The global warming of climate science: Climategate and the construction of scientific facts

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Abstract

This article analyzes 1073 emails that were hacked from the Climatic Research Unit (CRU) at East Anglia University in November, 2009. The content of the emails have been heavily debated in the public sphere, where it has been dubbed "Climate Gate" – indicating that the emails reveal a scientific scandal. In this article we analyze the emails differently. Rather than considering the email exchanges objectionable on the basis of some idea about proper scientific conduct, we see them as providing a very rare glimpse into a situation where a group of scientists collectively prepare for participation in a heated controversy, and with much focus on methodological issues. This approach allows us to study the practice of prominent scientists when they communicate informally about how to frame propositions of facts in the best possible way, preparing for the possibility that their papers will be subjected to what Collins calls experimenter's regress. Through the eyes of Science and Technology studies (STS) the emails provide an opportunity to study communication as part of science in the making across disciplines and laboratories across the world. Analysed as "written conversation" the emails provide information about processes of consensus formation through 'agonistic evaluations' of other scientists work and efforts to persuade others to support ones own work. Also, the emails contain judgements about other groups or the work of individual scientists. Consensus-forming appeared as a precarious activity because of considerable diversity among core group scientists. Controversies could be quite resilient in the course of this decade-long exchange, probably reflecting the complexity of the methodological challenges involved.

Introduction

In November, 2009 reports surfaced that hackers had accessed and released numerous emails involving some of the worlds leading climate scientists from the Climatic Research Unit (CRU) at East Anglia University (EAU). The initial stories focussed on the attack, but also on the content of a few emails. The Times reported: ‘E-mails allegedly written by some of the world’s leading climate scientists have been stolen by hackers and published on websites run by climate change sceptics. The sceptics claim that the e-mails are evidence that scientists manipulated data’.[1]

Many newspapers quoted directly from the emails. The Telegraph reported: ‘One email seized upon by sceptics as supposed evidence of this, refers to a “trick” being employed to massage temperature statistics to ‘hide the decline’’.[2] The Financial Times similarly claimed that: ‘One e-mail from Professor Phil Jones (…) spoke of using a ‘trick’ to hide ‘the decline’ of temperatures’.[3] Over the following weeks, controversy raged in both mainstream media and more informal internet realms. James Delingpole asserted on his blog hosted by The Telegraph that this was ‘The final nail in the coffin of ‘Anthropogenic Global Warming’’[4] while columnist Christopher Booker in the same newspaper declared that this was ‘The worst scientific scandal of our generation’.[5] Jonathan Leake, Environment Editor of the Times made his sentiments clear. Under the headline ‘The great climate change science scandal’ he wrote: ‘those emails suggested (…) that Jones and some colleagues may have become so convinced of their case that they crossed the line from objective research into active campaigning’.[6]

Phil Jones, director of CRU, posted a reply on CRU’s website,[7] but the critics were not silenced. A week later Jones temporarily resigned, pending the results of an independent review.[8] In the following weeks more voices entered the debate, nuancing the implications of the content of the emails. Some claimed the sceptics had embarked on a ‘tabloid-style character assassination’,[9] while a story in the Telegraph depicted the scientists as having made ‘an error of judgement’.[10]

In this paper, we pursue a different agenda. Rather than considering the email exchanges objectionable on the basis of some idea about proper scientific conduct, we see them as providing a very rare glimpse into a situation where a group of scientists collectively prepare for participation in a heated controversy. This allows us to study the practice of prominent scientists when they communicate informally about how to frame propositions of facts in the best possible way. In particular, the emails provide an opportunity to study communication as part of science in the making across disciplines and laboratories across the world.

Such research questions are also fuelled by the need to provide more realistic representations of how scientists work than those that seems to underpin news media’s interpretation of the emails:

‘If the public could be helped to understand how scientific knowledge is generated and could understand that it is comprehensible and no more extraordinary than any other field of endeavour, they would not expect more of scientists that they are capable of delivering, nor would they fear scientists as much as they do. (…). It is sometimes discouraging that although we dedicate our lives to the extension of knowledge, to shedding light and exemplifying rationality in the world, the work of individual scientists, or the work of scientists in general, is often understood only in a sort of magical or mystical way’ (Jonas Salk in his Introduction to Latour and Woolgar 1979, p. 14).

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[1] “Sceptics publish climate e-mails ‘stolen from East Anglia University’”, 21.11.2009
[2] “Climate scientists accused of ‘manipulating global warming data’”, 21.11.2009
[3] “E-mail tirade boosts climate sceptics’ cause”, 23.11.2009
[4] 20.11.2009
[5] 28.11.2009
[6] 29.11.2009
[7] http://www.uea.ac.uk/mac/comm/media/press/2009/nov/CRUupdate, 24.11.2009
[8] http://www.uea.ac.uk/mac/comm/media/press/2009/dec/CRUphiljones, 01.12.2009
[9] Bbc.co.uk, 04.12.2009
[10] 03.12.2009

According to the news media coverage, the emails clearly represent disenchantment with respect to widespread understandings of scientific practice. When we think otherwise, we are partly drawing on rich contributions from the field of Science and Technology studies, in particular ethnographies of scientists at work, but also because the reading of the emails as a continuous correspondence rather than selective quotes give a different impression. Thus, we shall analyse the emails to examine what they tell about the way scientists informally may discuss their work, with particular focus on issues regarding measurements, analysis and interpretation. We leave any normative assessment of the communication to the readers.

Understanding the CRU-controversy from a science and technology studies perspective

Science and technology studies (STS) researchers have analysed the 'hard core' of scientific work; its technical content and production of knowledge (Knorr Certina 1995: pp. 140). In *Laboratory Life*, a detailed ethnographic study of the making of scientific knowledge, Latour and Woolgar (1979) demonstrated that 'elimination of alternative interpretations of scientific data and the rendering of these alternatives as less plausible' (p. 36) is a central scientific activity. They describe the contours of scientific practice as: 'a tribe of readers and writers who spend two-thirds of their time working with large inscription devices. They appear to have developed considerable skills in setting up devices which can pin down elusive figures, traces, or inscriptions in their craftwork, and in the art of persuasion. The latter skill enables them to convince others that what they do is important, that what they say is true and that their proposals are worth funding. They are so skilful, indeed, that they manage to convince others not that they are not being convinced but that they simply follow a consistent line of interpretation of available evidence' (pp.70). Thus, fact-construction relies on the persuasive skills of scientists, as also noted in Traweek (1988) and Lynch (1985).

A central persuasive activity is the production of papers. Laboratory work is described as 'constantly performing operations on statements; adding modalities, citing, enhancing, diminishing, borrowing, and proposing new combinations', resulting in new statements being different or merely qualified, providing the focus for similar operations in other laboratories, regularly observing how their own assertions were 'rejected, borrowed, quoted, ignored, confirmed, or dissolved by others' (Latour and Woolgar p.87). Their observations have been supported by subsequent work. A later wave of science studies by symbolic interactionists also noted that it was not only the outcomes of science and their interpretations that were negotiable, but also the process of investigation itself (e.g. Clarke 1987, Fujimura 1996, Star, 1986). Further, as noted by Knorr-Certina (1981: pp. 5) the products of science are contextually specific constructions marked by the situational contingency and interest structure of the process by which they are generated, which cannot be adequately understood without an analysis of their construction. Thus, laboratory studies have showed that technical objects are not only "technically" manufactured in laboratories, both also inextricably symbolically and politically constructed, for instance through the literary techniques of persuasion embodied in scientific papers, through political tactics of alliance formation or resource mobilisation by scientists, or through the selections and decision translation that build scientific knowledge "from within" (Knorr Certina 1995: 143).

Controversies are windows of opportunity to study facts in the making (Knorr Certina 1995: 140), and such studies show how scientific knowledge claims may face deconstruction (Yearly 1995: 465). An important example is Collins' (1992) phenomenon of experimenter's regress which he develops from observations that controversies rarely are settled by experiments alone. It seems always possible to challenge an opponent's experimental procedure in order to explain why the "wrong" result had been achieved because there are no formal criteria independent of experiments' outcome that can be applied to decide whether an experiment works properly or not. Thus, controversy about scientific facts tend to become controversy about method, and the phenomenon of experimenter's regress means that it is important not only to do good experiments but to be perceived to have proper methods and to be skilled in applying them. In turn, this means that it is vital to present one's methods in a persuasive way.

In a similar vain, when studying the infanticide controversy Rees (2009) found that fieldworkers are constantly confronted with what she calls the fieldworker's regress. From the beginning of the process fieldworkers recognise and discuss contingent questions as potential problems. Rees claims that the fieldworker's regress is the default state for field scientists – a constant state of interpretive flexibility perpetually provoked by the structure of the field space.

On this basis, we expect to find deliberations with respect to methodology to be a central aspect of the CRU email material because of the need to be prepared for experimenter's or fieldworker's regress. The laboratory ethnographies of Latour and Wolgaar, Knorr-Cetina, Traweek and others have demonstrated that conversations among practising scientists represent a fruitful source of data to understand important aspects of the making of scientific facts. The CRU emails, as "written conversation" could be expected to illuminate how climate scientists collectively reflect on methodological issues as well as rhetorical strategies in order to establish findings as facts, and how they constantly perform semiotical operations on each others statements. In this manner, the CRU emails represent a new type of data that has been called for (e.g., by Knorr Certina 1995) to allow the study of consensus formation. This would normally involve many laboratories; potentially an entire scientific field (e.g., Pinch 1986: 30). Further, it has been noted that the focus on intramural lifeworlds of laboratories ignores the contexts in which laboratories operate and the political aspects of science (e.g. Fuller 1992). The CRU email data allows us to analyse strategies of consensus formation involving many research environments, in a situation where scientists are particularly wary about the heated context of scientific exchanges.

In her study of high energy physicists, Traweek (1988: 117) discovered a distinction between restricted information, which was spoken, and public information, which was written. When talking, the physicists were found to evaluate their peers and their efforts, and to persuade the same peers to support their own work. Traweek noticed that this practise of "agonistic evaluations" of other physicists' work and enticing others to corroborate one's own data was in stark contrast to the traditional image of objective, neutral decision-making in science. The physicists also discussed the possibility of ignoring some colleagues' work and could refer to scientific groups as "dead wood" (p. 117). Traweek noted that access to this world of oral communication is limited and that disclosing oral information to outsiders is regarded as unacceptable. Increased use of email as a communicative tool has probably changed this by blurring the boundaries between written and spoken information. When Traweek's physicists dealt with uncertainties, they could make a phone call, they spoke together, and the results were often strategically important decisions. These were never documented in written form, as is now possible because of the way emails dominate as a communication channel. To what extent do the CRU emails resemble the oral communication observed by Traweek? Do the emails allow us to overcome the drawback of previous analyses of scientists' literary and rhetorical constructions, as pointed out by Knorr Certina (1995: 156): 'While we have an elaborate picture of the scientist as an author and writer, we still lack systematic analysis of how scientific texts and technical images are decoded by an audience and the deconstruction practitioners themselves routinely perform on images and texts'? Do the emails provide information on the construction of representational techniques of persuasion and on how they are read and decoded by other scientists in other places?

Climate science may be seen to differ from experimental research in high energy physics, endocrinology, molecular biology and biochemistry, which has been the main focus of STS laboratory studies. However, there are studies emphasizing how climate science knowledge is produced through climate models and how this knowledge is rendered credible and useful for the policy community (Edwards, 1996; 2000, 2001; Shackley et al., 1988, 1999, var der Sluijs et al., 1998, Lahsen 2005b, Sundberg 2005). This body of literature argues that sociological factors specific to the climate modelling community influenced the knowledge production in direction of consensus and continuity. For instance, van der Sluijs et al. (1998) show how ambiguity over the meaning and application of "climate sensitivity" does not obstruct the use of the concept, but facilitates an emerging common community of climate researchers. Experts have to negotiate support and credibility for their assessment reports both with scientific peers and policy audiences, while translating the scientific knowledge into a form appropriate for policy makers while keeping the support of surrounding research communities. It will be interesting to see whether we can detect similar consensus building anchoring devices in the current discussions among climate scientists.

Previous science studies of climate science have also revealed contrasting 'epistemic lifestyles' within the modelling community (Shackely 2001, Sundberg 2005, 2009). These differences are linked to the research funding system (P. Edwards 1995, Bloomfield 1986, see also Yearley 2008 for an overview). Of particular interest to us is Shackley and Wynne's (1995: 113) study which explored what they described as 'the sometimes terse relationship' between climate modellers and the climate impact community.

In this paper, we analyse the CRU emails to see what they tell us about the making of climate science knowledge. As we have learnt from previous research, as part of their daily work scientists discuss and assess procedures and findings, often in an informal tone. We assume that a large bulk of the emails is concerned with how to prepare to engage in controversies. What is involved in these preparations? To what extent is this about getting ready to counter efforts at experimenter's or fieldworker's regress and to what extent do the emails give evidence about more purely political engagement?

Methodology

The material hacked from CRU consists of 1073 text files containing e-mails, and nearly 3500 other documents. The first e-mails are from March 1996; the last was sent on November 12, 2009. We have analysed the 1073 emails but have not dealt with the remaining material. We accessed the emails online.[11] The material appears to be genuine,[12] but CRU has not confirmed the authenticity of *all* the emails.

We read the emails chronologically, while qualitatively coding the content along a number of central themes which emerged during the reading. These include "personal discussions", "discussions on research grants/applications", "general requests for comments on drafts, for texts or for data", "scheduling meetings/workshops and discussions on meetings/workshops", "politics of publication", "discussions on methodology", "discussions on presentation", and "discussions with or about sceptics". For our purposes, the three latter categories, containing roughly 480 emails, are the most interesting ones. They allow inquiry

[11] The website used was http://www.eastangliaemails.com/. We cross-checked some of the material with other websites, for instance http://climate-gate.com/.

[12] Official statements from CRU and UEA uphold that at least some of the material is real, for example in a press release from November 24, 2009. See http://www.uea.ac.uk/mac/comm/media/press/2009/nov/CRUupdate. Pensylvania state university has carried out an inquiry regarding Michael Mann's role in the leaked emails. The inquiry report says nothing about the authenticity of the emails, but the methodology mobilized suggests that those involving Michael Mann are indeed real. See http://www.research.psu.edu/orp/Findings_Mann_Inquiry.pdf

into methodology and presentation as scientific practice. To study exchanges about such issues is a key to understand the dynamics of science, and the data provides a unique insight into how a large scientific network negotiates such issues. We have approached the emails with an open mind, guided by the principle of methodological relativism to look at how scientific facts emerge as feats of consensus building (Collins 1981b; Demeritt 2001; 2006).

Many of the emails are concerned with fairly mundane issues, less interesting from our perspective. The main exception is the set of emails that we classified as "politics of publication", concerned with journals' policies and peer review processes. These emails have been given considerable attention in the media, but we believe that the emails informing about the formation of consensus and consensus-making strategies are more interesting and pertinent. Moreover, a scholarly study of the politics of publication would require a different approach than the one presented in this paper.

Using hacked emails as data poses several ethical dilemmas. First, should such data really be used? When we have chosen to do so, it is because the emails have been extensively quoted by news media, but also because we interpret them differently and in a way that counters the marginalisation efforts of news media. Second, the authors did not write the material with publication in mind; often, they use a seemingly un-scientific, oral style of language. Some emails are explicitly labelled "confidential". We have chosen not to quote such emails and other information of a more personal character. We have not made the participants in the exchanges anonymous, since many emails have been widely circulated and also because it would be easy to find out whom they are.

An outspoken goal of climate science is to reconstruct our planets climatic history, explain its changes over time and to predict the climatic future (IPCC 2007). However, originally the term climate science was a blanket label for a number of interconnected disciplines, including meteorology, oceanography, glaciology, some aspects of geography, and distinctive categories of earth sciences (Bray and Storch 1999). During the past two decades, there has been a movement towards a unified climate science as dynamical models of the atmosphere and ocean matured, global analysis techniques developed, conventional and remotely sensed data became available, and data about ice cores, tree rings, lake sediments, etc. became accessible. Today, thousands of scientists are involved in climate science work. Demeritt (2006) uses the work behind the "hockey stick" reconstruction as a case in point:

'Behind the hockey-stick graph stands the congealed labour of hundreds, perhaps even thousands, of scientists involved in assembling, manipulating, and transforming various recalcitrant materials annually laid down in tree ring and ice cores, laminated lake and ocean sediments, and ocean corals' (p.461).

This suggests that a large number of scientists is involved, but also that many methodologies are employed – methods potentially producing different types of data. A challenge for the scientists illustrated by the leaked e-mails is the establishment of associations between different methodologies and data so that they tell a meaningful story. This paves the way for two types of discussions. First, there are many examples of discussions reflecting on the relationship between the "core group" (Collins 1981) of climate scientists and the surrounding world. These discussions should be expected to be concerned with strategic positioning, a type of preparation for battle, and a deliberate insider/outsider demarcation which has been hidden from empirical scrutiny. The first part of our analysis consists of a study of emails focusing on methodological and rhetorical preparation for controversy. The second type of discussion is debates inside "the core group" dealing with insecurities related to methodological choices and scientific practices. We study the characteristics of such deliberations.

Preparing for controversy

As we expected, many – nearly half of the emails – focused on methodological issues, including concerns about how to frame findings in a persuasive way. A large part of these exchanges could be considered a kind of preparation to participate in fairly heated scientific debates: How to avoid being subjected to experimenter's regress, how to win trials of strength of arguments (Latour 1987)?

Many of the scientists whose emails had been hacked, are paleo climatologists who produce climatic reconstructions. Different types of data can be applied in this process; instrumental records date back to around 1850, but earlier climatic signals must be found in proxies, which are sources of uncertainty. How can scientists know that proxy information translates well to the phenomena they study? Paleoclimatic reconstructions involve dendro climatologists interpreting tree rings (Fritts 1976), ice core climatologists studying the presence of atmospheric gasses trapped in core samples (Alley 2000), borehole climatologists extracting borehole temperature profiles from subterranean rock (Gonzáles-Rouco et al. 2009), coral climatologists using isotopes in coral skeletons (Fairbanks et al. 1997), and marine climatologists examining lake and ocean sediments (IPCC 2007).

Thus, the core group is diverse. Still, the material shows that in relation to outsiders, they are often able to act as one. Michael Mann, Director of the Earth System Science Center at Penn State University, attributes this to a "common denominator" bridging many of the internal differences:

'As one of the lead authors on the 'observed climate variation and change' chapter for the 3rd assessment report, a key goal of mine will be to present fairly and accurately all of our different efforts, and the common denominator amongst them ...'.[13]

Raymond S. Bradley, director of the Climate Systems Research Center, University of Massachusetts Amherst, expressed similar sentiments to a group of colleagues when he stated that: 'Science moves forward whether we agree with individual articles or not....'[14] The science spoken of here refers to the science produced by members of one core group, which is seen as different from attempts by outsiders "invading" the field. In the material we see how this core group works to demarcate themselves from others, often perceived as common enemies of "honest science". This process involves extensive reflection about own practice and the perception of their work by others. Keith Briffa, professor at CRU, wrote to a number of colleagues, stressing that the worlds eyes are on them, and that they must thread gently:

'… we should be very wary of seeming to dam certain proxies and over hype 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 value, 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'.[15]

The quote provides an impression of how the core group perceives the relationship between itself and the world around. The outside world is unable to understand the finer points of climate science, and its insecurities. This is seen as cause for concern. Thus: elements outside the core

[13] 17.09.1998
[14] 19.04.1999
[15] 06.10.1998

group influence its practice. Latour and Woolgar (1979) points out that circumstance traditionally and popularly is seen as having no effect on science. The quote above indicates, however, in line with Knorr Cetina (1995), that the products of science are contextually particular constructions characterised by the situational contingency and interest structure of the process by which they are produced.

The discussions about "us" and "them" often revolve around methodology, or about how outsiders have failed at addressing the insecurities related to methodology. Thus, questions related to the status of different "facts" are often settled through constructing authoritative statements about the lack of methodological skill of individual protagonists of particular statements, in such a way that they are left as *unequally* probable statements compared to core group-statements, as earlier illustrated by Latour and Woolgar (1979). Another example of this is Michael Mann's reply after being pointed in the direction of solar physicist Douglas Hoyts website, where Hoyt argues that there is no evidence for warming during the last half of this century, relative to the last millennium. Mann replied:

'I checked out that page and, unfortunately what he has done is *so* ridden with problems that it isn't even worth confronting. Many of us (e.g., me, Phil Jones, Henry Pollack, Shao-Yang Huang, Rob Harris, and others) have been scratching our heads trying to find a statistically defensible way of combining the information in boreholes and "conventional" proxy indicators, and as yet it is not clear if it can be done (…). I don't think Hoyt has added anything scientifically productive in this regard. Looks more like he has recklessly convoluted borehole data with our reconstructions to get us the kind of result he wants to get...'.[16]

In other words, the statements are dismissed, not only because of their content, but with reference to flaws in the authors' methodology, and the fact that several skilled core group scientists have tried to do the same without success. This lends *credibility* to Mann's statement (Latour and Woolgar 1979) and retracts credibility from Hoyt's statement. The dynamics of experimenter's regress (Collins 1992) clearly become visible. There is no agreed-upon outcome ('[we]...have been scratching our heads'), but the results provided by Hoyt is dismissed as he is being perceived as incompetent and his results are dismissed on methodological reasons. In this case, the inside/outside dynamic of the core group is highlighted by the fact that Hoyts refuted statement is a non-peer reviewed piece on the web. There are, however, a number of examples where the "core group" uses this strategy to debunk peer-reviewed literature.

In a lengthy e-mail to Glenn McGregor, editor of the International journal of Climatology about an "addendum" to a paper written by a group of "sceptics" (Douglas, Christy, Pearson and Singer), Ben Santer explicitly links what he perceives as flawed methodology to the character of the individual scientists in question:

'The Douglass et al. 'Addendum' does nothing to clarify the serious statistical flaws in their paper. Their conclusion - that modelled and observed upper air trends are inconsistent - is simply wrong. As we point out in our paper, Douglass et al. reach this incorrect conclusion by ignoring uncertainties in observed and modelled upper air trends arising from interannual variability, and by applying a completely inappropriate 'consistency test' (…) The 'Addendum' does not suggest that the authors are capable of recognizing or understanding the errors inherent in either their 'experimental method' or their 'consistency test''.[17]

The quote illustrates how climate scientists, like other scientists (Latour and Woolgar 1979, Collins 1992), eliminate alternative interpretations of scientific data and renders these

[16] 03.08.1999
[17] 25.04.2008

alternatives as less plausible by highlighting methodological flaws and expressing doubt about the authors' methodological skills. It is not only the outcomes of science and their interpretations that were negotiable, but also the process of investigation itself (e.g. Clarke 1987). In this case, Douglass et al. did not present their methods persuasively enough to be accepted by the core group. The email excerpt to some extent also demonstrates how linking the quality of scientific production to the individual researcher is used as a strategy to separate outsiders from "insiders" by weeding out what is seen not only as poor "science", but also as bad scientists.

Thus, in preparing for controversy, it is as important to use experimenter's regress to undo rival research as it is to avoid being subjected to similar regress. Perhaps the latter is unavoidable; at least the email exchanges suggest a widespread understanding that the best defence strategy is to criticize the other party. There a less evidence that they engage in positive translation strategies (Latour 1987), perhaps a bit surprising.

Working to improve the construction of climate science facts: core group discussions

In the previous section, we studied how the core group of climate scientists interacted to confront the "outside world". However, the e-mail material also contains lengthy exchanges *inside* the core group, exchanges where the scientists discuss how to overcome various insecurities and dilemmas. A salient example is a debate where different proxies are seen as having different qualities. In the hacked emails, some scientists often cite tree-rings (dendro records) as the most reliable proxies, but others have diverging views. Addressing a number of colleagues, Tom Wigley, senior scientist at the National Center for Atmospheric Research, and former director of CRU, responds to questions about paleoclimatic proxies in the following way:

'How useful are paleodata? I support the continued collection of such data, but I am disturbed by how some people in the paleo community try to oversell their product. A specific example is the ice core isotope record, which correlates very poorly with temperature on the annual to decadal timescale (…) how do we ever demonstrate the usefulness or otherwise of ice core isotopes on this timescale?'

He goes on to contrast ice core isotopes with tree-rings:

'Climate variance explained by the proxy variable--close to zero for ice core isotopes, up to 50% for tree rings, somewhere in between for most other indicators. How valuable are such partially explained records in helping explain the past?'.[18]

These statements can be interpreted as acts of construction (Latour and Woolgar 1979), where the target audience is fellow scientists with diverging views. Protagonists of ice core isotopes could be seen as possible keepers of equally probable statements to tree-ring data supporters. Through statements like those above they are gradually transformed into *unequally* probable statements. This constructed inequality between statements about ice core isotopes and tree rings is illustrated by the same Tom Wigley seven years later, when he rhetorically asked a group of colleagues: 'Do we really want to perpetuate the myth that ice core isotopes are a good proxy for temperature?'[19] As Latour and Woolgar (1979, p.241) points out: 'By being

[18] 12.08.1996
[19] 13.06.2003

sufficiently convincing, people will stop raising objections all together, and the statements will move toward a fact-like status'. In this case one statement travelled down the ladder, from poor proxy to pure myth. This illustrates the power of "written conversation" as a tool of persuasion compared to oral exchanges, probably because written statements facilitate assessment over time.

Although the group remained favouring tree-ring data, they still acknowledged the weaknesses of the approach. For instance, as Jonathan Overpeck, head of the paleoclimatology program at the National oceanic and atmospheric administration wrote in an e-mail from 1998: "trees lack one important trait - they don't work for all parts of the globe". He went on in general terms to discuss different proxies and records in relation to each other, calling for a more balanced treatment of them in the literature:

'I think it is clear that each proxy has limitations (…), but the real need is to understand that each record (not just each proxy) has pros and cons, and that wise use requires knowing these pros/cons. Some coral, ice core and sediment records are no doubt better than some dendro records (…). I'm NOT trying to dis tree-rings, but rather to suggest more balance in what we all say in the literature'.[20]

This quote highlights the role of the scientist in interpreting data; "wise use" also suggests the existence of un-wise use. In other words, data may be read differently and thus are subject to so-called flexible interpretation (Latour 1987, Collins 1992). The quality of the data and of the proxies is very important, but this is not sufficient in itself to make a piece of research convincing. Indeed, the practical skills of individual scientists and how they are assessed by the relevant scientific communities are linked explicitly to the end result; the scientific literature. The scientists are highly aware of the fact that scientific knowledge claims may face deconstruction; experimenter's regress is recognised as a real phenomenon. 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. In this situation, whether a finding is convincing seem to depend just as much on 'wise use' of the data as on the data themselves and the tools that are mobilized to interpret them.

The quote from the scientist also demonstrates how scientists try to convince the collective of other scientists (in this case those using proxies) that they should be more balanced and nuanced in the way this form of data is treated in the scientific literature. Accounts of this sort may be seen as an attempt to form consensus across the whole field of paleoclimate science as well as figuring out how to proceed to make publications persuasive.

Despite explaining up to 50% of the climate variance, tree-ring data relationship to "real-world-climate" is rendered complicated in the exchange. This seems particularly true for data describing the post 1960-period. From this point, the correlation between observed temperatures and tree-ring signals are seen by the involved scientists as weakened. In other words: how to deal with a situation where direct measurement suggests that the world is heating, while some tree-data suggest that it is not. In an email to a few colleagues, Phil Jones, director of CRU gives a summary:

'This [the knowledge about the discrepancy between tree-ring and instrumental data] basically stems back to Keith Briffa's paper in Nature in 1998 (Vol 391, pp678-682). In this it was shown that northern boreal forest conifers don't pick up all the observed warming since about the late 1950s. It was suggested that some other factor or a combination of factors related to human-

[20] 06.10.1998

induced pollution (e.g. nitrogen deposition, higher levels of CO_2, ozone depletion etc) [was to blame]'.[21]

This is an obstacle; something which could decrease the credibility of statements about past climate based on tree-ring data. Tree-ring data now contain *more* information, but much of this is apparently treated as background noise. The situation is seen to challenge the very foundation of dendro climatology, to raise methodological problems, and once again points to the difference between science when you *know* the outcome of a standardized experiment, and science at the frontier, where facts are in the making (Knorr-Certina 1995). The preferred solution appeared to be to stop the tree ring series at around 1960, as described, for example, by Tim Osborn, academic fellow at CRU to collaborators in the intergovernmental panel on climate change (IPCC) in 1999:

'Keith has asked me to send you a timeseries for the IPCC multi-proxy reconstruction figure, to replace the one you currently have. The data are attached to this e-mail. They go from 1402 to 1995, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use'.[22]

However, this solution is seen to raise new problems. As a consequence of the tree-ring data issue, the making of longer paleoclimatic reconstructions requires the establishment of associations between different types of data, collected at various sites by different people, so that the assembled data may render a meaningful story. Previous STS studies of climate science have suggested that different "communities" of scientists have different cultures (Sundberg 2005). If we assume the same to be true for these scientists, making the associations represents not only making dissimilar data talk the same language, but a type of cultural translation is needed. Phil Jones explained it in the following way:

'The new reconstruction only runs to 1960 as did earlier ones based solely on tree-ring density. All the other long series (Mike's, Tom Crowley's and mine) include other proxy information (ice cores, corals, historical records, sediments and early instrumental records as well as tree-ring width data, which are only marginally affected). All these series end around 1980 or in the early 1980s. We don't have paleo data for much of the last 20 years. It would require tremendous effort and resources to update a lot of the paleo series because they were collected during the 1970s/early 1980s'.[23]

Thus, we learn that the lack of post 1980 proxies and the miss-match between some proxies and temperatures after 1960 is a cause for concern, opening for speculations about uncertainties in the results of earlier work. Raymond S. Bradly raises a concern about a reconstruction to be mobilized in the IPCC third assessment report:

'But there are real questions to be asked of the paleo reconstruction. First, I should point out that we calibrated versus 1902-1980, then "verified" the approach using an independent data set for 1854-1901. The results were good, giving me confidence that if we had a comparable proxy data set for post-1980 (we don't!) our proxy-based reconstruction would capture that period well. Unfortunately, the proxy network we used has not been updated, and furthermore there are many/some/ tree ring sites where there has been a "decoupling" between the long-term relationship between climate and tree growth, so that things fall apart in recent decades....this

[21] 11.08.2000
[22] 05.10.1999
[23] 11.08.2000

makes it very difficult to demonstrate what I just claimed. We can only call on evidence from many other proxies for "unprecedented" states in recent years (e.g. glaciers, isotopes in tropical ice etc.)'.[24]

He goes on to argue that the proxy-uncertainties might call into question the accuracy of past work, calling to attention the famous work by among others Michael Mann on the "hockey stick", material Bradley co-authored:

'Furthermore, it may be that Mann et al simply don't have the long-term trend right (…). We tried to demonstrate that this was not a problem of the tree ring data we used by re-running the reconstruction with & without tree rings, and indeed the two efforts were very similar -- but we could only do this back to about 1700. Whether we have the 1000 year trend right is far less certain'.

Such methodological challenges posed by proxy data (and the lack of them) were also seen to affect other questions. A controversial issue was the presentation of scientific results. Scientific papers provide (some) room for methodological reflection. However, thorough and complex methodological discussions are less convenient when scientific knowledge is to be presented in a more popularized form like for example the IPCC summary for policymakers, which supplements the IPCC assessment reports. This may raise challenges with respect to how to communicate results, and the hacked emails provide many contributions concerned with representational techniques and their possible deconstruction. An example is the following excerpt from an email from Chris Folland, currently Professor at the Hadley centre for climate prediction and research, which discusses how using different proxies might produce ambiguity in a place where certainty and clarity was perceived as vital:

'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 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)'.[25]

In a lengthy response to this, Keith Briffa first problematized the concept of truth:

'The latest tree-ring density curve (…) shows more similarity to the other two series- as do a number of other lower resolution data (Bradley et al, Peck et al ., and new Crowley series - see our recent Science piece) whether this represents 'TRUTH' however is a difficult problem. 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'.[26]

Again, notice how the status and credibility of the data and the status of the scientist were co-articulated in this account. As pointed out by Latour and Woolgar (1979, p200) "scientists frequently make connections between these superficially foreign issues. In fact, such issues are all part of one cycle of credibility. In this case the data were not seen to "*speak for itself*"; in Briffa's view Michael Mann spoke for the data, and their quality depended on, among other things, Michael Mann's confidence. Briffa went on to tie the choice of data going into the presentation of the material to the political pressure resting on the IPCC's shoulders:

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[24] 10.07.2000
[25] 22.09.1999
[26] 22.09.1999

‘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 CO2 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’.[27]

The “pressure” described by Briffa is consistent with van der Sluijs (1998), who found a similar dynamic based on interviews, actually with many of the scientists participating in the CRU emails. The reconstruction of the climate of the distant past was seen as a huge scientific endeavour with massive political implications. Extending the reconstruction up to present day was considered an equally challenging enterprise because proxy data are scarce in the post 1980-period, but Phil Jones offered a possible solution: ‘It is possible to add the instrumental series on from about 1980[28] (Mike [sort] of did this in his Nature article to say 1998 was the warmest of the millennium - and I did something similar in Rev. Geophys.)’.[29] In other words: the methodological struggle that was going on in the email exchanges was an effort to establish more and stronger associations between data that does not obviously provide the same information.

A year earlier, Jones in a discussion about a diagram to be included in a statement from the World Meteorological Organization, told that: ‘I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) [and] from 1961 for Keith's to hide the decline’.[30] This sentence became the epicentre of the “climategate scandal”, according to news media, contributing to Phil Jones temporarily resigning as director of CRU ten years after clicking the “send e-mail” button on his screen. However, read in the context of the exchanges we have analysed, this email appears much less dubious. Considering the informal and oral tone, the mail probably just described that a particular method (Mike’s trick) had been used to establish associations between different types of data, an act prompted by lack of post 1980 proxies and the diminishing correlation between observed temperatures and certain tree-ring data. The emails show that the scientists are aware of the methodological uncertainties tied to this practice, but they still chose to proceed on the basis that these were the best data available.

If one cannot be certain about the past, predicting the future could be at least as challenging. There are many instances in the hacked material where scientists wrestle with uncertainties related to both past measurements and future predictions. The emails show how the scientists discussed how to link measurements and predictions, again a task of establishing more and stronger associations between data from various sources. Since the understanding of the past was based on proxy- and instrumental data, and the future was represented by models and scenarios, the two were seen as not necessarily comparable. Frank Oldfield, at that time executive director of Past Global Challenges (PAGES), aired his worries over this issue with a few colleagues in the following manner: ‘I've lost sleep fussing about the figure coupling Mann et al. (or any alternative climate-history time series) to the IPCC scenarios. It seems to me to encapsulate the whole past-future philosophical dilemma that bugs me on and off’. He went on to raise a number of methodological questions, calling for assistance from his colleagues to resolve them:

[27] 22.09.1999

[28] The procedure is, for example, explained in Mann, Bradley and Hughes (1998, p. 784) where the caption of a figure displaying a reconstruction of Northern Hemisphere temperatures reads: “*‘NH’, reconstructed NH temperature series from 1610–1980, updated with instrumental data from 1981–95”*

[29] 11.08.2000

[30] 16.11.1999

‘1. How can we justify bridging proxy-based reconstruction via the last bit of instrumental time series to future model-based scenarios. 2. How can the incompatibilities and logical inconsistencies inherent in the past-future comparisons be reduced? 3. More specifically, what forms of translation between what we know about the past and the scenarios developed for the future deal adequately with uncertainty and variability on either side of the 'contemporary hinge' in a way that improves comparability across the hinge’.[31]

Such issues were considered challenging, as is evident from the way the scientist Francesco summarized the debate:

‘It seems to me it is still arbitrary to use one or another CO2 curve. However, in this arbitrariness, two easy solutions are possible (…): -one is dave's, i.e, assume no change in GHG[32] forcing mix from today, and apply 1% compounded increase to the 1990 actual levels. That gives a concentration of real CO2 in 2100 that is > 1050 ppm. THAT'S 50% higher than projected by IS92a[33], and even 17 % higher than the worst emission case devised in IS92f. - the second is tom's. Just use the co2 in IS92a, and assume that all other further changes necessary to get the hadley forcing (whatever they are) happen in GHG other than CO2. I will repeat that I like the latter solution.’

He goes on to air his general frustration about the process he is involved in:

‘Whatever the consideration of self-consistency and physics are when you make this decision, I do not think we should carry out the national assessment by using "unrealistic" CO2 numbers. I thought the numbers that come out of our exercises (from the impact side of things) were supposed to serve as some basis to be used in the process of decision making at the national and regional level. Am [I] out of line here? There are dozens of people right now, out there, including our group at giss,[34] who are gathering data, fine-tuning models, making connections among physical and socio-economic variables, etc., at a very low "effort spent/retribution received", and then we are going to run things at 1000 ppm[35] in 2100?’

In this quote, the scientist became an advocate for the practice of establishing associations between different types of data, arguing that their inclusion would enrich the analysis of future climate, making it more realistic. However, there are diverging voices. David Schmeiel, senior scientist at the National centre for atmospheric research (NCAR), responded by providing an epistemological distinction between measurement and scenario-based forecasts:

‘I want to make one thing really clear. We ARE NOT supposed to be working with the assumption that these scenarios are realistic. They are scenarios-internally consistent (or so we thought) what-if storylines. You are in fact out of line to assume that these are in some sense realistic-this is in direct contradiction to the guidance on scenarios provided by the synthesis team’.[36]

The de-coupling of “reality” and scenarios is not a new phenomenon. Shackley and Wynne (1995: 116) found similar actions in their study, which showed the complex scientific issues

[31] 07.10.2000
[32] Greenhouse gas
[33] A global energy scenario of the IPCC (1994)
[34] Goddard Institute for Space Studies
[35] Parts per million
[36] 17.05.1999

raised by the coupling of general circulation models (GCM)s and crop models. The resulting uncertainties led some impact researchers to characterize their work as the development of methodologies for how to analyse the sensitivity of crops to climate change. These kinds of researchers were however, reluctant to make claims about the realism of current climate scenarios for impact studies.

A similar discussion between scientists involved in IPCC-work dealt with the question of what the basis should be for an IPCC CO_2 emissions scenario which at the time was supposed to be released shortly. Tom Wigley wrote:

'Mike Hulme has told me something that is quite alarming (…). He said that the new 'IPCC' CO2 emissions scenarios will still begin in 1990 and will not use observed (Marland) emissions for the 1990s. You may either not realize, or not remember, that during the preparation of the SAR[37] and (especially) TPs 2 and 4,[38] IPCC was frequently criticized for using out-of-date emissions data that were manifestly wrong during the 1990s. It would be extremely embarrassing to be subject to the same criticism (…). Beginning an energy-economics model run in 1990 leads to considerable 'flexibility' in 2000 emissions; when, in fact, the 2000 emissions will already be fixed and known by the time the TAR[39] comes out'[40]

David Schmeiel responded, again by underlining that the relationship between models, scenarios and "reality" is not necessarily straightforward: 'I raised this issue at the scoping meeting in Bad (very bad) Munstereieffel, where it was greeted with general agreement but it appeared to come as a complete surprise to many that scenarios should have a relationship to reality'.[41] Obviously, there was a running controversy that was not to be closed.

The email exchanges we have analysed here, give evidence that consensus formation within a core group of scientists may be quite challenging. Moreover, it seems that the main type of problem is related to methodology, not theory. This may be a particularity of the kind of science studied here, but the challenge of aligning data from different sources is probably quite widespread. The absence of agreed-upon criteria that could have settled the controversy seems to imply that closure is a pragmatic affair: some reasonable choice has to be made. However, it is important and interesting to note that doubts about these choices mainly emerge in the internal conversations among core group scientists. Nobody proposes to have such debates in public. From the outside, one should be seen to be in agreement. Clearly, climate scientists experience significant political pressure to express the "voice of climate science" as unified voice, or as a 'nice tidy story about unprecedented warmth'. Consider the statement of the Danish Minister for Climate and Energy, Connie Hedegaard, when she spoke to an audience of climate scientists in March, 2009: '… science must inform us and guide our decisions. Or rather: YOU have to tell US what you know – LOUD AND CLEAR (…) Make your cases compellingly to all decision makers – and indeed to every citizen'.[42] There is no reason to assume that only climate scientists encounter such expectations.

[37] IPCC second assessment report
[38] Technical papers 2 and 4
[39] IPCC third assessment report
[40] 13.07.1998
[41] 15.07.1998
[42] Speech given at "Climate Change: Global Risks, Challenges and Decisions, 10-12 March 2009. Available at: http://climatecongress.ku.dk/speakers/conniehedegaard-plenarysession-10march2009.pdf/

Consensus building through email exchanges?

As we have seen, email is potentially a medium that allows scientific exchanges in an informal but still written manner, something between oral conversations and scientific publications. Analysed as "written conversation", emails may tell about the way that scientists informally discuss their work with colleagues in other research environments. As we have seen, many emails occupies a space between what Traweek (1988) labelled restricted and public information: as *semi-public information*. This is not a homogeneous category but rather a hybrid between formal and informal communication. Like in Traweek's analysis of conversations, in emails the climate scientists performed 'agonistic evaluations' of other scientists work and tried to persuade others to support ones own work, as well as making judgements about other groups or the work of individual scientists. Further, we observed a use of literary techniques of persuasion similar to those used in scientific papers (see, e.g., Latour 1987). In the emails, scientists perform operations on statements by adding modalities, citing, enhancing, diminishing and borrowing, which in turn result in new statements that may be modified, confirmed or rejected. Thus, email communication may be a new persuasive activity, in addition to scientific papers and informal oral conversations.

Also, as we observed, the written nature of emails may invite a more binding commitment than oral conversations, maybe because emails are in a sense frozen conversations and thus available to be read in sequence. Probably, this make emails a useful tool to form consensus (among different scientists located in different locations), for example about how to manage methodological problems or how to represent scientific results in given contexts. Consensus-forming appeared as a precarious activity because of considerable diversity among core group scientists. On the one hand, the emails show how communication and persuasive skills are vital in consensus building and the establishment of climate scientific facts, which is unsurprising from an STS-perspective (Latour 1987). On the other hand, we observed that controversies could be quite resilient in the course of this decade-long exchange. Actually, the email format with its features of informality may be more conducive to prolonged disagreements, also because disagreements through this medium are easier to disseminate. There are still considerable challenges in the understanding of consensus-formation among scientists and the role of different forms of communication.

Through the spectacles of science and technology studies, we found methodology to be a central theme in the hacked CRU email material. Because the scientists so often face experimenter's or fieldworker's regress, controversy among the climate scientists often became controversy about methodology. In accordance with Collins' (1992) argument, it is important to be perceived as using proper methods as well as to have the right skills in applying them. The informal character of the email medium may have made such discussions more visible. Clearly, the hacked CRU emails have shed light on the collective methodological reflexivity among climate scientists, highlighting that methodological struggles may be quite heterogeneous but also how this reflexivity was important in the preparation for participation in scientific controversy.

Why have the hacked emails caused the kind of shock and outrage, for example voiced by some newspapers? Some of the emails contain statements that, when cited out of context, may look suspicious. However, as we have tried to show by analysing a large part of the emails as methodological deliberations, this appears as scientific business as usual. Debates are performed in much the same way as is reported in many ethnographic studies of scientific work. Unfortunately, many journalists seems unaware of such work and have judged the email conversations from a kind of boy scout image of science. Thus, they appear to be surprised that scientific facts are made and not just discovered, that they emerge as products of deliberation and persuasion, that methodological doubts may be resilient, and that scientists' trustworthiness is important. Thus, in the long run 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.

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