, based upon the proof, that carbon dioxide is a driving force [of climate change]” (SCEPW, 2003: 34).

In his critique of the principle of induction, Karl Popper (1980) notes that there is no logical end to testing; there is no logical reason to choose y over x number of observations to consider a theory proven. Rather, in testing a theory, scientists decide to

accept a particular, inevitably somewhat arbitrary number of observations and their decisions are based on agreement – they are conventional not logical. For this reason “[s]cientists don’t speak in terms of ‘proof’ but in terms of likelihoods and the strength of evidence in support of a particular hypothesis,” Michael Mann corrected Legates (SCEPW, 2003: 181). The IPCC, for instance, has gone to great lengths to find a common language that describes scientific confidence in the likelihood of AGW being real. Their probability statements are assigned numerical values – in the IPCC 2007 report ‘very likely’ means 90%-99% likelihood – and degrees of confidence scientists have in that statement: ‘very high confidence’ means that the degree of confidence in being correct is at least 9 out of 10. These standards of proof have changed over the years and may vary from study to study; they are socially constructed.<sup>8</sup> Thus, what constitutes a significant number of observations for both scientists and their critics to accept the truth of AGW has to be agreed upon. And because agreement is conventional, it can easily fail opponents in scientific controversies.

#### *The role of observation in deductive logic*

Alternatively, and as it suits their argument, climate sceptics and mainstream scientists invoke the principle of falsification. The advantage of falsification over verification is that a single observation can, in principle, refute an established theory. In a controversy about satellite observations, instrumental surface temperature records, and climate models, sceptics proclaimed the falsification of AGW: because satellite observations by climatologists Roy Spencer and John Christy (1990; 1992; Christy *et al* 1995) reported less warming than instrumental surface observations do, and contradict climate models depicting a much higher rate of tropospheric warming, the physicist-turned-climatologist and conservative political activist Fred Singer (2008: 108) concluded:

Climate models all predict that, if GH gases are driving climate change, there will be a unique fingerprint in the form of a warming trend increasing with altitude in the tropical troposphere, the region of the atmosphere up to about 15 kilometers [...] This mismatch of observed and calculated fingerprints clearly falsifies the hypothesis of anthropogenic global warming (AGW). We must conclude therefore that anthropogenic GH gases can

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8 So-called social object constructivist critique (cf. Sismondo, 1993) is particularly effective in attribution and detection studies in which also single weather events are increasingly taken as ‘proof’ that man-made climate change is already happening. But social object constructivists may also deconstruct the ways by which scientists portray and communicate uncertainty in these studies (Van der Sluijs, 2005; Van der Sluijs *et al* 2005). This in turn requires an understanding of how different uncertainty metrics are agreed on, hence socially constructed (Guy & Estrada, 2010).

contribute only in a minor way to the current warming, which is mainly of natural origin.

Also for the palaeontologist Robert Carter (2007) “[t]he satellite data signal not only the absence of substantial human-induced warming [...] but also provide an empirical test of the greenhouse hypothesis as understood by the public – a test that the hypothesis fails.” By calling it a greenhouse *hypothesis*, sceptics insinuate that the theory of AGW is anything but close to the truth. Mainstream scientists in turn consider falsified the hypothesis linking late 20th century temperature rise to cosmic ray activity (e.g., Benestad, 2007). In their books there is no established theory which attributes contemporary climate change to solar variability.

For an observation such as satellite data to count as falsification of a scientific theory, analytic a priori and synthetic a posteriori sentences have to be logically demarcated. But no observational sentences are ever completely independent of theoretical commitments, and vice versa. Satellites do not record ‘pure’ temperature, but the upwelling microwave radiation from atmospheric oxygen, which, in physical theory, is proportional to the temperature of broad vertical layers of the atmosphere. Also the seemingly simple observation of temperature using a thermometer involves a theory which describing the properties of mercury used in thermometers. Even the analytic a priori sentence ‘All unmarried men are bachelors’ builds upon certain assumptions that require explanation. One first has to qualify the characteristics by which we might define and measure whether any individual qualifies as a bachelor. Any such theoretical statement too involves auxiliary assumptions and may need to be re-evaluated. And here too we need some kind of experience, writes the philosopher Willard Quine (1951: 39-40, cf. Hanson, 1958; Duhem, 1991):

The total field is so undetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-evaluate in the light of any single contrary experience [...] Re-evaluation of some statements entails re-evaluation of others, because of their logical interconnections – the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having re-evaluated one statement we must re-evaluate some others, whether they be statements logically connected with the first or whether they be the statements of logical connections themselves.

The fact that the statements logicians call analytic a priori are simply the more recalcitrant ones makes the principle of falsification particularly equivocal. Too many background assumptions might be wrong or unknown, wrongly disregarded physical

processes may have affected the (satellite or any other) observations, and so forth. Following Quine (1951) and Duhem (1991), the philosopher of science Imre Lakatos (1970) considers a theory falsified if and only if troublesome evidence forces a great host of changes in the auxiliary hypotheses, which are built around and protect the theoretical core. Together with the fall of an established theory, a new theory with greater explanatory force and predictive power has to be articulated. Because satellite data are not compared to AGW but to the auxiliary hypothesis which suggests an increase of tropospheric temperatures over the tropics, the theory has not been falsified. And should such an isolated observation be used to refute a theory, philosophers of science speak of ‘naïve’ or ‘trivial’ falsification. What is more, a new comprehensive theory that replaces AGW has not yet been articulated.

Though the standard theory of AGW and its predictions may begin to prove less appealing, scientists do not take the existence of anomalies as an indication that the theory is falsified (Fyfe *et al* 2013). But observations may be interpreted within the framework of a variety of hypotheses, argues climate scientist Judith Curry (2012): they may be used to argue for the “multi-decadal oscillations plus trend hypothesis” which explains 20th century climate change by the large multi-decadal oscillations with a superimposed trend of external, that is, anthropogenic forcing. Observations may also support the “climate shifts hypothesis” which explains 20th century climate change as synchronised chaos arising from non-linear oscillations of the coupled ocean-atmosphere system plus external forcing. Either anomalous observations become so overwhelming that a new core theory comes to replace AGW, or the latter undergoes defensible adjustments in its auxiliary hypotheses. But since no theory can be falsified by logical inference alone, a crucial test of AGW of the sort lauded by logical empiricists and their doyens who insist on ‘sound science’ is logically impossible.<sup>9</sup> Much like in the disputed verification of AGW, mainstream scientists and their sceptical critics cannot agree on the necessarily conventional terms of falsification.

#### *Climate model experiments as a special case of observation*

Together with observational data from classical experiments, computer models have been a preferred tool in climate science. These general circulation models

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<sup>9</sup> Scientists certainly continue to speak of falsification and declare for example intelligent design as falsified. But one cannot base the refutation of intelligent design on logic alone as this would invite its proponents to question most basic statements such as ‘All unmarried men are bachelors’.

(GCMs), which have been applied for some time in numerical weather prediction, are now used to predict climate change. Like in weather forecasts, climate modellers use observational data to initialise a model run to produce their own data, modelled data. Yet other than weather forecast models, which are continually reinitialised with updated real-world observations to keep in line with the weather's rapidly evolving actual course, climate models cannot be continuously updated. Thus, and given the chaos in the system, any predictions soon stray from an initial state leaving no chance for a unique climate prediction (for a given set of initial conditions) to be accurate. And because climate modellers can only calculate probable future climate states from so-called ensembles runs, climate sceptics have criticised their products. Testifying in Congress, the meteorologist and hurricane expert William Gray fumed (SCEPW, 2005: 25-26):

Around the globe there are 30 numerical models that are trying to predict climate. None of them gives you a forecast. I say, look, if these climate models are OK, why don't they tell us next season, next year whether the global temperature is going to rise or not? They don't do that. The reason they don't, they know they have no damn skill in doing so. So should we believe them 50, 100 years down the line, when they can't forecast 6 months or a year in the future? It is ridiculous. I predict that in 15 or 20 years we are going to look back on this whole business as the Eugenics movement. You know, there used to be, 400 years ago, the majority of the scientific opinion felt the sun went around the earth. Now, damn it, don't tell me the sun rises, goes around.

While the difficulty of forecasting climate in a chaotic system is evident, climate models also violate the principles of logical empiricism and 'sound science'. In climate model experiments, scientists use classical observational data to test their climate models, which are in turn used to test different hypotheses, and vice versa. Other than in classical experimental sciences where hypotheses and experiments are distinct, in numerical modelling the hypothesis to be tested and the numerical experiment are in a continuum. And because there is no clear distinction between observation and theory, the products of climate models are epistemologically different from real world data. This makes it difficult to verify or validate hypotheses, summarises philosopher of science H el ene Guillemot (2010: 244):

[T]o verify a hypothesis, one first has to translate it into algorithmic form and insert it into the model, then one carries out a simulation by running this model. In this way, the model is at once theoretical and experimental [...] but it is a limited virtual experiment, cut off from the experiment's fundamental capacity for confronting the real world.

In order to validate a climate model, scientists use real-world observations with which they test the performance or “skill” of a climate model (Hargreaves, 2010), i.e., a model that can simulate past climate as observed by instruments is more likely to predict future climate change. But in practice there is no systematic protocol of validation: “one never validates a model in general, but the capacity of a model to account for a defined climatic phenomenon or characteristic,” states Guillemot (2010: 242). If the model cannot reproduce the instrumental record well, modellers adjust its parameters in the hope that next time around the model performs better. To the Alabama state climatologist and remote sensing expert John Christy this ad hoc adjustment, which is common practice in climate science, does not count as scientific success. In his role as Republican-chosen expert witness, Christy explained (HSCCT, 2011: 65):

I view the whole modeling effort with more skepticism than most, perhaps because I do not receive funding to produce model results. Each global modeling group has had 20 years to look at the global surface temperature record and devise clever ways to reproduce what is in the record. This is “a posteriori” science in my view. No one has from first principles actually reproduced the record [...] Modelers are working to reproduce observations, and when a match is finally constructed, the insinuation is that the models are successful. In my view, this procedure is not a scientific success.

Alternatively, climate modellers may question whether the observational data are reliable. In particular when observational data are sparse or come from questionable sources, modelled data are considered more reliable than data from real-world observations. In his testimony on ‘Climategate’ in the UK House of Commons, the physicist Michael Kelly warned of such a practice (HC, 2010b: 18):

I take real exception to having simulation runs described as experiments (without at least the qualification of “computer” experiments). It does a disservice to centuries of real experimentation and allows simulations output to be considered as real data. This last is a very serious matter, as it can lead to the idea that real “real data” might be wrong simply because it disagrees with the models! That is turning centuries of science on its head.

In fact, if reliable long-term observational data are absent, modelled data may be compared to modelled data. Here scientists have to use “the internal variability atmosphere-ocean general circulation models [...] as a substitute for instrumental observations” (Hegerl, 1998: 759, cited in Demeritt, 2001), i.e., model runs simulating the internal climate variability are compared to model runs with GHGs as external forcing. Echoing Kelly’s critique, William Gray complained about the modellers’ lack of experience with the real world (SCEPW, 2005: 34; 59):

The trouble [with climate modellers] is that they don’t know how the atmosphere ticks [...]

They are people that make assumptions that are not valid and they believe them [...] Few have ever given real-world weather briefings or made operational weather forecasts.

It is easy to see how climate model experiments become hostage to the political controversy: “To a large degree these debates [about the epistemology of climate model simulation] are in fact about the model/data relationship: whether model results agree with observations, how much each of these can be trusted, and what role these model projections should play in policymaking,” avers science scholar Paul Edwards (1999: 439). H el ene Guillemot and fellow philosophers of science tend to view models as useful for guiding further study rather than as ‘policy-tools’.<sup>10</sup> Depending on the question scientists ask, some models are simply better experimental (Gramelsberger, 2010) or heuristic (Oreskes *et al* 1994) tools.<sup>11</sup> Others again have described models as metaphors (Ravetz, 2003) or as mediators that function more like portraits or maps do (Morgan & Morrison, 2000). Anything but insinuating a lack of natural agency these scholars make a point about the interpretative freedom that is constantly exploited in the political controversy, as John Christy, who is well known for his scepticism about the existence and scale of AGW, explained in Congress (HSCCT, 2011: 48):

The first basic problem with the entire issue here is that climate science is a murky science, not a classic, experimental science. As an emerging science of a complex, chaotic atmospheric and oceanic system, it is plagued by uncertainty and ambiguity in both observations and theory. Lacking classic, laboratory results, it easily becomes hostage to opinion, groupthink, arguments-from-authority, overstatement of confidence, and even Hollywood movies.

While Christy is correct about the difference between classical and computer-based climate science – data from numerical climate models are different with respect to how they have been arrived at and in terms of the phenomena they constitute, – he

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10 Betz (2009) argues that GCMs are more useful to falsify rather than predict possible future scenarios of climate change. He criticises the IPCC’s “modal inductivism,” in which possible scenarios are constructed based on what is well-known. Like classical inductivism “modal inductivism” assumes the existence of a secure epistemic foundation or background knowledge in the form of empirical data. For Betz only those creatively constructed future scenarios (hypotheses or theories) that have not been falsified shall be accepted as possible. Since few scenarios can be falsified, and there are no robust upper bounds for future temperature rise (Stainforth *et al* 2005) “modal falsification [...] prescribes even more drastic efforts to curb CO2 emissions than currently proposed,” concludes Betz (2009: 133).

11 Naomi Oreskes compares a model simulation to poetry: “A model, like a novel, may resonate with nature, but it is not a “real” thing. Like a novel, a model may be convincing-it may “ring true” if it is consistent with our experience of the natural world. But just as we may wonder how much the characters in a novel are drawn from real life and how much is artifice, we might ask the same of a model: How much is based on observation and measurement of accessible phenomena, how much is based on informed judgement, and how much is convenience?” (Oreskes *et al* 1994: 644).

implies that classic experimentation is proper science and does not easily become politicised. But also classic laboratory results have been controversial because classic or real data are not truly real in the sense of being direct, unmediated representations of the world either; any observations are laden with theory, or a priori theoretical commitments. In principle they too are modelled data, although the term has much broader significance than in numerical models (Cartwright, 1999). And because the difference between modelled and real data is one of degree not kind, as if the latter were simply given by nature, even the observation of rain, involving theoretical commitment of what constitutes rain (Hesse, 1980: 65-83), may become hostage to the political debate. This is why in contentious political issues so much emphasis is put on the replication of scientific experiments (e.g., *The Lancet*, 2010; *Food and Chemical Toxicology*, 2012).

#### *The replication of observations*

In order to test the veracity of observational data, whether from numerical or classical experiments, advocates of ‘sound science’ insist on their replication. In principle, any skilled scientist and team of scientists should be able to independently replicate any kind of climate observation. “And so before we go off the deep end I really do want to make sure that these models are independently reviewed and really are scientifically accurate and really can be replicated,” urged Congressman Joe Barton (HCEC, 2006: 67). He may know that the principle of replication, too, is practically difficult to sustain.

Even if the independent scientists have command of all the resources necessary to perform an experiment, the practical skills and tacit knowledge of experimentation (cf. Polanyi, 1958; Collins, 2010) ensure that “it can never be clear whether a second experiment has been done sufficiently well to count as a check on the results of a first,” writes Harry Collins (1985: 2) in his seminal study on scientists searching for gravitational waves. What Collins called the experimenter’s regress is a paradox which arises for those who want to use replication as a test of the truth of a scientific knowledge claim. Assigned to perform a replication, the independent scientists may not possess the original experimenters’ intimate knowledge of the data, the instruments, or the field site where conditions are particularly difficult to control (cf. Kohler, 2002; Rees, 2009). Moreover, scientists often form opinions about both the original



experiment and the replication before the results of the latter are in; they have made theoretical commitments (Collins, 1981).

Replication can fail or be considered a failure for a host of reasons. To be sure, Collins' observation does not mean that replication is impossible – replication remains a standard repertoire in any natural science – but that it too is a practical achievement requiring some and however small a degree of taste and discernment (cf. Godin & Gingras, 2002). Chances of a successful replication would certainly increase if the scientists provided a detailed description of the original experiment. And in order to share their tacitly held knowledge, which is difficult to teach since it cannot be written down, the original experimenters and the independent scientists would have to collaborate, in turn negating the demand for independence.

### ***3.5 Conclusion***

In this chapter I showed that the convinced and the sceptics have a similar idea about what science is and what it should do for them. They share the principles of verification as described in logical empiricism and falsification as described by Karl Popper; both use inductive and deductive logic as it suits their arguments for or against AGW. But the sceptics more so than mainstream scientists lean in their arguments against AGW on the impracticality of the principles' strict implementation: By insisting on the logical demarcation between observations and theory, they seize on the fact that their opponents would agree but cannot live up to it. Their important scientific tools, numerical models, contain both observations and theory and thereby violate the method; if modellers thoroughly complied with these demands, they would never be able to practice their science.

I should not come as a surprise that in the climate modelling community there appears to be not one climate sceptic. Scientifically credentialed sceptics who speak on the venues from where I gathered data almost exclusively populate the community of classical experimentalists; they are meteorologists and climatologists. As empiricists they criticise an understanding of the climate system based on theoretical principles, numerical models and modelled data, and insist on the primacy of real-world observations. To them the difference between real/empirical and modelled/theoretical data must be in kind – their claims against AGW hinge on this logical demarcation.

Informed by the logical empiricists' ideals, sceptics have so concluded that AGW has not been substantiated by real observations: whenever scientists claim to have attributed an empirical observation to AGW, sceptics would declare it unconvincing or insignificant. Here they seize on the fact that resolving any scientific debates must inevitably rest on some however small a degree of convention. And they agree to disagree over the conventional terms of verification and falsification.

Because sceptics assume the mantle of empiricists, they eventually turn towards the existing classical experiments which are thought to have tipped the balance of evidence in favour of AGW. If these empirical observations cannot be replicated, the theory of AGW must be discarded, or so they would argue. ***That is the scientific rationale for climate sceptics to focus on climate reconstruction***, and the email disclosures suggest that certain scientists tried to avert the replication exercise for political reasons. In the next chapter I tell the story of its attempted replication.

## *Chapter Four*

### *Replicating the hockey stick graph*

In climate science, it is permissible for raw data to be touched or modified by many hands. Gaps in temperature and proxy records are filled in. Suspect values are deleted because a scientist deems them erroneous. A researcher may elect to use parts of existing records, ignoring other parts. But the fact that the data has been modified in so many ways inevitably raises the question of whether the results of a given study are wholly or partially caused by the modifications themselves [...] What is at issue is whether the methodology of climate science is sufficiently rigorous to yield a reliable result. At the very least, we should want the reassurance of an independent verification by another lab in which they would make their own decisions about how to handle the data and yet arrive at a similar result.

Michael Crichton in US Congress (SCEPW, 2005: 19)

#### **4.1 Introduction**

In the late 1990s the physicist-turned-climatologist Michael Mann together with the palaeoclimatologists Raymond Bradley and Malcolm Hughes published studies on Northern hemispheric temperature patterns in the prestigious scientific journals *Nature* and *Geophysical Research Letters (GRL)*, the first reconstructing a six century-long time series and the second extending the first reconstruction back 1,000 years (Mann *et al* 1998, 1999, typically referred to as MBH98 and MBH99). Their climate reconstructions showed global temperature wiggling along the x-axis over nine centuries before rising sharply at the end of the 20th century, and for this reason earned the moniker ‘the hockey stick’, because the nine centuries of variability around a broad stable mean resembled the shaft of a hockey stick whose ‘blade’ was formed by the sharp upturn in mean annual temperatures over the 20th century (see figure 1.1 on page 12). The scientists used climate models to attribute the changes in temperature to various forcing factors, including volcanic activity, variations in solar radiation, and GHG concentrations. They concluded that the dramatic temperature increase in the late 20th century could only be explained by an increasing concentration of GHGs in the atmosphere. Furthermore, the flat shaft of the hockey stick contradicted previous scientific wisdom about pre-industrial global warming during the so-called medieval warm period (MWP) around 1200-1500 AD, which is apparent in European records but not, they insisted, over the hemisphere as a whole.

The publication of MBH98 and MBH99 served the climate policy establishment well. In his 2000 State of the Union Address, US President Bill Clinton referred to the graph: “Scientists tell us that the 1990s were the hottest decade of the entire millennium. If we fail to reduce emissions of greenhouse gases, deadly heat waves and droughts will become more frequent, coastal areas will be flooded, economies disrupted” (Clinton, 2000). It had been this belief, of course, that drove the Clinton administration to sign the Kyoto Protocol, committing the US to controversial reductions in its GHG emissions. And in the hockey stick Clinton and his vice-president Al Gore saw a legitimisation of their climate policies.

Climate sceptics saw in the Mann team’s study a threat potentially undermining their opposition to climate policy: because climate reconstruction presented an empirical test of AGW it was difficult to dismiss based on the principles of ‘sound science’, which are particularly useful in the critique of computer experiments. Unless the hockey stick failed the crucial test of independent replication, climate sceptics would have to accept its truth, and with it their contention that the science was still too uncertain to justify emissions reduction that would cost billions. “[L]et us see if it can be replicated. Let us see if in fact the facts as purported in that report are in truth the facts,” announced Congressman Joe Barton (R-TX) (HCEC, 2006: 34) in a Congressional hearing featuring the expert witness who tried to reproduce the hockey stick study from scratch, Steven McIntyre. If the climate reconstruction cannot be replicated it must not to be used as policy legitimization, the climate sceptics in Congress insisted.

We saw in the last chapter how advocates of emissions reductions and self-styled climate sceptics both subscribe to similar ideas about basing policy on ‘sound science’. In turn, their shared philosophy of science rests on ideas about observation, experiments, and verification that are difficult to square with accounts of the messiness of science in practice offered by science students. The aim of this chapter is to use the scientific controversy over the of the hockey stick graph to explore in more detail the practice of climate reconstruction. It is a story in two acts. First, I describe the methodological choices with which the scientists assembled the most heterogeneous materials to finally present to the world the hockey stick graph. Second, I follow McIntyre’s replication exercise which provides a unique insight into the practice of scientific work and the tension arising between idea and practice.

#### 4.2 *The science of climate reconstruction*

In order to arrive at a hemispheric-to-global<sup>12</sup> climate reconstruction, Mann, Bradley, and Hughes (henceforth the Mann team) collected climatic information from dozens of the most heterogeneous materials. Information came from wherever they were believed to hold information about climatic conditions – the deep seas, high mountains, vast woodlands and even deserts. Dendrochronologists core trees to analyse tree ring density and ring width, palaeoecologists take samples from peat bogs and examined their pollen distribution, glaciologists core into glacial ice to determine oxygen and other isotope ratios in the glacial ice, marine scientists measure atomic isotope and chemical compound ratios in corals and other marine organisms, geologists measure sedimentation rates and plankton abundance in lake and sea beds, environmental historians interpret written historical accounts related to weather and climate, and so forth (cf. Bradley, 1999).<sup>13</sup> Scientists call these tree, peat bog, glacier, coral, sediment, and historical accounts climate *archives*, and the measured variables of these entities (ring width, isotope ratios, etc.) are climate *proxies*. Michael Mann explained to Congress (SCEPW, 2003: 173):

One area of active current research of mine involves the analysis of climate ‘proxy’ records (that is, natural archives of information which record past climate conditions by their biological, physical, or chemical nature). These data are used to reconstruct patterns of climate variability prior to the period of the past century or so during which widespread instrumental climate records are available.

Measured variables from these climate archives are used to approximate temperature and other climate parameters. For their so-called multi-proxy reconstruction, the Mann team first collected and then calibrated those variables against instrumental temperature data, which are thought to be the most accurate measurements of atmospheric temperature. To this end, scientists take a sample from the proxy dataset and compare it against instrumental records to derive a statistical transfer function to relate features of the proxy record (i.e. tree ring density, or pollen counts) to some climatic feature, like average annual temperature, that is theorised to determine it. They

<sup>12</sup> Mann: “Although this record comes mostly from the Northern Hemisphere, it is likely to be a good approximation to the global anomaly based on comparisons of recent patterns of temperature fluctuations” (SCEPW, 2003: 179).

<sup>13</sup> Documentary proxies include historical descriptions of directly or indirectly climate-related phenomena. Sources include annals, pictures, almanacs, diaries, memoirs, ship log books and church records. Inferences about climatic conditions can also be drawn from tax, trade or newspaper reports, from stall-keeper and market songs and epigraphic sources such as (high water) marks chiselled into stones, houses, trees and so forth (cf. Jones *et al* 2009). Temperature reconstructions have been published based partly or entirely on either (bio)physical/chemical or documentary data and on combinations of both.

then apply this transfer function to convert past measures of the phenomenon to the past climate said to have determined it. The calculated relationship between proxy and instrumental temperature is applied to the period when only proxy data are available.

Once an individual proxy record has been estimated, it must then be combined with other records of varying lengths and types into a single measure. In order to do this the original point measurements have to be transformed into a gridded record for the hemisphere and the globe as a whole. This is roughly how the Mann team combined millions of records from thousands of places to arrive at figures for the Northern hemispheric climate. For each year a number representing the hemispheric temperature anomaly is calculated against a reference period and plotted chronologically over six centuries and ten centuries respectively. The Mann team's reconstruction looks much like a hockey stick.

To common-sense realists the proxy and instrumental climate data represent a pre-existing material reality. They regard as self-evident the works of thousands of scientists, that is, their observations, measurements, calculations and theories that ultimately enable scientists like Mann to test the theory of AGW – as if one stuck a thermometer into the planet or a measuring yard into a climate archive, sat back and observed the temperature rise and fall or the tree grow and wither. “[I]n principle, all you need to do is take all your thermometer readings and work out an average,” wrote Andrew Montford (2010: 41), a blogger and sceptical commentator of the hockey stick controversy. Obviously, as both scientists and sceptics acknowledge, reconstructing past climate change involves many more steps than this caricature suggests. In innumerable steps the material world is turned into representations thereof until the Mann team can speak of a graph representing Northern hemispheric temperature patterns over the past six (respectively ten) centuries.<sup>14</sup>

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<sup>14</sup> The Mann team was not the first to attempt a Northern hemispheric reconstruction. In 1979 doctoral student Brian Groveman and supervisor Helmut Landsberg from Maryland University presented the first quantitative multi-proxy reconstruction of Northern hemisphere annual mean temperatures, with all but 3 of the 20 proxy series used being instrumental temperature series (Groveman & Landberg, 1979). Their study simulating temperature departures from 1579-1880 is discussed in the specialised scientific literature and provoked fairly little criticism. Global palaeoclimatology was in its youth. The occasional palaeoclimatological reader was published (Lamb, 1990), but palaeoclimatologists were still relatively few, and climate science of relatively marginal societal interest and rarely newsworthy. Then in 1993, Raymond Bradley and Phil Jones published their much-cited multi-proxy study on Northern hemispheric summer conditions since 1400 using 23 proxy series including tree rings and ice cores (Bradley & Jones, 1993). New data and more sophisticated methodologies allowed scientists to ask and answer new questions, to go further back in time and to increase temporal resolution. Increasingly, climate came to be understood as dynamic and unstable.

### *Scientific practice in the case of dendroclimatology*

Because the Mann team believe that trees provide the most robust proxy data, they chose to use tree rings as their main source of proxy data (Ryghaug & Skjølsvold, 2010). If tree rings hold measurable climatic information, trees are climate archives. Dendroclimatologists thus core trees, extract tree ring cross-sections and take them back to their laboratories where they measure climate proxy variables. They measure a tree's ring width, wood density, radiocarbon content, and other variables that indicate climate parameters, such as temperature and precipitation. Higher temperature typically results in wider rings and denser wood, but at some point parameters other than temperature may become the principle drivers of growth. For instance, drought may impede tree growth, and the dendroclimatologist's aim is to establish the quantitative relationship between these measurable attributes of the tree core and the climatic conditions that determine them.

Before scientists set out to core trees they must have an idea of what trees to look for; they do not blindly sample any tree they come across. Their choice is informed by often tacitly held knowledge of the field site, information gathered from colleagues, and the textbooks they have studied in their offices and laboratories from where they have virtual access to the pre-existing observations, i.e., in one way or another their choice is based on theory. They then pick those climate archives they know will harbour climatic information; if tree growth shall be limited by the parameter temperature, they select specimens that grow in high altitudes and high latitudes, where the growing season is short and growth depends on the climate parameter summer temperature. In dendrochronological theory a tree grows wider rings in warm summers than in cold, and in wet conditions than in dry. But they have to be careful because trees sometimes pose unpredictable problems which are not yet acknowledged by that theory, or generally difficult to account for.

In a comparatively unruly field laboratory (Kohler, 2002), several confounding factors must be taken into account. Tree growth, ring width and wood density may depend on other environmental variables such as the tree's physical shape, interspecies competition for sun and nutrients, the site's topography, insect infestation and CO<sub>2</sub> fertilisation. The list continues and may include to the scientist hitherto unknown factors. Having noted an anomalous tree ring measure, a dendroclimatologist at CRU warned his colleague: "There are some problems still. I note that [the year] 1032 is not

cold in [the Russian site of] Yamal. Seems odd. Is it cold in \*all\* of the three chronologies at issue? Or did a reindeer crap next to one of the trees?” (EAE 2009, 10 January 2006). Apparently the scientists do not know why the growth pattern of one tree is at odds with the others. The dendroclimatologist humorously dispels the anxieties involved in trying to control for all potentially compromising influences: As hitherto unacknowledged factors, even reindeer may have to be taken into account, but for practical reasons they are not.

When the dendroclimatologist has picked, packed and shipped home their sample cores, the calculation can begin. Because trees do not always grow at the same speed, but usually grow slower with age, any suitable tree’s expected growth curve is calculated and normalised by reference to a set of other samples from trees of the same species. Many more calculations follow in the laboratory where the tree cross sections have been shipped to. Data from different trees is averaged to smooth outliers and create a proxy chronology representing a certain local site. In order to arrive at the long chronologies used in climate reconstructions, cores from different trees, alive and dead, must then be standardised. Missing data is interpolated from existing chronologies or simply continued, that is, extrapolated by persistence of the final available value until recordings are again available. The result is a chronology describing the indicated climate variable. All these steps are documented, the location is noted and samples of trees are archived in order to trace the chronology back to each single tree (cf. Latour, 1999: chapter 2)

When the Mann team set out to reconstruct hemispheric climate change the climatic proxy variables they used had already been compiled in 415 columns and a row for each year. For Mann the trees, corals, sediments, etc., exist as homogenised data on spreadsheets. Their materiality – the particularity and locality of the field site from which the values in each cell were derived – have long been left behind and the calculations that were performed on them can be safely ignored. They are stored away but could, in theory at least, be retrieved whenever the final result that is the hockey stick curve is challenged. In their publication the scientists (generally) mention potential uncertainties within the data, but for *their* task at hand they do not delve into comparably esoteric controversies about a single outlier in one of their chronologies. For them the discussions over “reindeer crap” have been closed off. Now all the scientists are interested in is relative universality and a global climate history.



### *Scientific practice in the case of instrumental climate records*

Proxy variables are calibrated against instrumental temperature records from weather and climate stations on land and on sea (ships and buoys). In order to calibrate their proxy series, the Mann team made use of 20th century instrumental temperature series compiled by Phil Jones and Keith Briffa from CRU at UEA (Jones & Briffa, 1992). Collecting data from Bradley et al (1985) and Jones et al (1986) among others, Jones and Briffa formed the hitherto most comprehensive compilation of monthly instrumental temperature data. They merged countless single instrumental measurements into regional and global climate data sets. And though these instrumental temperature records are deemed the most reliable – they are more accurate than equivalent proxy data because the relationship between mercury used in thermometers and temperature is both better understood and, by the magic of standardisation, has less variation from one instrument record to the next than the relationship between ring width and temperature, – their construction is not as straightforward as it is presented in the publications either.

Various instruments and techniques exist to measure the climate parameter temperature, to calculate temporal averages, and to interpolate data from point measures over regional and global scales. Most commonly, the recorded daily minimum and maximum temperatures are averaged to a daily mean before monthly and annual temperatures are calculated by averaging those daily means. In order to arrive at a spatially consistent regional and global temperature, land and sea surface temperature averages are combined using statistical techniques. These quite heterogeneous data sets can be combined in at least two ways: by using constant weights which are roughly proportional to the areas of land and sea in each hemisphere, or by a combination of grid point land and grid point marine data into a 5°x5° grid box data set. The value in a grid box is the calculated average of sometimes both land and marine observations, often measured by entirely different instruments. Scientists can then correlate each regional grid value against global values to test for spatial representativeness of global temperature. The resulting global series is available in the form of grid point data of temperature anomalies; for example, for each month since 1902, Jones and Briffa (1992) calculate the anomaly against the reference period 1950-1979.

The construction of such data sets makes instrumental records no less open to challenge than proxy records. Many steps have been taken to transform temporally and

spatially unique observations into monthly and yearly regional and global data sets. The particularity and locality of unique measurements dissolves in representative signs allocated to grid boxes which ring-fence the globe. In this way point measurements have been made commensurable with the output of climate models and can be tested against these models, and vice versa. Model output can thus be used to help identify anomalous data in the temperature records, and to smooth them out. Thus what scientists may call direct or real instrumental temperature observations are actually moulded data. They are not truly real data, in the sense idealised by the discourse of ‘sound science’ we considered in Chapter 3. Rather, as Paul Edwards (2009: xv) notes, they are so-called models of data:

Philosophers of science use the phrase “models of data”; practising scientists might say “data analysis.” Data analysis models are used to process historical weather and climate records. Observing systems have changed so much and so often that you can only combine long-term records by modeling the effects of different instrument behaviors, data collection practices, weather station site changes, and hundreds of other factors. You also need models to adjust for the tremendous unevenness of observations in space and time. In this process, which I call making data global, coherent global data images are created from highly heterogeneous, time-varying observations.

As with the preparation of proxy data sets, in scientific publications of instrumental temperature series the many steps from the material world to representations thereof are closed off and hardly mentioned. Eventually also the relationship between mercury and temperature is based on physical laws that require explanation (Chang, 2007). That relationship is described as a constant which is derived from empirical measurements, which, once again, contain some element of theory. And as in proxy data sets, the instrumental products too factor out many sources of bias which scientists have had to account for in their data analysis. Potential errors, as Jones and Briffa (1992) acknowledge, include changes in station locations, observing schedules and practices, in thermometer exposure, in the environments (especially urbanisation around many stations) and in spatial coverage. These biases affect single stations and the calculation of regional averages. In particular, the comparison of recordings from different stations must take many more variables into account, such as differences in station elevation – in scientific theory temperature decreases with altitude according to a so-called lapse rate – and differences in the calculation of monthly mean temperatures.

Sea surface and sea air temperature measurements are also problematic;

potential errors in measurement are smoothed out with sophisticated techniques. For example, the so-called bucket measurements are adjusted to accord with so-called intake measurements. Gaps in data and potential errors from changes in location, observers and techniques pose yet more problems. Such ‘noise’ or measurement errors are typically smoothed out in the aggregation of temporal averages onto a grid scale. Indeed, this is one of the attractions of using gridded data: scientists hope that errors are randomly distributed and will average themselves out through aggregation, leaving a clean climate signal. As such they are often deemed more reliable than the direct observations of an expanding and contracting mercury column.

Jones and Briffa (1992) conclude to be fairly confident that the world has warmed by about 0.5°C since the late 19th century. The Mann team certainly regarded their temperature product as most reliable and apt for their purposes (1998: 779):

Monthly instrumental land air and sea surface temperature grid-point data [...] from the period 1902–95 are used to calibrate the proxy data set. Although there are notable spatial gaps, this network covers significant enough portions of the globe to form reliable estimates of Northern Hemisphere mean temperature.

Jones and Briffa’s data and metadata are archived at CRU and their methodologies are briefly described in the scientific publication and on the institution’s website. If other scientists want to replicate the original studies, or perform further calculations, they have to track down the data, which, alongside the temperature series produced by the NOAA/NCDC and the NASA/GISS, have become what science scholar Michel Callon (1986) would call an ‘obligatory passage point’ for authoritative claims of global temperature rise in the 20th century.<sup>15</sup>

#### *The calibration exercise in MBH98/99*

In the Mann team’s calibration exercise climate proxies were weighted according to their so-called temperature sensitivity and calibrated against Jones and Briffa’s instrumental temperature time series. But because the Mann team’s proxy series are cut off in the year 1980, they only use a gridded 1902–1980 instrumental temperature product. Calibration simply means that a mathematical relationship

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15 The concept of an obligatory passage point is likened to the narrow end of a funnel that forces actors and challengers to take this route. These can take the form of important datasets, algorithms or computer models, but also of institutions whose reports have become essential when it comes to talking authoritatively about the reality of a phenomenon. Any claim against the reality of said phenomenon must pass through these scientists’ knowledge domains.

between proxy variables (for instance tree ring width and instrumental temperature) is established via regression analysis. In a multi-proxy reconstruction this is complicated by the great number of heterogeneous proxies. For this reason the Mann team chose to use the so-called climate field reconstruction (CFR) method after dendrochronologist Hal Fritts (Fritts *et al* 1971).

Out of a randomly chosen proxy index, CFR chooses temperature-sensitive proxies over non-sensitive proxies. The scientists use this well-established method to justify the selection of relevant proxies. To this end 415 proxy chronologies have been summarised into 112 dominant ‘indicators’ or ‘proxy series’, some of which are again summaries of larger networks of proxies. The summary is performed using so-called principle component analysis (PCA). PCA provides a natural smoothing of the temperature fields in terms of a small number of dominant patterns of variability or ‘empirical eigenvectors’ (Mann *et al* 1998: 781), i.e., PCA finds the key pattern in the data’s variance.<sup>16</sup> The Mann team thus isolated the dominant patterns of the instrumental surface temperature and proxy data. For example, Jones & Briffa’s 20th century instrumental data were reduced to 16 dominant patterns or ‘climate fields’. The Mann team then calibrated the individual climate proxy indicators against the time histories of these distinct patterns during their mutual interval of overlap (Mann *et al* 1998: 780).

At least three fundamental assumptions are implicit in the team’s calibration exercise. First, because in the calibration exercise some regions are not directly represented but indirectly through so-called teleconnections with regions that are, the Mann team assumes that these teleconnections hold. Second, the Mann team assumes that indicators used in the calibration exercise are linearly related to one or more of the so-called instrumental training patterns. In the relatively unlikely event of a proxy indicator representing a local climate phenomenon which is uncorrelated with larger-scale climate variations (or representing a highly non-linear response to climate variations) this assumption will not hold. Third, the patterns of variability captured by the multi-proxy network must have analogies in the patterns they resolve in the instrumental data. This last assumption is called the stationarity requirement. According to the stationarity requirement the statistical relationship between the proxies and the

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<sup>16</sup> The Mann team explained: “Each of these eigenvectors is associated with a characteristic spatial pattern or ‘empirical orthogonal function’(EOF), and its characteristic evolution in time or principal component (PC). The first PC filters data that explain most of the variance, for example 60%. The second explains less, the third only a fraction, and so forth” (Mann *et al* 1998: 781).

climate variable must be the same throughout the calibration, validation and reconstruction periods.

Though these assumptions “involve a certain leap of faith to trust that trees that are not responding to their own local temperature can nevertheless detect a signal in a wider temperature index,” as Montford noted (2010, 47), they must hold for the final reconstruction to be deemed ‘robust’. Whether the algorithm of the calibrated relationship during 1902-1980 is indeed correct is checked in the next step. The Mann team argued that “[t]he statistical cross-validation exercises we describe later provide the best evidence that these key underlying assumptions hold” (1998: 781).

#### *The verification and reconstruction steps*

The Mann team study continued with a verification exercise to establish “the skill of the temperature reconstruction” (Mann *et al* 1998: 781). Here the statistical validity of the algorithm as applied in the calibration period was tested in the verification period of 1854-1901 against previously withheld instrumental data sets. Once the calibration period tells the scientists how temperature translates into ring width, the verification period serves to check whether this relationship is statistically valid, i.e., the researchers want to know if any calculated relationship arises purely by chance. Various statistical techniques or methodologies exist to verify the algorithm (e.g., Reduction of Error (RE), squared correlation (R2), Coefficient of Efficiency (CE)). The primary diagnostic of the Mann team’s reconstructive skill is the R2 in which the algorithm calculates the square of each value resulting in a sum of squares – the smaller the sum, the more effective is the algorithm.

If the algorithm is deemed effective in the verification period, the Mann team can use it for the comparatively straightforward reconstruction exercise, which is done in a few steps. Using the respective available proxy series, the Mann team reconstructed temperatures for the periods from 1850-1980, 1800-1980, 1750-1980 back to 1400-1980 (Mann *et al* 1998; Mann *et al* 1999 includes the period from 1000-1980). The periods were “spliced” together and the instrumental record from 1980-1998 was added to form a coherent and up-to-date graph. A computer code written by Michael Mann then executed the reconstruction, which, speaking to Congress, he likened to the building of a house (HCEC, 2006: 726):

And you wanted to build a house and the data would be the materials you need to build the house, the nails, the wood, et cetera. The algorithm would be the architectural plan. Now what would the code be? Well, imagine that instead of builders you had a computer to make your house for you. Well, the code would be implementing the architectural plan by telling the computer to pick up the hammer, pick up the nail, hammer it in. And so the code is simply implementing the algorithm but the real scientific process is embodied within the algorithm.

## *Results*

In 1998 the Mann team summarised their findings in *Nature* (Mann *et al* 1998: 779):

Spatially resolved global reconstructions of annual surface temperature patterns over the past six centuries are based on the multivariate calibration of widely distributed high-resolution proxy climate indicators. Time-dependent correlations of the reconstructions with time-series records representing changes in greenhouse-gas concentrations, solar irradiance, and volcanic aerosols suggest that each of these factors has contributed to the climate variability of the past 400 years, with greenhouse gases emerging as the dominant force during the twentieth century. Northern Hemisphere mean annual temperatures for three of the past eight years are warmer than any other year since (at least) AD 1400.

And in the extended version of the hockey stick graph, published a year later in *GRL*, the Mann team came to the following conclusion (Mann *et al* 1999: 759):

Building on recent studies, we attempt hemispheric temperature reconstructions with proxy data networks for the past millennium. We focus not just on the reconstructions, but the uncertainties therein, and important caveats. Though expanded uncertainties prevent decisive conclusions for the period to AD 1400 our results suggest that the latter 20th century is anomalous in the context of at least the last millennium. The 1990s was the warmest decade, and 1998 the warmest year, at moderately high levels of confidence. The 20th century counters a millennial-scale cooling trend which is consistent with long-term astronomical forcing.

### ***4.3 The popularisation of the hockey stick***

In 1998 “Northern Hemispheric Temperature Patterns and the Climate Forcing over the Past Six Centuries” was presented to the readership of *Nature*. The graph came with a description of data and was accompanied by equations, tables, figures and supplementary methodological information. The language was sober and the many numbers and equations made it a rather inaccessible scientific literature for the layperson. A year later the Mann team published a follow-up study in *GRL* in which they extended the graph to cover a millennium. Despite appearing in prestigious scientific journals, comparably few people within the scientific community showed an

interest in the articles, and even fewer out in the public sphere.<sup>17</sup>

In 2001 the IPCC TAR reprinted the graph with a short description in both the scientific section and the report's SPM. The publicity was a great success for Mann, his colleagues, and the palaeoclimate community in general. Many palaeoclimatologists felt they had lost ground to climate modellers, whose work was well covered in the first and second IPCC assessment reports in 1990 and 1996, and were keen to make a better case for their work in the TAR.<sup>18</sup> In an email the contributing author to the IPCC 2001 report Jonathan Overpeck commented to Phil Jones: "The IPCC next time around should be much stronger than last on the paleo side of things (although still not as good as it can get!)" (EAE, 2009: 1 October 1998). In an email to colleagues Michael Mann, then lead-author to the IPCC palaeoclimate chapter, tried to gather support (EAE, 2009: 6 October 1998):

There is indeed, as many of us are aware, at least one key player in the modeling community that has made overly dismissive statements about the value of proxy data of late, because of what might be argued as his/her own naive assessment/analysis of these data. This presents the danger of just the sort of backlash that Keith [Briffa] warns of, and makes all the more pressing the need for more of a community-wide strategizing on our part.

But the idea of highlighting the hockey stick in the TAR's SPM was not unanimously shared. Neither was there agreement among palaeoclimatologists about the truth-value of the graph (cf. Ryghaug & Skølsvold, 2010). In an email Chris Folland, then coordinating lead-author, wrote to contributing palaeoclimatologists (EAE, 2009: 22 September 1999):

A proxy diagram of temperature change is a clear favourite for the Policy Makers summary. But the current diagram with the tree ring only data somewhat contradicts the multiproxy curve and dilutes the message rather significantly. We want the truth. Mike [Mann] thinks it lies nearer his result (which seems in accord with what we know about worldwide mountain glaciers and, less clearly, suspect about solar variations).

Keith Briffa, a contributing author to the respective chapter, replied (EAE, 2009: 22 September 1999):

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17 In the scientific literature Mann *et al* 1999 was cited 42 times in 2000, 42 times 2001, but twice as often in the two years after its publication in the IPCC TAR in 2001 report (ISI web of knowledge, Accessed on 17 March 2014).

18 Mike Hulme (personal communication) suggests geopolitical imperatives as one reason for the under-representation of palaeoclimatological research in the IPCC's first and second assessment reports. While Russian researchers used to be strong in palaeoclimatology in the immediate proximity of the Cold War the IPCC did not include much Russian science. Drawing on an Indian case study, in a first analysis on the geography of the IPCC, Kandiklar and Sagar (1999) note an over-representation of Western scientists as IPCC authors (cf. Hulme & Mahony, 2010).

Let me say that I don't mind what you put in the policy makers summary if there is a general consensus (sic.). However some general discussion would be valuable [...] I know Mike thinks his series is the 'best' and he might be right – but he may also be too dismissive of other data and possibly over confident in his (or should I say his use of other's). After all, the early (pre-instrumental) data are much less reliable as indicators of global temperature than is apparent in modern calibrations that include them and when we don't know the precise role of particular proxies in the earlier portions of reconstruction it remains problematic to assign genuine confidence limits at multidecadal and longer timescales [...] There is still a potential problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilisation through high CO<sub>2</sub> or nitrate input). I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored.

Despite these noises of private disagreement, the chapter was published as a consensus report. “From the outside, one should be seen to be in agreement,” observe Ryghaug and Skjølsvold (2010). And eventually the publication in the SPM served the community well – or so it seemed at the time. With the rise of climate science to global importance, palaeoclimatological research gained popularity and arguably also funding.<sup>19</sup>

The Mann team's work was subsequently cited in public lectures and presentations, in the popular science literature, even in TV. By the time it was featured in Al Gore's Oscar-winning documentary *An Inconvenient Truth* (Gore, 2006), the Mann team's science had been stripped down to the bare stick; the grainy and roughly hewn graph had become a smooth hockey stick lacking the detailed qualifying information provided in the original. For that, the reader was pointed towards the original papers.

In the wider context of climate change politics the hockey stick served as critical and convincing observational test of the theory of AGW. Importantly, it could be deployed as an empirically grounded argument against sceptics who criticised climate science for its heavy reliance on computer models and modelled data. If climate models were not taken seriously by climate sceptics, the Mann team's climate reconstruction would surely tip the balance of evidence in favour of AGW; during the years of the Republican Administration under George W. Bush (2000-2008), when climate sceptics occupied important government offices, such an empirical test would prove all the more invaluable. In that way the hockey stick graph became ever so

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<sup>19</sup> Between 1990 and 1999, 247 scientific titles included “palaeoclimat\*”, and in the following decade (2000-2009) 570 titles did (ISI web of knowledge; accessed on 12 October 2013)



tightly linked to the politics of climate change, at least in the US.

Its celebration has made the hockey stick probably the most popular but also the most contested icon of climate science – it became what Latour (2010) calls a “factish god” that would soon attract iconoclasts. Indeed, two years after the IPCC prominently exhibited the graph, the retired Canadian mining consultant Steven McIntyre took a closer look at MBH98 and spotted inconsistencies. Michael Mann initially fended off McIntyre’s criticism, but as more sceptics and scientists joined the debate he soon found himself in the midst of *The Climate Wars*, as Mann (2012a) would later describe the episode. Both on blogs and in the scientific literature, Mann and McIntyre rejected each other’s arguments, revealing ever more details about the graph’s construction. McIntyre’s meticulous dissection of the graph, his extensive critique, and Michael Mann’s defence thus provide unique insights into the practices of palaeoclimatology. This chapter proceeds with a description and analysis of McIntyre’s deconstruction of the hockey stick graph.

#### ***4.4 Deconstructing hockey stick science***

In 2002 Steven McIntyre took an interest in the graph after he had seen it in materials distributed by the Canadian government, probably in promotion of climate policies. He first delved into MBH98, but the team’s unwieldy explanation of their many steps made it difficult for the self-styled climate auditor to understand what exactly the scientists had done. While the space-saving word limit for scientific articles in *Nature* generally does not allow for an exhaustive description, editors at *Nature* also presume that only insiders who read and judge the papers have the tacit knowledge to appreciate what goes unsaid. Thus outsider McIntyre had to contact corresponding author Michael Mann; he wanted to reproduce every single step performed by the scientists so as to trace the graph to its material reality, that is, past atmospheric conditions as recorded by instruments and captured in climate archives. But while the article in *Nature* depicted the graph as a fairly straightforward representation of reality arrived at by way of the scientific method, McIntyre was bewildered by its complicated construction. For McIntyre and fellow critics, that construction of the graph opened plenty of vantage points. If only one step in the building of Mann’s “house” failed to live up to the scientific method, according to the philosophy of ‘sound science’, the

hockey stick study would be proven wrong. To help him navigate these novel scientific waters, McIntyre sided with professor of economics Ross McKittrick who knew how to publish in scientific journals.

### *Collecting the materials*

McIntyre wrote several emails to Michael Mann in which he politely asked for the data used in the hockey stick experiment. Mann responded late, arguably because he was not able to readily provide data. This only spurred McIntyre, since as soon as he saw the first figures he noted calculations on some of the data that had not been reported in *Nature*. Puzzled but intrigued, he began to examine each of the 415 proxy chronologies, many of which had become obligatory passage points for understanding millennial climate change. Andrew Montford (2010: 73) recalled in his take on the controversy: “The fact that the data had never been compiled into a single record also strongly suggested that nobody had ever asked to see the figures before.” Evidently, it took an outsider to replicate the hockey stick graph. But first McIntyre needed to know which proxy series had been used in which time step of which PCA calculations.

When McIntyre continued to request data, chief researchers from CRU decided to close ranks and, though somewhat reluctantly, to support Mann. One of the scientists wrote to his colleagues at CRU (EAE, 2009: 12 November 2003):

[Y]ou will have seen Stephen McIntyre’s request to us. We need to talk about it, though my initial feeling is that we should turn it down (with carefully worded/explained reason) as another interim stage and prefer to make our input at the peer-review stage. In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out preliminary and incorrect early responses – the waters are really muddied now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early or now deleted is really confusing things! Anyway, because McIntyre has now asked Mann directly for his data and programs, his request that \*we\* send McIntyre’s request to Mann has been dropped (I would have said “no” anyway).

What climate sceptics would call the ‘hockey team’ made it ever more difficult for McIntyre to fully understand the experiment.

McIntyre grew impatient and tried alternative avenues to gain access to technical information. An allegedly easy way was formal material complaints to *Nature*. Because information was still lacking by 2006, McIntyre drafted a letter of complaint to its editors as well as to the president of the NAS, Ralph Cicerone. In his

letter McIntyre reiterated the climate sceptics' demand for 'sound science': "Given the influence of MBH98, meticulous verification should be possible and this cannot be accomplished with the [hitherto] requested disclosure" (HCEC, 2006: 794). He would accept the truth of the graph if, and only if, it could be replicated and verified. Both *Nature* and Cicerone rejected his requests, either because they went substantially beyond what they would normally expect authors to provide, or because they had no authority over all those entities.

An increasingly irritated McIntyre decided to force the release of all relevant material by law. Using the Freedom of Information Act (FoIA), which is intended to provide a mechanism whereby information held by public authorities can be accessed by the public, he and a UK citizen named David Holland addressed the scientists' host institutions.<sup>20</sup> Though the FoIA is rarely used to gain access to scientific information, they decided to press for all data, Mann's computer code, as well as the scientists' internal communications. "Without Mann's input it was almost impossible to get an exact replication of the Hockey Stick," revealed Andrew Montford (2010) echoing the climate auditor. Again McIntyre was denied the disclosure of the computer code, however this time with a nod to intellectual property rights (IPRs). And David Holland's request was denied by the Met Office and the Ministry of Defence (MoD) with the argument that correspondence related to IPCC assessments was confidential.<sup>21</sup>

McIntyre's replication exercise became an increasingly frustrating pursuit. He had exhausted all options and felt as if the climate scientists had put obstructions in all of his necessary points of passage. And whenever he highlighted new points of contention, he said, "the nomads appear to have de-camped" (McIntyre, 2006). "Whenever anyone criticizes one of their papers, they've moved on. Never a place to

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20 In 2007 McIntyre filed FoIA complaints directed at Mann's host institution, the University of Virginia (UVA) in the US, and the UK's UEA as host institution of CRU scientists. Holland filed complaints to the British Met Office. Under section 12b of the Environmental Information Regulation (EIR), these public authorities must disclose information unless the public interest in maintaining the exemption outweighs the public interest in disclosing the information in all the circumstances of the case (cf. Abbot & Marohasy, 2010).

21 The Met Office replied that the requested information was produced in scientists' private capacity and on behalf of the IPCC, which was exempt from all countries' FOIAs. They argued that "[t]he release of information considered to be confidential could deter other scientists from participating in the IPCC or other similar scientific processes. Scientists would be likely to be inhibited from the free and frank advice or the free frank exchange of views for purposes of deliberation. It is essential to protect a free space in which scientists can think through the implications of various options under consideration [...] As a great deal of the IPCC process is undertaken during meetings and unrecorded telephone conversations, the requested information may not present a full picture, may not be of the IPCC and if taken out of context could do more to mislead the general public unnecessarily and on an unwarranted basis" (Met Office, cited in Abbot & Marohasy, 2010: 9).

lay one's head even for one night," he fretted. Any time McIntyre criticised one of their statements, or hypotheses, he would find it protected by further auxiliary explanations and hypotheses. In the end "[t]here was nothing for it except to use trial and error to try to discover the exact combination of methodological steps that Mann had used," Montford summarised (2010: 86).

### *Opaque calculations and methodological choices*

McIntyre published his initial findings in the journal *Energy & Environment* in 2003 (McIntyre & McKittrick, 2003). Among the 415 chronologies used by the Mann team he found dozens of items that proved difficult to replicate. Series had been truncated, inter- or extrapolated without any notice and justification. At times values had been added to allow for the inclusion of temperature-sensitive series: the so-called Gaspe (peninsula) tree series had been extrapolated to allow for its inclusion in the 1400-1980 proxy roster which includes the stick's flat shaft. According to McIntyre & McKittrick (2003), this data manipulation served to flatten the graph in the 15th century. Again other series had been bizarrely mislocated: a proxy indicator measured in North Eastern America had been assigned to a grid cell over France. "The rain in Maine falls mainly in the Seine," McIntyre (2005a) commented bitinglly. Other series had been used twice, were inaccurately cited, erroneously said to have been included, and so forth. At one point it was even unclear whether the Mann team had summarised the 415 chronologies into the reported 112 or 159 series. "[W]ith Mann's description of his methods being so vague, it was still a hard task to work out exactly what he'd done" (Montford, 2010: 78).

Though "[i]n most cases these simply present obstacles to replication [...] in at least one instance there is a serious issue of whether the series was edited with a view to affecting the final result," McIntyre & McKittrick (2006) remarked in a letter to the NAS. McIntyre detected this problematic proxy series while examining those that had been weighted highly in the calibration step. Apparently the Mann team had included a hockey stick-shaped tree chronology whose reliability to capture 20th century climate even its creator Donald Graybill doubted (in Graybill & Idso, 1993). As McIntyre contended in a Congressional hearing (HCEC, 2006: 792),

[o]ne of Mann's most important methodologies was his introduction of bristlecone and foxtail pine growth chronologies into proxy reconstructions. As early as the 1980s, these trees were known to have experienced an anomalous pulse in growth in the 20th century,

which specialists had concluded was unrelated to temperature, and speculated that it was due to CO<sub>2</sub> fertilization.<sup>22</sup>

Like the Gaspé series, the Bristlecone pine series had been included based on anything but “clear analytic a priori criteria,” noticed McIntyre (2005a). The Mann team had violated the scientific method as described in the philosophy of ‘sound science’.

McIntyre’s replication exercise revealed more contestable methodological choices. In 2005 he published his critique on the Mann team’s so-called ‘short-centred’ PCA. Because the Mann team had not adjusted the data to a mean of zero as in ‘normal centring’, the team’s PCA unduly prioritised any series with great variance. In other words, the team’s method gave series with great 20th century divergence from the long-term mean disproportionately high weight. McIntyre and McKittrick (2005ab) found that, contrary to what was claimed by the scientists, the allocation of weight was not justified by theory: if and only if data were normally centred, would the algorithm point automatically to the most important series. In his own experiments, McIntyre showed that the wrong choice could produce so-called ‘artificial hockey sticks’ (AHS) even out of randomly generated time series (McIntyre & McKittrick, 2005a). And though German climate scientists Hans von Storch and Eduardo Zorita (2005) concluded that Mann’s method alone would not necessarily influence the final result, McIntyre and McKittrick (2005c) insisted that it had done so precisely because it prioritised questionable proxy series as the foundation blocks of Mann’s house of cards.

Because the widespread acceptance of the hockey stick was related to the authors’ claims of unprecedented statistical skill, McIntyre next took issue with the verification statistics of the team’s proxy-temperature correlation (McIntyre & McKittrick, 2003; 2005ab). Apparently Mann had withheld certain validation statistics so as to make it impossible for anyone to gauge the true reliability of the reconstruction (cf. Montford, 2010). And in the important 1400-1980 time step of the reconstruction, McIntyre’s R<sup>2</sup> verification statistic values were extremely low, rendering insignificant the correlation established in the team’s calibration exercise. McIntyre thus concluded that the reconstruction was anything but robust; it did not show the “skill” the team had claimed it did, but proved a “catastrophic failure” in the verification test (McIntyre & McKittrick, 2006). To be sure, opinions on the appropriate use of verification statistics

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<sup>22</sup> Later he retracted: “For the record, I’m not sold on claims that bristlecone growth pulses are due to CO<sub>2</sub> fertilization as argued by Graybill and Idso” (McIntyre, 2009).

still differ among and between sceptics, mainstream statisticians, and the palaeoclimate community, and subsequent examinations of both parties' claims recommended the use of several statistical validation metrics in order to account for the trade-offs between single metrics (e.g. between precision and accuracy) (NRC, 2006; Wegman *et al* 2006).

The better McIntyre understood what the Mann team had done in their convoluted study, the more he became convinced that the hockey stick was an untrue (re)construction. He endorsed a study by climate scientists Gerd Bürger and Ulrich Cubasch (2005) who repeated the possibility of a different climate history. The reconstruction was “highly sensitive to the variation of [...] criteria [...], resulting in an entire spectrum of possible climate histories,” Bürger and Cubasch wrote. They found that, depending on six binary methodological choices, there were at least 64 “reasonable” shapes the reconstruction could take. And since “[n]o a priori, purely theoretical argument allows us to select one out of the 64 as being the ‘true’ reconstruction” (Bürger & Cubasch, 2005), the Mann team’s graph could be dismissed – though not out of logical necessity – as the scientists’ preferred choice among 64. “Music to our ears” was McIntyre’s description of what he saw as a “devastating critique of MBH98-type reconstructions” (McIntyre, 2006).

Climate sceptics in Congress celebrated the breaking of the hockey stick (Inhofe, 2006). The hockey stick graph had been refuted and AGW was falsified, triumphantly concluded fellow sceptics on various blogs.

Scientists interpreted the results differently. Asked in Congress whether he deemed Mann’s statistical analysis incorrect, Gerald North, who led a well-respected investigation (NRC, 2006) into the scientific controversy, answered: “Well, we found that it was not – there were many choices to make. They probably didn’t make the best choice when they did the analysis the way they did” (HCEC, 2006: 101). But McIntyre had certainly triggered a “healthy and broad discussion of the issue,” concluded Hans von Storch (2007). And since then climate scientists have found ways to exclude at least some variants proposed by Bürger and Cubasch on purely theoretical grounds.<sup>23</sup>

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23 For example, the variant based on inverse calibration is not justified because it requires the proxy to be error-free. The alternative, classic calibration is more correct because the error term in the measured temperature is very likely smaller. Another example is the detrending issue: theoretically, a regression analysis must be conducted on stationary data. If the time series of proxy and temperature display a trend, the estimation of the regression coefficients can be considerably biased. In those cases a so-called co-integration model should be used. A third example is related to the variance re-scaling, which is an ad-hoc procedure that has no theoretical grounding at all. It is still applied and, quite remarkably, turns out to be the best method of those tested in pseudo-proxy experiments.

### *Disputed trees*

Steven McIntyre's meticulous critique of the team's methodological choices revealed the many contestable ad-hoc steps in the reconstruction of the Northern hemispheric climate. Evidently, different methodologies could or should have been used, data could or should have been manipulated differently. And though he initially welcomed Bürger's studies, McIntyre has since become increasingly doubtful of whether a methodologically 'sound' and satisfying solution could be found. While Mann's PhD student Scott Rutherford and other colleagues gathered in defence of the team's choices (Rutherford *et al* 2005; Wahl & Ammann, 2007), McIntyre had already moved on: "The real problem," he announced in a Congressional hearing, "is that the [PCA] method as applied to low quality data caused a minor pattern, in this case bristlecones, to be exaggerated as a dominant pattern in worldwide climate" (HCEC, 2006: 681). Revealing his ambition, McIntyre argued that "[e]very construction using bristlecones will have to be reconsidered."

McIntyre's main focus shifted from the scientists' opaque calculations and methodological choices to the proxy data and notably the bristlecone pines: Did those trees really record changes in temperature? Michael Mann and many of his colleagues certainly thought so. In rare agreement with the sceptics over the superfluity of endless methodological debates, Mann insisted that the hockey stick pattern was a straightforward representation of Northern hemispheric temperature variability (HCEC, 2006: 178, emphasis added):

My critics also fail to recognize that even if their criticisms are accepted, it has no bearing on the outcome. Dr. Wegman's report argues that the hockey stick pattern derives from the statistical conventions used in our 1998 and 1999 studies. However, using alternative statistical conventions yields the same hockey stick pattern. *The hockey stick pattern is intrinsic to the data.* That was the conclusion of the National Academy. Page 116 of the National Academy report [NRC, 2006] says the statistical convention my colleagues and I used "does not appear to unduly influence reconstructions of hemispheric mean temperature; reconstructions performed without using principal component analysis are qualitatively similar to the original curves presented by Mann et al."

In fact, the professional statistician Edward Wegman, who Republican representatives asked to evaluate the Mann team's statistics, argued along similar lines: "[I]f you use a nice set of proxies that all have the same signal in them then it really doesn't matter a

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Sometimes scientists choose theoretically incorrect methods, so called ad-hoc methods, which turn out to perform better than more sophisticated and theoretically more convoluted methods. Here the idealised boundary between analytic and synthetic sentences is porous if not completely blurred (Eduardo Zorita, personal communication).

whole lot what [statistical] methodology you use” (HCEC, 2006: 735). “The shape of the graph will depend on the underlying data,” he agreed with Mann.

Mann, Bradley and Hughes understood data from bristlecone pines to be temperature proxies, and based on that theoretical understanding, the team included them in their study. McIntyre on the other hand was convinced that bristlecone pines were not reliable “antennae of world climate” (McIntyre, 2009). They would record drought rather than temperature, he and fellow critic Anthony Watts argued; they were rain gauges rather than thermometers. What is more, if the bristlecone pines’ late 20th century growth was indeed controlled by CO<sub>2</sub> fertilisation, as has been suggested by Donald Graybill, the stationary requirement, as one of the Mann team’s three fundamental assumptions, would fail. This problem of divergence between proxy and instrumental temperature data after the 1960s puts into question the trees’ ability to reconstruct the temperature in previous centuries.

McIntyre’s critique had been anticipated by dendroclimatologists at CRU. Early on they recognised that scientific uncertainties would pose a problem for the communication of their results. In 1998 Keith Briffa warned in an email to colleagues (EAE, 2009: 6 October 1998):

[W]e should be very wary of seeming to damn certain proxies and overhype others when we all know that there are real strengths and weaknesses [associated] with them all. The truth is that all of this group are well aware of this and of the associated fact that even within each of these sub-disciplines e.g. Dendro, coral etc. there is a large range of values, or concern with the external usage of our data. However, my own and Phil’s concerns are motivated, like yourself, by the outside world’s inability to appreciate these points and the danger that we will all be seen as uncritical or [naive] about the real value of proxy data.

Two years later, in another email, Briffa expressed “[suspensions] as to whether the negative trend in Mike’s hockey stick prior to the 20th century is not at least partly the result of a trend in the [...] high elevation western US trees he uses” (EAE, 2009: 1 October 2000). Yet arguably because of the significant political pressure to express the voice of climate science as unified voice, such criticism was muted in the 2001 IPCC report. For Andrew Montford “[Mann’s] failure to observe the divergence, or worse, if he had failed to report it at all, must seriously undermine the credibility of the Hockey Stick papers” (2010: 65).

Unaware of the scientists’ doubts, McIntyre decided to find out for himself whether bristlecone pines acted as rain gauges, CO<sub>2</sub> filters or thermometers. In late



2007 he ascended to the trees' habitat in the high Rocky Mountains to replicate and update Graybill's chronology.

To McIntyre's dismay, Graybill's 'Mannian data management', as sceptics would call it, impeded his exercise. He struggled to track down Graybill's trees, many of which were not even archived, but eventually managed to identify some of the tagged trees, retrieved his own cores, and with the help of dendroclimatologists from Arizona University compiled his own chronology. In a talk to the American Geophysical Union (AGU) McIntyre preliminarily concluded that tree growth was moisture-limited – despite climate change, their ring width declined in the globally hot 1990s and 2000s (McIntyre & Holzmann, 2007). And to make his expedition appear more credible than Graybill's, McIntyre provided extensive metadata in form of various maps, GPS data, pictures and rather extensive expedition reports so as to ensure readers of his determination.<sup>24</sup>

Meanwhile, other researchers had updated several existing bristlecone chronologies and drew the sceptics' attention to how tree physiognomy, soil sensitivity, precipitation, temperature and other variables all influence bristlecone pine growth on different time scales. Many hitherto little acknowledged factors all of a sudden demanded to be taken into account. And while Ababneh (2008) served McIntyre to argue against the inclusion of bristlecone pines in climate reconstruction, in 2009 a team of Arizona dendroclimatologists (Salzer *et al* 2009) described bristlecone pines from a site 120 miles south and 80 metres subjacent to McIntyre's, and therefore theoretically less temperature-dependent, as quite robust thermometers. By showing that bristlecone pines at this site are temperature sensitive, the study by Matthew Salzer and colleagues has served the Mann team in its defence of the hockey stick.

Ultimately, neither Donald Graybill nor Steven McIntyre can know without doubt whose experiment is correct and whose data reflect true temperature variability.

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24 Reads an excerpt of McIntyre's expedition report (2007):

When we left this site, after a little while, we encountered the following situation in the road. It's actually worse than it looks. The boulder would rip the underbelly of the truck we were in, stranding us on the mountain and the sides of the road on either side were pretty steeply pitched. Pete is a skillful 4-wheeler driver in addition to other skills (computers, wood working) and figured out how to slalom the 4-wheeler down the gully, riding the wheels just to the side of the boulder with the truck at what seemed to be about 35 (Pete amends: 35-45) degree angle to the horizontal, while the rest of us held branches back at the side with all our limited might. We got off the mountain with a deep \$500 scratch to the truck and thanked our stars that that was all. You'd want a different and less highway-comfortable vehicle for this section of the road, that's for sure. But at least there was no precipice at the side and the pictures of a similar situation in Pete's later adventures look much more hair-raising.

Aggravated by the unstable conditions in the high Rocky Mountains, the practical skills of dendroclimatological experimentation and the theoretical commitments, described as the experimenter's regress we have discussed in Chapter 3, hamper the succeeding scientist's replication exercise. First, the trees and the environment have changed since the early 1990s; some trees may since have debarked, introducing a 'strip-bark bias'. Second, McIntyre does not know for sure which of the cored trees Graybill actually used. Neither does he know why he decided to exclude samples from analysis. Some trees may have been cored at least twice and at different heights for reasons that elude the replicating experimenter – a taller scientist may decide to drill at a different position than their shorter colleague. Irrespective of that, an *exact* replication, as has been frequently demanded by sceptics, is impossible since a tree can never be cored twice at the same location.

Many of these choices made in experimentation are tacitly held and agreed upon by experienced scientists; they can hardly be articulated (Polanyi, 1958; Collins, 1985; 2010). Michael Mann so justified the interpolating of proxy data sets with a nod to his colleague's tacit knowledge (HCEC, 2006: 799):

Given Dr. Hughes' long-standing collaboration in field and laboratory with the authors [...], his detailed knowledge of their working methods, and his own direct experience of working with the same species, with these colleagues, and in the same region he recommended that we fill this major spatial gap by digitizing these data, and also made recommendations on which should be used [...] Our procedure was in fact as objective and rigorous as possible.

And Steven McIntyre too inferred from experience (cited in Montford 2010: 76):

At the time that [Mann's Gaspé] series was making its big excursion, I happened to be working for the Canadian company which owned the Gaspé. It's certainly hard to think of a reason why trees in Gaspé were making a 3 [standard deviation] excursion in the 1970s.

Since neither McIntyre nor Mann had access to respective information, successful replication would have required close collaboration between the two. But the sceptics' demand for an independent replication and the hostility between Mann and McIntyre ruled that out. Thus the scientists and the sceptics once again argued with the scientific method: Salzer et al (2009) held that Graybill had used inappropriate methods of standardisation, and McIntyre criticised Salzer *et al* (2009) for using modelled high-altitude temperature in the proxy-temperature correlation exercise (McIntyre, 2009). In line with the credentialed climatologists and physicists we encountered in Chapter 3, he argued that modelled data would not qualify as real data.

The logical empiricist principles of ‘sound science’ have so provided plenty of vantage points for critics, and the more complex the reconstruction, the more potential problems can be located. “There are so many different sources of data and so many ways of interpreting them, that there is much space for disagreements and no single set of logical criteria that allows for easy closure of the controversy,” conclude Ryghaug and Skølsvold (2010). According to Andrew Montford (2010: 77),

[t]he trouble started while McIntyre was trying to replicate one of the PC analyses [...] There may of course have been a rational explanation for this (why some series were worthy a PC compilation in their own right and others were not), but Mann’s paper was silent on the subject [...] Again Mann gave no rationale in the paper for what appeared to be an entirely illogical approach to the question of data compilation.

### *Disputed instrumental climate data*

Climate sceptics have since followed McIntyre’s strategy and examined also weather and climate stations. The compiled instrumental temperature records attract critics, not only because the Mann team used them in the calibration and verification exercises, but because they are often referred to as independent lines of evidence for AGW. The rationale for deconstructing these records is the same: like proxy series, instrumental temperature products factor out many sources of bias that scientists should have accounted for. Fred Singer (2011: 16) so asserted that

[i]t is well known that the quality of the surface temperature data is dubious [...] A further warming bias comes from the selection of stations, with the ‘best’ stations usually located at airports. While airports may generally be warming, that’s not global warming. None of the investigations of the Climategate principals has delved into this question. At the present time, the Berkeley-Earth Project is investigating this difficult but important matter. We should wait to see what they report.

Fellow sceptic and meteorologist Anthony Watts too believes that the changes in the environment and in particular urbanisation around many stations have gravely biased the climate records. Arguing with scientific studies such as “Unresolved issues with the assessment of multidecadal global land surface temperature trends” (Pielke Sr. *et al* 2007), these sceptics speak of an artificial warming trend, or artificial global warming caused by urban heat islands (UHIs).<sup>25</sup> And because most of the stations in the US Historical Climatology Network (USHCN) are ranked poorly according to WMO standards (Fall *et al* 2011), they eagerly await the re-analysis of instrumental records by a team of highly credentialed physicists from California’s Berkeley University.

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<sup>25</sup> <http://www.surfacestations.org> (Last accessed on 17 March 2014)

#### **4.5 Discussion**

In the previous chapter I argued with Harry Collins' observation of the experimenter's regress that the successful replication of scientific experiments inevitably rests on however small a degree of convention, requiring taste and judgement. I set up this chapter as a test of the principle of independent replication, which is also a key element in the philosophy of 'sound science'. To this end I first explained in some detail the science of climate reconstruction, including the many heterogeneous materials as 'agents' in scientific practice (cf. Latour, 1999). If the scientists put together the materials according to robust and well tested methods, their findings will approximate the truth about past climate change. In the scientific literature, the Mann team presented their assemblage as a fairly straightforward representation of an unprecedented temperature rise since 1,000 years, and in Congress Mann likened the scientific process to the building of a house, suggesting that any scientist-architect could with the same tools replicate his construction.

Upon close examination of the team's science, Steve McIntyre found Mann's celebrated graph to be the result of little robust and badly described validation statistics applied to questionable raw data. And because the scientists could not agree on a single best method of analysis, each of them having trade-offs, he attended to the heterogeneous materials of which there were hundreds and as many opportunities to challenge the Mann team. Mann's secretive behaviour and unprofessional data management only spurred the climate auditor. For advocates of 'sound science' to accept the hockey stick as empirical test of the theory of AGW, the selection of each and every of the 415 raw proxy series should be based on clear a priori criteria. And because McIntyre would not agree with some in principle analytic a posteriori statements, he had to give proof of the team's unsound science. He began with the replication of Graybill's chronology so as to show that bristlecone pines were other than what the Mann team assumed – they would be a proxy measure for rain, not for temperature.

Several factors hampered McIntyre's replication of seemingly straightforward observations. First, the two camps are bound to different theoretical commitments. While the Mann team likes to think of important proxies as thermometers, McIntyre likes to view them as rain gauges, i.e., neither observation is an analytic a priori, and if we follow William Quine (1951), there is much latitude of choice as to what statements

to re-evaluate in the light of any single contrary experience. Second, the unstable conditions of the field site together with an insufficient description of the original experiment complicated its replication – with weathered materials, poor maps and even poorer communication between them, no architects can ever replicate the constructions of their predecessors: The two camps were generally unwilling to collaborate and share their tacitly held knowledge and assumptions. It is thus little surprising that Mann's close colleagues claim to have successfully replicated the hockey stick graph, noted Edward Wegman (HCEC, 2006: 733):

[T]hese supposed independent replications of the original Mann work are done by Rutherford et al., which includes the top seven people in the social network that we identified [...] Every one of them is in there, and they are frequent co-authors with Dr. Mann.

Although these findings generally support Collins' observation, the experimenters' regress is not inevitable, but its possibility is exploited in scientific controversies with much capital invested by both sides, i.e., because of their overarching political disagreement, in the hockey-stick controversy either side decided to exploit that possibility.

#### ***4.6 Conclusion***

In rehearsing this oft-told story (cf. Montford, 2010; Pearce, 2010; Mosher & Fuller, 2010; Mann 2012a) I attempted not to take a side in the scientific controversy; my interactional expertise does not allow me to contribute in discussions on the most appropriate validation metrics. Rather, my aim was to show empirically how the principles 'sound science' come into conflict with science as practice.

By invoking those principles climate sceptics can always question the practices of climate science. Thus, if applied symmetrically, the principles of 'sound science' would pull the rug from under their own feet: However credible McIntyre's experiments are, they too can be challenged with the ideas of logical empiricism; McIntyre too has to take certain facts on trust. In a collaborative, multidisciplinary science, the practitioners rely heavily on others to have done their job properly, and it would be highly impracticable to check all those raw data and account for all those potential problems which have been re-opened in the controversy on climate

reconstruction.

But while open scientific questions are typically expressed as uncertainties, in the popularisation of the hockey stick graph they were ignored. For the purpose of legitimating climate policy, the politicians wanted certainty where there was arguably not enough. They used to hockey stick to issue warnings of a climate catastrophe and prescribed a remedy in form of governmental regulation. Thus, in response to what they perceived as the much-loathed elite's smug and pseudo-scientific claims, the sceptics demanded definite proof, which the replication exercise did not bring: "With the replication of the hockey stick in tatters, reasonable people might have expected some sort of pause in the political momentum," hoped Andrew Montford (2008).

**If scientific closure on climate reconstruction is necessary for the political process to continue, and yet cannot be reached because it will always be challenged with the principles of 'sound science', decision-makers must look for other than the putative foundational authority of facts.** Politicians and all kinds of observers of the hockey stick debate ask *who* can speak authoritatively on the history of climate and the likely causes of temperature change. To find out who is qualified to do so and whose expertise is relevant, and whose it not, sociologists of science have described (and prescribed) mechanisms by which science is (and should be) demarcated from non-science. The next chapter describes how in the hockey stick controversy the irresolvable question over the scientific method has been turned into that hopefully resolvable one of expertise.

## *Chapter Five*

### *Scientific norms and expert authority*

I think a problem like climate change is where our kind of analysis of expertise plays its part. If there's a consensus among experts, and you think you can trust these people, and they're working with integrity and trying to argue that opposition are wrong using the normal ways of arguing in science – rather than political suppression – then you should base policies on the consensus even if you can't be sure that consensus is the truth.

Harry Collins (2011)

#### **5.1 Introduction**

A key argument for the reduction of GHG emissions has been the expert consensus on AGW based on scientific evidence. But, as we saw in the previous chapter, even the proclaimed consensus on climate reconstruction is prone to criticism. Climate sceptics deny any such claims with a nod to the scientific method ascribed to 'sound science'. "‘Consensus’ is a political notion, not a scientific notion," avered John Christy in his most recent testimony to Republican-sponsored hearings into climate science (HSCCT, 2011: 173). Thus, for the purpose of science informing political decisions, the insistence on 'sound science' is problematic; the principles of logical empiricism can be used to deny any consensus, making it difficult for facts to speak truth to or against power.

Although scientific consensus is a difficult and in principle political notion, Western societies generally believe in its possibility and accept scientists as epistemic authorities (cf. Castell *et al* 2014). But with the ascendance of the knowledge society (cf. Frank & Meyer, 2007), science having become an 'ordinary' profession (cf. Shapin, 2008), it is less clear who the authorities that is the true experts are. Through the lens of Congressional hearings on climate science, this chapter examines how the various parties in the hockey stick controversy recognise climate experts, certify climate research as scientific, and thereby demarcate expertise from lay knowledge.

Epitomised in Congressional hearings is the modern order in which science is separated from politics, and disinterested experts from sly politicians so as to inform the latter of the most rationale course of action. Over the last two decades Democrats and Republicans have convened dozens of Congressional hearings to which they have

invited a range of climate experts as epistemic authorities (see Keller, 2009). And because politicians want to use expert knowledge to legitimate policy action, which meets its greatest opposition in Congress, the supposedly legitimating science is meticulously scrutinised. Congressional hearings are sometimes hailed as the most effective way to keep expertise pure and political decisions rational.

Before we delve into hearings on climate reconstruction, in the next section (5.2) I review the SSK literature on the problem of demarcation. Background information on US Congressional hearings in section (5.3) sets up the empirical part in which the first section (5.4) describes my protagonists' principles of demarcation, and the second (5.5) describes how they discuss and dispute their practical implementation. In the discussion section (5.6) the findings of the empirical part are put into conversation with the literature presented in the review.

## ***5.2 Demarcating science from non-science, and the theory of expertise***

### *Science in 'Wave One' SSK: A self-regulating institution*

'Wave One' sociologists of science typically view science as a self-regulating institution populated by scientists as a class of formally qualified people (Mannheim, 1954; Merton, 1968). Scientists are generally believed to speak the truth by virtue of their formal, meritocratic qualifications such as university degrees, which are obtained after long training under the auspices of experienced scientists. "Truth claims, whatever their source, are to be subjected to preestablished impersonal criteria," believes Robert K. Merton (1968: 598). These impersonal markers of expertise have also become a convenient shorthand for policy-makers to recognise epistemic authority. For example, expert witnesses in US Congress typically but not necessarily hold PhDs (see table 2.1 on page 41). And for political scientists who study the role of science in decision making impersonal criteria too serve as convenient markers of expertise. Ann-Campell Keller (2009, 18) so identifies a scientist as an individual who has a PhD in the natural sciences and is employed as a PhD scientist by either an academic institution, a corporation, or by a (non-)governmental organisation:

Though there are other routes to having a professional role in the sciences, the empirical evidence [...] shows that having obtained an advanced degree in the natural sciences and having gained professional employment on the basis of that degree characterizes the majority of actors who appear in policy setting under the label of "scientist."



Peer review is the self-regulating mechanism by which the fraternity of scientists distinguishes among fellow experts. In peer review the scientists lay open their research to the scrutiny of their colleagues and competitors. These peers anonymously assess each other's scientific propositions and either reject or recommend them for publication in scientific journals. Based on the reviewers' judgement, the editors of these journals, themselves established scientists, then decide whether the research deserves publication. The denied scientist typically revisits their research, re-submits, or approaches a different scientific journal. The higher the journal is ranked the more interest is triggered from its respective publications; by convention peer-reviewed journals are indexed and ranked by the Institute for Scientific Information (ISI). Any proposition that survives this process is deemed scientific, until it too is rejected by subsequent scrutiny via reiterative rounds of review. Eventually only true knowledge survives this process, which has been likened to the free market's invisible hand.<sup>26</sup> And although this process is not fool-proof, it is generally accepted as the gold standard by which the institution of science proves its independence.

Because the peer-reviewed literature has become too vast for politicians to keep track with the latest findings, they typically entrust scientific societies such as the NAS, "where the nation turns for independent, expert advice," to review the scientific literature on an identified issue of public interest.<sup>27</sup> For example, in 2006 the US Congressman Sherwood Boehlert (R-NY) asked the NAS' National Research Council (NRC) to assess the scientific literature describing the climate of the past 2000 years (NRC, 2006). The NRC (2007) defines assessment as "a process by which independent experts review and synthesize available scientific and technical knowledge [...] that is needed by policy-makers to help make decisions."

According to 'Wave One' science scholar Robert Merton (1968: 605) the scientists' qualifications, credentials, publications and awards signal professionalism and moral integrity which "are in varying degrees internalized by the scientist" as scientific ethos, which is an "emotionally toned complex of values and norms which

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26 In *The Republic of Science* Michel Polanyi (1962: 56) makes use of the idea of the free market: "In the case of science, adjustment takes place by taking note of the published results of other scientists; while in the case of the market, mutual adjustment is mediated by a system of prices broadcasting current exchange relations, which make supply meet demand. But the system of prices ruling the market not only transmits information in the light of which economic agents can mutually adjust their actions; it also provides them with an incentive to exercise economy in terms of money."

27 <http://www.nas.edu> (Accessed on 14 March 2013). In the UK, the Royal Society performs a similar function. These societies are staffed by highly credentialed experts who have for years and decades produced outstanding and often Nobel Prize winning research in their respective disciplines.

held to be binding on the man [sic.] of science.” Merton identified four norms: Communalism, Universalism, Disinterestedness, and Organised Scepticism (CUDOS). The norm of communalism holds that knowledge is equally and freely accessible to all members of the scientific community and so becomes “part of the public domain, shared by all and owned by none” (1968: 588). Universalism means that every scientist regardless of race, religion, sex, but also social place or political ideology, can contribute to science, i.e., the laws of science are the same everywhere and are independent of the scientists involved. To this end, a scientist has to remain disinterested in their experiments’ results. “The translation of the norm of disinterestedness into practice,” writes Merton (p. 613), “is effectively supported by the ultimate accountability of scientists to their compeers.” Related to these three norms is organised scepticism, which can be understood as the scrutiny of peer review internalised by the scientist as their moral obligation.

*Science in ‘Wave Two’ SSK: Human, all too human*

Though formal credentials and scientific norms are understood as a typical pathway to establishing credibility and epistemic authority, in practice this is not always the case. Exceptions to the rule suggest that scientific expertise is not restricted to a formal class of scientists. For instance, there is no rule that a witness has to produce a PhD in order to testify as an expert in Congress. And many people do possess expert knowledge but have no formal qualification (cf. Wynne, 1989, 1996; Epstein, 1996) – called ‘lay experts’ their institutional status is debated (Collins & Evans, 2002; Wynne, 2003).

In that vein the new sociologists of science have argued that institutional credentials and scientific norms do not sufficiently explain the perpetual maintenance of a boundary between scientists and non-scientists (cf. Barnes & Dolby, 1970). As part of a wider social reality scientists have always had to work hard through repeated demonstrations of integrity, rhetoric and exposure to be judged as independent, reliable and trusted public witnesses (cf. Shapin & Schaffer, 1985; Shapin, 1994, 2010). It has never been enough to show one’s credentials, to talk about the scientific method, experiments, observations and theories – for a scientist to convince a critical audience of a statement’s truth it is necessary to draw on ‘extra-scientific’ factors.

In demarcating science from politics, scientists play with public sentiments by

contrasting empirical or pure science favourably to theoretical or applied science, and in Chapter 3 we saw how ‘sound science’ qua empirical science was demarcated from computer experiments qua theoretical science. Adversaries are then portrayed as religious, political, populist and subjective. And to be immune from blame scientists construct a boundary between the production of science on one side and its consumption by business, politicians, the military etc. on the other side (cf. Gieryn, 1983; 1999). The ‘linear model’ of science and society describes the knowledge transfer from production to consumption.

According to the new sociologists Mertonian norms, too, have served more as an ideal than an accurate description of the reality of science today. Schmaus (1983) argues that there have never been special norms operating in science; scientists would simply follow general and ever-changing moral codes. Ben-David (1991) shows that norms are suspended in controversies, and Metlay (2006) argues that the norm of communalism has been replaced by the ‘norm’ of IPRs. There is also empirical evidence of a violation of organised scepticism: Some reviewers were found to be less critical of ideas they generally agreed with; they scrutinised research of well-established scientists from prestigious institutions less thoroughly than that of comparably unknown scientists from less prestigious institutions (Jasanoff, 1990: 68-76). And in principle any group of scientists, such as expert panels that review the state of knowledge, violate the ideal of scepticism. Depending on the size and practicality, different groups of scientists approach the idea of consensus quite differently, and to a reasonable degree they suspend with scepticism. That ‘micro-politics of consenting’ reveals that a strict distinction between science and politics is problematic and prone to criticism (cf. Fuller, 1988: chapter 8).

The friction between science and politics manifests itself in an observable tension between the idealised, objective expert and the political citizen, who live in one and the same body. As citizens, scientists are also granted a political opinion and therefore allowed to break the ideal of disinterestedness. But because they possess highly valued knowledge, they must strike this balance responsibly, argues political scientist Roger Pielke, Jr. (2007): Those who make policy statements and clothe them with the authority of science are branded as “stealth issue advocates.” Those who refrain from stating their preference are celebrated as “honest brokers of policy options,” who help clarify existing and identify new policy options. Because it again

serves as a normative yardstick, for Pielke, Jr., (2007: 115) honest brokering “more likely will be the result of institutional commitments to expanding or clarifying the scope of choice available to decision makers.”

*Science after ‘Wave Two’ SSK: The problem of extension*

“If it is no longer clear that scientists and technologists have special access to the truth, why should their advice be specially valued?’ This, we think, is the pressing intellectual problem of the age,” argue Collins and Evans in an essay on the so-called problem of extension (2002: 236). If neither a reliance on the method nor on institutional markers of expertise and the associated scientific ethos suffice to demarcate those who know from those who don’t, decision-makers who rely on scientific authority will struggle in the choice of the right experts, believe the two science students. Not least the political controversies over GMOs, the use of pesticides in agriculture, and global warming would prove that the question of how one should draw the line between expertise and non-expertise is as pertinent as ever. As a solution Collins and Evans (2002: 238) propose to “draw a boundary around the body of ‘technically-qualified-by-experience’ contributors to technical decision-making” so as to exclude those who cannot contribute to debates about, for example, the Kyoto Protocol.<sup>28</sup> “We resurrect the old distinction between the political sphere and the sphere of expertise, but in our model the boundary is found in a new place,” they posit (2002: 270).

For Collins and Evans that boundary is no longer found between the class of professional accredited experts and the rest: in part because discrimination between experts and non-experts based on formal meritocratic qualifications has sometimes led to bad decisions (e.g., Wynne, 1996), one should “take the advice of scientists and technologists in virtue of the things they do *as* scientists and technologists, rather than as individuals or as members of certain institutions” (Collins & Evans, 2002: 236, emphasis in original). Only those members of the public who can produce “experienced-based expertise” should be counted into Collins’ core-set of experts. For

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28 Collins and Evans (2002: 236) cite the Kyoto Protocol as an example of technical decision-making: “By ‘technical decision-making’ we mean decision-making at those points where science and technology intersect with the political domain because the issues are of visible relevance to the public: should you eat British beef, prefer nuclear power to coal-fired power stations, want a quarry in your village, accept the safety of anti-misting kerosene as an airplane fuel, vote for politicians who believe in human cloning, support the Kyoto agreement, and so forth.”

the sociologists the core-set is made up of those “deeply involved in experimentation or theorization which is directly relevant to a scientific controversy or debate” (2002: 242). The core-scientists’ special position would “[arise] from their long experience and integration into the specialist social group of which such expertise is the collective property” (p. 260).

Despite this more restricted principle for identifying whose expertise should count, in any given case, there is still the question of demarcating what and who should count as the core group. Demarcation, the scholars suggest, becomes a task for sociologists of science to contribute to. The science student should closely observe scientific debates and use the acquired interactional expertise to help in the erection of new boundaries. For example, depending on the context and public interest, Collins and Evans’ boundary requires a sociological definition of types of science. Other than in the “public domain sciences and technologies,” in the “esoteric sciences” (2002: 242) only the core-set can legitimately contribute to the formation of the consensus. And yet what counts as esoteric science awaits a positive answer; at times previously esoteric sciences have been propelled into the public limelight rather unexpectedly (e.g., Wakefield *et al* 1998).

Finally, as regards the public acceptance of controversial scientific knowledge claims, which ‘Wave Two’ scholars have taken particular interest in, Collins and Evans presume that the ‘chattering classes’ will somehow come to know who to believe in. Following a strong universalising rationale, they assert that people would understand whether experts’ claims are trustworthy and compatible with the “forms-of-life of Western science.” The public would make their decision by asking: “Does the author of a claim seem to have integrity? Is the author of a claim known to have made unreliable claims in the past?” (Collins & Evans, 2002: 258).

With these concepts, describing a theory of expertise, a better and more authoritative consensus on climate change could be found, believes Collins (2011).

### *The contingency of expertise*

In controversies over technical decision-making the central question is typically what is going to count as relevant knowledge, rather than who possesses the right sorts of knowledge, or how it is certified as scientific, argues Sheila Jasanoff (2003). For

Jasanoff, Collins and Evans' theory, including its prescription of core-sets and types of sciences and expertise, is desirable only to those who believe that decisions can be rendered entirely rational. She views expertise as the outcome of negotiations and controversies, and so directs our attention away from the production of science and expertise to the processes of public warranting by which the public can hold experts to account. Through what can also be called public-review we formally decide as a society who to believe on important 'matters of concern' (Latour, 2004a).

Public warranting succeeds scientific peer review and already includes audits and site visits, regulatory peer review, judicial review and legislative oversight such as Congressional hearings in which politicians evaluate contesting knowledge claims by publicly (cross)-examining scientists. The public warranting mechanisms Jasanoff has directed our attention to are neither universal nor objective – they differ within and between nation-states, and their various mechanisms reflect specific expectations that are grounded in historically shaped practices about how to arrive at truth (Jasanoff, 2005). Other than those sociologists of science who have argued for the universal importance of trust in the making of truth (e.g., Shapin, 1994), Jasanoff shows that in the US more than in any comparable Western liberal democracy public accountability of expertise is based on the assumption of distrust. This assumption shapes what comes to count as true public knowledge.

While Jasanoff has focused, in particular, on the role of the courts in US America's civic epistemology, my focus here is on Congressional hearings as a forum for public warranting that plays a crucial role in testing the constitution of relevant expertise. The next section introduces Congressional hearings as a venue on which climate expertise is tested and adjudicated.

### ***5.3 Background and setting***

#### *The hearings*

When Steven McIntyre took interest in the graph in 2003, the IPCC (2001b) along with the US EPA (2003) had already declared the hockey stick a policy-relevant fact. Their reports suggested that climate change was a global issue with potentially dire consequences. In response Senators John McCain (R-AZ) and Joe Liebermann

(ID-CT) introduced the Climate Stewardship Act in the Senate. Representatives Wayne Gilchrest (D-MD) and John Olver (D-MA) then introduced the Act to the House in 2004. The formal policy process was finally under way, and in order to set a binding target for the reduction of GHG emissions Senate and Congress only had to pass legislation. But both chambers of the 107th (2001-2003) and 108th Congress (2003-2005) had a Republican majority, which made the passage of legislation to regulate GHGs an uphill struggle.

Because climate-concerned politicians endorsed the scientific consensus as presented by the IPCC and EPA, some climate sceptics in both chambers of the 107th and 108th Congress called for Congressional hearings on climate science. Using the power of the majority, Oklahoma Senator James Inhofe (R-OK) convened the first of several hearings on climate reconstruction held by the Committee on Environment and Public Works. Hearings on July 29, 2003, entitled “Climate History and the Science Underlying Fate, Transport, and Health Effects of Mercury Emissions,” featured Michael Mann and the astrophysicist Willie Soon, representing either side of the scientific debate (SCEPW, 2003).

In October 2003 the first Climate Stewardship Act failed in the Senate but was re-introduced to both houses in February 2005 when it failed once more. Half a year later on September 28, 2005, Inhofe convened a second hearing entitled “The Role of Science in Environmental Policy Making” before the same Committee (SCEPW, 2005). The hearing addressed climate reconstructions as well as the impact of climate change on hurricane frequency, since only one month earlier Hurricane Katrina had devastated New Orleans, killing at least 1,833 people. Notable expert witnesses to the hearing included science fiction writer Michael Crichton and the hurricane scientist William Gray, who spoke on the debated impact of climate change on hurricane frequency and intensity.

In July 2006 fellow House Republicans Joe Barton (R-CA) and Ed Whitfield (R-KY) followed Inhofe’s example and called for a hearing dedicated solely to the hockey stick (HCEC, 2006). “Questions Surrounding the ‘Hockey Stick’ temperature Studies: Implications for Climate Change Assessment” was held before the Subcommittee of Oversight and Investigation of the Committee of Energy and Commerce on July 19 and 27, 2006.

Joe Barton explained the rationale for Republican-sponsored hearings on

climate science (HECE, 2006: 111):

Well, let me [...] say why we are doing this hearing, [...] I don't disagree fundamentally with some of what my friends on the minority side have stated. There is no question that the temperature is warmer today than it was in 1850. I think there still is a question about the cause of that [...] Where I disagree with some of my friends on the minority side is that before we make massive public policy changes that affect every American citizen in this country, we need to have with the highest degree of certainty that the facts really are the facts [...] If the United States has ratified Kyoto and if the United States Congress working with the Administration had begun to implement Kyoto, it requires a reduction in CO<sub>2</sub>, I believe about 30 to 40 percent, and that means you are not going to have coal-fired power plant combustion in many parts of this country. It means that you are going to have to reduce the automobile emissions of the vehicles that are made in Michigan. And before we go down that trail, I think it is imperative that we do the oversight and do the science --I am not opposed to talking to the climatologists but I agree [...] that we really get everything on the table. If that shows that the human correlation is beyond dispute, then I believe we do have an obligation to take what steps we can to remedy that but I don't believe that science yet shows that.

In the run-up to that hearing Barton and Whitfield ordered the statistician and fellow sceptic Edward Wegman to review McIntyre's critique on the hockey stick. They then invited both Wegman and McIntyre to counter the testimony subpoenaed from Michael Mann. Irritated by what he considered a misguided and illegitimate investigation, the Chair of the House Science Committee Sherwood Boehlert (R-NY) called upon the NAS to authoritatively assess the hockey stick controversy (NRC, 2006). The report's chair, Gerald North, the NAS president Ralph Cicerone as well as German climate scientist Hans von Storch were among the expert witnesses in July 2006. They were joined by climate scientists Tom Karl, who appeared for the absent Michael Mann during the first session, Thomas Crowley, and Jay Gulledge as the Democrats' witnesses, and John Christy as expert for the Republican side.

After 'Climategate' and years of Republican majority, in November 2010, the Committee on Science and Technology convened, under Democrat rule this time, the hearing grandly entitled "A Rational Discussion of Climate Change: The Science, the Evidence, the Response" before the Subcommittee on Energy and Environment (HCSS, 2010). This and a second hearing entitled "Undeniable Data: The Latest Research on Global Temperature and Climate Science" (SCEPW, 2011) convened by the Democrats Edward Markey (MA) and Henry Waxman (CA) should set the scientific record straight and reinforce the difficult legislative process.



### *The procedure*

A hearing starts with a formal procedure in which the politicians explain its rationale – scientific expertise is sought to help inform policy-makers – and the reason for their choice of expert witnesses. “Very often our subjects come from areas in which we have very little personal involvement or expertise, so we have to depend on expert witnesses,” explained Senator Lisa Murkowski (R-AK) (SCEPW, 2005: 13). Yet because they are selected independently by Republicans and Democrats, there is an incentive for either party to choose witnesses whose expertise is thought to align with their respective policy positions. Scientists whose knowledge claims do not align with the respective position are usually cross-examined by members of Congress as hostile witnesses. And though officially denied, it is fairly easy to tell who sympathises with whom, and politicians of both sides badly disguise their hostility towards particular witnesses as a dialogue between Senator Inhofe and Michael Mann illustrates (SCEPW, 2003: 16):

INHOFE: All right. Let’s see. Dr. Mann, since you have characterized your colleagues there in several different ways as nonsense, illegitimate, and inexperienced, let me ask you if you would use the same characterization of another person that I quoted on the floor yesterday [...].

MANN: I do not think I have questioned scientific integrity. I have questioned scientific expertise in the case of Drs. Willie Soon and David Legates with regard to issues of paleoclimate [...] If I could clarify one—— .

INHOFE: OK. Well, you can’t because there isn’t time. I am going to stay within my timeframe and I want to get to questions so others will have plenty of opportunity to respond to questions I am sure.

In order to forestall mutual accusations of biased selection the sworn-in witnesses then introduce themselves with a written statement stressing their impersonal credentials. After having sworn an oath, they give a sober account typically stating their academic rank (e.g., their title in their respective discipline), position, institutional affiliation (e.g., Professor of Earth System Science, Director of the Climate Research Centre, Member of the Academy of Science, etc.), and past and current research interests. Often expert witnesses cite the number of peer-reviewed publications to mark their scientific experience. Pledges of independence and disinterestedness follow once the debate has kicked off and politicians examine each other’s experts. Often the ‘hostile’ experts are first explained the rationale for their invitation. Congressman Jay Inslee (D-WA) lectured Ed Wegman (HCEC, 2006: 100):

I want you to make sure you understand the reality of the situation. I am giving you all the

sincerity that I can give to you. But the reason you are here is not why you think you are here, okay. The reason you are here is to try to win a debate with some industries in this country who are afraid to look forward to a new energy future for this Nation, and the reason you are here is to try to create doubt about whether this country should move forward with a new technological clean energy future or whether we should remain addicted to fossil fuels. That is the reason you are here. Now, that is not the reason individually why you came but that is the reason you are here. Thank you very much.

The hostile climate of Congressional hearings puts these scientists in an uncomfortable position. Whilst they make sure to appear as objective as possible, policy-makers try to elicit subjective statements by asking policy-relevant questions. They want more than just scientific advice; they demand of scientists to offer their impersonal authority as a resource in the political battle. But expert witnesses are particularly careful not to ‘talk politics’. They do not want to give the impression of favouring a policy position as this would undermine the rationale for their invitation and threaten the image of disinterestedness and neutrality they are supposed to uphold. Thus, observes Keller (2009: 176),

[i]n many scientists who participate in legislative decision making, this tension produces a visible effort to negotiate the boundary between science and policy. Through such negotiations, scientists attempt to retain their status as neutral experts and to provide relevant information to policy makers. But the ability to fulfil one of these roles almost precludes the ability to satisfy the other. In spite of this, scientists are repeatedly called upon to play both roles when they participate in Congressional hearings.

And in spite of this tension, scientists too have an incentive to attend these hearings: Whilst an invitation to the most authoritative political venue in the US is certainly flattering, some may also see it as a chance to actively shape political decisions. Scientists who enter the policy making process “are purposive and will adapt their behaviour to the setting in order to maximize their goals,” argues Keller (2009: 14).

By discussing the political implications of their research, and by demarcating themselves from each other, the parties’ respective expert witnesses actively engage in boundary work. In the next section I let the scientists and their political protégées describe the principles of demarcation to clarify whether and how different sides actually disagree about them (cf. Oreskes & Conway, 2010).

## ***5.4 Principles of demarcation***

### *Scientific credentials*

The authority of scientific credentials is repeatedly invoked by PhD scientists, Nobel laureates and academic societies signing petitions for and against action to curb the risks of climate change. In the 2006 hearings Congressman Henry Waxman (D-CA) presented a joint statement issued by 11 national science academies calling on world leaders to acknowledge that the threat of climate change is clear and increasing (see also UCS, 1997). On the other side of the climate divide, at a presentation to climate sceptics at the Heartland Institute, atmospheric physicist Richard Lindzen (2010) played down a letter signed by 250 scientists to the journal *Science* arguing “...most signers had no background whatever in climate sciences.” For Michael Mann a climate expert is “an individual with specific training in oceanographic, atmospheric, and coupled ocean-atmosphere processes relevant to understanding climate variability and the behavior of the climate system” (SCEPW, 2003: 178). They have obtained this training through “either an advanced degree in those areas of study, or through years of research in those areas associated with numerous publications in the peer-reviewed climate literature,” Mann added.

Both sides support meritocracy in science. Thus, Republican Senator James Inhofe deemed it necessary to defend the invitation of science fiction author Michael Crichton as an expert witness (SCEPW 2005, 32-33):

In a way, I kind of regret that Michael Crichton was an author. Because if he had not been an author, he would still be here today because of his scientific credentials, having degrees from Harvard College and Harvard Medical School, visiting lecturer of Physical Anthropology at Cambridge University, post-doctoral fellow at the Salk Institute for Biological Studies. We would have him here anyway.

Inhofe also emphasised institutional affiliation in his endorsement of a study by credentialed climate sceptics Sallie Baliunas and Willie Soon (2003, otherwise referred to as BS03) (SCEPW 2003: 2):

The 1000-year climate study that the Harvard-Smithsonian Center for Astrophysics has compiled is a powerful new work of science. It has received much attention, and rightfully so [...] In many important ways, the Harvard-Smithsonian Center’s work shifts the paradigm away from the previous hockey stick study. The powerful new findings of this most comprehensive study shiver [the timbers of the] adrift Chicken Little crowd.

The brand name and associated public authority of Harvard as a scientific institution was also recognized by climate scientists, who worried about its potential to lend credence to the sceptics. In an email to his colleagues, climate scientist Tom Wigley from the National Center of Atmospheric Research (NCAR) wrote (EAE, 2009: 23 July 2003):

What is worrying is the way this [BS03] paper has been hyped by various groups. The publicity has meant that the work has entered the consciousness of people in Congress, and is given prominence in some publications emanating from that sector. The work appears to have the imprimatur of Harvard, which gives it added credibility [...] It would (also) be useful to have Harvard disassociate themselves from the work [...] As scientists [...] we need to concentrate on exposing the scientific flaws. We also need to do this in as authoritative a way as possible. I do not think it is enough to speak as individuals or even as a group of recognized experts. Even as a group, we will not be seen as having the 'power' of the Harvard stamp of approval.

In trying to organise fellow scientists to get Harvard University to denounce the study, Wigley was taking the organised scepticism of peer review to new levels. Such behaviour would eventually support the sceptics' claim of a politicisation of climate science.

Because expert witness Steven McIntyre is a retired mining consultant without formal qualifications such as a PhD, Ed Whitfield (R-KY) deemed it important to highlight his visit at Oxford University dating back several decades, so as to bask in the reflected glory of that ancient university as a centre of scientific learning. And yet, to hedge their bets, Whitfield and Barton also asked the professional statistician Ed Wegman to review McIntyre's 'lay expertise' and offer the stamp of approval from someone with more conventionally recognised scientific credentials. Clifford Stearns (R-FL) explained Wegman's appointment (HCEC, 2006: 736):

Dr. Wegman is an appointed member of the National Academy of Science Board of Mathematical Statistics and Their Application. He is chair of the NAS Committee on Applied and Theoretical Statistics, highly credentialed in math and statistics, wouldn't you say? Shouldn't we take his judgement on statistical matters seriously, and don't they carry significant weight? Wouldn't you say his judgement about statistical matters is important and that he has credibility upon those credentials?

The rationale for having a professional and highly credentialed statistician assess and approve of McIntyre's critique was to authorise his 'lay' claims in front of a disbelieving audience of Democrat representatives.

To sum up, these examples of boundary work confirm that protagonists across the divide appreciate and agree on scientific credentials as in formal qualifications and

institutional affiliations as meritocratic, non-personal markers of expertise.

### *Scientific norms*

Scientists do not want to give the impression of having a stake in the outcome of their research: “[I]t would be problematic if our views on policy somehow influenced the way we went about doing our science,” Mann (2014) forestalled in the *NYT*. The norm of disinterestedness, which Mann alludes to, is particularly valued among experts operating at the legislative and implementation stages of decision-making. Yet because each side in Congress chooses witnesses who are likely to be sympathetic to respective policy positions, the politicians will try to push for statements they want to hear. Faced with the risk of losing credibility, the witnesses usually deny their interrogator a satisfactory answer. Asked by Henry Waxman (D-CA) whether he deemed it important to take cost-effective steps to reduce emissions, the NRC chair and atmospheric scientist Gerald North evaded: “Well, now you are stepping a little bit beyond my role here. I will talk about the science but what we ought to do is somebody else’s business” (HCEC, 2006: 65).

Those who express their opinions quickly find themselves at the centre of attention. In 1988, climate scientist James Hansen’s emphatic Congressional testimony that “[i]t’s time to stop waffling so much and say that the evidence is pretty strong that the greenhouse effect is here” (*New York Times*, 1988) became famous precisely because he overstepped the conventional boundary between expertise and politics. His candour gave him the contentious reputation of (stealth?) issue advocacy: “Dr. Hansen has pushed far beyond the boundaries of the conventional role of scientists, particularly government scientists, in the environmental policy debate,” summarised the *NYT* journalist and close observer of the climate controversy, Andy Revkin (2009). Given the Democrats’ reliance on climate expert judgement, and the Republican sceptics’ attempts to deconstruct any proclaimed consensus, it is easy to see why some climate scientists are tempted to jeopardise their status as epistemic authorities.

In view of the multi-million dollar campaign to cast doubt over climate change, contrarians too are advised to deny any profit or interest. Anticipating a loss of credibility, John Christy pre-empted any accusations: “No one is holding a gun to my head and no one is paying me money either above or under the table to arrive at the conclusions I (and others) have come to” (HCSST, 2011: 68). Neither did Edward

Wegman receive any compensation, “not even a taxi fare,” he insisted (HCEC, 2006: 36). Joe Barton vouched for the disinterestedness and independence of his chosen expert: “I would especially like to thank Dr. Edward Wegman who, on his own time and his own expense, assembled a pro bono committee of statisticians to provide us with independent and expert guidance concerning the hockey stick studies” (HCEC, 2006: 9). Wegman reaffirmed the point, telling Barton, “I didn’t even know what you looked like until--.” And when Wegman confirmed Barton’s rhetorical question to have voted for Al Gore in the 2000 presidential elections, he publicly dissociated himself from the Republican Congressman who ordered his expertise.<sup>29</sup>

To forestal the accusation of interest experts stress the motif of curiosity. Evading Janice Schakowsky’s (D-IL) rhetorical question about his lack of credentials, Steven McIntyre replied (HCEC, 2006: 739):

SCHAKOWSKY: Actually I wanted to ask you--I hope you don’t think this sounds rude but when I looked at the witness list I see, you know, everyone has got kind of a credential and then it just says your name, so I wanted to ask you about your credentials, Mr. McIntyre, [...] what I found was: ‘for the last 16 years I have been an officer and director of several small public mineral exploration companies, previous to that I worked for a large international mining company, and that mainly it is your experience in mineral exploration industry that you tout in your resume and your background’ [...].

MCINTYRE: Well, in this case this has nothing to do with any work that I have ever done. I just became interested in it as a citizen when I read the studies, and I thought that politicians were facing difficult policy decisions so I thought that it would be interesting to examine one particular paper which was being cited by the Canadian government. It wasn’t clear to me how people knew that 1998 was the warmest year in the millennium, and I was just interested in how—.

Here McIntyre appeals to scientific ideas of curiosity-driven research (cf. Daston & Park, 2001), seeking knowledge for its own sake without an eye to profit or other instrumental purpose. His noble virtues are hailed by the *WSJ*’s Antonio Regalado (2005): “After spending two years and about \$5,000 of his own money trying to double-check the influential graphic, Stephen McIntyre says he has found significant oversights and errors.”

The Congresswoman Schakowsky, however, was not satisfied and so she pressed further:

SCHAKOWSKY: So are you qualified to make a judgement on whether or not the Earth is warming at an unprecedented rate?

MCINTYRE: For the things that I have published on, my statistical and mathematical

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<sup>29</sup> Though at odds with Wegman’s choice, hurricane scientist and expert witness William Gray too discloses his preference: “I must say I have been a lifelong Democrat until Al Gore ran for president” (SCEPW, 2005: 24). It serves the same purpose.

skills are adequate for what I have published on. The findings that we have had about principal components have been—.

Now McIntyre adopts the role of a modest witness, who is qualified to speak on a limited range of technical topics – mathematics and statistics. His skills enable him to publish in those core areas, but in so doing he is merely an intermediary speaking on behalf of his data, his ‘findings’. The Congresswoman, however, turned to the wider conclusions that have been drawn as a result of those findings and queried whether McIntyre had the standing to draw them:

SCHAKOWSKY: But are you qualified to comment on whether or not the Earth is warming at an unprecedented rate?

MCINTYRE: Well, you asked whether the people knew or didn’t know. I am just saying I didn’t know.

It is typical for Congressional hearings to see expert witnesses evade wider, potentially policy-relevant conclusions of their research. For the sake of objectivity and in the spirit of ‘sound science’ politicians and expert witnesses generally deny scientists an opinion. There is politics on one side and science on the other. The latter is guided by the Mertonian ideals of universalism and disinterestedness, which all silently buy into. It is “[t]he scientific method’s impersonal rigor [that] has produced enormously powerful results for 400 years [...]. A truth in science is verifiable whether you are black or white, male or female, old or young,” intoned Michael Crichton during his Senate testimony (SCEPW, 2005: 52). Human fallibility is only reluctantly acknowledged: “Now, we are all human beings, we make a lot of mistakes. We are biased. We do this, we do that,” Ed Whitfield ‘confessed’ (HCEC, 2006: 256). Not least because of this inevitable human factor scientists rely on organised scepticism and the associated practice of peer review.

### *Peer review and expert assessments*

The scientific norm of organised scepticism guides the practice of peer review. As Michael Mann piously explained to Congress, “[s]cience progresses through an open, self-correcting process whereby scientists place their ideas in the marketplace, typically by publishing articles in peer review journals” (HCEC, 2006: 767). In the introduction to that 2006 hearing we learn that Mann has (co-)authored more than 70 peer-reviewed articles, more than 30 other peer-reviewed contributions and book

chapters on climatology and palaeoclimatology. Mann's colleagues too have substantial certified work experience in the field of palaeoclimatology and though they often co-author papers, the overriding imperative of organised scepticism in the marketplace of ideas limits cooperation and should ensure that truth emerges in the end. Mann explained: "My profession is highly competitive. We often disagree publicly. Scientists disagree publicly and in our articles, with each other on certain matters, and yet we can co-author on other areas where we agree [...]" (HCEC, 2006: 732). Gerald North made much the same point: "This is a pretty competitive business, and I will tell you, if somebody can find a way to knock down someone else's theory, that is their road to recognition and fame. We all do that. That is part of the game and we really enjoy that part of the game" (HCEC, 2006: 64).

The contrarians too subscribe to peer review. "Especially when massive amounts of public monies and human lives are at stake, academic work should have a more intense level of scrutiny and review," concluded Wegman (HCEC, 2006: 38-39). majority-side member Michael Burgess (R-TX) concurred (HCEC, 2006: 23):

As the U.S. Congress and even the international policymaking bodies look to the scientific community to provide information and analysis, it is especially important to make certain that the processes are in place to ensure that we are using sound and unbiased science that has undergone rigorous peer review process.

Because he cannot produce formal qualifications, Steven McIntyre more than anyone else relies on credentialed peers to certify his 'lay expertise'. Prior to the publications in the renowned *GRL* (McIntyre & McKittrick 2005ab), he presented his findings on his blog *climateaudit.org*. Because blogs are not subjected to formal review, they are "not an appropriate way to conduct science," concluded Wegman in agreement with the scientists (HCEC, 2006: 49). In an email Michael Mann reacted to an early critique of the hockey stick published on a sceptical blog (EAE, 2009: 3 August 1999):

This thing wouldn't have a chance at passing peer-review (at least, not on this planet), so he posts it on the web – the downside of absolute freedom of dissemination I suppose. The material in question is the scientific equivalent of trash, plain and simple [...] I think the best approach is to, as Jonathan Overpeck has so effectively been doing, try whenever possible to educate the lay public about the essential distinction between peer-reviewed science and un-peer-reviewed [...] well, whatever you want to call it.

In the multidisciplinary sciences it is particularly difficult to stay on top of the peer-reviewed debate, and decision-makers rely on groups of experts to review and assess state-of-the-art knowledge. At least two such expert panels, the IPCC and the



NAS' NRC, are regularly ordered to synthesise the peer-reviewed literature on climate science. While the IPCC wants to be recognized as the most authoritative intergovernmental scientific voice on climate change issues, the NAS has firmly established its reputation as the authority on scientific matters in the US. “[O]ftentimes when we have scientific disputes we [politicians] ask the National Academy of Sciences to review the matter”, explained Henry Waxman (D-CA) (HCEC, 2006: 20). Emailing privately to other climate scientists, Tom Wigley agreed “...[t]he Administration and Congress still seem to respect the NAS (even above IPCC) as a final authority” (EAE, 2009: 23 July 2003).

In the run-up to the 2006 Congressional hearing, Sherwood Boehlert asked a NRC committee on Surface Temperature Reconstructions (STR) to authoritatively review surface temperature reconstructions for the last 2,000 years (NRC, 2006). Having reviewed the existing literature, the 12 eminent scientists and statisticians, none of whom had collaborated with the Mann team, find it plausible that the Northern hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium (NRC, 2006: 115). This was endorsement of a sort, but much weaker than that given in the 2001 IPCC chapter, which repeated the Mann team’s conclusions one-to-one. First, the STR committee’s invocation of ‘plausibility’ here marks a shift from inductive to abductive logical reasoning, by which scientists infer from evidence the best explanation. Second, because the uncertainties inherent in temperature reconstructions for individual years and decades are larger than those for longer time periods, the STR placed less confidence in the claims made by MBH99 that the 1990s were likely the warmest decade, and 1998 the warmest year in a millennium.

The NRC report’s findings were accepted by scientists and expert witnesses across the divide, for reasons an NRC panellist explained:

There were enough conclusions that all sides were able to claim victory without being entirely dishonest. Many of the critics’ points were found to be cogent. The report did not vindicate the specific studies and approaches used by Mann and colleagues. But it did confirm that the planet warmed during the last century and that this warming and the prior cool period is consistent with all the other evidence about anthropogenic climate change. (anonymous, 2013).<sup>30</sup>

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30 The report is not endorsed by all scientists. Roger Pielke, Sr., writes on his blog (2006): “The Report is a disappointment in not adequately addressing the accuracy of the global surface temperature trend data. Since its accuracy is at the foundation of the entire Report, the absence of such an evaluation very substantially weakens the value of the Report in climate science”.

Politicians and think tanks too endorsed the NRC's findings, but they interpreted its conclusions in different ways that reflected their policy preferences. For Senator Inhofe: "[t]oday's NAS report reaffirms what I have been saying all along, that Mann's 'hockey-stick' is broken" (Inhofe, 2006). Likewise for the CEI (2006), "[h]aving 'high confidence the planet is warmest in 400 years' is a little like saying everyone who eats carrots eventually will die. We all know that. What would one expect after emerging from the little ice age?" The Heartland Institute (2006) titled its press release "Congressional Hearings break Hockey Stick." By contrast, in a statement, Sherwood Boehlert (R-NY) (2006) expressed satisfaction with the STR results, saying "...[t]here is nothing in this report that should raise any doubts about the broad scientific consensus on global climate change – which doesn't rest primarily on these temperature issues, in any event – or any doubts about whether any paper on the temperature records was legitimate scientific work."

In summary, the principals in the controversy and across the climate divide agree on scientific norms (CUDOS) which they hold binding upon the expert. In the next section I examine how they employ them against each other's arguments for epistemic authority.

### ***5.5 Demarcation in practice***

#### *The scientists' normative markers of expertise*

From the convinced point of view the motives behind climate scepticism are attributed to right-wing ideology and resistance to government regulations by the fossil fuel industry (cf. Hoffman, 2011). The sceptics' expert front group has a financial stake in denying climate change, argue analysts, scientists and politicians alike (cf. Gore, 2006; Hoggan & Littlemore, 2009; Oreskes & Conway, 2010; Brulle, 2013). "One Colorado electric cooperative has openly admitted that it has paid \$100,000 to a university academic who prides himself on being a global warming sceptic," noted Democrat Janice Schakowsky (D-IL) during the hockey stick hearing (HCEC, 2006: 629). Thus, to exclude their expert critics and notably Steven McIntyre, the 'hockey team' argued with the sceptics' courting of their 'kindred spirits'. Eager to illuminate the context in which McIntyre as well as BS03 gained popularity and support, mainstream climate scientists, sympathetic journalists (cf. *The Guardian*, 2011) and

bloggers (*desmogblog.com*) search the record for any potentially compromising links, whether financial or political.

And so do their opponents, who are equally suspicious of the motives of mainstream climate scientists (e.g., *climatedepot.com*). In his testimony, John Christy complained about former colleagues ‘having a dog in the fight’ over the hockey stick, thereby setting himself up as a neutral observer standing above the fray and thus able to speak impartially about the accuracy of the temperature record (HCEC, 2006: 664). And from a more avowedly political perspective Senator James Inhofe (R-OK) accused the ‘hockey team’ of clinging to disproven ideas of AGW as if it were a religion (SCEPW, 2005: 43):

I think in the case of global warming, it really has become a religion to a lot of people. A lot of people have so many years of their lives wrapped up in it that they don't want to all of a sudden realize that most of the science since 1999 has refuted it. How could I have been wrong; and did I waste 10 or 15, 20 years of my life? I kind of think this is some of the things that are going on.

The boundaries he is drawing here between faith and facts, religion and science are age-old and stretch back to the very dawn of the scientific revolution. And indeed, it is not altogether preposterous to advance such a suspicion. “As you know,” wrote Phil Jones in an email to John Christy, “I’m not political. If anything, I would like to see the climate change happen, so the science could be proved right, regardless of the consequences. This isn’t being political, it is being selfish” (EAE, 2009: 5 July 2005).

To maintain their epistemic authority when called to appear in Congress, scientists have to be careful to avoid making any such statements that might be seen to cross the conventional boundary between science and politics. Only occasionally have climate scientists in Congress been lured into some kind of advocacy. When asked by Clifford Stearns (R-FL) whether there is a scientific consensus that global warming is real and bad, Edward Wegman evaded: “I believe there is a consensus that global warming is real. My friends in Finland think it is a great thing” (HCEC, 2006: 97). Stearns had to press harder to get the answer he wanted. “It is probably less urgent than some would have it be,” Wegman finally conceded. Such a statement is enough for Stearns to understand Wegman’s policy view, and for Wegman to claim a neutral position. Of course, it is very difficult to derive any concrete advice, which is supposedly what the scientists are called in for, from such a statement.

Very few scientists in Congress willingly jeopardise their expert position. In

arguing against large scale GHG emission reductions, John Christy was careful first to dissociate his political opinion from his scientific position as remote sensing expert and Alabama state meteorologist. Instead he based his policy recommendations on his personal experience overseas as a missionary (HCEC, 2006: 187):

I believe my experience in Africa is important in this whole discussion of energy and climate. In the 1970s I taught science and math as a missionary teacher, and I saw the energy system there. The energy source was wood chopped from the forest. The energy transmission system was the backs of women and girls hauling wood an average of three miles each day. The energy use system was burning the wood in an open fire indoors for heat and light. The consequence of that energy system was deforestation and habitat loss while for people it was poor respiratory and eye health. The U.N. estimates 1.6 million women and children die each year from the effects of this indoor smoke. Energy demand will grow, as it should, to allow these people to experience the advances in health and quality of life that we enjoy. They are far more vulnerable to the impacts of poverty, water pollution, and political strife than whatever the climate does. I simply close with a plea, please remember the needs and aspirations of the poorest among us when energy policy is made. Thank you very much.

Although he dissociated himself from his climate expertise, his argument against energy policy whose overarching goal is the stabilisation of climate change derives its authority also because of his position as recognised climate expert. But because his argument has also been forwarded by the creationist climate deniers of the Interfaith Stewardship Alliance, Congressman Bart Stupak (D-MI) quickly dismissed Christy's emphatic humanitarian pledge instead of considering its political relevancy, i.e. it is interpreted according to party-political positions. In the hostile climate of Congressional hearings, Christy's attempt at honest brokering of policy options is not rewarded.

Explicitly political arguments such as Christy's are much more frequently found on the many informal venues offered by the news media and on the internet. Michael Mann's call to action, in which he blames climate sceptics in Congress for the political impasse, is printed in the *NYT* (2014):

In fact, there is broad agreement among climate scientists not only that climate change is real [...], but that we must respond to the dangers of a warming planet [...] This is where scientists come in. In my view, it is no longer acceptable for scientists to remain on the sidelines [...] there is nothing inappropriate at all about drawing on our scientific knowledge to speak out about the very real implications of our research [...] In fact, it would be an abrogation of our responsibility to society if we remained quiet in the face of such a grave threat [...] We scientists are citizens, too, and, in climate change, we see a clear and present danger.

Other than at the legislative stage of decision-making where his right to speak out honestly is curtailed by the expectations placed on experts, at the agenda-setting stage

on the internet and in newspapers (cf. Keller, 2009) scientists like Mann can speak as citizens who happen to be epistemic authorities. There is less incentive to live up to the norm of disinterestedness – Mann dismisses it as rather dangerous. And he does not seem to care about the conclusions the ‘chattering classes’ may draw about his scientific integrity either. He is preaching to the already converted, as it were.

While they are deemed to hold binding upon experts at the legislative stage more so than upon those who speak at the agenda setting stage, in the practice of demarcation, normative markers of expertise are generally adhered to. Both sides try to insulate their opinions from their expertise, political from epistemic authority, for the former must inform the latter – only occasionally do scientists speak out causing a stir. In Congress there are more likely to question each others’ epistemic markers of expertise: according to many mainstream climate scientists and their protégées (Oreskes & Conway, 2010), the sceptics’ experts cannot produce epistemic markers (formal qualifications or experience) appropriate to the debates about climate science; they lack certified knowledge in relevant disciplines; nor do they have a publication record in the appropriate scientific literature. They should not be heard in the political debate, whether in Congress, on the internet or in the print media.

#### *The scientists’ epistemic markers of expertise*

An advanced university degree is a widely accepted marker of expertise, but not all formally credentialed witnesses are necessarily qualified to speak as experts. Congresswoman Eddie Bernice Johnson (D-TX) so dismissed the witnesses called by her Republican opponents (HCSST, 2011: 9):

I look at this panel today and I must ask, well, where are they? Where are the masses of legitimate expert witnesses that will corroborate to the assertion that climate change is an unproven theory or worse yet a hoax? I don’t see them today. Instead the witnesses before the Science, Space, and Technology today include a business school professor of marketing, an economist, and an energy industry lawyer.

And because Michael Crichton’s university credentials were certainly not accepted by the Minority side, Crichton in turn questioned the boundary drawn by Congressional Democrats to exclude his view on global warming (SCEPW, 2005: 55):

I am not a climate scientist and I consider my observations useful precisely because I am an outsider looking at this field. I do consider myself a well-educated American citizen, and I share with my countrymen a healthy skepticism toward experts of all sorts. If war is too important to be left to the generals, science is too important to be left to the scientists.

Crichton's argument is at odds with the demarcation principles propounded also by Republicans. His testimony as an expert witness cannot be justified by his invitees' own standards, even though some of them seem to be unsure of where to draw the boundary: "[I]n a democracy anybody with an opinion is entitled to express that opinion and some are more qualified than others obviously because of their credentials, but I don't think we have a standard of witnesses that says unless you have a Ph.D. you cannot testify before--," explained Joe Barton (HCEC, 2006: 739).

Formal qualifications are not a prerequisite for witnesses to testify in Congress as such a standard would have excluded the majority side's arguably most important witness Steven McIntyre. Michael Mann noted in his testimony: "Neither McIntyre nor McKittrick is a trained climate scientist [...] Mr. McIntyre is a mining industry executive with no formal training in any discipline related to climatic research and Mr. McKittrick is an economist [...] hardly credentials that lend force to their academic arguments" (HCEC, 2006: 274). Forced to acknowledge his lack of formal qualification, which was readily exploited by Janice Schakowsky (D-IL), McIntyre insisted, albeit hesitantly, that in his restricted area of scientific publication, he was nevertheless still qualified to speak: "For the things that I have published on, my statistical and mathematical skills are adequate for what I have published on —" (HCEC, 2006: 739).

If we follow Collins and Evans (2002), McIntyre's practical skills and experience should lend his arguments authority. But many climate scientists thought otherwise, as McIntyre himself was all too aware: "I mean, quite frankly I could understand why there would be some reluctance to take the claims seriously at the beginning" (HCEC, 2006: 178). Because McIntyre lacked the "numerous publications in the peer-reviewed climate literature," Michael Mann would demand of climate experts, the 'hockey team' could dismiss McIntyre's critique as non-scientific. And arguably because they did not recognise McIntyre's epistemic markers of expertise, they did not see themselves as ethically obliged to share their data under Merton's communalist principle of science.

Other contrarians were harder to disqualify, since they possessed both advanced degrees in cognate sciences and had produced numerous publications in the scientific literature. Here the boundary was drawn quite explicitly along disciplinary lines. Forced

to acknowledge Wegman's expertise in statistics, Mann employed the discipline boundary to delegitimise the claims of his critic. Wegman's ignorance of basic radiation physics and his apparent lack of engagement with the climate science community would "underscore the hazards of an amateur seeking to draw conclusions in a field in which he has no expertise," warned Mann (HCEC, 2006: 766) in line with Oreskes and Conway. "And you shouldn't be ashamed of [not knowing the laws of thermodynamics] because you are a statistician, not a physicist," Jay Inslee (D-WA) sneered in Wegman's direction (HCEC, 2006: 75). In turn, Wegman used the discipline boundary to criticise the notable absence of professional statisticians in the Mann team, before he drew some sweeping conclusions about climate science in general: "What is insular thought I think is that it doesn't really involve people from the areas that I call the enabling sciences such as mathematics, computer science, and so on" (HCEC, 2006: 89).<sup>31</sup>

The discipline boundary also assisted Oreskes and Conway (2010) in dismissing climate sceptic, physicist and founding director of the NASA Goddard Institute for Space Studies (GISS) Robert Jastrow. With the same argument Mann snubbed Inhofe's witness, the astrophysicist Willie Soon: "I would not, for example, consider scientists with advanced degrees in Astronomy, Astrophysics, or Physics who have published primarily in those areas, as "climate scientists"—nor do I believe would most of my colleagues in the climate research community" (SCEPW, 2003: 178). The meteorologist William Gray in turn denied astrophysicist James Hansen any relevant expertise (SCEPW, 2005: 34-35):

James Hansen is a very bright, outstanding scientist, I have no doubt about that. He got his Ph.D., I believe, on the runaway greenhouse effect of Venus. I don't know what he knows about the atmosphere. He is not trained as a meteorologist, and I don't know why the press goes to him so much. I don't know why he could come down here in the hot summer of 1988, before a Congressional committee, and make these claims. They are ridiculous.

Oreskes and Conway on the other hand insist on Hansen's expertise precisely because he is a trained physicist and director (1981-2013) of the GISS.

Such inconsistent argumentation suggests that the drawing of boundaries along traditional epistemic lines, and disciplinary markers in particular, is an exceedingly

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31 Wegman sketched professional statisticians credentials: "Although there may be exceptions to the guidelines I am proposing here, in my view, a mainstream statistician will have graduated with a doctorate in statistics, will be actively doing research in statistical methodology or actively applying statistical methodology to a related discipline area, and will professionally identify with the statistics profession" (HCEC, 2006: 833). Also for Wegman (statistical) expertise is marked by official certification.

ineffective strategy of demarcation. They may offer some guidance to decision-makers but are no safe place to stay in the multidisciplinary climate sciences where physicists turn climatologists, who collaborate with dendrochronologists or meteorologists, whose vast amounts of data require statistical expertise, and so forth.

### *Peer review*

Because peer review is viewed as an authoritative, impersonal mechanism to separate science from non-science, the convinced scientists invoke the apparent lack of sceptical papers in the scientific literature to deny them epistemic authority. Naomi Oreskes (2004) even argued that not a single paper in the scientific literature seriously questioned the consensus. Unsurprisingly, her much cited analysis has since become a preferred argument in the political debate (e.g., Gore, 2006).

But peer review in (climate) science is not as straightforward as that gold standard implies (cf. Edwards & Schneider 2001). According to Edward Wegman, peer review at *Nature* failed in the case of MBH98 because no professional statisticians had scrutinised the team's research. More precisely, Wegman denied the reconstructive skill Mann cited in the hockey stick verification exercise. Mann justified the use of the term 'skill' with reference to the American Meteorological Society's (AMS) glossary, but to Wegman "...it doesn't matter that the American Meteorological Society says what statistical skill is. Statisticians do not recognize that term" (HCEC, 2006: 734). If the scientists had engaged with statisticians, the true uncertainties associated with a reconstruction's verification statistics would have been revealed, Wegman assured the Republican majority.

Yet because only "lip service was paid to the idea of formal peer review," Wegman's analysis would fail by his own standards, countered Mann (2012: 161). Also Bart Stupak (D-MI) noted that "Dr. Wegman's work is not yet published or peer-reviewed so it's very difficult for us to evaluate his work" (HCEC, 2006: 6). Jay Gullledge too came to Mann's defence (HCEC, 2006: 698):

Fortunately, a different group, one well qualified both statistically and climatologically to tackle this question of merit, had already performed the task several months before the Wegman Report was released. The study by Wahl & Ammann [2007] [...] was peer-reviewed and accepted for publication in the journal *Climatic Change*, early last spring, and has been publicly available in accepted form since last March.



But the sceptics do not accept examination or review by scientists who frequently co-author papers with Mann. Even if Wahl and Ammann had engaged statisticians, scientists who interact regularly would engender a common attitude or acquire shared biases, argued Wegman: “[T]here is some element of thinking that if they are frequent co-authors they are thinking the same way” (HCEC, 2006: 733). Anticipating the revelations that would come out with the publication of the ‘Climategate’ emails from UEA, Wegman went on to speculate: “I think there is evidence [...] that there is a tight-knit group of people who are interacting with each other and who frankly don’t seem to like to be criticized.” Hans von Storch tentatively confirmed: “I myself can say that they were always the same type of reviews we got, the same style and I am sure that it was the same person and I am sure it was the person we have spoken about here quite a bit” (HCEC, 2006: 214). Apparently, reviewer Michael Mann did not like von Storch’s research and gave bad reviews to delay or avert its publication. “I’m not sure [von Storch] isn’t somewhat of a skeptic himself,” Mann once wrote in an email to colleagues (EAE, 2009: 11 March 2003).

Wegman’s hypothesis of badly organised scepticism would eventually be corroborated by ‘Climategate’. The emails showed chief scientists in their attempt to keep sceptical science out of both the scientific literature and the IPCC. In the position of a journal reviewer, Phil Jones wrote to Mann (EAE, 2009: 31 March 2004):

Recently rejected two papers (one for JGR and for GRL) from people saying CRU has it wrong over Siberia. Went to town in both reviews, hopefully successfully. If either appears I will be very surprised, but you never know with GRL.

In the position of coordinating lead-author to the IPCC, Jones addressed Mann in yet another email (EAE, 2009: 8 July 2004):

The other paper by MM [McIntyre & McKittrick, 2003] is just garbage – as you knew. De Freitas again. Pielke [Sr.] is also losing all credibility [...]. I can’t see either of these papers being in the next IPCC report. Kevin [Trenberth] and I will keep them out somehow – even if we have to redefine what the peer-review literature is!

For Jones and Mann the mere possibility of politically motivated scepticism required active intervention in the peer review process to keep it out. More so than about McIntyre & McKittrick’s 2003 paper in *Energy & Environment*, they were concerned over the publication of the “Harvard-Smithsonian Centre’s work” (Soon & Baliunas, 2003) in the journal *Climate Research*. Since “some sceptics had identified

*Climate Research* as a journal where some editors were not as rigorous in the review process as is otherwise common,” according to editor-in-chief Hans von Storch (cited in Powell, 2011: 102), several scientists appealed for a boycott against publishing, reviewing for, or even citing articles from *Climate Research*. In an email Tom Wigley shared his reservations (EAE, 19 August 2003):

I have been closely involved in the [Climate Research] fiasco. I have had papers that I refereed (and soundly rejected), under De Freitas’s editorship, appear later in the journal – without me seeing any response from the authors. As I have said before to others, his strategy is first to use mainly referees that are in the anti-greenhouse community, and second, if a paper is rejected, to ignore that review and seek another more ‘sympathetic’ reviewer. In the second case he can then (with enough reviews) claim that the honest review was an outlier.

In his take on the hockey stick controversy Mann (2012: 121) remembers that the *Climate Research* editor “Chris DeFreitas’ advocacy against policies that restrict carbon emissions and his frequent attacks against the IPCC made him a highly unusual editor for an academic journal.” Mann dismissed McIntyre and McKittrick’s publication in *Energy & Environment* (2003) justifying himself (HCEC, 2006: 274-275):

*Energy & Environment* is not a peer reviewed journal; it is [...] primarily devoted to policy rather than science that appears to engage in, at most, haphazard review of its articles. Adding to the problem, the editor of *Energy & Environment* [...] has candidly acknowledged that the publication has a clear editorial bias [...] in her rush to get the McIntyre and McKittrick piece into print for political reasons *Energy & Environment* dispensed with what scientists consider peer review.

Quite evidently, the dispute between sceptics and mainstream scientists had chipped away at the golden image of peer review. In what Collins and Evans (2002) would call the public domain science of climate reconstruction one could not be sure if one’s research was treated with due diligence.

#### *The micro-politics of expert panels*

If peer review is no safe place to stay either, expert assessments are. They provide a pragmatic synthesis of the often incommensurable scientific views held by the various experts, and for the majority of climate scientists the IPCC presents the most comprehensive assessment of climate science. The Democrats’ expert witness Tom Crowley confirmed (HCEC, 2006: 138): “It involves hundreds of scientists, reviews of thousands of papers, and received on the order of 10,000 comments for each

of the earlier drafts [...] So my feeling is that it is a very, very thoroughly reviewed and vetted manuscript and I think just about the best thing we have.” Likewise, even as he conceded “that process isn’t exactly perfect,” NRC panellist Gerald North “cannot imagine a better, more efficient way to pull several thousand scientists together” (HCEC, 2006: 109). But the more ambitious the assessment is, that is the greater the number of disciplines covered and researchers involved, the more difficult it is for such an assessment to do justice to the wealth of scientific opinions.

To its critics the 2001 IPCC consensus on millennial climate change was nothing more than the opinion of a few scientists who acted as gatekeepers. In his capacity as lead-author of the chapter “Observed Climate Variability and Change” Mann also happened to review his own study. “[S]ome of the authors of the IPCC assessment dealing with global temperature history were not independent or impartial. They also happened to be the authors of the hockey stick studies themselves,” Joe Barton (R-TX) encapsulated the critics’ main charge (HCEC, 2006: 9). As we have seen, Mann’s scientific collaborator and IPCC co-ordinating lead-author Phil Jones also tried to keep critical papers out of the IPCC (EAE, 2009: 8 July 2004). His actions were facilitated by the institution’s hierarchical architecture, the IAC (2010) concluded in its review of ‘Climategate’. Being at the top of the hierarchy, lead-authors could exert disproportional influence on its content.

The NAS’ STR report (NRC, 2006) on the other hand was almost equivocally endorsed. The fact that all sides were able to claim victory without being entirely dishonest is attributable to both the small number of experts involved – “it may be, [...] that the NRC approach cannot be used for such a complex and large field, which the IPCC is covering,” noted Hans von Storch (HCEC, 2006: 220), – and the committee’s democratic approach to consensus. A panellist described the committee’s politics:

Consensus is hard to define as I see it. I think a majority vote on a committee would carry the day if it were very contentious [...] But there has been [...] a great effort to move things in such a way as to persuade the dissidents to be more agreeable to the outcome. For example, majority members will often bend the wording of a sentence to be more satisfactory to minority members. Often this works. As usual there are a wide variety of viewpoints on this panel (anonymous, 2013).

A majority vote among 153 scientists as in the IPCC chapter on “Observed Climate Variability and Change” would inevitably repel some of its participants, leaving a disgruntled minority. John Christy, IPCC lead-author and one of the 153, has since

turned his back on the IPCC, and once again resorted to the ideal of ‘sound science’: “Since consensus is a political notion, not a scientific notion, a goal of consensus in any form is at its heart a political goal” (HCSST, 2011: 59). If we take seriously Christy’s boundary, the NAS’ STR report is not scientific either – and yet Christy served as a panellist to that report.

The notion of expert consensus has provoked and will continue to provoke discussion. In the run-up to the IPCC AR4 published in 2007, climate scientist Eduardo Zorita noted and discussed the problem a scientific consensus entails. Emailing various colleagues Zorita mused (EAE, 2009: 17 August 2006):

Consensus. This paragraph may be problematic. Again what is the consensus? If we look at the recent NAS report, which again not every one would agree with, the ‘consensus’ is reduced to the past 400 years in comparison to IPCC, leaving ample space for speculation before this period. Does the NAS report belong to the consensus? [P]erhaps partially, but I am not sure to what extent.

“Let’s have a better consensus than we do now,” demanded Congressman Joe Barton (SCCST, 2007: 240). He would have to wait 6 years until a consortium of 78 scientists took up the sceptics’ criticism on the hockey stick and reconstructed millennial climate change almost from scratch. The so-called Past Global Changes (PAGES) 2k Consortium was a new project which relied on experts who had few if any ties to the ‘hockey team’.

## **5.6 Discussion**

In the previous two sections I have presented norms and principles which the protagonists in the hockey stick controversy hold binding upon the scientist. Because the scientific method alone is not enough to discriminate between a true and false climate reconstruction, certain markers of expertise should help politicians to exclude the objections by those ‘lay experts’ who have nothing to add but a more or less educated guess, such as Michael Crichton’s. But when the ‘hockey team’ and their protégées ignored Steven McIntyre, claiming that he was not a scientist, they invited counter-accusations along similar lines. His exclusion not only meant that politicians were withheld the best possible advice in their assessment of climate policy, his replication exercise also revealed that certain scientists found it difficult to perform the sorts of organised scepticism generally associated with their profession. Sceptics

courted McIntyre precisely because he was not part of that norm-breaking group.<sup>32</sup> With the concepts offered by the ‘Wave One’ and ‘Wave Two’ SSK, in this section I analyse the debates between the ‘hockey team’ and its critics, between Mann and McIntyre. What part would Collins & Evans’ theory of expertise have played in the dispute over climate reconstruction?

Worried that sceptics would overemphasise mistakes and sloppy, ‘Mannian’ data management for political reasons, the scientists were eager to illuminate the wider political context of climate denial. McIntyre’s contribution to the sceptical blogosphere, and his background as a mining consultant potentially funded by the fossil fuel industry, conveniently served Mann to ignore his requests for data; if we follow Merton, McIntyre’s putative political motivation was enough to deny him disinterestedness. The hacked emails then proved that the ‘hockey team’ had not only been inattentive to McIntyre’s claims, but themselves had an interest in certain scientific results – they were motivated by other than the noble pursuit of truth. They closed ranks, judged any theoretically valid critique based on the cultural authority of its claimants, and excluded sceptical experts from the putative consensus so as to further *their* agenda. They too violated certain Mertonian norms (cf. Grundmann, 2013).

Throughout the controversy, both sides qua ‘Wave One’ sociologists or Mertonians engaged in that kind of circular critique – in any aspect of the climate debate political bias is found in what should be strictly scientific. Thus, and if we follow the ‘Wave Two’ scholars, such bias is more of a rule than the exception (cf. Ben-David, 1991). Because scientific work and expert assessments always contain an element of ‘political bias’ – the NAS’ NRC panel on STR too *agreed* on the wording of their report, – the inevitable micro-politics of science, as described in seminal ‘Wave Two’ volumes (cf. Shapin & Schaffer, 1985; Latour, 1987), will continue to serve as an argument for critics of the proclaimed consensus on climate change. During these debates conventional boundaries between science and non-science are called into question and the analysts’ task would be to follow that boundary and to describe whether, where, and how it (re-)appears, and who is served as a result. “Expertise [does] emerge, but with respect to the allocation of power it [is] neither neutral nor

<sup>32</sup> With McIntyre climate sceptics have employed the popular image of the lone, disinterested empiricist, epitomised in Galileo, Newton and Einstein, who by virtue of being a sceptic disproves the pseudo-scientific other. Only the individual observer, who does not need to consent with anyone else, can follow the scientific method.

innocent,” summarises Jasanoff (2003: 397).

According to Collins and Evans, on the other hand, the analyst should not merely describe but assist in the making of new boundaries. If we follow Collins and Evans, McIntyre should always have been treated on a par with the scientists – arguably he was deeply involved in experimentation or theorization, and thus should have been included into the core-set as experience-based expert. But we can draw this conclusion only with the benefit of hindsight as in 2003, when he first published his critique on blogs as well as in the non-indexed journal *Energy & Environment*, McIntyre was not a recognised member of the core-set. He had experience in statistics, but did not yet have any publications in the scientific literature as a marker of his experience-based expertise. And, ultimately, it is the job of the established experts in the field to recognise and acknowledge appropriate experience. McIntyre knew that and therefore sided with McKittrick – he knew that he had to publish in recognised, high-ranking peer-reviewed journals and his publications in *GRL* in 2005 eventually mark his metamorphosis from lay to experienced-based expert. By the time of the hockey stick hearing in 2006 his exclusion was no longer tenable, yet in light of the sceptical backlash against climate science – sceptics have indeed overemphasised and de-contextualised tiny mistakes for political reasons (e.g., EAE, 2009: 20 April 2007) – Mann still disregards McIntyre’s arguments. But since the boundary has shifted in McIntyre’s favour, he cannot dismiss them any longer as non-scientific.

Collins and Evans of course recognise the difficulty of drawing boundaries in such highly contentious public controversies. In order to account for this context, they want to discriminate between esoteric and public domain sciences: if palaeoclimatology was a public domain science, the scientists should have been open to McIntyre’s critique. But as McIntyre once acknowledged, “[w]hen paleoclimate research had little implication outside academic seminar rooms, the lack of any adequate control procedures probably didn’t matter much” (2005b). It was the hockey stick’s popularisation, the public hearings, and eventually ‘Climategate’, which have turned palaeoclimatology into a public domain science. Thus, once again, the boundary between esoteric and public domain sciences is anything but clear from the outset of the controversy. It can be drawn more easily in retrospect.

Next Collins and Evans presume that the ‘chattering classes’ will find ways to discriminate who to agree with. “Their consensual view emerges from the making of

social judgements about who ought to be agreed with, not scientific judgements about what ought to be believed,” the sociologists assert (2002: 259), and ask whether the author of a claim has integrity or is known to have made unreliable claims in the past. In climate science, these questions have become increasingly difficult to answer. There is hardly one scientist who has not made a statement which could be used to question their impartiality – on the internet upset climate bloggers mine for any potentially compromising statements – and there is always at least one journalist that creates a scandal. It is strange, to say the least, that the rationalists Collins and Evans put so much hope in the ‘chattering classes’.

Speaking with Jasanoff (2003), the processes through which (climate) expertise emerges are difficult to anticipate. The making of authoritative scientific knowledge depends as much on the participants, some of who entered the debate via the internet by chance, as on the wider political context, and it is influenced by the venues onto which the controversy is carried. It is difficult, and maybe not even desirable, for the sociologist to try to foresee the many events that have lifted bristlecone pines onto the legislative stage of decision-making. And while US Congress has certainly helped to constitute palaeoclimatology as a public domain science, after the hearings the controversy has retracted to more esoteric scientific venues where the core-set, which now includes McIntyre, solidifies (Mann *et al* 2008; McIntyre & McKittrick, 2009; Mann *et al* 2009). This supports Jasanoff’s observation that it is never totally clear from the outset who the experts are. Soon many more may demand access; soon many more may claim to possess to the scientific controversy indispensable knowledge.

### **5.7 Conclusion**

Because both sides in the climate debate agree on the epistemic and normative markers of expertise intended to keep science pure, which is requirement of the linear model of science and society, they accuse each other of not complying with them. This has allowed both sceptics to deny the proclaimed scientific consensus on AGW and scientists to dismiss the sceptics’ science backing their claims against the consensus. **In this chapter I have shown that Collins and Evans’ proposed solution to the problem of legitimation in form of a theory of expertise is of little help. It has merely shifted our attention from the disputed facts to the equally disputed**

**persona of the scientist and the norms they should adhere to.**

Once it was revealed that high-profile climate scientists had violated the very principles they themselves invoked so as to justify the exclusion of their critics, publics in the US (and arguably also in the UK) lost some of their trust in the scientists, the IPCC and the dire scenarios of climate change, which are now often considered the opinions of some environmentalist lobby group rather than scientific predictions. Faced with this disbelief in climate change, the hockey stick controversy and ‘Climategate’ has triggered a debate about how timely such traditional boundaries and its markers of expertise are, especially if they give rise to accusations of a conspiracy.

Because boundaries cannot be easily prescribed or may prove inadequate in the next controversy, why not do away with them now and forever? Not least in light of the damning emails, such a seemingly logical solution to the problem of demarcation has been proposed by both some scientists, science students, and sceptics. For the latter, the politicisation of climate science demands an open and transparent re-evaluation of climate reconstruction by experts who are not part of the consensus. “[C]limate science needs adult supervision, [and] Congress needs at least one second opinion,” encapsulated John Christy (Congress, 2011: 46). And while the authoritative reviews of ‘Climategate’ univocally demand openness and transparency in the production of expert assessments (IAC, 2010; Muir, 2010), some climate scientists, too, see advantages in a public ethos of science, if only to bolster their authority in front of a mistrusting audience. In the next chapter, I show how these demands are implemented in the science of climate reconstruction and to what effect.



## *Chapter Six*

### *Public accountability and scientific expertise after ‘Climategate’*

And what about the issue itself? Are we really experiencing Anthropogenic Carbon-based Global Warming? If the public loses faith in that claim, then the situation of science in our society will be altered for the worse. There is very unlikely to be a crucial experience that either confirms or refutes the claim; the post-normal situation is just too complex. The consensus is likely to depend on how much trust can still be put in science. The whole vast edifice of policy commitments for Carbon reduction, with their many policy prescriptions and quite totalitarian moral exhortations, will be at risk of public rejection. What sort of chaos would then result? The consequences for science in our civilisation would be extraordinary.

Jerry Ravetz (2010)

#### **6.1 Introduction**

In the Congressional hearings on climate science the Republicans have repeatedly confronted their political opponents with charges about the unprofessional conduct of ‘their’ scientists. With the ‘Climategate’ email disclosures casting unfavourable light on the private interchanges among climate scientists, many have called for changes in the production of scientific climate knowledge and its expert assessments. While sceptics insisted that an independent re-analysis of climate reconstruction would reach a different answer than the Mann team’s hockey stick, and yield a different picture to the one presented in the IPCC assessment reports, others hoped that more transparent methods of analysis open for wider external scrutiny such as McIntyre’s would win public trust, re-authorise the science of climate reconstruction, and carry the day.

To this end, the Republican sceptics, credentialed contrarians, and conservative think tanks proposed an independent re-analysis of climate science of which several demands were made: “An independent effort to reconstruct the global temperature [...] demands a dedicated project with proper resources,” reasoned the prominent sceptic Fred Singer (Watts, 2011a). It should be “overseen by a non-activist team which includes those with experience in the scientific method, such as physicists [...],” insisted John Christy (HCSST, 2011: 46). “Everybody [should] take their shot at it,” or at least “anyone who has the scientific ability and the mathematical ability to study it,”

suggested Joe Barton (HCEC, 2006: 250).

Following in the tradition of the intelligence community, conservative think tanks called for the creation of a so-called Red Team to reconsider climate reconstruction (cf. The Marshall Institute, 2010). In the defence and intelligence world Red Teams are regularly convened to provide an independent, critical review of intelligence agency issues, systems and programs. Staffed by experts from outside the immediate agency or team whose conclusions are being assessed, the Red Team is charged with performing a competitive critique of a specific topic. Epistemically, the model merges two distinct models of truth seeking. From the sciences, there are strong echoes of peer review and the idea that the organised scepticism of the wider community of experts provides the best check against bias and error. But from the Anglo-Saxon tradition of common law, there is also the idea that truth is best served through adversarialism and the clash of competing viewpoints before a judge and jury. This of course was the logic organising the aggressive cross-examination of climate scientists before Congress, where John Christy made the case (HCSST, 2011: 167):

The IPCC continues to be led by an establishment of scientists and bureaucrats who believe humans are having a catastrophic impact on the climate system and who desire strong greenhouse gas controls. It is important to remember that the IPCC provides one view of climate change and that there are other views equally backed-up by evidence but which have been marginalized or eliminated from the IPCC venue. As such, at least one other venue independent from the IPCC, such as a “Red Team,” is necessary.

In view of the negative social relations ‘their’ scientists’ behaviour has provoked, the Democrats and the wider climate science establishment cannot dismiss the sceptics’ demand for a re-assessment. But since they want science to speak with a single authoritative voice, the proverbial one-handed scientist, they shunned a second and potentially adversarial report. Instead they urged climate scientists to modernise the processes by which science is produced, appealing to their ethical obligation to openly share their data and show their working (IAC, 2010; Muir, 2010). “Openness and transparency enables critical examination by a broad range of scientists and citizens [...] and frank discussions with skeptics are needed,” the climate scientist Judith Curry maintained in a Democrat-sponsored hearing on climate science (HCSS, 2010: 212).

Two new projects took into consideration the various points of critique, and this chapter describes the new and improved climate reconstruction. First, the independent PAGES 2k Consortium published a comprehensive climate reconstruction of the past

2,000 years (PAGES 2k Consortium, 2013). Second, a group of esteemed physicists from Berkeley University presented its independent re-analysis of instrumental temperature records (Rhode *et al* 2013). Also, in 2014 the IPCC published its AR5 (IPCC, 2013a). Unlike the PAGES 2k Consortium and the so-called Berkeley Earth Surface Temperature (BEST) project, which both conduct original research, the AR5 is an expert review of the existing peer-reviewed scientific literature.

Before I describe the updated IPCC assessment and the two independent projects, in the next section I review the theory of PNS by the philosophers of science Jerry Ravetz and Silvio Funtowicz: because it seeks to address the demand for an open, more inclusive science, PNS serves as this chapter's conceptual framework (Funtowicz & Ravetz 1990ab, 1993; Ravetz, 2004; 2006; also Gibbons, 1999; Gibbons *et al* 1994). Like Collins and Evans, Ravetz and Funtowicz are sceptical of the traditional boundaries drawn between scientists and non-scientists. But other than Collins and Evans, who find epistemic reasons to exclude 'lay experts', the latter propose a radical solution to the problem of extension. The "post-normal situation" of "uncertain facts, high stakes, disputed values, and urgent decisions" (Funtowicz & Ravetz, 1993) would ask not for new boundaries but for the dismantling of any existing ones. Among the measures to (re-)authorise political decisions which depend on the electorate's trust in science is the extension of the scientific peer review community.

## **6.2 'Climategate' in PNS**

In Chapters 3 and 4 we noticed that the practical turn in the philosophy of science opened up the so-called problem of closure. How do scientists come to agree on a scientific truth if the scientific method alone does not account for it? If social factors account for the closing of a scientific proposition as true, that is agreed upon rather than achieved through incontrovertible evidence and logical reasoning, questions about the scientific method can at least for analytical purposes be ignored. The method still serves as a guiding principle, but studies in the new SSK have shown that scientists habitually strayed from it.

Confronted with the resulting problem of extension, Collins and Evans (2002) proposed a theory of expertise which would exclude 'lay experts'. But because their exclusion has led to 'Climategate' and a loss of trust in climate science, that boundary

is no longer justified, argues Jerry Ravetz (2010) on a climate blog run by the meteorologist-sceptic Anthony Watts. And because in the post-normal situation science has been increasingly opened up to outsider participation, Ravetz supports his argument with the theory of PNS. Writing on ‘Climategate’ Ravetz and the geographer Mike Hulme contend (Hulme & Ravetz, 2010):

Where claims of scientific knowledge provide the basis of significant public policy, demands for what has been called “extended peer review” and “the democratisation of science” become overwhelming [...] Unsettling as this may be for scientists, the combination of “post-normal science” and an internet-driven democratisation of knowledge demands a new professional and public ethos in science. And there is no better place to start this revolution than with climate science. After all, it is claimed, there is no more pressing global political challenge than this. But might this episode signify something more in the unfolding story of climate change – maybe the start of a process of restructuring scientific knowledge?

In practice PNS achieves its goals by extending scientific peer review. The so-called extended peer community consists “not merely of scientists with some form or other of institutional accreditation, but rather of all those experts and lay people with a desire to participate in the resolution of the issue,” write Funtowicz and Strand (2007). These extended peers would identify unexplained assumptions and tacit value choices in science (why did the Mann team weight bristlecone pines higher than other proxy series?), they would challenge each other’s views about what science is and how it should be carried out, and they would debate what they hope to achieve, and how they would like to see science used (cf. Turnpenny, 2012; Turnpenny *et al* 2011). The extended peers, sometimes called citizen scientists or citizen peers, then organise in community-based participatory research (Bidwell, 2009), multi-actor approaches to decision-making (Frame & Brown, 2008; Frame & O’Connor, 2011), so-called upstream engagement in science (Wilsdon & Willis, 2004) as well as on blogs where they discuss scientific and political questions. In this way PNS would tap into local knowledge and restore a sense of trust and ownership (cf. Lahsen, 2007). Writing on Watts’ blog, Ravetz analyses the hockey stick controversy (2010): “From the record, it appears that in this case, criticism and a sense of probity needed to be injected into the system by the extended peer community from the (mainly) external blogosphere.”

Ravetz hopes that if the institution of climate science allows for outsider participation in knowledge production, the authoritarian climate policy prescriptions in danger of rejection will give way to authoritative climate governance. With this hope he

is not alone (e.g., Von Storch, undated; Bray & von Storch, 2009, Krauss *et al* 2012). Judith Curry explained in her Congressional testimony (HCSS, 2010: 179):

[C]limate scientists and the institutions that support them need to acknowledge and engage with ever-growing groups of citizen scientists, auditors, and extended peer communities that have become increasingly well organised by the blogosphere. The more sophisticated of these groups are challenging our conventional notions of expertise and are bringing much needed scrutiny particularly into issues surrounding historical and paleoclimate data records. These groups reflect a growing public interest in climate science and a growing concern about possible impacts of climate change and climate change policies. The acrimony that has developed between some climate scientists and blogospheric skeptics was amply evident in the sorry mess that is known as Climategate. Climategate illuminated the fundamental need for improved and transparent historical and paleoclimate data sets and improved information systems so that these data are easily accessed and interpreted.

But analysts who make the case for an ‘extended peer community’ are themselves unsure of what PNS actually is. “It is not clear from the numerous papers published by Funtowicz and Ravetz over two decades whether PNS is a phenomenon, a prescription for a new kind of science, a heuristic, a theory, or something else again,” criticise Wesselink and Hoppe (2010: 5). Indeed, Funtowicz and Ravetz’s model of PNS is not simply normative; it claims to be descriptive as well; it assumes that there is such a thing as normal science and that it has been succeeded by PNS (Goeminne, 2011). PNS is often described vis-a-vis normal science and instances such as ‘Climategate’, which, explains Brigitte Nerlich (2010: 436),

may have damaged public understanding of science and science-based public policy [...] by making people believe that science is based on the pursuit of certainty, or universal truth, or on the achievement of an absolute consensus, [which perpetuates] a very outdated understanding of (normal) science.

Also Ravetz (2010) understands “the root cause of Climategate as a case of scientists constrained to attempt to do normal science in a post-normal situation.” Other than “[t]he approach used by normal science to manage complex social and biophysical systems as if they were simple scientific exercises [which] has brought us to our present mixture of intellectual triumph and socio-ecological peril” (Funtowicz & Ravetz, 2003:1), the new public and professional ethos of PNS shall lead to a better public understanding of science and shall re-establish trust in climate scientists and decisions informed by them. Thus, for the purpose of this chapter and the argument of the thesis as a whole, I treat PNS as a theory of expertise that speaks to the problem of authorising science qua science for technical decision-making.

In the next section (6.3) I tell the story of climate reconstruction as presented in

the IPCC reports since 1990. Because in the run-up to AR5 the IPCC's non-transparent review-process, which allowed a few gatekeepers to highlight their science at the expense of other research, had been heavily criticised, in the latest assessment report the IPCC promised to re-assess the science including the new climate reconstructions in an open and transparent way (IPCC, 2013c). This should re-establish the authority of their reports in the contentious politics of climate change.

Then, in sections 6.4 and 6.5 I describe both the production of the new science (PAGES 2K Consortium and BEST) and the reactions its results have provoked across the climate divide. In the aftermath of 'Climategate' this new research was conducted without the contribution of the established IPCC cadre and following the sceptics' demand for Red teams. They too answered calls for a more open and transparent climate science as heralded by PNS theorists. And because climate sceptics would accept those independent and open re-analyses of climate reconstruction, their reaction to the new science will prove most interesting. The findings are discussed in section 6.6 followed by the conclusion in 6.7.

### ***6.3 The IPCC assessments of centennial to millennial climate change***

Since 1990, in intervals of 5 to 6 years, the IPCC has assessed and synthesised the peer-reviewed scientific literature. Its task is to prepare policy-relevant knowledge for decision-makers, who typically read the reports' SPM. With respect to the science of climate reconstructions, "[t]he main policy-relevant question could be phrased as follows," explained the oceanographer and former IPCC lead-author Stefan Rahmstorf: "Does the past climate history tell us how sensitive the climate system is to CO<sub>2</sub>?" (EAE, 2009: 1 October 2004). In an email to IPCC contributing authors the coordinating lead-author Jan Overpeck elaborated (EAE, 2009: 22 April 2009):

[T]he key for inclusion in an IPCC assessment, is to synthesize the published literature in a way that informs policy makers (the top audience) on what is happening in the climate system, and more important even what will happen in [the] climate system. Taking the terrific speleothem work for example, what are the key lessons that are NEW and important to highlight to policy makers?

Assuming a pivotal role of science in the politics of climate change, Jan Overpeck notes that even speleothems, which are cave dripstones whose variations in growth rate and composition reflect environmental changes on the land surface above the cave, may

provide or contribute to policy relevant knowledge.<sup>33</sup>

In its first report in 1990 the IPCC reproduced a schematic diagram in which the late climatologist and founder of CRU Hubert Lamb noted a marked period of medieval warmth during which the Vikings settled in Greenland (Lamb, 1965). But since most of Lamb's data came from a few regions in the Northern hemisphere and largely in the form of anecdotal evidence, the certainty over the global extent of medieval warming was low. "[I]t is still not clear whether all the fluctuations indicated were truly global," concluded the IPCC in 1990 (p.202). And after the IPCC dropped Lamb's diagram for the hockey stick graph in its TAR in 2001, the scientists wondered how Lamb's diagram could ever have been so uncritically accepted by the authors of FAR. Raymond Bradley mused in an email to Mann and colleagues (EAE, 2009: 2 January 2007):

In any case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what happened, like something we might sketch on a napkin at a party for some overly persistent inquisitor.... (so make sure you don't leave such things on the table...). What made the last millennium graph famous (notorious!) was that Chris Folland must have seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he gave a citation and it thus appeared to have the imprimatur of the IPCC.

Bradley seems to be well aware that Lamb's hand drawing, much like Overpeck's speleothems, itself had little authority – it is its publication in the IPCC that would lend knowledge authority in the politics of climate change.

After this 'U-turn' and McIntyre's critique on the hockey stick, scientists involved in the preparation of AR4 in 2007 were wary about the sensitivity of the subject. "We will to a large extent be judged on how we tackle the hockey stick, sensitivity, unprecedented 20th century warming [issues] in view of palaeo[records]," wrote one coordinating lead-author of the chapter on palaeoclimate, Eystein Jansen, to his colleague Jonathan Overpeck (EAE, 2009: 10 January 2005). Four months later his addressee made up his mind. Overpeck wrote to Keith Briffa (EAE, 2009: 23 May 2005):

We're hoping you guys can generate something compelling enough for the TS [Technical Summary] and SPM – something that will replace the hockey-stick with something even more compelling.

A month on and the two coordinating lead-authors Overpeck and Jansen reminded Keith Briffa and fellow lead-authors that "figures MUST BE POLICY RELEVANT

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<sup>33</sup> To be sure, speleothems have played a comparably minor role in the reconstruction of centennial to millennial climate change (McDermott, 2004).

AND COMPELLING” (EAE, 2009: 14 July 2005, capitals in original). “Thanks again for helping us lead the next big push!,” they bowed.

Overpeck and Jansen’s pressure on lead-authors to support their political agenda put the latter in an uncomfortable position. “I have been accustomed to write about scientific facts. Now I am confronted with a new problem how to serve the purpose of another style,” wrote an unidentified lead-author to Overpeck (EAE, 2009: 12 Jan 2005). Also the IPCC veteran Briffa was worried about the imperative of policy-relevancy; in a reply to Overpeck and Jansen he defended the lead-authors’ choice of figures (EAE, 2009: 3 February 2006):

[W]e 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 (and as a total range as before) allows the uncertainty to swamp the magnitude of the changes through time. We have settled on this version (attached) of the Figure which we [hope] you will agree gets the message over but with the rigor required for such an important document.

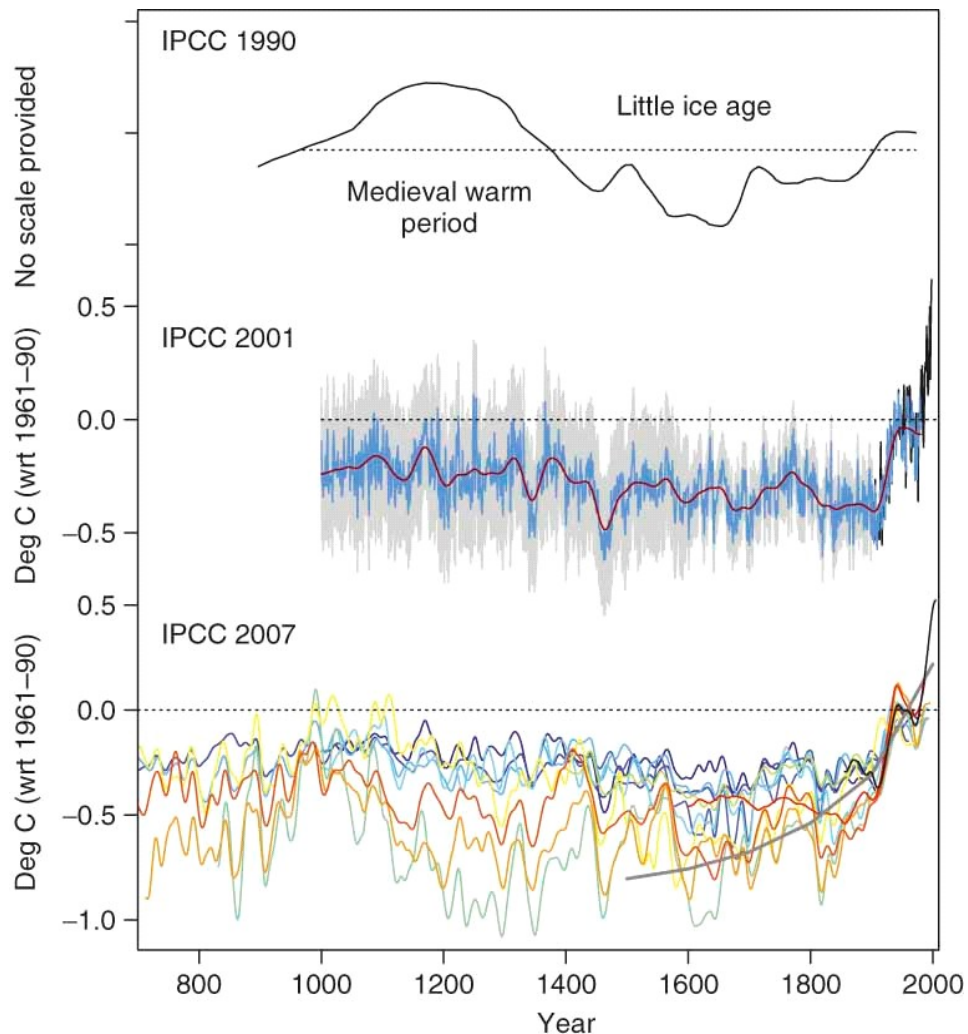
In all of these conversations the scientists are shown to struggle with the task of bridging the gap between science and politics, which gives ample scope for choice, judgement and interpretation. Aware of the subject’s sensitivity they are trying to find a consensus they can all live with.

The result of the IPCC authors’ negotiations was a diagram presenting several climate reconstructions (figure 6.1, p. 136). In figure 6.1, palaeoclimatologist David Frank and colleagues assembled Lamb’s graph from FAR, the hockey stick graph from TAR and the graphs from AR4, summarising the change from a single “noodle” in 1990 to the hockey stick in 2001 and finally a “spaghetti plate” of graphs in 2007: “[F]urther consideration of existing and the development of new reconstructions, methodological disputes, and analysis called for a retreat from the 2001 position that reconstructed temperatures were well understood. Numerous, smoothed reconstructions in the 2007 report testify to significant remaining uncertainty” (Frank *et al* 2010).

In 2009 ‘Climategate’ revealed how those IPCC authors have exerted disproportional influence on assessment and synthesis of climate science. Reviewing the affair, several committees urged the IPCC to reform this process. “Because the individuals involved in the IPCC assessment process carry the burden and responsibility of maintaining the public’s trust, it is important for all involved to act



with transparency and integrity and to abide by appropriate codes of conduct,” warned the IAC (2010: 58). Echoing PNS, according to the IAC and other inquiries (e.g. Muir, 2010), the IPCC has to “respond to a larger and more demanding group of stakeholders” (IAC, 2010: 59). As a consequence the IPCC (2013c) subscribed to an “objective, open, and transparent review process.”



**Figure 6.1:** IPCC icons of temperatures over the past millennium. “Sequence of the pre-industrial to industrial temperatures as expressed in the 1990 (upper), 2001 (middle), and 2007 (lower) IPCC reports. Dashed lines represent mean temperatures at 1900 in the upper panel and for 1961–1990 in the middle and lower panels. The upper panel was graphically recreated, whereas the middle and lower panels are based upon data obtained at the NCDC webpage. Curves illustrate the evolution of the ‘consensus views’ for large-scale temperature change in the IPCC reports”, Source: Frank et al (2010).

In September 2013 the IPCC finally published its AR5 SPM (IPCC, 2013b) followed by the full report in April 2014 (IPCC, 2013a). The chapter on palaeoclimate contains a graph showing different climate histories of the past 2,000 years for both the Northern and Southern hemispheres as well as its global aggregate (figure 6.2, p. 138). As regards the disputed Northern hemispheric climate history, the report concluded (IPCC, 2013a: 408-409):

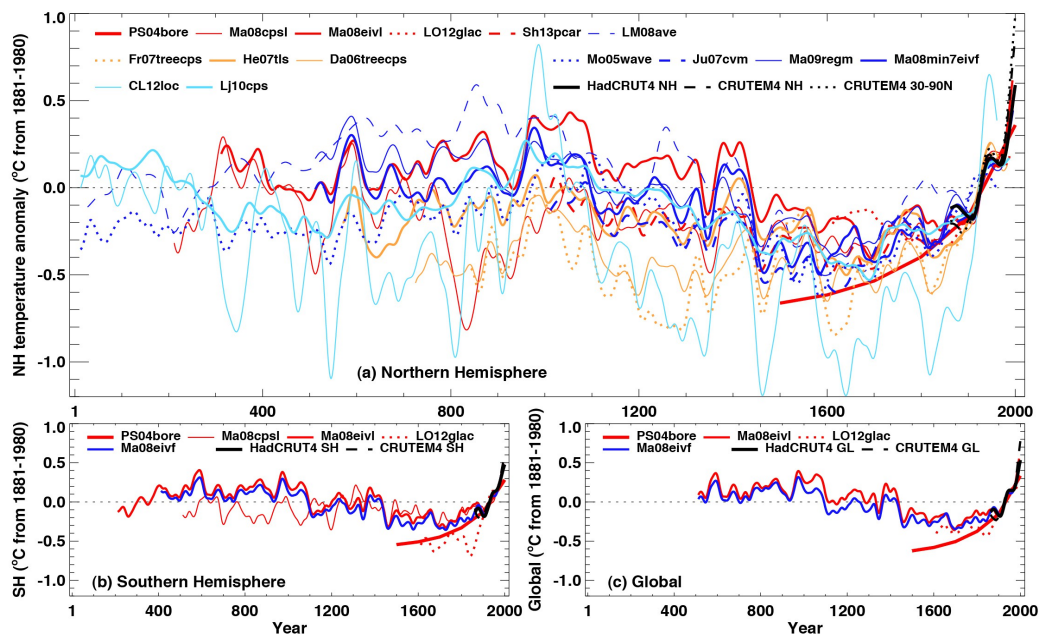
Based on multiple lines of evidence using different statistical methods or different compilations of proxy records [...] published reconstructions and their uncertainty estimates indicate, with high confidence, that the mean NH temperature of the last 30 or 50 years very likely exceeded any previous 30- or 50-year mean during the past 800 years [...] reconstructions covering part or all of the first millennium suggest that some earlier 50-year periods might have been as warm as the 1963–2012 mean instrumental temperature, but the higher temperature of the last 30 years appears to be at least likely the warmest 30-year period in all reconstructions. However, the confidence in this finding is lower prior to 1200, because the evidence is less reliable and there are fewer independent lines of evidence.

First, and arguably as a response to the sceptics' calls for more robust statistical analyses, the report stressed the use of different methods. Second, the use of 30- or 50-year means instead of single decades or years – the 1990s and 1998 in MBH98/99 – allowed for more confidence in the results. 1998 may still have been the warmest year over a millennium, but the confidence in annually resolved data, in particular when they come from qualitatively different sources, was much lower. Third, there is a chance that there have been extended warm periods in the first millennium A.D. and not covered by the hockey stick study.

Policy-makers were offered the same conclusion however in slightly amended form (IPCC, 2013b):

Warming of the climate system is unequivocal, and since the 1950s, many of the observed changes are unprecedented over decades to millennia [...]. In the Northern Hemisphere, 1983–2012 was likely the warmest 30-year period of the last 1400 years (medium confidence) [...]. Continental-scale surface temperature reconstructions show, with high confidence, multi-decadal periods during the Medieval Climate Anomaly (year 950 to 1250) that were in some regions as warm as in the late 20th century. These regional warm periods did not occur as coherently across regions as the warming in the late 20th century (high confidence) [...].

In the SPM, past periods as warm as the late 20th century were acknowledged as a distinct possibility, but they did not occur on a global scale. And because GHGs affect the climate globally, no matter where and how they are emitted (cf. Argawal & Narain, 1991), the SPM deemed that scale the scientifically and politically most interesting one.



**Figure 6.2:** The IPCC AR5 2,000 year temperature reconstruction. “Reconstructed (a) Northern Hemisphere and (b) Southern Hemisphere, and (c) global annual temperatures during the last 2000 years.” Source: IPCC 2013a.

### *Reactions to the IPCC AR5 re-assessment of climate reconstruction*

The IPCC’s assessment of climate reconstruction was endorsed across the climate blogosphere. On a well-known climate blog Stefan Rahmstorf (2013b) reported: “The last 30 years were probably the warmest since at least 1,400 years. This is a result from improved proxy data. In the 3rd IPCC report this could only be said about the last thousand years, in the 4th about the last 1,300 years.” Likewise, on his Facebook account Michael Mann quoted extensively from the SPM, crowing that this latest IPCC assessment “...is in fact a **STRENGTHENING** of the original Hockey Stick (and Third Assessment Report) conclusion that recent warmth was **LIKELY** unprecedented for the **PAST 1000 YEARS**” (Mann, 2013c, capitals in original). For these scientists the IPCC draws its authority from both the extensive review process and the great number of expert participants from diverse disciplines. In Rahmstorf’s view the IPCC remains authoritative precisely because of its consensual construction which “tends to produce very cautious and conservative statements [...] Despite or perhaps even because of this conservatism, IPCC reports are extremely valuable – as

long as one is aware of it” (Ibid.). The “conservative” scientific consensus serves as argument against the sceptics’ charge of IPCC alarmism.

Climate sceptics interpreted the AR5 review of millennial climate change quite differently. Relying on a leaked document of the SPM, for Anthony Watts (2013b) “they’ve gone from saying warmest in the last 1300 years to the last 800 years,” thus back to its position of 1990 when they published Lamb’s graph. For Douglas Keenan (2013) “the correct conclusion is that there is no demonstrated observational evidence for global warming.” Invoking ‘sound’ scientific principles, he justified his conclusion with “flaws [in statistical analysis which] imply that there is no demonstrated observational evidence that global temperatures have significantly increased (i.e. increased more than would be expected from natural climatic variation alone).” “The Medieval Warm Period, located by AR4 in the centuries spanning the end of the first millennium, is now apparently accepted as probably being warmer than the current warming,” asserted the author of *The Hockey Stick Illusion* Andrew Montford (2013). “There doesn’t appear to be a single skeptic (correct me if I’m wrong) in the author list,” observed Anthony Watts (2013b).

#### **6.4 *The PAGES 2k Consortium’s 2,000 year climate reconstruction***

Included in the scientific literature which the IPCC assessed for AR5 is a climate reconstruction produced by the PAGES 2k Consortium. In 2006 the consortium of 78 scientists began to reconstruct continental-scale temperature variability during the past two millennia (PAGES 2k Consortium, 2013, N.B.: the article is often cited as Kaufmann *et al* 2013 or Ahmed *et al* 2013). They justified their effort with a lack of regional-scale climate information beyond the instrumental period, “which is important as we prepare for the full range of future climate changes due to both anthropogenic and natural factors,” the scientists noted on their webpage.<sup>34</sup> Lead-author Darrel Kaufmann (2013) rationalised the so-called regions-up approach they have adopted, pointing both to the way in which it mobilises local expertise and to its potential for including new data that would have been more difficult to assemble for a centralised global reconstruction. One of the original architects of the PAGES 2k Consortium, the Swiss climate scientist Heinz Wanner, reiterated: “A key aspect of the consortium effort

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<sup>34</sup> Knowledge of climate variability on regional scales can inform the adaptation of ecosystems and societies to climate change (<http://pages-igbp.org>; Accessed on 5 November 2013)

was to engage regional experts who are intimately familiar with the evidence for past climate changes within their regions.”<sup>35</sup> The inclusion of local expertise is also a key aspect of PNS, however there it tends to carry a different meaning that is ‘lay expertise’.

In order to both maintain the regional effects and arrive at a robust global reconstruction, the scientists adopted and applied various statistical methodologies and mathematical procedures. By doing so they can assess the extent to which the main conclusions of the study stand up to the different analytical approaches. Thus, and whether consciously or not, the PAGES 2k Consortium responded to the sceptics’ criticism of the hockey stick. In a Congressional hearing Senator Inhofe (SCEPW, 2005: 2)

look[ed] forward to determining whose data is most comprehensive, uses the most proxies, maintains the regional effects, avoids losing specificity through averaging statistics, considers more studies, and most accurately reflects the realities of the Little Ice Age, reflects the realities of the Medieval Warming Period, and more.

Moreover, the PAGES 2k Consortium was open to all interested participants and they made available all data and methodology on their webpage. In this way, the PAGES 2k Consortium exercised openness and transparency with the aim of forestalling the negative social relations the Mann team’s secrecy had provoked. On their webpage they answered to the frequently asked question about the peer review process:

The manuscript was reviewed carefully by anonymous peer reviewers and it was revised extensively to address their concerns. Specifically, the first round of reviews by three referees included 12 pages (6200 words) of comments, to which the authors’ point-by-point replies spanned 6 pages (3500 words). A fourth referee was added for the second round of reviews, which amounted to 6 pages (3500 words) of comments, with 3 pages of author replies (1400 words) [...] This is an extensive review by any standard, not to mention the vetting of the manuscript among 78 co-authors during both the writing and review/comment phase.<sup>36</sup>

The peer review was organised by the editors of the journal *Nature Geoscience* where the study was published as Progress Article in 2013. *Nature Geoscience* publishes studies as Progress Articles when the discussion is focused on a developing field that might not yet be mature enough to be considered a Review Article, which, however relative, describes a form of scientific closure.

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35 Ibid.

36 Ibid.

The PAGES 2k Consortium offered three primary conclusions. First, the most coherent feature in nearly all of the regional temperature reconstructions was a long-term cooling trend, which ended late in the 19th century. Second, temperatures did not fluctuate uniformly among all regions at multi-decadal to centennial scales. For example, there were no globally synchronous multi-decadal warm or cold intervals that define a worldwide MPW or so-called little ice age (LIA). Third, the 20th century ranked as the warmest or nearly the warmest century in all regions except Antarctica. During the last 30-year period in the reconstructions (1971-2000), the average reconstructed temperature among all of the regions was likely higher than any time in nearly 1,400 years. However, some regions experienced 30-year intervals that were warmer than 1971-2000. In Europe, for example, the average temperature between 21 and 80 AD was warmer than during 1971-2000 (PAGES 2k Consortium, 2013).

The Consortium did not attempt to attribute causes to these observed changes. Nor did they claim to have achieved scientific closure: “The new PAGES 2k database will no doubt be analyzed using alternative approaches that will reveal additional patterns and address further research questions.”<sup>37</sup>

#### *Reactions to PAGES 2k Consortium*

Climate scientists welcomed the Consortium’s reconstruction and disseminated its findings primarily on social media platforms. On Facebook Michael Mann (2013b) offered his summary: “PAGES 2k Consortium (70+ paleoclimate scientists) reports in latest *Nature Geoscience*: “[global] average reconstructed temperature likely higher than any time in nearly 1400 yrs.” Rahmstorf (2013a) likened the study to a twin of the original MBH99 hockey stick reconstruction: “The global average age of the new reconstruction looks like a twin of the original “hockey stick”, the first such reconstruction published fifteen years ago.” The environmental scientist Dana Nuticelli (2013) explained that the hockey stick has won the rivalry with sceptics: “PAGES two main results are a confirmation that current global surface temperatures are hotter than at any time in the past 1,400 years (the general ‘hockey stick’ shape), and that while the MWP and LIA are clearly visible events in their reconstruction, they were not globally synchronized events.” “It confirmed that recent global and continental-scale warming was ‘very unusual’ in recent Earth history,” stated Jonathan Overpeck (SMH, 2013).

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<sup>37</sup> Ibid.

In front of ‘their’ audience, who they address via (social) news media, these scientists described the study’s authority as a function of the great number of certified experts who have intimate explicit and tacit knowledge of the unique regions and the local sites of measurement. For example, wrote Dana Nuticelli (2013):

This network consists of scientists from 9 regional working groups, each of which collects and processes the best paleoclimate [...] data from their respective region. It’s a clever approach because it allows the experts in their local proxy [area] to contribute to a much larger global project.

But since their interpretative focus is on global climate, Nuticelli, Rahmstorf, Mann and Overpeck either omitted or mentioned in passing its third primary conclusion (some regions experienced 30-year intervals that were warmer than 1971-2000). Nor have they mentioned uncertainties or explained the assumptions inherent in the calibration exercise.

Climate sceptics on the other hand stressed a particular finding as well as uncertainties stemming from proxy data. “[C]laims of unprecedented recent temperatures are not supported by the regional reconstructions,” opened Anthony Watts (2013a), noting that in Europe the average temperature between 21 and 80 AD was warmer than during 1971-2000. Steven McIntyre, who despite his expertise on proxy data had not participated in the project, adopted a well-known line of critique (McIntyre, 2013). He found three studies which would

clearly demonstrate that the sediments are contaminated as climate proxies [...] Kaufmann and paleo peer reviewers ought to be aware that the recent portion of varve data [NB: a varve is the annual layer of sediment that typically contains the climate proxy pollen] can be contaminated by modern agriculture [...] The contaminated series is readily identified as an outlier through a simple inspection of the data. The evidence of contamination by recent agriculture in the specialist articles is completely unequivocal. This sort of mistake shouldn’t be that hard to spot even for real climate scientists [...] It is unclear why Kaufman selected pollen accumulation rate out of all the available measurements.

McIntyre seems to have accepted the authority of PAGES 2k Consortium qua real climate scientists and so agrees with his opponents on the markers of expertise.

Because the 70+ paleoclimatologists are all credentialed experts, sceptics once again invoked the empiricist ideals of ‘sound science’: “So much of climate science looks far different when the raw data is inspected,” commented Watts on McIntyre (2013). On their respective blogs Watts and McIntyre then added satellite images and photographs which show the contaminated site whose data should have been excluded based on insufficient a priori criteria. “Pages2k need to have a policy on when and how

an in/out decision (about proxy data) is to be made. And they have in their rules delegated that to the original authors, their journal and reviewers, whose job it is to make that decision for publication,” noted the sceptic Steven Mosher (in McIntyre, 2013), echoing the institutional constructivist critique on the Mann team we have examined in Chapter 5. Arguably because of its inconvenient conclusion, Mosher mused over the micro-politics at PAGES 2k Consortium and *Nature Geoscience*.

Despite these voices of disagreement the sceptics’ critique did not make headlines of the conservative news press. Nor did right-leaning think tanks such as Marshall, American Enterprise, Cato and Heartland report on another potential controversy. Neither was there a rebuttal in the scientific literature. But most citizen scientists on Watts’ blog had already made up their minds. Read a comment made on Watts’ blog *wattsupwiththat.com* (Watts, 2013a):

Of course, it’s an established and accepted scientific fact that the MWP was a worldwide warm period; warmer than the present. We don’t need doubtful proxies, flawed studies or so called climate experts to tell us that. The Vikings grew potatoes on Greenland, for goodness sake!

If these peers were genuinely interested in the science of climate reconstruction, they would follow up on McIntyre’s deconstruction and (fly to Greenland to) replicate pollen proxy series. And if they wanted to repeat an institutional constructivist critique on the Consortium’s politicking and aim at a Congressional investigation, they would have to hope for a Republican majority. But there was no visible critique on the processes by which the PAGES 2k Consortium authorised its findings. Because their conclusions have not yet been cited by politicians pledging for emission regulation schemes, climate sceptics have for the time being ‘accepted’ its science of continental-scale temperature variability during the past two millennia.

### **6.5 *The BEST instrumental temperature reconstruction***

In the chapter “Observations: Atmosphere and Surface” the AR5 assessed a re-analysis of surface temperature reconstructions produced by an independent team of scientists at Berkeley University (Rohde *et al* 2013). In 2010, highly credentialed physicists from Berkeley’s renowned Lawrence Livermore National Laboratory, among them the Nobel Prize winner Saul Perlmutter and Enrico Fermi Award winner Arthur Rosenfeld, decided to “carefully stud[y] issues [on instrumental climate reconstruction]



raised by skeptics” (Muller, 2012). Led by the esteemed physicist Richard Muller, BEST re-analysed the hitherto authoritative but increasingly criticised instrumental climate reconstructions produced at CRU, NOAA/NCDC, and NASA/GISS. These all showed a marked 20th century warming as predicted by the theory of AGW, and based on this convergence of opinion, in 2007 the AR4 concluded that the planet has warmed by 0.74 degrees Celsius since the year 1900. According to Richard Muller, authoritative scientific closure over this number and its causes was necessary for the political debate to continue (cited in Revkin, 2012):

I hope that the Berkeley Earth analysis will help settle the scientific debate regarding global warming and its human causes. Then comes the difficult part: agreeing across the political and diplomatic spectrum about what can and should be done.

While reaching the same conclusion using different methods was taken by the IPCC as cause for greater confidence in these instrumentally based reconstructions, sceptics countered that all three projects shared much of the same data, and so were hardly independent. In order to independently replicate instrumental climate reconstructions they began to examine the weather observatories which produce the underlying data (see Chapter 4, pages 90-91). As strong proponents of the so-called UHI hypothesis they argued the observed warming to be attributable to urbanisation around observatories, and not to GHG emissions. As critics of the IPCC they claimed that at least two studies (De Laat & Maurellis, 2004, 2006; McKittrick & Michaels, 2004) which document a statistical correlation between temperature and patterns of socio-economic development (urbanisation) were purposefully ignored by its lead-authors – they were so excluded from the ‘consensus’. The blending out of this research would seriously undermine the 0.74 degrees warming trend, wrote the climatologist and fellow of the Cato Institute Patrick Michaels (2009):

Imagine if there were no reliable records of global surface temperature. Raucous policy debates such as cap-and-trade would have no scientific basis, Al Gore would at this point be little more than a historical footnote, and President Obama would not be spending this U.N. session talking up a (likely unattainable) international climate deal in Copenhagen in December. Steel yourself for the new reality, because the data needed to verify the gloom-and-doom warming forecasts have disappeared.

Subscribing to the linear model in which climate policy follows climate science, Michaels ridicules his ideological adversaries who have invested much political capital in an international climate deal.

To help settle the scientific debate, the BEST team sought to reduce four distinct

biases which are thought to have adversely affected the existing reconstructions: The ‘UHI bias’ as explained above, the ‘time of observation bias’ which holds that a change in observation hours results in different mean temperatures, the ‘station move bias’ which holds that the relocation causes a ‘jump’ in its temperatures, and the ‘change of instrumentation bias’ which holds that a change in thermometer type introduces an ‘offset bias’. Because sceptics called for a (1) non-activist, (2) inclusive and (3) transparent re-analysis, BEST performed its reconstruction in ways that answer to these demands.

(1) First Muller seized upon the revelations of ‘Climategate’ to demarcate himself and his project from the activist climate scientists. “They are advocates and no longer scientists. The bad effect of this is that the public then loses some of its trust in science, and that is deeply unfortunate,” Muller remarked in a Congressional hearing (HCSST, 2011: 116). At BEST, he announced, “[n]one of the scientists involved has taken a public political stand on global warming [...] our goal is to not have any political views, not to become advocates, simply do the best job we can on the science” (HCSST, 2011: 116). And Richard Muller (2011a)

get[s] even more upset when some other people say, oh, science is just a human activity. This is the way it happens. You have to recognize, these are people. No, no, no, no. These are not scientific standards. You don’t hide the data. You don’t play with the peer review system. We don’t do that at Berkeley.

As we have seen in Chapter 5 these statements of demarcation in order to affirm one’s expert authority are typical of scientists who appear at the legislative stage of decision-making. Muller’s insistence on Mertonian norms also relates to the ideal of ‘sound science’, which denies the scientists any agency in the construction of knowledge.

(2) By calling upon the climate sceptics Anthony Watts and Steven Mosher to participate in BEST, Richard Muller also sought to enrol outsiders who had previously been marginalised or eliminated from the IPCC venue. The Berkeley scientists showed particular interest in Watts’ expertise on UHI bias; they used data provided by his Surface Stations Project to weight temperature records in accordance with station quality. In his research Watts (2012a) found that approximately 90% of the examined 1,000 stations had been “compromised by encroachment of urbanity in the form of heat sinks and sources, such as concrete, asphalt, air conditioning system heat exchangers, roadways, airport tarmac, and other issues.” Unlike climate modellers and scientists

who “refuse to get out of the office, to examine firsthand the condition of the network” (Watts, 2012b), Watts could claim to have performed an empirical observation in the spirit of ‘sound science’. What is more, the Surface Station Project conveniently connected Muller’s BEST with Watts’ extended peer community: for the documentation of the encroachment of urbanity Watts had to enrol dozens of uncredentialed volunteers in a photographic survey.<sup>38</sup> To the meteorologist and critic of the IPCC, Roger Pielke Sr. (2012a),

the BEST use of Anthony’s data illustrates that the surface station project led by Anthony is a robust scientific endeavor [...] Anthony Watts is as much a part of the climate science community as are those who wrote the IPCC reports. It is just that those who wrote those reports consciously decided to exclude viewpoints such as Anthony’s.

And Richard Muller acknowledged Watts’ contribution (HCSST, 2011: 41):

Without the efforts of Anthony Watts and his team, we would have only a series of anecdotal images of poor temperature stations, and we would not be able to evaluate the integrity of the data. This is a case in which scientists receiving no government funding did work crucial to understanding climate change. Similarly for the work done by Steve McIntyre. Their “amateur” science is not amateur in quality. It is true science, conducted with integrity and high standards.

Whether Watt’s revised data would leave the desired impact on the global temperature index, which is the entity both sides in the political debate are most concerned with, remained to be seen. The innumerable point measurements were once again moulded into 5°x5° grid boxes spanning the globe, turning ‘real data’ into ‘models of data’. And because it is unlikely that Watts’ data from 1,000 corrected (out of 39,000) stations would leave any trace in the corrected global temperature index, in the UK the engineer Tim Channon prepared to follow Watts’ example. Assuming the scientific value of these projects, Jerry Ravetz (2010) celebrated the inclusion of sceptics and their volunteers as examples of PNS.

(3) In the name of openness and transparency the BEST team posted all raw data and their analysis code online, arguably so as to lower the barriers to entry into climate science. Muller also announced the project’s sources of funding, notably the Koch Foundation which is a group known for having supported climate sceptics in science and politics, such as the Heartland Institute. Committed to total transparency the scientists went yet one step further and publicly released their findings before

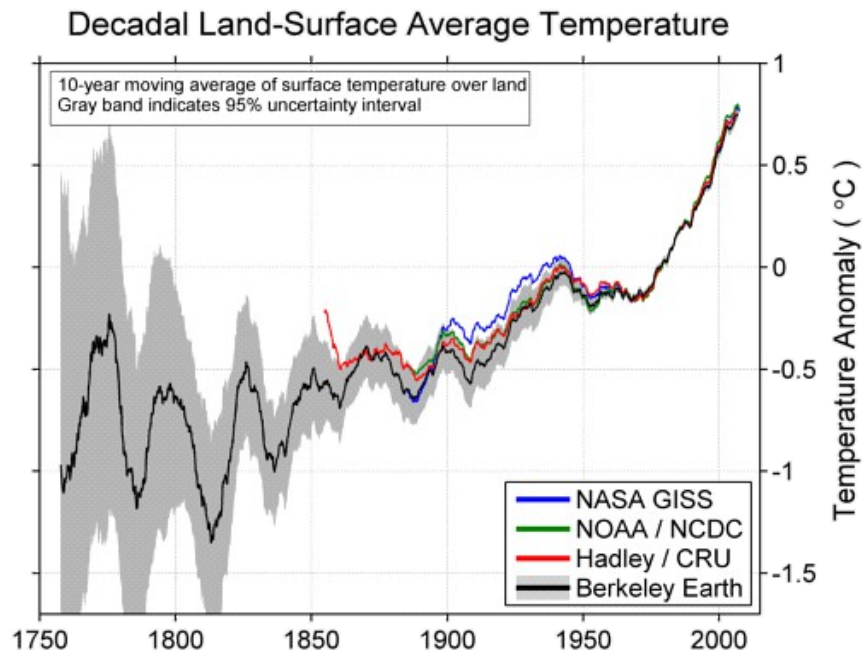
<sup>38</sup> The photographs of weather stations do not follow any discernible standard but are released online with information on 22 photo properties including ISO, shutter time, saturation, exposure in order to evoke their objectivity. (<http://surfacestations.org>; Last accessed on 23 December 2013)

anonymous reviewers had a chance to either reject or accept them. They justify this move with the urgency of decisions (in post-normal times): “I believe that the findings are too important to wait for the year or longer that it could take to complete the journal review process,” explained Muller’s daughter and executive director of BEST Elizabeth Muller (cited in Revkin, 2012). A year later formal peer review in the newly launched journal *Geoinformatics & Geostatistics* eventually confirmed the results. “A New Estimate of the Average Earth Surface Land Temperature Spanning 1753 to 2011” (Rhode *et al* 2013) depicted a temperature increase of 1.5 degrees Celsius from 1753-2011, and a rise of 0.9 degrees Celsius from 1950-2010. Figure 6.3 shows that the BEST results essentially agreed with previous estimates of 0.81 to 0.93°C for the 1950-2000 period (figure 6.3, p. 148).

The pre-release of its results meant that by the time of its formal publication in 2013 BEST was little newsworthy. As early as 2011 Muller expressed “surprise [...] that the new results agreed so closely with the warming values published previously by other teams in the US and the UK” (Muller, 2011b), claiming “...the issues raised by skeptics, such as possible biases from urban heating, data selection, poor station quality, and data adjustment do not unduly bias the results.” “Essentially all of the warming of the past 250 years is caused by humans,” Muller (2013) concluded in a key-note speech at a gathering for information around climate change policy and carbon markets. Unlike in Congress, where the disinterested physicist refrained from making such strong statements, on the many informal venues he frequented, Muller concluded that “you should not be a skeptic, at least not any longer” (Muller 2011a). As far as Muller was concerned scientific closure over global warming has been achieved, clearing the path for a rational policy response, which BEST should spearhead. In late 2013 the Centre for Policy Studies (CPS), a free-market British policy think tank, published Richard and Elizabeth Muller’s study “Why Every Serious Environmentalist Should Favour Fracking.” With BEST the linear model of climate science and climate policy was revived; the Mullers’ study should convince both the left and the right of fracking as the most rational course of action.<sup>39</sup>

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<sup>39</sup> Fracking is the process of drilling and injecting fluid into the ground at a high pressure in order to fracture shale rocks to release natural gas. Because the burning of natural gas as energy source emits less CO<sub>2</sub> than does the burning of coal, anyone concerned about climate change should endorse fracking, argue the Mullers.



**Figure 6.3:** The BEST 250 years temperature reconstruction. “Land temperature with 1- and 10-year running averages. The shaded regions are the one- and two-standard deviation uncertainties calculated including both statistical and spatial sampling errors. Prior land results from the other groups are also plotted. The NASA GISS record had a land mask applied; the HadCRU curve is the simple land average, not the hemispheric-weighted one, Source: <http://www.berkeleyearth.org> (Accessed on 13 November 2013).

### *Reactions to BEST*

Because BEST was conceived explicitly as a response to their critique, climate sceptics gave their praise in advance. Wrote Anthony Watts in early 2011(b):

I’m prepared to accept whatever result they produce, even if it proves my premise wrong. I’m taking this bold step because the method has promise. So let’s not pay attention to the little yippers who want to tear it down before they even see the results. I haven’t seen the global result, nobody has, not even the home team, but the method isn’t the madness that we’ve seen from NOAA, NCDC, GISS, and CRU, and, there aren’t any monetary strings attached to the result that I can tell. If the project was terminated tomorrow, nobody loses jobs, no large government programs get shut down, and no dependent programs crash either. That lack of strings attached to funding, plus the broad mix of people involved especially those who have previous experience in handling large data sets gives me greater confidence in the result being closer to a bona fide ground truth than anything we’ve seen yet.

On Watts’ blog Fred Singer voiced his agreement with the meteorologist (Watts, 2011a):

The Project is in the hands of a group of recognized scientists, who are not at all “climate sceptics” — which should enhance their credibility [...] I applaud and support what is

being done by the Project — a very difficult but important undertaking. I personally have little faith in the quality of the surface data, having been exposed to the revealing work by Anthony Watts and others. However, I have an open mind on the issue and look forward to seeing the results of the Project in their forthcoming publications. As far as I know, no government or industry funds are involved — at least at this stage.

Their advance enthusiasm for BEST, notably for the lack of involvement by government which they accuse of politicising climate science so as to further a state-socialist agenda, makes their subsequent hostility to its findings when they were issued all the more interesting.

Once Watts' premise was proven wrong by the released results, he quickly criticised their station siting method. A new and improved rating method for weather stations would show less warming for US stations, and thus should have been employed by BEST, argued the meteorologist (2012b). Next Watts resented Muller's presentation of preliminary conclusions in a Congressional hearing, arguing "...today's hearing presenting preliminary results seems rather topsy turvy" (Watts, 2011c). "But post normal science political theater is like that," he added. According to Watts, scientists are only allowed to release their unpublished research if they respect IPRs. And since he shared his data with Muller in confidence, he felt his right to publish first was undermined. Muller's strategy would rather resemble a "PR blitz" which Watts (2011c) emphatically disapproved of.

Conservative think tanks joined in to bash Muller and BEST. Patrick Michaels of the Cato Institute lamented its "massive pre-peer-review public relations campaign" before criticising BEST's omission of sea surface temperature: "As a result of these and other peccadilloes, the BEST team has yet to publish one peer-reviewed paper, despite conspicuously dominating the op-ed pages for a year now." In conclusion, "BEST really does not tell us much that is new," Michaels (2012) stated, discounting the team's effort. For the Marshall Institute's CEO William O'Keefe (2011),

releasing results to the media before independent review stretches credibility [...] If the study had not been partially funded by the Koch Foundation which is characterized as being skeptical that human activities are the primary cause of global warming, Muller's research probably would have received zero attention.

Writing for the Heartland Institute, James Taylor dismissed BEST as "the very definition of meaningless." According to Taylor (2011) Muller did not even remotely address the issues presented by global warming sceptics:

It is very difficult to imagine that someone like Richard Muller is so clueless about the position of global warming skeptics. Is Muller really living so deeply under a rock that he truly has no idea about the substance of skeptics' objections, or is Muller deliberately presenting a straw man argument with the intent of deceiving casual observers about the true nature of the global warming debate?

In the sceptics' view, Richard Muller, who calls himself a converted sceptic arguably in order to shore up his credibility as critic of mainstream climate science, never was a true sceptic.

Mainstream climate scientists too take issue with Muller the convert. To the climate modeller William Connolley (2012) "all of that is bollocks." "What Muller is saying," Connolley went on, "is that he read a few sceptic blogs, didn't bother read any of the scientific literature, and so decided to run his own project. So is that his model for converting sceptics?" For Roger Pielke Sr. (2012b) "[Muller's] comments show what occurs when scientists with excellent research credentials within their area of scientific expertise go outside of their area of knowledge." In Eric Steig's opinion "the Berkeley results are newsworthy [...] only because Muller had been perceived as an outsider (driven in part by trash-talking about other scientists), and has taken money from the infamous Koch brothers." "As far as the basic science goes," Steig continued, "the results could not have been less surprising if the press release had said 'Man Finds Sun Rises At Dawn'" (Steig, 2011). "The basic scientific results have been established for a long time now, so I do not see the results of Muller et al as being scientifically important. However, their result may be politically important," affirmed the atmospheric physicist Ken Caldeira (2012, also Santer, 2012). Following the release of BEST, Michael Mann (2012b) noted on his Facebook account:

It's great that he reaffirmed what we already knew. But for him to pretend that we couldn't trust this entire scientific field until Richard Muller put his personal stamp of approval on their conclusions is, in my view, a very dangerously misguided philosophical take on how science works.

It turns out that scientists and sceptics agree over the scientific and political value of BEST: it is deemed little newsworthy and it did not bring about closure of the debate as Muller hoped it would.

In the next section I analyse their reactions to BEST, the PAGES 2k Consortium, and the IPCC AR5. I discuss what openness, transparency and public participation in science, as heralded by PNS as theory of expertise, can add to the scientific debate on climate reconstruction.

## **6.6 Discussion**

After the hockey stick controversy and ‘Climategate’ two demands were made of new climate science. On the one side climate sceptics argued for a Team B to reconsider climate reconstruction and its much criticised IPCC assessment. On the other side critics urged for more openness and transparency. These should be the new markers of the trustworthy (climate) expert, making more or less redundant the traditional ones we considered in Chapter 5. The two new projects presented in this chapter, PAGES 2k Consortium and BEST, answered to these demands. The IPCC on the other hand is neither new nor open about the processes by which it arrived at its policy-relevant conclusions. Despite its promises to act on the IAC recommendations, its review process was anything but transparent. It continued to disallow outsiders to study its consensus formation (cf. Hulme & Mahony, 2013) arguably because the inevitable micro-politics of consenting could be interpreted as a politicisation of science – in light of the sceptical deconstruction of climate science, ‘Climategate’, and the perceived loss of trust, the IPCC wanted to keep the image of a pure science alive. In this chapter, then, the IPCC has served as a decoy or as a negative example to which the PAGES 2k Consortium and BEST are compared to – if openness and transparency in the form of a new scientific ethos make climate science more trustworthy, the IPCC cannot be trusted; it is not authoritative.

Taking into account the sceptics’ demands, independent scientists of the PAGES 2k Consortium openly and transparently addressed critique on previous studies extending multi-proxy reconstructions in both space and time. Yet despite inviting outsiders to participate, and contrary to claims by PNS theoreticians, the public has not been interested in arguably the most contentious time period in climate history. The PAGES 2k Consortium had to go without the sceptics’ expertise on climate proxies as neither McIntyre nor any citizen peer signed up to the project. And it is hardly discussed on those blogs which represent the internet-driven democratisation of climate science. The reason for this ignorance can be found in its comparably humble conclusions and lack of public endorsement. The PAGES 2k Consortium authors did not claim to have closed the scientific debate, they did not claim to have produced policy-relevant knowledge that puts an end to scepticism, and politicians have not (yet) cited the study in their avowal of orthodox climate policy.

Addressing the sceptics’ demands head on, in BEST independent physicists and



outsiders to the consensus re-analysed the much criticised work of CRU, NOAA/NCDC, and NASA/GISS. To avoid negative social relations they showed their working and released all data and conclusions on their webpage. Muller also drew from Watts' Surface Stations Project and his citizen peers' local expertise. He hoped that its socially more adequate construction would make the results more trustworthy, especially in front of a sceptical audience. And yet it failed. BEST failed to persuade climate sceptics because Richard Muller proclaimed its results to be politically relevant – “you should not be a sceptic, at least not any longer”. If he had not made such public statements, his scientific claims would have faced less resistance. But apparently Muller truly had no idea about the substance of sceptics' objections which are motivated by their rejection of governmental proposals to regulate GHGs. This suggests that even an open and transparent IPCC would do little to cool the debate. Its self-proclaimed political relevancy, and the politicians' endorsement of its assessments, keeps the heat on and scepticism alive.

But why did BEST fail to charm the convinced climate scientists who celebrated both the PAGES 2k Consortium and IPCC AR5? The reason lies in Muller's arguments on why BEST should be trusted. Like the sceptics, many convinced scientists consider the authority of science to be foundational, to rest safely in its method, not in its social construction. Therefore it should not matter that the credentialed physicist Muller gave his approval – “unless a physicist has spent some time reading atmospheric science and climate texts and journal articles, the physicist is unlikely to know much of anything about how the climate system works,” confirmed Curry (2012), – that the Koch Foundation gave the money, and that the papers were pre-released for extended peer scrutiny. In fact, and as we have seen in Chapter 5, these are the very arguments climate scientists and their protégées used to employ against sceptics who come from a pure physics background and do not publish in the climate scientific literature. Pretending to stand above the fray, Mann considered such arguments for or against scientific authority a very dangerously misguided philosophical take on how science works – and yet we have seen him employ them against his critics.

The signal achievement of BEST, then, is not its science but is revealed through Muller's attempted reconstitution of legitimate climate scepticism: by re-constituting which people in which institutions and through which media can authoritatively discuss climate science, Muller made blatantly obvious that the debate over climate

reconstruction is only marginally about the science. The scientifically little controversial debate to which Muller contributed provided many opportunities for stealth issue critics of climate policy to challenge their political opponents, the stealth issue advocates of emission reduction schemes (Pielke, Jr., 2007). Open critics of such climate policies would have no reason to challenge (the re-analysis of) climate reconstruction. For example, opponents of fracking hardly challenge the ‘legitimising’ BEST science.<sup>40</sup> But *because* science has become a key argument in climate policy-making, stealth issue critics debate the foundations of its authority.

The ‘post-normal’ process by which Muller sought to authorise his science for the political debate has not only estranged sceptics and mainstream climate scientists but entails several boundary paradoxes.

First, Muller and his cohort Watts offer no markers or mechanisms by which citizen peers can be demarcated from politically motivated critics. Because citizen peers have no credentials (per definition), both the scientists and the sceptics, if they adhered to their own standards, should dismiss their ‘lay expertise’. In fact, most of Watts’ peers did not even attempt to give critical reviews. Read a ‘review-comment’ on Watts unpublished research on station siting methods (Watts, 2012b):

What we all suspected UHI does have a tremendous effect on surface temps reading so we can conclude maybe 50% of the warming not significant well in the USA there is no AGW so there is no global either AGW thank you Mr Watts!

Revealing his ad hoc preference for UHI over AGW, this comment by veterinarian Rogelio Diaz exemplifies the sorts of opinionated, ill-founded critique many sceptical peers contribute. In the face of such lay claims Collins & Evans’ proposition to exclude non-scientists based on epistemic grounds seems only reasonable.

Second, the opening up of the review process offers no mechanism by which to demarcate a scientific from a non-scientific publication. The BEST double-truth strategy – peer review serves to demarcate soberly vetted scientific knowledge from an emotionally charged public who have been discussing the pre-release on various blogs – is dismissed by both sides as not properly scientific. What would be the value of a pre-release that is not eventually published in the certified scientific literature?

Third, pre-peer-reviewed publications entail a conflict between the goals of openness, the norm of communalism and the (intellectual property) right to publish

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<sup>40</sup> see, e.g., <http://www.dangersoffracking.com/> (Accessed 12 June 2014)

one's own data first. As Michael Mann once explained to members of Congress (HCEC, 2006: 764),

[a]sking scientists to release their codes before they have had an opportunity to apply them to a number of potential interesting problems is asking them to sacrifice their competitive advantage. This would be no different than asking Microsoft to release the code for its latest operating system as soon as it reaches the market. Microsoft is not about to do that, and most people would consider a requirement that Microsoft freely dispense its intellectual property – its codes – as antithetical to the principles of a free market. The argument is no different in the case of scientists and their computer codes or other tools of their trade.

Watts agreed with Mann when he criticised Muller for having presented his data in Congress. But Watts' citizen peers could turn that critique against the sceptic himself: according to IPRs the photographers would have the right to publish their data first. Sure enough, because of their shared political commitment against climate policy, they refrain from doing so. This also explains why Mann shared his data with like-minded colleagues rather than with Steven McIntyre.

### **6.7 Conclusion**

In this chapter I examined the promise new 'post-normal' scientific practices of climate reconstruction hold for the contentious politics of climate change. My findings suggest that the hopes invested in an open, transparent and inclusive climate reconstitution will not materialise. In fact, the opening up of a proclaimed politically relevant science entails more problems and paradoxes than it solves. The participation of amateurs alongside formally qualified scientists and the likely conflict between the goals of openness and IPRs yet again require new boundaries. And if closure of a scientific controversy requires openness, openness will require control since the extension of the peer community means that more people will smuggle their political views into esoteric scientific debates.

The logical next step would be to draw a boundary around the body of technically-qualified-by-experience contributors to technical decision-making. But since Collins and Evans' markers of expertise too have become contested, we should instead examine the assumptions uniting these theories of expertise. **Both Funtowicz & Ravetz and Collins & Evans assume that political decisions rest to a significant degree upon the authority of the facts and experts informing them, i.e., an**

**authoritative closure of a scientific controversy is important only if the legitimacy of political decisions depends on it. Their theories of expertise testify to the belief in the linear model of science and society always. They have not been substantiated with empirical data; they are ‘normative theories’, an oxymoron.**

Bt what if political closure over climate policy does not primarily depend on the authority of science qua science, whether normal or post-normal? In the next and concluding chapter of the thesis I elaborate on this idea.

## *Chapter Seven*

### *Summary and conclusion*

#### *7.1 Summary and discussion of the main findings*

I started my thesis with ‘Climategate’ as evidence for the politicisation of climate science. A few days before the COP15, the hacked emails caused even the convinced public to question the basis for its trust in climate scientists and the institutions charged with addressing climate change. And the ‘shocked’ climate sceptics triumphed, since in a vain effort to close off political debates (and arguably to regain popularity) influential politicians had cited from what turned out to be politicised scientific research. By quoting amongst other ‘IPCC science’ the hockey stick study, many Western leaders (Bush, 2000; Blair, 2004; Gore, 2006) had hoped to convince the electorate of the rationality of their policy position. They followed a strategy modernists of all stripes have employed for decades and centuries; they seized the authority of science to justify a controversial political programme of action.

In Chapter 2 I criticised climate-concerned analysts for their asymmetrical approach to the climate debate (e.g., Oreskes & Conway, 2010). Historians and sociologists of science have ignored or brushed over the sceptics’ claims against the scientific consensus on the theory AGW as if they mattered little. What is more, their arguments against climate scepticism and the political economy of their science have been reversed by sceptical analysts who spot a political agenda behind mainstream climate science, as has been revealed in the ‘Climategate’ email disclosures. As a consequence, the focus has shifted from difficult scientific questions onto the scientists themselves, turning in principle esoteric scientific debates into discussions over the epistemic and normative markers of expertise, and eventually proposals for an extension of the body of relevant experts. As we have seen in Chapter 6, these are no safe epistemic places to stay and sceptics like Anthony Watts and Steven McIntyre continue to insist on the primacy of ‘sound science’.

I set up the substantial part of this thesis in opposition to the existing sociological accounts and followed David Bloor’s advice to remain agnostic about the truth value of science. But I also argued that one must take the scientific critique

seriously in order to understand why sceptics ultimately reject those analyses: they continue to dismiss any official accounts of ‘Climategate’ (such as IAC, 2010; Muir, 2010) for failing to address the remaining ‘hard’ scientific questions.

In Chapter 3 I began my symmetrical investigation with an examination of the idea of ‘sound science’ to which both the accused scientists, the sceptics, as well as their respective political protégées subscribe to. I then described the philosophies of ‘sound science’ and showed that in the hands of climate sceptics that idea is explicitly married to a logical empiricist philosophy: to sceptics any differences between a priori and a posteriori statements, real data and modelled data, and observations and theory are understood to be in kind, not in degree. And for them to be credible, empirically sound observations must be independently replicated. Contrary to the claims made by the critics of climate scepticism, these demands are anything but unreasonable *per se*. In fact, the scientists, who the historians and sociologists so passionately defend, subscribe to the very same principles.

The sceptics qua empiricists criticise computer experiments which most obviously defy these ideals. Climate modellers make no empirical observations in the strict sense but favour a theory-based approach to understanding the climate system, claiming their authority from first physical principles. In other words, modellers constitute the phenomenon of climate change quite differently from classical experimenters, some of who are climate sceptics. And these often highly credentialed contrarians are anything but crooks who know little about science; their critique on climate models is based on a serious philosophy of science (see also Pearce, 2013). But the existence of two epistemic approaches (classic and computer experiments) has complicated scientific closure and unfairly casts a negative light on anyone who dares to criticise climate models. And yet also within the epistemic community of classical experimentalists scientific closure has not been reached.

In Chapter 4 I examined the hockey stick as a classical, empirical test of the theory of AGW. From a scientific point of view, the study by climatologists Mann, Bradley and Hughes became interesting because of the sceptics’ insistence on empirical verification. If a critical observation in the form of climate reconstruction does not adhere to empiricist logic, it must not be used as evidence of AGW, it may even falsify the theory. And if the original hockey stick experiment cannot be independently replicated, it will have to be discarded, argue the doyens of ‘sound science’.

I first described the arduous scientific work that goes into reconstructing temperature changes on various spatial and temporal scales: assumptions have been made, data have been manipulated, and unknowns were accepted, whether prematurely or not, in order to arrive at global temperature series. In scientific practice, the differences between a priori and a posteriori statements, between real data and modelled data, and between observations and theory are always in degree, not in kind. At some point the scientist has to accept in principle synthetic a priori statements as sufficiently scientific. But to the sceptics this has been quite outrageous and they turned to the single observations of which there were thousands. Once again their insistence on logical empiricist ideals allowed them to find ever new bones of contention, making exact replication an impossible task. And yet the sceptics' experiments are not 'sound science' either.

Chapter 4 confirmed that the logical empiricist demands for purity cannot do justice to how science must necessarily work – strict adherence would make science impossible. As a consequence, the idea of 'sound science' is not adequate for arriving at policy consensus on climate, since agreement about climate science is to some degree itself conventional and so cannot be invoked to explain that consensus. And since the politics has infected the science of climate change – disagreement over their practical implementation of sound principles has offered an opportunity to smuggle value judgements into esoteric scientific debates about bristlecone pines, 'impure' weather stations and so forth – it is fruitless to look solely at facts to close off political debates about carbon taxes and energy policy.

Because the facts are never pure, expert reviewers are asked to find an authoritative consensus the scientists in the field can more or less agree with. For this reason 'Wave Two' sociologists of science have studied not endless methodological quarrels but the strategies by which experts form consensus and convince a lay audience of scientific closure. But by arguing for a normative theory of expertise to discriminate between real and non-experts, the sociologists Harry Collins and Robert Evans (2002) have simply substituted the irresolvable question over what the true evidence is with the no less difficult question over who the real experts are. In Chapter 5 I examined how hand in hand with the scientific debate over the purity of facts that boundary work was performed in several Congressional hearings, during which either side of the political divide participated in the drawing of boundaries around the

normative core of ‘proper’ climate expertise. Although Collins and Evans offer a new conceptual language in the analysis of scientific controversies, their markers of experienced-based expertise too become contested. Their normative theory cannot entirely eliminate the political either. It too will always fall short of a demand for purity as it simply adds another layer prone to deconstruction by the political opponent.

Eventually my protagonists acknowledged and agreed that although they guide sound scientific conduct, norms should not matter. Instead, uncompromising open-access policies was the proposed mechanism by which science would be re-authorised qua science. They so failed to notice that the demand for openness too is a particular norm, or culturally contingent tradition of policy legitimation (cf. Jasanoff, 2005), to which the sceptics’ favourite target, the IPCC, is not answerable. As an intergovernmental organisation the IPCC is not accountable to any national public, nor the FoIA. But instead of discussing these highly pertinent questions of knowledge politics, the goal of authorising science to achieve political closure once again attracted most attention.

In the final analysis chapter I asked whether an open and transparent science would resolve the controversy on climate reconstruction and pave the way for bipartisan action. To this end I presented two projects which had tackled scientific questions. With improved methodology and updated data, the PAGES 2k Consortium of dozens of climate scientists reconstructed global climate change over 2,000 years. They published their results in a science journal as a work in progress. Scientific closure, they acknowledged, is relative at best. In the BEST project on the other hand, highly credentialed physicists and ‘converted sceptic’ Richard Muller reconstructed temperatures since AD 1750, and in their view closure was achieved both scientifically and politically. Climate sceptics largely ignored the PAGES 2k Consortium – once again the principles of ‘sound science’ served to oppose expert consensus on their climate reconstruction – but zeroed in on BEST. Although BEST followed demands of openness and transparency, sceptics denounced its self-proclaimed scientific and political closure. Also, in the view of both sceptics and mainstream scientists, BEST’s attempt to get the epistemic and normative markers of expertise ‘right’ at once diminished the credibility of its facts. The authority of science would rest in its method, they agreed.

Because the debate between the convinced and the sceptics is political at its



heart, the opening up of climate science as PNS has not led to its resolution. The sceptics refuse to accept the science of climate reconstruction *because* it is used as legitimisation of public policy. They resist regulatory climate policy via the deconstruction of its ‘knowledge base’, whether normal or post-normal.

This finding supports a key argument made by analysts who dismissed the sceptics’ charges against AGW. But these historians and sociologists have arrived at that conclusion for the wrong reasons. If they expanded their analyses to BEST, they would have to dismiss Muller for his lack of formal training in climate science and for his political engagement. They would have to brush off his claims to expert knowledge since he has not yet published in the climate science literature. They would have to criticise BEST for being funded by the Koch brothers, who openly support the climate change denial machinery. They would have to expand this critique onto any science with links to for instance the renewable energy industry. And with the revelations of ‘Climategate’, they would have to dismiss the ‘hockey team’ for the violation of norms they hold to be binding upon scientists. But because their science can be brought into alignment with their wish for governmental intervention, these analysts do not attack BEST or the ‘hockey team’. They chose an a-symmetrical approach because it offers a convenient argument against their ideological opponents from the right.

Since it is perceived as a threat to rational policy-making, these analysts along with climate-concerned scientists fear that the symmetry principle and its agnosticism devalues the authority of their science in that process (cf. Dunlap & Catton, 1994). The deconstruction of so much climate science has only reinforced worries that such critique has been usurped by the wrong people, that it has run out of steam (Latour, 2004b). However, had not analysed scientific arguments agnostically, I would not have arrived at this thesis empirical part’s main conclusion. **Theoretical sociological claims about expertise and its role in the climate debate are practically inadequate, and no science, whether open or closed, is going to resolve the contentious politics of climate change.** In the climate debate, normative theories of expertise have failed to achieve their primary goal that is to bolster the authority of science in decision-making. Whilst Collins and Evans’ aim to demarcate political from technical questions and to authorise experts qua unchallenged experts (intentionally) failed to take into account the inherent politics of knowledge production (Jasanoff, 2003), Funtowicz & Ravetz’ attempt to open up that process to outsider participation (intentionally) deters those

boundary workers. It seems as if science has become so important to decision-makers in the US, attaining a quasi-religious status, that these debates over scientific knowledge production (and who is allowed to read and interpret its ‘scriptures’) inevitably pits purists and traditionalists against pragmatists and reformers.

Because there is no easy resolution to the debate of how knowledge should be produced (cf. Durant, 2011; Owens, 2011), following up on this thesis, one should study scientific controversies using different conceptual frameworks. For example, in a comparative case study (Yin, 2014) one should ask what political institutions and mechanisms have allowed climate scepticism and scientific controversies about climate reconstruction to rise to such prominence. Using the concept of the co-production of science and social order (Jasanoff, 2004), one should inquire into the relationships between politics, interest groups, and science in other democracies where climate policies are on the political agenda (e.g., Mahony, 2013). The analyst would include to ‘scientists constrained to attempt to do normal science in a post-normal situation’ the ‘normal’ and constraining political cultures as explanatory variable. The analysis of scientific controversies and changing knowledge production regimes would go hand-in-hand with a critical examination of the premise underlying science-based climate policy as ideology, and how it serves to mask the value-laden character of such decision-making in the service of the status quo – to what end should publics participate in science? The examination of an esoteric scientific controversy would open up a whole lot of other questions which have more to do with politics than with science (cf. Healy, 1999; Demeritt, 2006; Irwin, 2006; Levidow, 2007).

This thesis already contains an answer to the first question (What political institutions and mechanisms have allowed climate scepticism and scientific controversies about climate reconstruction to rise to such prominence), but since much of the data came from the US, my conclusions speak almost exclusively to that specific political context. In fact, the title of this thesis could have been “Climate reconstruction and the making of authoritative scientific knowledge in the US,” since in the US more so than anywhere else climate reconstruction became a public domain science. The selection of data, biased by my personal fascination with climate history, certainly influenced, if not limited, the explanatory power of my thesis. If I had decided to study “Climate modelling and the making of authoritative scientific knowledge,” I would have focused on different characters or blogs. Importantly, I would have included a

range of UK Parliamentary hearings which would have added a *comparative* political dimension.

In the next section I reflect on how the US culture of adversarialism, which is mirrored in the Congressional hearings, and the organised scepticism of special interest groups have generated controversy and provided a fertile soil for climate scepticism. And because the US science-sceptical discourse gives support to similar debates also in other countries (e.g., Klaus, 2006; Lawson, 2009; Plimer, 2009; Vahrenholt, 2012), I draw some tentative conclusions of how ‘Climategate’ may influence climate policy-making in Europe where decision-making over similar risks used to look differently.

### ***7.2 After ‘Climategate’: The Americanisation of the climate debate***

The climate change controversy is typical for how US American society arrives at public truth fit for decision-making (Jasanoff, 2005); it epitomises US civic epistemology. Three observations support this claim.

First, the recourse to science in difficult political decisions characterises debates over governmental regulations in the US. Such an approach to decision-making has great normative force, noted Jasanoff (2005: 265), since “[i]t allows governing bodies to claim the cognitive high ground, a place from which they can be seen to be acting for the benefit of all without bowing to any particular interests of the governed.” Sometimes science has indeed been indispensable to the description of risks and in the search for solutions, the hole in the earth’s ozone layer serving as the prime example of science-based decision-making (Grundmann, 2001; 2005; Parson, 2002). But the scientisation of a great host of not primarily technical decisions has provoked a politicisation of science by special interest groups, in turn leading to the politicians’ notorious insistence on ‘sound science’. And while many requirements for ‘sound science’ have been written into US legislation on scientific risk assessment, leading to high and unreasonable expectations about the role it can play in the development of public policies (Herrick, 2004), a similar discourse is notably absent from the UK Parliamentary hearing on ‘Climategate’.

Second, the requirement for openness and transparency paired with a distrust in experts and regulators is diagnostically US American, and the findings herein, for

example in the politicians' pledges for an open and transparent Red Team, support that notion. The UK on the other hand has historically favoured a closed-door approach to regulation (cf. Brickmann *et al* 1985; O'Riordan, 1985, Löfstedt & Vogel, 2001), but at least since the BSE scandal also UK publics have been keeping a zealous watch over the government-expert relationship, forcing government officials to rebuild expert credibility through new institutional forms, such as public participation exercises (Jasanoff 2005: 261). And with 'Climategate' that erosion of trust in regulators continues. Speaking to that erosion of trust and echoing those in the US are calls for more openness and transparency across all levels of regulatory decision-making: "The American experience is instructive here," concluded the Independent Climate Change Emails Review (Muir, 2010: 94). But openness and transparency makes public policies all the more precarious as it provides opportunities for its opponents to undermine the policy by deconstructing the science.

Third, whereas Jasanoff describes Congressional hearings on science as an mechanism by which scientists are held to account – the possibility of subpoenaing testimony should discourage scientific foul play, – my study shows them as an instrument politicians use in order to seek the authority of experts to (de)legitimise controversial public policy. The Democrats have convened hearings and invited 'their' experts hoping that a scientific consensus would legitimise their policy proposals. In turn the Republicans have convened hearings to which they invited those experts who issue statements questioning respective consensus. Once again this happened in hearings on climate science convened by the Republicans James Inhofe, Ed Whitfield and Joe Barton. Seizing the Republican majority in both chambers, their hearings on climate history and the hockey stick study functioned as a veto put on a legislative process that has been facing resistance long before climate reconstruction attracted politicians' attention (see, e.g., Byrd-Hagel, 1997).

Subpoenaing testimony from intimidated scientists in order to influence the policy process has proven inefficacious at best, since neither party takes the opponent experts' rare advice seriously, and counter-productive at worst, since it reinforces the stalemate between Democrats and Republicans. And arguably the purpose of Congressional hearings is anything but to expand or clarify the scope of choice available to decision makers, nor to convince neutrals or to win over the other side to one's point of view, but to show and confirm solidarity with one's own side (Kahan,

2010; Kahan *et al* 2011). Similar dynamics can be observed in climate science: instead of deliberating in an open discourse with the scientific community, the scientists asked for solidarity. The Congressional discussions over the breakdown of the scientific scepticism so masks that breakdown of democratic deliberation. By comparison, when the UK Parliamentary seeks scientific advice controversy within science is given less attention. And rarely do politicians try to catch out the witnesses with ‘gotcha’ questions, since other than in the US in the UK the election of expert witnesses is a consensual decision.

Although the happenings surrounding the hockey stick controversy are typical of the US, ‘Climategate’ also speaks to the changing character of European regulatory regimes in which science has become ‘more and more important’ but at the same time ‘less and less sufficient’ as trust in institutional authority and codified expertise declines, i.e., the scientisation of environmental politics in the EU has begun to mirror affairs in the US (cf. Löfsted & Vogel, 2001). In the cross-Atlantic discourse of ecological modernisation (Thatcher, 1990; Bush, 1992), which sees environmental protection and economic development as mutually reinforcing goals and regards scientific expertise as the key to environmental progress, debates over the authority of climate science are likely to continue. These scientific controversies are being connected to technical debates over the (global) environmental impact and the costs and benefits of carbon-based life. Two years after the watersheds of ‘Climategate’ and Copenhagen, at the COP17 in South Africa’s Durban, Ban Ki-Moon, Secretary-General of the UN, once again urged carbon-technocrats to act on the basis of science (cited in Hulme, 2012: 2):

It would be difficult to overstate the gravity of this moment. Without exaggeration, we can say: the future of our planet is at stake. The science is clear. The WMO has reported that carbon emissions are at their highest in history and rising. The IPCC tells us, unequivocally, that greenhouse gas emissions must be reduced by half by 2050 – if we are to keep the rise in global temperatures to 2 degrees [Celsius] since pre-industrial times. You are the people who can bring us from the edge. The world is looking to you for leadership.

This very modern idea, born in Europe, perfected in the US, and reiterated at every international climate conference, is at the heart of the ‘Climategate’ affair. The final section of the conclusion wraps up the findings and speaks in support a new direction of climate policy (Prins *et al* 2010) which puts less burden on science to deliver the undeniable truth.

### ***7.3 The breakdown of orthodox climate policy***

Following in the footsteps of the Montreal Protocol on the Depletion of the Earth's Ozone Layer, scientific evidence of climate change has provided policy-makers with an authoritative rationale to act on global warming primarily via the reduction of GHG emissions prescribed in an international treaty. Viewed as science-based public policy issue, the reduction of GHG emissions became a business-style management exercise in which nation states like companies have to work towards a fixed target – it beckons after the successful completion of the task a nice bonus in the form of climate stability. With the Kyoto Protocol, describing a mix of governmental regulation and market-based solutions, scientific evidence would be brought into alignment with politically and economically viable options (e.g., Stern, 2006; EPA, 2009a, 2009c).

Politicians typically weigh the scientific evidence and their policy options against a potential loss or gain of salience, credibility, legitimacy, economic, administrative and political viability (cf. Hall, 1989). For example, in the GMO debate the legitimacy of public opposition to the food's underlying political economy had to be acknowledged and has, for the time being, kept genetically modified food out of European supermarkets. In the MMR debate UK health authorities seem unimpressed by irreducible scientific uncertainties surrounding the vaccine's safety. Authorities are much more concerned with the economic and public health costs a measles outbreak entails, as well as with the associated loss of credibility. In the tobacco smoke debate, which was about indoor air quality as much as nicotine's stimulative and disruptive effect on neurons, Democrats recognised the political viability of a smoking ban.

In the case of climate change it initially seemed as if the science could be brought into alignment with a new green economical spirit. In the 1990s climate change may indeed have filled the vacuum which the evaporated cold-war nuclear threat left behind (Ross, 1991). And during (or maybe because of) the emerging war on terror, in the early 2000s, political leaders found in it an opportunity to re-position themselves and to do good: Heavily criticised for his engagement in the war in Iraq, Tony Blair took up climate change arguably in order to regain credibility among the left. For Al Gore climate change even became a personal matter, which should eventually bring him and the IPCC the Nobel Peace Prize – a very modern recognition indeed. Also Hollywood celebrities rallied for action on the 'most important issue of our days'. These were the times in which the hockey stick graph became an icon for climate

activists, many of which express a genuine fear of a coming catastrophe.

Once it became clear that the only ways by which the Kyoto targets would be achieved were rigid self-discipline and coercion in the form of carbon taxes, the underlying ideological differences came to the fore. Unsurprisingly, most vocal opposition against the potentially far-reaching grasp of the ‘Climate Leviathan’ has come from conservative quarters of society, as climate change would “provide a marvellous excuse for worldwide, supra-national socialism,” noted Margaret Thatcher (2002: 449). Other than opposition to policies based on capitalist science, for instance the commercialisation of GMOs, opposition to policies based on climate science became popular among the right-leaning electorate. As a consequence, in the US more so than anywhere else climate change politics became primarily understood in the context of the Republican-Democrat opposition. But because the ideological trenches Republican climate sceptics and their political opponents dug themselves into were very deep, the only rational solution, or so both sides thought, would be science-based. It is thus little remarkable that a Republican together with a former Democrat introduced the Climate Stewardship Act – it too pretends to be based and legitimated by the authority of science.

Since Kyoto came into force the impracticability of large-scale emission reductions within the capitalist political economy, notably its addiction to economic growth (Pielke, Jr., 2011), and the geopolitical stalemate between industrialised and industrialising nations, notably between the US and China, have presented a sobering reality for international and national political negotiators. Over the years several nations and big emitters have pulled out of the Kyoto Protocol. Russia, for instance, has not joined the second phase of the Kyoto Protocol, which started in 2013. It decided to discontinue its participation because the US, China but also India as the world’s major emitters of GHGs still refuse to commit themselves to reduce their emissions. For the latter the gross inequity in patterns of development serves as a compelling argument against a treaty that favours the historically biggest emitters. For many of these reasons the many parties at the COP15 failed to reach a binding agreement. Quite evidently, in Copenhagen the science of climate change could not be brought into alignment with those wider, conflicting political and economic rationales. That plan did not work (Sarewitz, 2011).

But even if science can formally be made compatible with powerful economic

interests (Stern, 2006), a climate policy based on an ideology-free, God-like gaze upon the world runs into many more problems as it continues to meet the always situated and local investments societies have been making for decades and centuries (cf. Jasanoff, 2010). Ever more issues troubling the world have been woven into the tangled knot called climate change politics, observe Prins *et al* (2010): “the loss of biodiversity, the gross inequity in patterns of development, degradation of tropical forests, trade restrictions, violation of the rights of indigenous peoples, intellectual property rights [...].” Most of all, climate change is an opportunity for the global South to remind the North that capitalism destroys their livelihoods as dirty industries and emissions are simply outsourced, leading to further material insecurity in the global South. Less than half a year after Copenhagen, at the World People’s Conference on Climate Change in 2010, the People’s Agreement of Cochabamba criticised the political economy head on:

The capitalist system has imposed on us a logic of competition, progress and limitless growth. This regime of production and consumption seeks profit without limits, separating human beings from nature and imposing a logic of domination upon nature, transforming everything into commodities: water, earth, the human genome, ancestral cultures, biodiversity, justice, ethics, the rights of peoples, and life itself. Under capitalism, Mother Earth is converted into a source of raw materials, and human beings into consumers and a means of production, into people that are seen as valuable only for what they own, and not for what they are.<sup>41</sup>

Disagreement about climate change is intimately connected to these ideologies and values we hold dear and want to defend, the things we believe in, the risks we face from environmental threats, the ways we govern on regional, national and increasingly global scales, and the ways we envision future life on this planet (Hulme, 2009). As ‘wicked problem’ climate change evades a silver bullet solution, argue Rayner and Prins (2007). Several scholars instead suggest pragmatic no regrets policies which all involved parties can in principle agree on (Prins *et al* 2010). These policies do not tackle global warming directly via emission reduction targets. Environmental and human health benefits rather emerge as positive side-effect of policies dealing primarily with energy security and the modernisation of inefficient energy producers. In this view climate policy should be connected with established institutions and forms of decision-making, for instance with national health policy-making. A focus on health and energy security also aligns with the humanitarian ambitions of some climate sceptics.

The advantage of this approach to climate change is that policies do not hinge

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41 <http://pwccc.wordpress.com/2010/04/24/peoples-agreement/> (Accessed on 19 March 2013)



on scientific truth; they do not provoke endless deconstruction exercises and debates about the authority of science and scientific experts. In fact they allow the science to reclaim its right to be wrong. And whether this conclusion has been arrived at for the wrong reasons – sceptics might claim that they were correct in their opposition to climate science – does not matter a whole lot. Such policies are an advancement over the stalemate a orthodox and party-political driven approach to climate policy has reinforced, at least in the US.

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